Communication Scholars Oral History Project

Annenberg School for Communication Library Archives

University of Pennsylvania

Philadelphia, PA

# KLAUS KRIPPENDORFF

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*Note:* This modified transcript was significantly edited by Klaus Krippendorff.
The original transcripts, synced to the video interviews, may be reviewed at
https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field

*December 20, 2016*

*January 18, February 22, April 12, and May 17, 2017*

Philadelphia, PA

## BIOGRAPHY

Klaus Krippendorff (1932–2022) was a distinguished communication scholar, who spent his career at the Annenberg School for Communication, University of Pennsylvania. Krippendorff made notable contributions to a range of disparate fields, including the methodology of content analysis, information theory, cybernetics, discourse analysis, and design. Krippendorff was born in 1932 in Frankfurt am Main, Germany, and spent his childhood in the city of Halberstadt. After World War II, Krippendorff served as an engineering apprentice in Halberstadt, in what was then the Russian zone of control. He and his family migrated to the Federal Republic of Germany (West Germany) in 1949, settling near Düsseldorf. Krippendorff studied engineering at Hannover's state engineering school, graduating in 1954. After briefly serving as an engineering consultant in Düsseldorf, Krippendorff matriculated to the new Hochschule für Gestaltung in Ulm (the Ulm School of Design), where he was exposed to a variety of lifelong intellectual influences. Soon after completing his Ulm degree in 1961, Krippendorff traveled to the United States on a Ford International Fellowship and Fulbright travel grant. After visits to a number of universities, he took up doctoral studies at the University of Illinois Urbana-Champaign, where he took courses with, among others, Ross Ashby. Before completing his doctorate, Krippendorff was appointed in 1964 to the young Annenberg School, where he remained affiliated until his 2022 death. In the late 1960s and early 1970s, as an assistant professor at Penn, he wrote on a variety of topics, notably information theory and cybernetics. He was, in this period, working with Annenberg School Dean George Gebner on the Cultural Indicators Project, with Krippendorff's contributions centered on the methodology of content analysis itself—the topic of his 1967 dissertation. Krippendorff's 1980 book *Content Analysis*, updated in multiple editions, established his reputation as a leading methodologist. In the late 1960s he introduced a measure of inter-coder reliability, known as Krippendorff's alpha, to measure the level of agreement among trained analysts, which remains in wide use. His work on cybernetics and information theory culminated in *Information Theory* (1986), published after his 1984–1985 presidency of the International Communication Association. It was in this period that Krippendorff revived his interest in, and engagement with, design and design analysis, particularly product semantics, as marked by *The Semantic Turn* (2006). Over his decades of teaching at the Annenberg School, Krippendorff taught a series of long-running graduate seminars, notably *Content Analysis*, *Models of Communication*, *Semantics of Communication*, and *Language and Social Constructions of Realities*. When he died in 2022 at the age of 90, Krippendorff was the longest-tenured faculty member in the School's history.

## ABSTRACT

### Session One (December 20, 2016)—page 6

The interview focuses on Krippendorff's childhood through to his decision to leave for the United States in 1961. His parents' familiarity with the U.S., including affiliations with a German-American exchange program, is discussed, alongside his father's occupational background as an academic engineer. Special attention is paid to Krippendorff's childhood years in Halberstadt, including the city's history and character. The interview discusses Krippendorff's remembrances of the Nazi era, including the treatment of Jews in Halberstadt, up through the end of World War II. The Allied bombing of Halberstadt in April 1945, which hit Krippendorff's house, is recounted in great detail, including his family's re-establishment in the nearby village of Schwanebeck in the Russian zone of control. He describes his father's improvised machine-repair business, subsequent imprisonment by Russian authorities, release, and emigration to West Germany (near Düsseldorf). The interview traces the plan for the rest of the family, including Krippendorff, to escape what had become East Germany, after completing a three-year engineering apprenticeship in 1949. The escape itself is described in great detail, followed by an account of Krippendorff's matriculation to Hanover's state engineering school. After recounting a stint as an engineering consultant in Düsseldorf, he describes his decision to apply to the Hochschule für Gestaltung in Ulm alongside his involvement in the informal youth association Wandervogel. His experience at Ulm with students and influential professors (including Max Bense, Horst Rittel, and Bruce Archer) is discussed, along with the school's faculty politics. Krippendorff's practical diploma project, a motor-grader, and especially his thesis, on the sign and symbol characteristics of objects, is described in light of his subsequent intellectual trajectory.

## Session Two (January 18, 2017)—page 33

The session focuses on the 1960s, beginning with Krippendorff's move to the United States in 1961 on a Ford Foundation International Fellowship and Fulbright travel grant. He recounts his brief stint with the psychology department at Princeton University, leaving at the suggestion of Princeton psychologist Hadley Cantril. On Cantril's suggestion, Krippendorff traveled to meet with George Miller (MIT), Jerome Bruner (Harvard), Anatol Rappoport (Michigan), and George Gerbner (Illinois). He recounts his encounters, including an important visit to Michigan State University, where he was recruited to join its communication doctoral program. Krippendorff describes how, visiting Illinois, he visited with both Heinz von Foerster, Ross Ashby, Dallas Smythe, and Gerbner, and decided to join the Institute for Communications Research doctoral program. Krippendorff recounts his experience with Illinois faculty, especially Ashby's teaching around systems, information theory, and cybernetics, as well as his appointment at the young Annenberg School of Communications (ASC) at the University of Pennsylvania alongside Gerbner, the School's new dean, in 1964. Krippendorff's dissertation project on content analysis, along with a major conference he organized on the topic in 1967 at Annenberg, are detailed. His early participation in, and experiences with, Gerbner's Cultural Indicators project are recounted. Krippendorff also touches on his memories of the Annenberg School as it transformed from a media arts orientation to a scholarly focus. He discusses some of his late 1960s and early 1970s engagement with information theory and cybernetics in published papers.

## Session Three (February 22, 2017)—page 65

The session begins with Krippendorff's recollections about the Annenberg School of Communications (ASC) in the late 1960s and 1970s. He touches on ASC student discontent in 1973, the resulting unrest, and George Gerbner's renewed tenure as dean. The history of Krippendorff's engagement with content analysis is a major theme, including his conceptual and epistemological ideas. He recounts the backstory to his dissertation on the topic, his ongoing work through the 1970s, Krippendorff's Alpha, and his Sage-published *Content Analysis* book (1980). Krippendorff describes his involvement, beginning in the late 1960s, with the International Communication Association, including his 1984–1985 presidency. He returns to the influence of Ross Ashby on his thinking about, and work on, information theory in the 1970s. The session concludes with Krippendorff describing his early courses at the ASC.

## Session Four (April 12, 2017)—page 89

The session focuses on Krippendorff's lifelong engagement with cybernetics, beginning with his exposure to ideas at Ulm through to his 1980s turn to second-order, social constructionist cybernetics. He revisits his graduate school encounters with Rosh Ashby, and his ongoing importance for his (Krippendorff's) thought. His involvement in cybernetics-related conferences and scholarly societies, like the American Society for Cybernetics and the Society for General Systems Research, are recounted. Considerable attention is paid to Krippendorff's organization of a 1974 Annenberg School of Communications conference, on Communication and Control in Social Processes, and the 1979 book that emerged from the conference. Krippendorff traces his constructionist turn to Margaret Mead's paper at the 1967 Gaithersburg American Society for Cybernetics gathering, though he explains that his full engagement with what he called the cybernetics of cybernetics occurred in the early 1980s. His Annenberg teaching on cybernetics-related themes is discussed. Krippendorff describes the cybernetics implications for communication theory and ethics, through to publications appearing in the late 2000s.

## Session Five (May 17, 2017)—page 118

The session centers on Krippendorff's engagement with design and design analysis. After briefly revisiting Krippendorff's experiences at Ulm, the session turns to his revival of interest in design issues in the early to mid-1980s. Particular attention is paid to Krippendorff's collaboration with Reinhardt Butter on product semantics, including the backstory behind early publications and the idea's reception among designers and others. His Annenberg School teaching on semantics and the social construction of reality is discussed. He recounts his 1986–1987 sabbatical at the Ohio State University, where he also worked with a design consulting firm, beginning his engagement with Phillips Eindhoven. He recounts how his interest in design led to his first serious engagement with discourse, in particular his 1998 keynote at the Society for Science of Design Studies. He discusses the overlap, and resonances, between his cybernetics work from the period and the product semantics idea. The background to the 2006 book *The Semantic Turn* is also discussed, including the influence of the later thought of Ludwig Wittgenstein.

## RESTRICTIONS

None

## FORMAT

Interview. Video recordings at the home of Klaus Krippendorff, 510 South 24th Street, Philadelphia, PA 19146, USA. Five mp4 files of approximately two hours each.

## TRANSCRIPT

Transcribed by Beatrice Field. Audited for accuracy and edited for clarity by Jefferson Pooley. Transcript reviewed and approved by Klaus Krippendorff, Jefferson Pooley, and Jordan Mitchell. Transcript edited by Klaus Krippendorff. Transcript 145 pages.

## BIBLIOGRAPHY AND CITATION FORMS

**Bibliography:** Krippendorff, Klaus. Interview by Jefferson Pooley. Transcript of video recording (edited by Krippendorff), December 20, 2016, & January 18, February 22, April 12, May 17, 2017. Communication Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania. **Footnote example:** Klaus Krippendorff, interview by Jefferson Pooley, transcript of video recording (edited by Krippendorff), December 20, 2016, & January 18, February 22, April 12, May 17, 2017, Communication Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania, pp. 34-35.

# Transcript (modified) of interview conducted December 20, 2016 with KLAUS KRIPPENDORFF (session one)

Philadelphia, PA

Interviewed by Jefferson Pooley

***Note:*** *This modified transcript was significantly edited by Klaus Krippendorff. The original transcript, synced to the video interview, may be reviewed at [https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field](https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field).*

Q: This is day one of an oral history interview of Klaus Krippendorff, conducted by Jefferson Pooley in Dr. Krippendorff's home in Philadelphia, Pennsylvania. The interview is part of the Oral History Project of the Annenberg Library Archives of the Annenberg School for Communication at the University of Pennsylvania. The date is December 20th, 2016. Thanks, Klaus, for sitting for this interview. I wanted to start out just by asking you to tell us about your childhood.

KRIPPENDORFF: I guess my childhood starts with my parents; my father was born in 1900, and my mother three years later. Both grew up in Dresden in Saxony in Germany. My grandfather—whom I never met—died early during the first World War. And that was tragic for my father but tells about how things were. He was a lawyer with a doctorate. He was drafted into the German Army at the beginning the First World War. However, as an academic you couldn't be an ordinary soldier, so he was immediately given the role of an officer. He had to ride a horse. I was told the story that he fell from the horse, got hernia and died in a military hospital. So this is kind of the background of an unfortunate European history of war and the tragedies and unnecessary dealings.

My father studied engineering at the Technical University in Dresden. Although he was to be an academic engineer, he wanted to take off, I think, for half a year, to engage in something practical and applied for a job on a ship, a freighter. So he became an engineer on a ship going to India. As the economic situation was dire all over the world, his salary included a huge bag of rice to which he added gifts for my grandmother. He became a student again until in 1924, I believe, when he signed up in a work-study program in the United States. It was actually a very basic program. I believe, students were given the fare for crossing the Atlantic by ship, but once arrived, they had to mostly take care of themselves. So he went to the United States and was fascinated by mass production. He worked, actually, among other places, at the Ford assembly plant in Detroit, where he made several friends, among them a Frenchman with whom he communicated for the rest of their lives. So, yes, this was, I think, his first American debut. But later, he was hired to manage [an] office in New York. Because these German students came to the United States with minimal support—some of them got lost—he was in charge of introducing incoming German work-study students into their new environment and keeping track of where they ended up. This was the American Work Studies Program, AWD in German abbreviations. As children we heard much about his adventures. This was between 1927 and ’29.

My mother’s father died also when she was very young, shortly after the First World War. He was a painter, became ill with bronchitis and other minor complications, but because of malnutrition and maybe bad medical service at this time, he didn’t survive what nowadays would be minor medical problems. So he died in 1919. That was for my mother and her family a big loss. So my mother had to work to support her mother and her grandmother, all three of them. Actually, my mother had also an older sister who married relatively early and was out of the house. My mother found work, first replacing a bookkeeper in her sister’s husband’s business who had been dishonest, then in a bank, and when the Technical University [Technische Universität Dresden], where my father studied, looked for some kind of bookkeeper for a student housing project, she was hired. There she was surrounded by students who had various aspirations, among them to study abroad. So several of these German students were very enthusiastic about going to the United States, and she said to herself, I want to go too. I recently read in her autobiography that she actually was in some sense primed to go, for one of her teachers in geology was always fascinated by Yellowstone Park and its geysers, etc., etc., and she said, I want to see that. So in 1928 she came to the United States and worked as an au pair girl, as a housekeeper in New York and in New Jersey. At some point three other German students wanted to cross the country, driving with a Chevy cross-country. And they invited her, saying, You can cook, and we’ll drive, and she said, No [laughs], I want to take my share of driving. And that’s what she did. So they went all the way through the United States, and among others of course she had to go to Yellowstone Park, and so most of my life as a child, we heard stories about the United States, from both of my parents—their adventures, the people they both met and continued to stay in contact with. Actually, my parents got engaged at the Niagara Falls. Years later I too visited the Niagara Falls with my wife, and when I was there I called my mother to tell her and she recalled the Misty Queen, the very sightseeing ship we had just taken. She remembered the name and her experiences [laughs].

So that was part of her background, and there are lots of fascinating stories—my mother never had a good education, and she basically became a housewife, one could say, but one who had so many initiatives and energy that she just couldn’t stay put like everyone else. For example, after we were born, she helped in a project to distribute baby milk from mother who had too much to others who had too little. And so she did a lot of things besides being the mother of four children. As I said, we were kind of primed to come to the United States because we constantly heard about it. Actually, my parents talked in English when we children couldn’t/shouldn’t understand it [laughs]. It is important, which is kind of interesting, to realize that during the Second World War—it was dangerous to have any kind of association with the enemy. In our family, the United States was never really an enemy of Germany. I recall that my father had always hidden two things from his past memory. One was an American flag with 48 stars, and another, a white ball that he told us he played with in the Panama Canal on the way to India. So these two things, they were always there [laughs], but they were hidden and nobody else knew about them. Also, during the Nazi time my parents had always books on shelves behind which there were other books not to be seen.

So, as I said, my father was an engineer and he changed jobs about every four years, and during the war, from 1942 on, we ended up in the city called Halberstadt, which is in the middle of Germany. It is a small city of 56,000 people. My father was saved from being drafted by working in a factory producing airplane wings. It was for Junkers [Junkers Zweigwerk Halberstadt], a factory in the suburbs of Halberstadt, and that's what he did.

But I think maybe I should say something about Halberstadt, just briefly. In my experiences, when you move around as a child, when you start being someone, you begin to make contacts outside your family. So, while my earliest recollections go far further back, but where I really feel at home is Halberstadt. The main reason, when I was about 10 years old, I started to hang out with friends—a gang of friends. We climbed all trees on the street, we knew every building and whether their occupant were friendly or not. We knew the holes in the garden fences where we could climb through and bother neighbors [laughs], if you want. Behind one group of houses there was a place, a parking lot of sorts, and there were no cars at that time anymore. We frequently tried to play soccer there until we were always chased away. So I mean that was kind of where I became more independent from the family. This had also to do with becoming a student at the *Gymnasium*, a high school, where I learned a lot about the city. And if I'm being honest, I think I still know more about Halberstadt—about its history, outstanding characters, fairy tales and myths—than even about Philadelphia [laughs]. Halberstadt is an old city that was founded in 800 as a bishop's residence. I can show you later on an old map, there is one wall around the bishop's seat and two churches and then a larger one containing the inhabitants that settled around the accommodations for the bishop. One thing that I learned much later [was] that these bishops gave Jews protection to live in the city for whatever reason. They probably collected a lot of taxes. I don't know, I can't say. At some point, Halberstadt had the largest population of Jews in Germany. There had been a pogrom in Halle, a city not so far from Halberstadt, the Bishop invited the victims of that pogrom to settle in the city. Also persecuted Huguenots that came from France found a welcome place in Halberstadt. There was not only a synagogue but also a French church—it must have been a very interesting multi-cultural city.

As a child, I didn't know that much about religion and didn't experience antisemitism. My parents were agnostic Lutherans. The only experience I recall, and I remember it distinctly—walking from school home I saw an unusual old couple dressed all in black with the yellow Jewish star. I didn't really know what that meant, and I asked my parents, my mother, and she explained they were Jews. I didn't know what that meant. But there was no antagonism, at least on my parents' side. Much later, I learned that in 1943—oh no, certainly years before that time, there was a Catholic program in Halberstadt called KLAUS, K-L-A-U-S, which was an acronym of "*Kinder Lieber Außer Sicht,*" or Children Better Out of Sight. It took Jewish children for a vacation to an island in the North Sea so that their parents could find places to settle abroad in England, France, the Netherlands, or Sweden. I believe the Halberstadt Jews were probably better off than in other German cities. Many escaped the horror that awaited them. I read that in 1942 there were about 400 German Jews left in Halberstadt who were put on a train and never seen again. I was 10 years old and [had] no knowledge of that. I am not sure what my parents knew. I once asked my mother. She told me that an acquaintance told her that she saw Jews being picked up in the earliest hours of a day when nobody was watching. This must have been in 1942. Evidently, the public was not to notice such actions.

In 1945, at the end of the war, we were bombed out totally. It actually happened on April 8th, a Sunday morning. I was with a friend not so far away, exchanging stamps which we both collected when the first stage of the alarm sounded at about 10 am. So, I had to rush home, and at 11:30 the carpet bombing of the city started. Within a few hours, 86 percent of the city was very badly damaged. But when I say damaged, it was really destroyed. A famous German author, Alexander Kluge, whom I knew from that time. He was in a high school different from mine. Halberstadt had a reformist or science-oriented high school which I was [attending], and another more humanist or Greek-oriented high school he was in. He researched what happened and why, that people recalled fleeing the city. He described their experiences in great detail. He also interviewed British pilots who were on this mission, from whom he learned that Halberstadt was not the original target. It became a secondary target because the primary target city was covered by clouds. The pilots couldn't see but had to get rid of their bombs before returning to England and they had always secondary, tertiary targets if they couldn't get rid of their bombs. So, I learned that the two waves consisted of about 200 bombers each. Actually, Halberstadt had absolutely no military presence and was of no strategic importance. There were several hospitals, one military with a big red cross painted on their roof, presumably to let them be. There were the Junkers Works [Junkers Zweigwerk Halberstadt], but they were outside the city, and spared in the bombing. The Allied army was two days away from capturing the region. So the destruction served no purpose. It was a tragic incident.

Personally, as I started to say, I went home after the alarm sounded, right away into the basement. As it was Sunday, my father was around, that was very comforting, but my mother had taken my three younger siblings to a small village, Schwanebeck, about seven miles away, mainly because almost every night we had alarms and had to go in the basement, had to wrap us in blankets, and waiting hours for the alarm to be over. At some point this became too much for everybody, and so she took them out of the city for two days. My father and I were in the basement when the bombing started. Our house was hit by a bomb about ten yards from where we were. Half of the four-story apartment house collapsed. Had I been closer, I wouldn't be sitting here. We were spared. There was no electricity for lights, and we could barely breathe because of the thick dust surrounding us. I remember one incident—my father was 45 years old, at the prime of his life, and knew what to do. There was a refugee family from the Rhineland temporarily living in the house consisting of a mother, her daughter who had a baby. When the bomb hit the building they were pushed out of their shelter. Because of the thick dust, the baby stopped breathing and the mother, she was Catholic, screamed holy Catholic prayers, and so my father simply took the baby and dumped its head in a bucket of water that was required to be around and gave it back to her. The mother and her baby was fine.

When the bombing stopped I ventured out of the basement and climbed through a window onto the street. I couldn't recognize my own street. And as I said, I knew to climb all the trees, but none was left. The street was covered with loose bricks. I had to stumble over them. There was a gymnasium [high school] for girls nearby, surrounded by a park. My father had told me to go there where he could find me. So I went in this direction, but when the noise of a second wave of bombers approached, I knew I had to find a shelter and went into the house of a friend of mine that was just half a block to the gymnasium hoping to see him, but I never did. However, this house was also damaged. Its staircase had collapsed, and I had to manage walking over the edges of those steps, which was difficult. When the noise of the second wave of bombers became louder, I experienced something very unusual to me. I found myself with other people huddled together, maybe 15, naively protecting each other from this extreme and unexperienced situation of bombers unseen and explosives heard. Luckily, it turned out that the bombing was not near us but in another part of the city. However, when I looked up I saw that we were laying with half of the house hanging over us. So any small shaking of the ground would have brought it down and killed us all. When I came closer to the gymnasium, I met other residents from our house. They convinced me that it wouldn't be safe for me to stay in the city, soon everything would be in flames. I should also add what I learned later that the mass bombing strategy was to first drop a carpet of explosive bombs on a city which opened up everything and then drop firebombs—I am not sure, were they called napalm bombs at that time?—that set the exposed wood on fire. Indeed, when I left the house in which we had our apartment, I already noticed the firebombs spraying flames on the collapsed part. So, I walked with our neighbors out of the city, eventually to a nearby village, where a farmer gave us food and let us sleep in his barn. The following day I walked to join my family in Schwanebeck where my mother had gone. I remember being afraid of low flying airplanes. This all happed two and a half weeks after my 13th birthday.

Meanwhile, my father tried to save whatever he could. Right after the bomb took half of the house down, he dared to go up two flights of stairs, still standing, to our apartment or what was left of it. He saw his writing desk down from there but could still enter our guest room where we kept our shoes. He took the sheet from the bed, put the shoes on it, bundled it up and carried it out of the house. Shoes were very valuable. He also took an oil painting of Dresden's cityscape to a nearby house, which had not been hit by bombs, but then he had to move what he had salvaged to another house as the former started to burn. The fire was so all consuming. I recall on my way from Halberstadt to the nearby village, we had to cross a mountainous area from which I could see Halberstadt burning. I had never seen a whole city burning, the flames—a black smoke filling the sky with red flames, perhaps two houses high flashing through that smoke. So, this is the last thing that I saw of the Halberstadt I had known.

Of cause my family was very worried of whether I made it through the chaos and relieved to see me. Three days later, on the 11th of April the American Army moved through the region. There was no fighting whatever. At the entrance of the village of Schwanebeck, where we found refuge, right in front of the tiny house we stayed in, the leading tank made one shot in the direction of the village, and absent any response, the Army drove through.

That was the end of the war as far as I'm concerned. We were a displaced family, we lost everything materially, but at least we didn't lose any one of us. My father had no job. He was an academic engineer and not really a practical one. But he recognized the need in this little village of farmers to restore some of the agricultural machines whose maintenance was neglected during the war, and so he had the idea of creating a mechanical workshop to repair them. He also knew that there was a freight train stranded in a nearby village that carried machine tools. I do not know where they came from. It was a last-minute war effort to use them in production facilities near Halberstadt. My father appealed to the American commander. The occupational authorities were actually very set on getting the German economy going again. He got some machines and started that mechanical workshop. So, and that was in May of 1945.

But then the Russians came and took over the area, captured largely by American troops, and my father was arrested and put in prison, and that was of course a very big problem for my mother. She had four children, and then this mechanical workshop, she tried to keep going. And maybe I should tell you why this happened. It’s a sad story but it explains the chaos of that time. My father had been, as I said, an engineer in the Junkers factory in charge of internal transportation. At that time, transportation used mostly electric vehicles. The driver stood in front of it, bringing the tools and materials to where they were needed. With most men drafted to fight in the German army, at that time, German factories employed many women from the east, from Poland and the Ukraine. Some of them saw opportunities they didn’t have at home; others were commandeered to work in German factories. But there was one German driver who was often drunk and harassed these women, behaving pretty badly. So, at some point my father scolded him to respect these workers, not talk down to them, and exploit their underprivileged position for personal gains. This was one part of the story. It continued, I believe maybe in June 1945, when my father went to Halberstadt for reason I don’t know and ran into this guy on the street. After saying niceties to each other, that person mentioned that the director of the Junkers factory, their former boss, was in prison. My father expressed his condolences and also told him where we had found refuge and of his new workshop in Schwanebeck. Two days later, the Russians came with a truck to Schwanebeck and picked him up without explanations. When he was checked in into the prison in Halberstadt, that guy had the keys to open up the cells! That was the missing explanation and how justice was administered after the war.

My father spent about half a year in a prison, first in Halberstadt, then in Magdeburg, the regional capital. At some point he was transported to a concentration camp which the Russians now ran. In the Soviet Union, Russians tended to manage production by assigning quotas. So, the Magdeburg prison probably had to deliver a certain number of prisoners to this concentration camp. As prisoners could die on the way, they put more than needed on the train. On admission to the camp, he and a few others were left over and sent back to Magdeburg. Nobody knows why he was spared. In the Magdeburg prison, he must have been off the books, one could say, because the Russian commander of the prison called him in his office to tell him, You innocent, tomorrow you back with wife and children, but what can you give us? Now, we were bombed out, had no valuables to offer, whereupon my father offered him to do repairs for him in his mechanical workshop and whatever else he could do. But providing services in a distant village was not what the commander was looking for. On the following day, my father found out from others who were released at the same time that they had to bring a wristwatch to the translator and other valuables to the commander on the following day. So this was the way it was.

That summer, while my father was in prison my mother didn't have it easy with four children, managing my father's workshop, and not that much to eat. My brother and I, he 11 and I 13 years old, worked as much as we could for farmers harvesting their agricultural products. As payment we got potatoes and vegetables to eat. And we were also allowed to pick up from the fields what the machines left behind. When my aunt, my mother's older sister, visited us, she saw my mother's struggling with four children between 2 and 13 years of age and suggested to take one of us to Dresden where she lived. That one was me. I was the oldest one.

So I was in Dresden when schools started again after the war. I went in fact to the same high school where my father had graduated from three decades before I entered. One of the teachers even remembered my father. What a coincidence or perhaps not. I was very good in mathematics, liked biology, could draw flowers and the like, but didn't have any interest in Latin, which was an obligatory subject. And now there comes a kind of warning: as a 13-year-old kid, you don't appreciate what is maybe important in the future. I hated it. I thought, Who in the world would want to learn a dead language? Nowadays I know better, and I have to say I blame my parents because they listened to my asking them to get me out of this school. After almost a year in the Dresden high school, from 1946 to '49 I became an apprentice in a mechanic's shop. It meant entering this old German trajectory of craftsmanship—you are an apprentice for three years, then you become a journeyman and work at different places, until you come to a point at which you get settled, get married and ideally become a master of your trade. This is what I could have expected. In all fairness to my parents' decision to yield to my ill-informed wishes, East Germany had adopted a rule that discriminated against children of academic parents to study at a university. I am not sure whether this came into their mind. But the prospect of an academic career was bleak. For three years I learned to work machines to make tools and produce spare parts for machines.

But at some point the Russians decided that whatever was owned by the government before they occupied the East of Germany should not remain in private hands, even if legally acquireds. So in 1949, they picked up all the machines from his workshop, and my father was again out of work. He had good friends among the work-study students who like him spent some time in the United States. One of them got him a half-time job in an organization charged with revitalizing German industries, but this was in the West Germany. In 1949, there was a tightly controlled boundary between the Russian occupied zone of Germany and the American, French, and British parts. So he and my brother, two years younger than me, went to West Germany and he started this job. We continued staying in Schwanebeck and I continued my apprenticeship.

But now how to get out of Schwanebeck, where we had developed many roots? Well, maybe I should go back a bit. After being bombed out of Halberstadt the six of us were living in the half-finished attic of a very small house, at the digression of a childless couple. I still can't imagine how my mother coped, especially after my father became a Russian prisoner and how the couple tolerated our intrusion in their routines. It was intolerable for all of us. At one point the mayor of the village made us a proposition. There was an empty storage house, cheaply built for an unknown military purpose. It was divided it into four equal sized square spaces. The mayor offered us to live in one. It was constructed of prefabricated cement panels, unfinished.

We were offered four doors and several panels to arrange the rooms the way we wanted. The three other tenants divided their space in four equal sized rooms. I found an amazing solution using the panels and doors we had available, creating a large living room, two unequal-sized bedrooms and a corridor from the front entrance to the back of the house where every tenant had their separate own outhouse. I was 13 years old. This was my first architectural design project, one could say [laughs]. I am still amazed that my mother trusted my ideas.

We were well known refugees of the nearby larger city and outsiders in that small village. My parents adapted well to village life. We had our own chicken, raised rabbits for food, got turnips to make sugar syrup, and we had good relationships with the farmers, who appreciated my father's efforts to keep their agricultural devices in shape. Being well connected was beneficial to us but created obstacles to escape unnoticed across the border to a different world. When my father went to West Germany, we couldn't tell anyone. In a small village everyone knows of everyone else. Rumors could easily reach the authorities. I wanted to complete my apprenticeship. My mother sent parcels to her sister in Dresden so that the people in the post office expected us to move to there. She bought the train ticket to Dresden but stayed there only for a few days. I couldn't even tell my best friend. As soon as I had my apprenticeship finished I planned to meet my mother and two siblings in Halberstadt with a guide we hired to bring us over the border. The very last evening I was to be in Schwanebeck a farmer came late at night to tell me that he was in the mayor's office and overheard a telephone conversation from Dresden that they couldn't find my father there. That caller was told of a Krippendorff in Schwanebeck. He asked that this Krippendorff, I, be apprehended to tell where his father was. I was of course shaking the whole night and left Schwanebeck with the first train, 4 o'clock in the morning.

I met my mother, my eleven-year-old sister and my six-year-old brother at a friend's house in Halberstadt. Halberstadt was not too far from the border. The guide we hired had been a teacher for some time in the border region and was well acquainted with this area. So we started out late in the afternoon, taking a train to a place very close to the border, but from there we had to walk, quite a bit. Not to raise suspicion we had to avoid people who lived in tiny villages. Most roads go through them, of course, so we had to go around them. I can still hear the dogs hauling, stimulating each other when they heard unusual noises, making us afraid that someone would inquire about its cause. I also remember a moment when our astute guide heard something ahead on the country road we were walking. Now country roads in Germany tend to have trees on either side, then a ditch for rainwater after which agricultural fields start. Our guide suspected someone coming and asked us to get into the ditch, lay flat, make no noise, and wait out what may be coming. A six-year-old child is not so easily controlled, but he was quiet when three Russian soldiers came by, talking and talking. That was a scary moment of our escape from East Germany. All of this took place at a moonless night in almost complete darkness. When it started to get lighter we were guided through a forest and when we arrived at an opening we ran into two soldiers, but they were British. And I remember my mother hugged them [laughs]! I don't know what they were thinking, but she was so elated to be out of this and be now in freedom and so on.

Taking a train from there to Düsseldorf in the western part of West Germany where my father found a place to work and for us to live, I remember being surprised to see so many cars, railroad stations in good shape, and well-dressed people. Everything seemed so different. This was our escape from East to West Germany.

Maybe I should tell you another story. While we lived in Schwanebeck, my father had invented and could manufacture a hand cranked mill to make poppy seeds into cooking oil. He had made several for people in Schwanebeck and Halberstadt. Poppies was a common crop, but oil was scarce. So a farmer in West Germany heard of it and wanted to acquire one. The plan was that my mother would bring such a mill and get a piglet in exchange. We had no place to raise a pig, but a farmer promised to raise it for us. Now, a piglet is something you can carry in a knapsack, so my mother and I went over the border. At that time it was easier than during our escape, delivered the mill, and got the piglet. However, the farmer warned us that piglets make noises, which we could not afford when crossing the border. So, he gave the piglet sleeping pills or whatever and we thought to be safe. On the train to West Germany, just before the last stop near the border, we looked at the pig and it was dead. So we had to get rid of it. I remember distinctly my mother opened the window and just dropped it out of the knapsack. I remember how it bounced on the treads then typical of German passenger trains. But we had at least no piglet for which we could have been arrested at the border if caught. Many, many years later, my mother met a woman who related to her an experience she had being on a train to Helmstedt, the place near the border, and there was a woman who had a pig that died and she threw it out of the window of the moving train [laughs]. So it was. Amazing coincidences, amazing experiences, and we survived them all.

In Düsseldorf, or rather Ratingen, a city older than Düsseldorf, smaller but close to it, is where we came to live. We occupied a very small wooden house rented to us by the same friend who got my father the job. All of it through these American connections [laughs], you know. And actually he was married to an American also, so that's also another story. So we lived in his former house, and I got a job in a nearby factory, as a mechanic, as a toolmaker—but I was never really satisfied with what I did.

So, one of the things you wrote down. You wanted to know about was the Wandervögel, a youth movement I got involved in. Now–

[INTERRUPTION]

KRIPPENDORFF: Well, the Wandervögel is a very German phenomenon. One could call it a movement. It started around 1900 when German youth discovered nature. Before that, everything turned around the city life or in isolated farmer communities. Nature was just not of popular interest and therefore not really accessible. So, around 1908, largely in Berlin, there emerged a group of young people who determined to explore nature, taking excursion by foot, becoming acquainted with forests, swimming in lakes, and appreciating the beauty of their environments. They called themselves Wandervögel ("wandering birds" in bad English translation). Their ideas had traction for a lot of young people who formed other groups in support of each other, recovering folksongs, camping in forests—all in opposition to constraining city life. In 1911 there were big celebrations of the 100-year anniversary of defeating Napoleon. These young people were appalled by the bourgeois celebrants in big cities who used the occasion to get drunk and learned nothing from that victory. In explicit disagreement quite a number of Wandervögel groups met, camping on a mountain in the middle of Germany called the Hoher Meissner, where they agreed on a manifesto, one could say, or mission statement, basically saying that we, young people, want to shape our own future, independent of the values of our backward-looking elders. We don't want to be bothered by the bourgeois, we want to guide our inner and outer life by ourselves. These were big words, but they meant a lot to an increasing number of loosely affiliated groups, celebrating exploration of nature, mutual understanding and action, not following the established traditions. That movement was against the establishment, promoting different values and exploring new lifestyles. For example, they didn't want to drink alcohol, opposed smoking, women didn't wear makeups. Women were respected equal to men and sexual oppression, as we would now say, was recognized as such and rebuked. But above all Wandervögel developed an appreciation of nature, enjoyed the adventure of travelling, singing folksongs from different cultures, and promoting a different lifestyle. During the First World War many members died but the movement continued, questioning the war. My parents, both of them, were in some ways involved in that movement. In 1934, the Nazis outlawed all of these groups and tried to draft them into the obligatory state sponsored Hitler Youth. Several groups went underground. I read that the Nazi authorities had difficulties tracking them because members knew each other only by their nicknames. When the Gestapo tried to infiltrate groups that didn't join the Hitler Youth they were befuddled because they couldn't figure out who a member really was [laughs]. Some groups went underground. Hans Scholl, a member of the Weisse Rose, who distributed pamphlets trying to make Germans aware of some of the atrocity they observed, was caught in 1943 and executed with others. He had been a member of one of the branches of the Wandervögel, joined the Hitler Youth but practiced opposition to authorities and paid for it with his life.

But after the Second World War there were several people who had been Wandervögel before the Nazi period and whose children started it again. I was one of them. For me it was a kind of a liberation movement, away from the generation that we though had not done anything while witnessing if not collaborating with the Nazi regime. We too wanted to do our own thing. Unlike Boy Scouts in the United States, we never tolerated anyone as a leader that was maybe more than two or three years older than us and we treated girls equally. We had not much money, so we hitchhiked all over Europe trying to learn how other people lived. I met students in Italy, Denmark, Sweden, and France. When hitchhiking we didn't think that we are taking advantage of people's generosity, giving us rides. We thought of providing a service by offering good conversations and we often heard amazing stories about the driver's lives. We did not only travel—we joined a project to reforest areas that had been deforested as part of reparations paid after the war. We organized seminars to understand the origins of the German Nazi past, Marxism, and the newly emerging differences in Western democracies, and what happens in East Germany.

At one point the East German youth organization, FDJ [*Freie Deutsche Jugend*], a state run youth organization, invited some of us to a big meeting in East Germany, thinking they had gotten hold of some bourgeouisistes [laughs]. They made a big mistake. We were so well-prepared. We, maybe 15 of us, were invited to participate in a so-called open forum. The questions that the East Germans were asking seemed all staged. We were asking questions the East Germans did not dare to ask and were worried about. This created quit a ruckus. We were officially ostracized, almost arrested, but made numerous friends who stayed in touch with us in many ways. So, that was kind of our way of coping with the social reality we were facing with the generation of our parents, with authoritarianism in the East and in the West. Perhaps as a result of these involvements, my brother became a political scientist testing the establishment and trying to undermine the forces of authoritarianisms and discrimination.

The Wandervögel was very formative for all of us. Although I was working in a factory, I had academic ambitions. But having become a tool maker without a high school diploma, this was not easy. I took evening courses in Düsseldorf and cleared several practices that were required to start at least in an engineering school. In 1951, I became a student at the State Engineering School in Hannover [Staatliche Ingenieur Schule Hanover]. This was a first step towards a more scientific orientation, albeit limited to engineering. I should like to say three things of my time in Hannover. One is I stayed in touch with Wandervögel friends and in the summer, six of us decided hitchhike to Lapland, the most northern part of Sweden where roads no longer exist and only nomadic Laps roam with reindeer. Lapland is a mythical place largely because you are where nobody goes [laughs]. So we—the six of us, hitchhiked through Denmark, Sweden, and met in Umeå. There, we found a truck that would bring us to the last outpost where Laps could still buy things. From there we walked into the unknown. I have still a reindeer horn in my study [laughs]. So, I don't want to tell you of our adventures but mention that I had another life besides becoming an engineer.

I should like to say, also, that the student at the engineering school were an unusual lot. Several of them were former prisoners of war who had come back from Russia in 1950. Others were refugees from the East whose career was derailed by the war. They were very interesting students. I was one of the youngest, in fact, and I stayed in touch with a few.

In 1954, I graduated from the Schule and there was a possibility of taking an examination that could qualify me for studying at an academic university. I passed the examination, so I was qualified to become a university student without a high school diploma [an *Abitur*].

During my studies, a group of friends pursued the idea of visiting Yugoslavia. At that time, Yugoslavia was a pretty unknown place. First of all, few people ever visited there, and little was known about its sights. Second, it was a communist country but independent of and opposed to the Soviet communism. Yugoslavia had developed the idea of self-government, of distributing the responsibility for factories to its workers, largely because Yugoslavia was afraid of being taken over militarily by the Soviet Union. Third, Yugoslavia embraced quite a number of different cultures with their own languages and rich folklore. And finally, there had been a German-Yugoslav co-produced movie called *The Bridge* [*Die Brücke*] featuring a tragedy of war in very human terms. Some friends dropped out of the project, so my brother and I were hitchhiking to Belgrade, visiting Mostar's famous bridge which was featured in the movie, but when we told people in Montenegro we planned to go to Bosnia they strongly discouraged us, telling us they are all thieves and we would not come out of there alive. We went anyway. We had learned of a small village on a mountain that made a guslarz, an unusual musical string instrument. We were a little bit afraid, soon learned that they had not seen a German since the war, but when we told them where we had come from, they were surprised and happy that we survived because the Montenegrins being all murderers [laughs]! This mutual distrust played a role much later in Yugoslavia's civil war and ultimate breakup.

Well, we came back alive. I took a job in an engineering consulting firm. And I was there for maybe one and a half years or so. I have to say I was the Young Turk among older engineers and as such I had not much of a voice. I was very competent, could calculate almost everything, stresses, speeds, whatever engineers need to calculate, but not everyone could. So I felt in many ways under-utilized. That was one factor that I thought I couldn't—shouldn't stay there, but the work itself was not unpleasant.

But then came a very important event that made me question this occupation. There was a manufacturer that consulted our firm for solving a problem they had with the organization of the company. Our boss went to the board of that company and three of us engineers went to the ground, to the workers. We saw the problem clearly and were developing the proposal for a solution but were stopped because the board didn't want to see its own role in that problem, and the board was paying our boss. We were appalled and I submitted my resignation on ethical grounds [laughs].

However, my resignation was also grounded in a backup plan. As I said, I was pretty competent in handling the technical problems that our engineering consulting firm was faced with. But I increasingly found the human part missing, the social conditions in which technologies had to be embedded. They were far more complex and challenging than making technical objects work as intended. Actually, the above-mentioned consulting was a demonstration of that. At that time a Wandervögel friend entered my world. She had just been in Ulm [Hochschule für Gestaltung, founded 1953] for her first year of studying design and described her experiences, what they were teaching and doing there, in glowing terms. I visited the school and I was impressed about this as well. So, I applied, and I mentioned—or you mentioned the application form, no? Besides having to tell about my background the Ulm application also asked what newspapers I read, what kinds of art I liked, to comment on this and that art. These questions showed me that they wanted to evaluate you as an individual, as someone who participates in society, what you would do, etc., etc. For me, that was very attractive and very unusual. And I was taken—admitted. But later on, I had the feeling that I was taken for the wrong reason, for my record as an engineer. This was, one could say—well, it was a design school, it was a very avant-garde school. It had just been formed in 1953, and in 1956 I became a student at this very young school. Its conceptualizer, Max Bill, was Swiss, and, one could say, a very advanced thinker. He conceived the mission of this school as contributing designs that would help constructing a new technological-artistic—I would say popular—culture in Germany from the ground up. His ideals of good forms were without irrelevant symbolism and ornamentation, which of course the Nazis had applied variously. He wanted to overcome years of exploitation of design for political and commercial aims, to regenerate the momentum lost during the war—realizing his vision of design.

There was another attraction for me to join the school. While Max Bill was the intellectual founder of the school's mission, Inge Scholl, Inge Aicher-Scholl, was a co-founder of the school. She was the surviving sister of two people, Hans and Sophie Scholl, who had formed a group, a resistance group, against the Nazis called the Weiße Rose [White Rose]. They wrote various pamphlets exposing the behavior of the Nazis regarding Jews, regarding the populations in occupied areas. They distributed their critiques as widely as they could, often through the mail, and to people that they thought would be sympathetic to that. But mail can go into the wrong hands or reach people who were afraid to act, even of receiving such messages. They were caught in 1943 in Munich while distributing these leaflets in the university and executed in the same year. There is a famous film called *Sophie* of Sophie Scholl which engagingly describes their involvement and the show trial. It turned out that Hans Scholl had been a member of the Wandervögel, actually a branch of it called *Jungenschaft* (*dj.1.11*). His group was incorporated by law into the Hitler Youth. He collaborated to some extent, but his group was singing songs not approved by the Nazis, and they continued hitchhiking and other daring things that the Hitler Youth didn't do. So he was suspect from the beginning, well before he was arrested and executed. We considered him as one of our heroes. So the school was founded in the name of the executed siblings of Inge Scholl, and this was another motivation for me to study there.

But there is a disappointing side story to that. When I came to Ulm, in the first year we didn't have a place to stay in the dormitory at the school. We had to rent a room downtown. We were two students at the same place. Our landlord, a woman, started telling me that Inge Scholl had been a committed Nazi in Ulm. She was the second highest—no, she shared with another woman the highest leadership role of the BDM [*Bund Deutscher Mädel*], the Hitler Youth for women in Ulm. I was told she was feared by everyone. Our landlord was a member. She was drafted—like everyone else had to be—when she was 10 years old and knew her as a higher up leader. Because Inge Scholl presented herself as a victim of the Nazi time, I was shocked. But I was even more shocked when she related to us that Inge Scholl felt betrayed by her siblings [Hans Scholl and Sophie Scholl] for what they did. Obviously, her siblings didn't dare to tell her what they were doing, presumably because they knew she wouldn't let them get away with opposing these Nazi atrocities. I am still disappointed that I never acted on these revelations. I should have asked her [Inge Aicher-Scholl] how she transitioned from a high-ranking BDM leader to someone presenting herself as an opponent of Nazism and forming the school in the name of her heroic siblings? There was a conception in German law at that time called *Sippenhaft*. It meant that every family member is responsible for the political crimes of their members. So, the father Scholl and Inge Scholl were arrested and put in prison as soon as Hans and Sophie Scholl were caught and executed. But Inge Scholl was a Nazi official. So when I later tried to find out what happened, and was in contact with several researchers, one American—actually she was Jewish, and had lived in Germany—who had researched available evidence, she found that there exists no Gestapo protocol of any interview of the Scholls. Now, it could be that there never was any taken. It could be also that she managed to remove it. Nobody knows. We do know that her father was a well-regarded tax accountant and his clients included several high-ranking Nazis. Surprisingly, he continued his work while in prison—this was all very dark, strange, and suspicious to me.

After the war, to cite Inge Scholl's time in prison was convenient to claim having been a victim of the Nazi regime and she played that role very well. I don't really know whether the execution of her siblings was an eye-opening experience for her or whether her transformation was just an act—I cannot say. But she impressed the U.S. High Command for the American zone in occupied Germany, of which Lucius Clay was in charge, and received the funding for a school, a million dollars or something to that effect, for politically reeducating the German youth coming out of the Nazi period. Because her plans were not as clear as Max Bill's vision and her husband being Otl Aicher, a graphic artist, the school became a private university of design. I regret to have never followed up on this dark political history to which I came much later. Maybe it shows that as design students we were not really that oriented towards the past, we were more focused on creating futures.

Q: Can you say something about what the school was like—its grounds, the social atmosphere when you first got there, especially that first-year course that was basic for everyone?

KRIPPENDORFF: Yes. Well the school was actually located on a hill on the outskirts of Ulm, called the Kugberg. Max Bill designed it and it was a very interesting architecture. It hugged the hill which resulted in several levels. It included places for faculty and students to live–not enough as it was not finished—that's the reason why the first-year students had to find accommodations in the city. There were two faculty tracts, and a big meeting space with a famously designed bar, curved to facilitate conversation. There was a kitchen that served us all the meals. And then you could go up to the lecture rooms and workshops, passing the library and administration. That was the physical layout, all walls were concrete grey, the ceilings white and the window frames showed they're made of natural wood. Everything was either white, grey, or natural wood. Omitting what is not essential, natural, and functional is also what Ulm's designs stood for.

Now, in the first year you asked about, everyone had to take the *Grundlehre* [basic course], whether you wanted to study architecture, photography, graphic or product design. This actually was an interesting idea. Because students came with various backgrounds, from different countries, and with diverse conceptions that populate the arts, humanities, or professions, whatever; the educational idea was to challenge these diverse preconceptions of incoming students and develop a common language able to build something new together. So the first year, obligatory for everyone, was a course where every three weeks there was a new project introduced by one professor, whether in residence or invited for this purpose. For example, we had one, [Helene] Nonné-Schmidt, who had taught at the Bauhaus before the Nazis closed it, to teach color theory. And we had to paint with watercolors, experiencing systems of how overlapping primary colors create all kinds of secondary colors. I still have one of these examples. Then there was a famous guy named [Hermann Von] Baravalle who was interested in geometry and taught us how different geometrical constructions through overlaps and intersections created other and quite unanticipated shapes. To experience these phenomena, we had to draw very many fine lines and then getting hyperbolas and other more surprising shapes. This task was not for everyone. I remember there was one fellow student who really was an artist, drawing straight—that violated his feelings [laughs]. When you have to draw very many lines to reveal a pattern, if one of them is either crooked or of different thickness, the whole pattern may not become apparent.

But there were also a lot of very future-motivated projects. Recently I was reminded by a former student from Ulm—who was not in my *Grundlehre*—one resident typographer posed the project to develop a computer readable typeface. Now at that time there were not computers as we now take them for granted, but he talked of mechanical readability. It implied exploring also the mechanisms that could identify shapes of characters. There were many assignments dealing with advancing ideas, all of them geared to becoming sensitive to different forms, different processes, patterns, and thinking out of the box that everyone brought to the school.

That *Grundlehre* featured also weekly lecture series. For example, by a cultural anthropologist, [Erich] Franzen. I didn't know at the time and learned later that he was one of several professors who escaped Nazism, went to the United States and had come back. He was teaching sociology. I was very impressed with the concepts he developed. There were several now called returnees who spent the Nazi period in Sweden, in England, and South America.

Ulm was a kind of magnet for new approaches. Besides the *Grundlehre*, there were also general lectures by cutting-edge people from all over the world. In the year before my time in Ulm, Norbert Wiener, the mathematician who coined the word cybernetics, was invited and presented his ideas at Ulm. Buckminster Fuller came and talked about dome structures. Not everything was directly connected with design. At least in the beginning, Ulm simply attracted intellectuals and practitioners who were at the cutting edge of developing new theories and ways of thinking that we students absorbed as much as we could. Many came from the United States, some from England. It was an amazing place and totally different from the engineering education I completed a couple of years earlier. It opened my mind to unimagined possibilities.

You earlier mentioned Hungary. There is something else to tell. Recall that I started studying in Ulm in the fall of 1956. So, I was in the *Grundlehre*, near the end of its first quarter, the then rector of the school Tomás Maldonado, gave us an exercise that required painting a great number small 1x1 cm squares—mixing colors by a certain rule—in a very systematic fashion. The conception this exercise was interesting, but its execution was a very tedious. In October 1956, the Soviet Union invaded Hungary in response to a revolt against its communist regime. This caused a flood of refugees crossing the border from Hungary into Austria. It became a really big problem that Austria couldn't cope with, having never experienced anything like this. And so, in December, several Wandervogel members, including my brother, took advantage of their Christmas breaks and hitchhiked to Austria to volunteer. In view of this dire situation, I could not sit still and tried to convince the co-students in my *Grundlehre* who were equally frustrated with this assignment to go there and help out for the Christmas vacation. I had convinced quite a number of fellow students when Tomás Maldonado found out what I was proposing and strenuously objected. He dissuaded many of them to go but four of us went, actually almost to this day exactly 60 years ago.

So, in December 1956, the four of us, one American who owned a Volkswagen, two German students, and I drove to Austria, aiming at the border with Hungary. When we arrived, there was snow everywhere, it was cold, for which we were not prepared, and the people living in the border region were strange. We arrived there at four o'clock in the morning. People came with their horse carriages to bars to drink their schnapps and then took their horses and go to work. When looking for who would be in charge of the refugees, we came across an amazing number of very interesting people: There was a Swiss cook who had said goodbye to his hotel and cooked for refugees; there were teachers, nurses, and drivers from a West German Catholic charitable service, a diversity of people with fascinating histories all willing to help in various ways. We heard stories that reminded me of our border crossing from East to West Germany. Refugees were often uncertain of whether they had crossed the border and suspicious of anyone in sight. So it was difficult to assure them of being safe. A couple of days before we arrived, there had been two Americans who with a small boat ferried refugees over a small river. They were caught, not knowing that both sides of the river was Hungarian territory. This created difficulties for the Austrian government, which in turn prohibited all foreigners to come close to the border.

We were foreigners, of course, and worked for two days in a big hall repurposed to feed refugees, putting up furniture and cleaning tables, but felt underutilized. People told us, to make a more meaningful contribution, we should talk to a princess, a former nobility in Vienna. She was in charge of all rescue operations and would appreciate any help she could get. Well, this was Austria. We were kind of naïve, we didn't know the social landscape of the place. We, four somewhat artsy-looking students without [laughs] really outstanding competencies, offering help to a princess. In Vienna, we traced her to a hotel. When asking to talk to her, we were told she was at a ball in that hotel. We wanted to talk to her and asked a reluctant manager whether he could ask her to spare a minute to talk to us waiting outside. Her response was she is not available [laughs]. In Austria, nobility played still a big role. This effort brought us into the evening, and we had to find a place to stay. We certainly didn't have the money to stay in that or any hotel.

Now, when hitchhiking through Sweden we went several times to a prison that was made available to let us sleep there in the absence of a youth hostel. I proposed we try to do the same in an Austrian prison. So, we went to a police station. To stay in a prison overnight turned out not an Austrian practice, but the officers couldn't really deny our request. So, there was lots of formalities, telephone calls, checking our identities, and filling out forms. There was one moment when I thought our effort to stay at a prison would collapse. The American among us made funny remarks about a picture that was hanging in the police station. What was this picture? It was Kaiser Franz Joseph of Austria. I was simply amazed that he still played a role in a governmental office, but my fellow student's comments made the officer really upset. How could you make funny remarks about a long-gone king? Although we were not equipped to handle the Austrian culture, it was also the American who could say that, having no sense of nobility. Anyway, so we went to the prison where we were really treated like prisoners. We had to empty our pockets into boxes, our passports were exchanged for numbers, and the whole process took a very long time. By I think by one o'clock in the morning, we were finally led to our cell, guided through long corridors, from one gate to another, opened with keys from standing by guards. We could sleep only a few hours because the prisons had routines to follow. At 4 am we were awakened and asked to get dressed. An hour later came a cleaning crew of prisoners. They inquired what we had done, why we were here [laughs]. So, we had no acceptable answers. After another hour we got something to eat. It consisted of coffee and bread, just bread sliced—that's it. After additional hours of waiting, we were given back our personal possessions and released at maybe ten o'clock—in any case, late. We had hardly slept but relieved. We gave up our mission and drove back to Ulm and painting squares. I was upset that we tried to help in a country unable to cope with a human tragedy at its border, but were unable to navigate Austria's bureaucratic structure. It told me that pursuing unconventional proposals can run into conventional limits.

On another note, actually I didn't tell you about my engineering work. Do you want me to say?

Q: Yes, please do speak about—you're speaking of the engineering school at Hanover?

KRIPPENDORFF: Yeah, in Hanover to graduate, everyone had to develop a technical device. I decided to design a steam motor for automobiles. You know, almost all cars have piston engines. Even steam locomotives do. Translating the ups and downs of pistons into the circular motions of wheels occupies much engineering effort. I had the idea of translating their function directly into rotary movements. In some sense, I invented the idea of a Wankel engine that surfaced much later. The engine I suggested used two rotating gears, driven by high pressured steam that could pass between their teeth only one way to be released on the other side. Everyone found this to be a great idea. Nobody could tell me whether it would work. Today I doubt it. But I submitted that design also with my application to study at Ulm. It is now in its archives, so you can see it there [laughs]. I mentioned earlier that I believe Ulm accepted my application not for what motivated me to go there but because I was an engineer.

So now where were we?

Q: Well, you were about to perhaps speak of the rest of the basic course year and your experience in that.

KRIPPENDORFF: As I said it was eye-opening on very many dimensions, and that was the whole idea, to break down prejudices in favor of opening us up to new visions. And I think this basic course succeeded in many ways. Students had to design and explore something they'd never even dreamt of, like I had never designed typographical characters, I had never discovered the geometric makeup of complex shapes, and so on. There is one interesting thing that [Hermann von] Baravalle, whom I mentioned earlier, showed us. He had a model and that consisted of two circular plates with lots of strings attached connecting them, perhaps two feet apart. He used a slide projector and started to project a straight line into this cylinder of strings. Of course, you see an ellipse. So a line becomes an ellipse cutting through a cylinder. And then he projected other curves into the cylinder and amazing new shapes emerged we had never seen before and for which we had no name. Obviously, the lesson of this exercise was to demonstrate how innovations can emerge from a combination of what is known. Two of us, one who became [an] architect, made replicas of the device which I wanted my parents and my family to experience. My model disappeared somehow, but his survived and he recently showed this to a department of architecture in Germany and they made a film of it. I mean—

this all comes from Ulm, you know? To me, the main idea of this course was to expose us to new concepts, to open us up to new creations of the world.

Actually, we recently had a discussion at the Annenberg School [University of Pennsylvania] on the requirements for first-year students, and I brought up the idea of a basic year for everyone. We knew that incoming students came with very diverse and often unclear preconceptions of communication, coming from different academic and practical areas. Instead of nurturing their preconceptions, I still think it vital to provide a place to challenge them or channel them to what we as faculty think could develop in the future. I didn't think we could agree to something like this, for not fitting into our course structure, but fractions of this idea were implemented in a proseminar during which students were exposed to what diverse faculty members were seeing important and motivating their research.

Back to Ulm, after the first year, students had the choice to study in one of four departments. There was product design, there was visual communication, there was architecture, and there was the information department. So we could specialize. But there were enough courses that cut across these departments. For me, one of the important early influences, maybe two, I think was Max Bense. He was an unusual professor of philosophy from the University of Stuttgart. He had developed a—one can say theory of aesthetics, based on [Claude] Shannon's information theory. In retrospect, I realize that it was not really a theory of information but let's say, a cultural theory of the aesthetic receptions. He started with the observation that all art needs to be innovative, meaning containing new information—something that you've not observed before, and if it is not innovative then it is not art. That was his definition of it, and I think it can be applied to technological artifacts as well. Indeed, radical innovations always attract at first only a few people. Later, when a piece of art becomes more widely known, distributed and popular, it loses the informativeness it had in the beginning. He connected this phenomenon to information theory, whose measures assign rare messages more information than frequent ones. Now, I dare saying that he wasn't able to get into the details of Claude Shannon's information theory, but he had a sense of the philosophy behind it and got us actually to think along these lines. This was conceptually very productive for all of us and the core of the information department which he headed when I came.

In my second year, Max Bill was on the way out, mainly because—there was a revolt against him by the junior faculty he had hired. I could say, as intellectual founder of the school, he assumed a certain privilege to be heard. While I think he was a dominating figure, his arguments were always profound, supported by evidence. His judgments were almost always correct. But he was Swiss, he was direct, and he had no inhibitions of criticizing everyone who had failed in some respect, which was socially not particularly effective [laughs]. So he was effectively fired by faculty that felt intimidated. In his last and my first year, he gave an informative seminar on the twentieth century Bauhaus thinking. I learned a lot from him about the different schools, different art and design movements, in Germany and elsewhere. He had been a student in the Bauhaus [in Dessau] from which he took many ideas into our presence. Some outsiders consider Ulm the successor of the Bauhaus. I am not in full agreement with that attribution. Ulm certainly accepted being an avant-garde institution and it was. The Bauhaus started in Weimar, then moved to Dessau. [Walter] Gropius, who had presided over the

Bauhaus until Nazis shut it down, subsequently worked in the United States. He realized that Weimar did not provide the cultural environment for an avant-garde school and found a place in Dessau. Dessau was an industrial city. It embraced the Bauhaus' mission of making well-designed products available to everyone. In Dessau the Bauhaus became famous internationally, perhaps less for this mission but for its minimalist style. It attracted creative figures—[Wassily] Kandinsky and [Paul] Klee and other avant-garde painters. It started a movement of performative arts. The idea of making socially responsible mass production by designing easily producible and widely usable products, not only for those who could afford them, attracted also communist ideas—but strange ones, not in a Soviet sense. Perhaps for these reasons, the Nazis closed it in 1933. It moved briefly to Berlin, where it was completely dissolved. In 1937, László Moholy-Nagy, the last director of the Bauhaus, took several of its teachers to Chicago and founded the IIT Institute of Design.

Max Bill was an architect, sculpturer, and painter with a vision of the school. To me, Max Bense gave Ulm the intellectual status that other faculty lacked. Most faculty did not have academic degrees. Bense stayed only one year after Bill left. In search for a replacement and because the notion of information that Bense had planted into the curriculum, which mobilized not only the department of information but our thinking as well, they found a mathematician who knew information theory and that was Horst Rittel. I remember the very first lecture he gave to all of us. He had no clue of what designers were concerned with. He didn't know how little we knew about the subject, what the school was like, and he presented what he was asked to do: the mathematical foundations of information theory. Assuming we knew probability theory, he developed the various theorems of information theory, and got almost everyone lost. Bense taught us generalities of information, always in a cultural/mass distribution context of the reception of art and poetry. Nobody was prepared for the mathematical arguments Rittel provided. But Rittel was an amazing character. He was a fast learner and very adaptable to his new, our environment. He very quickly saw what design was to achieve—albeit through his own lens—and what he could contribute to the school. He became, one could say, a theoretician of design, a label applicable to the rest of his life. He enriched our thinking by exposing us to numerous cutting edge mathematically founded ideas among which was cybernetics, the importance of feedback; planning theory, the idea of articulating plans that could be realized in the face of competing plans; systems theory, an approach that guided us away from designing single products but interconnected technologies. One of his examples was designing a gasoline station. It can't just be one station but a design that is reproducible widely, adaptable to various configurations of streets, and able to compete with other systems of gasoline stations. He soon became the intellectual engine in Ulm, at least for me. And my current thinking embraced his ideas in many ways.

There were of course other for me equally crucial influences. Ulm attracted several Fulbright professors from the United States. One was a professor of ergonomics from Ohio University. He could barely speak German, but managed, and we learned a lot from him. He was a very advanced thinker on ergonomics—not that I liked the philosophy of it but still. Another Fulbright professor was Joe [Mervyn W.] Perrine. He had just graduated from Princeton University with a social psychology degree and he acquainted us with the notion of social perception. It directed us away from talking about forms as if they were so for everyone, independent of who sees them, from which perspective, and with which social background. His examples were minimal but convincing. For example, I remember he talked of a study of the perception of one dollar coins, which were seen larger by poor people than by rich ones. Although he didn't know very much about design, he directed our arguments away from objectifying the forms of products, architecture,s and graphic designs by raising questions for whom something is what they think it is. As all perceptions are tainted by our background. I applied this insight later in my diploma thesis. For me, this was eye-opening, and questioned common conceptions, especially of artists' claim that pieces of art speak for themselves. In Germany, the word of *Gestalt* means how something appears in fact. The name of the school was Hochschule für Gestaltun*g*. In the United States, Gestalt psychology, and Gestalt perception is considered central to matters of design. For example, when you draw an incomplete circle, you see it as a circle by subjectively overlooking the missing part, and you talk about that circle, often not recognizing that it was incomplete. Perrine added the social dimensions to perception. And that turned out to be very important for me.

There were four departmental in the school. Was in the product design [*Produktform*] department and I do not want to go into what we did there. I went to lectures and participated in discussion at other departments whenever I could. I think it was in my third year that I became the student representative of the product design department. You mentioned earlier about the pipe. At that time, we had lots of disagreements with the administration, partly because teachers began to devote more time to lucrative industrial projects than to their students, but also [Max] Bill's departure left a number of students disappointed because they came to Ulm to study with him, many Swiss students did. There was one group of students dismissively called Billists, and an opposing group that sided with the administration. I didn't come for Bill, but also felt it unfortunate that a very good professor and devoted mentor to several students was dismissed without a plausible cause. There were uncertainties about the curriculum, and heated discussions with the administration about the direction of the school without Bill. As a representative of one department I was in the midst of these conflicts. So, one of my [fellow] students, told me, Klaus, you respond too fast, you have to take this pipe, keep it lit and speak only after taking a puff. That is what I did, and I would say we won a lot [laughs]. Perhaps not so incidentally, that fellow student later became a therapist.

Now, whether we won because of that, is an open question, but I continued to smoke at social occasions, leisurely and without inhaling, up to 1972 or something like this and then I gave up. But I later found out that the same strategic advice was given to Bertolt Brecht before his hearing by Joseph McCarthy's House Committee of Un-American Activities [House Un-American Activities Committee]. Bertold Brecht was a famous German playwright and poet who escaped the Nazi regime to the United States. He was a favorite poet and playwright of mine. He was given the same advice not to answer any question before taking two puffs with the cigar he tended to smoke. And I actually have the recording of this hearing. There he was asked a question—by one of [Joseph] McCarthy's lawyers, [Roy] Cohn. Cohn read one of Brecht's poems to him, which had to do with the proletariat rising up. It was clearly communist-inspired. Many of his poems addressed the horrors of war and injustice to workers. According to the recording, the poem was read in part and Brecht was asked, Did you write this poem? There was no answer. The question was repeated, and after another pause, Brecht said, No, I wrote a German poem. So—the recording suggests that the two puffs on his cigar enabled him to give an answer that challenged the translation of his poem and Cohn's qualifications of using it against him. I learned that right after that hearing Brecht left the United States for good.

As I mentioned, I crossed several department lines in Ulm. I spend a lot of time doing photography and became a pretty good photographer. I ventured into the visual communication department to use their typesetting equipment that a student helped me to master. I played chess with one research assistant and designed chess figures that symbolized the moves that the figures could make. I competed with a poster for an event the student body was planning. I didn't care too much about architecture, which specialized in industrial designs, but I did learn from lecturers in the information department.

In the fourth year, acquiring a diploma in design called for satisfying two requirements. One was a practical work, a design developed in sufficient detail and justified, and the other written thesis on a theoretical or conceptual contribution. For the practical work I thought to design not just another household gadget but something more challenging, something in which I could blend my engineering skills with what I had learned since. I settled on the design of a motor grader—one of those utility vehicles that could grade gravel streets and be used in construction. I visited a company that produced such equipment not so far from Ulm. They were not sure of what a designer could add to their machinery but gave me their technical drawings and related stories of what the company saw as outstanding and where they found inadequacies. To my regret, I could not interview any drivers of such graders. Reconceptualizing the motor grader and making it user-friendly was not a big problem for me. I could do that. And the model I created with a book of worked-out details is now in Ulm's archive. Its photograph was reproduced in various publications. Besides what the architecture department at the Ulm school worked on, in the department of product design it was the largest design attempted and later recognized by the cultural arm of the Federal Association of German Industry [Kulturkreis im Bundesverband der Deutschen Industrie] as the best design in 1961.

But for the theoretical part I wanted to do something very different. I had come to Ulm to escape the determinism that engineers thrive on for the challenges of the complexities of the human uses of technology. I thought I could demonstrate my engineering background in the practical part but use my theoretical thesis to contribute something more human-centered, based on what I had become increasingly fascinated by: the relationship between human social perception and use. So, I proposed my thesis to be about objects as seen not according to how engineers designed them but according to how they are embedded in social processes of communication. The distinction between engineering technology and the communicative role they played in the lives of users, bystanders, and critics became the guiding framework of the thesis. I argued that artifacts needed to be designed to accommodate what users had in mind to use them for, that they should be seen as composed of signals that are interpretable as signs able to point to the actions that artifacts actually afford. While I don't recommend reading this thesis, it was the first piece I had ever written. However, its ideas have actually fueled much of my later work in design. When I look back, it is surprising to note how much we rely on very earlier formed ideas.

Q: So could I jump in there? I mean, given the description I've read of this thesis work, it seems to be about the kind of communicative role of things in everyday life, and this would seem to be connected to your entire trajectory.

KRIPPENDORFF: Yes, in fact—Horst Rittel frequently talked of communicating information—not in the sense of posters announcing something, but as interaction between people. I framed my whole thesis in these terms. It was surely naïve by contemporary standards, but I suggested that designers should see their work as communicating the usability of their designs to their users. This ran into difficulties with a dominant conception that I am still fighting. I presented my thesis proposal to [Tomás] Maldonado. He had been a cutting-edge painter, from Argentina, and had introduced the idea of semiotics into the Ulm curriculum. I thought it made sense to ask him whether he would be willing to be my thesis advisor. I went to his house to explain what I wanted to explore. Unfortunately, my ideas didn't resonate with his conceptions. He said, Klaus, you are making a categorical mistake. Objects are the referents of signs. Signs are what refers to the real world, including of the artifacts we design. He had solidly bought into the traditional two-world conception of semiotics: the unbridgeable distinction between [the] realm of the real world phenomena and the realm of signs and symbols and the two shall not be mixed. So I realized I couldn't work with him and I worked with Rittel instead. This experience was, I think—crucial in my later trajectory. I have always been against this simplistic worldview and associated representational theories of language. I wrote this thesis. It was long. It is written in German. It was my first writing ever. I can't recommend reading it except in the above-mentioned context, although it is available from the [University of Pennsylvania] Scholarly Commons,[1] and I get occasionally notices that someone downloaded it. There are better and later publications of mine, but that was me in 1961 [laughs]. So, then, after this, I stayed on—

Q: Could I just interject quickly, because I'm curious about the student culture at Ulm and you had mentioned in your memoir[2] a bit about adventures around the area and indeed going to this fortress with students—fellow students and wandering around. You also mentioned an anecdote about a "red dot" controversy and I thought you could mention—

KRIPPENDORFF: Let me relate a few indicative happenings. To start, Ulm introduced a sometimes-stifling requirement of purity of forms and sought to preserve such appearances to the public. The classrooms had large windows. At one point a student in the *Grundlehre* fastened a big red dot on his window, visible from the outside. Otl Aicher, head of the visual communication department, saw it and got so upset to threaten dismissal of the student. I mentioned that the walls of the buildings were grey cement walls. Bill designed them to be true to the nature of the material used. Subsequently, grey became the color of choice for most Ulm

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[1] Klaus Krippendorff, *Über den Zeichen– und Symbolcharakter von Gegenständen: Versuch zu einer Zeichentheorie für die Programmierung von Produktformen in sozialen Kommunikationsstrukturen* (Diplom Thesis, Hochschule für Gestaltung, Ulm, 1961), https://repository.upenn.edu/asc_papers/233/.

[2] Klaus Krippendorff, "Designing in Ulm and off Ulm," in *hfg, ulm: Die Abteilung Produktgestaltung: 39 Rückblicke*, ed. Karl A. Czemper, pp. 55–72 (Dortmund, Germany: Verlag Dorothea Rohn, 2008), http://repository.upenn.edu/asc_papers/138.

product designs. It was an unarticulated taboo to use colors—except for posters in the visual communication department of which Otl Aicher was its master.

Physically, there was the city of Ulm downtown and there was the Hochschule für Gestaltung on the Kuhberg, which was the name of the elevation at the city's edge. The traditional city folks considered the school outlandish. Although there was an avant-garde art gallery in Ulm where Max Bill and a few others exhibited their works, and a very few important Ulm politicians were sitting on the board of the school's foundation. Socially, the city folks didn't want to have much to do with what happened on the Kuhberg—except when it was Carnival [*Fasching*]. Each year, the students in Ulm invited everyone—of course especially girlfriends, boyfriends, and personal acquaintances—to come to the school and celebrate Carnival with dancing to band music, snacks, and alcohol. The preparations for this event grew every year in magnitude. At some point it was decided to use egg cartons, not merely as decorations, but all over the walls and windows. Thousands of egg cartons were purchase, and contrary to tradition, they were spray-painted. The public places inside of the school were transformed. This upset the purists among the faculty who refused to be part of the event. But for younger people in Ulm, Carnival was one experience not to miss. They also found a windowless room, dimly lit with red lights, originally designed to be a storage space, now transformed into a place for intimate schmoozing. It became known as the red dungeon [*rote Höhle*].

This red dungeon witnessed also another telling event. On the day before Max Bill left there was a party arranged for him in the red dungeon. He had been the architect of the building. As a spoof, someone decided "to give him a house tour." He was a big guy, and a student who was also big and another from Holland with whom I am still in contact with, decided to carry him on their shoulders and show him around. So Max Bill was momentarily held facing a grey wall above the entrance of the red dungeon when he took a piece of chalk out of his pocket and just for fun signed that wall with his name. The next morning I talked to other students and we asked ourselves how we could preserve that signature. So, someone had a hammer and a chisel and with the help of a ladder we engraved his signature into his building before the caretaker of the building could prevent us. I presume it is still there.

But the student body, as I said, it was very divided. First because of the departure of Max Bill and later because of the development of another schism. I already mentioned that several professors had their institutes where they worked on lucrative contracts from industries and had less time for their students. For example, Hans Gugelot was a major source of innovations of electronic, kitchen, and other household devices for Braun, which made the manufacturer as well as the school famous internationally. His designs became the face of the Hochschule für Gestaltung and of modern design in the world. While we admired his work we interpreted his unavailability to us as a lack of interest in teaching. For example, he became the advisor for my practical diploma work. He signed the papers, but I never had a chance to tell him what I was doing nor did I receive any feedback from him on its result. On the other side was Horst Rittel, a full-time faculty member, and several temporarily employed professors who tried to fill the gaps the practicing designers left. They devoted more time to teach, advise, and critique what we were doing, and their ideas got traction among students which sidelined the very design practitioner who had managed to remove Max Bill earlier. The schism was defined as one of designers versus theoreticians, with the implication that designers were the only ones who could decide on the direction of the school. Theoreticians were to be considered as mere appendices. Trying to treat those who brought new conceptions into the curriculum as secondary faculty, moreover, undermined the public aim of the Hochschule to be recognized as an academic institution, led by designers who had no academic experiences or degrees, in opposition to those who had both and were very much admired by students.

The opposition of theory and practice was really unfortunate. I think it hid an underlying problem. By contrast to the designers, the so-called theoreticians brought an amazing array of new ideas into the curriculum that enlarged the student’s horizon and raised new questions. The so-called designers became increasingly unable to understand what developed outside their institutes, must have felt isolated but had the power to curb these threats. Actually, I had—I don’t know if we should deviate from that topic—a recent experience of being in Basel, at a reunion of the former graduates from Ulm. The Hochschule für Gestaltung und Kunst in Basel invited us to see what they were doing following the Ulm example. I was so disappointed. They were entirely focused on shapes, colorful forms, and gadgets and exhibited them claiming to have developed from Ulm. There was no evidence of what had mobilized most of us in Ulm, namely the larger cultural context of design, seeing the networks of technological connections in which we live, and the larger responsibilities designers ought to assume for the well-being of our society and culture.

Q: Well, you said that you finished up your thesis at the same time you completed the motor grader project and that won an award. You decided, I think, to stay on for another year with the Research Center for Visual Perception, and can you say what that experience was like? It sounded as if maybe this tension you describe between the practitioners and the theoreticians kept plaguing that research center.

KRIPPENDORFF: Yes, exactly. That center was Joe [Mervyn W.] Perrine’s idea. Perrine came as an American Fulbright Scholar to install in Ulm a variety of demonstrations for visual perception that were developed by William Ittelson at Princeton University. I don’t know if you have ever heard about him or his creations. For example, one consisted of a distorted room that from a designated position looked like a perfectly rectangular room with equal sized windows left and right. But someone entering that room appears either huge or small depending in where he or she stands. Ittelson had created several demonstrations showing the relativism of visual perception. While installing them, I already mentioned that he introduced research on social perception into the curriculum. While working on these demonstrations, he proposed and with the encouragement of the director of the school realized a Research Center for Visual Perception [*Institute für Visuelle Wahrnehmung*]. While finishing my diploma work, I joined two other employees of that institute as a research assistant. Perrine secured a big contract from the Fraunhofer Society, which was actually connected with the German military, on issues of camouflage. And so we studied contrasts and distinguishability of colors, for almost a year. But our research orientation was different from what the institutes of the practicing designers aimed at, which caused, as you suggested, totally meaningless tension with them.

Otl Aicher, an outstanding graphic artist, was particularly critical of what we were doing. Color was his métier. As the husband of Inge Aicher-Scholl, he had considerable influence in what happened in the school. At some point he made clear to us that, Whatever you find out is meaningless because I know more about color than you can tell me. Although Tomás Maldonado, then the rector of the school, publicly advocated the use of science, his conception did not include the careful collection of data and their analysis. I granted Aicher his expertise but asked him whether it would not be worth finding out how other people see things. His answer was, They are irrelevant and studying them is not worth pursuing. Whether he echoed or created opposition to serious research without understanding what it could reveal is not clear. Soon after I went to the United States, the administration of the school imposed financial demands on the institute it could not meet and it disappeared from Ulm. In my view this was one instance that reduced the chance to live up to the school's academic ambitions.

Q: One last question before we conclude, about that decision to move to the United States. And that is: You mentioned in this memoir about going to various factories for summer work or apprenticing. And one such summer, maybe it was 1959, you went to Oxford in the UK and you described a pair of experiences, at least, that were interesting to you—

KRIPPENDORFF: Ulm was on a quarter system and as you said we had the summer available to us. The first summer I went back to my old engineering consulting firm. I needed the money and could live with my family nearby. The second summer I interned with a manufacturer in Ulm to get needed organizational experiences. Before the third summer, I had applied for a Fulbright fellowship to study in the United States, and decided I had to become more fluent in English. Because my high school education was disrupted as I mentioned earlier, my English was inadequate. With the help of a British professor who briefly taught at Ulm, I got an internship in the design department of a refrigerator company in Oxford. That company was next to the production facility of the Mini, the British Mini, which is much smaller than the ones now produced in the U.S. I was fascinated to see how assembly lines work. The work at the company—actually I was given the chance of developing several details of their products—was not particularly challenging, except for one assignment that pointed me to my later work on product semantics and interface design. I observed that these refrigerators were not too clear about how users could adjust their temperatures. So I designed icons on a knob that made the setting of temperatures more obvious. Whether it was produced, I don't know.

Interesting to me was meeting someone who had studied at the Bauhaus in Dessau. And he was of course interested in finding out all about Ulm. He became kind of a mentor to me, helping me to navigate in this new environment. In return, I wanted to know from him about the Bauhaus, what he took away from there. To my surprise, he couldn't tell me much. And I realized that, even after a good education, an avant-garde education, you can become an ordinary draftsman, which he was. And I didn't want to go that route [laughs], you know. I rented a room at the boarding house of an Irish family with several workers. I learned a lot of everyday English food and could practice my English, very basic though.

Oxford is of course a famous university town. I roamed that town and I went among others to Blackwell, which is the famous publisher's original bookstore in Oxford. And I picked up two books that unknown to me would shape my academic future. One was by Ross Ashby. Horst Rittel had mentioned his name in conjunction with cybernetics. The book was titled *An Introduction to Cybernetics*, just published in 1956. My English wasn't good enough to appreciate what it said but I thought I would be able deepen my understanding of the concepts Rittel had taught. The other was Ludwig Wittgenstein's *Tractatus Logico-Philosophicus,* published in 1922. This book was attractive to me for one reason: It had a German Text next to its English translation and I thought I would learn English from that. Although I worked through many pages, I was not yet prepared to appreciate his significance. As it turned out, both books were instrumental in my later preoccupation with communication. Ashby's book because, when I went to the University of Illinois, or maybe I should talk about that later?

Q: You could mention it quickly—

Krippendorff: Well, I found Ross Ashby at the University of Illinois and I became a student of his. After a one year course on cybernetics, using that very introduction I bought in Oxford as one of the texts, I organized a Cybernetics Club of his former students to digest all the ideas he had given us. He was a member of my dissertation committee. Later I wove cybernetic conceptions into my conceptions of communication, conversation, and social organizations.[3] So he became an important figure in my life. When he retired, he named me as a possible successor of his professorship, but the University of Illinois didn't want to continue his line of interdisciplinary inquiry. Wittgenstein's theory of language, especially his later work, gave me the philosophical grounding of thinking of language performatively, less representationally. It opened me the doors to question ontology, the existence of a language-independent realities and led me to social constructions and discourse conceptions. So both of these books had totally different and unanticipated impacts on me.

But in the fall I was back in Ulm writing my diploma thesis, then I stayed for one more year to work in the Institute for Visual Perception, and during that time, I followed up on my application to study in the United States, was interviewed at the American Consulate in Stuttgart which offered me a totally new, the Ford Foundation International Fellowship, which financed two years of studying in the United States and a travel grant from Fulbright.

In retrospect, as I mentioned earlier, this move was not entirely unexpected. My parents had already primed us as kids with the stories of their adventures 30 some years earlier. My brother met me in New York when I arrived by ship. He had arrived a year earlier as a political scientist to do research. We both relived some of the adventures of our parents and add our own.

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[3] Klaus Krippendorff, "My Scholarly Life in Cybernetics," *World Futures: The Journal of New Paradigm Research* 75, no. 1–2 (2019): 69–91. https://doi.org/10.1080/02604027.2019.1568803.

Q: Well, that's a perfect place to conclude this first session, so thank you very much Klaus.

KRIPPENDORFF: Thank you for asking questions and giving me the opportunity.

## END OF SESSION ONE

# Transcript (modified) of interview conducted January 18, 2017 with KLAUS KRIPPENDORFF (session two)

Philadelphia, PA

Interviewed by Jefferson Pooley

***Note:*** *This modified transcript was significantly edited by Klaus Krippendorff. The original transcript, synced to the video interview, may be reviewed at [https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field](https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field).*

Q: This is day two of an oral history interview of Klaus Krippendorff, conducted by Jefferson Pooley in Dr. Krippendorff's home in Philadelphia. The interview is part of the Oral History Project of the Annenberg Library Archives of the Annenberg School for Communication at the University of Pennsylvania. The date is January 18th, 2017. So, thanks for joining us for the second session, Klaus, and the question I had for you and where we left off in the first session was about your trip to the United States: It was 1961 and I was curious about your motivation for coming to the United States and what enabled it—the fellowships you had and the trip itself.

KRIPPENDORFF: Well, I think I mentioned last time that my parents, when they were young, they were in the United States. When I grew up I always heard about the wonders of the United States, Niagara Falls and Yellowstone [National] Park, and the adventures, particularly of my mother, who was a very enterprising young woman, and just coming to the United States to see what is going on. And so during all my childhood the United States was always in the conversation. But that was not the only thing. When I was in Ulm [Hochschule für Gestaltung, Ulm, Germany]—this was an avant-garde school, as I mentioned, and by avant-garde I meant they had, you know, cutting-edge scholars from all over the world coming there. They were pleased, actually, to give lectures, and we were exposed to ideas that all came from the United States: information theory, cybernetics, ergonomics, cultural anthropology. And there was also, I have to say, a lot of professors that were re-immigrants. That means they spend the Nazi period outside Germany, had taught there, and came back, some only recently, to a democratic country. And so it was an environment in which just amazing ideas were populating the conversations among faculty and students.

I would say now that this was the atmosphere I grew up intellectually. I had been an engineer—but my experiences in Ulm are what opened me up to a broader worldview. Most of the ideas we students were exposed to in Ulm came from the United States, so I decided that I had to dig deeper into them. That certainly was an important motivation for me. I didn't come to get a degree. I wanted just to grow my understanding of the seeds planted in Ulm, getting to the core of the ideas I mentioned—that's why I came. What we had learned was, I would say, second-hand. I wanted to learn from the originators of these ideas, and they were all in the United States. So, I had applied to the American Consulate in Stuttgart for a Fulbright fellowship, from where I was also awarded, without knowing what it meant, a Ford Foundation International Fellowship, which was a new fellowship that had just been established.

The Fulbright fellowship was a travel grant. In September 1961, it brought me to the United States by a passenger ship called the *Berlin*. The *Berlin* turned out to have been the first ship that the well-known shipping company, the Norddeutscher Lloyd, acquired after the war. It was originally owned by a Swedish king, but during the Second World War chartered by the United States as a repatriation ship to pick up people from Germany and Japan to become U.S. citizens. It was built in 1924, old fashioned with few luxuries. There were a lot of students on board who didn't expect anything else. Crossing the Atlantic by ship was very different then. Nowadays, nobody would take a ship, just for the trip. I suppose this was the cheapest way to get students to the United States. So we spent seven days on the sea before arriving in New York harbor. Among the students I bonded with was one, Hans Haacke, who was an artist. He showed us some of the work he had done, it kind of gelled with my thinking in Ulm. I stayed in touch with him. He came to Philadelphia actually, to Temple [University]—the art school—while I came to Princeton [University]. He became a famous socially critical New York artist.

I should also explain why I became a student at Princeton University. There was and still is an Institute for International Education in New York. It handled all international fellowship programs and had to connect students with universities that would be of benefit for both. I came from a new avant-garde school of design that was largely unknown and I wanted to deepen my understanding of certain growing ideas. I had just received a prize from Kultur Kreis des Bundesverbandes der Deutschen Industrie for the best diploma work—the design of the motor grader—and I came with several excellent recommendations that reiterated the abstract ideas I wanted to pursue. Among them was one recommendation by the American professor with whom I worked as research assistant in his Institute for Visual Communication at Ulm. Mervyn W. "Joe" Perrine wrote a glowing recommendation.

I can't blame the Institute for International Education in New York for assigning me to study at Princeton University. This agency had to find places where fellowship recipients would fit. I did not fit into any of the traditional disciplines. Design departments were far removed from addressing any of the concepts I wanted to explore and there seemed to be no university in the United States able to accommodate what I wanted to know more of. The substance of Perrine's recommendation may not have mattered much to that Institute, but written by an American who had just graduated as a social psychologist from Princeton University may have guided its decision to assign my fellowship to Princeton's Department of Psychology.

But in Princeton—first of all, my English was very, very bad, and I remember starting to take notes in German from the English lectures, which I soon abandoned because that was not the way to gain fluency in English or to aid subsequent discussions. I made a good decision not to room with German or other foreign students but lived with three American students. They were the ones who introduced me to shopping, television, college sports, and everyday student life.

But the psychology department at Princeton had since been taken over by rat psychologists. Harold Gulliksen was famous for having developed and taught his mathematical theory of rats learning—nothing could have been more different from my interest. There was only one professor left who was interest in social psychology, Harold M. Schroder. I developed good relations with him and his assistants. He was interested in what I had to say but he was the only one and the overwhelming number of courses available were depressingly irrelevant to me. My breaking point came when the *Princetonian*, the university's newspaper, wanted to publish an article to report on the first time that a Princeton student received a Ford Foundation Scholarship. A picture was taken of me in front of rat cages and published. Seeing that, I couldn't help making up a caption of that photograph: K.K. is a German psychologist coming to Princeton to study American rats. This did it for me, and I knew I had to get out of this place. The prestige of Princeton University meant nothing to me.

There was a well-known Princeton professor—you know of him probably—Hadley Cantril. During his tenure he expanded the field of social psychology to embrace public opinion research, was known for his research on how the United States public came to accept going to war, and the real life responses to that very realistic radio show. He had chaired of the department, was now retired, but in Turkey for much of my time at Princeton. When he returned in December I managed to see him in is home and shared my frustrations. It didn't take long for him to say, Klaus, you're in the wrong place. I knew that of course. But then he gave me a list of several names of scholars he suggested to talk to until I would find a better match. Among the names were George Miller from MIT, Jerome Bruner at Harvard, Anatol Rapoport from the University of Michigan, and actually also George Gerbner of the University of Illinois, Urbana.

So, in December, after classes were over, I took my Volkswagen and I drove through the East Coast and Midwest to find a place. I had an excellent conversation with George Miller, but it didn't really look like that I would find that much else there. He was very interested in my design background and we gelled in a way, but around topics I had left behind in Ulm.

Jerome Bruner was fascinating, but he didn't really see a place for me. I went to Michigan State University before I visited the University of Michigan—no, no, after I went to the University of Michigan, to see Anatol Rapoport but he was not there. I talked to an assistant of his and he basically discouraged me from going there because the environment would not be conducive to what I wanted to learn.He confirmed that Anatol Rapoport was amazingly creative with larger than life perspectives, but the rest of the academic environment wasn't that supportive. At Michigan State University, I knew of a professor of psychology, Hans Toch, who was the advisor of two former graduates from Ulm who received their PhD from that university. Although I thought one of these dissertations was extremely narrow and unimaginative, not what I would want to pursue, I decided to talk to him and he said, from what you're telling me you should talk to someone at the Department of Communication.

There I met David Berlo in his office. Little did I know of his position in the field and how he approached communication. He did what would nowadays be difficult to imagine. He organized a party at his house that evening—just for me. There, I met Malcom MacLean, and a few faculty members and assistants, maybe eight or ten people. We had lively conversations. I wanted to know what their department was like, they questioned me about Ulm. I didn't know that this was in fact an interview about my conception of communication—which, coming from a design school, was rather rudimentary. I must have made a promising impression. The party ended with Berlo telling me that I had an assistantship at the department. I had not filled out an application, did not submit a resume. It was all verbal. This informality appealed to me. However, our conversations turned mainly around psychological issues, and I told him that I didn't really want to explore communication from an individualist perspective, as important as psychology is, but I did not want to abandon social and technological dimensions. For that, David Belo said, You'll have to go to Urbana [University of Illinois at Urbana-Champaign].

I already had the name of George Gerbner on Hadley Cantril's list and drove to Urbana, Illinois. But I don't recall why, I first went to the design department of the university. There I met the chair of the department. He was an influential design educator, and questioned me about Ulm, which was already internationally famous. He had never met anyone from Ulm and was very pleased to talk with me. When I mentioned my interest in cybernetics, he searched and found among his papers one written by a Heinz von Foerster. It featured the image of an intriguing piece of art which probably was more interesting to him than what the paper said, but told me that von Foerster headed an institute for cybernetics [the Biological Computer Laboratory].

I didn't know Heinz von Foerster but visited him in his office. Besides greeting me with his Viennese charm, he told me that W. Ross Ashby taught a graduate course on cybernetics. I think I mentioned last time that I had bought his *An Introduction to Cybernetics* when I interned in Oxford, England. I was elated. Then I talked to Dallas Smythe who was, I believe, the chair of the Institute of Communication Research [ICR]. This PhD program was interdisciplinary, leaving me many options to branch out, which I hadn't seen elsewhere. I remember our relaxed conversation with Smythe's legs on the table, which made clear to me that this was the right place for me to be both emotionally and intellectually. I also met George Gerbner but only briefly. The openness to interdisciplinary inquiries at Urbana made my decision to join ICR.

This was in December 1961. I packed up my belongings in Princeton, said goodbye to my roommates, with one I am still in contact with, and started attending courses in Urbana. However, before I could, I had to register. The Ulm school was not known to the administration and coming from a prestigious private university to a state university raised suspicion. This social stratification of U.S. universities was totally alien to me. They thought I was faking my identity. So, I could register only as an undergraduate. I didn't mind this categorization, but it had implications about the courses I could take. George Gerbner, to whom I turned with dismay, assured me that I could take graduate courses and if I would get decent grades this misclassification could be fixed.

So, after one semester, I was a graduate student. Before that moment, I acquired all the credentials of an undergraduate—invitation to fraternities, to honor societies, and so on—I had no use for that. So I think becoming a student at the University of Illinois, Urbana, was a very good decision. The most attractive feature of the Institute of Communications Research was its true interdisciplinarity. None of the faculty had a communication degree. Everyone had appointments in other departments. Incidentally, Percy Tannenbaum was one of the earliest ICR graduates but was not on its faculty. The diversity of faculty teaching communication courses provided students many choices. It consisted of scholars from sociology, linguistics, anthropology, economics, education, you name it. Dallas Smythe was—I don't know for sure his background—a political economist, critical of the mass media and telephone monopolies. He was obsessed with the FCC [Federal Communications Commission]—the regulation. Although I was less interested in his domain of expertise but fully respected his research.

Howard Maclay became my academic advisor. Maclay was a linguist, anthropological linguist, with very broad interests. And when I wanted to take a course with Ashby—for which I had to wait for half a year because it was a one-year course and started always in fall—he was very supportive of that. In fact after seeing how much I took away from that course, he encouraged others to take it as well. He recognized more than others did that cybernetics is basically an approach to communication. The only requirement of communication students was to take a proseminar course for one year, divided into two parts, called micro and macro, micro being concerned with psychology of communication and linguistics, macro with public opinion, the economics of mass communication and the like, plus a one semester statistics course. I enjoyed them all. I tried to wean myself out of statistics because I had some minor exposure to statistics while studying in Germany at the design school but realized that it was not enough to match the level of teaching at the university. I didn't take the course, studied on my own and passed the final examination.

So Urbana was, as I said, very free: My cybernetics course with Ross Ashby was one of two most influential courses of study. There I learned about information theory of which Ulm had given me only a skeleton. I learned to appreciate the mathematics of complex systems, especially including circular causal connections that rendered them self-organizing. Ashby also taught us that the role of cyberneticians cannot be separated from the systems of their concerns.

The other area which I attended with increasing fascination was, actually, anthropology, and I took a course, several courses, two courses, with Joseph B. Casagrande. He was a Whorfian scholar and taught linguistic anthropology. Benjamin Lee Whorf was a major scholar who explored the relationship of language, culture, acting and being in the world. For me he put meat to the mere recognition that perception is influenced by one's social background. Casagrande showed us that studying language without the context of its use misses the most important reason for using it. Yes, there are linguistic rules, like grammars, but they are largely shaped by culture. I had a memorable experience in one of his courses. Casagrande gave us an assignment to look at kinship terms in different cultures, and so everyone was assigned a different culture, and readings we were asked to search for their kinship terms. The question was, What counts as kin? And as you probably know India had so many more elaborate kinships terms than Indo-European languages have, defining relatives in relation to speakers in for us unimaginable detail. Almost all cultures have their ways of categorizing relatives. The culture I was assigned to explore—I've forgotten exactly which it was and could probably check it out—used terms very similar to the German kinship system. I expected something very different and was most surprised until I realized that the anthropologist who reported his study was German. It made me again aware of the influence of where someone came from, coming back to social perception, applicable also to trained anthropologists. Scientists who claim to publish their findings, even if they try hard to be objective, can rarely completely escape from where they come from, their own history of conceptualizations. My report to the class was discussed at length, not its kinship terms but our own vulnerabilities, the often-unjustified claims of offering evidence that other may see quite differently. It set me on a path articulated much later that data are made not found.

Anyway, so this was one of many takeaways from my linguistic anthropology courses. I also took a course with Jerry Fodor, who was a visiting scholar, and whose course my advisor wanted me to take. He was a hard-nosed Chomskyan linguist. I learned a lot about how linguists see their world, but not too much else. I took courses in social psychology, which focused on group dynamics but didn't really address the communication structures underlying these phenomena. I remember writing a paper that my professor found lacking empirical tests. After I explained that communications are quantifiable as well did he see its empirical grounding but of a different kind. I also took a course with George Gerbner. From my perspective, Gerbner's course was too narrowly Marxist for me. We had to apply certain key phrases like formulas that would explain everything, for example, that mass media content is the product of industrial processes of mass production—true enough—but all of his arguments ended up blaming capitalist interests underlying what we believe to be true. He certainly had a point. He argued that watching television and reading newspapers makes everyone an arm of industrial interests, but he was unwilling to consider that he could also be influenced by what he viewed and read.

Herbert Schiller was at the Institute at the same time. But I didn't take a course with him because he was even more of a Marxist. As I mentioned to you earlier, you know—as a child in East Germany, I had first-hand experiences with the results of Marxism. Later on, in West Germany, we studied Marxism and its socio-economic consequences, I knew something about its reality, its theoretical appeal but also its lack of self-reflection—that it is the belief in a theory and enacting it makes it real. While I think there are a lot of good ideas in Marxism that can apply to communication studies, like looking below the surface of what is said, to the materiality of what is going on—I would now say the technological infrastructures—which makes what is said disseminable, sharable, and accessible in everyday life, but its meanings is not as determinable. Understanding their impact could be achieved by ethnomethodological approaches, which I learned to appreciate from my involvement in anthropology. Nevertheless, I maintained good relationships with Herbert Schiller, I worked as an assistant to Gerbner, and so on. In fact, in 1967, when I got married in Urbana, Herbert Schiller was at the wedding, Howard Maclay too, and lots of other people I interacted with at ICR. I was a recognized part of the Institute.

Q: Well, can I even follow up just about Illinois, and the ICR [Institute for Communications Research]—just two aspects of it? I mean, you mentioned this proseminar and you mentioned there was a kind of cluster of Marxists and there was a grouping of more psycholinguistic behavioral scientists like Charles Osgood and there was, maybe, a motley group of folks that James Carey would eventually, around that time, actually, start to call "cultural studies." And I thought if you could reflect on what the Institute was like given those rough divisions, and then if you could also just elaborate more on Ashby and the experience in the class.

KRIPPENDORFF: Well, let me start with W. Ross Ashby. Ashby was British. He had never taught, really. He was a psychologist—a psychiatrist by training and a researcher close to the earlier computer developments. His main focus was to understand the human brain, all brains, and mechanical systems that work brain-like and could serve as a model of it. This brought him close to cybernetics. In 1956 he published his introduction to cybernetics eight years after Norbert Wiener introduced the term. Ashby told us his motivation that probably shaped his whole career. He felt Wiener's cybernetics was too abstract and his mathematics—using three or more summation or integral signs in his mathematical expressions, limited cybernetics to limited data structures and to specialized experts. He was convinced that, in order for cybernetics to make sense to those who could benefit from them, it had to be boiled down to simpler and more general expressions and concepts. And that's what he did. He used not integral mathematics but set theory. He looked at the kind of transformations one could state simply. His book was certainly abstract but related these abstractions to what one could more easily relate to. For me, his interest in understanding the brain easily translated into conception of social systems, and that is what I was interested in and that is what I took away from it.

Information theory was one topic that grew out of the need to account for cybernetic feedback loops. After my superficial exposure to it in Ulm, it was an eye-opening experience. In fact, later on I taught courses and wrote much on information theory, but not from a technical point of view—rather than from a point of view of translating its terms into human communication—without loosening its precision. The other major concept I took away from Ashby was I clearer understanding of the notion of systems, that things are not just entities by themselves, but they are the result of connections, typically circular causal connection.

While Ashby developed set theoretical expressions that allowed us to formally examine these connections. He built physical models to demonstrate these abstractions. For example, to demonstrate what he called ultra-stable systems, systems that do not merely adapt to changes in the environment, like the size of an eye's pupil to changing strengths of light, but can change their behavior as soon as their comfort zone is threatened, he built a model of two such devices. He coupled them to interact, making their behaviors visible to us, until they reached a dynamic equilibrium. One could say that ultra-stable systems adapted their adaptations to ever changing environments. This was communication alright. When I introduced these ideas in ICR's proseminar, other students were enthused about cybernetics and, with the already mentioned encouragement by Howard Maclay, took Ashby's course. After that course was over I started a cybernetics club in which we tried to elaborate its rich ideas, each in our own areas of concerns. When I left Urbana, it was still alive.

You mentioned Charles Osgood. When I joined the ICR, he had become head of it. I didn't take a course with him. He presented himself as a psycholinguist who pursued a behaviorist conception of communication. His innovation was to distinguish the process of decoding messages from outside into internal cognitive representations—I would call that interpretation without the normative flavor of "decoding" which always implies a code—from processes of encoding these cognitive representations into messages given off to others. This distinction allowed him to separate the meanings of linguistic expressions from responses to their understanding. While the idea of encoding and decoding of messages clearly came from Shannon's information theory, in all fairness, Osgood had a wealth of other behaviorist ideas, many of which were turned into dissertations by his students. His conceptions lend themselves to theories of attitude change and gave rise to many tests, including a contingency analysis for content analyses.

What else you wanted me to say about ICR? Well, you mentioned tensions: I don't think they were that evident in our informal deliberations. Theodore Peterson, who with Fred Siebert and Wilbur Schramm had written their widely read *Four Theories of the Press*, was the dean of the College of Communication of which ICR was its PhD program. He was preoccupied with administrative duties and was not teaching. Wilbur Schramm, who headed the University of Illinois Press and founded the Institute, had left for Hawaii. And Siebert participated in the proseminar. The book developed four conceptions of the press diversely pursued in the world: Authoritarian, Libertarian, Socially Responsive, and Soviet Communist. I believe that Shannon's mathematical theory of communication and their book distinguished the early identity of ICR from journalism schools, which taught journalists to write and research their facts with little attention to communication. Through the distinction of micro and macro parts of the proseminar, students were pushed either in the direction of psychology or the social sciences. This division was weakened by linguistic and social psychology courses which built bridges between the two areas. The only serious tensions were between students from the advertising department who had to study at ICR for their PhD and whose narrow commercial orientation was largely detested by the majority of the other students, pursuing larger conception. Naturally, Marxist and other critiques were of no use to them. James Carey was two years senior to me. He worked closely with Siebert and Peterson. His cultural studies approach emerged later. Although Marxism and behaviorism are almost incompatible, we could still talk across their lines. Dallas Smythe was obsessed, actually, with FCC regulations and the monopoly of AT&T. He introduced a political economy approach to communication studies. While I never took a course from him, he had organized the macro part of the proseminar during which I learned what I wanted to know. In any case, having grown up in Germany the big telephone and television monopolies did not mean that much to me personally.

From my first encounter with Smythe, I maintained close relations with him. In the summer of 1962, I wanted to see more of the United States than the flat, sparsely populated agricultural surroundings of Urbana. So my brother and I drove across the country. I ended up taking a summer course on collective behavior at the University of Southern California. In Los Angeles, I visited Dallas Smythe in his home where he spent his summers. In Urbana, I also met Peter Berger who taught a course in sociology at that time. I still regret that his course conflicted with other obligation. Little did I know how important his and Thomas Luckmann's book on the social construction of reality would become for me later.

A minor point to the climate of student-faculty relations. The College of Communication, including ICR, was located in Gregory Hall. Faculty had offices there, students populated the library when not in classes or at home. Across the street there was a cafeteria in the basement of the Christian Association. When faculty were not in their office, especially Howard Maclay, one could almost always find them at the cafeteria and join conversations with them. Howard Maclay, my academic advisor, often invited students to his house when an important visitor came by or at other occasions. I enjoyed the informal nature of the Institute.

I want to say something about my dissertation. During my preliminary examination, which was actually unlike at the Annenberg School [for Communication, at the University of Pennsylvania] which included the approval of a proposed dissertation, I was asked what I wanted to work on. I remember that I had three ideas but now forgot one. One was to study how communication structures can undermine structures of authority. I thought of the historical example of how the Spanish captured the Incas in Mexico. The Incas had no conceptions of the Spanish intents, welcoming them as gods. They had built roads, all of which led to the center, largely to transport fish and other goods. The very small Spanish invaders were led directly to the center of Inca power with no resistance, where they dethroned the authority that was and by the way discovered gold. I abandoned that project for two reasons. One was that I didn't speak Spanish and would have had to go to Mexico and be able to work through Spanish documents, but more importantly, this historical incident had little resemblance to how western authoritarian regimes can be undermined by processes of communication.

The other idea I presented to my committee was to work on a research technique called content analysis. I was introduced to its practices by two people. One was Shel Feldman, an assistant professor at ICR who later on came to the Annenberg School as well. He was a psychologist and, when my two-year Ford Foundation Fellowship was done, I became a research assistant at ICR. As such, I was available to everyone who had work to do, and I found myself coding test results for Feldman. As I was following his coding instructions, I saw all kinds of problems in meanings that seemed to escape the rules I was asked to follow. I pointed this out to Feldman. He listened but ended up telling me, Don't worry, just code, just assign given categories to what is there. I did this, of course, but kept thinking, What are we describing there? Later, I worked for Gerbner, coding popular magazine covers. This task seemed less artificial to me, but I couldn't help thinking that the process of coding was not very developed.

Searching for conceptual clarification, I found Bernard Berelson's 1952 book on content analysis, which was the only textbook there was, and it was not very satisfactory. I realized that communication research has adopted methods from various disciplines and in my opinion thereby surrendered to their epistemological assumptions—except for two analytical conceptions it originated: analyzing networks of communication and the contents of messages transmitted through channels of communication, content analysis. Both seemed central to my understanding of the field but in very rudimentary stages of their development. I wanted to contribute to communication studies and decided to write about content analysis. One thing I faced, and I actually tell my students nowadays, was a very diverse committee—the cybernetician Ashby, the linguistic anthropologist Casagrande, the social psychologist Stein, and the linguist Howard Maclay, who chaired the committee. This was not unusual for the interdisciplinary ICR, but each had rather different academic interests and actually none really had any knowledge in content analysis. Maybe the social psychologist who was measuring interactions in small groups. I ended up writing different chapters of my dissertation for the different committee members. For Ashby, I developed a qualitative information theory for content analysis which still informs me in many ways. I presumed that if one wants to understand certain phenomena—social phenomena—then one has to be sure that the information in these phenomena is correctly represented in analyzable data and furthermore informs the conclusion of a research project. The whole process from reading texts to reporting findings should be seen as a process of communicating information. And so, one ought to know the information one is analyzing and the information that is forced out by the nature of given coding instructions or lost due to extraneous circumstances, i.e., noise. In my conceptions at that time, these things were problematic in content analysis.

As I mentioned, in the chapter written largely for Ashby, I developed, one can say, a qualitative information theory, one that is not based on probabilities but on issues of coding. For addressing issues of the readers of texts, I relied on anthropological insights and wrote a chapter from this perspective. In response to my linguistic exposure I slowly realized something that formed much of my later work, that the notion of message content was a wrong metaphor. Messages have—contain nothing. When you make a Xerox copy of a document you copy the character strings, not its content. It is an inappropriate metaphor whose entailment leads to the misconception that authors put the very meanings into texts that readers have to remove from it. This chimed with information theory's coding and decoding as two complementary uses of a code but was so alien to anthropological notions, to ethnographical conceptions. I argued that content analysts read texts unlike their authors who composed them and unlike their readers who have all kinds of reasons to interpret what they read in their own terms. The physicality of the character strings mediate between people but what they mean is not necessarily shared. So these insights came from my anthropological or ethnographical leanings. My dissertation consisted of different chapters for different members of my committee bringing them together in my growing conception of content analysis. I continued that integration.

I should like to just mention in passing: Of course I wrote the book on content analysis[4]—in fact three editions of it. Complying with their publisher's wishes, it had to be called and always goes by the name of content analysis. But already in the first conceptual chapter, I made my objections to the simplistic but common use of the content metaphor clear and warned readers not to be sucked into this misleading framing of their analyses. While I yielded to the publisher's naming, I'm always warning my students to be careful not to get sucked into the widespread use of the conceptually misleading content metaphor. Its use in communication research goes back to Berelson and Paul Lazarsfeld's framing of the field.

I later discovered that Berelson's book, published in 1952, was actually written by Lazarsfeld and Berelson in 1948. They had a fall-out and divided the intellectual ownership of their collaborations and *Content Analysis* became Berelson's text. I found the original and it's virtually identical to the published version. It's in Annenberg's library. For Berelson and Lazarsfeld, content analysis was defined as the analysis of content. In this, their definition of content analysis, content was assumed obvious, generally understood, and not worth questioning. I suggest they were unaware of the role of metaphors in their own articulations, much less with what they proposed to analyze. So this was the kind of thing that I was against and my dissertation took a different road.

I should also mention that I felt very comfortable with my committee at the defense of my dissertation. I felt secure because I was sure to know far more than anyone else about my subject matter. I could answer all the questions I was asked from different angles.

Q: If I can even interject there just to take us back a couple of years and we'll get back to content analysis, but I was wondering, three years before you defended your dissertation you, I think, went with George Gerbner to the Annenberg School when you were still a doctoral student. And so how did that happen? What was the context of the invitation from Gerbner?

KRIPPENDORFF: That was interesting. Actually, before I answer your question, I want to interject something that I didn't mention earlier. When I transferred from Princeton to Urbana, I had no intention to get a PhD. I was only interested in expanding the ideas planted in Ulm. But at the ICR, I was told you can't just take courses here. You have to formally enroll in a program and work towards a PhD. And I said, OK. Frankly, when I came to Urbana, I still entertained the idea of going back to Germany, maybe as a designer or teaching design with a communication perspective somewhere. This certainly naïve intention was suddenly shuttered by the demand that I had to work towards a PhD.

Now you asked me about how I came to the Annenberg School. I received credit for the one semester I spent at Princeton University and, after taking the required courses at the University of Illinois, Urbana, passing the preliminary examination and settling on my dissertation topic after two and a half years in Urbana, the University of Pennsylvania looked for a new dean of its Annenberg School. It invited Charles Osgood. Osgood was interviewed and came back and let

[4] Klaus Krippendorff, *Content Analysis: An Introduction to Its Methodology* (Beverly Hills, CA: Sage Publications, 1980).

everyone know he didn't want to take that position. Shel Feldman, who was a young assistant professor at ICR and as a psychologist close to Osgood, wanted to be interviewed for a faculty position at Annenberg. I worked for him at that time and he asked me whether I would want to join him to explore the Annenberg School for possible openings. We drove by car and were interviewed by the temporary dean of the school, Robert E. Spiller from the English department. Feldman was considered a good candidate for a faculty position at the school. I, merely an ABD [All But Dissertation] was told of the possibility of becoming a research assistant, all decisions subject to the approval of the future dean of the school. After Osgood declined the job offer, George Gerbner expressed interest in that position, was invited, made a good impression, and was appointed dean of the Annenberg School entirely independent of us.

Of course, George Gerbner knew me wellI had taken his course and worked for him for a while. When he was appointed, he was delighted that I would join his effort to improving the academic standing of the school, which was the University of Pennsylvania's motivation for hiring him. Feldman was offered an assistant professorship at the school. Gerbner invited a fellow student of mine, one year ahead of me at ICR, Wendell Shackleford, to join him in Philadelphia. His wife, Linda Shackleford, had worked in the ICR office and continued in the Annenberg office. I became a research assistant. These were the five people from Urbana entering that new environment in the summer of 1964.

I had not written my dissertation but faced another problem. My visa required me to go back to Germany after getting my PhD, at least for two years. Although I finished writing it while in Philadelphia, I delayed its formal submission, revised and polished it a bit during that time, but defended it only in 1967, which was the last date possible for my defense.[5] After that I had to decide between going back to Germany or remaining part of building up the school. I felt the latter more challenging. George Gerbner wrote a letter, I still have a copy of it somewhere, declaring me indispensable for the school, which was of course a strategic exaggeration, claiming that the Annenberg curriculum would suffer, and I should be exempt from the visa obligation [laughs]. The university managed to convince our Pennsylvania Senator Hugh Scott to introduce a bill in Congress for exempting me of this requirement. In the end, he didn't have to do it. Independently, a law was passed that one could apply to remain in United States with a green card, which I already had. So could stay on.

At the Annenberg School, Shackelford and I worked in the proseminar that Gerbner orchestrated and I started teaching two courses, one in *Theory and Analysis of Message Content*, one on *Models of Communication*—an Ashby-inspired course. At that time, the proseminar was a very different entity. Gerbner modeled it after the Urbana practice. But he was in charge of all conceptualizations, had Wendell Shackelford and me taking over occasional presentations, and invited other faculty members in support. The University of Pennsylvania wanted the Annenberg School to shift from a media-art kind of program to a more academic one. Gerbner took this mission very seriously, albeit in his terms, starting with the proseminar,

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[5] Klaus Krippendorff, "An Examination of Content Analysis: A Proposal for a General Framework and an Information Calculus for Message Analytic Situations" (PhD diss., University of Illinois, 1967), http://repository.upenn.edu/asc_papers/250.

obligatory for all students. That's the reason why he was very selective of who would be presenting what in the proseminar.

I should also mention—you know, I previously related to you my background as a designer. And when we came to Penn, its printing office had created a brochure of the Annenberg School's program of study. I forgot how many pages, very few, a greenish cover, and miserably printed. I still have a copy of it. I suggested that we needed something more informative and more appealing to the kind of students we wanted to attract. And so, Gerbner challenged me to propose something better and I did. I worked with Mary Ellen Mark, a photographer who had graduated from the Annenberg School just before we arrived. She already had a master's degree in fine arts but came to Annenberg for the one year course of study of the media of communication. She told me that she never took a picture before she became an Annenberg student, had no interest in photography. However, after she was asked to take photographs in the school's graphic communication lab, she was never seen without a camera. She discovered her medium and was an amazing photographer. I worked with her on our first bulletin. It was full of great action-oriented pictures, showing students utilizing our facilities and faculty gatherings as opposed to portraits of the most important professors. Mark went on traveling the world, I designed all our bulletins for the following ten years. I even was invited to design the bulletin for the new decision science department at the university. And I'd like to show them to you.

Q: What about, in keeping with the bulletin and your arrival there, the proseminar. Do you have other recollections of the Annenberg School in 1964–65—those very first couple of years that you were there—Gerbner's leadership—that kind of thing?

KRIPPENDORFF: Well, as I said, Annenberg initially was a media arts program. We had several studios dedicated to different media: broadcasting, which included the television and radio labs, documentary film, graphic communication, and the writing lab, to which Feldman added a psychological research lab and Rolf Meyersohn one for sociological methods. I had never been in a television studio and so I was fascinated, went there frequently, and enjoyed interacting with students and teachers. It was the physically biggest studio, located in the center of the Annenberg building and occupying one and a half floors. George Dessart was the head of the broadcasting lab, teaching television, aided by Lewis Barlow and Albert Rose. He came from New York, where he was a television producer, a very energetic teacher who was highly regarded by Annenberg students. He essentially taught students how to produce television shows. The radio studio was located, well—the Annenberg School has changed so much—in the space of the current student lounge. It had a one-way mirror, separating the control room from where people where taped, later used for monitoring psychological experiments. Sol Worth taught the documentary film lab and presented the short movies of his students to all Annenberg folks on various occasions. When I came, Lou Glassman, a magazine editor from New York, was the head of the graphics communication lab. He worked with Sam Maitin, a Philadelphia artist, who taught the lab after Glassman left. Maitin is the artist who created the big mural in the Annenberg lobby, and was responsible for many public arts projects in Philadelphia. We shared an office, G22, which has now become the passage from the ground floor to the atrium. Sam remained one of my best friends in Philadelphia. The more spacious offices on the second and third floors were given to senior professors. I preferred to have my office on the ground floor where the action was.

Although my contributions to the proseminar where more conceptual, maybe because I was a student as well, I bonded with many of the then current class of students. Just a few months ago, these very alumni organized a reunion, the first of its kind, celebrating the fiftieth anniversary of their graduation. I was the only professor they still recognized, then an ABD research assistant. They shared their amazing stories not only about their career paths but also talked about their hidden complaints about their educational experiences as Annenberg students. I was not aware of the fact that the Annenberg School had a higher percent of women students than other schools at Penn, that working to get a graduate degree in communication was discouraged even by a dean for women students who tried to direct them to nursing or education, and the advice. In one example of their parents, predicting that they would never find a husband! At Annenberg they felt encouraged to excel. Male students who came with expectations of learning practical skills in a medium had often difficulties seeing why they should bother with the concepts of communication presented in the proseminar. I was not aware of major grievances against the administration requiring two years of studying towards a master's degree instead of one. However, all alumni who came to the reunion affirmed what they were inspired by.

One has to realize that the concept of communication was not that well established and not a discipline either. After receiving their degrees, Annenberg graduates had difficulties explaining their acquired competencies. I learned from this fiftieth reunion that, especially from some women graduates who mentioned their graduate degrees, were asked whether they were superior typists, and a male graduate who applied for a position was asked if his education would qualify him to manage the mailroom. These graduates were an amazingly courageous group. By and large they made it in the world against many odds. The Annenberg School gave them the conceptual tools they didn't know they needed and the courage to continue. The Annenberg School thrived as well, not without identity struggles. At some point, the sociology department questioned whether communication deserves an existence outside of sociology, and social psychologists claimed they studied communication as well. I was the first professor who had a communication degree at Annenberg, and for the longest time I was the only one. And so this was the beginning of it. I don't know if you want me to talk about the ICA [International Communication Association]? Or maybe that comes another time, but this was also at that time.

Q: That is [the ICA] was being named as the "ICA," coming from its previous existence as a piece—an appendage—out of the NCA [National Communication Association], which was the SCA [Speech Communication Association] at the time, right? So, well, I am curious about that, but since your deep involvement in ICA came a little later, I was curious if you would talk about that 1967 conference–this major Annenberg conference that had funding from the NSF [National Science Foundation], on content analysis—where the idea came from, your role in it?

You were the co-editor of the book that was published out of it in 1969[6]—its relationship to your dissertation? Just that ’66 to ’69 period in content analysis.

KRIPPENDORFF: Well, as I mentioned, my dissertation was on content analysis, and one of the important publications besides Berelson’s 1952 book was actually the result of a conference that took place in 1955 in Urbana. There was Charles Osgood, there was Ithiel de Sola Pool, Alexander George, and lots of scholars. The conference was taped and subsequently analyzed. participants presented papers, later edited by de Sola Pool into a book. And to me this conference advanced content analysis beyond Berelson’s book. It covered several disciplines and because I learned a lot from their differences, I approached Gerbner and suggested that we should do something like that. He asked me for a list of scholars I would like to see invited. I gave him a list and in the spring of 1966 an interdisciplinary conference at the Annenberg School followed. My role was chairing the introductory session about theories and analytical constructs to which I contributed a widely cited paper on three prototypical models of messages embedded in content analyses.

A lot of creative scholars from all kinds of disciplines were invited: psychology, political sciences, sociology, anthropology, journalism, literature, music, linguistics, communication research, and especially computer sciences. Harvard’s Philip J. Stone, for example, the inventor of a computer software, somewhat over-ambitiously called the General Inquirer, was one outstanding scholar. I remember, the first time I met him, he impressed us with a book-like box he opened up to show us two computer tapes telling us this is the whole General Inquirer. His software applied specialized dictionaries to machine-readable text and coded it into analyzable categories. Its algorithm involved a lot of understanding of natural language, had stimulated much research, and occupied the largest part of the conference. Ole R. Holsti, who was in the process of publishing his *Content Analysis* [text], was there. The conference attracted anthropologists, psychologists, who presented quantitative and qualitative papers.

Edwin Shneidman was very fascinating to me. He was the head of a Center for the Study of Suicide Prevention in California, and one of his problems was to identify suicide notes that psychologists or social workers would have to take seriously as opposed to threats written for other purposes. In the community of psychologists dealing with similar issues there emerged an interesting competition. Osgood was convinced that the motivations given in such suicide letters would identify their genuineness. He had a quantitative theory that could be applied. As an experimental psycholinguist, it was natural for him to create a control group of suicide notes written by his assistants which the theory should distinguish from the genuine ones. The test was pretty good but not perfect. Stone wanted to apply his General Inquirer to Osgood’s two sets of suicide notes and collaborated with Earl B. Hunt, who had written software to select features of data that would differentiate between two kinds of dependent variables. The combination of these two software was more successful than Osgood’s test. The, in my opinion, false conclusion drawn from this result was that Osgood’s focus on motivations was inadequate

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[6] George Gerbner, Ole R. Holsti, Klaus Krippendorff, William J. Paisley, and Philip J. Stone, eds., *The Analysis of Communication Content: Developments in Scientific Theories and Computer Techniques* (New York: John Wiley & Sons, 1969).

if not wrong. In fact, Stone and Hunt's distinguishers included a lot of irrelevant features like the lengths of the notes and mentioning "father." However, Osgood was from the midwestern University of Illinois, Stone and Hunt's computer approach was developed at Harvard University. So "Harvard beat Illinois" [laughs].

This competition was long ago. Perhaps the conference celebrated computer content analysis more than was justified, at least in retrospect. But it was the new thing to do. Now computers have become better, faster, working on big digitized texts, and are developed no longer at prestigious universities. Incidentally, the kind of analysis that Hunt added to Stone's approach has nowadays morphed into what is being called machine learning. When applied to texts, its limitations remain.

To me, Sheidman's conceptions were and still are interesting also in another more fundamental respect. He developed a logical content analysis of what he called concludifying and demonstrated its analytical powers by applying it to arguments of famous politicians. It didn't get much traction in the communication research community, but its conceptions are important today. He suggested that we all argue with our own idiosyncratic idio-logic. We make assumptions that we may not realize and engage in logical fallacies that are natural for us. To extract them from arguments calls for analysts to construct a contra-logic which would render the idio-logical propositions of speakers conclusive and account for what is taken for granted, not articulated. To understand why speakers unwittingly follow their contra-logic calls for a psycho-logic of how speakers relate to their world. This was of course the domain of Shneidman's preoccupation with the reality of suicide. He realized that communication aimed at preventing suicide can't come from people who argue with the same idio-logic. It follows that efforts to influence people to do something they cannot already imagine of doing, common to what therapists seek to accomplish, politicians aim at, or teachers hope for, requires a pedago-logic which provides practical access to speakers trapped in practicing their own idio-logic. To me his conception led me to issues of liberation from oppression, overcoming injustices, and social discrimination.

Q: Anatol Rapoport was—?

KRIPPENDORFF: I invited him. You'll recall that I tried to talk to him in search for a place to study. Meanwhile, I had read much of his work. Motivated by my cybernetic inclinations I thought he could take a larger view on content analysis. He was, of course, not a content analyst, but to my surprise he did not disappoint even the empirically oriented content analysts who counted coded texts. Rapaport identified content analytical efforts as leading to indices and suggested to expand the empirical domain of content analysis to the whole sphere of language-based communications and proceeded to discuss the challenges one would face describing that sphere as a system of indices regulating the social, economic, or material sphere of existence. He examined several mathematical systems theories for what they would offer analysts interested in not merely describing that sphere as systems of indices about subject matters of concerns, but also their own dynamics. His talk was a call to think out of the box of small content analyses.

I cannot possibly discuss the many contributions that attendees of the conference made. Not everyone presented a paper. I met William A. Scott, who proposed his pi coefficient for measuring the reliability of content analyses. As co-editor of the volume, it was not always easy to get the written versions of presented papers. I remember, actually, Anatol Rapoport didn't have a written version of his presentation, but we had a tape. I transcribed his talk, he edited it heavily, which made it an excellent chapter in the edited volume. This was my first conference, I had proposed it, gave it its name, Gerbner organized it, the editing was a collective effort. But there were also some tensions. Gerbner didn't present a paper and not having a chapter in the edited volume bothered him, giving the impression that he was a mere organizer. So, he added his proposal for the study of Cultural Indicators to the edited volume.[7] It formulated the four questions he wanted content analysts of mass communicated messages to answer: What is? What is important? What is right or wrong? And what is related to what? Answering these questions occupied much of his future research. It was an important proposal.

Q: I mean, that's a great segue to ask about that first sort of moment of the Cultural Indicators project—in this commissioned report on mass media and violence.[8] I don't know its exact relationship to the Cultural Indicators project—

KRIPPENDORFF: That comes much later—'67 or '68, maybe.

Q: So can you talk about both the—I don't know—to what extent you were exposed to the planning for that first Cultural Indicators proposal? And then you wrote a chapter in what became published out of that report on mass media and violence—so your experience of that?

KRIPPENDORFF: Well, no, actually, the fruits of the Cultural Indicators proposal came up later. In pursuit of this project, Gerbner relied mainly on some of my students, Michael F. Eleey and Nancy Signorelli, to which came Larry Gross, who had joined the faculty, and his students.

But I can tell you how we came to the study of television violence on television. At some point the U.S. Surgeon General approached Gerbner for whether he could collect data about violence on U.S. television in preparation for a Congressional hearing. This was something that nobody had done systematically. It had become a significant public concern, an issue calling for potential congressional regulations of the television industry. Unlike today, legislators were very worried about the effects of violence on the public.

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[7] George Gerbner, "Toward 'Cultural Indicators': The Analysis of Mass Mediated Public Message Systems," in *The Analysis of Communication Content: Developments in Scientific Theories and Computer Techniques*, ed. George Gerbner et al (New York: John Wiley & Sons, 1969), 123–32.

[8] George Gerbner, Marten Brouwer, Cedric C. Clark, Klaus Krippendorff, and Michal F. Eleey, *Dimensions of Violence in Television Drama* (Washington, DC: National Commission on the Causes and Prevention of Violence, 1969), http://web.asc.upenn.edu/gerbner/archive.aspx?sectionID=155&packageID=766. See also Marten Brouwer, Cedric C. Clark, George Gerbner, and Klaus Krippendorff, "The Television World of Violence" and "Content Analysis Procedures and Results," in *Mass Media and Violence: A Report to the National Commission on the Causes and Prevention of Violence*, ed. David Lange, Robert K. Baker, and Sandra J. Ball (Washington, DC: National Commission on the Causes and Prevention of Violence, 1969), 311–39, 519–91, https://repository.upenn.edu/asc_papers/214/.

So, with this request in mind, Gerbner summoned three people into his office: Marten Brouwer, a visiting professor of public opinion research from Holland who had a solid quantitative background; Cedric Clark, a postdoctoral fellow who had just completed his dissertation with a content analysis of the representation of Afro-Americans actors on U.S. televisions—concluding that Afro-Americans almost exclusively played inferior roles and rarely taken seriously and made fun of; and me. I was already teaching my *Content Analysis* course at the school. Gerbner related to us what he was asked to consider studying, the timeframe in which Congress needed to have data, and concluded that doing such a study would go beyond any single scholar's ability. Only if we could work as a team, divide the work among us, could such a project be completed in time. Unless we agreed to work together, he would have to decline the Surgeon General's invitation and a study like this could probably not be done elsewhere.

We agreed. And that was the beginning of the TV violence study. We had different skills that could easily be joined. Clark had the most recent practical experiences of studying TV content. Brouwer was a competent statistician of public opinion data, counted on IBM machines, once coded on Hollerith cards. Gerbner posed the initial research questions: how much violence, how severe, committed by whom, with what effects, and what were the motivations of networks to feature it in the first place. Every one of us added some twists to answering these questions. Clark was interested in how racism entered the justifications of violence, Brouwer focused on how the personality traits of perpetrators and victims interacted. I was more interested in the process of coding.

The project was challenging on several levels. Most known content analyses had been done on printed matter, on texts. We faced television images that had no obvious units of analysis. Our film maker, Sol Worth, suggested all kinds of filmic categories to distinguish units within continua, for example, scenes, defined by when a camera shifts, or new actors enter. But violence could not be unitized by what a camera did. We had to identify units of analysis in a complex continuum, moreover, drawing distinctions within the wide range between jokingly issuing threats and actual killings. We were all struggling and trying to get hold of something analyzable.

Brouwer had the idea of just asking questions, like in interviews, and code the answers. It turned out to be very unreliable. Worse, I remember, after punching cards for pairs of coders' answers, feeding them though our card sorter and receiving frequency distributions of coders' coincidences, trying to lump confusing categories to preserve at least some reliable distinctions, we recognized a few cases that seemed acceptable statistically only to find out they were literally meaningless—like distinguishing between "cannot code" and all the categories we thought relevant combined. Not all categories were that useless. Yet, we could not eliminate problems of defining clear categories partly because we developed them from our own preconceptions.

We realized the benefit of collaboration among the three of us and introduced one methodological innovation that eventually gave us reliable data. After viewing many TV episodes, the three of us agreed and put in writing why we'd consider an episode violent in terms of the presence of active perpetrators, and/or affected victims or bystanders, and non-violent when none of these constituents were present. And we developed separate categories for each constituent of violence. We then collectively applied our coding scheme to a large collection of representative episodes and created records for each. Our instrument consisted of the written instructions, the taped episodes, and our codes for each. Not that the development of this instrument went as smoothly as it sounds. We had to refine our instructions and early coders pointed out what we overlooked. But once we were satisfied we used it to train our coders. The instrument was designed to give coders the opportunity to examine an episode, code it according to our instructions, compare their judgments with ours, learn from possible discrepancies, and go to the next episode. We employed only coders who were capable of adjusting their conceptions to ours as evident in high agreements among them.

This instrument served to select standardizable coders. In principle, other researchers could apply our instrument as well. Brouwer warned that we may not be able to defend findings based on data generated after this training, especially at a public hearing that will surely attract tough critics. We needed to measure agreements once coders worked on episodes beyond those we had used for training. In my dissertation I had developed a conception of reliability as the absence of noise transmitted from the phenomena of analytical interest to the coded data. With information theory in mind, I had argued that analyzed data that are polluted by noise are not likely to inform valid results. I developed a measure that seemed to respond to Brouwer's worries, published a few years later, but for mathematical reasons, which I do not want to detail here, it produced odd results and we had to abandon this path to a reliability coefficient.

The idea of accounting for noise or unexplainable variations in data led me to develop another agreement coefficient, the beginning of Alpha. We didn't know the literature of the rarely used agreement coefficients. This was a blessing, as it avoided being guided into their limitations, which I discovered much later. However, we had little time to spare and had to calculate it manually—actually in an assembly line fashion using six students in a classroom with a long table. Each checked the results of the previous students' summations, tabulations, and calculations—very primitive and labor intensive but yielding defendable assurances of the quality of our data.

But then I decided to learn computer programming and took a course in electrical engineering on Fortran IV. I was the only social scientist over there. And after this course, I programmed the first version of what is now called Krippendorff's Alpha. We had to punch Hollerith cards, carry them to Penn's computer center, and picked up the results the following day.

But analyzing the reliability of our data led to a very interesting epistemological controversy among us. Nobody was happy to find unreliable variables among the data we wanted to report on. Gerbner insisted that the industry deliberately introduces ambiguities in media content, partly to allow larger audiences to accede their productions, and partly to avoid being held accountable for the consequences of TV violence. He was correct, of course on both accounts. However, we insisted that if we wanted to study such intents, we would have to define appropriate ways to record evidence of them which would have to be reliable as well. Well, the three of us could not convince him—in fact I still have a letter of his questioning our effort of limiting the reporting of findings to data of satisfactory reliabilities. We refused to buy into the idea that low reliability is giving credit to the media industry [laughs]. In the end, when Gerbner made his presentation to the U.S. Senate he was asked to address the reliability of our findings. In opposition to our report, the industry defended and minimized the role of violence on TV, but to my knowledge nobody challenged our findings.

There are lots of other stories to tell from what we took away from this project. For example, Marten Brouwer did an amazing experiment to understand how my Alpha measure relates to the reliability of data. He gave English-only speaking coders a set of Dutch words as categories to describe a set of TV personalities. The Dutch words were chosen not to resemble any English words and have no similarities with each other. The befuddled coders were asked to do the best they could. Well, surprise, surprise, the agreement was not zero, which would indicate no relationship between the phenomena coded and the resulting data. It was 0.440. Unquestionably, if content analysts would be given such data, they would not have a clue as to what the coders saw and recorded—whether they were Dutch or English speakers. Not measuring zero suggests some kind of associations, perhaps between the size of the personality and the length of the Dutch word—nobody knows. To me a simple cut-off point of when data are reliable and when they are not is untenable anyhow. To me, the reliability of data depends on the extent to which analysts can be sure to know what they are analyzing, how data inform their findings, and the costs of drawing wrong conclusions from them. Further experiments of this kind led Brouwer to suggest that in academic research, the alpha-agreement should be larger than 0.800 and between 0.667 and 0.800 be used only for inconsequential explorations. As soon as life and death decisions are at stake agreement should measure close to 1.000.

This was the early beginnings of my interest in data reliability issues. I can talk more about this and I don't know if I should do that, but—

Q: Well, I definitely want to make sure we pick it up. But I thought I would ask about the communication models. Well, you had this paper you delivered at the conference in 1967 and it talked about three different models of content analysis.[9] And you really are celebrating the third, the communication model. And it's very demanding—it's informed by cybernetics; it involves formalizing in notational form. And I wondered, given that very demanding theory of how content analysis should be conducted, with its kind of philosophical background, did that inform the mass media and violence work that you were doing just at that time, or just after, I should say? You had come up with—

KRIPPENDORFF: Yes. It was my contribution to the content analysis conference, intended to be an overview of how content analysts conceived of what they were analyzing (their conceptions of message content), but ended in my appeal to adopt processes of social communication as

[9] Klaus Krippendorff, "Models and Messages: Three Prototypes," in *The Analysis of Communication Content: Developments in Scientific Theories and Computer Techniques*, ed. George Gerbner et al (New York: John Wiley & Sons, 1969), 69–106, https://repository.upenn.edu/asc_papers/282/.

the context in which to understand and analyze what is happening in the world. With a PhD in communication and teaching at the Annenberg School for Communication, I didn't want to neglect the different disciplinary perspectives of other content analysts while opposing their claims of generality—to analyze THE content of communications. While all empirical research proceeds from observations to conclusions about them, by my conceptions, what links the two spheres is the adopted analytical constructs which implicitly model the world of scholarly interest. I considered it a mistake to equate all of these constructs with content. The first model, I argued, was at home largely among psychologists. Its adoption justified the connection between analyzed texts and the insights sought about them in terms of the associations that people make when reading texts. This required only a limited understanding of language. Osgood's contingency analysts, for example relied on evidence from the cooccurrences of words. His semantic differential investigated how polar opposite words scale and correlate.

The underlying conception of meaning is quite unlike what I observed scholars who in line with my second model pursue. This model is at home in literary scholarship, rhetoric, and linguistics, but also in computer approaches involving the use of dictionaries. It takes the meanings of words within the grammar of sentences if not larger narratives seriously. However, its analytical constructs assume that these meanings are shared within a target population of people concerned with the analyzed body of texts. Content analysts who adopt its analytical construct assume that authors, readers, audiences, even content analysts share the same linguistic habits. The results of using this model amounts to generating summaries of these meanings, albeit in scientifically relevant categories. My third model reaches beyond the former two, addressing the relationships between texts and their contexts of use. It embraces the influence of texts, what language actually does, including what later became a major concern of mine, the constitution of realities. Here the later Wittgenstein came handy. I was and still am most interested in what the circulation of texts set in motion, including how content analysts impact these processes.

Although this chapter was frequently cited, especially in other disciplines, it only marginally informed the violence study. I would now say that it conformed to the linguistic model. We faced unusual complexities due to the visual representation of violence in TV dramas. We had no dictionary. However, our training program provided coders with our visual-verbal correspondences they were asked to learn. Our training program generated standardized coders that demonstrated the competence to go on without much supervision. The violence study was my first practical experience with that research method.

Later on, I consulted with several content analysis projects and served as advisor of PhD students using elements of that method—some came from nursing, others from political science and psychotherapy. This broadened my horizon and taught me how to solve diverse conceptual problems and find epistemological clarities, like the earlier mentioned controversy with Gerbner about whether the lack of reliability is evidence of a strategy of the media industry to make content analyses invalid or a researcher's challenge to generate trustworthy data.

Q: Well, then, stepping back from that project in particular. Mentioning Gerbner over those years—so when you got there in ’64 up through around the early seventies—folks who would stay at the school for a long time were starting to arrive, people like Sol Worth and Charlie [Charles R.] Wright in ’69. So I just was wondering—

KRIPPENDORFF: Sol Worth was there before me.

Q: He was there before, excuse me, and I was wondering if you could just talk about your impressions of the school in that period, the late sixties. And Gerbner’s efforts at fulfilling his charge, which was to make it more of a research-oriented place—what your kind of impressions were of that time as the school was in formation, in effect?

KRIPPENDORFF: Well, actually, since you mention Sol Worth, after Gerbner had been interviewed and his appointment as the Dean of the Annenberg School was in progress, coming back to Urbana from Philadelphia, he told me that he had already met one student for the planned PhD program, a painter and film maker by the name of Sol Worth. Worth lacked academic credential but wanted to stay and teach film at Annenberg. But after the five of us moved from Urbana to Philadelphia, it turned out that he could stay and teach a film course anyway and never worked for a PhD. Before my time at the school, he worked with an anthropologist who hired him to film in a Navajo reservation. Worth was astonished that Navajos, when given a camera, revealed a very different conception of their world, evident in movies of a landscape with no action. These anthropological experiences were his entry into academia, and they shaped his conceptions of making documentaries. He had a good grasp of visual phenomena and influenced several students’ works, Paul Messaris, for one. He wrote also a very important paper that revealed a still valid but rarely recognized insight: Images can never communicate denials, say “no.” To me, this is a profound insight. His claim was questioned by several scholars, but in my opinion, they were all mistaken. A traffic sign featuring a bar across its face presupposes language to learn what it means. Daylight is the opposite of the darkness of night, but neither denies the other. To me he made clear that raw experiences, visual contrasts and sounds are present or not but cannot say anything about their logical opposite except in the language we talk of them.

Charles Wright, I don’t know exactly when he came. Certainly came later, ’69. Well, when I was a student, I actually read Charles Wright’s *The Sociology of Mass Communication* [*Mass Communication: A Sociological Perspective*, 1959] which was part of the staple that we discussed in Urbana. So after the sociologist Rolf Meyersohn left, Gerbner suggested his name and everyone was very happy to have him come and expected to make major contributions. But we had a lot of other professors and I just—I would have to look through the bulletin and you have to let me have some time to prepare myself.

One important professor was Hiram Haydn. Hiram Haydn was actually the editor of *The American Scholar*. He was a writer, had promoted important novelists and taught writing at the school. After he retired to Martha’s Vineyard, Barbara Herrnstein Smith—I don’t quite know how to categorize her—but she was an important literary scholar, taught writing at the school.

We had a sociologist, Rolf Meyersohn, who was very influential in shifting the school towards sociology. I already mentioned Shel Feldman, who taught at Annenberg. He was more an Osgood-type psychologist—and, yes, there were a lot of exciting people besides those I mentioned.

The radio lab first phased out for lack of interest. The graphics lab stayed for quite a while with Sam Maitin in charge of it. And I don't know exactly at which year it was abandoned—it must have been at the end of '60s, no in the '70s, because—well, I don't want to get into this, but Oscar Gandy was a student at that time. He took the course with Sam Maitin and made a famous poster that I have in my office, calling for a revolution. And so in '72, the graphic communication lab continued, the television lab continued longer, but we had to change its philosophy. It became increasingly clear that the equipment we had, the television cameras and taping equipment, was always out of date relative what was used in industry. And so we realized that it is not feasible to teach people how to make movies and produce television shows—when the equipment is so outdated and graduates who would go to the industry would have to learn everything anew. Our approach had to be more conceptual. What we could teach are the principles of visual communication, what it takes to write a script, turn it into images, ready it for distribution and make a difference. And when students took television courses that's what they could take away from them. It was a good justification to maintaining the media labs.

Q: And what was the curriculum like and how was it changing—I mean, that bucket system as it came to be called, was it in place in some primitive form back then, in the late 1960s? And if so where did it come from?

KRIPPENDORFF: If you don't mind, I would like to look at the bulletins again. That is a better way of looking at it. There are the courses all listed and they changed slowly from media orientation to understanding behavior and the sociology of mass communication. Charles Wright was, I think, an important contribution, and then later on Percy Tannenbaum came, but that was very much later. But the sociologist Meyerson, he introduced this issue of sociology. But let's do that at some later point and I get the bulletins in front of me and I can tell you—give you more of the impression.

Q: Perfect, and what about Gerbner's leadership style during this period of the school's being established, effectively, or reborn, let's say?

KRIPPENDORFF: Well, this was complicated, I have to admit. I mean, he was hired to make the Annenberg School academically respectable. That was the University of Pennsylvania's expectation. To understand this change, maybe one should go further back. The Annenberg School was founded by Walter Annenberg, the owner of *The [Philadelphia] Inquirer*, and his idea was, basically, to finance an institution that would prepare journalists to work at *The Inquirer*. So he conceived it as a training site for writers, photographers, and journalists, who could work for *The Inquirer*. This was precisely what the first dean of the Annenberg School, Gilbert Seldes, wanted to get away from by making it a general media-philosophy type of school. He succeeded, actually, quite well by expanding the scope of the school to include different media labs. I mentioned Mary Ellen Mark who was introduced into photography taught in conjunction with graphics. There was the radio, film, and television lab—which didn't really serve the *Inquirer*. Writing was, of course, but this was not strictly journalism.

The relationship between the school and Walter Annenberg was somewhat complicated. When I came to the school Bob Sayer taught the writing lab—but only for a very short time. I was told that he had at some point led a union strike against Annenberg's *Inquirer* and Walter Annenberg managed to get him fired. Of course I was not a witness to how this came about, and whether it was so. But the story attests to Walter Annenberg's detailed interest in what happened at the school. In his place Hiram Haydn was hired. He was a far more accomplished literary scholar and became the head of the writing lab.

The relationship between [Walter] Annenberg and the Annenberg School surely was unusual. At the time I joined the school, Walter Annenberg always referred to the Annenberg School as my school, my faculty, etc. He had created a foundation that had paid for the building and funded the operation of the school and felt he owned it. Once a year, Walter Annenberg invited the whole Annenberg faculty and students to a formal party in a fancy hotel in downtown Philadelphia, at the Barclay, for instance. I forgot some others, no longer in existence, in any case, big places. The whole Annenberg School, its staff, faculty, and students were invited. Walter Annenberg, and all the Trustees of the Annenberg School came. There usually was an Italian band, dancing, dinner, and alcohol. It was Walter Annenberg's event, his school, and he saw everyone almost as his employees. The parties stopped only when Walter Annenberg became the U.S. Ambassador to Britain.

So it was an atmosphere very different from the ICR in Urbana. For Gerbner, it undoubtedly was difficult to balance the forces he faced. He had to report regularly to the Trustees of the Annenberg School, satisfy the long-range expectations of the University of Pennsylvania, chair faculty meetings, administer the curriculum, and above all please Walter Annenberg. He frequently reported back to the faculty that the trustee meetings went well, and his plans and budgets were approved. No doubt, this balancing act was successful, for quite some time, but also difficult for him. He had to hide his political philosophy and was always nervous before those meetings.

Let's get first into Gerbner's teaching style. You asked about that. He was hired to move the school in an academic direction but aimed also at the discipline of communication research. Actually, the field of communication had already moved in that direction. Gerbner divided communication inquiries into three areas, you called them buckets: codes and modes (of communication), systems and institutions (of communication), and (communication) behavior. He could apply this division to hire Annenberg faculty and list courses, but he could also be somewhat dogmatic. Students who came to the school had often different conceptions—not always well grounded.

I remember, in the proseminar there was a session on the function of mass media institutions. As a graduated designer, I never bought into the idea that anything serves only one function. For once a function depended on who was explaining what. I went into an encyclopedia and came up with more definitions than I expected to find and when it was my turn to address the proseminar I presented I think about 15 different conceptions of functions. Many of them could explain aspects of communication but differently and they depended on the perspective taken. But for Gerbner and Wright there was only perspective.

In the proseminar, students were asked to write and submit reflections of what they'd learned from the presentations. I recall, when I was a graduate student in Gerbner's class, he too asked us to summarize what was presented—in fact a good pedagogical practice of getting feedback to a teacher. I recall, in Urbana, Gerbner always insisted on repeating and elaborating on short memorable catch-all phrases, for example "mass media content is the product of industrial production." Of course this invited worthy elaborations but was confined to that phrase. To me this was never the only formulaic account of social phenomena. At Annenberg, proseminar students resisted being molded into Gerbner's categories and earned bad grades on their reflections. The presence of several faculty no doubt added some diversity to Gerbner's categories, but he graded their reflections.

So Gerbner was a bit strong-headed. He knew how things had to be understood, insisted students to get it. In faculty meetings Gerbner largely informed the faculty of his decisions. Gerbner's management style contrasted most sharply with that of the most recent dean, Michael Delli Carpini, who regularly met with the Graduate Student Council, regularly invited its representatives to faculty meetings to air any grievances, and in faculty meetings encouraged discussions of important issues almost to the point of exhaustion until an issue was resolved. This did not happen then.

Undoubtedly, Gerbner was a major force in the emerging field of communication research. However, outside the Annenberg School, his three buckets didn't get much traction in the ICA [International Communication Association]. He was actually more involved in the IAMCR [International Association for Media and Communication Research] because IAMCR was more international at that time than the ICA and there he had Marxist friends.

Q: Well he was at the time taking over the *Journal of Communication* as the editor, right around that period [*sic*: Gerbner was editor from 1974 to 1991]? And I mention this just because you—I want to shift, if you want, to a couple of remarkable papers you wrote right around that time. Or at least they were published around 1969 and 1970. They are both in the *Journal of Communication* and one of them was that "Values, Modes, and Domains of Inquiry" piece—you know, where you were talking about a cybernetic mode of inquiry.[10] And the second one was published the next year which was on data—on generating data in communication research.[11]

[10] Klaus Krippendorff, "Values, Modes and Domains of Inquiry into Communication," *Journal of Communication* 19, no. 2 (1969): 105–33, https://doi.org/10.1111/j.1460-2466.1969.tb00835.x.

[11] Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

And already in your content analysis work there's clearly Ashby and cybernetics and systems theory, but here it's really coming to the fore.

KRIPPENDORFF: Let me speak to the paper on "Values, Modes and Domains of Inquiry into Communication." I was struggling with what scientific inquiry means and how this applied to the field of communication. To me communication is a process not a thing, and it occurs in relationships, not in anyone's mind. There has to be something that makes researchers curious to actively engage with where it occurs, continuously revising their conceptions, and able to communicate the conclusions of their inquiry to others. In other words, communication researchers study their conceptions of communication and cannot do without practicing them. For me, this reflexivity brought cybernetics into play. I distinguished and elaborated on three modes of inquiry: practical, scientific, and cybernetic. They define their object of curiosity differently—controlling or improving their object, communicating their descriptions to others, and inventing formal systems of possible worlds. They also value different outcomes, pursue different values. I considered this to be true for all kinds of inquiry. However, practicing these modes of inquiry make different demands on researchers depending on whether they are applied to social, biological, or artificial systems. The need for this paper came out of faculty meetings and discussions of what communication is versus how we wanted to conceive of the process, why we would engage in the study of communication, and to what else we wanted to generalize it. These distinctions became important to me because I felt it important to reframe from blindly adopting mechanical or computational models of social phenomena, reducing social communication to their biological or psychological bases, or following commercial interests. I can still recommend this paper to researchers who blindly adopt explanations of social phenomena from limited disciplinary perspectives. Of course, for me Ashby's definition of cybernetics as the study of all possible systems, which is only secondarily informed by whether they could be found or realized, was where my curiosity about communication came from and grew. I presented the diagram once in a faculty meeting. Nobody liked seeing cybernetics in the center of the diagram. To me cybernetics merely represented the most abstract approach to communication inquiry. It was not meant to take over the field.

The other paper on "generating data for communication research" was my critique of—well, let's start a little differently: Harold Lasswell, who coined the much-cited formula meant to define the field of communication research: Who, says what, to whom, and with what effects? He continued to define distinct aims of communication research. For him, the question of "who" is addressed by communicator research. The "what" is what content analysis is to explore. "To whom" is audience research, and "effects" are revealed by engaging in effects research. So, he divided the field of communication, into four separate empirical domains of inquiry, each asking and answering different research questions. I thought: to understand communication, this is precisely what one shouldn't do. If one understands communication as what people say to each other while living together, and by extension, how organizations work and the connection that media make possible in society, the idea of partitioning the field of communication in four different areas makes processes of communication invisible or unrecognizable. It would create four kinds of specialists, that have little to share. And worse, pursuing these four research questions would leave processes of communication to those—

individuals or institutions—that could reap unchecked profits from what it can be used for. This paper was to counter such artificial divisions and encourage communication researchers to not lose sight of the larger picture of processes of communication's unfolding relationships in time and space.

I submitted the manuscript of this paper to the *Journal of Communication*. In 1969, the *Journal of Communication* was a slim publication of the ICA [International Communication Association]. Its editor was Paul D. Holtzman. He sent the manuscript to several reviewers, and they advised him not to publish it—not good. Holtzman told me about the reviewers' rejections, without any justifications—I suspect because they didn't understand it. But he told me he would publish it anyhow. I don't know what made Holtzman overrule his reviewers' judgements. It received the 1970 best paper award from the publisher. It is not an easy read and has not made a bestseller list, but it was appreciated by those who mattered to me. Just a few days ago I met someone at the annual meeting of the NCA [National Communication Association] in Philadelphia who is writing a book based on that.

Q: You know, I imagine that there must have just been a sense of you bringing in a set of concepts and ideas from systems theory and cybernetics, and applying them to communication phenomena—bringing in the notion of over-timeness, recursivity, interactional dynamics and all that—that aren't even conceived of in psychological-style research. And so, in addition, you're formalizing all of this, and claiming it should be formalized, in these papers from the late sixties and early seventies, and so how was that received? Given that you—I mean, of course, like you say, that paper was cited and continues to be influential—[but] it also must have befuddled lots of people.

KRIPPENDORFF: Oh yes. It has befuddled some people. The paper was concerned with the kind of data that communication researchers might want to collect in order not to miss what I think communication does across communicating agents and over time. Underlying the conceptions advanced in this paper are the complexities of interacting information quantities. This may well not be for everyone. But its overarching objectives, I hope, are clear: focusing on separate entities loses sight of the larger process which is constituted by how they interact. Over my career, I have become increasingly qualitative without losing sight of the data that makes processes of communication recognizable, understandable, and manageable. The paper was an invitation to try doing the same.

Now, you asked earlier about Gerbner, no? Do you want to talk—

Q: Yes, although I realize that he didn't take over [the *Journal of Communication*] until '76 or something [*sic*: 1974].

KRIPPENDORFF: We could do that later.

Q: You know—thinking of those papers we've just talked about—there was yet another one that was published in more of a systems theory journal—I think it was *General Systems*—but it was called "Communication and the Genesis of Structure," and it has this same character of being infused by cybernetics and systems theory.[12] And you talk about a general law of communication process, that—you know—communication generates structure. And it sort of downplays intentionality and purpose. And so I had a question—if you know about where that paper came from—remember? But also of all of this cybernetics-oriented work, how would you place it in the rest of your trajectory as you went along, you know?

KRIPPENDORFF: Well, I would say that this trajectory started with my working with Ashby. Indeed, I think many of these conceptions came from cybernetics. Cybernetics, its openness of possibilities has still much potentials that communication researchers have not explored. Now, the paper on communication generating structure without intentionality. There is no doubt that intentionality is almost always present in human communication. What this paper explores is the inevitability of structures to emerge as a consequence of communication. Often we have intentions to convince, help or affirm something but in the process on communicative engagements we are led astray, and find ourselves in a totally unanticipated situation. The paper was intended to show that any process of communicating—it doesn't matter what it is about—by its very nature of relating to communicators causes constraints on possibilities, which is a mark of structures. This conception of structure had an information-theoretical flavor, namely of the emergence of constraints on the co-occurrence between various kinds of communicating components. The paper proves this dynamic mathematically and I admit its limitations. But one can find accounts of the inevitable emergence of structure also in the communication literature. Already Gregory Bateson always insisted that anything said carried both content and relationship aspects into what was happening. Relationships evolve or devolve often unnoticed, sometimes getting us into unexpected trouble but are occasionally also liberating. Communication research that focuses only on intentional acts—successes of communication or failures like advertising research aims at—miss the reality they establish.

Q: And, you know, going into that period in the early 1970s. It was a long gestation between your dissertation on content analysis through to the publication of the 1980 book on content analysis.[13] And if you could just talk about how the period in the late sixties, you know, gestated through to that book. How did it come about? It obviously got published to huge acclaim and interest and so on. What was the through-line from this period we're talking about and that book?

KRIPPENDORFF: I don't know how to answer the through-line issue. I had submitted my dissertation on content analysis in 1967, the same year I presented my paper on three models of content analysis to the earlier mentioned conference and co-edited the 1969 book. I published half a dozen papers on the subject, collaborated with other researchers on content analysis matters, and became increasingly known for what I was teaching. There was no content analysis text other than Berelson and Holsti's. Sage wanted to publish one. So, Sara

[12] Klaus Krippendorff, "Communication and the Genesis of Structure," *General Systems* 16 (1971): 171–85, http://repository.upenn.edu/asc_papers/225.

[13] Klaus Krippendorff, *Content Analysis: An Introduction to Its Methodology* (Beverly Hills, CA: Sage Publications, 1980).

McCune, co-founder of Sage Publications, invited me to write one. I did it and it was published in 1980. I would say that without incorporating my meantime acquired experiences, teaching the subject, and consulting on numerous content analysis projects, it wouldn't have been as attractive to communication scholars and fueled numerous studies.

But content analysis was not the only subject that occupied my attention. I explored more fundamental issues of communication and contributed to cybernetics. For example, one of my students, Charles [*sic*: James] Taylor and I explored a few very basic ideas. Taylor was Canadian and went back to Canada after receiving his PhD. Do you know him?

Q: Is this the philosopher Charles Taylor?

KRIPPENDORFF: No, sorry, it was James Taylor, one of my PhD students at Annenberg. He was a few years older than me and was also teaching in the television laboratory. We made several interesting experiments, one involving the feedback speakers receive of their own voice, normally instantaneous. We delayed the voice of a speaker for a few seconds so that they couldn't hear what they said only after a few seconds had passed. We wanted to know what effects this delay had. It turned out that it almost completely debilitated speakers from completing their sentences. The conclusion: We speak inside an instantaneous feedback loop. Another cybernetically informed experiment concerned what it means seeing one's own seeing. We started by exploring what happens when a TV camera takes its own pictures. When people made a hand movement between the camera and the screen it is focused on, that movement was repeated inside the loop, infinitely often, at least in theory. But because the camera can rarely focus exactly on its own image, the hand movement become either smaller and disappeared or it become progressively magnified beyond recognition.

While Taylor and I explored all kinds of television phenomena we normally take for granted, for his PhD dissertation he applied cybernetic systems conceptions on bigger issues. He had taken a summer job working at the Philadelphia Museum of Art. His project was to find out why do people not go to the museum [laughs]? Coming back with these experiences we talked, actually, endlessly about organizational communication: the struggles he experienced while working within the museum, the authority structures. And that informed both of us in many ways, especially how authority is socially constructed, how it is maintained, etc., etc. So that was preparatory to Jim [James] Taylor's dissertation. He wrote it while being in dialogue with me, which is how I have always worked with my PhD students. He became a professor of communication in Montreal and later chaired the department. He was responsible for mentoring several now outstanding scholars, a second generation—no, a third generation of students. One of the second generation is François Cooren, Jim Taylor's student—you know him? He always says that I am his intellectual grandfather. He has students, you know [laughs]—some of which trace their lineage all the way to Ashby.

So there was a lot of things happening, not all of it resulted in papers, but they certainly influenced what I published. For example, I was asked to contribute a paper to a book on the recursiveness of communication. It started with circular self-referential diagrams of human communication.[14] This book was edited by another Annenberg graduate and is frequently cited for its cybernetic conceptions. And I think that is how many people know me for—my cybernetic orientation.

Q: So, you know, given that we are coming to about two hours, I thought I might ask you—because it's perfect to sort of think about how your cybernetic insights informed work in the mid-1970s. During this period of the late sixties and early seventies when you were teaching. I know you were involved in the proseminar. What other courses were you teaching, and do you remember any students before that Taylor period?

KRIPPENDORFF: Well, *Content Analysis* was a natural subject to start teaching because I knew so much about it. But soon, very soon, I started and continued for many years teaching a course called *Models of Communication*. This course was very much informed by cybernetics. It started with conceptualizing communication in information theoretical terms, but emphasized not so much its mathematical aspects, but the relationships people develop when communicating, the networks that emerge and the systems formed and constituted thereby. For parts of the course I used Ashby's [*An*] *Introduction to Cybernetics* as a textbook, and students got a lot out of that. I had students from architecture, city planning, the Wharton School of Business, and was pleased seeing what they did with its conceptions. When I meet earlier students, they always referred to *Models of Communication* as a primary inspiration for their future work, including of their dissertations. Frederick Steier and Larry Richards, who were not Annenberg students, took my course, and later became presidents of the American Society for Cybernetics.

My book on information theory provided an overarching framework for several conceptions that I had learned since my initial exposure.[15] I made a few additions to Claude Shannon and Ross Ashby's mathematical theories. When I wrote it, I had also a National Science Foundation grant and did some programming to work things out. This helped me to teach a separate course on information theory. After 40 years, my information theory book is still in print. But shortly after having done the foot work of my models of communication course, I felt the need to expand my focus of attention and taught a course on *Social Cybernetics*, taking a larger perspective on socio-cultural phenomena that cybernetic models could tackle but the traditionally linear conceptions so deeply ingrained in sociology and media studies could not. I made a similar move regarding content analysis, teaching a *Seminar in Message Systems Analysis*.

I'm thinking of students, early students, that made something of the courses I taught. A Filipino student, Herminia [Corazon M.] Alfonso, is one example. She had journalistic experiences in covering communities in need, taught communication at home, wanted to get a PhD, and write a dissertation related to her interest in communal communication. She took several courses with me, including my *Information Theory* course, and she wanted to explore the kind of

[14] Klaus Krippendorff, "A Recursive Theory of Communication," in *Communication Theory Today*, ed. David Crowley and David Mitchell (Cambridge: Polity Press, 1994), 78–104, http://repository.upenn.edu/asc_papers/209.

[15] Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

communication research one could teach and engage in to aid community development in the Philippines. Her mission was tainted by the bad experiences that most development initiatives in the Philippines originated in the government. It pursued laudable aims but that often had little to do with the needs of the communities in question. Typically, a development expert was sent to a village, notices that its water supply was inadequate, decided it needs a well, which is drilled, of course, in front of the mayor's house. Or someone decided that a village needed a street and gets a street with little regard of how the life in the community was affected. The Philippine government is committed to community development but, without bad intentions, in its experts' terms. The picture Alfonso painted for me was as bad as what many American development agencies do, relying on quantitative measurements that would enable them to show improvements after their interventions—in both cases orchestrated from a distance and without eliciting the voices of those affected. So, I was pleased to work with her on the development of a new research paradigm that, not modeled on Western traditions of scientific inquiries, would start by educating communities to ask, answer, and evaluate their own research questions and give them the conceptual tools to explore their collective consequences—effectively creating community competencies they did not know they lacked. I'm very proud of her 1999 dissertation. She published it as a book which, I am told, has become a blueprint for what she aptly called *Socially Shared Inquiry: A Self-Reflexive Emancipatory Communication Approach to Social Re-search*. It included the proposal of a new job description she called an enabler—not an expert, scientific observer, or researcher, but someone who would be able to bring the community together, to create poieta—not data or evidence of what is the case, but narratives of what could be realized.

Now from a behaviorist point of view this is an absolute no-no [laughs]. You never let the people to be studied ask, answer, and evaluate their own questions, and worse, allow them to change their condition if they don't like their previous answers. Scientific researchers are expected to operate above the objects of their descriptions and avoid communicating their findings to those described for fear of changing what they had found right in front of their eyes. Alfonso's proposal realized the possibility of communication between people who by being describers and described have the power of changing their lives. She took advantage of the lessons of cybernetics in the social domain. It gave me a great pleasure when Philippine students of Herminia Alfonso approached me at a recent ICA conference who knew of my connection to her work mainly through the foreword she had asked me to write for her book, telling me of the lasting impact of her work.

Well, there were several other PhD candidates I mentored who explored different aspects of the cybernetic epistemology, a subject which I subsequently refined in various publications.

Q: That has design implications, too, and I know you elaborated some of those later, but—

KRIPPENDORFF: Yeah, that is true. But—as I said earlier, my design background carried me through many areas I have been and am currently thinking about and actively exploring.

Q: Well, you know, that's a perfect place to stop here for the second session. So thank you very much, Klaus, and we will get back together soon.

KRIPPENDORFF: OK.

## END OF SESSION TWO

# Transcript (modified) of Interview conducted February 22, 2017, with KLAUS KRIPPENDORFF (session three)

Philadelphia, PA

Interviewed by Jefferson Pooley

***Note:*** *This modified transcript was significantly edited by Klaus Krippendorff. The original transcript, synced to the video interview, may be reviewed at [https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field](https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field).*

Q: So I thought we could pick up, Klaus, where we had left off last time, and that was discussing the Annenberg School itself in the period when you arrived, and up through the 1970s. You were talking about your role with the catalog, your recollections of George Gerbner's leadership style and other faculty—indeed Walter Annenberg's role. So with that as a kind of broad prompt I thought we could pick up your recollections of the Annenberg School [for Communication at the University of Pennsylvania] itself.

KRIPPENDORFF: Well, when I came, the school turned around media of communication—practicing a kind of media philosophy. It had numerous labs: broadcasting, which included television and radio, film, graphics, and writing. All of which enhanced students' abilities to understand and engage with media of communication. The university wanted the school to be more academically focused in line with all its other schools. George Gerbner was hired as its dean to accomplish this orientation. The faculty we inherited was largely teaching the media labs and a few more general courses. With Gerbner came Shel Feldman as assistant professor, Wendel Shackleford, a co-student of mine and I, all from ICR [the Institute of Communications Research at the University of Illinois, Urbana-Champaign]. The teacher of the writing lab, Bob Sayer, was replaced by Hiram Haydn whose academic credentials included being the editor of the—I forgot now, the scholarly—

Q: *American Scholar*.

KRIPPENDORFF: *American Scholar*—yes—and he worked also as the editor for a publishing house and promoting poets, writers, historians, etc. So he was, I think, a major force for the writing lab and taught a course in book publishing. Very soon thereafter Robert L. Shayon was hired. He didn't have a PhD but was a highly regarded television columnist and critic, award winning book author, and I would say a media activist. He brought to the school an awareness of how the broadcasting industry was organized that nobody else could explain. He organized a colloquium to which he invited guest speakers to discuss issues other courses did not cover. He stayed on the faculty for many years and influenced many students' work. Charles Hoban had been at the school before Gerbner came. He taught a course on classical studies of mass communication. Gerbner ran the proseminar and taught a course on popular culture. Several faculty from other department of the university were invited to teach. I recall Julian Wolpert teaching a course on information diffusion. We hired a sociologist, Rolf Meyerson, who ran a sociological methods lab and taught courses on media audiences and collective movements. When Meyerson left, Charles Wright was hired—also as a sociologist. As a student in Urbana, we had read his book on sociology of mass communications [*Mass Communication: A Sociological Perspective*, 1959], so I knew his approach. These are only snapshots that moved the school towards a more academic curriculum.

I don't know whether I should talk about details but—let me just give you a story that I mentioned earlier. Initially, with the school turning around media labs, communication as the unifying umbrella was very unstructured. As the dean, Gerbner felt the need to define communication research for the Annenberg School with an eye of structuring the field of communication research generally. He conceived communication research in terms of three areas of studies, later called buckets because everything had to fit into one or the other. He called one "Codes and modes (of communication)." It was broadly conceived of as "what" is communicated in [Harold] Lasswell's terms. It embraced content analysis, of course, semiotics and interpretation. The second area was "Institutions (of communication)." It concerned the socio-economic organization of the mass media as an institution in society, not merely as a channel for dissemination mechanism. And the third one was called "(communication) Behavior." This area was to address not only how audiences behaved in response to communication, the effects of communication on individuals' lives, but also including the dynamics of attitude change—mostly addressed by psychologist in terms of statistics of individual behaviors.

I have to say that I personally was not bothered, even pleased, that my work was not categorizable by Gerbner's distinctions. For my interest in content analysis I was put in the codes and modes bucket. Trying to understand message systems a la Anatol Rapoport moved me out of that bucket. My work in cybernetics and approach to systems put me into the systems and institution bucket, but that bucket allowed no room for the kind of explorations I was interested in. My interest in discursive constructions of realities was orthogonal to the buckets' epistemological commitments to understand and describe different areas of existence, not how they evolve, are created, oppress, and call for liberation. Nevertheless, these buckets served Gerbner to balance the diversity of the curriculum at the Annenberg School, the hiring of faculty, and presented the Annenberg School to the university as an increasing research-oriented entity.

We had a weekly Wednesday colloquium that everyone had to attend—not for credit—and then other courses. For some time, that colloquium was ran by Bob Shayon who invited outside lecturers, well known communication scholars. I heard [Paul] Lazarsfeld, I listened to a presentation by [Harold] Lasswell, lots of people that actually made the Annenberg School much more recognizably academic. For example, one person who had great influence on me personally—and I had suggested to invite him, was Gregory Bateson. His colloquium attracted quite a number of faculty from other departments at the university. He came also into my seminar, to which I invited former students for very stimulating discussions.

Attending the proseminar was required of all incoming students about thirty-five candidates for a master's degree year. Unlike merely attending the colloquium, the proseminar was a course for credit and students received a grade. Gerbner orchestrated the proseminar around topics that were certainly informed by his buckets. Faculty was invited to share their experiences and did not always agree with each other. However, Gerbner was the one who graded the students. And I don't know how deeply I should get into students' dissatisfaction with Gerbner and his proseminar. But it once came to an explosion shortly after Hiram Haydn left and Barbara Herrnstein Smith, a renowned literary theorist, took over the writing lab. To understand that climate, one has to recognize that there was a schism among students who felt the need to commit themselves to study in any one lab: writing, TV, film, or graphics, whose teacher often commuted from elsewhere, rarely knew what was happening in other courses at the school, and didn't care much about them either. Students often came with the preconception of making one lab their primary educational commitment. The proseminar was intended to introduce students to a larger perspective of what communication research embraced. It was a good idea and in line with making the school more academic in orientation. In my opinion, it was marred by two problems. One was the misfit of Gerbner's three buckets and students' conception of communication in the labs' term. The three buckets did not seem to address what could have been relevant to their work in the labs. The other problem was grading in the proseminar. As I mentioned, students were required to write short essays of what they took away from each meeting. Gerbner read and graded them and gave most of them Ds to start. I can't speculate about his reason. Perhaps he pursued the educational philosophy that students would work harder when fearing to fail. Perhaps, he just did not recognize in these essays what he wanted to get across. In the end students got better grades, but they could not know that. I personally feel that this educational philosophy is just discouraging.

At some point, I think it was in 1972–73, students' dissatisfaction with the proseminar boiled over into an open revolt. Students did not address the issue of being put down by bad grades but complained about the relevance of what they were asked to listen to and address. At one point, lectures in the proseminar were disrupted by students demanding justifications of the relevance of the topics discussed. They had come to the school with different backgrounds and expectations. They argued that the school had deceived them by not delivering what it promised. A special meeting had to be scheduled for airing all of the students' grievances. It was a tumultuous event. Some threatened to quit. I sympathized with the students but disagreed on the ground that they cannot be so sure about what they needed to know in the future. I had my own experiences in Ulm in mind, which exposed me to ways of thinking I could not possibly have anticipated. I recall one student who had been a pilot in the Air Force and made my point to his fellow students. He encouraged his fellow students to become competent in what the Annenberg School had to offer and hope for the best. This was an event that seriously challenged the school's curriculum and the way students were treated. It also showed Gerbner as an inflexible teacher.

I don't know if I should mention individual faculty. The annual bulletin of the Annenberg School contains a record of the people that we hired and let go. But actually, Sol Worth is a very interesting example. I remember, when Gerbner came back to Urbana from his job interview at Penn, he was very excited and mentioned that he had already talked to someone who would be the first PhD candidate at the school. He was an artist and film-maker, who had worked with anthropologists and aspired to have an academic career. Although Annenberg did not have a Ph.D. program yet, that expectation didn't come true. Bob Shayon didn't have a Ph.D. but became very influential, which showed Sol not to need an advanced degree. Sol Worth stayed the way he was and taught the [Documentary] Film Laboratory until he died. He had much to say on his own. I was relatively junior and could not participate in decisions of the tenured faculty, and I didn't really want to get involved.

In 1972 or so, there was another controversy. A sign of it is in my office. It is a poster made by Oscar Gandy, who was a student at the graphics lab at that time. It says, "Support Revolution." He was a black student and he was—as many of us were—against the Vietnam War. Although Gerbner undoubtedly shared the same sentiment, he didn't want to see this sentiment associated with the school, discouraged students to protest. I certainly was surprised. Recently, I asked Oscar to interpret that poster again, and he reminded me in stronger terms than I recalled, that Gerbner indeed was against demonstrations in the school. I now believe he was afraid to offend Walter Annenberg. And so Oscar Gandy was one of the many students who protested not only against the Vietnam War but also against the authorities at Annenberg not being on the side of students.

But in 1973—it must have been around 1973—Gerbner's deanship neared its end. He invited the faculty to a retreat in North Philadelphia for a weekend and announced that he will soon no longer be our dean and invited us to help him finding a successor. He expressed the hope that this should be a collective decision. And so we talked about the future of the Annenberg School, without coming up with names. Incidentally, this situation was very similar to what we had on Monday with our current dean who found himself in the same situation, but it went very differently.

Anyway, so we were all prepared for a new kind of dean, but then came the unexpected request by Gerbner to sign a petition to keep him as the dean. He shelved looking for a replacement and expected to continue as the dean as if the faculty retreat had not happened. It turned out that Walter Annenberg wanted to keep George Gerbner as a dean and demanded of the university to grant him an unprecedented extension of his appointment. This created a lot of tension within the faculty. There was Hiram Haydn, Bob Shayon, Shel Feldman, etc. and I, we didn't want to put us at odds with Walter Annenberg's intervention, but hoped this would be the university's decision, and it would be OK to have him as a candidate, but not because Walter Annenberg insisted on it. About half of the faculty didn't want to blindly take sides, especially because we had not been explained how this change came about.

Students realized this situation as well but saw it more as being forced to commit themselves to endorse Gerbner whom they had just confronted with their grievances regarding his conduct of the proseminar. They wrote an unsigned document of complaints, a copy of which I recently gave to our vice dean to record. It articulated their misgivings in great details—the proseminar was only one of many. The document was not signed, presumably because of fearing retribution. But it was a detailed and terrible critique. It came after the continuation of Gerbner as a dean became known and support for that continuation was expected but not met. Well, I don't want to get too deeply into these controversies but must say that it was not always a smooth ride.

The three divisions that you asked me to comment on, slowly transformed the Annenberg curriculum and hiring practices at the school into so called "buckets," where everyone had to commit to one area and one professor in that area but was required to take at least one course in the other two. This division was only slowly undermined by more recent developments in communication technologies and new kinds of issues emerging that did not fit the buckets. Today faculty and students divide themselves in different terms, for example pursuing more quantitative versus qualitative approaches or cognitive versus culturally informed sociological or political approaches. And so that's very different.

Q: Well, I wanted to just follow up, on a lighter note, and ask you about the copy machine—the Xerox machine, I should say—story?

KRIPPENDORFF: [Laughs] OK. That was kind of funny. For the longest time we multiplied papers on mimeograph machines. One had to produce matrices on a mechanical typewriter common at that time. Instead of letting the hammer with a character hit a ribbon and produce a letter on a piece of paper, one had to use a special wax paper on which the hammer created an opening through which the ink of a mimeograph machine could eventually flow. For each typewritten page one had to create such a wax sheet. Mount it on the drum of a mimeograph machine and hand crank it for the number of copies wanted. I am telling you this because nowadays the amount of work needed is unimaginable.

At one point I had lunch with Shel Feldman, the psychologist, at the university's faculty club. Gerbner had also lunch with someone else there. Shel and I talked, lamented about how much time it took to get a mimeograph copy of a page and we agreed it would be nice to have a more modern copy machine. I thought we should talk to Gerbner to get one for the school. He was sitting just a few tables from us. We didn't know with whom he had lunch with, and I certainly didn't want to disturb him. And so I wrote on my red paper placemat that we really should have a Xerox machine and passed it to him with a smile but without saying a word. He read it and wrote [gestures writing motion] on the same red placemat: If you can make a copy of this, you get one. I did not know, and I am sure Gerbner neither, that Xerox copiers sensed documents with a blue light and cannot normally distinguish what is written on a dark red background. The university had a copying center but efforts to make a readable copy of the placemat failed. It produced uniformly black copy. I didn't want to give up and went, actually, downtown to the Xerox shop, explained to the technician what was at stake and urged them to make a readable copy of it. They somehow made it, and we got a Xerox machine [laughs]. That's kind of a vignette of how it was done. It was placed in the library, so reading and Xeroxing could be done together [gestures].

Q: Well, I thought I would ask about content analysis. We talked about that theme last time too, but I would love it if you could describe the development of your work in content analysis through, at least, to your 1980 book, and including work you did that was commissioned by the Surgeon General—that work—and if you have recollections about how the Krippendorff's Alpha came about, and the story of its emergence.

KRIPPENDORFF: OK. Well, as I mentioned, when I had the choice of selecting a dissertation topic, content analysis was one of my options. I thought that it would be a good topic because content analysis was kind of an underdeveloped methodology and a key to communication research. In fact, I still insist it is. While communication research has adopted so many research methods from other disciplines, such as survey research from sociology and political science, or making behavioral experiments from psychology, there were two areas—two methodologies—that are indigenous to communication research: One is the analysis of messages—of content—and the other one is actually network analysis. The notion of networks emerged in the recognition that communication is not taking place just from A to B but rather it networks organizations, administrations, etc., into wholes. So, network analysis and content analysis were to me two research methods that were unique or indigenous to the field of communication research, hence in need of development.

When I was a student at the University of Illinois, content analysis was kind of underdeveloped. I wrote my dissertation, largely conceptual, from the literature with the aim of exploring what content analysis could do and develop it as a methodology central to communication research. I was versed in information theory and used it to examine the flow of information in complex systems and I thought to take processes researchers engaging in content analyses as just one example—and we will probably talk about this at another point. But it was for me, largely, one should say, conceptual—theoretical, based on literature and my minimal experience as a research assistant at the Institute of Communications Research. And I have to say also that, based on where I came from, I had many epistemological disagreements with content analysts who saw content as an object of study independent of its contexts and the people involved. It was very early on, during my dissertation period, I realized that the idea of content, the idea that messages could contain something that you could simply take out, was a wrong conception. The consequence of conceiving of content as contained in a message is that its receiver has to take out of messages what an author had put into them. It doesn't allow two people to take different contents out of the same message and grants content analysts the authority to claim certainty of what they were analyzing. It physicalized content for the sake of content analysts claiming objectivity. Later, I wrote of metaphors of communication, which offered a more detailed critique of the common use of the container metaphor of communication. However, already when I wrote my dissertation I realized the conceptual constraints the use of the container metaphor entails for the advancement of communication research.

So my conception of content analysis denied its ability to analyze what messages contained. Instead, it focused on drawing inferences from texts to the social contexts of their use. This embraced the possibility that authors and readers, producers of entertainment and their audiences, including content analysts, may well and typically do interpret the same messages differently depending on the context in which they generate texts, circulate messages, act according to them, or analyze them. I gave several papers on my critique, starting with my contribution to the 1967 conference on content analysis at the Annenberg School, discussed in an earlier interview. Since that time I accumulated many experiences when analyzing violence on U.S. television, becoming involved on a project of analyzing various conceptions of democracy across different countries, working with political scientists at the University of Pennsylvania, presented papers at the International Communication Association meetings and teaching two graduate courses, an introductory course on the subject later extended to a seminar in message system analysis. I became somewhat known when Sara McCune, a founder of SAGE [Publications] approached me to write a textbook on content analysis—there was none really available. I agreed but preferred not to name it content analysis but an introduction to text analysis. I thought at least the word text should be part of the title. But the publisher said, No, content analysis is the more established term. So I decided to do so, but its first chapter actually undermines the veracity of the notion of content and put in its place the analytically more productive conception of drawing inferences from texts to their contexts of use. This conception epistemologically embraced the notion that analyzed texts are always read, interpreted, and shared by particular communities of users of these texts and most importantly, enacted, creating realities that may not exist without these texts. So this was the conceptual starting point of the first, 1980, edition of my content analysis book. Although I'm going a little bit ahead of your questions, the book featured several methodologically different approaches and also made the reliability of generating analyzable data from given texts an important issue.

I don't know—should I talk about the Krippendorff's Alpha? Well, one of the distressing experiences of our initial coding effort in the television violence study was how little coders agreed on what they recorded as data. Marten Brouwer, who was an experienced opinion researcher, insisted that we had to measure this in some form. He argued, if we don't demonstrate that there is reliability, our findings can be easily debunked. And he was correct, especially because our findings would become part of the public debate and Congressional records. So, our question was how to do that? In retrospect there were actually some coefficients to measure that, but none of us knew about them. So we had to start from scratch.

In my dissertation I had discussed reliability in terms of information theory. The notion that information theoretical noise was a sign of the unreliability of coders appealed to me. So my initial proposal was to measure the reliability of the process of generating data as the degree to which the process approximates perfect transmission of information from the unstructured television images to the data we wanted to analyze.

I was pretty sure that this was the way to go and I had written a paper on this possibility which now came handy, so I thought. I had no practical experiences with measuring the reliability in information theoretical terms. It turned out that there was something odd about that, mainly because information theory deals with logarithms of probabilities and not with actual numbers of mismatching assessments by coders. What had attracted me to information theoretical measures was the ability to partition a set of codes into subsets whose entropies were conveniently additive. When coders agreed, all values assigned to a unit of analysis were the same and entropies were zero. Disagreements, it seemed, could be measured by entropies. However, entropy measures are a function of the logarithms of frequencies. What I did not know but soon discovered was that entropies depended on the number of codes whose unequal granularity made them not comparable across different number of values. I had not experienced this as a drawback. I turned to the analysis of variance mainly because its variance measures could be interpreted as disagreement measures and it had similar algebraic qualities and was not that much affected by the granularity of data. I could easily translate the notion of noise in data in variance terms. So we decided to variance analytic expression and I defined an agreement coefficient in these terms. However, while we had a few scales that gave us some variance measures to start, the majority of our codes consisted of unordered categories to which variance analysis is not applicable. Brouwer suggested that we decompose the unordered values of variables into a series of binary distinctions which could be evaluated in the new coefficient's terms, but this would give us numerous tattered reliability scores. I could not solve this problem. I was determined to make it work. [gestures emphatically] I remember, on a very long flight to Bangladesh, then East Pakistan, with nothing else to do and plenty of paper in front of me, I tried out numerous approaches, but nothing seemed to work.

I almost gave up when it occurred to me—something I had not read and nobody taught me that the mathematical conception of variance, a measure of how much an observation deviates from a mean actually equals the average of the square of the differences among all possible pairs of values formed from a finite set of values. Once I understood this equivalence it became natural to define the disagreements within nominal data in terms of the number of mismatching pairs of values. I could replace the differences in terms of which variances are defined by whether two categories were matching or not, same or different. This was the beginning of generalizing the variance-based agreement coefficient to other kinds of data subsequently developed. We used these two strictly comparable versions—now called Alpha for agreement— for assessing the reliability of our data.

I should also add to when I said "we used that" measure, we had very little time to and we were not equipped to process our data efficiently. We had card-sorting machines; we could calculate correlations and simple statistics in our computer center. So I remember that we organized calculating our reliabilities by asking five or six students sitting at a long table, each enacting in an assembly line fashion different steps of counting, adding and crosschecking each other according to the formula and recoding the results.

Well, then, I decided, this is outlandish. And so I took a crash course at the university's physics laboratory to learn Fortran IV, a programming language that enabled me to write a program for getting us reliability measures. So we had to transfer the coding sheet that coders produced onto Hollerith cards, carry them in boxes to the university's computer center on 3400 Market Street and picked up reams if computer printout on the following day.

Subsequent to our development we found other agreement coefficient in the literature. However, it turned out that Alpha was far superior to and more generally applicable than known others. For example, there was a coefficient π (pi) proposed by William Scott. Scott was a public opinion researcher and solved problems similar to those we had been facing. Scott's π resembles Alpha, except for being limited to two coders and assuming infinite sample sizes. Interestingly, a more influential bio-statistician, Jacob Cohen, took Scott's coefficient and changed it into one he called κ (kappa) in line with the more common measures of inter-coder relations. He thereby ruined is as a measure of the reliability of data. This taught me the lesson that developing something by relying on existing examples as opposed to focusing on the empirical problem one needs to tackle can be confining and unproductive.

At some point, someone from sociology sent me a rejection letter from the editor of *Biometrics*, a journal to which he submitted a critique of the Cohen's kappa. I thought, the author was actually correct. And so I decided I'll write to this editor, and simply say, He is correct, and you should just publish this piece. It turns out that the editor was Joseph Fleiss, a student of Cohen and defensive of him. He allowed me to write a comment, which was published but discounted by Fleiss, the editor in charge. This is still available [laughs].[16] It was an interesting example of the politics of loyalty overriding solid arguments—at least in my opinion.

Anyway, since that time I have been actually working to generalize this Alpha to other areas. One important area was its extension to segments of continua. After all, published texts, narrative, movies, even histories extend over lengths of character strings in documents times in videos or periods whose beginnings and ends are chosen by narrators. Already in the violence study, unitizing a television drama was difficult. We could not develop an agreement measure that responded to the disagreement on where a violent episode began and where it ended. In qualitative research it is common to underline relevant text and the question arises do researchers agree on the segments of continua they categorize one way or another. Finding a measure of agreement for freely selected incidents that were relevant for a particular research project was a hard nut to crack. But I developed a generalization Alpha made many mistakes, but settled on three approaches.

Another challenge was to cope with the common practice. Particularly of qualitative scholars to assign more than one quality to a phenomenon. After all people have different hobbies, a product can be described in terms of several dimensions, a party platform is committed to several points. So I developed a way to assess multi-valued phenomena.

Alpha underwent many generalizations, usually driven by unusual data but always motivated by resolving the uncertainty of whether coded or quantified data could be trusted. For another

[16] Klaus Krippendorff and Joseph L. Fleiss, "Reliability of Binary Attribute Data," *Biometrics* 34, no. 1 (1978): 142–44, https://www.jstor.org/stable/2529602.

example, someone recently approached me with the challenge of analyzing huge numbers of crowd coded data obtained by volunteers recruited from the internet. He had 3.7 thousand unique coders, judging over 2.6 million units by between 1 and 56 coders each and 11.3 judgments in total. He recognized that Krippendorff's Alpha is the only coefficient that can cope with unevenly distributed coder participation, but there was no software capable measuring whether such big data could be trusted. I was lucky to be able to contribute to the solution of his problem, learned a lot from these challenges, and developed three members of the Alpha family shedding light on different qualities of data. But I don't want to get too deeply into that.

The point is that Krippendorff's Alpha, responding to the initial challenge of assuring that easily generated data are resistant to critical examination and do not lead to invalid conclusions, proved to have advantages that drove its development into a whole system of generalizations. The use of Alpha has migrated into numerous disciplines from medical research, educational testing to computational linguistics. Communication research was only its origins.

Several scholars have written software to compute Alpha, for example, Andrew Hayes from the OSU [Ohio State University]. He is a communication researcher with a statistical bent. He wrote a program called KALPHA which is widely used. I once presented a paper at an ICA [International Communication Association] meeting outlining an approach to multi-valued coding. At that time this was a difficult problem for everyone I know, for me too. A British student in computational linguistics wanted to take up one of the proposals I had made. We collaborated in finding solutions to problems that I had not anticipated. He wrote a computer program for his master's thesis. I refined its mathematics and we published a joint paper. Unfortunately, the program ran into computational limits that encouraged me to find a different approach. Recently I was working with a group of French linguists. They approached me with a problem I could solve, ending in a joint paper that includes freely available software to calculate the reliability of character strings.[17] I could mention many more examples that suggest Alpha to be the result of my may cooperative involvements and challenges.

I am in the process of writing a book on Alpha, largely to free my mind for other academic challenges.

Alpha is not the only contender. Cohen's π is far more popular despite serious criticism and limitations to a small set of applications. Chronbach's α bears the same name, but measures something very different from the reliability of data. Besides formal differences among coefficients that claim to address reliabilities, there are also conceptual disagreements of what agreement should establish, what reliability is to be about. For example, there is a Chinese scholar from Hong Kong decided to equate reliability with the difficulties coders experience in the process of generating data. While such difficulties are a well-known source of unreliable data, to me, perfectly reliable data are perfect surrogates for the phenomena of analytical

[17] Klaus Krippendorff, Yann Mathet, Stéphane Bouvry, and Antoine Widlöcher, "On the Reliability of Unitizing Textual Continua: Further Developments," *Quality & Quantity* 50 (2016): 2347–64, https://doi.org/10.1007/s11135-015-0266-1.

interest. The competence of the designers of coding instructions given to coders have to be assumed. They are not the target of analyzing the data.

Well, now Krippendorff's Alpha has now migrated into all kinds of directions. In fact, I'm writing now a book on that and I've written, I think, eight chapters or something. But I will write a few more and then it will be done. Whether it will be published–it's full of mathematics. But, anyway, that is the story of the reliability. In the meantime, between when I started and where I am now, I have been asked by so many researchers, What to do, how to do it? And I have become kind of an expert in the measurement of data reliability.

And several people have written computer programs, for example, Andrew Hayes from the OSU [Ohio State University]. He is a communication researcher with a statistical bent. He wrote a program that is probably the best, as it's widely used. But then, recently, I was working, actually, with some French people, a group in Normandy, and they found a problem. So I have to say I'm always pleased when someone finds a problem that I then can solve. So I solved the problem in collaboration with the French group and that is now just published. And so that is there.

Then, as another thing: There's these multiple coding things. It was very difficult for me, and I wrote once for the ICA [International Communication Association] a paper of different kinds of approaches where we should move. And I formulated the mathematics of that, but there was someone in England who wanted to write a master's thesis in computational linguistics, and he wanted to take this up. And so we worked together, and we solved that too. So, I mean, it's not just my project, but I think I responded to many challenges that were formulated by others. Also, I have to say, I made several mistakes, things that I couldn't foresee. For example, Andrew Hayes—I mentioned him—he had a student that put a certain kind of data in there and it produced an odd result. So, I struggled very hard—he sent it to me—very hard. How one can solve this problem? And now I solved it, and so it's done. So it's, in a way, a kind of a good scholarly exercise: working with lots of people, not just with my own idea.

Q: Great. Well, I thought we could take a different direction, though you just mentioned, a moment ago, a paper you delivered at ICA, the International Communication Association, and I was curious about your involvement with the association from the late 1960s at least through to the time when you became the association's president in 1984.

KRIPPENDORFF: Yeah. Well, I came to the Annenberg School in 1964. My dissertation was finished in 1967. And I don't remember for sure, but I think joined the NSSC, the National Society for the Study of Communication, in '68 [the organization's original name, which was changed to the International Communication Association in 1968]. And I think, first, I was just there and then I was invited by Randy Harrison to give a paper—that was in '69. That was actually the paper that you mentioned at some point, about what it means to study communication from the point of view of data. And I presented that in 1969. In 1970 it was published, and then afterwards, I got the first prize of this paper.[18] But it was Randy Harrison who encouraged me, and in 1970 to run for the chair of the Information Systems Division and was elected. And I stayed, actually—I believe for four years as the chair of that division. And I think my impact on this was that I, first of all, introduced information theory as a kind of a starting point, but not just information theory per se, but the whole notion of looking at communication from the point of view of the information transmitted, and computational issues. We developed a newsletter called *Systems Letter*—after all our division was named Information Systems. It was not just information theory, but it also embraced computational approaches to understanding systems and the widespread digitalization that developed as a consequence of computational technology.

And so this division thrived, I think, quite a bit. I had a lot of support. Rolf Wiegand edited *Systems Letter*. We produced a t-shirt which I designed. It was the first t-shirt [laughs] that any ICA division had ever made. Our division was not as large as others, for example the Mass Communication Division, but we were a proud bunch of scholars. I remember Ed [Edward L.] Fink, who succeeded me as the chair the Information Systems Division, he was very aggressive in selling the t-shirt to everyone [laughs]. And so, I think, the Information Systems Division was pretty successful. It has now migrated more to kind of a methodology-oriented kind of division, but my mark is still there because there is recently—they founded an award for the best dissertation, [the] Krippendorff Award [Klaus Krippendorff Book Award]. So I was kind of instrumental in this whole Information Systems Division.

Unfortunately, much after my chairmanship, another division split from the Information Systems Division which focused on communication technology [the Communication & Technology Division]. This was unfortunate—not necessary conceptually, but entirely personally motivated. But so it was.

I was elected to the [ICA] board as a general member. And then in—was it 1982?—I was elected to be [ICA] president, starting in 1983 or 1984 [1984]. And so, one of the first tasks before one becomes the president is to organize ICA's annual conference. It was scheduled in San Francisco. At this time, lots of ICA members called such meetings a convention. I decided for a more academic alternative and we called it a conference ever since. I introduced the idea of a conference theme and invited several scholars to form a committee to help me structure the conference around the topic of the future of communication. My motivation was simple. The ICA was formed by a group of scholars who walked out of an organization concerned with speech communication. They had the vision of making the process of communication its primary focus. Meanwhile there had been so many new technological development worthy of considerations necessitating shifts in emphasis in the study of communication. So the conference was called "Communication in Transition." And it was, as I said, the first ICA

[18] Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

conference that had a theme, a topic. It became an ICA tradition to assemble under an umbrella concept.

One of the biggest challenges of incoming ICA presidents was to organize the conference schedule. ICA had an executive director, Bob [Robert] Cox, who took care of hotel reservations and communicating with members of the association, but my task was to organize the conference program, to schedule the paper sessions, which were vetted by the division, some requesting an ordering. I had never done such a task and it seemed overwhelming to me, especially in view of presenters of several papers that could not be scheduled at the same time, of divisions that wanted their sessions not to overlap, to which came my idea of theme sessions that were not to compete with other panels. Moreover, each division had business meetings that could not overlap with other sessions of that division, and it was the tradition that the ICA president would have to be able go to each division, albeit briefly, to report on issues of the association as a whole that the division was asked to consider. Before all the requests would come on my desk I feared unable to cope with this complexity. So, I decided to write a computer program, which was the first time this was ever done, at least in this context. The program distinguished among the divisions that submitted a session, the authors and coauthors who presented papers therein, the rooms available as well as special provisions for some presenters who were not available on all days. It was a combinatorial problem of minimizing conflicts. To my surprise it worked out surprisingly well. It computed a schedule in which all authors and coauthor could attend the sessions in which they presented co-authored papers minimizing the typical conflict of having or wanting to be at different sessions at the same time. There was one exception, and that was Ev [Everett] Rogers. He was the coauthor of too many of his students' papers and just could not physically attend to most [laughs]. Except for him, the result was amazingly successful. People reported only minimal conflicting decisions. Although by comparison today there would be more cross-divisional interest. Subsequent organizers of ICA conferences wanted to use my program as well, and I offered it to them. However, at my time there were fewer ICA divisions and the computer program was written in Fortran IV that not everyone could tailor to other operating systems. Nowadays there are commercial conference organizing systems available and that task has become far more mechanized. I do not know the extent to which the same criteria I used would apply.

But I was also a graduated designer and eager to design a new conference brochure, which was the traditional job of ICA's Executive Director, who would have simply listed the sessions. I wanted to create a more user-friendly brochure and introduced several innovations. One was a numbering system for all of the sessions. The first digit for the day of the conference, the second was the—I forgot.

Q: The division?

KRIPPENDORFF: I'll check it out [flips and reads through catalog]. Yeah. The first one was the day, the second was the time slot of that day, then came a period, followed by the room number, which I was able to correlate with the divisions sponsoring the sessions. So, conference participants who were interested in only one division could come back to or stay simply in the same room. So that was just a way of numbering the various sessions. I also introduced, what had never been done before, a directory of conference contributors at the end of the brochure, a list of names together with which days, times, and rooms/divisions they would have to present and could be found by their intellectual colleagues. I am happy to see that in more recent conference brochures, this numbering system has been kept, slightly changed because it had to accommodate more divisions, different kinds of presentations, locations at different hotels, etc. But I am pleased it is still there, and continues to enable conference presenters to find their obligations and to find each other.

Another innovation concerned the conference themes. I already mentioned that it was the first ICA conference that had a theme. Every division could contribute their visions of the future of communication technology, social problems and methods of researching them and were eager to do so, but I didn't want them to compete with other sessions. This posed considerable scheduling constraints but encouraged conference participants to cross their divisional confinements. It even featured a room in which short communication-related avant-garde movies and documentaries were shown on a continuous basis for participants to relax a bit between intellectually demanding sessions. It was curated by Rolf Wiegand, who was also the editor of the *Systems Letter*, a publication of the Information Systems Division I started when I chaired it. The San Francisco conference was the most attended ICA conference up to this date, for which the attraction of the city undoubtedly was the main reason.

Q: Well, if you want to continue with ICA, I wanted to ask a quick follow up if possible.

KRIPPENDORFF: Ok, let me just mention the following year we met in Hawaii during which time I had become the ICA president and Brenda Dervin was my elected successor. She chose "Paradigms in Transition" as the theme for the next conference and invited several international scholars to participate. One of my earlier concerns was the visible gender disparity among communication scholars. I asked one communication scholar, Rita Atwood, who I knew as a feminist, whether she would be willing to consider organizing a session for the following year, potentially leading to a new division concerned with gender issues, and she enthusiastically embraced this idea. In Hawaii her session was jam-packed with woman communication researchers, some presenting their research but mostly venting their ideas for an ICA division. There were only two men present, one was Gary Gumpert and I. We became the first male members of what would later become the Feminist Studies Interest Group and soon thereafter an ICA Feminist Scholarship Division. So that was one aspect.

For the Hawaii conference I wanted to design a logo. We expected Hawaii to attract a lot of Asian participants. As a European-trained designer, I was thinking of a single graphically attractive icon. I had several Chinese students and thought to consult them to find a single character that could represent all of what communication embraces. I learned first that communication is too abstract an idea. In Chinese, one had to say exactly what was passed on. Then I was told that single characters do not have a place in the Chinese language. There have to be at least two to make sense. I didn't want to give up that idea. So with the help of one of my Chinese students, Clement So, now a professor of communication in Hong Kong, we went through his Chinese dictionary looking for the many ways communication could be referred to, and I found one character that seemed to be most commonly occurring among these expressions which could conceivably serve as the logo. And I have it here, that is the logo [holds up booklet]. To me, from an undoubtedly Western perspective, it had attractive graphical qualities which could be made even more abstract, but Clement representing the Chinese perspective it missed its meaning. Moreover, my effort to simplify it was immediately dismissed as too Japanese. I waded into unknown linguistic conceptions. Yet I was satisfied that the character occurred in abilities to move something, to come together, to be close, to meet, to join, unite, to mate, have intercourse, intimacy, a friend. All of these expressions seemed to be connected with this character. And so it became the symbol of the 1985 conference in Hawaii. I also created a t-shirt to be distributed there. For my Chinese friends this logo was a Chinese character alright, but whether my logic made sense to Chinese speakers remained unclear to me.

I should also mention that, at that time, the ICA was struggling with the word “International” in its name. ICA had largely American members. The name “International” was probably adopted to convey that the concern of the association was not limited to what happened in North America. But the international component was minimal. Having had conferences in Germany, Canada, Mexico, and now Hawaii was partly to assert that human communication is a general phenomenon and to connect with communication associations in other countries who would host ICA visiting them and supporting their local recognition. In ICA there existed a not too explicit difference of opinion between those who preferred to see ICA expand its membership to include other than U.S. communication scholars, and those who saw ICA’s mission to expand communication research worldwide. So it was in Hawaii towards the end of my ICA presidency that I invited representatives of different communication associations to a roundtable to discuss the state of the art of communication concerns and what we could all do to aid each other’s efforts. Frankly, I didn’t want the American ICA to monopolize communication research worldwide, although we probably had more members and resources than other regional association—Japanese, Korean, French, Scandinavian, Canadian, or German. Those present decided to join in an International Federation of Communication Associations whose members would inform of each other’s plans for meetings, invite collaborations on international research projects, and make each other aware of their scholarly publication. The Federation was registered in Canada, and we had several meetings organized by its members, published an electronic journal listing the publications each produced. We shipped volumes of publications to libraries at universities outside the U.S. My initial presidency went to representatives of communication associations in the Netherlands, then Germany, Poland, and Croatia, after which it fizzled for lack of resources. I think the Federation helped a lot of communication associations to gain international recognition and they undoubtedly profited from joint conferences and collaborations, but meanwhile ICA membership became increasingly international and slowly growing to justify its name.

By tradition the outgoing ICA president gave an address at its business meeting. Usually such speeches celebrated the state of the association, reported on the increasing significance of communication scholarship, and acknowledged the help received in running the association. I was certainly pleased of having been part of what the ICA accomplished since I became one of its members a decade earlier, but the conference theme was too important to me to pass by. I wrote a paper proposing five imperatives of what communication research should address.[19] My time was cut short. I could not finish what I had to say, but it was important enough to be published[20] and republished.[21] I have been credited to have been the first outgoing ICA president to present an academic proposal, now increasingly common.

Q: Well, I thought I would follow up about the two stories you've just told: ICA and Krippendorff's Alpha. In both cases, in the early history of Alpha and when you were programming the ICA conference, you turned to computer programming to solve a problem. And you've done other programming. And I just thought I'd ask about your interest in, and reliance on, and use of programming for purposes that you have over your career.

KRIPPENDORFF: Well, actually my programming started in 1967 or something—'66 or something. The last program I wrote was in 1973. During this brief period programming was pretty simple. It was Fortran IV, then Fortran H, but I have not advanced beyond that since. However, these early programming experiences have always stayed with me. It gave me a deeper understanding of algorithms. To me computation is a very important part not only of communication research but increasingly so in society. We communicate with algorithms all the time, starting with calling an office and having a machine asking you a series of questions that hopefully leads you to the answer you were looking for, to searching the vast cyberspace of the internet for the solution of your problem, to automatic stock market exchanges that respond within seconds to changes in prices outperforming human beings. I have been interested in the performance of algorithms ever since—not as a programmer—the developments of computer languages have far surpassed my abilities. Although I have written specifications to develop computing aids for content analyses, measuring interactions, the reliabilities of big data, and consulted with academic and commercial projects, I'm not really involved with actual programming. However, in view of the ongoing digitalization of very many social phenomena, I think it is important for communication researchers to have at least a sense of what programming entails, what can be accomplished and recognize overblown or false claims—too often accepted by people who have no clue of what programming entails.

My current interest in computer programming concerns largely the consequences of describing social phenomena in algorithmic terms, often without realizing them as such. What is gained but, more importantly, lost when human communication is confused with their algorithmic accounts. When programming the scheduling of the 1984 ICA conference sessions, the human conceptual part dominated the computations. Now, algorithms have increasingly entered the

[19] Klaus Krippendorff, "On the Ethics of Constructing Communication," presidential address delivered at the International Communication Association Conference on "Paradigm Dialogues," May 23–27, 1985, Honolulu, Hawaii, http://repository.upenn.edu/asc_papers/275.

[20] Klaus Krippendorff, "On the Ethics of Constructing Communication," in *Rethinking Communication: Paradigm Issues*, ed. Brenda Dervin *et al*. (Newbury Park, CA: Sage, 1989), 66–96.

[21] Klaus Krippendorff, "An Alternative Paradigm," in *On Communicating; Otherness, Meaning, and Information*, ed. Fernando Bermejo (New York: Routledge, 2009), 11–36.

conceptions of communication. In order not to confuse their differences you have to have a modicum of understanding how algorithms come about by programming and where one can rely on them and where relying on them can become deceptive.

Q: Well, I mean, speaking of information theory, you brought it up in the context of the Information Systems Division. And you wrote a lot in the 1970s—I mean, it informed some of your dissertation, it informed the content analysis book—all the way through that 1986 book that was called *Information Theory*.[22] I thought, maybe, you could talk about the role that [Ross] Ashby's interest in complexity and simplification played with your interest in information theory, over the '70s. And in particular, you know, your sense that in 1978 with George Klir, if I'm pronouncing that right, to change some of your opinions about what information theory could do.

KRIPPENDORFF: OK. Actually, the first time I heard about information theory was in Ulm [School of Design, Germany] where I was in the process of becoming a designer. There was one professor of philosophy from Stuttgart University, Max Bense, who promoted what I would now call a cultural interpretation of information. He observed that avant-garde artists and poets whose creations are highly unusual, rare, i.e., of low probability, gained only slow acceptance after their works became duplicated, redundant, more common, and more probable, i.e., their initial information eroded. Redundancy had something to do with being meaningful. The other professor, Horst Rittel, became kind of my mentor. He was a mathematician and introduced us to all kinds of novel conceptions—among others, cybernetics and information theory. The Ulm School of Design had an information study track that was the brainchild of Bense. When Bense retired, Rittel was hired to take his place. I remember during his first lecture on information theory most students of the information department—largely journalists and art critics—got lost because Rittel developed information theory from probability theory to several of Claude Shannon's information theoretical axioms. Although Rittel could adapt quickly to the level of students in Ulm, but my knowledge of information theory was still very rudimentary when I started studying with Ross Ashby at the University of Illinois at Urbana-Champaign.

In his cybernetics class Ashby introduced us to information theory proper, not just in [Claude] Shannon's terms, but also to quantify the complexity that analysts would have to cope with. The more information data contain the more difficult it is to theorize them. My dissertation in content analysis included a chapter which is still, to me, conceptually, I think, a key to understand social research—namely, that data have to be generated to represent the differences in the phenomena of analytical interest and inform the research questions one pursues. However, what the analyzed data mean derived largely from knowing the coding instructions that observers or coders employed, or the known makeup of the measuring instruments used in the process. Data had to have the required amount of information about the phenomena of interest, preserve it in the cause of their analysis, and select the conclusion

[22] Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

drawn from them. This was the information theoretical grounding of my dissertation on what content analysts can infer from analyzed text.[23]

So, I conceived all empirical research as a process of information transmission. In the process of any analysis, irrelevant distinctions are ignored, the information in data becomes simplified, more abstractly represented, for example as indices, in the form of correlations, most of which went beyond the linear Shannon model of communication. Ashby added to Shannon's conceptions quantifications of the kind of complexities that systems analysts faced, had to partition into analytically meaningful components and relate to each other. Shannon's conceptions became the most elementary parts of the decomposition of quantifiable complexities. Already in my dissertation, I developed an information theory, a qualitative information theory that nevertheless involved measuring the information in data in bits and distinguishing how many bits are extracted from an outside, unknown, and complex world, transformed into analyzable data and then slowly transformed into a form able to select among the answers to a given research question.

As an aside—I don't know if you know the origin of the Institute for Communications Research (ICR) at the University of Illinois. It was founded by Wilbur Schramm. He came from [the University of] Iowa, where he had hoped to change the department of journalism into one of communication, largely because he thought writing for newspapers is just a very small part of the skills needed to contribute to radio, television, and other media. He was ahead of its time, did not succeed in Iowa, was appointed to head the University of Illinois Press, but accepted that position only under the condition that he could develop a department of communication research [a Division of Communication, including the Institute of Communications Research].

One of the developments he [Schramm] saw as providing a new and scientific foundation for communication research was information theory. Shannon developed his information theory during the war but could not publish it until 1948 in the form of journal articles. In 1949 Schramm published Shannon's information theory [*The Mathematical Theory of Communication*], preceded by an interpretation by Warren Weaver. It's also interesting how Weaver got into it: There was a biology lab in New York, which sponsored an advanced scientific discussion group. The head of this group—I forgot his name—had heard of Shannon's work and asked Weaver to report on that to the group. From what I was told, Weaver struggled through Shannon's conceptions, missed the concept of meanings in Shannon's quantifications but reported his struggles to the group. Wilbur Schramm sensed the possibility of a new scientific foundation for conceptualizing communications and published the Shannon-Weaver book at the University of Illinois Press.

So Shannon was always part of the discussion at the ICR. But to several people, me included, the strictly linear conception of Shannon's theory was severely limiting. There is nothing wrong with asking how a sender conveys information to a receiver, how noise interferes with accurate

[23] Klaus Krippendorff, "An Examination of Content Analysis: A Proposal for a General Framework and an Information Calculus for Message Analytic Situations" (PhD diss., University of Illinois, 1967), http://repository.upenn.edu/asc_papers/250.

transmission and how redundancy can overcome in part the noise otherwise disturbing. Charles Osgood suggested some minor modification of Shannon's communication conception, but it was Ross Ashby who used Shannon's measures to quantify more complex systems conceptions. Ashby was fundamentally interested in conceptualizing how the brain coped with its uncertain world, described it as an adaptive system facing complexities it would have to reduce to manageable quantities. For him, the issue of transmission of information from A to B was less important than how a brain coped with seemingly unmanageable complexities and by implication of how researchers could quantitatively analyze complex systems.

Ashby integrated Shannon's information theory into cybernetic conceptions of complex systems. Ashby's Law of Requisite Variety generalized one of Shannon theorems, stating the quantitative requirement of an adaptive systems to survive in an environment of disturbances to which it had to find appropriate responses. It stated that for such systems to survive the threats from their environment, it had to generate at least as much variety as present in the environmental threats to their essential variable.

Whereas Shannon's measures were concerned largely with linear transmission of information, Ashby developed information measures to analyze complex systems, consisting of multiple interacting variables. Their analysis amounted to decompose complex systems into smaller interacting parts that could be described in simpler terms. For this purpose he developed multi-variable information measures that enabled the analysts of apparently complex systems to recognize parts that are independent of each other or transmitted small amounts of information between them and variables whose interactions defied further decomposition and had to be described whole. While the information transmitted between loosely connected parts of complex systems could easily be quantified by measures resembling Shannon's, the interactions between several variables of non-decomposable parts of complex systems turned out to defy Shannon's measures. Ashby defined interaction measures intended to quantify non-decomposable complexities. The beauty of the information calculus that Ashby developed was that it provided the analysts with accounting equation that summed the entropies in parts plus the information transmitted between them into the entropy of the whole system. However, this accounting equation included one kind of measure, the Q-measure of the interaction within non-decomposable multi-variable parts that behaved oddly, in the sense of being sometimes positive and sometimes negative. When positive, it made sense to identify it as a measure of the complexity that the individual variables of the part could not explain. When negative, Ashby explained it as a measure of the extent to which the variables of the part overdetermined its complexity. To me that Q-measure seemed an artifact of the need to preserve that the accounting equation of all the quantities sum to the total, but it was never clear what it actually said of the part it measured. A few years after Ashby developed his information calculus and we all used it for analytical purposes, I discovered the cause of my uneasiness with that measure. I realized that Ashby's calculus did not embrace what he always instilled to his students, not to ignore the circularities in complex systems. The Q-measure went outside Shannon's linear conception of information and had no place for that.

This insight came to me when I started to learn computer programming and had to think in terms of iterative loops. To get to a program for analyzing the entropies of multi-variable systems into additive quantities, I had to develop an iterative algorithm that started from given multi-variable probability distributions, partitioned them into parts, calculated their circular interactions between them—sets of linear connections being a special case—adjusting the probability distributions for each part for what their interaction do not explain, etc. At that time Sage had asked me to write a book on information theory. While I felt obligated to start reviewing the classical information theory, I could present that newly gained insight into measuring the complexity of systems in general terms, whether the relations among parts involved circular dependencies or not. This book had passed the test of time. 35 years after its publication it is still in press.

I used computer programs to prove its worth but could not go much further. Someone in computer science at the University of Pennsylvania, who became a student of mine, wrote a dissertation making use of this approach as well. He wrote a computer program to analyze systems with more variables than I could. Unfortunately, it was not portable. At the end of my ropes, I received National Science Foundation funding to develop a more general software, hired someone who was more enthusiastic about the project than capable and in the end did not succeed. I made the limited program I wrote available to several scholars. I am pleased that one researcher, Martin Zwick, devoted much of his academic career to develop it further, including all the measures I had developed.

You mentioned [George] Klir. Klir was a systems theorist, and he was—where was it?—anyway, Albany, I believe [*sic*: Binghamton University]. At a General Systems Society meeting he presented a paper that proposed another method of decomposing systems build on Ashby's constraint analysis. I observed that his diagrams of possible decompositions omitted all systems with circular connections. I took this as a prototypical demonstration of where cybernetics and general systems theory historically differ. I knew Ashby's constraint analysis and had just struggled with measuring circular information flows, so I challenged him to a debate after which I developed what he was missing. My decomposition of complex systems, including feedback loops, was published in a yearbook of the General Systems Society.[24]

Actually, the last big paper I wrote on information theory was a review of Ashby's work. In it, I followed up on a question Ashby had posed and answered. While most empirical research measure their object of attention, Ashby did this too but almost always with other possibilities in mind. In fact, he defined cybernetics as the study of the dynamics of all conceivable systems, information of their existence or possible realization being only secondary to examining them. Yet, there are limits. As information theory can be described as quantifying limits of communication, it made sense for Ashby to inquire about the limits of complexity we can face on earth. I think most people would say it is infinite or inaccessible. Ashby started with the

[24] Klaus Krippendorff. "On the Identification of Structures in Multi-variate Data by the Spectral Analysis of Relations," in Brian R. Gaines (Ed.), *General Systems Research: A Science, A Methodology, A Technology* (Louisville, KY: Society for General Systems Research, 1979), 82–91, http://repository.upenn.edu/asc_papers/207.

hypothetical: Suppose we transform the whole mass of the Earth into the most sophisticated computational hardware we know, how much can we compute? We know the mass of the earth. We know the time since the earth solidified. Combined with Bremermann's limit of the ability to observe atomic changes he concluded that everything material cannot exceed 10 to the power of 95 bits. We can imagine systems far more complex than that but not compute them within the material resources and time conceivable. While this is a highly theoretical quantity, it encourages us to be more humble in conceptualizing our ability to construct the world.

My background as a designer encouraged me to ask how this ability grew historically from creating primitive tools to distinguishing building blocks for constructing ancient Egyptian pyramids, alphabetical characters in libraries to what the internet can currently house. In 2008 this growing number was 10 to the power of 38 bits per year. This number, far smaller than Ashby's limit, allowed me to examine the spaces occupied by the artifacts on the internet. These are our current computational limits. There are other limits, for example [Werner] Heisenberg's uncertainty principle and to spell out what one can possibly observe. I weaved these limits into my paper on the capacity of the Internet.[25] And it was fun.

Q: Well, we're not going to have time to go into detail about cybernetics—a related theme—but one thread that we can pick up is Ashby himself, who informed both of these areas for you. And in particular, at one point you mentioned going to a conference in 1972 and learning about Ashby and his health.

KRIPPENDORFF: Well in 1972 I attended a cybernetics conference at Oxford, England, and presented a paper reporting on the above-mentioned efforts of decomposing complex systems in terms of information theory. A keynote speaker, [William] Grey Walter, a British cybernetician known for his development of an automaton that could find [its way] through mazes and so [on] mentioned in passing that Ashby is as good as dead; he has a brain tumor. I was just shocked not only because of his characterization of another person but also this other person was my teacher. I couldn't get over this news and shared my feelings with another conference participant from Switzerland, named [Christof] Burckhardt who had been Ashby's student as well, unknown to me. So we decided we have to go to see Ashby. And so we took a train from Oxford to Birmingham, if I recall correctly, to visit him. We made an appointment by telephone, talked to his wife, and when we arrived, she came out of their house to greet us but tell us to be careful about what we say. He does not know of his diagnosis that this is the end. We don't want to rock the boat, and so on and so on.

It was truly disheartening to see a brilliant scholar who was my teacher in Illinois, from where he retired to work from home. Much of his academic work centered on understanding the brain. Now he had a fatal brain tumor and was prevented from knowing his fate. So we didn't have very much time to talk with him, but I gave him my paper about information theory, which

[25] Klaus Krippendorff, "Ross Ashby's Information Theory: A Bit of History, Some Solutions to Problems, and What We Face Today," *International Journal of General Systems* 38, no. 2 (2009): 189–212, https://repository.upenn.edu/asc_papers/237.

continued his work. He thanked me and said that studying it will have to wait until he is better. I appreciated his intention, knowing that it would never happen. We continued with small talk during the few minutes we had with him. It was disheartening. We probably were the last scholars and former students of his who saw him alive. In retrospect, having made that tip and saying goodbye to our mentor at the moment he was no longer the creative scholar he always was, gave us a sense of closure we wouldn't have had after merely hearing Grey Walter's devastating assessment. Our visit brought me also to an intellectual closure. After all, the paper that I presented at this conference and of which I gave him a copy was a continuation of Ashby's work. I am sure he would have been proud to see his work developing.

Q: Well, in keeping with cybernetics, knowing that we can't talk about the full theme today, I thought we could return to Annenberg, where we started today, which is about a class you taught right away, I think, very early on anyway, on your arrival, which was *Cybernetics and Society*. And I wondered if you could just talk about your approach to the class and also just as a way of talking about your teaching in general, at least at that time—you know, this class *Cybernetics and Society* and your teaching.

KRIPPENDORFF: As I was saying earlier, when I came to the Annenberg School in 1964 I was ABD [All But Dissertation] and actually not a professor—I was a research associate. As soon as I had defended my dissertation, I became an assistant professor and I taught three courses: One on *Content Analysis*, the other on *Models of Communication*, to which I added the course entitled *Cybernetics and Society*. The *Models of Communication* was basically an introduction to communication conceptions, largely informed by cybernetics. I chose this approach not only because it was rich of ideas but because I thought the simple conception of mass communication—institutionalized senders producing messages for undifferentiated masses of docile receivers—never appealed to me. I was always interested in relationships between people, how institutions emerge and are maintained in communication between their constituents, and what they do. My *Models of Communication* course, taken by many students, was an introduction to various communication theories, blending qualitative and quantitative approaches as well as introducing cybernetic conceptions of feedback and systems conceptions larger than found in the mass communication literature. After this introductory course was settled—actually, already in 1965, I decided to expand cybernetic conceptions to address larger social phenomena. In *Cybernetics and Society* I looked into the cybernetic mechanisms that make a society a society. And as you said, maybe we should talk about that at some later point. But I think the *Models of Communication* was probably the course that many students took as an introduction to information, communication, and cybernetics at the Annenberg School in contrast with Annenberg's initial emphasis on media, television, writing, graphics, etc. At that time there was very little attention to more general theories of communication, which the *Models* course started, and the *Cybernetics and Society* course expanded.

Q: Maybe that gives us a chance to just chat about your general approach to teaching at the time and whether your, you know, interaction with graduate students—I presume there were master's and PhD students at the time—in *Models*, in *Cybernetics and Society*: how you approached that, if your teaching informed your work at the time or vice versa—just about teaching in general.

KRIPPENDORFF: Well, let me first say we didn't have a PhD program—that came much later, I forgot exactly when—I think in 1968 or something—we could check the date of adding the PhD program to the MA program in the catalogs. Although Gerbner was given the charge of eventually developing a PhD program, we didn't have the faculty to do that. Many of the teachers of labs we inherited—there were six of them—didn't have a PhD and could therefore not be advisors of PhD dissertations. These were the head of the television lab [Paul Desard?]—who was a brilliant teacher, commuting from New York and working with two other teaching assistants in television production—[Lou Glassman?], head of the graphics lab. He was a magazine editor and commuted from New York as well, later replaced by Samuel , a Philadelphia public artist. There was an instructor of the writing lab, all devoted teachers but without a PhD. Charles Hoban came from education, taught a history of communication research course and had a PhD, and so did Hiram Haydn who took over the writing lab shortly after Gerbner became the dean of the Annenberg School. It took a while to hire faculty that enabled us to have a PhD program. The sociologist Rolf Meyersohn and Percy Tannenbaum, one of the first graduates from ICR, tipped the scale toward launching our PhD program. To these came several secondary appointments, Seymour Mandelbaum from History, William Kelly from Marketing, and Julian Wolpert from Regional Science, who contributed to the curriculum.

My *Models of Communication* course was designed to generalize communication, transcending traditional disciplinary notions of communication and push students to think differently about the media. I should also note that students were not always satisfied with the emerging academic curriculum. For once, an academic institution cannot keep up with the changing production technologies employed in the industry. Our equipment was quickly outdated. So we had to redefine our labs, not to train students to produce for a medium but to teach the principles of communication underlying the production of messages and their analysis. My courses were, actually, precisely trying to do that. So I had, actually, good resonance with a lot of students, and also embracing cybernetic notions.

Jim [James] Taylor, for example, was an early PhD candidate, who had started to teach in the television laboratory. He became interested in circularity and self-reference, which is what I was discussing in my *Models* course. He asked, What do you see when seeing yourself? So, we designed an experiment in which we directed a TV camera on the image it transmits. One may think that nothing happens. However, if your portrait is on the TV screen to start, focusing a TV camera on its initial image of you, then you become either smaller and shrink into nothingness or become magnified, an increasing smaller part of you occupies the whole screen until your identity disappears in an unrecognizable detail. Such experiments with TV technologies were fun. But Jim was interested in organizational communication. One summer he was hired by the Philadelphia Museum of Art where he was given the task of exploring what would make the museum more attractive in the city. He looked into all the means through which the museum communicates with the public. His work expanded on concepts discussed in the *Cybernetics and Society* course.

I taught *Models of Communication* for many years. And at some point I thought that there were other things I wanted to tackle. But *Cybernetics and Society* stayed alive longer. Actually, my move from *Models* to *Cybernetics and Society* was paralleled by adding to my *Content Analysis* a more advanced course on *Message Systems Analysis,* that looked beyond the traditional content analyses of media, literature, and letters to the larger flow of information in systems, constituting the networks within which institutions grow. In effect, this more quantitative course complemented the qualitative issues discussed in *Cybernetics and Society*. Also, out of *Content Analysis*, I developed another course, *Semantic Analysis*, which looked into different kinds of meaning systems. It relied on anthropological approaches to studying different meanings—[Ward] Goodenough was one source that I could rely on. I didn't like Umberto Eco for his commitment to a representational theory of meanings, but he had other insights that made it worth discussing some of his work. Actually, right now Lisa Henderson is at the Annenberg School. She took my semantics course and mentioned it recently as still having major influences on the way she approached things. So, I think my teaching left a lot of things for others to develop on their own and grow with. This is all that matters to me.

## END OF SESSION THREE

# Transcript (modified) of Interview conducted April 12, 2017, with KLAUS KRIPPENDORFF (session four)

Philadelphia, PA

Interviewed by Jefferson Pooley

***Note:*** *This modified transcript was significantly edited by Klaus Krippendorff. The original transcript, synced to the video interview, may be reviewed at [https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field](https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field).*

Q: This is day four of an oral history interview with Klaus Krippendorff conducted by Jefferson Pooley in Dr. Krippendorff's home in Philadelphia. The interview is part of the Oral History Project of the Annenberg Library Archives of the Annenberg School for Communication at the University of Pennsylvania, and the date is April 12, 2017. So, welcome Klaus. I thought today we might trace your journey through cybernetics. We've touched on it a little bit in the past, but could you talk about your encounters with Ross Ashby in particular?

KRIPPENDORFF: Well, as I was saying at some point my first hearing about cybernetics was as a student at my design school [Ulm School of Design, Ulm, Germany], where one teacher, Horst Rittel, tried to move designers away from designing little products to looking at larger systems, and cybernetics was part of it. And then, as I mentioned, at some point I was in Oxford in the summer of 1959, where I bought two books at Blackwell, in the bookstore of the famous publisher. And without knowing, they accompanied my academic trajectory. One was [Ludwig] Wittgenstein's *Tractatus* [*Logico-Philosophicus*], and the other one was W. Ross Ashby's *[An] Introduction to Cybernetics*. I had heard of Ashby in Ulm and getting more familiar with cybernetics made sense. I vaguely knew Wittgenstein's name, but his book was attractive because it had the German and English text side by side. I spent that summer as an intern in the design department of an Oxford company in the hope to gain some fluency in English. Naïve as I was, I thought Wittgenstein's book would be a good way to improve my written English [laughs]—certainly one cannot really learn English from a philosopher.

However, when I came to the United States one of the incentives of moving from Princeton University to the University of Illinois [at Urbana-Champaign] was that Ashby happened to be there and taught a one-year course on cybernetics. With his book in hand, I took that course as soon as I could. I should say that the Institute for Communications Research [ICR] was very interdisciplinary. Its faculty came from different departments of the university. I was surprised and very pleased that my academic advisor, a linguist, hearing about my intention to take

Ashby's course, was very supportive of the idea, although it was taught in the engineering department. Most students at ICR took courses in the social sciences. After telling him about what I learned there, he was very impressed and encouraged other communication students to take it as well. So, I was the first but not the only one. Ashby used his 1956 *Introduction* as the primary text but in the meantime he had done much additional work and guided us through other related publications as well.

The basic concepts addressed in the course were, first of all, an appreciation of complex systems; second, attention to the occurrence of circular causality; third, that circular causal systems have behavior that linear accounts could not capture. They either converge to stable or repetitive behaviors or explode. Although systems that accelerate to the point of their self-destruction are often viewed negatively, and may not last long to be studied, Ashby pointed out that accelerations may yield novel systems. Evolution relies on processes during which new forms develop. The fourth topic was his attention to variety, as he called what information theories quantified. A fifth issue, not independent of the second, was the question of how systems regulated their essential variables, counteracting challenges from their environments or changing their structures more or less radically to preserve their identity.

These topics sound very abstract. One of his motivations, starting before he came to Urbana, was to understand the brain—not the way cognitive scientists approach it now—but in relation to its environment. This enabled me to relate his explorations to social organizations which always interface with what is outside of them. One of the concepts he developed was that of an ultra-stable system, a system that could reorganize itself after encountering that routine adaptive responses to changes in its environment would no longer suffice and prevent breakdown. While he saw the ability of a system to shift to an alternative approach as what a human brain does, this conception was not limited to biological phenomena. His generalizations were usually stated in the form of mathematically supported propositions that could be applied to numerous empirical domains. In the case of ultra-stable systems, he proposed a Law of Requisite Variety, suggesting that any—in his term, regulator—viable system has to have at least as much variety as the disturbance it faces from its environment. So there again the issue of variety was part of it.

Ashby's attention to variety, alternatives, options, and choices appeared in many of his conceptions. Inasmuch as variety is also underlying processes of communication, it became natural for him to turn to [Claude] Shannon's measures, which essentially quantify the probabilities of alternatives and distinguish between the quantities of uncertainty in senders and receivers, the information transmitted between them, and the redundancy and noise in the channel. Expressed in information theoretical terms, Ashby's frequently cited Law of Requisite Variety turned out to be a generalization of Shannon's Tenth Theorem, which concerns the limit of extracting information from noisy communication channels. For Ashby, simple communication channels were not that interesting. It was the additivity of uncertainty measures that enabled him to apply Shannon's quantities to the analytical decomposition of larger systems. So he developed a general calculus of uncertainty and information measures applicable to analyze complex systems by decomposing them onto weekly connected parts.

I already discussed my effort of applying Ashby's calculus to analyzing all kinds of multivariate data, my frustration with his Q-measure of interaction between three or more parts, how I identified its cause, and my encounter with George Klir at a 1976 meeting of the Society for General Systems Research. The standard practice of systems theorist was to describe complex wholes as a network of its part and depict their pairwise connections by lines. Klir's proposal, applying Ashby's constraint analysis, did just this. In his enumeration of possible networks that his analysis was to reveal, I noticed the absence of circular connections. When three or more parts are connected it makes a great difference of whether these pairwise connections are simply independent of each other or form a circle in which one connection affects another and ultimately itself, introducing a complexity that escapes simple decompositions. I saw the omission of circularities typical of how systems theorists view systems, fundamentally unlike how cyberneticians do. I didn't want to merely criticize Klir's approach but show the reality of that difference.

However, at that time I struggled to make sense of the oddity of Ashby's Q-measures of interaction. I solved this problem by means of an algorithm that started from a multivariate probability distributions, calculated the circular complexity of the whole system to the extent it could not be explained by lower order circularities, then recomputed the probability distributions minus what the identities complexities explained, and did this repeatedly until everything was accounted for. I wrote a paper of this analytical method.[26] I think generalizing information measures to embrace circular complexities that Shannon had not addressed, and Ashby could not solve, is the contribution I made to information theory. My 1986 book on information theory presented a solution. To my surprise it was widely accepted and is still in press.[27]

To demonstrate the concepts Ashby wanted us to understand, he built models to convince us. Once he wanted to show us our limits of understanding systems by observation. He brought two black boxes into the class, assured us that their output is determined by their output. We could select their inputs by pressing buttons and were asked to predict their outputs in the form of lights. The task was seemingly simple as the number of options were small. The behavior of the first box turned out to be simple to learn. As soon as we had repeated our choices we could predict their consequences. The second box, similar in appearance, defied successful predictions—they seemed to respond almost randomly. Our inability to understand that box was due to its design of responding not only to our input but also a previous input, which squared the number of connections we would have to learn. Heinz von Foerster took this simple demonstration as the basis of a fundamental difference between trivial and non-trivial machines. Trivial machines involve direct input-output relationships—non-trivial contain a loop inside relating past and present input states to what they do. This distinction offered us a solid critique of behaviorist conceptions of human being, in effect trivializing their human nature.

[26] Klaus Krippendorff, "Information of Interactions in Complex Systems," *International Journal of General Systems* 38, no. 6 (2009): 669–80, https://repository.upenn.edu/asc_papers/334.

[27] Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

Ashby had been a psychiatrist before he tried to understand what brains do, how they adapt to their environment, how intelligence is manifest, and how the human interior organs are regulated. Building models and then observing what they do got him not only to discount fundamental misconceptions but allowed him to speak to all systems one could possibly examine, whether they are cognitive, physiological, technological, or social, and he discussed these models always in relation to cyberneticians as designers and analysts.

Although Ashby's mechanical models had their limitations, being deterministic in nature, this uniquely cybernetic approach of not just describing observations, rather involving cyberneticians in the process of understanding the world as builders, observers, and explorers of the dynamic consequences of their constructions gave cybernetics a different epistemological foundation. Gregory Bateson recognized the novelty of Ashby's epistemology—actually Ashby never used the word epistemology—and interpreted Ashby's cybernetics as an evolutionary epistemology. Unlike the epistemology of the traditional sciences, cybernetics leaves the space of possibilities open, providing mainly negative explanations, what cannot be observed, constructed, realized, and experienced. Bateson linked Ashby's cybernetic as translating Darwin's evolutionary theory of biological species to the evolution of knowledge. He [Bateson] was one of the few scholars who recognized the importance of cybernetics in understanding social and cultural phenomena.

When I came to the Annenberg School in 1964, besides teaching the more traditional methodology of *Content Analysis*, the aim of my course on *Models of Communication* was to introduce students to cybernetic alternatives, different conceptions of models, how one can describe them, and what one can learn from exploring complex systems in various empirical domains. In my follow-up course on *Cybernetics and Society* I tried to apply these principles to large social systems.

I think my mission was really to apply cybernetics to my own work, and also to communication as a discipline. I joined the International Communication Association [ICA], I think, in 1966 or something—that was before I joined the American Society for Cybernetics. At Annenberg as well as at ICA the field of communication research was, and I would say, still is not entirely circumscribed. Not that I had answers, but in the spirit of cybernetics, I presented a paper that sought to overcome one widely accepted division of the field of communication research promoted by the political scientist [Harold] Lasswell. He defined communication as the study of 'Who' 'Says What' 'In Which Channel' 'To Whom' and 'With What Effect.' On the surface this embraces much of verbal communication. However, he continued to assign each of these five parts to different independent inquiries. 'Who' defines research of the nature of authors. 'Says What' is content analysis. 'In Which Channel' is media research, and 'To Whom' is audience research, etc. I thought, dividing communication research into five separate parts, each pursued by experts in their incommensurate methodologies, blinds communication researchers from appreciating the complex systemic and dynamic nature of communication in society.

So, my paper, still very mathematical, suggested not to divide data in five independently researchable domains but consider them as representing one complex system to be analyzed for all complexities communication was involved with. My proposal of how to analyze such data benefitted of course from what I took from Ashby's course, respecting the complexities of systems, not to arbitrarily make them invisible. I presented this paper at what may have been my second ICA meeting. To my surprise, after submitting it to the *Journal of Communication*, the reviewers evaluated this paper as not worthy of publication. However, the journal editor said, I'm overruling the reviewers' assessment and I'll publish it. It was published in 1970 and earned the award of the best paper published in 1970 in this ICA journal.[28] Nobody knew of the difficulties the paper faced and the courage of the editor to override the assessment by his reviewers. Although I didn't even mention the word cybernetics in this paper, the struggles my paper evoked demonstrated the reluctant acceptance of cybernetic conceptions of complex systems and of quantities of information had among communication scholars. This has changed of course. Only last year a communication scholar from Canada approached me to tell me that a new book he was about to publish was built on this paper.

I joined the American Society for Cybernetics [ASC] right after it was founded. Warren McCulloch, then the ASC president, signed my membership card. The very first of several annual conferences I attended was in Gaithersburg [Maryland]. There I met Ashby, several cyberneticians I knew as a student in Urbana, and other scholars I recognized from their publications. Later some of my students joined the ASC. I am proud to have had excellent students that carried cybernetics into other areas. Students from city planning, decision sciences, the Wharton School of business, history, and nursing participated. Two of my students later became presidents of the ASC. This was an exciting time.

This, my first ASC conference, took place during the Vietnam War. Some participants were upset with the leadership of the ASC, which included many government employees. One stood up in loud protest when he learned that U.S. government had sponsored the conference. The CIA and the U.S. government supported cybernetics for fear that the Russians put more resources in developing cybernetics but for weapons development. There were in fact some Russians academicians at this conference. As individuals they were welcomed.

My most important take away from the first ASC conference was the keynote address by Margaret Mead. She was a well-known cultural anthropologist who had been part of the Josiah Macy, Jr. Foundation conferences on Cybernetics, I believe from 1946 to '53. She started to describe her experience of being part of this group that made cybernetics happen. She affirmed her excitement and narrated for the audience that at some point she didn't even notice that she lost a tooth during the proceedings, being overwhelmed with discovering important new insights. She was implicitly critical of Norbert Wiener, who as a mathematician had developed a self-referential mathematics which lend themselves to be realized in the form of circular causal mechanisms that maintained the determinisms of his mathematics. Mead's criticism of this

[28] Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

traditional preoccupation of cyberneticians was she wanted cyberneticians to shift gears to address what she saw as the more important issues cybernetics.

She did not review the original purpose of the Macy conference to develop the foundations for a "general science of the working of the human mind" and the adoption of cybernetic ideas by cognitive scientists, biologists, and communication scholars. She observed, though, that cybernetics is being implemented everywhere, largely in the form of automata in industry, computerized weapon systems in the military, and governmental procedures, affecting international relations. Obviously, in 1967 computer applications were far from where we are now. Digitalization began in small pockets of society and has now penetrated almost all spheres of like. But she recognized that many international processes, trading issues, the Cold War, were heavily influenced by the use of computers. She was worried that Soviet and American weapon systems are so automated that a small error could trigger unmeasurable disasters. Ignorant of where cybernetic technology could lead us, she invited cyberneticians to make the understanding of the consequences of automation and computation their primary target of inquiry—not just building cybernetic technologies, but doing so with their social and cultural implication in mind.

Her more important contribution was to suggest that cybernetics is not just a theory to explain and build cybernetic hardware. Cyberneticians should recognize that cybernetics is a language, not what that language is about. It is an interdisciplinary language by which cyberneticians solve problems that individuals could not. 40 years later, I dare to interpret what Mead referred to as an "interdisciplinary language" captured her experiences of participating in the interdisciplinary conversations practiced during the Macy Foundation conferences, conversations during which remarkable new concepts emerged that participants could not have developed on their own. Also, while its conference participants spoke mostly English, in contemporary terms I would say what was practiced during these conferences was a conversational discourse shaped by collectively defining terminologies unlike those of the discourses of other disciplines.

Phenomena of the kind, now considered of a cybernetic nature, had been recognized as odd in antiquity but were not understood until recently. During Wiener's times servomechanisms where already used in industry, but what emerged during the conversations at the Macy Foundation conferences were vocabularies, agreed upon definitions, and theories, placed under the umbrella term of cybernetics, the beginning of a discourse among creative scholars who were able to step out of their original disciplinary confinements neither could handle on their own. I am convinced that Mead, calling cybernetics an "interdisciplinary language," referred to these conversational discourse practices when she invited cyberneticians to be aware of these practices and switch gears from conceiving feedback mechanisms to making the circular conversational discourse the prime target of their inquiries.

Finally, Mead implied that the language of cybernetics, its conversational and interdisciplinary discourse, cannot have the effect it has without its practitioners who collaboratively maintain and improve it at every turn. That cyberneticians are indispensable parts of what their discourse brings forth was already anticipated by Ashby. But, whereas Ashby directed cyberneticians to study all conceivable systems, Mead added to Ashby's inclusion of the cybernetician in what is known as cybernetics the recognition that such examinations are undertaken in conversations, in discursive practices, which when communicated to others can evolve and create cybernetic technologies as well. That plainly suggested that this discourse and what it does elsewhere should be a primary concern of cyberneticians.

I have to say that listening to Mead's address 40 years ago, although I had studied cultural linguistics in Urbana, I did not fully understand the radical implications of her invitation to cyberneticians to turn to how they communicate among themselves and to others and assume responsibility of what their language enabled others to do. The significance of her suggestion for the future of cybernetics became clear to me much later. I also like to add that many hardnosed cyberneticians did not appreciate her radical proposal either and were largely ignorant about what their language enabled them to do. In teaching at a school for communication, to me, Mead's proposal merged with Ashby's and Bateson's ideas. I tried my best to pass on cybernetic ideas to my students, applied them to my own work, but also encouraged fellow communication scholars. I had joined the International Communication Association [ICA], in 1966 where I presented several papers with roots in information theory and cybernetics before joining the American Society for Cybernetics.

Q: Well, you know, that ICA paper, in some ways, was in the middle of a period when you were attending more of those conferences with the American Society for Cybernetics. There was a conference in particular in 1972 at Oxford where you encountered an ailing Ashby.

KRIPPENDORFF: Yes. Well, in 1972, actually, I had been working through the problems I saw in information theory, basically. At the Oxford conference on cybernetics, I presented a paper on the algorithm I had developed for decomposing the entropy in complex systems into additive measures of information flows between their parts and the interactions that information flows could not explain. The keynote address was presented by [William] Grey Walter, a British cybernetician—maybe I should say one thing before I come to his talk.

When Ashby retired to England he was asked to nominate someone that could replace him, and that was me. However, at that time in the Biological Computer Laboratory at the University of Illinois, headed by Heinz von Foerster, fell in disfavor, one could say. And it was in the process of being closed, and so the teaching of cybernetics was discontinued at the university. While it was formally housed in engineering, it attracted students from all kinds of other department like me from ICR and I was told, sadly, that the engineering department was not supportive of interdisciplinarity and nobody replaced Ashby. So, as Ashby's student I felt very close to him and had hoped to meet him at the conference.

Grey Walter was known for having designed an intelligent mouse that learned from mistakes in the process of finding its way through a maze, and he talked about intelligent machines as I recall. In passing he mentioned that Ashby was as good as dead. He had an advanced brain tumor—he is not expected to survive. It explained Ashby's absence from the conference. I was speechless. I happened to stand next to someone from Switzerland in a line to get food and I couldn't help sharing my feeling and there's nothing to be expected. After that talk, I was standing next to someone from Switzerland [Christof Burckhardt] and could help sharing with him my shock about the dismissive news we had just learned. It turned out he too was an Ashby student and we shared our sadness. We decided to visit Ross Ashby in Birmingham and took a train there. We were greeted by his wife in front of the house, she briefed us about his terminal condition, telling us he doesn't know of his imminent death and we should mention anything about that, as it would upset him. He would certainly like to talk with you. It may make him happy, but it had to be a very short visit as he would be quickly exhausted.

For me the sad part was that after we mentioned that we participated in the Oxford conference, I gave him a copy of my paper, which advanced some of his own work. He took it in his hand, looked at it, and said I will look at it a little later when I am feeling better. But he was, already, I wouldn't say somewhat incoherent. He mentioned his experiences when he was a British soldier a long time ago. So he was not really there anymore. It was sad to see him, whom we knew as a brilliant teacher, attentive listener, and full of ideas one had to take time to digest. I think the two of us were probably the last ones, at least the last cyberneticians, that saw him. It was important for me to say goodbye, thank him for his lasting influence, but we were unable to say so. It was a big loss for us but also for the whole community of cybernetician. But he did not remain invisible after his death. He had kept a log of all his thoughts during his whole career, the questions he wanted to find answers to, the analytical steps he took to answer them, ideas he was fascinated by. This log was discovered by his relatives. It consisted of several volumes of handwritten notes. His son wants to make them available but encountered some resistance from relatives as it also accounts personal struggles. For example, about his military service he felt very uncomfortable, his brief practice as a psychotherapist, becoming a brain researcher, etc. Earlier he had a small room, a closet space in their house where he had continued to work on cybernetic issues. But I do not know what came of it. All of his explorations that followed his retirement from the University of Illinois were shelved by his tumor.

Q: Well, going forward a couple of years at Annenberg, I think you organized a conference that was on "Communication and Control in Social Processes" and so, if you could just talk about how the idea came about, and what was significant about the event to you?

KRIPPENDORFF: Ok, after 1971 the annual conferences of the American Society for Cybernetics had stopped. I assume that government support for ICA [*sic*] meetings had dried out, and the community of cyberneticians lost a platform to exchange ideas. ASC had a president, Roy Hermann, who chaired ASC's Board of Directors, all of whom lived in Washington, DC, and met occasionally. In the absence of any academic events, its active membership had shrunk, and that Board of Directors did not plan any initiative. In addition, the ASC leadership was to be elected annually but nobody had been asked for a vote for several years. The board members had mostly government jobs. I wasn't even sure what their connection to cybernetics was. In the absence of ASC activities, several cyberneticians teaching in the Philadelphia area met frequently to discuss their work. Some taught at Drexel University, one at Villanova University, another at Lehigh University, and several joined from the University of Pennsylvania's Decision Sciences and Social Systems Science [departments of the Wharton School] and from the Annenberg School for Communication. We created a supportive momentum of diverse interests in cybernetics, which was not visible in Washington.

So, in 1974, I proposed to organize an ASC conference in Philadelphia. The board was initially skeptical, as none of us occupied and elected position at ASC, but then consented. The Annenberg School provided the rooms and the ASC was supposed to publicize the conference, get people interested to attend, and provide logistical support. A group of several professors from diverse schools at the University of Pennsylvania worked on a program, invited speakers that we thought could speak to social applications of cybernetics, who in turn made other suggestions. The Decision Sciences department, which is concerned with decision-making in business and politics, provided new dimensions, and I managed to get several communication scholars to attend. Heinz von Foerster from the University of Illinois came with a busload of his students. In the fall of 1974 we had what I considered to be a stimulating conference, entitled *Communication and Control in Society*. Subsequently I edited the presentations into a book.[29] It covered a diversity of areas demonstrating what cybernetics can contribute to the creation of knowledge, the modeling of social systems, large and small, economic controls and instabilities in international relations.

Unfortunately, the ASC board in Washington contributed little. For one, of its members only the ASC president attended. The conference was well attended but mainly by people that the presenters brought with them. There had been no publicity as promised. The presentation were to be published and ASC agreed to hire someone to record them. But Roy Herrmann instructed the sound engineer that ASC had hired to tape only what followed each presentation. Anatol Rapoport's presentation was not written. I had to transcribe its recording. But when I listened to the tape I was shocked to hear only a sequence of applauses and the announcement of the next speaker. I still have the tape and its total uselessness was far from funny. I still have it. Luckily someone had taped Rapoport's speech. I could work from that recording and let him edit it afterwards. Also, the ASC board promised to publish the edited contributions. However, after I completed all the editorial work I was told they had not lined up anyone to publish the book. I had to find a publisher on my own, which was not easy, as most publishers want to have some say in the book's organization. In 1979, Gordon and Breach was pleased to publish it. It has since become a must-read book in the social application of cybernetics, a snapshot of cybernetics at that time. Its title echoed Wiener's definition of cybernetics but considered 'control' not in the sense of forcing people to do things they would not do otherwise, but to understand social processes in a variety of area as enacting circular dependencies, negative feedback accounting for converging to stabilities, equivalences, and balances, or positive feedback causing instabilities, creating inequities, magnifying social differences if not leading to breakdowns.

[29] Klaus Krippendorff, ed., *Communication and Control in Society* (New York: Gordon and Breach, 1979).

Obviously, our Philadelphia group was elated with the conference but not with how ASC lived up to its promised support. At the conference a membership meeting was held to which everyone was invited. I did not have the time to attend, but ASC's incompetence was an issue and that its current president failed to schedule elections. Our loosely organized Philadelphia group was given no voice. So after the conference we decided to form our own society, called it the American Cybernetics Association (ACA). We agreed that the problem with the ASC was due to its hierarchal organization, occupied largely by government employees who enjoyed the privileges of their title but did little to understand much less to serve the community of cyberneticians. Most of us were teaching at universities and engaged in research. With lots of suggestions from our Philadelphia friends I wrote democratic bylaws that interestingly included the position of an ombudsman, taking what the U.S. Constitution regrettably excluded, journalism as a fourth estate. We did not formally adopt these bylaws and did not elect a president. We merely hoped that our effort would challenge ASC and it did. During our conference, Hermann promised to hold an election, which selected Barry Clemson, a starting assistant professor at the University of Maryland. I knew him well. We shared our assessment of the current ASC leadership. I didn't tell him that our group consisted only of about eight members, didn't have the intention to secede, but promised to reunite with ASC if they adopted our more democratic bylaws. Barry scheduled a vote and all 31 remaining ASC members were in favor of adopting them and so ASC was not merely saved, it gained the ability to expand.

After the conference had focused on social applications of cybernetics, and resumed annual conferences, it attracted a new kind of member, family therapists. Gregory Bateson had paved the way for this interest. He was dissatisfied with individual therapy, which targets psychological or mental dysfunctionalities as the root cause of individuals' inability to cope with everyday social life. Bateson was clear that all mental illnesses are identified in the language of psychotherapist but often resulted from interpersonal communications, which could drive people into distress and hopelessness. He wrote a paper on pathologies of communication, which could lead people into untenable emotional states, not necessarily mental illnesses. Not only would individual therapy be unable to identify such causes of conditions, but treating them as mental illnesses, talking patients into believing its diagnosis, would not eliminate what caused it.

For example, when a mother assures her child that it is being loved and punishment for misbehavior is for its own good, that a child would have difficulty reconciling the two assertions. While Bateson was criticized for speculating that schizophrenia could come from living with such paradoxical communications, the therapeutic value to involve the families of so-called mental patients became increasingly recognized. For example, almost all cases of someone starting individual therapy is preceded by family members or close acquaintances identifying that person as the black sheep in need of psychological help, as the 'identified mental patient,' unaware or not considering that their communication could have caused this identification. In reality, most families decide who among them is the odd person, the misfit, crazy, mentally ill, and in need of psychiatric treatment. Bateson was correct in making the communication pattern within families responsible for such assessments and suggested that the whole family of the so-called identified patient to come to see a family therapist. Family therapy, as this approach came to be called, usually comes to conclusions that individual therapists could not approximate, much less "cure."

I already mentioned Bateson's embrace of Ashby's cybernetics. Bateson's family therapy went far further than Ashby could. Bateson was not interested in the complexity of systems and was not a determinist. Rather he suggested to look at patterns of verbal communication in which their members were defined and treated. Family system therapy gained considerable traction. Philadelphia's Children Hospital had a Child Guidance Clinic that was committed to practicing family therapy. Several family therapists attended ASC meetings, not just at Philadelphia. They made important contributions in moving cybernetics away from building mechanical models and explaining social phenomena in deterministic terms. One student of mine, Mariaelena Bartesaghi, wrote an excellent dissertation after spending several years at the Philadelphia Child Guidance Clinic observing family therapists in action and interviewing the participating family members after their sessions. Another PhD student, Charles Goodwin, who was hired as a videographer, wrote his dissertation about conversation and analyzed taped therapeutic and other less structured conversations.

Before going back to your question about the 1974 conference, let me provide some context. Margaret Mead's above-mentioned keynote, suggesting that cyberneticians focus their attention on what their discourse does elsewhere, was delivered eight years earlier. Meanwhile Gregory Bateson advocating family systems theory before individual therapy, had entered several conference presentations. Both backed the idea that language use is not merely descriptive but participates in the construction of cybernetic realities. This, while to me obvious, was not generally acknowledged by cyberneticians at that time. Even some family therapists embraced Humberto Maturana's work, who as a biological determinist merely went as far as acknowledging that humans live in language. This acknowledgement, while it challenged objectivist scientists, was never enough for me because Maturana linked language exclusively to observations.

Back to the conference: Its first day ended with a dinner for all attendees at the faculty club of the University of Pennsylvania. I had invited Heinz von Foerster to give the keynote. Mind you he had been what we now call a cognitive scientist by training. He headed the Biological Computer Laboratory at the University of Illinois where Ashby had taught, Maturana had been one of several important visitors, and cybernetics was the primary focus. In his talk, von Foerster suggested to baptize Maturana's proposition "Anything said is said by an observer" as "Humbert Maturana's Theorem Number One," to which he proposed an addition, naming it "Heinz von Foerster's Corollary Number One: Anything said is said to an observer." To von Foerster, this corollary established the link between cybernetics and society, which our conference was addressing. It made a lot of sense. That everything said is said to someone else is also fundamental to human communication—after all, language is a social phenomenon.

Then von Foerster proceeded to ask the logically necessary follow-up question concerning the defining properties of observers. For him, observers are fundamentally concerned with describing their observations. He was aware of Bertrand Russell's theories of logical types, which created unresolvable paradoxes when describers enter their description, for example when "Epimenides, the Cretan says that 'all Cretans are liars'"—we wouldn't know whether Epimenides was lying or telling the truth. He claimed that these paradoxes have been resolved recursively, which led him to his main distinction between first-order cybernetics, the cybernetics of observed systems, and second-order cybernetics, the cybernetics of observing systems which includes the observer. This distinction has split subsequent generations of cyberneticians into two camps: those with ontology concerns—with what exists independent of observers—and those with epistemology concerns—with what is observed or experienced and described as such by someone.

I am disappointed with this distinction for two reasons. First, von Foerster privileged observations which are limited to presently experienced phenomena, cognitive or psychological in conception. While acknowledging that anything said by one observer is said to another observer recognized the intersubjectivity of descriptions, opposes the abstract-objectivist conception of language, but kept second-order cybernetics stuck in understanding the observations of cyberneticians. Second, von Foerster defines second-order cybernetics as the "cybernetic of cybernetics," the title of Margaret Mead's famous 1967 keynote to ASC, without mentioning her in subsequent publications. He once mentioned to me that she wasn't clear of what she was talking about. I think von Foerster's preference for cognitive explanations prevented him from understanding Mead's anthropological/linguistic perspective, inviting cyberneticians to be aware of what their language was doing. Anyhow, after this conference, second-order cybernetics became a leading concept. Many family therapists identified themselves as practicing second-order family therapy, the main innovation being to see themselves as part of the therapeutic practices.

Incidentally, the acknowledgement that cyberneticians are constitutive parts of what their examinations yields was already articulated by Ashby. True, imagining systems, building models of them, examining their dynamics, and generalizing their properties cannot possibly be understood without the creativity of cyberneticians. Ashby's cyberneticians were not stuck with describing observations. Whether a system exists, can evolve or be built was secondary to what cybernetics was about. Similarly, therapists do not merely observe their patients, they intervene to aid their patients. Elected politicians create laws for the benefit of their constituencies. It is fair to generalize from these few examples that all uses of language have observable consequences—they participate in the construction of realities.

This reminds me of a personal experience where the role of the theorist and the awareness of the consequences of using language became an issue for me. At a meeting of the International Communication Association (ICA), I was invited to a working group dedicated to comparing communication theories. Participants brought different examples of published, transcribed, informal accounts, or tapes of communicating individuals to the table. I did this too. But as our discussion progressed, I was surprised that everyone privileged their own conception of communication. We dealt with communication as an abstraction. I became frustrated that nobody considered what might have been the observed communicators' conceptions of communication they were enacting—as if their conceptions were all alike, absent, or not worth considering. I thought this was unconscionable. I argued that without communicators enacting their conception of communication, we wouldn't observe communication. Although some participants agreed that our theory of human communication needed to include the theories that communicators enacted, I became even more annoyed when members of our group projected their own conceptions of communication onto the observed communicators' cognition, as if there was only one, our way, to conceptualize communication. I called that evidence of intellectual imperialism that communication scholars should not exercise. I wrote a paper condemning the practice of communication researchers assuming the role of intellectually superior observers and reducing observed others to trivial machines merely responding to messages. I am suggesting that social phenomena should not be theorized in terms of scholarly convenient abstractions that deny their constituents a voice in how they are theorized.[30] Undoubtedly, communication scholars have more freedoms to reflect on what ordinary people say in response to what they hear from moment to moments, while getting on with their lives. von Foerster's second-order cybernetics sought to and succeeded in nudging cyberneticians away from mindless objectivism. But it didn't go much further.

There was another lesson I carried from cybernetics and my increasing awareness of the consequences of language into communication research. I forgot where I published it. It is fair to say that the significance of all scientific theories tends to be measured by what their readers can do with them. In the natural sciences, they may stimulate more research, critique, or outright dismissal. The life of social theories may follow additional paths. Some readers may use them for improving or discontinuing their business practices, introducing so-called unanticipated consequences. In communication research, those theorized therein may enjoy the attention awarded to them and go for what the theory generalizes about them or find their representation despicable and change their behavior into the opposite, thus either amplifying the validity of published theories or invalidating them. In the social sciences, these two responses to published theories are called self-validating or self-denying theories. In cybernetic terms they trigger either negative or deviation, reducing feedback, or positive or deviation, amplifying feedback. In either case, to the extent social cybernetic theories enter the lives of their stakeholders' lives through the medium of language, their validity might well change right in front of their observing theorists' eyes.

As early as I recognized the obvious point that theories are formulated by theorists, that [human] communication is constituted by individuals engaging each other verbally, that at least in the social domain, theories enter the communication practices of their stakeholders and change, and that banking on second-order cybernetics of observers describing their observation leaves us stuck in our own conceptions—my notion of conversation emerged in response to the above-mentioned intellectual imperialism. But it was also nourished by my students. To me, teaching is always a dialogical process where participants learn from each other. Let me mention three students who very early on nudged me in that direction. The earliest one was

[30] Klaus Krippendorff, "Conversation or Intellectual lmperialism in Comparing Communication Theories," *Communication Theory* 3 no. 3 (1993): 252–66, https://repository.upenn.edu/asc_papers/257/.

John [Henry] Clippinger [Jr.] who wrote his dissertation on conversation. I lost track of him. The second one was Chuck [Charles] Goodwin. While a student, he worked as a videographer at the above-mentioned Child Guidance Clinic. He had a chance to record conversations in family therapy sessions and among friends. Whereas most conversation analysts rely on transcripts, visual clues brought him new insights. I remember difficulties getting him through the Annenberg School because conversation was not mass communication. To overcome these difficulties, I invited Bill [William] Labov from the linguistics [department] and Ward Goodenough from anthropology, powerful scholars, sympathetic to Chuck's topic. He passed with flying colors, published his dissertation as a book and is a professor at UCLA. The third student is Mariaelena Bartesaghi, who collected her data not only at family therapy sessions, but also by interviewing therapists and their clients after each session. Her dissertation showed how therapists established their authorities and constructed treatable realities that their clients did not always buy into.

Q: So you've described in some ways papers that were published, in a couple of cases, like your turn to conversation, maybe even in the early 1990s and I'm wondering: It seemed to me, anyway, from that period in the mid-70s, when von Foerster talked about second-order cybernetics, when you were reflecting on Margaret Mead, that there was a period, at least in the published stuff you had, in which you only started to kind of talk about the implications of this for the observer being reconceptualized in the early 80s, maybe. And so I wonder if you could just talk about the process of kind of coming to social constructionism over time—you know, beginning in, maybe, that mid-70s period, but maybe back to Ashby in some ways too, through to when you really became a kind of full-fledged social constructivist.

KRIPPENDORFF: Don't forget, I had a background in design from an avant-garde school that stirred us away from designing industrial products and architecture to being concerned with larger systems. So, looking out for what can be changed to the better has a long history for me. Then came my exposure to Ashby, who built models of phenomena he wanted to understand and demonstrated principles not yet in the common vocabularies. Having been married to someone from East Bengal, which got into trouble in 1971, got me involved with Pennsylvania Quakers, who taught me what the combination of words and non-violent actions can accomplish. As I already mentioned, it became increasingly clear to me that social theories do not merely generalize observations but can change, even create, realities when enacted by their readers. For me, the idea that we are constructing and continuously reconstructing our world, had a long and multifaceted history. When I had to organize the 1984 ICA conference, communication technologies began to grow beyond comprehension, and I saw it as an opportunity for communication scholars to reinvent themselves for the future. As outgoing ICA president, I had to give a major address at the general assembly and took this to be an opportunity to spell out five imperatives I saw as important for the future of communication research. I am attributed to have been the first outgoing president who delivered an academic address. Unfortunately, the previous speaker had taken up too much time, preventing me to present my suggestions in full, but it was published.[31]

The first imperative I proposed goes back to Giambattista Vico, an eighteenth-century philosopher, whose anti-Descartian scholarship was based on the insight that we cannot understand what is, only what we have created. He was talking of culture, governmental institutions, technologies, and art. Today, he might have added scientific experiments, which reveal the consequences of how they are set up and generate analyzable data—one would not find without being made observable. Realizing that theories are not discovered but made echoed Ashby's definition of cybernetics as the study of all imaginable systems, whether they have been observed, are constituted by their members (as for social systems) or planned.

The second and empirical imperative suggests to invent as many alternative constructions as you can and enact them to experience the constraints on their viability. It goes back into Gregory Bateson's evolutionary interpretation of Ashby's cybernetics for the social sciences. Whereas mechanical systems are deterministic in the sense of being predictable from their current states. Of social conceptions we tend to be more certain of what cannot work rather then what could. It counters the ontological assumption governing most natural sciences that only one theory can be true and alternative explanations are considered inconsistent and motivate the search to find a singular truth. Social phenomena involving human agents in communication are rarely so predictable. Conversations, for example, are not repetitive. Their focus of attention evolves and are recognized as the most common source of innovations. Admittedly, not all social phenomena are that open. However, communication is always associated with creating and reducing uncertainties. Communication scholarship is well advised not to follow the natural sciences, rather exploring the empirical constraints on what cannot happen, leaving alternative interpretations open.

The third and self-referential imperative states—include yourself as a constituent of your own constructions. I cited Heisenberg's uncertainty principle, who recognized that even in physics, the act of observation changes what is observed. In the social world, interviews rarely reveal what interviewees are thinking, only how they answer the interviewer's questions. Interviews tend to be conducted by a certified interviewer and someone agreeing to answer but not to ask questions. This inequality cannot be explained away when interpreting the data so obtained.

The fourth and ethical imperative, builds on the third: Grant others that occur in your constructions at least the same capabilities that you employ in constructing them. It was meant to discourage communication researchers from constructing theorized others in trivial terms, terms they would not dream to apply to themselves. That practice is widespread in the social sciences. For example, Michel Foucault theorized discourse as a regimen that might change over time but at each moment determined what authors write, excluding him from this

[31] Klaus Krippendorff, "On the Ethics of Constructing Communication," presidential address delivered at the International Communication Association Conference on "Paradigm Dialogues," May 23–27, 1985, Honolulu, Hawaii, http://repository.upenn.edu/asc_papers/275.

determinism. More related to communication research, our content analyzing of violence on television was motivated by the assumption that the mere frequency of exposure to violent shows would cause violent acts to become legitimized and enacted. The proponents of this hypothesis considered themselves immune to such causes. Undoubtedly, television viewers have cognitive abilities and speak to each other. I consider the praxis of trivializing their mentalities for the purpose of a theory unethical. Theories of human communication should be applicable to the theorists as well.

The fifth and social imperative reads, When communicating, preserve or open new possibilities for others to respond. In conversations, opening or maintaining possibilities to respond to what is said is a condition for its continuation. For example, raising a question for which you do not know the answer leaves addressees possibilities to respond. The opposite, issuing a command that a subordinate is expected to follow, renders that subordinate as a choiceless follower, which redefines communication as an issue of control. Similarly, interviewing members of mass media audiences assumes their ability to select among predefined answers to prepared questions but builds this asymmetry in the generated data. The social imperative was to avoid such enforced inequalities. It suggests that theories of human communication should be applicable to the theorists as well, recognizing the preference for communication theories that when enacted would open possibilities not imagined before.

I'm still proud of having proposed these imperatives for communication researchers. But they fell short of what became a far more important reference point for conceptualizing communication and that was the ideal of conversation. I had supervised two dissertations on conversation analysis, which did not, however settle my problem of when can we consider conversation to be authentic. I forgot now the year—but then I started exploring its nature, asking whether there was such a thing as authentic conversation. And writing about this, asking what can we say about authentic conversation as opposed to constrained conversation? I argued for several propositions that could identify authentic conversation: One, which derived from cybernetics, is that conversations are self-organizing or autonomous. It is a closed system of people that converse, bringing with them all they know and are capable of contributing. They have to speak for themselves, not in the name of absent others, authorities, gurus, or people whom they represent. Participants take turns in being attentive listeners and responsive speakers, creating an interactive reality that respects all contributions equally. When one speaker dominates a conversation leaving the other participants in the role of listeners, the conversation has become a monologue, as when delivering a lecture. Similarly, when one participant starts to manage a conversation, the conversation has become a faculty meeting or one of numerous business institutions.

It follows that genuine conversations develop in their own terms. Although the vocabularies used in conversations may have common meanings to start, in the course of conversations, vocabularies are constantly redefined, used in different contexts and modified by mutual consent. Conversations do not repeat the sequence of interactions. As everything said is said in response to something said previously, what happens in genuine conversations is always new and unanticipated—at least in its details.

Another characteristic of genuine conversation is that participants respect each other's contributions. As soon as someone calls a response out of order, moderates the taking of turns, or imposes a purpose on what a meeting is to achieve, like solving a particular problem, the genuineness of conversation is lost. Meetings that have an agenda, set by a superior the assembly of people, are not self-organizing.

The motivation of genuine conversations cannot be anything other than continuing the conversation, a process in which participants provide each other spaces to participate. In reality, of course, every communication comes to a physical end. Someone dies. Someone has to go do other things. But the point of a genuine conversation is that it can be continued in principle. If it ends by someone feeling prevented to speak his or her mind, or in violence, then it was not a genuine conversation. So the whole purpose of conversation is to stay in conversation.

I developed, I think, nine propositions for what should count as a genuine conversation. I don't want to describe them here. While in everyday life we do not often engage in genuine conversations—Martin Buber speaks of experiencing "dialogical moments." I suggest we invoke the concept of a genuine conversation as a reference when noticing deviations from this ideal, for example, when someone talks too much, cuts someone short, describes other participants in stereotypical terms, talks about them in third person pronouns, etc. Genuine conversation doesn't require that everyone speak the same amount of time—easily measured by quantitively inclined conversation analysts—genuine conversation requires only that every participant feels free to participate.

If participants announce—and I'm thinking now of, for example, committee meeting of different faculties—that they could not offer an opinion about what is being discussed until they cleared it with their department, these participants do not speak for themselves but in the name of departments absent from the deliberations. Similarly, when someone claims to have data he or she is unwilling to share, refers to a higher authority which cannot be addressed during such meetings, or speaks for absent others, this makes it very difficult to come to a consensus unless issues are uncontroversial or inconsequential. So, I would say that participants' invoking privileged access to authorities derails conversations.

In a representational democracy, politicians are meant to represent their constituencies. Unless they are assured to have their constituencies' total backing, the fear of not being reelected often determines politicians' debates and voting. This is what makes solving problems in parliaments difficult. When politicians speak for their constituencies, they try to weight their arguments against each other by the size of their respective constituencies. The larger their constituencies the more power they claim. This conversation-killing strategy is common in many non-political situations. Credibly speaking for the poor, for disadvantaged women, for potential customers, or unborn generations is common in board meetings, design teams, civic action groups. Such places may well retain some elements of conversations but drawing absent others into ongoing interactions are far from genuine conversation.

Conversations are constituted by participants responding to each other's responses. John Shotter introduced me to a very important additional conception of these interactions, that of human agency. It is taken for granted in conversations. All interaction requires actors. For Shotter, agency should not be taken for granted. Indeed, we never know why people do what they do unless we ask them. Such questions can arise in conversations as well and are answered in terms of giving accounts for why we behave the way we do. His accountability was built on performative conceptions of language or speech act theory. It came to me when spending a semester living with him in Durham while teaching at the University of New Hampshire. He argued that in conversations but also in other situations, everyone is faced with having to answer questions like "what do you mean by that," "what evidence do you have for your claim," "why did you do this," including being blamed as unfair, insulting, and prejudiced. Shotter developed a reflexive conception of human agency by noting that we rarely ever speak or do something in the presence of others without having in mind what we would say if held accountable for what we did or said. Accountability is a way of making oneself and the actions one proposed or performs meaningful to others who may accept, reject, or modify such accounts, after which they can become social reality.

There are three kinds—four, actually, four kinds of accounts, roughly. One are explanations. Explanations follow questions like: What do you mean by that? and are given when something said or done was not fully clear or understandable by those present. A second account is justifications—for example, for the virtue of a proposed action. Proponents of plans of actions may offer justifications while proposing something to be done or in response to questions about its costs and benefits. A third kind of account are apologies, which are offered when a speaker admits having caused some harm and promises not to repeat it. The fourth kind are excuses, offered when something untoward happened but the speaker denies being responsible for it. There is a literature of finer distinctions among accounts, but these four are to me basic acknowledgements of where the human agency is practiced and made meaningful to others. While explanations add missing meanings to what was said or happened, justifications and apologies accept a speaker's agency, the former being proud of the latter regretting its consequence. Excuses, by contrast deny agency in the situation in question.

Shotter's concept of human agency gelled with another area which was equally important to me. It was the sociologist C. Wright Mills' research into how power is exercised in business and politics. In 1956, he published his findings in a book on *The Power Elite* in the United States. Unfortunately, he died very young and he didn't really complete, so to speak, his work. To find answers to how power was exercised in practice, he spent time in board meeting of corporations, analyzed policy debates in Congress, and interviewed members of financial institutions. He concluded that the exercise of power rarely conforms to the common conception of powerful persons who have the resources to force others into submission but is based on proving motivations for plans of actions that are judged beneficial and executable by those willing and capable of realizing them. He didn't call that process accountability but revealed the communicative nature of how things are accomplished and what makes people consent to be part of larger efforts. His concepts of motivations given to convince others are essentially justifications, which are not causal in nature but subject of consensual modifications that could ultimately proceed.

The distinction of accounted human agency and taken-for-granted human behavior became to me an important epistemological distinction in understanding what happens when the genuineness of conversations is threatened by abnormal deviations. Some deviations can be "repaired," by asking and answering questions of why something was said or done and getting plausible answers. The point is that one never knows what others understand, why they do what they do, and how they expect others to respond, unless one asks questions of this nature and get appropriate answers. However, in practice we may assume we know what others understand and are up to, do not ask questions that would verify or correct our projections, and get into unrepairable difficulties, or more likely accept what conversations have eroded into.

So I wrote one paper and several derivatives of it starting with the criteria of genuine conversation and describing how it can erode into other forms of communication—by, for example, accepting someone as an authority, implying yielding to its opinion, accepting someone as the designated speaker, implying being a listener, using the Internet and limiting oneself to its options, speaking as a member of one discourse community and feeling incompetent in communicating in terms of another. This paper proposed a continuum between genuine conversation on one extreme and computation on another—that is between conversational openness on one extreme and algorithmic determinisms on the other. This continuum allowed me to distinguish technologically mediated conversations, formal rule-governed communication situations, communication within specialized discourse communities, and interfaces with computers.[32] Along this continuum, discourse occupied an important role.

Unlike Foucault, who conceives of discourse as an inescapable universal regimen determining everything we say and write, I conceive of discourse as practiced by distinct discourse communities that have institutionalized specialized vocabularies and are focused on particular artifacts. For example, communication among physicists is limited by their specialized vocabulary, the standardized methods of inquiry practiced by physicists, the kind of arguments that are acceptable, and the theories physics is known for. The discourse of physics is not shared by, let's say, biologists, who are concerned with very different explanations. For physicists causal explanations are of the essence. For biologists the functions of body parts in living organisms are important sources of explaining how organisms persist. For physicists, functions do not exist. So I was fascinated by the notion of discourse and published several papers on how different discourse communities define themselves and how they see their own work.

The search for understanding discourse, describing different discourses, how people become members of different discourse communities through education, certification, and recognized

[32] Klaus Krippendorff. "Conversation: Possibilities of its Repair and Descent into Discourse and Computation," *Constructivist Foundations* 4, no. 3 (2009): 135–47, http://repository.upenn.edu/asc_papers/134.

practices, including how people can cross discursive boundaries, being teachers in one, researchers in another, and subjects in still others, takes in a sense a meta-perspective on discourse. In taking this position, I couldn't help taking cybernetics into that discussion.

If you go into the literature and ask how discourse is defined you'll read merely references to talk and text. To me such definitions are very unsatisfactory. From a cybernetic point of view, talk and text is not only a way of different communities to distinguish themselves from one another, practicing different vocabularies, have different consequences. I now increasingly say, language does not just represent realities, they construct realities. Every discourse constructs their own artifacts. Scientists propose theories, but physicist construct theories unlike biologists do. Engineers design objects that can be realized and work. The medical discourse enables doctors to identify symptoms that lead to treatment of illnesses. Historians write histories using their own criteria of qualification. Every discourse creates its own artifacts and controls their creation and reproduction. One could say that the discourse-specific artifacts that discourses create and leave behind for others to build on have material dimensions. After all, physical theories are published on paper, airplanes are built, medical patients are healed. However, these physical dimensions have different meanings at different times, for different people practicing different discourses depending on what their members do with them. It is remarkable that members of scientific discourse communities are rarely aware that the artifacts they create don't exist without them.

There is one experience that I like to mention. I was in England talking about discourse in the above-mentioned terms when a physicist by the name of Andrew Pickering approached me and declared: You have it all wrong. I was surprised. Pickering had written about the history cybernetics, but introduced himself as a physicist and evidently was committed to the epistemology that physicists have adopted to justify their artifacts. I am not sure I could convince him, but I presented him the example of the famous Hadron accelerator [Large Hadron Collider] in Cern, Switzerland: This is the biggest experimental set-up in the world. It is a circular structure underneath the surface of the earth of 27 km in diameter, crossing the boundaries between Switzerland and France. Its idea had energized many physicists, took several years to build, costed millions of dollars, and employs a large number of researchers. Why was it built? A Nobel laureate physicist by the name of Peter Higgs had hypothesized a particle that would make existing atomic physical theories consistent and could explain how the physical world was built. This particle came to be called the "God particle." According to his theory, this particle could have a lifespan of only a few seconds and would not exist on Earth. The Hadron accelerator was built to create this particle and it succeeded.

Now, by all definitions, anything that does not exist naturally but is created by human efforts is an artifact. It was a theory- or discourse-driven artifact, a plan of actions that was executed with an enormous effort and produced what was intended. Creating the God particle, or Higgs bosom by its scientific name, is just one example of demonstrating that scientific theories, here created in the discourse of physics, direct human activities to produce the artifacts that validate the theory. This circularity is what cybernetics addresses, but physicists have difficulties seeing.

Such circularities are at home with wherever language and actions go together. I was surprised that Pickering wasn't willing to see this connection.

In my seminar on language and the social constructions of realities, I am using a book by Ludwik Fleck, who traced the history of syphilis from its earliest notions to its current scientific conception. This book preceded Thomas Kuhn's *Structure of Scientific Revolutions* by 30 years, and I think he took many ideas from Fleck's *Genesis and Development of a Scientific Fact*. Fleck convincingly demonstrated that scientific facts have histories. They are not found and picked up for what they are. The earliest explanations of syphilis, in 1400-something, of a vague but painful disease, was that it was "obviously" caused by a distinct astrological configuration, followed by being God's punishment of carnal sins. Neither explanation was open to scientific knowledge of the nature of the disease but did not prevent searches for relieving pain. Fleck found the first rudimentary scientific response to the disease pharmaceutical in nature, prescribing mercury cream. At that time, mercury was a remedy widely applied to all kinds of bodily issues and syphilis was not yet distinguished from several other related illnesses.

Only by opposing astrological and religious explanations could scientific explorations begin to search for possible causes of the disease. Yet the first hypotheses focused on contaminations of the blood. At that time "bad blood" was a widely held preconception of the cause of many illnesses. Biologists started to compare the blood of people who had syphilis and those that did not. But scientific efforts to find the culprit acquired a political dimension. In Germany the minister of—I don't know if it was science—but a Prussian minister [Friedrich Althoff] realized that the French were ahead of Germans in syphilis research, and he picked out a biologist named [August] Wassermann, promised him all the financial support he would need to advance syphilis research beyond what the French had discovered so far. This exemplifies science not to be immune to national politics [laughs]. Wassermann was an experienced experimenter. He developed seemingly successful tests to identifying syphilis-affected patients, but they later on were found wrong-headed. Their successes were due to the testers' intuitions. Nevertheless, Wasserman's research established serology as an important branch in the biology research of blood.

Without going into further details, Fleck made clear that the development of the scientific fact of syphilis was far from being a straight line of stepwise improvements of as many scientific disciplines claim their artifacts to have evolved, and while all scientific discourses present their current state of knowledge as being the final truth, this can hardly be claimed. Oh, I might want to mention two more things about Fleck. First, he was Polish, wrote in German, but had difficulties finding a publisher for his manuscript, largely because the scientific discourse between the two World Wars was dominated by the Vienna Circle of positivists whose conception of objectivity had no place for cultural ground of science. He eventually found a small Swiss publisher who in 1934 published seven hundred copies of it, very few of which ended up in non-German speaking countries. Second, Fleck was Jewish. In 1939, when the Nazis came to Poland, they deported him and his family to a Jewish ghetto. But then they realized he was an important biologist with expertise in typhus and syphilis. So they asked him whether he would be willing to develop something against syphilis for Aryans.

Today such a proposition would of course be ridiculous [laughs]. I am sure Fleck knew it, but recognizing the seriousness of the antisemitism, he said yes, was given laboratories in several concentration camps and survived the war. After the war, he became head of a microbiology department in Lublin, Poland, published extensively, retired to Israel where he died.

While Fleck's conception of thought styles and thought collectives avoided the role of language and communication, which I am stressing, he described the development of discursive artifacts, as I am calling them, as an ongoing evolutionary process. At any one time, the artifacts of a discourse may appear to have arrived at their final form, but they are never immune to be questioned, redesigned, recontextualized, or replaced for a newer generating of artifacts. I am encouraging my students not to take our current social, technological, or academic artifacts for final. Recognizing their discursively constructed histories should make it obvious that they can be questioned, reexamined, if not by us than by future generations of scholars.

Besides the evolution of scientific facts, exemplified by Fleck, I am arguing that members of discourse communities not only share a thought style, as Fleck describes their commonalities of a period, they also institutionalize practices that are indigenous to them. To become a medical doctor requires rigorous academic and practical training, keeping up with advances in medicine by reading medical research journals, demonstrate competencies in using available technologies to the other members of their community. I have said that all discourse communities institutionalize their recurrent practices, whether they generate lawyers, politicians, or physicists. It is these institutionalized practices that unite the members of a discourse community and distinguish one from another.

So that was, in a nutshell, my conception of discourse, and it leads me again to ask how it relates to conversation. By adopting rules of conduct, membership qualifications, and highly specialized vocabularies, discourses are certainly far removed from conversations, for once by being able to coordinate the activities of many more people than could possibly participate in genuine conversations, jointly creating artifacts of greater complexity, and providing incomes. Discourses demand of their practitioners a significant amount of conformity, restricting their human agency to what a discourse community deems appropriate. Members of discourse communities tend to accept such limitations for the benefits that conformity provides them.

Discourses also exhibit repetitions that would not occur in conversations. A hospital treats very many patients, some with the same illnesses. Lawyers specialize in particular areas and their arguments in court rely on precedencies. Repetitions invite the use of technologies that operationalize what may have started as uniquely human abilities, which mechanize certain routine discursive practices. I have asked myself what happens when conversations erode into discourses which are very organized, using specialized vocabularies, institutionalizing formal rules of conduct, including the use of communication technologies, and are geared to create specialized artifacts. These artifacts do not need to be physical, like building bridges which engineers do among many other devices, but they can be medical like curing illnesses, juridical like deciding whether a crime was committed, commercial like selling goods on the market, or scientific like establishing the validity of theories.

There is another feature I should have mentioned in conjunction with discourse which is very important. Every discourse institutionalizes its recurrent practices. That means what you have to do again and again can becomes a methodology. A methodology is written for every member of a discourse community to follow. When you become a student in the social sciences you have to acquire statistical skills, be able to perform standardized experiments, make reliable measurements and so on. Demonstrating the ability to handle the methodologies of a discourse tends to be required to become a certified member of a discourse community.

Any discourse normalized its institutionalized practices, which define its discourse community and distinguish one discourse community from all others that institutionalized other practices. To me, regarding institutionalized practices two things are important to recognize. One is that institutionalized practices limit human agency. As a member of a discourse community you have to do what everyone expects you to do. The other is that institutionalized recurrent practice that can be performed without thinking can often be replaced by mechanism. In the 1950s, when I was a research assistant at an institute for visual perception, I remember we had one member on our team who could calculate a factor analysis. All he had was a desk calculator and it took him many hours of repetitions to come to a result. None of us was able to do it. Even now, very few people could do such calculations by hand, and nobody would aspire to try because we have software that gives us almost instantaneous results. Very many recurrent discursive practices can be delegated to a computer and doing so renders discourses more efficient but also less human.

I'd say, most discourse communities are eager to replace routinely performed practices by algorithms when possible. For example, physics has developed standardized measuring instruments whose validity is no longer questioned. Its theories are constructed on top of them. Basic transactions at banks can be accomplished at teller machines. Online airplane reservations have become common. A few years ago you went to a travel agent who worked by telephone until he could issue you a ticket. Now, doing it on your own and online gives the airline the ability to determine the size of the airplane to fly its customers. You shop online, which triggers a nearly automated process that delivers the needed products to your doorsteps. We can use software to complete annual tax returns. Departments of city governments, designed to respond to citizens' concerns, to the extent their concerns are repetitive, employ automated telephone answering systems meant to lead a caller to the answers of predefined questions. Bureaucracies whose employees are largely rule governed have a good chance to become digitized automata. Replacing institutionalized social practices by computers was feasible, transforms past human communication into algorithms of automata. While this is a sea change for routine discursive practices, it shifts the human population to other forms of communication.

Coming back to Margaret Mead's keynote, she did not live to see these technological transformations, but recognized even then the danger of automating the war machines in the United States and In the Soviet Union. Any small error could have non-stoppable catastrophic consequences. She made two recommendations to the cyberneticians present at that meeting which have become increasingly important to me. One was for cyberneticians to focus on the social consequences of cybernetics, not what cybernetics created—feedback mechanisms, digital computers, and automata—but what people, institutions, and governments do with them. It entails a conception of language that is not merely descriptive of observed facts and proposed designs but what cybernetic technology enables others to do. It meant studying language, written discourse not as text, as containing something—through a content analysis, if you want. But to ask, what social consequences cybernetic knowledge gives rise to. The second recommendation was to redefine cybernetics not as theorizing circular causal mechanisms, as conceived of by Norbert Wiener, but as the dialogical practice she experienced during the Macy Foundation Conferences during which cybernetics was born, namely as an interdisciplinary language, I would say conversation, in which social problems could be addressed that nobody else could handle. She mentioned the eye-opening experiences of multiple feedback in these proceedings, during which scholars from diverse disciplines added to each other's understanding what none could envision on their own. In my reading, she wanted this dialogical practice to distinguish cybernetics from what other discourses are about. In terms of the continuum between conversation and computation, cybernetics would have to be located closer to conversation, practicing circular communication while conceptualizing and implementing it elsewhere.

Q: Well, you know, that the complexity of the trajectory you took, including this rich description of conversation in discourse, in some ways leads me to ask about how you institutionalized this in the classroom at Annenberg, in particular this class that you've been teaching that's based on these constructivist ideas that in some ways trace their roots back to Margaret Mead, if you will, and have cybernetics, of a certain sort, underneath them. What about the *Social Construction of Reality* class—when did it first to start? I'm curious about that, and I'm also wondering how your increasingly constructivist view of things was met by colleagues, given that most other social scientists have a kind of lay epistemology that would find that threatening, perhaps. And so, just how that played out given your teaching and your supervision of doctoral students and that sort of thing?

KRIPPENDORFF: Well, how did it play out? Speaking of doctoral students, as I was mentioning, Charles Goodwin had very great difficulties with defending his dissertation using and developing conversation analysis. He had to have three members from the Annenberg faculty. One didn't like the topic. Because I managed to get two highly respected scholars into the dissertation committee, Ward Goodenough from anthropology and Bill Labov from linguistics, he passed with flying colors.

Similarly, another student, Mariaelena Bartesaghi, who wanted to write a dissertation on family systems therapy, an approach to therapy promoted by Gregory Bateson among others. She asked to observe the process at Philadelphia's Child Guidance Clinic but was hired without pay to make exit interview of clients. Therapists were interested to know whether the clients would come back. So, she was given the privilege of observing the therapeutic process from behind a one-way mirror, able to listen to the comments made among the observing therapists about what they saw happening and being able to hear from the families how they thought their sessions went.

She collected an amazing wealth of empirical data of these dual views of what the therapists—or triple view [laughs], one could say. Her dissertation analyzed how therapists established their authorities, their difficulties in eliciting the stories of why families came to therapy and how they reinterpreted the families' narratives so that they could provide meaningful interventions. Correlating these three sources revealed how the therapist managed to elicit hidden stories that the families lived with, the therapists' effort to reframe them so as to be able to suggest interventions, and what the families took away from these sessions. However, already the dissertation proposal was badly received. One committee member—and I don't want to name my colleague—insisted that this PhD candidate was not a therapist, not even a psychologist, and was not qualified to write about this topic. From the candidate's perspective, which I shared, she was not interested in contributing to family systems theory, but to explore how conversations were constrained by a discourse, in her case by family systems therapists eager to contribute to the well-being of families but in the therapists' terms. The lack of support from one member of her proposed dissertation committee caused personal problems for the candidate and delays, but resulted in a remarkable dissertation—parts of it ended in separate publications.

But now coming to my course on language and the social construction of realities that you asked me about. I already mentioned that I had a graduate degree in design, directed my attention to how things came about rather what can be found. To this came an influential graduate course in linguistic anthropology, which showed me how different cultures cope with similar phenomena differently. Add to this Ashby's definition of cybernetics as the study of the dynamic of any conceivable system. Whether it is imagined, existing in nature, or constituted by the actions of human beings is secondary to this study. I already mentioned that all discourse communities create discourse-specific artifacts that come into being in the language used to make sense of them. Scientific discourse communities create theories, medical discourse communities categorize illnesses and find cures. Political discourses create political parties, governments, and revolutions. None of these phenomena come about without language.

So I wanted to teach a course that explores how language participates in creating the realities we live in. It actually developed and ultimately replaced my *Cybernetics and Society* course, which was not that language-oriented. It integrated three threads. One was John Austin's speech act theory, which explored the conditions under which declarations, commitments, and commands change their recipients' actions. Another was the sociology of knowledge in the form of Peter Berger and Thomas Luckmann's *The Social Construction of Reality* [1966]. They argued that all concepts have originators, but their histories tend to be forgotten, taking the present form for granted and as real—much as the natural sciences theorize their present observations. This applies to how people categorize each other and how such categories are being institutionalized and practiced. I wanted to go beyond the construction of social reality and include my insights about how scientific discourse communities construct their facts, or artifacts in my terms. The natural sciences, for example, disclaim having anything to do with what they merely describe, yet so-called laws of nature do not grow on trees. Earlier, I described the example of producing the so-called God particle that physicists continue to claim to have been found, completely ignoring the herculean effort to build a machinery that could produce it, following a theory in the discourse of physics. Without that language it could not have been created.

In my view, the course opens a broad perspective that integrates several conceptions central to the study of communication. I proposed this course to the faculty—as you mentioned—the year after George Gerbner had stepped down as dean. I was surprised that Gerbner wrote a two-page memo in opposition to it, which I still have, essentially arguing that this is just one way of looking at reality, relativizing the traditional ontological view of the world he was more comfortable with. He was not wrong, of course, in noting my opposition to the objectivist God's eye view of the world he practiced, criticizing the media industry as the dominant and falsifying force of the cultural sphere we occupy. I regret that a faculty meeting was not the place to sort out these epistemological differences.

Nevertheless, the course was approved, and I taught the seminar ever since with increasing fascination of the insights it offered me and my students. I usually begin by alerting students to rather different conceptions of language, after which we talk of qualitative concepts and methods to study them: metaphors, conversations, discourses, affordances of technologies, and information, to name but a few, that students are encouraged to employ as building blocks for their own work. Although I mentioned my earlier interest in [Claude] Shannon's information theory, here I am relying on Bateson's conception, who equated information with the differences that make a difference. After that, I encourage students to bring their own concerns into the deliberations. The origins of various social pathologies—racism, sexism, and terrorism are frequent topics which, while real, can be turned into questions of liberating oneself from being entrapped in present conceptions. A model for such analyses is the concept of power. I have written a couple of papers showing it not to exist without submission and largely the result of using inappropriate metaphors from physics that prevents recognizing ways to overcome oppression. Although I have a long list of topics to discuss, I am encouraging students to bring their own topics into the conversations that the seminar provides. Students from other departments of the university, from nursing, social work, city planning, education, the Wharton School of Business and linguistics have taken advantage of the seminar to discuss their own concerns. I was on several dissertation committees outside Annenberg regarding work that grew out of the seminar, and I have written many papers on what I learned while teaching this seminar.

I mentioned Gregory Bateson's notion of information. Unlike Shannon's conception, his involved human beings for whom not all possible differences mattered. He demonstrated this by breaking a piece of chalk into two pieces and argued that there are millions of other ways one could break the chalk into pieces, but they did not matter thereafter. From my constructivist perspective one cannot ignore that the difference that made a difference for Bateson wasn't there without his breaking the chalk into pieces, without his action. This recognition brought me back to cybernetics, which had a history of embracing Spencer Brown's laws of form, who argued that making distinctions was the most fundamental human ability and developed a mathematical logic of distinctions. Francisco Varela expanding his logic as a logic of actions. Consequently, one would have to say that all differences come about by drawing distinctions, which encouraged me to rephrase Bateson's conception of information as resulting from drawing distinctions that make a difference for an actor. I once wrote a paper, which you mentioned earlier, on the epistemology of communication, proposing that all conceptions of communications amount to drawing distinctions and explaining the differences thus created.[33] This applies to the distinction between senders and receivers, messages and their contexts, and responses to responses during conversations. This epistemological axiom was also underlying the analysis of systems into subsystems whose interdependence could be revealed only after decomposing them, their independence being a special case. I need to add that we may not always recognize our involvement in creating the differences we interpret as natural and obvious. Most of us would consider differences in colors as independent and external to us. However, there is sufficient evidence that even color perception has cultural histories that relate differences in colors to how we use the objects we associate in color terms. This is more obvious when it comes to distinctions among emotions, communication styles, ethnicities, social organizations, and ways to describe nature.

Now comes the issue, it doesn't stay with reporting. It has to do also, when these differences and relationships come into the public domain or are enacted. People act on the differences that they have been told, and that has an influence on whatever you observe. Now in, for example, racism, a good example. While there is a physical difference between blacks and whites, skin color, but now, how do you explain it? First of all you have to make a distinction.

Speaking of colors, not every culture makes the same distinctions. I have lot of Chinese students who do not consider themselves as looking yellow. I have never seen an African-American student who is pitch black or an American Indian student who was red-skinned. Racial stereotypes are enacted in language, and my view is that we have to hold each other accountable for drawing these distinctions. In my seminar on the social construction of realities, I am particularly concerned with social scientists being unaware or unwilling to consider being responsible for introducing social categories on the social world which may not exist without the theories creating them. In the history of economics, the notion of a market began as a mere metaphor, which today's business executives consider hard facts. Making ethnic distinctions in a published study can create real differences in a population. For example, if a questionnaire includes the ethnicity of the respondent, a statistical analysis of these questionnaires inevitably finds correlations with these categories. A particularly irresponsible example is the correlation found between the measured intelligence and the ethnicity of those tested, resulting in [Richard J.] Herrnstein and [Charles] Murray's infamous *The Bell Curve of Intelligence* [*sic*]. It provided empirical evidence for African-Americans to have lower intelligence coefficients than the remaining population and that this difference has not changed much over the years. The authors explained these correlations in genetic terms, implying that these differences have to be taken as unalterable biological facts. Publishing these findings renders African-Americans not merely distinguishable by the color of their skin but also by their intelligence.

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[33] Klaus Krippendorff, "An Epistemological Foundation for Communication," *Journal of Communication* 34, no. 3 (1984): 21–36, https://repository.upenn.edu/asc_papers/538.

Related to this widely distributed correlation, a student of mine, actually in my course on *Content Analysis*, designed a study of the effect of knowing the ethnicity of job applicants. She used numerous pairs of identical job applications, attaching to one a photograph of a white individual and to the other a black one and had executives indicate whether they would hire the applicant "based solely on their qualifications." It turned out as expected, black applicants were rarely hired. We do not know whether these executives knew of *The Bell Curve* or what they had in mind when they rejected black applicants overwhelmingly, but the belief that African-Americans have lower intelligence may well have been part of their motivation.

Regarding publishing scientific research, one should recognize that observed correlations between two variables are context insensitive. The authors' genetic interpretations of these correlations not only ignored the social and linguistic history of intelligence tests, but also the social consequences of publishing their findings. First, intelligence tests are based on the familiarity of vocabularies spoken largely in white, educated, and suburban populations. Second, the test does not measure the ability to cope with novel situations or collaborating with others, which is part of everyday understanding of how intelligence is manifest. Third, old prejudices have made access of the African-American population to good schools and higher education difficult, making it difficult for its members to have the same vocabularies and assume responsibilities afforded to the white population.

The correlations of *The Bell Curve* are not in doubt. However, publishing their irrelevant genetic interpretation as unquestionable facts can only further cast African-Americans in economically and intellectually inferior roles, offering no routes to escape from this construction. This is an example of ignoring the social consequences of social research, reporting differences as objective truths without taking responsibilities for the distinctions that created them and being unaware of supporting realities that deepen already existing differences when accepted as true.

Q: And they are self-validating in that sense.

KRIPPENDORFF: Self-validating. Again, a cybernetic circle.

Q: Right. So maybe this is a good place to stop by, in some ways, setting up our next conversation which is, you know, this 1984 paper you mentioned, where you really are talking about how a scholar's description of the world doubles back on the world. And those who are the descriptives then react to it. And, you imply, there's a kind of ethics of taking their reactions and contributions into account. And I'm just curious, since it turns out you are reviving your interest in design at right around this time, in the early 1980s, whether that cybernetics-infused idea about the observer and so on had anything to do with your conception of design as being more participatory. And so just as a quick kind of preview of next time.

KRIPPENDORFF: Yeah, that would be great. I think there is a connection with the design issues and cybernetics, social construction of realities. Yes, that would be nice to talk about that.

Q: OK, good.

KRIPPENDORFF: Thank you.

Q: Well, thank you very much. That concludes today's session.

KRIPPENDORFF: Thank you.

## END OF SESSION FOUR

# Transcript (modified) of Interview conducted May 17, 2017, with KLAUS KRIPPENDORFF (session five)

Philadelphia, PA

Interviewed by Jefferson Pooley

***Note:*** *This modified transcript was significantly edited by Klaus Krippendorff. The original transcript, synced to the video interview, may be reviewed at [https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field](https://www.asc.upenn.edu/research/centers/annenberg-school-communication-library-archives/collections/history-field).*

Q: This is day five of an oral history interview of Klaus Krippendorff, conducted by Jefferson Pooley in Dr. Krippendorff's home in Philadelphia. The interview is part of the Oral History Project of the Annenberg Library Archives of the Annenberg School for Communication at the University of Pennsylvania. The date is May 17, 2017. So, why don't we begin where it did begin for you, which is back in Ulm [Germany] and your experience at the design school there, in the late 1950s and early 1960s. I've learned over the course of these interviews that almost everything goes back to Ulm [School of Design]. You've talked about, in previous sessions, the range of intellectual experiences you've had there. In particular, I'm curious about looking through the lens of what you ended up writing on design issues in the 1980s through to the present. How much of it was there, in embryonic form, at Ulm—including in this thesis that you wrote in 1960 and 1961—how much was there from the beginning?

KRIPPENDORFF: Well, actually when I re-read something I wrote a long time ago—and I rarely ever do—my thesis at the Ulm School of Design turned out to be very instrumental in my subsequent development. I can now recognize many kernels of statements that I didn't really—couldn't back up at that time—but they seem to have stayed with me. The last time I reread it, I was wondering what [laughs] I really learned in the meantime. But this is of course just a, kind of, theoretical question. As I said previously, Ulm was an avant-garde school of design. It dealt with novel ideas that nobody else dealt with at that time. Personally, it overcame my lack of higher education consequent to the war and opened me up intellectually at a rate I never experienced before. To graduate, we had to write a theoretical or conceptual thesis and demonstrate our ability to complete and defend a practical design project. Regarding the design, I should have brought you a picture of what I did. I designed a motor grader. I don't know if you know what that is. Motor graders are big drivable construction vehicles that plow dirt roads or prepare surfaces for other uses. This was the biggest design undertaken in Ulm at that time and was awarded the first price for a student design by the Kulturbund der Deutschen Industrie—which one can describe as the cultural arm of the association of German industries.

And then I had to satisfy the theoretical part of the requirement for graduating from the Ulm School of design.[34] I chose a topic that was not entirely in line with what Ulm practiced and stood for. And that was interesting. I believe I was admitted as a student mainly because I had an engineering background, which would contribute to the functionalist philosophy of the school. However, my main motivation to apply to the Ulm school was to escape the determinism of engineering design which I had mastered but was not satisfied with. In retrospect, I think they accepted me for the wrong reasons. Everything I could pick up in Ulm directed me to a more human-centered approach to technology. In my thesis, I decided to explore the meanings of artifacts. The first step was to select a thesis advisor who would be sympathetic to my project. One candidate for this role was a faculty member named Tomás Maldonado, the director of the school at that time. Maldonado had come from South America, where he had been a painter. He had a knack of recognizing new ideas and was the first who introduced the notion of semiotics into a design curriculum.

Naturally, I went to him with my still unformed proposal of writing about the meaning of objects, artifacts, designs. But he turned out to be a very traditional semiotician. For him the meanings of all signs are their referents. This was more obvious for the design of visual communication, the department where he taught, which always amounts to creating images or using words of something else, their referents. Soon after hearing what I had in mind, he said, Klaus, you are making a categorical mistake. Objects are referred to, but they don't have meanings. Later I realized that I was facing Alfred Korzybski's well-known but in my opinion epistemologically mistaken semiotic axiom, not to confuse the map with the territory, as he phrased it, or Bertrand Russell's theory of logical types. So I could not work with him. I worked instead with another faculty member, Horst Rittel, who was not a designer but always was my open-minded mentor, who later on became a professor at Berkeley [University of California] where he became known for theories of planning.

Ulm's design philosophy celebrated functional designs. A screwdriver had to look like one and be able to turn screws. Tableware had to be usable in getting diverse kinds of food from a plate into one's mouth. The primary function of a car is to drive from one place to another. Ulm's functionalism entailed a commitment to show, not hide, how something worked, and its minimalism called for omitting from the form of a design anything not essential to its purpose. I was not opposed to this philosophy. I want to show you a prototypical example. This is a desktop blower [presents blower]. To me, it exemplifies Ulm's functional minimalism, which we referred to as honesty, omitting anything not justifiable in functional terms. The blower was based on a heretofore unknown technology, whose working it demonstrates. You see the fan in the form of a rotor. It is covered by the see-through plastic cover, which you can turn to redirect the flow of air. It has a switch that does not need an on or off icon. The blowing of the fan indicates what it did. This is a typical example of Ulm design which I cherish.

[34] Krippendorff, Klaus, *Über den Zeichen– und Symbolcharakter von Gegenständen: Versuch zu einer Zeichentheorie für die Programmierung von Produktformen in sozialen Kommunikationsstrukturen* (Diplom Thesis, Hochschule für Gestaltung, Ulm, 1961), https://repository.upenn.edu/asc_papers/233/.

You do not know that Ulm's preferred color was gray as is this blower. Most of its designs had no color. This was in opposition to using colors for the sake of being adjustable to different fashions getting attention at sales events or in one's home. Gray was considered getting along with everything else. Although the colorless Ulm design was sometimes considered odd, for us, gray—sometimes white, black or being transparent—was considered decent, honest, essentialist, and functional. Actually, Apple design continued this interpretation.

As I started to think about the thesis I wanted to write, there were two things that I began to question. Ulm's functionalism directed our attention to the essential purpose of any design. I would now say that it amounted to a strong preference for something close to what category theory calls the prototype of a category. A spoon had to be what all spoons share. A chair had to look like a chair for everyone to recognize it as such. For me this noble conception of functionalism was made less attractive when I asked myself the simple question of who decides on the function of a design. In any educational setting it may be the teacher, but in practice it is the one who hires a designer for a particular purpose. Surely, the commitment to develop something for an assigned purpose renders everything else irrelevant and amounts to a serious conceptual confinement. It does not support thinking out of the box unless a designer happens, working for an innovative manufacturer, as was the case of the desktop blower for Braun. But it still limits designs to conceptions articulated by an authority.

A second misgiving that motivated my thesis was the association of functionalism with engineering. In the blower you see almost all of its technology exposed. This is a very simple device and I like it as such. However, I left engineering for Ulm to escape the limitations of solving technical problems. Engineering was too easy for me. It omitted the complexity of all human concerns and the social contexts in which technologies ultimately had to work. I felt strongly that design should not be limited to figuring out how to display the technology of functional artifacts but focus on larger issues. So I expressed my misgivings in the form of an article in our student paper shortly before I left Ulm. In fact it was the first article I ever published. It contrasted engineering design with what we now call human-centered design, summarized the basic ideas of my thesis on the importance of what artifacts mean to non-designers, what it takes for designers to embrace this broader perspective, and ended with the proposition: Engineers design functional objects that serve technological functions. Designers should focus on communication through, with, and about artifacts.

Naïve as this association of design and communication may have been at that time, little did I know that communication was what I ended up studying, teaching, and writing of. As I already mentioned earlier, when I came to the United States, I felt misplaced at Princeton University's psychology department, started shopping various universities. Before I decided to enroll at the Institute of Communications Research (ICR), of the University of Illinois in Urbana, I visited its design department. It was a traditional department, not comparable with Ulm. I talked to its chairman for a very long time. He knew of Ulm and asked me all kinds of questions. When I mentioned the word cybernetics, he pulled out a paper that had a piece of art on its first page. He told me that it was about cybernetics and written by someone who headed the Biological Computer Laboratory at the university. That someone was Heinz von Foerster. So, I saw him in his office. We too had a long conversation during which he told me that [Ross] Ashby was teaching a course in cybernetics at the University of Illinois, and that made my decision.

So, at ICR I studied communication, linguistic anthropology, and cybernetics. But between 1964, when I came to the Annenberg School, and 1984, when I wrote a key article on product semantics in design, I had several connections with design issues. I stayed in communication with Reinhart Butter, who had been a co-student, two years behind me in Ulm, and was now a professor of design at Ohio State University (OSU). It turned out he had bought a copy of my 1961 thesis for its costs of reproduction, which was customary at that time, took it with him to Ohio, and invited me to lecture at his department.

So I visited the design department at OSU several times. At one point he arranged for a long workshop together with OSU's communication department. This workshop combined content analysis with design. During this workshop students were asked to rate various objects in semantic differential scales whose polar opposite terms were negotiated to reflect what designers wanted to accomplish in particular objects. To analyze the associations among these attributes, I had started to write a computer program. I remember, it did not run as expected but I got help from an OSU programmer, and we managed to get some data on the table to discuss. Some designers were shocked about how different their judgements were with that of others, which was the point of subsequent discussions of why this may have been so. Professionally, such engagements were not significant, but it made use of what I knew and kept me in touch with friends in the design community.

In 1965 or so, I happened to be Germany for a conference and was invited to give a talk at my alma mater. It was a pleasure to see old familiar faces. Maldonado was still there, and a couple of former co-students were now teaching. I talked of communication of course, but from a perspective that was absent when I was a student. In 1967, a year before Ulm closed, I received an invitation to become a member of the faculty in Ulm. The letter came from Otl Aicher, the then director of the school. I had bad memories of how he disregarded the scientific orientation of the Institute of Visual Perception, where I had been a research assistant for a year, and I declined.

I maintained in contact with Horst Rittel, who had been my thesis supervisor in Ulm, now a professor at Berkeley's [University of California] architecture department. Its design department was looking for a chair. So he asked me, would you come? I was already on track at Annenberg, was hesitant about whether I could function in this capacity but decided to go. In a public presentation I chose to talk of designing toys that would draw on the curiosity of playing and be of educational value. I had fun preparing the talk with slides. The questions I was asked showed me that it was reasonably well-received. At a meeting with the design faculty, I thought they would grill me with questions, but they mainly answered my question aimed at finding out who they were. The faculty included ceramic artists and engaged in a long and animated discussion about how to ensure that everyone was able to get their individualized mug. Collectively, they had no vision of where the faculty wanted the department to go, and I didn't see much hope of contributing to this department. They ended up offering the chairmanship to an aerial photographer who had nothing to do with design. I was relieved and went through the exercise largely because Rittel, my mentor in Ulm, had invited me.

But in 1984 I got re-involved. Reinhart Butter had a sabbatical. He was asked by the editor of the journal *Innovation* of IDSA, the Industrial Designers Society of America, to edit an issue on the meaning of designed products. Twenty-three years after writing my thesis on the subject, this was considered a novel idea at IDSA. He called me and asked me whether I could help him with that, as well as writing a joint article on the conceptual issues of what we coined "Product Semantics." Butter was not much of a writer, but we had worked together, the issue was important to me, I agreed, and he came to Philadelphia to write the joint piece. But writing is still a bit of a solitary activity. We could decide whom he would ask for a contribution, developed an outline of what we wanted to say. I wrote a couple of sections, but this is all we could do while he stayed in my house for two days. I promised him to work on the rest. I felt this is all we could do with him looking over my shoulder. I drove him to the airport with my open Volkswagen Thing. On the trip, we continued our discussion with the papers we had worked on in hand. I remember it was April 1 because, when he called me from Columbus, Ohio, I joked saying that most papers flew away on my return home [laughs]. And he believed it until I reminded him of the date.

In May this issue was published. Our part re-articulated the skeleton of my Ulm thesis, expanded by what I had learned since about communication, to which Butter added examples of his teaching and the lesson learned from the workshops he had asked me to organize at OSU. I argued that designers should not take for granted that the users of their proposed designs would see and use them as their designers did. Instead, we argued that designers should utilize the concepts that users had available to understand their world, hence explore their users' often very different perceptions and needs. By that time personal computers came to be, which became widely usable precisely because their interfaces employed metaphors from the paper world of which everyone was familiar with.[35]

I no longer believe in this simple design conception, but the issue so fascinated the designers association—the IDSA—that it organized in August of that very year a one week-long workshop, at the Cranbrook Academy of Art [Dearborn, Michigan]. It was an open invitation to all who read the issue and participation was huge. Besides Butter and I, IDSA invited John Rheinfrank, also from OSU whom we knew well, and Michael McCoy from the Cranbrook Academy, to organize the event. We presented introductory lectures, divided participants in teams for practical exercises in the end discussed their experiences and started to talk of empirical methods of evaluation. Participants were amazingly excited and the concept of product semantics caught on like a wildfire.

The following year we were invited to Philips in Eindhoven, the Netherlands, its headquarters. Philips is a big company which asked its designers from all over the world to join us for a one-

[35] Klaus Krippendorff and Reinhart Butter, "Exploring the Symbolic Qualities of Form," *Innovations* 3, no. 2 (1984): 4–9, http://repository.upenn.edu/asc_papers/40.

week workshop. Philips' design was recently centralized, and looked for and found in product semantics a new identity. Among the organizers, Butter, Rheinfrank, and McCoy were design professors. I had a design background but was more analytically oriented.

Just as in Cranbrook, participants were divided into working groups and asked to focus on practical products with user-centered criteria we had set. Some of the big-name designers were at first resistant. After all, several had been in top positions and not accustomed to follow younger design professors. However, in the end they demonstrated that our approach to issues of meanings created very exciting proposals whose semantic dimensions we could discuss. For example, one group I worked closely with wanted to develop a radio, a portable radio. It included someone who was a drummer and explored where it would take him when considering the radio user as a drummer and giving him control over two metaphorical drums in the form of loudspeakers, the source of sound. He connected the two drum-like loudspeakers with the necessary electronics and controls. After a few minor modifications it became the prototype of Philips' famous Roller Radio.

This design experienced a telling history. Convinced that this design would be a successful Philips product, the head of its design department took it to the marketing department. It studied its marketability and came back concluding that its unconventional form would make it not marketable. However, the director of Philips' design department had a friend in Italy, who saw it and guaranteed him to buy one thousand if manufactured. So the marketing department was bypassed, and the roller radio became a big success beyond the first one thousand. A lesson learned from this story is that marketing research is inherently traditional and conservative, unable to cope with unconventional and innovative ideas, which is what product semantics was encouraging.

Following the Philips adventure, Butter and I attended an IDSA meeting in Amsterdam where I presented a paper on the subject. There we were approached by someone from Finland, Yrjö [Sotamaa], who turned out to be the head of the design school in Helsinki—now part of Aalto University. He invited us to do a workshop in Finland. We went not once but participated in three conferences about product semantics, initially mainly for Finnish designers but subsequently joined by designers and design researchers from other European countries and a few Americans as well. We all learned a lot and each conference generated widely read books.

I can't enumerate all the workshops I contributed to in the U.S., Germany, Finland, and Sweden. Butter and I organized a workshop in Taiwan. Shutaro Mukai, a former co-student of mine in Ulm, now head of the design department at the Musachino Art University, Tokyo, Japan, invited me as a visiting professor. While the interest in product semantics grew in terms of courses at universities, publications, and conferences since our 1984 *Innovation* issue, there is an odd tendency in the design community to celebrate the latest innovation and consider everything older than 10 years as of yesterday. Already in 1990, I read someone arguing that product semantics was a decade old. But the scholar in me kept me going and I wrote, actually, many papers combining communication and design.

Maybe I'll mention what got me into issues of discourse, about which I had not written before 1998. One phenomenon that puzzled me to no end about the design community was their language. Designers often claim they can identify designers from how they talk. Yet they were generally unaware of how much their work depended on the language they used to solve problems and present their designs to those who mattered. While one could distinguish departments by how they argued, they were collectively hesitant to codify their terminology, agree on textbooks and design methods. They were eager to acquire reputations but failed to investigate their failures. Although Ulm had prepared me to think more systematically of what design entailed, Ulm was an exception.

Historically, designers emerged as professionals with artistic sensitivities. They were hired by manufacturers to make their products more attractive. With the broader perspective we discussed in Ulm, I found such a subservient role demeaning. With schools of design growing, I saw several established disciplines trying to define design as one of their subdisciplines. For example, marketing considers design as its way to expand the markets of consumer products. For economists the sole purpose of design is to add value to goods. For businesses the purpose of design is to create recognizable brands and improve the public image of manufacturers. For ergonomists, design is to make technologies safe and efficient in use. Not that such issues were unimportant, but I saw each of these disciplines trying to usurp design as one of their subdisciplines.

I realized this as a struggle of several disciplines for control of how designers were to talk of themselves and evaluate their work. So I wrote a paper, suggesting that designers need to develop their own discourse—continuously redesign it to preserve their professional autonomy against efforts of other discourses to colonize their profession. To strengthen the design discourse also meant developing standardized curricula, teaching design methods, engaging in research of their own successes and failures, and publishing design-related work. In interdisciplinary projects, an autonomous design discourse would certainly grant designers the respect they deserve and reduce the chance to being colonized.[36]

As a communication scholar, I had experienced colonization efforts at home. Recall, Walter Annenberg had hoped that the school in his name would educate journalists for the *Philadelphia Inquirer* he owned. Under the deanship of Gilbert Seldes it became, one might say, a media art school. The University of Pennsylvania appreciated its financial independence but hired George Gerbner to make the school a more academic place of study. But as communication became a more prominent feature of society, sociologists claimed it under the umbrella of the sociology of knowledge. Social psychologist saw communication fully covered by its own theories. In fact, psychology succeeded in planting psychological concepts like attitudes and the use of controlled experiments into the curricula of many schools of communication. Linguists often claim content analysis as a linguistic topic but would focus on

[36] Klaus Krippendorff, "Redesigning Design: An Invitation to a Responsible Future," in *Design: Pleasure or Responsibility*, edited by Päivi Tahkokallio and Susann Vihma, 138–62 (Helsinki: University of Art and Design, 1995), http://repository.upenn.edu/asc_papers/46.

issues of the formal properties of language, not on how I conceived it as a method of inferring social phenomena in the context of analyzed messages. I think the Annenberg School preserved its discursive independence not only for financial reasons but also because communication technologies could no longer be addressed by journalism departments all over the world, and communication researchers developed their own discourse integrating elements from other disciplines without attempting to colonize them.

So I saw a lot of parallels in how discourse communities form around new social problems, develop their discursive practices, justify the artifacts they create and try to move into each other's more lucrative domains. For an interesting example, computer science, part of engineering, became energized by cybernetic ideas of replacing human intelligence by machines. AI didn't quite succeed but gave rise to cognitive science, which started to colonize psychology because computational implications of cognitive theories are more easily studied than human intelligence. Cognitive science is in the process of replacing many concepts in the discourse of psychology. Discursive explanations of social phenomena and interdiscursive dynamics fertilized my seminar on the social construction of realities.

I don't want to get too deep in that but feel the need to point out that the issue of a design discourse had a hard time being accepted among designers and still is not widely recognized as what identifies the community of designers. Designers tend to be very visually oriented. While they all engage in talk to justify their designs, the terminological consequences of product semantics were not generally recognized as an intervention in their design discourse. The first paper specifically addressing the need to redesign the discourse of the design community, which stressed the issue of its colonization by design-unrelated disciplines, was presented at a Helsinki product semantics conference. The editor of the book featuring the conference papers didn't really understand its significance, had no place among her categories of papers addressing different design issues, and placed it at the end of the volume with miscellaneous papers, you know [laughs].

However, for me the need of designers to attend to their own design discourse became increasingly important to stem the efforts of other disciplines trying to usurp design. Several people encouraged me to write a book. And so I published *The Semantic Turn: New Foundation for Design*,[37] which organized many of my ideas I had written of in separate publications into a coherent proposal.

One fact underlying all design considerations—I would even generalize to most social phenomena—is the unpredictability of their trajectories. Although industrial products are mass produced, their designs are always tied to something unprecedented, to small or large innovations. This makes design an exciting practice and *The Semantic Turn* a challenge.

Maybe I should mention four concepts developed in that book as it relates to my teaching at Annenberg and writing in journals of communication. The obvious one had its roots in my

[37] Klaus Krippendorff, *The Semantic Turn: A New Foundation for Design* (Boca Raton, FL: Taylor & Francis, 2005).

graduate theses in Ulm. However, the increasing complexity of everyday artifacts forced designers to shift from integrating the materiality and functionality into appealing products to considerations of how their meanings could guide users to interface with their artifacts. The driver of a car does not need to know how its engine works, how the repeated ignition of gasoline drives its pistons. What matters and goes beyond the aesthetics of a car is how the user handles its hidden complexity. Artifacts have to be designed so that their users can make sense of what an artifact affords them to do. Product semantics redirects designers' attention from appearances to enactable meanings. The word interface did not exist when I studied in Ulm—interface design has since become a prominent design occupation, which protects the users of artifacts from having to understand its underlying complexities. For example, personal computers would not have achieved their current use without its interfaces.

While all interfaces with artifacts involve physical actions—operating a keyboard, driving a car, opening a bottle—the physical actions turn out to be subordinate to recognizing how to interact with them. *The Semantic Turn* proposed what I called an axiom of human-centered design—*humans never react to physical qualities of things, but to what they mean to them*. By physical qualities I mean the kind of qualities that physics can measure. In the practice of everyday life of physical phenomena are always translated into what people understand, what they mean to them.

The second challenge to designers posed by *The Semantic Turn* actually came more directly from teaching my seminar on *Language and the Social Construction of Realities*. It argued that artifacts occur in the language of their users, bystanders, and stakeholders, and are comprehended accordingly. It offers many examples where the language used when talking of artifacts is critical of how they are perceived and interfaced with. Users' instructions are often keys to interfacing with them. Negative attributes can make artifacts, their users, and their practices undesirable. This goes back to what I had learned in Ulm, including when working in its Institute for Visual Perception. For example, in Germany there once was a small car produced and available. It drove well but had an odd shape reminiscent of a breadbox. It was immediately ridiculed publicly. Drivers were made fun of and it didn't go anywhere. The point is that the terms used to describe especially novel designs can make or break their popularity. Important is the use of metaphors, especially for novel designs. I mentioned the use of metaphors from the paper world that made personal computers understandable, although the conception of documents, files, and trashcans in which unwanted document are discarded has nothing to do with what is going on inside a computer. Once the earlier-mentioned car was characterized as a breadbox, nobody wanted to be seen driving in one.

Regarding the effects of language on design, there are several empirical methods available. I wrote a couple of papers showing how ethnography can reveal the concepts potential users are familiar with and on which designers can rely when designing interfaces, especially with relatively unfamiliar artifacts. There is category theory, which addresses how we recognize what something is. For example, for something to be seen as a chair it must resemble a chair prototype, a super category that defies visualization but defines the class of actual chairs by how close they are to its prototype. Subcategories of chairs require adjectives like baby chair, dining room chair, etc. Designers have to be aware of how their designs are categorized, fused, or confused.

Many of these conceptions came from my teaching of the *Semantics of Communication* course at Annenberg. It was a qualitative version of my *Content Analysis* course, which dealt with conceptions of meanings from the point of view of public perception, political categorizations, ethnic prejudices, etc., not of design. But the process of constituting reality was of course the key to my seminar on *Language and the Social Construction of Realities*.

In *The Semantic Turn,* I trace the third challenge for designers to three sources. After completing my course work in Ulm, the first was due to a guest professor, Bruce Archer, who introduced the distinction between users, bystanders, and producers of any design. A second source was Butter's experience when consulting with Caterpillar, a big construction company. The drivers of construction equipment had no say in what their employer commissioned the designers to consider. It made the concept of a user mute. Also, children's toys are bought by their parents, leaving open who had to be convinced by a design. The third source of influence was management science. I knew Russell Ackoff's writing and had a secondary appointment in the Social System Sciences [Department] chaired by Ackoff. In management science there was much talk about stakeholders. So-called user-centered design, focusing only on one, mistakenly limiting them to the end users, was a self-imposed limitation that had a long history. It ignored all stakeholders preceding this end user, the stakeholders that serviced the design while in use, and the stakeholders that had to cope with the consequences of a design's retirement.

*The Semantic Turn* argued that any design of a reasonable complexity had to pass through a complex network of stakeholders who had very different stakes in bringing a design to fruition or opposing it. It could include the board members of manufacturers whose interests was the well-being of their corporation, bankers concerned with the return of their investments, engineers in charge of running the production of a design, marketing researchers finding out how many to sell where, retailers, repair service providers, government regulators, recyclers, environmental activists, and many more. Proposing stakeholder network theory could draw on and in turn developed my understanding of how different discourses collaborated, fought for dominance.

During the Industrial Revolution, at which time the design profession emerged, designers were employed by manufacturers who had such networks under their administrative control. Today the production of technology is distributed and takes place in a far more complex stakeholder network of which designers are a part. Their role can no longer remain focused on the imagination of potential end users, rather to propose innovations that motivate potential stakeholders to form cooperative networks to bring a design to fruition. Instead, their designs have to energize potential stakeholders and to do so, designers can no longer dictate the specifications of a design but need to leave spaces open for subsequent stakeholders to make their contribution. This is a different world in which communication is the glue that holds a creative culture together.

*The Semantic Turn* outlined these ideas. I brought this new perspective to designers' and design educators' attention wherever I was invited to speak. I advised design teams of what it would take to enroll potential stakeholders of their designs to collaborate in networks in which diverse interest would be taken care of. At the Linnaeus University in Kalmar, Sweden, where I was teaching on several occasions between our semesters at Annenberg, I found a lot of interest in applying stakeholder conceptions. For example, one student project was the design of an unusual museum exhibition. Its director wanted to exhibit the work on a controversial avant-garde artist who physically deconstructed traditional art—not real ones of course—in front of museums visitors who could purchase resulting pieces. He experienced opposition from his board whose members represented museums patrons. The sales department feared bad publicity. An architect warned that the exhibition space would be ruined. Yes, visitors of the exhibition were also important, but they were the least articulate among all who claimed a stake in this project. The team of design students and their teachers interviewed the principal players, charted how their diverse interested connected, in support or opposition to the implications of this project. Then they examined what could conceivably overcome their worries, went back to them with one proposal, listened to what the stakeholders felt, made another proposal which constrained the artist without taking away his aesthetic mission. Not everyone came on board but enough to realize the exhibition.

In this case, the number of stakeholders involved were few who could be talked to and the project affected one local community. But it demonstrated two points: One is that the overwhelming attention of designers to one stakeholder leaves the fate of a design to those ignored and is likely to fail. The other is that a design needs to provide spaces for stakeholders to motivate their contribution. It led to another axiom: Designers need to leave spaces that motivate their stakeholders to add their needed contributions. Delegating enough and perhaps less important features of a design to their stakeholders treats them with respect, whether they are entrepreneurs, financial institutions, engineers, manufacturers, sellers, repair services, government regulators, or environmental activists.

Although I developed stakeholder theory in the domain of design, it fertilized my scholarly work in communication. To me, stakeholders differ by their expertise, the dimensions of artifacts they shape, and the discourse they develop to coordinate the members of their communities. Although stakeholders are the ones who talk and act, but typically as members of discourse communities. And to the extent they form networks their links have to be described as interdiscursive communications. This leads again to my seminar on the role of *Language and the Social Construction of Realities*.

One example of how this played out is a dissertation written and defended by Nicole Keating. Originally, she wanted to write an ethnography of how a documentary came to be. Unfortunately, funding for that documentary was denied and she felt stranded. However, we talked of alternatives. Her original topic involved the collaboration of different players, which was close to my writing on stakeholder conceptions, and she decided to study the contributions made by the stakeholders of an already completed documentary. The list of parties generally involved in such a project was relatively uncontroversial: the film makers of course, the writers of their scripts, the relevant historians, the funding agencies, and distributors. Each had different stakes in getting the documentary done. Merely interviewing those involved would not have revealed the nature of the network. So she interviewed these stakeholders not only about their stake in the project but also with whom they communicated, whether their contributions were taken seriously, what difference they encountered, and how their contributions could be seen in the final documentary. It revealed several interesting discursive conflicts. For just one example regarding the history of what the documentary presented, there was the almost classical conflict described by Donald Spence as between *Narrative Truth and Historical Truth*. The historians involved insisted on the latter, while the writers and film makers felt the need to create a compelling story. Nicole was worried that her dissertation would merely be another, her narrative. So we agreed that she should delegate her last chapter to be a critique of her dissertation by those interviewed. It did not work quite out that way because the stakeholders of this documentary didn’t mind being interviewed but had no time to read her dissertation. So she provided them a synopsis of her findings. From those who responded, she received no criticism or corrections, but praise for providing insights they had not thought of, to which several added valuable accounts they said they would have mentioned had they known what she was driving at. Her last chapter in effect validated her dissertation, a feat rarely accomplished by other qualitative dissertations. In effect she got stakeholders to reflect on their own interactions.

The fourth and final challenge for designers was to take an even larger perspective of the role their designs played in what I call the ecologies of artifacts. Even while passing through a stakeholder network, a design takes on many shapes from drawings, prototypes, marketing results engineering drawings, displays in showrooms, including trash or pollutants of the atmosphere. During all of these processes it interacts with other artifacts, competitive evaluations, production machinery, assembling them into larger wholes, when fueled as for cars at gasoline stations, disassembling them and inserting valuable parts into other artifacts. A design is never alone, although designers often conceive them as such. Also, all designed artifacts eventually are retired and come in contact with other artifacts. For example, old ships that are no longer useful cannot simply be discarded. I recently learned that Bangladesh is one country that makes it a business to take them apart and sell valuable pieces. I understand that unusable cell phones contain valuable components. It is a challenge for designers to allow their designs to be disassembled and disposed, by not ruining the environment of the future.

While all artifacts are subject to material transformations and decay, they often turn up in different contexts where they acquire different meanings and uses. For example, I once went with my children to the Philadelphia Museum [of Art]. They were interested in the section displaying medieval armaments—I don’t know if you have been there. So there was one armament that was actually black. A small sign said it was worn by the Count of Brunswick [an area in Germany] at the occasion of his marriage, and it gave a date. Now, I grew up in Germany with the heroic story the black knight of Brunswick who came at the night to places where injustice was committed and made things right. It was a popular mythology. In the presence of this black armor, I told this story to my children but said to myself, nobody in this whole museum knows of that. True or a myth, it is a story that made this armor meaningful to me.

Then I decided to write a paper, imagining the history of this armament, when it changed hands, what it may have meant and for whom, until it entered the museum. Surely, it was made by master craftsmen. The armament is not a simple piece of metal. Lots of craftsmen must have hammered large pieces of steel into thin sheets of metal. The count must have come by to see what needed to be adjusted to fit his body. In the process of making it meant something different to the craftsman, to his assistants, and to the count. The sign said that the count wore it during his wedding. But at that time, weddings featured tournaments at which knights rode a horse and tried to push their opponent off their horse. The groom was the count, so probably nobody wanted to hurt him. At other occasions, an armament like this was to protect you from being killed. It was heavy, rigid, and constraining your movement. I cannot imagine the fear inside the wearer of an armament during life or death battle.

What may have happened after it was made and worn at the wedding of the count? If the sign is correct, I took my imagined history as an example of describing the process of how the meaning of an artifact changes in the process, passing from one bricolage to another, a bricolage being defined as many artifacts interacting and defining their meanings relationally for those present and able to talk of them.

In medieval times victors go to the armaments of the defeated as trophies. As knights no longer fought, nobilities kept the armament of their ancestors like we preserve photographs, and tell stories of their forebears. And so it became a family heirloom, a demonstration of the importance of something. After the class of noble knights disappeared, antique dealers made the armament a sellable good. The collector who purchased it and the Hertz family were wealthy competitors. They traveled through Europe and acquired medieval artifacts to decorate their homes. In fact, the museum displays a photograph of the living room of the donor of these armaments to the museum decorated with all these armaments. Now these armaments were not measured by their purchasing price but as an expression of the wealth of a cultural elite. Now in the museum, this history is reduced to just this label and a date. Viewers are not given a hint of what it meant [laughs]. So, I was suggesting that the meanings of all artifacts—if they are durable—go through transformations. They are never stable and often disappear in favor of their present use.

Now comes the ecology of artifacts, of which the above may well be a part. As I mentioned, designed artifacts are rarely if ever used alone. They always play a role in their bricolage. If you buy tableware you do so in view of having a table with chairs, plates to eat from, and friends or family to eat with. They all go together. These connections are not made by designers but have to be afforded by their design. Or when you buy a new drive for your computer, you have to be able to insert it and it connects you to various software, files, the cloud, and to other systems. To me, a bricolage is a network of meanings. It informs underlying mechanisms, best described as an ecology of artifacts and object of nature. In ecology we talk of species that support or compete with one another, that mutate and survive in interaction with other species, and in the case of artifacts they compete for being attended to by stakeholders, by individual users and institutions.

So when you see something new then you think, Should I replace the old one? Unlike in ecologies of biological organisms, in ecologies of artifacts, you make the decision, not the artifact. So an ecology of artifacts is part of culture. We know of cultures that died when they were not supported by the ecology of their artifacts and their natural environments. The Incas in Mexico were driven off their monumental sites by exhausting their ecological resources. So I always advocated a larger perspective for the role of design, which should not be limited by proposing artifacts to be produces and used. Their role should be to introduce innovations that keep a culture viable. Ultimately, the viability of a culture is all that matters. Mass reproduction of practices, without variation, is the death sentence of a culture that faces changes, including those resulting from the consequences of its repetitions.

In 1996, for a National Science Foundation–sponsored conference on Design in the Age of Information I wrote a paper advocating this larger perspective. To make this perspective attractive to designers and design researchers, I proposed a history of design problems in the form of a trajectory. It acknowledged its origin in the design of marketable industrial products, to the slightly more general aim of concerns for larger systems of products, still serving their manufacturers, to interface designs, to the design of multi-user systems like the internet, startup businesses, or governmental bureaucracies, to collaborating in interdisciplinary projects that no individual discipline could handle on their own, to the design of design discourse, all wrapped up in the larger responsibility to keep our culture viable. So that, to me, is the highest level I could think of.

Of course, I applied such conceptions to the responsibilities of social scientists as well. Traditionally, all social research aims at describing what exists, truthfully, accurately, sometimes critically, even in the form of general theories. However, what I taught in my seminar on *Language and the Social Construction of Realities* was grounded in the recognition that language mattered when enacted. Scientific theories are not exempt. They can change the social realities they claim to merely describe. Whether social research is applied and funded by institutions with social missions, for example health communication research, inquiries into sources of criminality, gender discrimination, or racial prejudices, even seemingly neutral accounts of observations can affect what they describe, not to ignore the role of science fiction or visionary theories, for example, Norbert Wiener's cybernetics. Viewing them as proposals brought them into the domain of design.

Q: Maybe I can jump in there. You've described these four levels and taken us through to *The Semantic Turn* itself, and I want to go back to 1984, if that's okay. Because I was struck, as you mentioned those four levels, how many of them were in place, in some ways, in that 1984 product semantics article with Reinhart Butter. And that I couldn't help notice in the same year you were publishing your first major statement of your constructionist position, the second-order cybernetics that we talked about last time, and the notion that the so-called observer is in fact a participant in something that's continuously reconstructed as a system.[38] So at the

[38] Klaus Krippendorff, "An Epistemological Foundation for Communication," *Journal of Communication* 34, no. 3 (1984): 21–36, https://repository.upenn.edu/asc_papers/538.

same time you're talking about—maybe not in these words yet—networks of stakeholders in 1984, you're also talking about how observers, the cybernetician, for example, might be a participant in what he thinks he's merely observing. The notion that you develop over time of a designer who is a participant seems so in sync with the epistemology you had been working on at the exact same time. So I just wanted to invite you to say whether there was an interplay in either direction.

KRIPPENDORFF: There was a connection but most of these conceptions developed in the course of teaching and writing about communication and cybernetics. For example, the 1984 coauthored paper that introduced product semantics into the literature did not mention interfaces, stakeholders, and ecological conceptions. I think in 1984, I was quite naïve, building my arguments largely on my 1961 thesis and talking of designers as communicators. The conceptions I outlined above emerged in response to what the 1984 paper set in motion.

But you mentioned my connection to cybernetics and its epistemology. Cybernetics has many contributors, but there's one—my mentor, Ross Ashby—who deviated from the standard definition of cybernetics as a concern for circular self-governing systems. Ashby insisted cybernetics to have its own foundations, not derivable from existing disciplines. He defined cybernetics as the study of the dynamics of all conceivable systems, whether they exist in the observable nature, are constituted in the language of their human users, or are mere proposals, is secondary to exploring how we can interact with them. So defined, cybernetics includes designers' interest in systems that could conceivably be realized and lived with.

Acknowledging possibilities of changes was for me the most attractive part of cybernetics. Actually, Gregory Bateson, citing Ashby's conceptions recognized cybernetics as providing a new kind of epistemology. He suggested it to resemble Darwin's theory of evolution, but on the level of variety, information, and knowledge. Darwin dealt only with species of organisms that could mutate and succeed or failed when interacting in their environments. But cybernetics was concerned with communication. Bateson's insights resonated with me, as it included one important conception, that of affordance. Affordance complements that of interactions with artifacts, being defined as the range of possible uses of an artifact, analogue to the mutations of organisms that assure their viability. Bateson noted that one cannot predict the future of evolving systems, only their constraints—and he suggested this to be the case of systems that thrive on information.

Actually, the origin of the word affordance comes from [James J.] Gibson, who developed what he called an ecological theory of perception. It entailed the claim that we do not see things as named objects but what they afford us to do with them. So, when we say we face stairs, we see our ability to step up or down on them. We call something a chair but perceive our ability to sit on it. We buy a personal computer for what we can do with it. The concept of stairs, chairs, and computers are abstractions in language for what they afford us to do with them. All artifacts tend to afford many more activities than we normally use them for. A chair may be stackable, step-on-able, usable to store books, and more. There are cultural constraints on what an artifact affords. For example, for terrorists, trucks afforded being used to kill many innocent people by driving one into a crowd. The range of an artifact's affordance are almost always far larger than imaginable. What Gibson did not dwell on was the constraints on an artifact's affordances, that is, actions we think we can perform but the artifact fails to let us do it. Experiences of failing to accomplish something on a personal computer that it just wouldn't let us do are common. The notion of affordances and its constraints countered deterministic accounts of social phenomena, as Bateson noted, gave me a clearer conception of what my thesis in Ulm sought to accomplish, and made cybernetic explanations increasingly attractive to me.

Q: Well, you mentioned that there already, in that period in 1984, was an interplay. It seems to me that from 1984, through to maybe around 1989 when you had this second Reinhart Butter special issue in a different journal named *Design Issues*—you already touched on this a little bit—but there were a series of conferences and gatherings, and a year of sabbatical in particular that I wanted to ask you about. I think it was 1986, 1987, when you were at Ohio State with Butter, but not just with Butter, and even worked in a design firm, if I understand. If you could just talk about that period—it seems to me that certain ideas like the stakeholder one, like the affordances, James J. Gibson's idea, those only appear in 1989, so that this intervening half-decade seems to have been a period of lots of ferment.

KRIPPENDORFF: Before I get to the specifics of your question, in 1984 I was president of the ICA [International Communication Association] and I had the privilege, as all presidents do, to give a concluding speech.[39] I think I must have talked about that speech earlier. I proposed several imperatives for communication scholarship that combined insights from design and cybernetics. The first imperative suggested, Create the world to see. Although this aesthetic imperative related to the creation of affordances, it goes back to the eighteenth-century practical philosopher Giambattista Vico, who argued, we can only understand the results of our own actions, not what is given to us by God. Its relation to constructivism should be obvious. The second and empirical imperative suggested to never get stuck with one account of things, explore as many alternatives you can imagine. This was close to Ashby's definition of cybernetics and Bateson's highlighting the ecological epistemology of cybernetics. The third imperative asked communication researchers to recognize the choices communicators open for each other, for example in conversations. It was meant to discourage formulating causal or deterministic theories of communication that, when practiced, rendered communicators as causal mechanisms. Another imperative was to discourage communication researchers from assuming a God's eye view of communication, which excludes the applicability of communication theories to themselves. After all, communication research has to be communicated and should not create social pathologies. Obviously, I saw parallels of design, which aims at changing something to be ecologically viable, and communication research, whose effects on society may be more implicit but can hardly be denied.

[39] Klaus Krippendorff, "On the Ethics of Constructing Communication," presidential address delivered at the International Communication Association Conference on "Paradigm Dialogues," May 23–27, 1985, Honolulu, Hawaii, http://repository.upenn.edu/asc_papers/275.

But now coming to my sabbatical at Ohio State. Reinhart Butter wanted me to spend some time there, but at the same time I was invited to a conference in India [at the Industrial Design Centre of the Indian Institute for Technology in Bombay]. They had heard about product semantics in part from one Indian design researcher whose research we included in our co-edited volume. It was about Gandhi's use of artifacts with generic meanings—what he wore and the basic spinning wheel he became associated with—which bridged the diversity of India's cultural traditions and brought the whole country together. The conference was huge. It was named Arthaya, which is a Hindu word for meaning. Some participants still struggled to overcome Ulm's functionalism, but most of them saw its universalism as detrimental to India's multicultural traditions and wholeheartedly embraced a cultural interpretation of product semantics. I had been in India on several occasions but learned a lot about radically different conceptions that Indian designers had to consider.

At OSU, I had a joint appointment at its design department, at its communication department, at its systems engineering department, and an office at the consulting firm, Richardson-Smith. Students came from the three OSU departments. The designers at the experimental design laboratory at this consulting firm, headed by John Rheinfrank, were initially skeptical about what a professor of communication could possibly contribute to their projects, were merely curious and had planned to let me be in my office. At one point someone showed me the project one team was working on and asked whether I had any ideas about how to approach their problem. I joined one of their internal conversations, made some suggestions, and from then on I was invited not only to join larger projects, but some designers also actively participated in the seminar I taught with students from the three OSU departments.

Let me interject that this experimental design laboratory aimed at big and future oriented projects. Before my time there, it had completed a fascinating project for a large furniture company. That company had given the consulting firm a free hand. Based on a study of what future office work would be like, in various business branches, and supported by modern office equipment, the design lap developed an amazing modular system of easily combinable units, allowing customers to design open or closed individual work rooms, spaces for conference meetings, places for meeting accidentally near beverage automata—not just desks and chairs. This was one example of the larger than usual and futuristic design projects the experimental design laboratory aimed to undertake.

While I was there, Philips in Eindhoven, the Netherlands, had heard of the experimental design laboratory and approached the consulting firm in the hope to develop a unit within Philips that could tackle truly big design projects of which Philips had many in mind. The request did not come from their design department, which in 1985 organized the second product semantics workshop. Philips sent two of its designers to Columbus to work with the lab on a then considered big and consequential project of developing software for insurance companies. The two Philips designers introduced the general idea and explained how far Philips had progressed in their realization and where they were stuck—in my opinion. I have to say that the reputation of the design lab exceeded its design methods, which were more vision-oriented than methodologically grounded. After a short while the two Philips designers went home but left the project in the hands of the design lab.

The first step taken by the design lab was to video and interview insurance agents in their traditional offices. We learned a great deal of how they organized their working space, the manuals and books they consulted, with whom they talked about what, the evidence they had to find, the sources they trusted, the risks involved in misjudging cases, and the interruptions they experienced. Needless to say, my experiences working with Charles Goodwin, who wrote his PhD dissertation about conversation analysis, incorporating video data, came handy.

After collecting such data and analyzing them qualitatively, we were flown to Philips to meet what for me were the stakeholders within the company. They consisted of project managers, programmers, hardware designers, engineers, businesspeople, and at some point a CEO joined the meeting. Philips showed us the latest technological developments. We orchestrated the meetings, started by presenting our data, made suggestions, and listened, particularly to the diversity of expectations these stakeholders had of each other and what they shared. We ended each meeting with an understanding of the next steps that each group of stakeholders was to explore on their own, flew back home, worked out new proposals, and met again after each had something to show. To me this process was fascinating for two reasons. First, it tested what I had been preaching about the importance of stakeholders but not practiced, and second, I experienced first-hand what conversations and mutual respect could create that none of their participants could imagine on their own.

After my sabbatical, while teaching at Annenberg, the design lab wanted me to stay involved in Philips' projects, flying to Eindhoven many times, and as the project developed, working with a slightly changing cast of stakeholders. I learned to participate in negotiating often seemingly irreconcilable conceptions, and to appreciate the complexities involved in developing computational technologies. Academics rarely are challenged by working with such interdisciplinary stakeholders, trying to reconcile diverse conceptions.

Q: Well, you know, thinking about the Helsinki conferences, you mentioned that there were at least three—and at the third you engaged with the idea of discourse, and design discourse, and argued for designing a design discourse. You also, in that period, seem to be writing about design education for the first time, and it's a theme that you've kept up with. I'm wondering whether the recommendations you've made have gained purchase, and what the reception has been inside the design education complex?

KRIPPENDORFF: My influence was mixed. One disadvantage I had was being known as a professor of communication in a university without a design department that resembled where many design professors came from. Incidentally, I received many requests from all over the world of students wanting to study design issues with me. Regretfully, I could not help them. On the positive side, most design professors didn't have the social science background I could draw on. I saw myself as translating communication conception into design educators' worlds. Whether my 1961 design-diploma thesis in Ulm or subsequent publications caused the increasing awareness of issues of the meanings of designing artifacts is hard to tell. Design education has shifted since towards a concern for meaningful interfaces, one conference after the other. Sometimes I'm credited for this shift, sometimes my conception of product semantics became something quite different. For example, one school of design in Germany reinvented the approach, naming it a concern for product language. I argued that products don't talk, designers do, but this was their way of incorporating meaning into their design discourse.

Recently I was invited to give a keynote address to a large international conference on interaction design in Amsterdam. I wasn't familiar with UX-design, a worldwide association that brought its members to the conference. I was known by the organizers, followed what I was told they wanted to hear from me, and talked of teaching a design discourse able to facilitate innovation in human-centered interface designs. I contrasted ecological conceptions of artifacts with traditional notions, had great slides, an attentive audience, was able to meet and talk with several interested educators who saw and appreciated the connections I was making. But the majority of attendants were practice-oriented interface designer who looked for new ideas, but were less interested in how they could be generated in the interdisciplinary conversations I had experienced. The speakers that followed me had given TED Talks before, made sophisticated audio-visual presentations, and delivered inspirational messages that shadowed my scholarly talk. I had a mission that was important to me. I was pleased to meet and stayed in contact with several interesting people, but I am not a performer. Grasping the importance of a design discourse required awareness of what language does. This was not everyone's forte.

You asked how my work affected design education. Clearly, the simple ideas of communication I worked into my 1961 thesis in Ulm guided me into teaching communication. I was several times invited to talk about the meanings of artifacts at the Ohio State University's design department. But it was only after two decades of academic work on content analysis, communication theories, cybernetics, applying ethnographic methods to messages, and pursuing constructivist conceptions of reality that I started to actively apply my meanwhile acquired insights in communication to design. I can say that since 1984, design education shifted from functionalist to human-centered design conceptions which were rooted in how meanings came about. You mentioned the three conferences in Helsinki to which one needs to add others and numerous international workshops, all sponsored by design departments in universities. Design educators were eager to adopt concepts that enabled them to cope with the changing ecology of technologies. I can't take credit for their motivation, but I made my contributions to conceptualizing these developments. The development of digital technology encouraged the shift to conceiving of objects, natural and designed, in terms of their human interfaces. Now this conception is almost universally accepted and taught. I argued that designers need to acknowledge that they rarely are in charge of the realization of their proposals, that networks of stakeholders had to be enrolled to bring a design to fruition. This went against the traditional conception of designers but was not their option. It was forced on them by the complexity of contemporary society. The conception of stakeholders is now taught in a less general form as participatory design—inviting users into the design process—and it is implied in courses on ethnographic methods to survey the conceptions of stakeholders that designers need to consider.

Where I think design education is still lacking is preparing designers for the kind of interdisciplinary collaboration in which designers play just one role. Unfortunately, and in my experiences, the role of designers in such collaborations is diminished by the weakness of their design discourse, facing collaborators from disciplines that bring statistical data, empirical evidence, and rhetorical devices into these discussions that are difficult to dispute. Although I have written and lectured to designers about the importance of being able to rely on a sound and indisputable design discourse, I am not aware of any design department that teaches the subject explicitly. Recent discussions of design research methods and so-called evidence-based design do recognize the role of the discourse designers are using to justify their work. However, far too often their advocates take research methods from established scientific disciplines that leave little room for innovations and their adoption in the discourse of designers confines them to implicitly conservative agendas. I criticized this practice in a book chapter. Whether this was widely cited and translated made a difference needs to be seen.

An area where design methods and communication practices meet is in recognizing that most innovations emerge in conversations, in teams. It is our Western individualist tradition which mistakenly celebrates individual geniuses as inventors. For example, at the Ulm School of Design, the history of design was taught as a history of styles exemplified by well-known designers. Not surprisingly, all of Ulm's successful designs were attributed to designers whose fame depended on being credited for them, completely ignoring the fact that they were always surrounded by and in communication with their teams. There is plenty of empirical evidence that virtually all technological innovations emerge from conversations. Most recently, two Stanford cognitive scientist concluded their research stating succinctly, *We Never Think Alone*.

Earlier, when you asked me about my conceptions of communication, I mentioned my participation in a Comparative Communication Theory workshop during the 1989 ICA conference in San Francisco. The frustration with my fellow communication researchers grew out of the disconnect between our discussion of communication theories and what the theorized communicators were conceived of as doing. We certainly were aware and respectfully compared the conceptions of communication that each of us brought into the discussion, but we ignored that communicators we talked of had any conceptions of each other. When I suggested to consider them as acting on their own conception they had of each other like we did, the overwhelming consensus was to consider them as merely encoding and decoding messages according to a shared code. This was of course Claude E. Shannon's conception of transmitting information, but we wouldn't even dream of explaining our ongoing discussion in its terms. I suggested we shouldn't talk of human communication in terms not applicable to us. I had experienced a similar disconnect when our content analysis of violence on TV as causally connected to violence in real life—applicable to viewers of violent shows but not to their analyst. After unsuccessfully arguing against us communication scholars assuming to be superior to the communicators we were talking of, I wrote a paper entitled "Conversation or Intellectual Imperialism."[40] It contrasted our developing communication theories in discussions with assuming intellectual abilities that observed communicators are denied. It also argued that ideally, communication theories should emerge in conversations with those whose lives is to be understood. It realized the second-order cybernetic injunction against separating the observed from its observer—and in the case of communication, the communication theorist from the practice of communication.

My opposition to the intellectual imperialism I saw practiced in many scientific domains, also called the God's eye view of the world, led me to explore the communication practices that transform genuine conversation into other forms of communication.[41] To me the concept of genuine conversation, which may well be rare, is always invoked when deviations from it are experienced. Genuine conversations are not predictable. Participants provide each other spaces to respond. Everyone's contribution is respected equally. Interactions flow naturally without invoking rules, a predefined purpose, or references to absent authorities. Participants notice when the natural flow of genuine conversations is disrupted, for example, when someone talks too much, is addressed to in terms of categories, insist on being correct at everyone else's expense. Conversation analysts often focus on strategies of repairing such disruptions. However, in my paper, I argued that deviations from genuine conversations, once accepted as normal, generate other forms of communication.

For example, we accept a situation as a lecture, by granting someone the privilege of monopolizing communication for a dedicated period of time. We tolerate not being able to respond to what is said when watching television. We may give someone the authority to call on individuals to respond to what is said when we call it a board meeting. Among the many definitions of communication are the kind of interactions that professional discourse communities consider as normal. The communication between medical doctors and their patients are restricted to the terms of the medical discourse. Communication in courts of law are confined by the legal discourse.

My paper recognized several cybernetic conceptions in genuine conversations and their descendants. For one, saying something and receiving a response constitutes a feedback loop. Conversations enact a multitude of such loops. Saying something in anticipation of a particular response involves circular expectations against which the actual responses may be judged and responded to. Much of communication can be explained by such reflexive loops. From a cybernetic perspective, conversations are self-organizing. They proceed without reference to anything outside of it. As soon as participants claim to speak for absent others, they are no longer equal. Yet there is a difference between claiming to speak for the poor, or for all Americans, and being an arm of an outside entity. I have been in many committee meetings at the University of Pennsylvania that could not come to a consensus because some participants had to consult their department. Claiming access to an outside authority that others don't is

[40] Klaus Krippendorff, "Conversation or Intellectual Imperialism in Comparing Communication Theories," *Communication Theory* 3, no. 3 (1993): 252–66, https://repository.upenn.edu/asc_papers/257/.

[41] Klaus Krippendorff, "Conversation: Possibilities of its Repair and Descent into Discourse and Computation," *Constructivist Foundations* 4, no. 3 (2009): 135–47, https://repository.upenn.edu/asc_papers/134.

another deviation from conversations, whether this authority is what priests claim to have access to, empirical data nobody else can read, or an algorithm whose results are expected to be complied with for its supposedly higher level of intelligence.

For me, the most outstanding cybernetic quality of conversations it its evolutionary nature. Even in somewhat less genuine conversation, as long as participants' open spaces for each other to respond to what they say, elaborate on a thread of responses or drop them in favor of something more worthy of pursuing, everyone is respected for their contributions, and neither is in charge or judging other conversationalists, conversation is the most efficient evolutionary process I know. In other words, collective mutations are immediately followed by consensual selection and what survives is both unprecedented and consensually acceptable. In biology, the viability of mutations takes a long time to be evident. In conversation they are almost instantaneous. As Gregory Bateson noted by reference to Ashby's cybernetics, evolution concerns the epistemology of language use. Like all evolutionary processes, conversations proceed unpredictably. Their results are unprecedented, or novel. All of what one may be able to articulate is what may not happen. The primary constraint on the possibilities that can emerge is the discursive ability of its participants.

Obviously, and in my own experiences, designs that make a difference in the lives of others emerge in conversations among participants whose consensus represent the possibilities available to their stakeholders. To me this applies also to communication research as well. Communication and theories of communication cannot be settled in the mind of a theoretician, nor are they predictable by means of mathematical formalisms, which celebrate God's eye views of a deterministic world. I would suggest that all social theories that are able to be practiced, constitute social organizations, or account for the technological infrastructures of society, need to evolve with or independent of the work of professional designers. Appreciating conversations is also my primary motivation for working with students and crossing the boundaries of communication research, cybernetics, design, and discourse.

You ask about where my constructivism came from. Actually, I do not like -isms for they entail a commitment to an exclusive set of ideas. Obviously, my design background shaped my conception of the world not as a causal system we had to accept as given but as changeable to the better. My initial effort to conceptualize the change needed was to redirect designers' attention from the design of functional products to the design of meaningful human interfaces with the world. [Ludwig] Wittgenstein played a role in shifting my attention to the role of language in these efforts. Wittgenstein lived several lives but ended as the initiator of the linguistic turn in philosophy, making it his mission to overcome the common conception of language as descriptive of what is. His conception of language as a collection of language games acknowledged its interactive use. He exemplified how language worked by how a master would communicate with his workers, arguing that the meaning of assertion is the responses it elicits. The word communication was not in use at his time.

Then came several influences from authors who translated the linguistic turn in philosophy into the social sciences. Whereas the objects of the natural sciences do not talk, all social phenomena are constituted by human beings who enact their conceptions and talk of what they face. This required a different epistemology. I already discussed John L. Austin's conception of speech not as an expression but as a form of action capable of constituting social realities, and the sociologist C. Wright Mills, who is the cause of trying to understand the source power that elites in the U.S. were exerting, found it in the form of compelling motivations that actors gave each other to justify the changes they had caused or were proposing. I worked with the communication theorist John Shotter, who advanced a reflexive theory of communication on top of Austin's conceptions, linking two levels recursively: that of the actions, proposed or performed in the presence of others, and that of the accounts of what these actions could do or did. Accounts are intended to make sense of actions. When accepted, they are valid. The point of these conceptions is that actors have options and make choices that the natural sciences rarely acknowledge if not deny. The relations of these linguistic conceptions to what I described designers do should be obvious. All proposals for introducing something unprecedented into a network of stakeholders are rarely self-explanatory. They require plausible arguments that can enroll these stakeholders into a designer's project. To these language-based notions comes the cybernetic epistemology of Ernst von Glasersfeld's radical constructivism, which focused historically on how individual perceptions are acquired and acted upon when perceived as real. Heinz von Foerster's second-order cybernetics, which builds on the observations by observers, not the observed, echoes this constructivism.

A frequent critique of constructivism is its presumed relativism, the idea that anything goes, and that reality is just a fiction. This surely can be a philosophical nightmare, but one that rarely enters the world of designers. I mentioned Gibson's ecological theory of the perception of objects in terms of what they afford someone to do with them. Gibson exemplified his conception of affordances largely with artifacts, objects of nature, people, and institutions that have a long and culturally stable history. Their perceptions have hardened to the point of not causing problems. However, all designs are to some extent unprecedented by definition and the perception of their appearances may well mislead the user into interactions that designs cannot afford. Thus it is important to add to Gibson's conception of affordances as the set of possible uses that an artifact provides to a user, the set of affordances perceived by a user based on the object's appearance and promises of associated accounts. Usually, the set of perceived affordances is far smaller than the set of possible affordances. But appearances of artifacts can be misleading, accounts of what they enable may not be realizable, and users may be unable to read either, and using an artifact accordingly may lead to nowhere, to experiencing failures, or have disastrous consequences to the user. Designers have to be aware of the dangers of misleading the users of their designs. But they also need to be aware that users may discover new uses among the possible affordances that designers may not have imagined or could not prevent when undesirable. I mentioned terrorists' use of trucks as a weapon of destroying innocent lives, which nobody imagined until this possibility was discussed among terrorists, and actually used that way. The point is that constructivism offers many possibilities, usually more that anyone can imagine. However, it cannot erode into radical relativism if the affordances of people's actions are seriously considered.

The other point is that technologies, their social and cultural evolution, always generate unprecedented possibilities and are genuinely unpredictable. Most scientists who have tried to predict technological developments failed. Only the boundaries of these complexities can be accounted for. For example, we can safely predict that tomorrow's computers are faster than today's or that the rate of population growth changes very little. The reason for this unpredictability is not only the result of designers, exploring new technologies, trying out new business models, but foremost relying on the combinatorial possibility inherent in language and encouraged in the conversations that take place on all levels of society. Design discourse is just one of many discourses practiced in society. Not all mutations are viable on the long run, but we don't know until we tried, starting in conversations and proceeding to practices. Communicators and designers can expect unforeseeable future consequences of what they do but have to be careful not to afford uses that harm their users or other communities. Safety devices in cars and production machinery limit deadly accidents. In the United States the ease of purchasing guns makes suicides killing others possible. Ways to limit these affordances have not been found. Affordances define the range of possible uses. They imply the users' choices of actions. By contrast, the ideology of the natural sciences—whose practitioners construct their discursive realities from a God's perspective and in terms of causality—may aid the engineering of technology but are unable to explain its social use. Adopting an algorithmic language for the design of social realities renders their users machine-like and stifles the viability of discourse communities that adopt that language.

Let me add a related story. A decade ago, I was invited to contribute a paper in the spirit of second-order cybernetics. It was published entitled "The Cybernetics of Design and Design of Cybernetics."[42] Most recently the editor of a book on design cybernetics wanted to republish it. I reread it and realized its epistemological limitations. Second-order cybernetics acknowledges the role of cyberneticians as observers describing their observations, not the object observed. As important it is for theoreticians to acknowledge their observer role in the theories they publish, limiting the role of theorists to observers was not enough. It merely replaced objectivity by subjectivity. Just as designs are produced to be used, scientific theories are published and may well have practical consequences for which theorists should be held accountable. This is especially so for social scientists whose theories may inadvertently affect what they are about. Causal social theories, if adopted by readers, do not grant agency to those theorized and encourage predictable behaviors. Designs, by contrast, create affordances for their users. So, I felt the need of rewriting this paper. It ended with that contrast. Designers introduce novelty into the world in which their designs enter, making that world increasingly difficult to predict. I tell my students that creative social scientists should do the same and accept being held accountable for what their scholarly work does to its readers and stakeholders.

Q: So if I could jump right off of that comment and the 2007 paper you were mentioning, because you describe there that design is indeed about improving, it's about constructing. It's

[42] Klaus Krippendorff, "The Cybernetics of Design and the Design of Cybernetics," *Kybernetes* 36, no. 9/10 (2007): 1381–92, https://repository.upenn.edu/asc_papers/48/.

constructionism in that way. And you use the phrase "desirable futures"—that you're not just acknowledging participation but that you're actively trying to participate for a better future. And throughout the whole engagement with design, from the early to mid-80s all the way through to *The Semantic Turn*, you are talking about methodology at points. One of your other hats is that you're a methodologist, and in *The Semantic Turn* itself there's a whole development of a kit of mostly social scientific methods, and those are presumably, at least in the way they're normally understood, observational methods. What's the relationship then, in your mind, between design and observation in this mode? They seem to be in tension?

KRIPPENDORFF: It's a tricky question. As I said previously, what designers and social scientists have in common is that they perform albeit different discourses. They institutionalize their recurrent practices. Methodology codifies recurrent analytical practices. In the social sciences, statistics is a method accounting for distributions of units of analysis. It can be taught and be made available as software. All statistical data are of past phenomena. Designers by contrast need to know what is possible. Possibilities cannot be observed but may be inferred from the concepts that people are willing to leave behind or visions of desirable futures. Most social scientific methods are concerned with predicting future events from what is, what was, assuming any observed trend to continue. Such methods do not support the possibility of introducing unpredictable innovations by design. Research, when interpreted as re-search, repeated search of what already exists, is an oxymoron for designers.

Yes, I outlined several such methods in *The Semantic Turn*. All ended up in present problems that stakeholders would be pleased to see solved, and desirable futures that they would be happy to live in, as well as surveys of technologies available to be combined in a design. Ethnographies of experienced problems are well established anthropological methods. Desirable futures don't exist and cannot be observed. However, they circulate in a population in the form of futuristic literature which can be content analyzed [laughs] and be taken as evidence for what potential users of future designs can imagine and might accept. Prototypes of a design can be evaluated by focus groups, familiar in marketing research. Surveys of available technologies, potentially available for recombining them into a design, is not too different from qualitative research methods. So, yes, *The Semantic Turn* provides a framework for design research and its relationship to existing social science methods. However, adopting research methods from other disciplines entails the danger of design discourse being colonized by adopting their epistemologies, mentioned earlier. In a paper I contributed to a book on *Design as Research* I argued that design discourse needs to free itself from other disciplinary commitments, appropriately entitled, "Design, an Undisciplinable Profession,"[43] meaning designers should not allow their discourse to be disciplined, for this would prevent it to be innovative and socially responsible.

[43] Klaus Krippendorff, "Design, an Undisciplinable Profession," in *Design as Research: Positions, Arguments, Perspectives*, edited by Gesche Joost *et al.* (Basel: Birkhäuser Verlag/De Gruyter, 2016), 197–206, https://repository.upenn.edu/asc_papers/628.

One feature that design and communication have in common is facing possibilities. One can describe any novel design as an innovative combination of existing technologies, technologies, and materials that have been used in other artifacts or developed elsewhere. On the technological level communication researchers have to cope with communicators' networking of media, platforms, website, and texts. There usually are too many combinations to be evaluated or studied empirically. Although there is a mathematical discipline called combinatorics, designers usually are unable to try out all combinatorial possibilities for satisfying a design, as observed by Herbert Simon, and the social sciences tend to limit themselves to its observable elements. Combinatorial possibilities, central to everyday life, are rarely if ever enumerable, much less observable, severely limiting social and design research.

The other feature that design and communication have in common is coping with language. Designers talk, present their ideas to their stakeholders who converse among themselves, figuring out to what use a design may be put. Human communication obviously takes place in language, use of which constitutes who communicates with whom and how facts are verified. Language can be described as a combinatorial system. I described its use in conversations as a process of evolving innovations.

Let me interject here an experience of using ordinary language to inspire designs. A few years ago I attended a conference on the semantics of design in Basel, Switzerland. One presenter was a Turkish student, Ozge Celikoglu, who presented her ethnography of Turkish tea drinking. Her findings were very interesting and sound but didn't lead to any design, which was her intent. We had a long conversation on what was missing. She took my comments to heart and much later, when she had to start developing her PhD dissertation at a university in Istanbul, she asked whether she could study with me for a year. I told her that I was teaching at a school for communication, which wouldn't contribute to what she wanted to do. She was undeterred, came to Annenberg. Luckily, there was an office available—the best office, in my opinion, better than mine.

Ozge was committed to develop ethnographic methods in the service of design. She already accepted that traditional ethnographies of how people lived their lives would not be of much help to designers and searched for a new ethnography. We agreed that design-relevant insights could be found primarily where problematic concerns were discussed. She settled on issues of health and joined several online discussion groups: Fitbit, Weight Watchers, etc., as a participant. Although she identified herself as a design researcher, but her motivation to join the online sharing of concerns was soon forgotten, fulfilling ethnographers' ideal role. She became a fully accepted member of these online communities concerned with health issues, weight loss, exercise, nutrition, sleep, etc. She could ask questions and received personal answers. She shared her issues and received well intended advise. She recorded her interactions, which revealed a rich account of concerns in these group.

She was thinking of content analyzing these recordings. However, it wasn't difficult to dissuade her from applying fixed categories to these records, as they would be her categories, violating ethnographic standards. In fact, the members of these discussion groups applied their own interpretations of what they heard and responded to them by revealing their own experiences to each other. These records were the best accounts of the health issues within a community of common concerns. So, I suggested that designers were ones best qualified to speak for themselves and respond to these accounts. I knew a professor at the design department of Drexel University who volunteered a group of graduate students in design to speak for designers.

Ozge's tape recordings were voluminous. She could not transcribe them all. I also know that volunteering designers could not be expected to read lengthy transcripts. So, Ozge selected a few transcripts that more obviously revealed health problems, gave them to the group of design students plus three color felt pens to highlight sections of something they had not known, of something that inspired a design opportunity, and of something that provided information needed to evaluate an eventual design. A content analysis of the three color-coded segments formed the substance of Ozge's dissertation. Her basic insight was that interviewing people about their needs is likely to reveal only what is already known, seen in the hands of others, or heard from others. However, listening attentively to how people described their health issues, how they coped with them or failed, what was difficult for them to do or remember, got them into troubles, required too much attention, or was wasting their time, is what inspires designers to think of designing useful interventions. Indeed, it is a professional skill of designers to not merely solve problems given to them by a client, but more importantly recognizing possibilities not seen by those coping with their health issues and conceiving useful interventions. Ozge's dissertation was not only providing a new research method for user-centered design. She completed it in one year while in Philadelphia and freed of the usual obligations. Other PhD students in Turkey work maybe three years on their dissertation. She passed her dissertation with flying colors—I went to Istanbul for her defense. She wrote two articles from her dissertation, one just published.

Recently, we collaborated in proposing a generalization of this method, calling it *An Ethnography of Unimagined Possibilities*. It recognized that traditional ethnographies, concerned with understanding and describing the actualities of everyday life of people, may not reveal the unrecognized possibilities that exist in what people do. But asking people to describe how they cope, what they have to do to succeed or avoid failures, offers designers insights into what they might contribute to what people faced and even be somewhat assured that designs that realize such unrecognized possibilities have a chance of being accepted as improvements or substitutes—in their users' terms.

I made the role of language accompanying any new artifact a point in my paper on the cybernetics of design and design of cybernetics. However, if that language comes from a producer it takes far more effort to make a new product popular than when the language comes from those whose lives are improved by it. Relying on descriptions of what *is* keeps designers confined by existing concepts, limiting innovations.

One aspect of the ethnography of possibilities is to exploit non-obvious correlations. I'm always fascinated—for example, when I learned to drive, actually in a Volkswagen in Germany, all cars had red blinkers on each side. To signal the intention to turn required you operate a switch that extended the blinker out like this. After you made your turn you had to operate a switch to put it back. But many drivers forgot to do so [laughs]. And it often caused fellow drivers to point out to each other to turn the blinker back. I have no idea when and where it was realized that there is a correlation between the moment one should turn the blinker back and straightening the steering wheel after the turn was completed. We are now accustomed not to worry about switching off the turn signal on account of that correlation. Ordinary drivers would at some point realize that their turn signal was on but surely not that this action could be accomplished by operating the steering wheel as usual. People are able to tell you "I forgot" to turn the turn signal off, to take my medicine, or to switch off the gas flame after removing a pot from the gas range—the possibilities of coupling such mistakes with normally unnoticed correlation is what designers can explore.

So, research of what is possible is fundamentally different from research of what exists. Interestingly, information theory deals with unobserved possibilities. In its term, correlations of the kind I described above are redundancies. In communication theory possibilities are expectation, and messages received reduce the uncertainty of what could have happened. Information theory provides another link between communication research and the domain of possibilities in which designers operate.

What an ethnography of possibilities seeks to reveal are issues that people may not have the ability to conceptualize and the language to express, but could lead designers to possibilities worthy of explorations. Design is always concerned with realizable futures. In the domain of health, people do not want to be overweight, depend on pills with side effects, become invalids, unable to pursue their missions and enjoy their life. These general goals are easily stated but say little about what designers can contribute to achieve them. This is true for many domains of everyday life. Actually, I had a student, you know her. She is now back in China interviewing especially young women about their frustrated aspirations. In China women don't play important roles. If you look at the [Communist] Party Congress, there's not a single woman. So Chinese women face many obstacles. Having studied in the U.S., she has acquired self-conceptions she may not be able to realize at home. So she is interviewing women to elicit accounts of the obstacles they are facing, by implications what they envision for themselves but seem unable to achieve. She discovered that accounts of frustrated aspiration fail to recognize the interviewee's participation in what holds them back. Instead of recording such frustrations, she is looking for possibilities of reframing what her interviewees take for granted but don't have to, thereby opening previously unimaginable paths to achieve better futures, not necessarily the one envisioned but equally or even more promising ones. She is not interested in designing technologies. Her approach resembles more that of a social therapist educating women about what keeps them entrapped in search for previously unimagined emancipatory paths. Her approach is not too different from extracting unimagined possibilities from the narratives of people concerned with health issues.

So design as well as the aim of my Chinese student to reframe the confining perception of her interviewees is very much related to my long-term commitment to constructivism. I don't know if you want me to get into this now or another time. Personally, I don't like "isms," because that looks like a commitment to an ideology, and I don't have that constructivism is one. But I do think there is something to understanding language not as representations of what exists but actions that can do things. We need to become aware of the fact that even talking about things, describing what is observable, can have social consequences. I mentioned earlier that the work of journalists, committed to describe facts, have unintended yet essential consequences—for example, for citizens to make informed decisions that shape the politics of a government. Facts are rarely neutral especially when cast in words.

Describing facts accurately is the aim of the sciences. Objectivity is a celebrated quality of scientific research, but its claim merely absolves scientists from being held accountable for the social consequences of their findings. For example, racism has certainly been nourished by early anthropological categorization of human races. I know, I'm getting objections when I claim that any battery of interview question that includes questions about the interviewee's race has the potential to find discriminating correlations that wouldn't be there if not asked, answered, analyzed, and published. Herrnstein and Murray's widely cited work on *The Bell Curve* of intelligence found correlations between African Americans, low income, low intelligence, and high probability of being convicted for a crime. The authors interpret the persistence of these correlations as genetically determined. Armed by such questionable interpretations, which employer would want to give a responsible job to someone with such a genetic makeup. I had a Wharton School [at the University of Pennsylvania] student. She did a telling experiment before she came to my seminar. She presented the same job applications once with a picture of a white candidate and once with the picture of dark skinned one to a board of employment specialists. As to be expected, the application with the picture of a white candidate was considered far more qualified than the application with the picture of a black candidate. The consequences of publishing correlations with ethnic variables supports existing prejudices and keeps minorities from advancing out of poverty and being admitted to better universities. The consequences of interpreting these finding in genetic terms rather than as the result of the long history of discrimination suggests that the world we take for real is not only accessible through the language we employ but constructed, made painfully real in these terms.

So, even carelessly introduced categorizations may have profound consequences for the targeted populations. Categorizations of people is the source of all prejudices, causes of wars, and how interchangeable employees in bureaucratic organizations are defined.

My constructivist stance is informed by what I mentioned earlier, my conception of discourse. I am not taking discourse as an all-embracing regime like Michel Foucault does. I see coherent discourses practiced in more or less self-organizing, autonomous, and hence distinct discourse communities. The legal discourse practiced in courts of law has little in common with the medical discourse practiced in hospitals of the medical system more generally. The social scientific discourse is practiced in academic institutions. While all may be based on a common natural language, specialized vocabularies, shared metaphors, logics, and certifications of competence, for example, academic degrees in the scientific community, makes it difficult to cross discursive boundaries—except as functional appendices. For example, a philosopher may choose to be a patient in a doctor's office or a plaintiff in a court case. Mutually exclusive discourses can entrap people to see their world in a discourse-specific way without appreciating what other discourses do. To me, the most important feature of discourses is that their communities construct their own artifacts, their own realities. The world of physicists consists of a coherent universe of theories about nature, together with the instrumentarium to prove them. Physics is incompatible with biology and both have little to say of how the social world is constructed. Commitments to particular ideologies can create political institutions, distributions of wealth, methods of production that can entrap their believers, unable to think otherwise. In the United States political life is interpreted by its Constitution, references to enable an ecology of numerous discourses to create their realities.

I have one student right now who is a Kurd from Syria. His term paper analyzed the discursive struggle for defining Kurdish identity in a world of competing interests. The Kurds have a common language and occupy a contiguous geographical region that was split into three now hostile counties after the fall of the Ottoman Empire. Most Kurds are Muslims, but it was Iraqi Muslims who attacked them, bombed them, and committed genocide. Kurds had an older religion that is opposed to Islam. But as soon as its practices are invoked it divides the Kurdish people. Kurds are determined to be recognized for who they are but are struggling how to define themselves without challenging the nationalistic discourses that seek to claim the Kurds as part of their own. The Kurds have histories to draw upon that the competing countries do not recognize. The point of his paper is that these deliberations, if one can call them as such, have very real consequences and take place in language. The process exemplifies the discursive construction of competing realities, hopefully coming to a non-violent conclusion.

Q: Well, I thought maybe because we've had a chance to talk for these five sessions, and you've had this career trajectory that has placed you in one institution for most of your career, and that has had this communication focus and label—and yet you have carried on, using the discourse idea, with other major lines of thought, cybernetics and design seem to be two major ones. And, to mix my metaphors still more, you are kind of acting as an ambassador between communication, on the one hand, and design, and design back to communication. And the same thing being true with cybernetics. So I just thought it might be interesting to close on the question of how you have navigated between these different discourses, including the one that was your institutional home for the entire stretch. How have you managed to intermingle them in a way that enriches them all?

KRIPPENDORFF: You are right. When asked this question I'm always saying that I am wearing at least three hats—design, cybernetics, and communication. Well, I think maybe that has something to do also where I studied in Urbana [at the University of Illinois], this truly interdisciplinary program where I took courses in anthropology, linguistics, communication, cybernetics, and learned to combine them. When I'm talking to designers, I am likely seen as a communication scholar and cybernetician. When I'm communicating with cyberneticians they recognize my working with Ross Ashby and other folks at the University of Illinois but respect my focus on communication and the use of language. At the Annenberg School I'm known for my work in content analysis and contributions to theories of communication but have never hidden my cybernetic or reflective approach to communication and my background in design. In fact, I was the one who designed the *Bulletins* of the Annenberg School for quite a number of years after I joined the school. So I think what I'm teaching at the Annenberg School is, as you say, very much influenced by all of these areas.

At the Annenberg School most colleagues do not know or care to know the sources of my inspirations, largely because they too come from different areas to the school. George Gerbner got his degree in education. Other early faculty members came from psychology and sociology. Although I became the Gregory Bateson Professor for language, culture, and cybernetics, I did not want to brand myself too narrowly. I prefer the interdisciplinarity that fascinated me to come to the United States and study in Illinois. I have designed a lot of things, but if I were to present myself as a designer [laughs]—which I am not—I would easily be boxed into a category that has no place among communication scholars. I have made, I think, a lot of contributions to the communication field for theories to grow and research methods to practice without having to justify where the ideas come from. Some of them appear revolutionary within the discourse of the community of communication scholars.

I already mentioned my ICA presidential address. It was preceded by several innovations I introduced in the conduct of the ICA. To me, it was very important to propose changes in communication scholarship in response to changes in the world of communication practices—not that these changes were adopted with ease. You could say that this was the designer in me speaking. Personally, I think I'm blessed by having—by being actually recognized and competent in all three discourses. Recognized—I got an honorary doctorate for my contributions to design from the University in Kalmar [Linnaeus University in Sweden]. I received numerous awards in cybernetics, among them the Norbert Wiener Medal from the American Society for Cybernetics (ASC) and a similar recognition from its European counterpart. I became a fellow of the American Association for the Advancement of Science (AAAS), and an honorary member of the board of the World Complexity Science Academy. I spent a year at the Netherlands Institute for Advanced Studies (NIAS). These were recognitions from outside the field of communication within which I served as ICA president—the first member of the Annenberg faculty. I have received several best paper awards in communication, was elected a Fellow of ICA and my content analysis book was recognized for its contribution to communication scholarship.

Because I could cross from one discourse to another, that made me, I think, a productive contributor to all. When asked to advise young communication scholars, I emphasized the importance of being able to draw on several discourses. The worst is being stuck in one. The second most important skill is to be versed in more than one language—not textbook competence but having experiences of how other people's language bring their reality into being. Knowing another language allows one to appreciate the nuances of one's own. In my seminar on the discursive construction of realities, I always ask students what languages they speak and what cultures they have experienced. I don't mind teaching English-only speakers, but speaking a different language gives you access to alternative realities.

For example, this just completed semester I had an undergraduate in my graduate seminar. She had traveled all over the world, experienced different cultures and had much to say even being an undergraduate among graduate students. To make creative contributions, as I said earlier, conversation is its key. In conversation one has to respect alternative realities and be open to challenges from different perspectives. In all of my teaching I try to create an atmosphere for dialogue and conversation as far as possible. Actually, the undergraduate I just mentioned wrote a superb term paper about different kinds of tourism in foreign countries. Although from a social scientific perspective she had far too few interviews to draw upon and I will talk to her next week. She examined different kinds of tourism, and asked tourists who organized their trip, what did they visit, what did they eat and drink, what contact they had with the native culture and language, and what did they take away from their trip. She distinguished between tour tourism, like touring with a bus, as opposed to backpacking, and interviewed some of both. I have the suspicion and will ask her whether she was a backpacker [laughs]. It was very clear that backpackers were going to places where not everyone goes, often following leads that were not planned, quite unlike bus tourists, who went to must see sites, read about them in tour books, had barely contact with native speakers and ate food that resembled what they were accustomed to at home. Her account of what backpackers experienced gelled with me, because I was a backpacker when I was a student in Germany. Each summer we hitchhiked to different parts of Western Europe from Lapland to Yugoslavia, France, Italy, you name it. As students we didn’t have the money to pay for trains, so we hitchhiked on highways and country roads, met a diversity of people who were eager to talk to us on long rides, invited us to their homes for meals, showed us what they thought we should see, brought us to folk gatherings we could not know of, much less plan for. This student paper resonated with my own experiences and explained why she, an undergraduate in a high-level graduate seminar, was so exceptionally insightful about alternative realities and the difference that language makes in talking our world into being.

The point is that scholars of communication need to be open not only to appreciate alternative ways of being but also see the unfamiliar distinctions people make in language which give rise to alternative texts, technological artifacts, discourses, and ways of being in the world. Observing language not merely as grammatical representations of existing realities but its role in the process of communication—creating livable realities—is a path that communication scholars can explore and make available to everyone who cares.

Q: That is a wonderful point to stop, and I just want to thank you so much because I found the conversations we've had to be incredibly stimulating, and informative, and thought-provoking. We have got right back to your childhood in Germany in the last moment [laughter] with the talk about hitchhiking and backpacking, so there's a kind of narrative bow-tie there, too. So, thank you.

KRIPPENDORFF: Thank you. It was also enjoyable, for me, to articulate some of the experiences you asked me about.

## END OF SESSION FIVE