

Communication Scholars Oral History Project
Annenberg School for Communication Library Archives
University of Pennsylvania
Philadelphia, PA

KLAUS KRIPPENDORFF

interviewed by

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transcribed by

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recorded by

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December 20, 2016

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

Philadelphia, PA

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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 30

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 53

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 73

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 94

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 111 pages.

BIBLIOGRAPHY AND CITATION FORMS

Video recording

Bibliography: Krippendorff, Klaus. Interview by Jefferson Pooley. Video recording, 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, video recording, 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.

Transcript

Bibliography: Krippendorff, Klaus. Interview by Jefferson Pooley. Transcript of video recording, 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, 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 of interview conducted December 20, 2016 with KLAUS KRIPPENDORFF (session one)

Philadelphia, PA

Interviewed by Jefferson Pooley

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. They grew up both in Dresden in Saxony in Germany, and my grandfather—whom I never met—died early in 19—during the first World War. And that was also a tragic situation. He was a lawyer and then he was drafted by the German Army in the First World War, and as an academic you can't be an ordinary soldier, so he was immediately an officer. [He] had to ride a horse, fell at some point from the horse, got a hernia and died in the military hospital. So this is kind of the background of an unfortunate European history of war and the tragedies and unnecessary dealings.

My father was—became an engineer, and—an academic engineer, and later on wanted—in the term of his practice, he had to go, I think, for half a year, [to] do something practical, and he hired himself or was hired on a ship going to India. So he was an engineer on this ship and going to India, and came back with rice and gifts for my grandmother. And then at some later point he was a student again and, in 1924 I believe, he—there was a program to study in the United States—[a] work-study program. It was actually, basically, giving the—offering the fare, but once you're there you have to almost take care of yourself. So he went to the United States and he was fascinated with mass production. He worked, actually, among others, at the Ford assembly plant, and made many friends: a Frenchman that he communicated for the rest of their lives with each other. So, yes, that was, I think, his American debut. Then he was hired a few years later to a man in [an] office in New York, because these German students that came with this minimal support in the United States—some of them got lost. So he was actually in charge of keeping track of these German work-study students, actually [unclear] or "American Work Studies Program." And so that was in between 1927 and '29.

My mother's father died when she was very young, also in 1919. He was a painter and he had bronchitis and other minor complications, and because of malnutrition and maybe bad medical service he just didn't survive what nowadays would be no problem. So he died in 1919. That

was for my mother a big threat and so then she had to work, and her mother and her grandmother, the three of them. And actually my mother had also a sister—she married relatively early, she was out of the house—so my mother had to work in the bank first, and then she worked in Dresden in the Technical University [Technische Universität Dresden], where my father studied, and she became kind of an account manager for student budgets, etc., etc. So all these German—well, several of these German students—they were very enthusiastic going to the United States, and she said, I want to go too. And she wrote, actually—and I read it recently in her biography—she was actually in some sense primed because she had a teacher who, in geology, was always fascinated with Yellowstone Park and 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 and at some point three other German students said, We want to go cross-country, driving with the Chevy cross-country. And they said, Well, 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, actually, I got from both of my parents, stories of the United States: the adventures, the people that she met—they both met. They got actually engaged at Niagara Falls, and I went also at some point to Niagara Falls, and when I was there I called my mother and she reminded me of the Misty Queen, which is the ship, and she remembered that, and we just took this boat also [laughs].

So that was kind of her background, and there are lots of fascinating stories—my mother never had a good education, and she basically was a housewife, one could say, but one who had so many initiatives and energy that she just couldn't stay like everyone else. And she helped, for example, in a project later on when we were born—she helped other mothers to exchange baby milk—some had too much and others had too little. And so she did a lot of things besides being the mother of four children. As I said, I was kind of primed because we had constantly talked about the United States. And my parents actually talked English when we children couldn't/shouldn't understand it [laughs]. There was also, which is kind of interesting, because it was during the Nazi time—it was dangerous to be kind of, I wouldn't say associated with the enemy, and the United States was never really an enemy of Germany—but my father had always hidden two things from his past memory: One was an American flag with 48 stars, and another one a white ball that he played with in the Panama Canal when he was going to India. So these two things, they were always there [laughs], but they were hidden and not many people knew about this.

So, as I said, my father was an engineer and he changed jobs every four years maybe, and during the war and the end of the war we ended up in the city called Halberstadt, which is in the middle of Germany. It is a small city, 56,000 people, and my father was actually—saved himself, I think, from the draft by working in an airplane factory and building wings. And that was for Junkers [Junkers Zweigwerk Halberstadt] and that was a factory in Halberstadt, and that's what he did.

But I think maybe I should say something about Halberstadt, just briefly. I think, in my experiences, when you move around, then you start being someone when you move out of the

family. So while? my earliest recollections go further back, but where I really feel at home is Halberstadt. The main reason was when I was 10 years old, I went out—have a gang of friends. We climbed all trees, we knew everything on the street, we knew the holes in the fences where we could climb through and bother neighbors [laughs], if you want. And there was a place, a garage of sorts, and there were no cars at that time anymore, but we wanted to play there and we were always chased away. So I mean that was kind of where I went out of the family. And then came, of course, my studying in the Gymnasium, so then I learned a lot about the city. And if I'm being honest, I think I know more about Halberstadt still—about the mythology, the history—than even about Philadelphia [laughs]. So, and [Halberstadt] is an old city that was founded in 800—it was a bishop's seat—and one thing that I learned far later, that the bishop that was there—actually I can show you later on, there is a map of the old city and there is one wall around the bishop's seat with two churches and with the accommodations for the bishop, and then there is a bigger wall for the city. And the bishop gave, actually, Jews protection for whatever reason, and probably they paid a lot of taxes. I don't know, I can't say, but Halberstadt was at some point a large city—had the largest population in Germany of Jews. There was a pogrom in Halle, which is not so far, and they were all moved to Halberstadt. Halberstadt was also open to other kinds of immigrants. There were the Huguenots that came from France, so Halberstadt had a French church—It must have been a very interesting city.

As a child, I didn't know that much. I actually, I didn't—I met—I remember very distinctly I had one experience of seeing an old couple with the Jewish star, and I didn't really know what that meant and so I asked my parents, my mother, and she just explained these are Jews, but there was no hostility, at least on my parents' side. And I learned much later in 1943—oh no, before that I learned there was a program called KLAUS, K-L-A-U-S, that means Kinder Liebe Außer Sichtweite, or Children Better Out of Sight, and this was a Catholic organization that took Jewish children for a vacation in the North Sea so their parents could find places elsewhere in London, in the Netherlands, to go there. So the Halberstadt Jews, they were actually maybe better off than most, and most of them disappeared. But in 1942—and as I said, I have no recollection of that but I read later—there were about 400 that were picked up and never seen again. So now but, as I said, this is what I found out much later, but I had no clue, my parents neither. I mean, yes. There was in 1945, the end of the war, we were bombed out totally. And that was actually on April 8th, that was a Sunday morning. I was with a friend not so far away, and exchanging stamps which we both collected. And then came the alarm at about 10 or something, so I had to rush home, and at 11:30 bombing started, and the whole city was very badly damaged. But when I say damaged, it was really I think something like 80 percent of all houses were destroyed, and this was, one could say, an unplanned attack. I read later that there is a famous German author, Alexander Kluge, who was in Halberstadt and knew him from that time. He was in a different high school. There was a reformist or science-oriented one which I was [attending], and the other was more humanist or Greek-oriented and he was in that. But he made this research as to what happened and what people recalled and what they did and so on. So he described in great detail, among others, that Halberstadt was not really the target. This was the secondary target, because there was another area that they had to bomb, and there was no—there were clouds and they couldn't see, so they had always secondary targets,

and tertiary ones if they couldn't get rid of the bombs, because they couldn't land with the bombs in England.

So anyway, there came about two waves of 200 bombers each, and they just threw their bombs on Halberstadt. Now, Halberstadt was not a military town, there was nothing of—there was not strategically anything important in Halberstadt. There were several hospitals. There were the Junkers Works [Junkers Zweigwerk Halberstadt], but they were outside, and they were not actually harmed. So it was an unfortunate incident, and in my case my parents—my father was around, that was very nice, but my mother took my other two children to a small village in Schwanebeck about seven miles away, mainly because almost every night we had alarms and we had to go in the basement, and huddling in the basement with blankets over their head. And so that was at some point too much, and so she took them to Schwanebeck. Myself and my father we were in Halberstadt and when the bombing started our house was hit, and there was a bomb that was maybe about 10 feet away, maybe 12, 15 feet away from where I was, so had I been just this much closer I wouldn't be sitting here. So, half of the house collapsed, and I remember for example, one scene—my father was 45 years old, and he was kind of at the prime of his life. He knew what to do, and there was a refugee from the Rhineland and there was a mother and daughter and the daughter's baby. So they were living for temporarily in our house somewhere, and when the bombs fell and everything was thick of dust, the baby stopped breathing and the mother, she was Catholic, and she screamed holy Catholic prayers, and so my father simply took the baby and dumped it in the water bucket that was there and gave it back to her, and she was fine. I mean this was the kind of things that I do remember.

Then I went out of the basement and climbed through a window on the street, and I couldn't recognize my own street. And as I said, I knew all the trees, there were no trees left, everything was bricks. I had to stumble over the bricks and then I went to a—there was a school close by and we had made an agreement—my father always said go to this and I will pick you up or something. But when I came there I was with other people from the same house, they said you can't stay here, this is too dangerous, everything is burning, etc., etc. I should also say the strategy was, first, they put explosive bombs on it and then on top of it came—what is this called?—napalm-type bombs that got the wood that was now exposed all on fire. And when I left the house and walked over, then I saw already the fire bombs starting and exploding. And so I went to, actually—to a house of a friend of mine that was just half a block and I wanted to see this friend, or at least I couldn't see him. But I stumbled through a staircase that was collapsed and it was very difficult to walk on the edges of those steps and then I—then another wave of bombing came, and then something very unusual for me. But in these emergency situations human beings huddle together, and so we, lots of people, maybe 15, we just huddled together. And then—just because there was so much noise and droning and it was actually not in our area where the bombing took place. But then I looked up and half of the house was hanging over us, so any small thing would have killed us all. So I went the other way and went to another village, by myself or with these friends, and later on I joined my family in Schwanebeck where my mother had been.

My father, meantime, he—as I said, he was a very energetic and strong person, 45 years old. He tried to save whatever he could. He went, in fact, up on the house, the stairs were still intact,

he saw his writing desk down there, and then he—we had one guest room and there were our shoes, and so he took the sheets of the guest room, put the shoes on it, bundled it up and carried it out because that was kind of a very valuable thing. Later on he tried to save this from our burning house to another house. Anyway, so it was pretty bad, but I went, as I said, outside and when I—there was a mountainous area before we went to this village, there I could see Halberstadt burning. And I had never seen a whole city burning, the flames, they—I mean two houses high, red flames of otherwise black smoke in great—I mean in great quantities. So that is the last thing that I saw of Halberstadt, you know, at that time.

And then I went, as I said, and joined my family. And they were very worried, rightly so, that I would be not available anymore, and so that was the end of the war. I should like to say, this Halberstadt on 8 of April, the bombing, was just three days before the Americans marched in, and it was, as I said, totally meaningless militarily speaking. And when I was in this little village, Schwanebeck, then the Americans came and this was really not fighting anymore. There at the entrance of the village where we actually lived for a moment, they were shooting one shot in the direction, and then there was no response and then they drove through. That was the end of the war as far as I'm concerned. Well my father had no job so—but he knew that there were—in close by there was a train with machines that was in—I mean they were transported from, I don't know where, in the last minute of the war. And so with the help of American commanders who were actually very set on getting the German economy going again, he got some machines and started a mechanical workshop. Now he was an academic engineer and not really a practical one, but he had the idea that there are so many cultural machines in this little village that were neglected during the war, and so he would repair them. And he had this mechanical workshop. So, and that was right in May of 1945.

But then the Russians came, 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 get it going. And maybe I can tell you just why he was arrested. It's a sad story but it's—that's what it was at that time. He was, as I said, an engineer in the Junkers factory in charge of transportation, and the transportation at that time was kind of electric cars where someone was standing in front had to do all the stuff, and he went from one place to another. There were lots of—at that time there were lots of workers from the east, from Poland and Ukraine. They were in Germany and some of them were basically forced to live—work in German factories. Some of them volunteered because there was nothing over there, whatever. But there was one driver, a German driver, who was fraternizing [sic: franternizing] with these women and was behaving pretty badly and [Krippendorff's father] said at some point, Look, you have to respect these people, you cannot just talk down to them or misuse them or get involved with them. And so that was one story. Now after '45 he went to Halberstadt for whatever reason and met this guy, and so he said, How are you? etc., etc. And I have to tell you that the boss of Junkers, his boss, is in prison. And my father said, You know I'm sorry to hear that. And that was it. The next day, and they asked also, Where are you? And my father said Schwanebeck and so—and the next day the Russians came with a truck and picked him up. And then, when he went in Halberstadt prison, who opened the keys? That guy.

So this is the way justice was administered after the war. And he spent about half a year in prison. Then he was transported to a concentration camp which the Russians now ran, and the Russians had a system of quotas, so they had to deliver a certain number of prisoners from Magdeburg, where he was, to this concentration camp, and many of them died on the way, so they gave additional and he was left over. He was left over—everyone was picked up and he was left over—he was sent back. And now he was kind of off the books, one could say, and the commander of the—the Russian commander of the prison called him and said, You innocent, tomorrow you back with children, but what can you give us? Now, we were bombed out, we had nothing. So my father said, Well, I have—I run this mechanical workshop. I'm happy to repair things, whatever, I can do whatever you want. But that's not what they wanted, and he found out later when there were several others who were released, that someone had to bring a watch to the translator or—something like this. So this is the way it was. Well, we stayed there—oh no, while my father was in prison my mother had difficulties with four children and in a small village, and not that much to eat. But my father, because he had worked—had this mechanical workshop—he had lots of friends among farmers.

So we worked also a bit, as much as we could, on farms picking potatoes, etc., etc. So we got as a payment potatoes and something to eat. So we managed pretty well, but my aunt, my mother's sister, said, Why don't you give me one? I'll take them to Dresden. That was me. I was the oldest one. So in Dresden I was, and I went—that's when the school started—and I went in fact to a school in Dresden, and it turned out to be the same school that my father was in a long time ago, and one of the teachers even remembered my father. So it was, you know, coincidental, whatever. But I have to say I was very good in mathematics. I was very good in biology. I could draw flowers and all of these kind of things. But I didn't have any interest in Latin, and Latin was part of it. And now there comes a kind of—as a 13-year-old person you don't appreciate what is maybe important in the future, and I hated it. I said, Who in the world would want to learn a dead language? So, well, nowadays [unclear] have done very differently, and I have to say I probably blame my parents because they listened to me. And I said, I want to get out of here, and they took me out of school and I became—I went home to Halberstadt after a year, and then I became an apprentice in a mechanic's shop. An apprentice—there's this old German craftsmanship, so that you are [for] three years [an] apprentice, then you become a journeyman, and—then the journeyman you go from one place to another, ideally, and then at some point you get married and you become a master, and so that's kind of the tradition. So I became, actually, an apprentice from 1946 to '49, and during that time my father then came back and worked further on this mechanic workshop.

But at some point the Russians decided that whatever was before owned by the government should not be in private hands. So they picked up all the machines, and then my father was again out of [a] job. That was in 1949, and then there was of course the border between the Eastern zone and the British and French zones, and he had a good friend from the time when they were both in the United States as American exchange students, and he said, Well, I can give you a half-time job—find a half-time job in a company—in an organization that is charged to revitalize the German industry. So he and my brother, two years younger than me, they went to West Germany and he started this job, and I continued through my apprenticeship.

But now how to get out of there? Well there is—maybe I should go back quick. Before we decided to go, I mean just to get food—we had chicken, we raised rabbits for food, and we were connected with the farmers, we got turnips to make sugar syrup. I mean—it's amazing to me how my mother coped with it all. But anyway, we did—oh, I have to tell you something else. Before, as my father was in Russian prison, we were—that was right after the war ended and the Russians had come. We were living in a small—on the second floor of a tiny, tiny house on the roof, all five of us—six of us. It was intolerable, but the mayor of the town said, Well, you have to have a place to live. And there was an empty storage house, one could say, and they said, Well, we'll make that into four apartments, four places. And it was empty but there were—this was made of prefabricated panels, very primitive, not much insulation, but—and it was unfinished. [Unclear] said we could arrange the walls in the way that we wanted. So I was 13 years old and I found an amazing solution using the number of panels and the number of doors I had available, and arranged the interior in ways others didn't. The standard way was to divide the area in four rooms and have an entrance and that was it, and the entrance was kind of a second door against the window, and that was it. And in my case I decided to make a hallway along the line to have a door in the back where there was a shed, and where there was chicken and all of this, and the toilets. And then the rest went one big room and two small ones. So the number of panels I had available—and that was, 13 years old, the first architectural project one could say [laughs].

So, that's where we lived in, and it was really very livable. There were lots of minor inventions that I made, but now, coming back to the end of our stay, when my father left to the West—that, we couldn't tell anyone. And one should also say this village was very small. Everyone knew each other. Everyone observed each other. There were rumors of everything. So we had to carefully plan to escape without notice. Now I wanted to finish my apprenticeship. My mother sent parcels to her sister in Dresden, so that the post people would know that we moved to Dresden. She bought the train ticket because they know also who is going where, and all to Dresden, and then they stayed there for a few days. And as soon as I had my apprenticeship finished, the next day I was supposed to meet them in Halberstadt. I was in this small place in Halberstadt, and there we hired a teacher to get us over the border. But the very evening, the last day I was supposed to be in Schwanebeck a farmer came and said, You know, I was in the City Hall and they—I overheard a conversation. They couldn't find the Krippendorff in Dresden, and where is he? And then the City Hall—there is one Krippendorff here. Get him to tell him where his father is. So that was kind of a—the very evening the farmer came and told us this. So the next morning the first train was for 4 o'clock in the morning. I was on the train and was gone out of the village.

So later on I met—in that place, I met my mother and my sister. She was then nine years old, and my little brother, who was then six years old—five years old. So we had this teacher—now I have to say Halberstadt is not too far from the border, that's one thing, and we knew kind of the area, but this teacher, he had connections. He lived there for a long time. So we started out late in the afternoon taking a train to a place very close to the border, but from then on we had to walk, and quite a bit. But in order to come to the border one has to avoid people, and there are lots of tiny, tiny villages, farm villages, and most streets go, of course, through the village and then out, so we couldn't do that. We had to go around. And I remember it was dark and

dogs have a way of hearing unusual noises or sensing things, and when there are many dogs, this doesn't—you have probably no experiences of that, when there are many dogs in a small village they stimulate each other's howling. And I still hear this howling of the dogs when we passed by these tiny villages. Then there was another incident, that this guide—he was just very, very clever I think, or sensitive. He heard some noises in the back on the street. Now, on the streets in Germany in general there is a street and there is a ditch and then the field starts, and then are trees before the ditch. So he thought, there's someone coming, so we had to go in the ditch and waited. And now this is also difficult—a little child is not so easily kept silent. So that's where we stayed and then three Russians came talking, talking, talking on the street passed by, and then—so that was kind of a tricky moment. Then we went to the other side and then it became morning and then suddenly we ran into soldiers, but they were British. And I remember that 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.

I remember I took the train from there to where my father was in Düsseldorf in the western part of West Germany, and I was surprised to see so many cars, and so many—it was just very, very different. This was West Germany. So, that was our escape from East Germany. I should also—maybe I should tell you another story. While we were in Schwanebeck, and my father had come back, he invented or made, produced a mill to make poppy seeds into oil. So kind of a hand cranked—and so he made several for people inside for Schwanebeck and Halberstadt—and someone in West Germany got hold of the idea, I don't know how, and so there was a plan that my mother would bring one of those mills and get a piglet in exchange. Now, a piglet is something you can carry in a knapsack, so the farmer said they make noises and we cannot afford that when we go over the border with controls, and so he gave the piglet some sleeping pills or whatever, and so on the way—on the train to West Germany—to the last village before the border, we looked at the pig and it was dead. So I remember distinctly we had to get rid of it, and so she—we opened the window and she just dropped it out. And I remember how it bounced! There were kind of threats—how it bounced, I have these sounds in mind. But we had at least no piglet, we had no fear of being arrested crossing the border. Many, many years later, my mother was a good storyteller and, you know, with other friends, and there was a woman that said, I was on the train to Helmstedt, this village, and there was a woman, she had a pig and she had to throw it out of the window [laughs]. So it was just, I mean, amazing things, amazing experiences, but we survived that all. So in Halberstadt—in Düsseldorf or rather Ratingen—there's Düsseldorf and there's Ratingen where we were—where we lived on the outskirts, and that was actually even called a different kind of village. We had a very tiny wood house given to us by the same guy who got my father the job. There was all 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 this, and I got a job in a factory, as a mechanic, as a toolmaker—but I was never really satisfied with what I did. So one of the things is, and you wrote down, they wanted to know about the Wandervogel and I got involved in the Wandervogel. Now—

[INTERRUPTION]

KRIPPENDORFF: Well, the Wandervogel is a very German organization, one could say. Or not even more than a movement, let's say, and it started around 1900 when German youth

discovered nature. Before that, I mean, everything turned around the city life or farmers and so on, but nature was just not accessible or of interest. So in, I think, 1908 there were a group of people that thought, We should go on walks, see the forests, and the beauty of the country and so on. So that was in 1908—or maybe a little earlier. That was in Berlin largely. But then this caught on and lots of people were interested in participating, camping in a way—all young people. And in 1911 there was a big celebration of the 100 years defeat of Napoleon. And there were all of these camps—[from] the Wandervogels' perspective, all the bourgeois that didn't really do anything to defeat Napoleon. They celebrated in big cities with wine and beer, got drunk, and so on. And most of these Germans thought, We don't want to do that. And they gathered on a mountain area in the middle of Germany called the Meissner, and they formulated a philosophy, one could say. And saying, basically, we the youth, we want to be our own, determine our own history. We don't want to be bothered by the bourgeois, we want to guide ourselves and our inner—there were lots of kind of strange philosophies, but the point was, it was a movement against the established older generation, towards nature, towards understanding each other. There was, for example, they didn't want to drink alcohol, sex was ruled out, I mean among young people. I mean, this was kind of a back-to-nature in many ways. So, that was in 1911. Many of the people died, actually, in the First World War—they were drafted—but in the Second World War, after the First World War, it started again. And my parents, both of them, were in some ways involved in that. But in 1934 the Nazis took over. They outlawed everything that was of this kind of movement and incorporated all of them into Hitler Youth. There was no way of doing that. I actually—I read a lot of things, the history—one of the things is that we all knew each other by nicknames. Nobody knew who they really were but we knew each other by nicknames, and when the Gestapo tried to infiltrate the groups that didn't want to go in the Hitler Youth they were befuddled because they couldn't figure out who's [a] member [laughs]—what they were talking about. So I think during the Second World War there were pockets of the Wandervogel still existing, resisting as much as possible, but the Hitler Youth basically forced everyone to be part of it, and adopted many of the symbols, like with walking with flags and having—there was a certain kind of a jacket that came from the Wandervogel and they took that over. So it was usurped, if you want.

But after the war—after the Second World War there were several people that lived through—before the Nazi period. Their children started it again, and I was one of them. And it went for me also a kind of a liberation movement, away from the generation that we had said, they collaborated with the Nazis or didn't do anything about them. And we wanted to do our own thing. And, for example, unlike, for example, Boy Scouts in the United States, we never tolerated anyone as a leader that was maybe two or three years older than us. So we were all young people and we were hitchhiking all through—I mean, I was twice in Italy, Denmark, Sweden, and France. We hitchhiked everywhere and we were students, or young people. We had no money, and we all decided that in some sense, you know, we should take advantage—we provided service for people that gave us a ride by giving good conversations and we often did. So we were flexible and moving everywhere but we also participated in many other activities. One of which was, for example, a project to reforest areas that had been cut down. The British took a lot of forests away as reparations, so they cut the trees and left everything there. So reforesting was kind of a summer activity that we participated in, but then also we

tried to make seminars among others to understand the Nazi past of Germany, to study Marxism, and the new emerging—the differences in democracy in the West, and what happens in the East.

And so we tried to educate ourselves in the times in which we lived or what happened before us or what we wanted. There was a very interesting thing that, at one point the East German youth organization, that was a state organization, Freie—FDJ [Freie Deutsche Jugend], Free German Youth Organization. But it was a very communist organization. They had a big meeting in East Germany, and they invited some of the bourgeoisists and they got hold of us [laughs]—that was a big mistake. We were so well-prepared, we came there, maybe 15 people, and we came in this big meeting, and we simply asked questions that the East Germans were not allowed to ask. So, we were ostracized, we were heavily criticized, but we made also numerous friends that stayed in touch with us in many ways. So, that was kind of our way of coping with that problem, and I'm—that was one minor thing. My brother became a political scientist as a result, in fact, of perhaps these wonderful involvements and the trying to find out the ideologies behind that and the structures etc., etc.

So that was, I think, for us, very important. Now then I was working, when I was in the West, in a factory but I wanted to study—and in the East I have to say also, one reason why perhaps the decision of my parents were not so wrong to get me on to a practical track, if you want, of apprentice or away from academics was that as a child of an academic there was a quota and there was a very small number of children of academics who could study. It was made available to workers' children. So there was a good chance I wouldn't have even gotten there. But I wouldn't say that I had academic ambitions, but it was not satisfactory to me to work in a factory, even though I was okay, I could handle this. So I took evening courses in Düsseldorf and I made several practices that were required to become—to start at the engineering school, and then in 1951 I joined the engineering school in Hanover [Hochschule Hannover]. And I graduated in 1954 and that was kind of a first step into a more kind of scientific or engineering activity. I should like to say two things. One is I stayed in touch with Wandervogel and at some point in the summer I decided, we decided, six people, to go to Lapland. The most northern part of Sweden where nobody goes, only Laps exist, and this was kind of a wonderful, always a mythical place where you go, where nobody goes [laughs]. So we—the six of us, we hitchhiked through Denmark, Sweden, and met in Umeå, and then we took a truck to kind of the last outpost where Laps could buy things, and from there we walked into the—into nowhere. I have still a reindeer horn upstairs [laughs]. So, I don't want to tell you our adventures there, but this is one of the things I constantly stayed inside the Wandervogel and also afterwards.

But now in 1954 I graduated and there wasn't a possibility of having an examination to qualify for university education. This was kind of, one could say, a professional education in the engineering school, and there were three people that applied and I passed the examination, so I was qualified to become a university student without the Abitur, which is the high school diploma that one would normally get. I should like to say, also, this the climate in the engineering school—the people that were graduating there or in it, many of them were actually former soldiers that came back in 1950 from Russia, and people that were thrown off in the war. I was one of the youngest, in fact, and there were very interesting people, I have to say. I

stayed in touch with a few—but it was a very different kind of—it was not the normal studies thing. So, anyway, I graduated, then I had—we had another kind of dream to go to Yugoslavia. Now, Yugoslavia at that point, it was a very interesting place. First of all nobody went there, second it was communist but it was independent of the Soviet Union. They had the idea of self government. They distributed the responsibility for factories to the workers and so the government was communist but in a very different ways. And in part that was in case the Russians would attack them, they would distribute the responsibilities so that everyone could defend their factory, etc., etc. So it was an amazing place and we wanted to go there, and so we hitchhiked again, and it was very adventurous. We went to Belgrade, and then we went to Mostar. Mostar is in the middle of Montenegro. It is on a river, there is a famous bridge, and there was actually at some point a movie made, a German movie or German-Yugoslav co-production, of partisans in Mostar, and a German military nurse who worked then for the partisans. And so this was again a fascinating symbol, and we went there.

And then we wanted to go to a place north, and there was already something that came later, very apparent. They said, Don't go there, they are all thieves, they'll kill you. So we decided, let's see, and we hitchhiked and we walked, and then we went there and they said, Where do you come from? From there—What, you came from there? These are all murderers [laughs]! And so there was already, at that time, lots of antagonists between these various kind of groupings that later on in the Yugoslav war came to the surface. But we had always good connections—we were human beings and we related to other human beings in a human way, and so it was a great adventure. And, well, we came back and then I continued my work in an engineering consulting firm. And I was there for maybe one and a half years or so and I have to say I was a Young Turk. And as a Young Turk you don't have very much of a voice. I was very competent, I could calculate almost everything, stresses, speeds, whatever engineers need to calculate, though others couldn't. So I felt in many ways I was underutilized. That was one factor that I thought I couldn't—shouldn't stay there, but the work itself was not so unpleasant.

But then came a very important event that made me question that—this occupation. There was a firm that asked our firm to consult with a problem they had with the organization of the company. Our boss went to the board and three of our engineers went to the ground, to the workers. And we saw clearly the problem, but the board had other ideas and they paid for it. So whatever we found was discarded, and then I thought, That isn't an ethical kind of way of being, and to be involved with that, that was not for me—not in the long run. And I remember kind of, somewhat arrogantly, I wrote a letter of resignation in which I cited the unethical practices [laughs], etc., etc.

Well, but then the other thing was what to do, and there was a friend, a Wandervogel friend, who had just come—went to Ulm [Hochschule für Gestaltung, founded 1953] the first year and she described in glowing terms what they were doing there. And I visited the place and I was enthused about this. So I applied and I mentioned or you mentioned the application form, no? Besides having to tell you the little bit about the background, they [in the Ulm application] asked, What newspapers do you read? What kinds of art do you like? Make a comment on this and that art. And so there were a lot of questions that showed that they wanted to evaluate you as an individual, as someone who participates in society, what you would do, etc., etc. That

was very attractive or very unusual. And I was taken—admitted. But I had a feeling later on, I noticed, that I was taken for the wrong reason. 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, so it was really very young. And they wanted—actually one of the founders, Max Bill, was Swiss, and he was a very, one could say, advanced thinker, and he said design should help Germans reconstruct Germany from—I would say now ‘popular culture’—from the ground up. And he had the idea of what is a good form without, you know, symbolism and ornamentation, which of course the Nazis explored in numerous ways, and before that the Art Nouveau, etc. And he wanted to get rid of this all, so this was an effort to—his vision was to help Germany to rebuild itself and not get into the same type of trouble.

There was another important element and that was probably even more important—Max Bill was the kind of the intellectual or the architectural grandfather. There was Inge Aicher-Scholl. Inge Aicher-Scholl was the sister of two people, Hans and Sophie Scholl, who in 1943 formed a group, a resistance group, against the Nazis called the *Weiße Rose*, White Rose, and they wrote various pamphlets exposing the behavior of the Nazis regarding Jews, regarding the occupied countries—occupied areas—and they distributed that widely, often through the mail, and to people that they thought would be sympathetic to that. But of course mail is—first of all you don’t know all the people and there were lots of people that got that and they were really afraid that they would even get it and they went to the Gestapo and said, I got this, etc., etc. And they were caught in 1943 in Munich distributing these leaflets in the university. And they were executed in 1943. There is a famous film called *Sophie* of Sophie Scholl and so that was—and also I have to say Hans Scholl was in the *Wandervogel*. Actually he was in the branch of it called *Jungenschaft*, and he became, as many of them, he became a Hitler Youth after everything was taken over but he continued the culture. They were singing certain kinds of songs which were not Nazi songs, they were hitchhiking, doing daring things which the Nazis didn’t do. So he was suspect from the beginning, and so he was then executed and we thought always, he is one of our heroes. So the school was founded in the name of the executed brother and sister. So that was, I think, kind of my motivation to go there.

But there is a side story to that: When I came to Ulm in the first year we didn’t have a place to stay on the mountain or the hill where the Hochschule was. We had to stay downtown—and I was staying—renting a place with someone who started telling me, saying Inge [Aicher-]Scholl was the biggest Nazi in Ulm. She was the second highest—no, she was the highest next to two people for the BDM [Bund Deutscher Mädel], the Hitler Youth for women. There were two officers, one could say top officers, of Ulm and she was one of them. She was a devoted Nazi, and she was feared by everyone, and this woman [the co-renter] she was just—she was probably also one of the—like everyone else had to be—was drafted when they were 10 years old so—and she just reported that. And I was actually shocked, but I’m even more shocked now that I did not really act on it. I should have asked her [Inge Aicher-Scholl], What is the transition between being a high-ranking Hitler Youth for women and now against Nazism and forming the school in the name of the children? I found out later on that when this happened in 1943 the two, brother and sister, were arrested, she was totally shocked. She couldn’t—she was not in it, nobody told her because they knew she wouldn’t tolerate it. There was a German law called *Sippenhaft* that means when you are a relative you are responsible, and so now the father of

Sophie and Hans, and also Inge Aicher-Scholl, they were arrested and put in prison. But Inge Aicher-Scholl was a Nazi, so I searched later, and I was in contact with lots of people, one American—actually she was Jewish and she lived in Germany and she investigated that all, and there is no Gestapo protocol of her [Aicher-Scholl's] interview. Now, it could be that there was never any taken. It could be also that she managed to squash it. I don't know. But her father was actually a tax accountant and his clients were many Nazis. So he continued, while in prison, his business of tax accountant, so it was all very dark and very strange.

After the war this was convenient that she was arrested because she could now claim to be a victim of Nazis, and she played that role very well and I don't really know whether or how that—whether this was really an eye-opening experience or whether this was just an act, I cannot say. But she got the money from the United States—the High Command in Germany, Clay was the—whatever, I don't know what it was called—Justice [Lucius D.] Clay, and he was in charge of the American zone, and he gave a million dollars or something, and she founded that school with Max Bill's help and also Otl Aicher, her husband, etc., etc. So this was one of the motivations to go there but it was also a disappointment, but as I said it was more a disappointment that I never followed up with—I found that out later. But maybe it shows that we were not really that oriented towards the past. We wanted to create the future and that's the reason why I think I didn't follow up with that—only later.

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 built on a hill, and Max Bill designed it and it was a very interesting design. It was hugging the hill. There were several levels and there were places to live—not enough, they were not finished, but that's the reason why I went for one year outside. And then there were faculty tracts, and then there was a big—well, a meeting space. There was a famous bar that was kind of curved so as to allow conversation. And then you could go to the lecture rooms and to the workshops. That was the physical layout, and it was all in concrete grey, and the windows were a natural wood. Everything was either white or grey, and that was also what Ulm stands for in the end. Now, the first year, you asked, everyone had to take that *Grundlehre* [basic course], and it was actually an interesting idea. The idea was, when people come from all kinds of areas, from art and design, from engineering and architecture, whatever, they have all their crazy preconceptions, and the idea was actually to break this down and build something new. So the first year, obligatory for everyone, was a course where every three weeks there was a new project by one of the professors. They asked them to develop something, and there were people invited from all kinds of areas, for example we had one, [Helene] Nonné-Schmidt, I remember. She was actually from the Bauhaus, a former teacher, and she taught color theory. And we had to paint with watercolors, systems of where overlapping colors create different kinds of colors, and I have one of these examples still. So that was one kind of three weeks. Then there was a famous guy named [Hermann Von] Baravalle who was interested in geometry and different kinds of—how different kinds of shapes come out of other shapes. So we had to draw fine lines and then getting hyperbolas, etc., etc. That was not for everyone. I remember there was one guy, he was really an artist artist, he had a [unclear] and to him drawing straight lines was already a violation of his feelings [laughs]. And

when you do one—and one of them is either crooked or not straight, the whole diagram is ruined.

But there were also a lot of very advanced projects. Recently I was reminded—and that was not in my *Grundlehre* [basic course]—but there was one guy talking about typography, and he said we have to develop a typography, a script computer-readable. Now at that time there were not computers, but he had already said that it should be mechanically readable. And that was one project to do. So, there were just these very advanced ideas and all of them had to do with becoming sensitive to different forms, different processes, systems, etc., etc. But there was also other lecturers. For example, we had one guy, cultural anthropologist—a sociologist, [Erich] Franzen. And I didn't know, I learned it later, he was one of them, and there were many of them. He was one of them who escaped Nazism to the United States and had come back. And he was teaching now sociology, and I was almost enthused with that. There were many of these people. One of them went to Sweden during the Nazi period and came back, and they came all to Ulm because that was kind of a magnet of something new happening.

At the same time there were also lectures of cutting-edge people from all over the world. Before my time, the year before, Norbert Wiener, the inventor of cybernetics, was invited and gave lectures at Ulm. I don't know how that connects with design, but they had simply a knack of what is important and so we were actually exposed, much as forced, in the first year, to do all this kind of exercises, but also to be exposed to different ways of thinking and new cutting-edge theories that were cooked up in the United States and everywhere, and then the immigrants from outside, they brought their view of Nazism. So it was an amazing place.

But now there is something that you mentioned, actually, that is, you said Hungary. In 1956—I started in 1956, so in the *Grundlehre* this was now after the first quarter actually. The then-director [Tomás] Maldonado gave us an exercise and it required painting small squares in a very systematic fashion—mixing colors—and that was a very tedious type of thing. And in October or something of 1956 the Russians invaded Hungary, who had a revolt of sorts, and there were numerous refugees from Hungary going to Austria. And that was really a big problem. Austria couldn't cope with that, had never done anything like this. And so come Christmas, or December, and it turns out to be now exactly 60 years ago almost to the day, I decided—and my brother went there and several other Wandervogel, all of them, they went there, and I tried to convince the members of the *Grundlehre* we should go there and help out for the Christmas vacation. So, and I had almost convinced quite a number when the administration found out that that is what I was doing, and they said, No, no you can't do this. And so we were—many of them were dissuaded to do so, but four of us, we went down anyway, you know. And so that was December 1956.

We went to Austria, to the border and one—there's one American who had a Volkswagen and the others were Germans and so we went there. And it was a very strange place. First of all Austria at that time, I mean, and also in the countryside it's just unheard of. Like at four o'clock in the morning they go drink their schnapps, and then they take their horses and do their work. So we went to the border and tried to help, but this was very unsuccessful. We met an amazing number of very interesting people: There was a Swiss cook who said goodbye to his hotel, and

he cooked for the refugees; there were teachers, and there were—I mean lots of people. They were all fascinating people that helped the refugees that came over. There were two Americans; they manned a boat over a small river, and shipped these refugees over, but they were caught because they didn't know that the river on both sides belongs to Hungary. So, and because of that the Austrians said, We have to be careful getting foreigners too much involved in that. There was a Catholic organization and they—I forgot now the name—but they had cars and they helped people to come from the border into the city. But also the refugees, many of them, they didn't know the area, and this related also to my—refugees from [the] East, they didn't know when they were in fact in Austria and so one had to catch them virtually and say, You are safe! And that was not easy.

But we felt under-utilized. We spent there two days helping in a big hall cleaning tables, and then they said, Well, in order to get really meaningful—go to Vienna and there is one person, a princess—a former princess—of Austria, and she's in charge of that, go to her. So we went to Vienna, and this was again, you know—it was kind of naïve, one could say, but it's an odd situation, no doubt. There come these somewhat art-related students without [laughs] really great competencies willing to do something, and we tried to get to this princess. Now she was in a ball in a big hotel, there was a ball of sorts. And so we were outside and we said—wanted to talk to her. Of course she was not available [laughs]. But this is Austria, first of all; the nobility played still a big role. And so it was evening, late, and then where to go? We didn't have any money.

Now, from my hitchhiking through Sweden we went several times to prisons—they let you—let us sleep there. So I had the idea, Why don't we go to prison? So we went to a police station and said we want to stay overnight in prison. Ah, they had not heard about this but they couldn't really deny that, so there was lots of formality, telephone calling, formalities, filling out things. And there was one interesting thing: The American among us, he made fun of a picture that was hanging there. What was this picture? It was Kaiser Franz Joseph of Austria. Now, I'm amazed that he still played a role but he made the comment and the officer was really upset. How could you? But it was also the American who had no sense of nobility, he could do that. Anyway, so we were going into the prison and there were really greeted like prisoners where they emptied our pockets in boxes, we had to—there were numbers and put in the place, and the whole process took a long time. By I think one o'clock in the morning we were finally in the cell. In the cell we were guided from one gate to another, key opened and then the next one [unclear], and then we got a room, and then we slept but only a few hours because then comes the routine afterwards. Prisoners came to clean, and they said, What did you do, why are you here [laughs]? So, we had no good answers but we were really prisoners. Then we got something to eat, and that consisted of coffee and bread, just bread sliced—that's it. And then we were finally released at maybe ten o'clock or eight o'clock, very late. I mean, we had hardly slept, we were released and then we drove back to Ulm and painted squares. But the thing is I think what it tells me about myself is that I think I—for me kind of bigger issues like the world is burning, and we paint squares. I solved in the direction of helping the world not to burn, so I think—and also the relationship with my fellow students, I later on became actually a representative of the student body, but that's what I was already then. I wouldn't say I was a

leader but I was always being interested in new things. 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 everyone had to design something, you know, and I decided to design a steam motor for a car. And the traditional one also was with pistons and so on. I had the idea we should do that in a rotary fashion, and I in some sense invented the Wankel engine that came later. I'm not too sure whether my design worked but it was totally new, with wheels going in and cutting and the steam would just turn these wheels and turn the gears, and then turn also the wheels, and that was one of them, you know. And it was pretty innovative and everyone said this is innovative but whether it worked, I don't know whether it would work. But I used that—I submitted that also in Ulm in my application and it is now in the archives, so you can see it there [laughs]. 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, and that was the whole idea, to break down prejudices and open new visions. And I think they succeeded in many ways and people had to design things or do things that they'd never even dreamt of, like I had never designed characters in typography, I had never made these shapes and so. But there was one interesting thing that I also did with [Hermann von] Baravalle, I mentioned him. He had a model and that was two circular plates with lots of strings attached, and you could project with a projector a line in there, and this line, if it changes a little bit then it becomes an ellipse. So a line becomes an ellipse with a cylinder. And then you project other things in there, and amazing new shapes that you've never seen before—and I made one of them for me to show my parents or my family, and I made it together with another one, [an] architect. We made two of these models, his survived, mine disappeared somehow, but his survived and he recently showed this to a department of architecture in Germany and there was a film made of it. I mean—this all comes from Ulm, you know? And the main idea to me is to open up new ways of vision. Actually we had recently, in the Annenberg School [University of Pennsylvania], a discussion on the requirements of the first year, and I brought that up because everyone realized that people come from so many different areas, there is a place to challenge them or channel them to what we as faculty think is important. I don't think we can accomplish something like this, but there is the idea of doing that, and that was I think, very instrumental.

Then after the first year there were these various departments, there was product design, there was visual communication, there was architecture, and then there was what they called information. So then we could specialize. But let me say one of the important influences, maybe two, but most important I think was Max Bense. Max Bense was a philosopher from Stuttgart, he came because he developed a—one can say a theory of aesthetics, which was based on [Claude] Shannon's information theory. And the idea was actually—in retrospect I realize that it was not really a theory of aesthetics but, let's say, the cultural reception of art. And he said that, first of all, that art needs to be innovative, meaning new information—something that you're not accustomed to before, and if it is not then it is not art. That was the

definition of it. But then when it's innovative it attracts only a certain number of people, and later on after it becomes widely known, distributed, it loses the information. That is information theory. Now, he didn't really know very much about the information theory but he had a sense of the philosophy behind there. So he got us actually to think along these lines and that was very productive.

Max Bill, he was at that point already on the way out, mainly because—it was a revolt of the people that he had hired against his, one could say, a bit authoritarian role. Now, I think he was authoritarian. I also think he was—he knew his judgement was good, and he was right most of the time but he was a Swiss, he was direct, and he shot everyone down if they had done something wrong and that was not socially very good [laughs]. So he was thrown out, one can say. He gave a good seminar on the 20th century Bauhaus thinking, and I learned a lot from the different kinds of schools, different kinds of movements, and in Germany, the Bauhaus which was said to be—or they took as a predecessor of Ulm. It had also the flavor of an avant-garde institution, it was moved—it started in Dessau, not in Weimar, as an arts—in the time of an arts and craft movement. [Walter] Gropius, who later came to the United States, he realized that it's not really the kind of environment and he found a place in Dessau. Dessau was an industrial city, and there the Bauhaus became famous, and it attracted avant-garde people—[Wassily] Kandinsky and [Paul] Klee and whatever—painters. There was a movement of arts, of performance, and there was a lot of communist influences, but in a very strange way, not in a Soviet sense, but the idea was that one should make products available to everyone—mass production, celebrative of mass production but doing with good products. That was kind of the motivation of Bauhaus and it entered also Ulm. Then later on the Bauhaus moved to Berlin and then it was completely eradicated and people went to Chicago, etc., etc. But I learned a lot about the history of these kind of ideas, of design, of architecture, etc.

So, Max Bill was one and the other was Max Bense, and Max Bense was I think—he was the intellectual, and he gave Ulm, I think, the intellectual status that others lacked because most of them didn't have academic degrees. When he—he stayed only one year after Bill left, and then he left. And then there was—because information was kind of the term that mobilized the whole department of information, as well as also our thinking, they said we have to have a replacement and they found an information theoretician, and that was Horst Rittel. And I remember the very first lecture, he didn't have a clue of what designers are like, he didn't have a clue of what the school was like, and he presented information theory starting with what is a probability and how one can account for that and he gave that. And people just—that was just too much. But Rittel was an amazing character, he was very adaptable, he saw very quickly what is design and what he could contribute, and he became, one could say, a theoretician of design, which he maintained for the rest of his life, and he pumped into our thinking so many new ideas—one of which is cybernetics—the idea of feedback; planning, the idea of making plans and realizing them; what it means to plan; competing plans; systems theory; thinking that you know when you design a gasoline station, you can't just design a gasoline station. You have to also see where all the other gasoline stations are—looking at systems not as a single product but a system of products—and so he was I think the intellectual engine that was critical to me. And I turned around these ideas in many ways.

There were of course others also. There were several Fulbright professors coming from the United States. One was actually from Ohio University on ergonomics. He could barely speak German, but he did 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. Then the other one was Joe [Mervyn W.] Perrine. He had just graduated from Princeton University with a social psychology degree and he introduced the notion of social perception, which to me was very critical and, I mean, the examples from the studies were kind of minimal but, for example, I remember he talked always about a dollar note—a dollar coin is far bigger for a poor person than for a rich person. So that perception is tainted by your background and—even though he didn't do very much about design, but that's what I did later. But for me I think this was eye-opening, that even the notion of perception is determined by the social background of people. And you know that's artists and much of the design—they thought perception is simply registering what is there and all you have to do is present the form, and that shifted my whole perspective—that it is not the form that you make, rather than the perception of it, [that] is important. There was in Germany the word “Gestalt”. Gestalt is also in the United States, Gestalt psychology, and Gestalt perception is very critical in matter of design. For example, the circle—when you make a circle and you make it not complete, it's still a circle, so that you complete this in order to have very simple forms. That's another part of perception issues, but what Perrine added is the social dimension, not just the geometric part. And so that was very important for me.

There were trios of the departmental work and well—I did a lot of products or things and I don't want to go into that, but I became also a representative of the product design department, and you mentioned earlier about the pipe. I was a representative and we had lots of struggles mainly because when [Max] Bill left, he left a schism between people that came actually to study with him and were disappointed, many Swiss, and others who were on the side of the administration and were opposing him. There were the Billists and the non-Billists. I didn't come for Bill, but I felt also that it is a bit unfortunate to weed out someone even though he was, as I said, a bit authoritarian. But he was still a very good teacher and mentor for lots of people, and he was very unconventional in many ways. So there were these discussion with the administration and I was a representative and someone said from my [fellow] students, Klaus, you act too fast, you have to take this pipe and light it and you answer only after you take a puff. And that's what I did, and I would say we won [laughs].

Now, whether we won because of that, is the question, but I continued to smoke for social reasons, just leisurely and without inhaling, up to 1972 or something like this and then I gave up. But I noticed also that the same type of strategy was given to Bertolt Brecht. Bertolt Brecht was one of my favorite poets and writers. He was a communist, he emigrated to the United States, and he was at some point cited in front of the House of Un-American Activity [House Un-American Activities Committee]. And there he was given the same advice. He was smoking a cigar, and they said, Don't answer until you have smoked a bit because that would give him time to think. And I have actually a record of this hearing and there was one question—there was, I think, a guy named [Roy] Cohn who was a lawyer for [Joseph] McCarthy. He read a poem to Brecht and it had something to do with the proletariat and whatever. It was clearly communist of sorts—inspired—but he was not really a communist communist. But he was—whatever. And there was no answer and it was said, Did you write this poem? No answer

on the record. And then he said, No, I wrote a German poem. So—he took advantage of a cigar, I took advantage of a pipe. That’s one of the things.

Then I would say—well, as I said, I did a lot of minor innovations. I was actually crossing many departments. I took a lot of photography and became a very good photographer, and I went to the visual communication thing and I used typesetting. I did that and I participated in the information department in lectures over there. I didn’t do very much in architecture but I was kind of overall, but my house was of course there.

Then came the issue of graduation and I had to do, the last year, two things. One is a practical work and the other one is a theoretical work. In the practical work I thought one should do something, not just another household gimmick or gadget. So I decided to do something that had also something to do with engineering and I designed a motor grader—one of those that does work on the street. And I went to a company not so far and got their drawings and talked to them and I did this. That was not a big show—that was not a big problem. I could do that.

But for the theoretical part I had during the time developed this interest in perception—and I was a little bit annoyed, one could say, because I was accepted as an engineer and several of the faculty wanted to become more engineering-oriented or scientific-oriented and so they thought—they had hoped that I would be someone who would contribute. But I had gone through the engineering practices and the idea, this determinism that engineers thrive on—this is precisely what I wanted to go away from when I went to Ulm. I wanted to have a more human-oriented—the complexity of humans are far more interesting than the determinism of engineering, designing bridges. So I was just opposed to this this kind of functionalism, and I wrote my theoretical work by saying that we should look at objects in terms of the signals or the signs they have in order to use it. So the object should not be the object, rather than the use. And that fueled, actually, much of my later work. It’s surprising me.

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—one of the [unclear] also talked about communication—information, communication. And I framed the whole thing as communication. I had a bit of a limited idea, namely that objects should communicate something—and then I said, Well, designers should use objects to communicate with something and that fueled it. But now that wasn’t the interesting development. The director of Ulm was [Tomás] Maldonado and he was also a cutting-edge painter, actually from Argentina. And he introduced the idea of semiotics into Ulm. But, anyway, that didn’t gel with me. But since I was interested in this kind of thing, I went to him and said, That’s what I wanted to do. And he said, Klaus, this is a categorical mistake. Objects are referents. Signs are what refers to the real world. And it was this traditional semiotic trajectory that there is a world of signs and a world of referents and you don’t mix them. So I realized I couldn’t work with him and I worked with [Horst] Rittel. And so that was, I think—that determined also my later trajectory. I think I have always been against this simplistic worldview of: Here are the symbols—knowledge, if you want—signs and there is the reality. That is, to me, connected by perception, by understanding, by lots of things. So I

wrote this and it was long. It was German. It was my first writing ever, so I don't really recommend it to anyone, even though it is now on the [University of Pennsylvania] Scholarly Commons and I get occasionally notices that someone read it, and I wouldn't say I'm embarrassed but I think I'm embarrassed now.¹ But at that time that was me [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 memoir² 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: It's not a controversy but the Höhle—that means a dungeon, no? That was a left over room underneath, through kind of a stairways, that was used sometimes as a storage place, but it was also used as a place to have parties. I have to also say something about the Ulm—you mentioned the climate and so on. Well, there was the city of Ulm, very traditional. There were some very avant-garde galleries that sympathized with Max Bill, with the Hochschule, and then there was the mountain Kuhberg and there was the Ulmer. So there was a split of sorts. But there was Carnival [festival] and, each year, the students in Ulm invited everyone—of course, girlfriends and so [on] from Ulm—to come there and celebrate Carnival. At some point that went almost out of control. For a month before that, the whole school was transformed into—you name it.

I remember one year someone had the idea of using [egg cartons]. So we got thousands of them, put them all in windows and everything and painted it. And so this was a big project. Everyone still talks about the fashion at Ulm. Many of the girls downtown, also men, they came there and had fun. And the Höhle was one of the things that one could explore for as a bar or something. And it was dark and dingy and we had, of course, red light or something red light Höhle. So that was it. But when Max Bill left—the last day before he left—there was a party in the Höhle and I was there. And I have to say, Max Bill was a big guy. But we had also one of the big students, who was also a big guy. He and another one from Holland whom I stayed in touch with, and I'm still in touch with—they decided to take Max Bill on their shoulders and show him—make a tour of his building that he had designed. So this was just fun, if you want. And then Max Bill took a piece of chalk out of his pocket and—over the Höhle, there was a grey cement wall and he signed “Max Bill.” Well, I mean, this was it. And then, the next day, I thought—I talked to other people and said, This we should fix. So we went there with a chisel and hammer and a ladder and we chiseled that signature and it's still there. So that was the story about this.

But the student body, as I said, it was very divided because of Max Bill and later on because of Rittel not being a designer—being more like an intellectual. And he was kind of given a second-

¹ 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/

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

rate role in the school. He was at some point elected in the directorium, but later on they made a rule that only designers, professional designers, could be a part of it. So there was a lot of struggle and tension between the practitioners and the theoreticians, one could say. And there was another element that I always resented. Namely, the so-called practitioners, they had their studios and they could take jobs from industry and got money. They had, of course, to pay something to the school, but they were more interested in industrial jobs than in their students. And that was really offensive to me and there was one guy, [Hans] Gugelot in particular. He made major innovations and Brown Instruments—he made so many new things that the chairman, really—the face of the Hochschule as well as of modern design in the world—he was an amazing character. But he was not that open to—not interested in students. He was technically my advisor for my practical diploma work. I never met him. I mean, I showed him the model and that was it. So, I mean, he was not interested.

So the schism was not just theory versus practice, but also concern for people. And the so-called theoreticians—Rittel, etc.—they were of course teaching and they were really working with students and they were the ones that learned. The other ones stayed stable. So that was really the unfortunate tension that determined it. But still, I think the new ideas that came into Ulm through external lectures, through Horst Rittel, Max Bense, and lots of other people—that was truly amazing. And I actually, I had—I don't know if we should deviate from that. But I was recently in Basel, in a reunion of the former graduates. And the famous Hochschule für Gestaltung und Kunst in Basel invited us to be there and they wanted to show their—whatever. And I was so disappointed. They were so focused on shapes and colorful forms and gadgets and not on that what really mobilized me, what mobilized most others, namely the ideas. And I thought, Why don't, instead of making more exhibitions of the past, why don't we do something for the future? And I'd still like to do that: introduce the idea of a different kind of education system where you assist [unclear] of new ideas that haven't—may not be meaningful, but at least it gets you thinking in a different track. And that was, for me, Ulm in the end. And with these ideas, I could go almost anywhere and communication was one of them.

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 was Joe [Mervyn W.] Perrine, the American, and actually he was hired as a Fulbright Scholar to install in Ulm a system of demonstrations that were developed in Princeton [University] for perception. I don't know if you have ever heard about this? For example, there was a distorted room and when you take a picture, it looks like exactly a rectangular room. But if you walk in it from one side to the other, you become huge or very small. So this was [to] demonstrate the effects of perspective, and lots of things, on human perception. Now, a lot of other demonstrations—and they were installed there. He did this with the help of the workshops that were available in Ulm, and it was amazing. And then we developed or he developed this Institute [Research Center] for Visual Perception and we got

actually a big contract from Fraunhofer Society, which was actually connected with the military on issues of camouflage. And so we studied contrasts and so, for about a year, we did that. But we were the only kind of research institution in it [Ulm], and that created, as you suggested, tension between the practitioners and, well, researchers—whatever.

There was one guy, actually, he was very instrumental—he [Otl Aicher] was the husband of Inge Aicher-Scholl. He was a graphic artist and he was a very good artist. He changed the face of advertising in Ulm. In Ulm there were small columns this wide [gestures] for the latest information and he made the posters. He was an amazing character, and color was his metier. So he said, whatever colors, I'm the expert. Whatever you find is meaningless. Now with this attitude you can't survive very much because he was kind of in. And I suggested, I said, Well, you are a good artist but ordinary people—we study ordinary people in research and that is a different kind of thing and you could probably learn from that. But he didn't. So there was a lot of opposition to that and then I went to the United States and soon—and then there was, one could say, an administrative problem or—no, an administrative way of getting rid of it [the Research Center]. Namely, that asks the institute, the research institute, to pay a hundred percent of all the income to the Hochschule [Ulm], whereas the practitioner had only a small percentage. And that killed, in fact, the possibility of getting contracts, even though we were probably very cheap by comparison. But it faded away shortly after I went to the United States.

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: Well, as you said, we had the summer. There was the quarter system and the summer quarter was available to us. And, the first, I went back to my old engineering company, because I could make money. I had to support myself. And then I worked for another company. That was in Ulm. And I thought it would be good to have a different kind of experiences and that was very helpful. But then I decided I need to learn English because I had—my education was rather minimal. As I said, with 14 years I went out of school and became a practitioner, a craftsman. So my English was miserable. And I went to Oxford and, I mean, I knew a little bit but this was totally inadequate. And there I went to get a job at the refrigerator company, which, incidentally, was next to the production facility of the Mini, the British Mini, which is much smaller than the ones now produced. So I went to Oxford—the assembly line, I saw that. But this was more like a cultural experience. The work itself—actually I designed—and this was maybe the beginning also of my interest in communication. One of the things is—I saw problems with adjusting the temperature of refrigerators, so I designed a way to indicate whether cold or warm, etc., etc. And that's what I did. And whether it was produced, I don't know.

But I met there—that was interesting to me—I met there someone who was in the Bauhaus. And he was of course interested in finding out all about Ulm, and so we developed a very good relationship and he became kind of a mentor of mine. But I wanted to know more about the Bauhaus and he couldn't say very much. And I realized that, even with a good education, an

avant-garde education, you can become an ordinary draftsman, which he was. And I didn't want to do that [laughs], you know. But I lived actually in a, one could say, in a house that housed many workers—an Irish family—and that's what my English came from. The conversations with factory workers, which was not very good.

Oxford was of course a university town. And I roamed that town and I went among others to Blackwell, which is the famous publisher and original bookstore in Oxford. And I picked up two books. One is by Ross Ashby on cybernetics, which I heard of from Horst Rittel, and that was called the *[An] Introduction to Cybernetics*, published in 1956. And it was complicated because, as I said, my English wasn't that great. And the other one was [Ludwig] Wittgenstein's *Tractatus [Logico-Philosophicus]* [1921]. That was more easy because it was both in German and in English. That was really very instrumental. And I worked through that and that helped me a lot, thinking about it. And later on it turned out that both of these books were absolutely instrumental in my later preoccupation with communication. With Ashby because, when I went to the University of Illinois, or maybe I should—should I talk about that?

Q: You could mention it quickly—

Krippendorff: Well, I found him at the University of Illinois and I became a student of his. And later on I became—we organized an Ashby Club to digest all the ideas among the students, ourselves. So he became an important figure in my life. And then Wittgenstein of course, with his theory of language, that was also, to me, I think instrumental, even though, I mean—there was a later Wittgenstein that was far more important in the end, but he was the one who started questioning the issue of language, reality, etc., etc. So both of these books were instrumental. But then I went back to Ulm and then I wrote my work and then stayed for one more year, and during that time, actually, I applied—before I went to England—I applied for a Fulbright travel grant. And that was with the American embassy [Consulate] in Stuttgart. And I was interviewed, actually, much later and they agreed to finance it. So I got actually a fellowship that was totally new, [the] Ford [Foundation] International Fellowship. And the Ford Company wanted to support studies in the United States and I got that. And then the travel grant from Fulbright, and with that I came to the United States. But as I said, you know, earlier on, to me that was not such a big step because, as I said, I was so primed by my parents to come to the United States and see the adventures. And I wanted to see, actually, many of the adventures and many of them I didn't. So I had different adventures. But it was to me a very important target. Yeah, I have to also say, my brother, who became a political scientist, he got, the year before me, also a Fulbright grant to come to the United States. And he was here already and picked me up at the ship. So that's also the influence, I guess, of my parents.

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

Philadelphia, PA

Interviewed by Jefferson Pooley

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—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 during the Nazi period they were outside, they came back to Germany, and they taught there. And so it was an environment in which just amazing ideas were populating the students.

And so that's where I think I would say I grew up intellectually. I was before an engineer—but this is what opened up my worldview. Most of these ideas, they came from the United States, so I decided that I had to dig deeper in them, and so that was actually my motivation. I didn't come to get a degree. I wanted just to expand what I had gotten there, like the kernels, and, you know, all of these ideas that I mentioned—that's what came, I would say, second-hand, and the first-hand people—they were all in the United States. So I applied, actually, at some point to the American Embassy [*sic*: Consulate] in Stuttgart for a fellowship, and I got, without knowing what it means, but a Ford [Foundation] International Fellowship, which was a new fellowship that was not very familiar, and I got a Fulbright Travel Grant, and then I came to the United States—by ship.

The ship was called the Berlin and it was actually the first ship that, after the war, was acquired by a famous German shipping company, and this was actually a Swedish ship from the Swedish king, and was called the Gripsholm. It was an old-fashioned ship and we were just lots of students. And I have to say, also, it was very different. Nowadays, nobody would take a ship, but this was the cheapest way to get students to the United States. So we had seven days on the sea and there were many, many students—not that I bonded with many, but there was one, particularly, Hans Haacke, who was an artist. And he showed us some interesting works that he did, and it kind of gelled with Ulm, with where I came from. And I stayed a little bit in touch with him and he came to Philadelphia actually, to Temple [University]—the art school—and I came to Princeton [University].

And I have to say, also, why I came to Princeton. I was a misfit: There was an Institute for International Education in New York, and they got all the applications, fellowships, etc., etc., and they found universities. But I wasn't really interested in a university. I was interested in [unclear] ideas, and there was no university in the United States that were interested in these ideas. And I had a recommendation by one of the American professors with whom I worked in this visual communication laboratory [at Ulm: Mervyn W. "Joe" Perrine], and he wrote a glowing recommendation. And he had just graduated from Princeton as a social psychologist, and actually his influence is also very important. And I mentioned last time, I believe, in social perception—the idea that we don't see things the way they are, rather than with our own background, and we distort, if you want, or see it in our own way.

So he wrote this glowing recommendation. And I don't blame this Institute [for] International Education to put me into Princeton because that was a natural decision. But in Princeton—first of all, my English was very, very bad, and I remember I took notes in German from the English lectures, which I soon abandoned because that is not the way to learn English or to get into the groove. I made a good decision not to house with other Germans or other foreigners, but I lived with American students and they were the ones who introduced me to shopping, to television, to whatever.

But Princeton was a psychology department and they were rat psychologists. I learned mathematical theory of rat learning and a guy named [Harold] Gulliksen was famous—I mean, I could learn that, but it wasn't of any interest to me. I was seriously unhappy. There was one professor, [Harold M.] Schroder, who was a social psychologist, and I had a good connection with him, but that was only one. And that was just not enough.

There was a famous [psychologist]—you know him probably, Hadley Cantril. He was famous for doing studies of how the United States went into war, and public opinion, etc., etc. And he was not there in Princeton. He was at some point the chair of the department when it was not rat psychology, and he came back from Turkey somewhere in December. And I made an appointment to [see] him in a private house and so, and he said, Klaus, you're in the wrong place. And I knew that of course. And then he gave me several names and said, Just find another place. And the names were, for example, George Miller from MIT, Jerome Bruner at Harvard, Anatol Rapoport in Michigan University [University of Michigan], and also actually George Gerbner.

So, in December, after these classes were over, I took my Volkswagen and I drove through the East Coast [to] find a place. And I talked to [George] Miller and it was good conversations, but it

didn't really look like that I would find that much resonance. He was very interested in my design background and we gelled in a way, but it was not enough.

Jerome Bruner was fascinating, but I didn't really have a place for him. I went to Michigan, but before I went to Michigan—no, no, after I went to Michigan, I wanted to see Anatol Rapoport and he was not there. I talked to an assistant of his and he basically discouraged me from going there because the environment—he himself was fabulous but the environment was, he thought, not so good. But there was in Michigan State [University], there was a guy named Hans Toch, who was a psychologist, and two graduates from Ulm had at some later point come to the United States and got a PhD with him. And although, I think, one of them, the PhD dissertation was, in my opinion, unimaginative and uninteresting but, nevertheless, I decided to talk to him and he said, From what you're telling me you should go to the communication department.

And so I went there. I was invited by David Berlo. And I didn't know really what his status is and so—this was something that we would now not be able to do anymore: He organized a party. He organized a party with Malcom MacLean—some other faculty members and assistants, maybe eight, ten people—for me. But I didn't know it was for me, but basically they wanted to interview me, and they interviewed me about communication. Now my conception of communication, again coming from a design school, was rather minimal. But I had certain ideas, and they were impressed, for whatever reason, and at the end of this nice social gathering they said, You have an assistantship. But I said, You know, I don't want to be a psychologist. I want to see it from the sociological point of view, or see the social dimension. And then he [Berlo] said, You have to go to Urbana [University of Illinois at Urbana-Champaign].

And I had the name of [George] Gerbner and I went to Urbana. But I didn't go to Gerbner first. I went actually to the design department, and the design department—there was a famous designer, chair, and we talked, and he was very impressed. He had never seen anyone coming from Ulm, and Ulm was at that time world-famous already. And he pulled out of his documents a paper by a guy named Heinz von Foerster. And Heinz von Foerster wrote a book on the construction of reality, and he had an image of an artist's construction. That was actually what the designer was interested in, and this other one was kind of a sideline. And I didn't really know Heinz von Foerster, but I said, I have to talk to him. He says [unclear].

And I went to two people—saw two people first. One was Heinz von Foerster and he told me that Ross Ashby was teaching a course in cybernetics. And I think I mentioned last time when I was in England, I bought two books not knowing that much about how influential they might be. One was [Ludwig] Wittgenstein's *Tractatus [Logico-Philosophicus]*, the other one was *[An] Introduction to Cybernetics* by Ashby. When I heard that Ashby is teaching, that was certainly a big influence. And then I talked to Dallas Smythe. Dallas Smythe was actually kind of the chair of the department and he was very impressive. I remember, when I was sitting, he put his legs on the table and we had a great conversation. And then I met also, briefly, Gerbner, but he was first not there. But that was only very briefly.

Anyway, then I decided to go there. So that was in December and I went back to Princeton, packed up my things and went to Urbana. So that was in January 1962, I started there. But that was also interesting that—who I was. When I came there I had to register in some form. The Ulm school was not on the register of the administration—they didn't know what that was. So, I mean, I came from Princeton and they said, Well, why did you come from Princeton to Urbana?

And they thought I'm a total fake. So they categorize me as an undergraduate, and I said, No, I'm in—and I talked to, in this case, Gerbner, and he said, Don't worry, we will fix that. So, after a semester I was a graduate student. During that time I got all the credentials of an undergraduate: Invitation to fraternities and so—I had no use for that—I didn't care for it. And so, anyway, I became a graduate student—of the Institute for Communications Research [ICR]. So I think that was a very good decision because one of the most attractive features was that the Institute for Communications Research was truly interdisciplinary. There was nobody who had a communication degree. Actually, [Percy] Tannenbaum was one of the earliest graduates from there but nobody—everyone came from other disciplines. So I had actually considerable choice. The committee that organized it [was] made of people from sociology, linguistics, anthropology, economics, you name it. And Dallas Smythe was—I don't know, I think he was an economist, I cannot say. But the point was, actually, he was obsessed with the FCC [Federal Communications Commission]—the regulation—that was his shtik, so to speak. But as I said, I had a very good relationship with him. And so I saw the opportunity to take courses from everywhere.

My advisor became, actually, Howard Maclay. Howard Maclay was a linguist, anthropological linguist, and he was also very broad. And when I said I'd like to take a course with Ashby—and I had to wait for half a year because it was a one-year course and it started always in fall—he was very supportive of that. And in fact he encouraged everyone else after I was there to do the same thing because he saw too that cybernetics is basically an approach to communication. So there is a proseminar that one takes, and a statistics course and the proseminar goes over one year—one [semester] is more quantitative and one is more qualitative, and I enjoyed that all. I tried to wean myself out of statistics because I had some minor amount of statistical background from Germany from the design school, but I realized that it is not enough to match the teaching. And I took then the examination—I didn't take the course, I took the examination and I passed and that was it.

So Urbana was, as I said, very free: I took courses with Ashby, as I mentioned, and he was probably the most influential teacher I had. That was based on, well, my superficial knowledge of information theory. He did it more systematically. He wrote about it himself and asked us to read, to make contributions—the notion of feedback, the notion of systems—that came all from Ashby.

The other area that I was fascinated by was, actually, anthropology, and I took a course, several courses, two courses, with [Joseph B.] Casagrande. He was a Whorfian, and Benjamin Whorf had still a major influence, even now, to deal with the issue of culture: that language is not just something that you study by itself. It has no rules that are specifically linguistics—linguistic rules—rather than it has to do with culture. There was one interesting experience: He gave us an assignment to look at kinship terms in different cultures, and so everyone got a different culture and then literature, and then we had to search, What are the kinship terms? And as you probably know India had so many more elaborate kinship terms and, I mean, almost every culture has their own ways of categorizing relatives. And I got—I've forgotten exactly which kind of culture, I could have checked that out, which kind of culture—and it was very German, and then I realized the anthropologist was a German. So that made me aware again of the influence of where someone comes from, what you see, coming back to social perception. This guy had no openness to the differences of various cultures.

Anyway, so this was minor insights. I took courses also with Jerry Fodor, and I didn't really like it. But this was a hard-nosed Chomskyan linguist. I learned a lot from that area. And then social psychology: I took a course, but that was not really very much eye-opening. I took a course with Gerbner and, frankly, I didn't like it much, because he was kind of, from my perception at that time, a very narrowly Marxist criticizing. His phrase was always that, you know, Mass media is the product of mass production industries. That's all true but he basically—his criticism was the mass media industry, the economy and so on. And he was right in many ways, but this was too one-sided.

Herbert Schiller was there at the same time. But I didn't take a course with him because he was even more of a Marxist. And I mentioned to you earlier, you know—when I was a kid in East Germany and I experienced the results of Marxism. Later on, when I was in West Germany, we looked at Marxism, studied Marxism, and I knew something about it, and I knew also the operations and the irrelevance of it. And I would not say it now anymore, because I think there are a lot of good ideas in Marxism, like going to the ground and looking what happens to people—I would look at this from the point of ethnography, which I learned from the anthropological courses. But I didn't like the ideological component. At the same time I had a very good relationship with Herbert Schiller and so on. In fact, when I got married in Urbana, Herbert Schiller was there, Howard Maclay was there, and lots of other people were there at the wedding. So I was part of that group.

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's start with Ashby. Ashby was British. He had never taught, really, but he was a psychologist—psychiatrist, actually, originally. And his main emphasis was actually understanding the brain. But he went far beyond that and he developed—one of the things is he wrote his book in 1956 and he told us—which was carrying off of his whole career—he said, Norbert Wiener is too complicated. He uses immediately three to four summation science, integral science. Nobody understands what that means. And in order for cybernetics to be meaningful to lots of people, one has to boil it down to something that is understandable. And that's what he did. He used not integral mathematics, but set theory. And he looked at just what kind of transformations are possible, and he was very abstract in that way. But his main interest was really understanding the brain. However, this understanding translated into social phenomena and that is what I was interested in and that is what I took away from it.

Now, one of the things was of course information theory—that was, to me, also influential and later on I taught in fact courses and wrote much on information theory, but not from a technical point of view—rather than from a point of view of mathematics and how that translates into human communication. So information theory was one thing, and for the first time I got really a thorough education as opposed to the glimpses that I got from Ulm. The other one is the notion of systems, that things are not just entities by themselves but they are systems connected, causally connected. And he [Ashby] developed some sort of a mathematics

that allowed us to see these kinds of connections. And it was a one-year course, and he used his *[An] Introduction to Cybernetics*, but then expanded it as he learned more and more.

He built some models of, for example, ultra-stable systems—which is a term that has not really survived but it has a very important, to me, very important consequence. Namely, he said, When you have systems that adapt, if you couple them, then the adaptation of one system has an effect on how the others adapt. So he developed actually machinery and he started running it, and it had greatest time to come even to stability. He was always looking to what brings it to stability. The point is, actually, that when you have systems that adapt, that communicate with other systems that adapt, there is constant change—that is communication! So I think I got a lot out of these kind of concepts and, as I was mentioning, when I introduced them back into the proseminar, and talked about this, many people were enthused about this. And Howard Maclay encouraged many people to go to Ashby. And we formed—after the course was over I formed an Ashby or a cybernetic club in which we just continued developing the ideas or at least talking about them. Until I left Urbana, that was alive and it was a very exciting thing. So I think Ashby was a very crucial influence.

Maybe I should also mention Charles Osgood. I didn't take a course with him. He was a psycholinguist and he had, in my opinion, a very narrow kind of theory that human beings encode messages from outside, inside, and there are two processes, one is encoding and decoding, and inside there is cognition. And so that was kind of his model and he developed actually numerous ideas, one of which is his semantic differential. I'm not so sure really how that relates to his model but he put it into that model. And that was kind of a psychological theory which had social implications—allowed us to measure attitudes. And he's well-known, as I mentioned earlier, for being a very, very creative lecturer, and every lecture outlined several dissertation topics and people that wanted to write a dissertation, they circled around him. But it was all psychological and I was really not interested in the psychological aspects of it. What else I wanted to say about that? Well you said about the tension: I don't think that was so dominant—that say, Marxism vs. behavioralism. It was more psychology versus sociology. Dallas Smythe, he was obsessed, actually, with the FCC regulations, and actually I never took a course with him because of that. I just thought it was too narrow. He was very dominant in the proseminar and I learned a lot from him.

Actually, I had a very good connection with him. When I was—in '62, I had a chance for my fellowship to take a summer course somewhere. And I was in Urbana. Urbana is a very flat place in the middle of nowhere. And so I said, I have to see something different and I went to USC [University of Southern California]. And there I took a course in collective behavior. And that was very eye-opening because that was something Urbana didn't offer. And there I visited also Dallas Smythe, who came, actually, from Los Angeles and he was always there in the summer. So I had always good relationships with him and with Howard Maclay, and most of them.

But I was really more interested in the social dimension. I met also [Bennett] Berger—he was in sociology teaching at the time. But for whatever reason his course that he was giving—I was interested in it, but it conflicted with other things and I couldn't take it. I should have, probably, because later on I use his book now in my classes, and it would have been—probably made a difference.

But I want to say something about my dissertation. At some point when I had the preliminary examination. Actually, it's not so like in the Annenberg School [for Communication, at the University of Pennsylvania] where you have to have a dissertation topic ready-made. That's where you start thinking about the dissertation, afterwards. And I remember that I had three proposals and I forgot one. But the two important ones were, one was—and I wanted to look at how communication structures can be undermined by authoritative structures, by systems, etc., etc. And the example was how the Spanish went into Mexico—Incas—and used the Inca streets that go right into the center and occupied the center—of course then with welcome being the gods that would help the country. But I decided then it's probably too complicated mainly because I didn't know Spanish. I would have had to go to Mexico, maybe, and look at lots of Spanish documents. And also it was not so clear what I would find. And the ideas I had—I mean, I could verify them but that would not be surprising in the end.

And then I decided to take a topic named content analysis. How I was introduced into the issue was actually through two people. One was Shel Feldman, who later on came actually to the Annenberg School. He was a psychologist and—when I was an assistant in the Institute for Communications Research, I was available to everyone who wanted to have someone to help, and I was coding things. And I thought that was kind of, well, not satisfactory for me. And I saw all kinds of problems and they said, Don't worry, just code, just take [unclear]. And I was really thinking, What are we describing there? And then I was with Gerbner, and Gerbner was also involved with coding magazines. And so I did some work with him. But I thought that was all not very developed.

And there was [Bernard] Berelson [who] had already published his book in 1952 and so that was kind of the only textbook there was, and it was not very satisfactory.³ And so I decided to write about content analysis. And what I actually sometimes tell my students nowadays—I had a committee: Ashby was one of them; the anthropologist was one of them; the social psychologist was one of them; and Howard Maclay. They were all very different and actually none really had any knowledge or interest in coding or in content analysis. Maybe the social psychologist. He was doing that too. I wrote, actually, several chapters each for a different advisor. For Ashby, I developed actually an information theory for content analysis and that still informs me in many ways. I said to myself, If one wants to understand certain phenomena—social phenomena—then one has to use the information that is in this phenomena in some form into the nature of the data, and that is an information issue. And so one needs to see what is it—what information one loses when one codes or what information is, let's say, indirectly imported through the nature of the coding instructions. And so these are the kind of things that are problematic in content analysis.

Now for Ashby I wrote simply a separate—one can say qualitative information theory, one that is not based on probabilities but on issues of coding. And then I had also an anthropological kind of version of the issue of different kinds of readers. And I made them all—at that point already a kind of a decision that formed much of my later writing—that content is a wrong metaphor. Messages have—contain nothing. When you copy a paper you copy the character strings and not the content. And so that became the anthropological notion, the ethnography, that one has to understand things how they are read, how they are interpreted, and not what

³ Bernard Berelson, *Content Analysis in Communication Research* (Glencoe, IL: Free Press, 1952).

they in quote[s] are physically. So that informed me, actually—that was the anthropological or ethnographical moment in content analysis and I continued that.

I should like to just mention in passing: Of course I wrote the book on content analysis,⁴ in fact three versions of it. And it's always content analysis. But from the beginning, I mentioned precisely my attitude. It was the publisher who said, You want to have a book on content analysis, not on text analysis or anything else. So I yielded with that, but I'm still insisting with my students [that] they have to be careful in not using this kind of metaphor of content. In fact I'm writing now a piece on issues, or against content. And there is such a metaphor which is destructive, and it has the consequence, actually—and that goes back to Berelson and [Paul] Lazarsfeld.

Actually I found out also much later, I found out that Berelson's book in 1952 was actually written by Lazarsfeld and Berelson in 1948. And then they had a fall-out and they divided—they divorced—they divided the intellectual ownership and it became a Berelson book. But I have the original and it's virtually the same. But at that time content analysis was simply the analysis of content. There was no definition of what content was. There was—content is just there, and you have to observe it objectively without readers, without anything. So this was the kind of thing that I was against and my dissertation developed that in my own terms.

So I should also like to say, you know, with this kind of committee I felt, actually, unlike, I think, what I hear now many Annenberg students [say]. I felt very comfortable when I had to defend it because I knew more than anyone else. And so the difference was good. And there were no problems and so that's—became the staple that I started at Annenberg.

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, I didn't mention one thing that, when I went to Princeton, then Urbana, I didn't have any intention to get a PhD, but in Urbana they said, Well, if you study here you have to work towards a PhD. And I said, OK. So I really—my mission was not to get a degree. My mission was really focused on the ideas that I thought were very productive. And, frankly, I had the idea of going back to Germany and maybe being a designer or design teacher or something. That was kind of my idea but it was blown by the demand that I have to get a PhD.

Now you asked me about how I came to the Annenberg School. I was, actually, I have to say also—I had this one semester in Princeton. I spent only two-and-a-half semesters in Urbana, and as soon as I had my dissertation, my preliminary defense done, there was—the Annenberg School looked for a dean, and they turned to Osgood. And Osgood was interviewing at the University of Pennsylvania and he didn't like it. And he came back to Urbana and said, I don't want to do that. And Gerbner wanted to do it. So Gerbner went down and was interviewing. But independent of that, it was Shel Feldman—who was a young assistant professor at the [Illinois] Institute [ICR]—he wanted to go to the University of Pennsylvania and he said, Klaus, why don't you come with me? So—and actually I had an interview with Shel Feldman—and we drove in the same car, actually, to Philadelphia—and we were interviewed by the temporary

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

dean, a guy named [Robert E.] Spiller from the English department. And he was a dean, temporary dean, and he interviewed me and he was actually the one who hired me. Of course I have to say Gerbner—who now, then, was also hired—he must have been approving it. There's no doubt about it. But I didn't really come with Gerbner. It was a separate type of thing. But it was convenient. He knew me and I think he liked me. And so I was beginning here. And there was also another, actually, fellow student, Wendell Shackelford, who came from Urbana. And he was actually a real Gerbner student and he came also to the Annenberg School and so we were both beginning.

But I had not written my dissertation. I had another problem and that was my visa. And I was required to go for—with this visa that I had—to go back for two years to Germany after I finished my PhD. So I delayed it a little bit. I came as I said in 1964 to the Annenberg School and I finished writing it [the dissertation] soon. As usual one polishes a little bit and so [on]. But I defended it only in 1967, which was the last date of that defense for me possible.⁵ And so that's what I did. But in the meantime, it was the issue, Should I go back or not? And there was an effort to put legislation in Congress that I would be exempt. There was a Senator [Hugh] Scott of Pennsylvania, and he wanted to do that. And I have still this letter from George Gerbner which was of course a tremendous exaggeration. But he certified that I was indispensable—that the whole Annenberg—I'm exaggerating—would fall apart if I would be going and I cannot possibly go back [laughs].

Anyway, he didn't have to do it. There was, independently, a law passed that one could in fact apply to be there if one had a green card, etc., etc. So it was solved. I could stay on. And I started teaching courses in content analysis and I worked in the proseminar. The proseminar was at that time a very different entity, and actually Gerbner modeled it after the Urbana thing. But it was very different. He was in charge of it, but he had Wendell Shackelford, me, and then invited some other faculty members. But he wanted to shift and with us, actually, the University of Pennsylvania wanted to shift the Annenberg School from a media-art kind of program to a more academic one. And so that was his mission. That's the reason why he was very selective at who would be in the proseminar and give lectures or tell things, etc., etc. But I was helping out there and that was kind of my mission before I had even my PhD. I should also mention—you know, I said previously I was a designer. And when we came here we had a brochure of the Annenberg School. I forgot now how many pages, very few, green, miserably printed and I have the copy still. It was so awful that I said, We have to do something better. And so Gerbner said, Why don't you do it? And I designed it and I worked at that time with Mary Ellen Mark, who had just graduated from the Annenberg School with a master's from the previous idea as an art, media art, thing. She had actually a master of fine arts from the fine arts department, but then went to the Annenberg School for the media exposure. And she was actually a remarkable person who—well, she says that she had no idea about photography. She was once asked to take pictures and as soon as she had a camera in hand she knew that this would be her medium. And she was an amazing photographer. I worked with her and the first bulletin is full of great pictures, all action-oriented, and all, you know, seeing people as opposed to, you know, pictures like talking heads or like with the Annenberg School, when you look at

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.

the faculty, they're all just faces, but there were actions. Anyway, it was an exciting invitation for people to come. And I'd like to show this to you, and I did it several years afterwards also still and it became somewhat better as I processed. But that made, I think, a major impact in making the Annenberg School somewhat more attractive to the outside world.

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, it was, actually, initially a media arts program. But there were a lot of people that did this kind of thing. For me it was, for example, interesting. There was a television studio. I had never been in a television studio and so I was—I went there and enjoyed [it]. There was Paul [unclear] from London, from New York. He came over and taught that, and he was a very energetic teacher, and the Annenberg students enjoyed this tremendously, to be part of that learning television. There was also a radio studio which is actually—was located in, well—the Annenberg School has so changed—but it is now where the student department is. There was a radio studio. And this was very nicely done with a one-way mirror—no, no, that was two ways. But there was a separation between the people directing and taping. So there was lots of media. Then it was also—one is graphics and photography. There I met Sam Maitin, who became, later, a good friend, and you'll see several pictures here and I have lots of them. And he had actually an office with me, in G22, which is now the door from the student area to the lobby. That's where my first office was. And there are also pictures in the first bulletin of me being in there. And it's a very great picture.

Anyway, so it [the Annenberg School] was very media-oriented, but for me it was interesting that I got really first contact with this kind of production issues. And I made, actually, a lot of good friends because I was basically a student, one could say, and the people that were there were also students. So we bonded in many ways. There was, just a few months ago, a fiftieth anniversary, a reunion, of that graduating class. And there were all those people that had the television laboratory—and so this was an amazing thing. And they told me also, but I almost forgot, the kind of environment—the outside environment—that made them go to the Annenberg School—and what happens afterwards. For example, women escaped basically the force of being submitted to their ordinary role by taking another degree, and the difficulties they had even with their own family to study a master's degree—You don't ever get a husband!—is one of the things they were saying. So I think this was an amazing group of students trying to escape from the oppressive environment that existed outside.

Also, communication was not really known or a discipline. People had no idea, when they were afterwards applying for a job, [saying] I studied communication. So what? I learned from this fiftieth reunion when they said, you know, I have a master's degree from the Annenberg School, [the response was] Oh well, then, you can type. So this was kind of the perception at that time. So communication was not a discipline at all. In some sense, not the Annenberg School, but we made it. We made it and that was the beginning. 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 it [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 1969⁶—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 was actually the result of a conference that was in 1956 [sic: 1955] in Urbana. There was Charles Osgood, there was Ithiel de Sola Pool, and lots of people. They had a great conference. It was taped and analyzed later. People wrote papers.⁷ And to me that was actually an advance beyond the Berelson thing. And I learned a lot from that. And so I went to Gerbner and said, We should do something like that. And then he said, Why don’t you give me a list of people that you could invite? And so I gave him that list and then it became a conference of the Annenberg School of Gerbner. But I played a role in chairing a session about theory, and I wrote one paper that is widely cited on three models of communication, in which content analysis can be embedded.

And we invited a lot of great people—Philip Stone was, for example, very big with his *General Enquirer*. I remember, this was very impressing at that time. He came with a little, one could say, a book-like folder. It had two tapes in there and he said, This is the *General Enquirer*. The *General Enquirer* is a general enquirer—it enquires on anything. So he had these two tapes, that was it! Now of course it was preposterous in some ways. The generality—the claims for generality—but he had lots of good ideas, and he’s stimulated much of research, and he had amazing sessions there where people used the *General Enquirer* to study, quantitatively, content analysis. There were lots of people that were—I forget now the name—but people, they looked at, actually from an anthropological point of view—so it all came together. I remember one person was, to me, later on very influential, is Edwin Shneidman. Edwin Shneidman was the head of a suicide prevention center in California, and his task was to separate fake suicide notes for extortion, etc., etc., and real ones that one should take seriously from a psychological point of view. So he made a content analysis—tried to—to separate them. And there was an interesting competition. Osgood had an idea, saying that one would be able to look at the motivations for—if one looks at the motivations, one would probably sort that out quickly. And he developed, actually, in Urbana—he asked people, panelists, that were not suicidal, to write suicide notes. And then he put them together with the real suicide notes and developed a content analysis to sort them out, and he had results.

And then came Philip Stone and he wanted to do that with his *General Enquirer*. And that’s of course a competition between Harvard and Urbana. And Philip Stone was more successful. So, winner [laughs] of computer content analysis at Harvard, etc., etc. So this was kind of the competition. But the idea of this analysis, of using computers to do the work—that was, I think,

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).

⁷ Ithiel de Sola Pool, ed., *Trends in Content Analysis* (Urbana, IL: University of Illinois Press, 1959).

a very important development and we presented many of these ideas. Perhaps we celebrated more the computer content analysis at that conference than it was worthwhile. But it was the new thing to do. And it is still, in fact, now is even more so. But that was then published in, you said, in 1959 [*sic*: 1969], and all the panelists, they were the co-authors.

Q: Anatol Rapoport was—?

KRIPPENDORFF: I invited him—I mean, that comes again [from] my more cybernetic approach. And he was, of course, not a content analyst. But I thought it was important to have someone who looked—could look at the systems of messages, let's say. And he did actually a very good job. And he described that one should look at not just at individual messages rather than their connections, and that was, I think, to me—I was glad to have invited him but he was also an important contributor in this whole conference.

And there were so many people. [Ole] Holsti was there. [William A.] Scott, the one with Scott's pi, was there. So I think it was an amazing collection of good people that contributed. It was not always easy to get papers written by them. I remember, actually, Anatol Rapoport didn't produce a paper, but we had a tape and I wrote many of the things up and then he edited heavily—and so that was a very good result. But this was kind of my early experience, the first conference again, in a way. But I was not really—I was the one who proposed it and gave the names and so [on], but Gerbner was the organizer. But it was also, you know, a bit of a tension. Gerbner didn't present a paper and that was actually bothering himself later on, because it revealed that he had no part of the conference. So then he added the Cultural Indicators proposal that was not presented in the conference but it was—he wanted to be a part of it and rightly so.⁸

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.⁹ 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 Cultural Indicators itself was not a term. That came later. But there was at some point, actually, someone in Congress asked Gerbner whether he could make a content analysis of violence on television. And that was something that nobody had done. And it was an issue, actually—a political issue—of regulating television stations for showing violence and many people—unlike today—they were very worried about the effects of violence on the public.

⁸ 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.

⁹ George Gerbner, Marten Brouwer, Cedric C. Clark, Klaus Krippendorff, and Michal F. Eeley, *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 at some point Gerbner came to—there were, actually, at that time there was—when I was teaching content analysis. There was a public opinion researcher from Holland, Marten Brouwer, and then there was a postdoctoral fellowship, Cedric Clark. Cedric Clark had written a dissertation on the depiction of black actors on television. He was black himself and his thesis was that blacks were just made fun of—had inferior roles—and he made a content analysis to make that point. So he had at least real experiences with content analysis.

Marten Brouwer was a public opinion researcher. He had no idea about content analysis, but he had the quantitative background and he knew how to count on a Hollerith Machine—the cards and so [on]. That was his contribution. And he, Gerbner, summoned us in his office and he said, I'm asked to do that. I cannot do it unless you help us. It has to be a team work. It has to be distributed, and will you do it? And I was, of course, in favor—everyone was. So that was actually the beginning of it, and it actually transformed much of the Annenberg School—kind of underground. We developed—and every one of us in a different way. For example, Marten Brouwer was interested in the personality of the victims versus the perpetrators and so developed some kind of characterization of it. And Gerbner was interested in the nature of violence or how many people killed, etc.

And it was very difficult. Most of the content analysis that had been done was actually done on text, and text is much more easy than visual. What is the unit of analysis? So that was one of the biggest problems: What is the unit of analysis? Well, we had Sol Worth and he, at some point, said, Well, you have to look for scenes. As soon as a camera shifts, it's a different scene. But that didn't work either because, peaceful situations can become violent, but the violence is what we were interested in. So it was very complicated.

Well, let me say it differently. We were all struggling and trying to get something analyzable and we had the greatest difficulties. Marten Brouwer, he had these—from a public opinion point of view—had the idea of just asking questions and to code the answers. And it was so unreliable. I remember there was one case where we—actually, I have to also say at that time it was very different: We punched cards and we grounded through a card sorter and got frequencies, and then we looked at the frequencies. And they were so unreliable. At one point—and I wrote, in fact, later about this—at one point we found there was a category like this and this and this or “cannot code.” The only thing that was reliable was, “I cannot code.” So what can we do? So that is a tough issue and so we struggled very hard.

Actually there is one thing that—an innovation, if you want—I made. We developed a testing tape, a tape [of] an original violent show. And we asked—we coded it and we said we want to be sure that this is card-categorized as a violent scene, this is not, and when it is a violent scene, that these and these dimensions that we were interested in. So we developed a coding scheme, we researchers coded it, and then we showed that to students and we tested the extent to which they complied with our coding. We found, of course, that some of our coding was mistaken, because they had actually better reasons for doing it differently. But nevertheless this was one standard—ways of standardizing issues of reliability. And out of that came the notion of how to count, how to study reliability.

And I don't know if you want to go in that [direction] but I—well, Marten Brouwer said, This is not usable. We cannot possibly publish this. And I started thinking, We have to find a way of measuring this. And I found, with my minimal knowledge of statistics—I developed a coefficient, a measure, and that came actually out of information theory initially. And because

information theory has also the ability to separate noise from information. And when you look at information, if there is a clear connection between, let's say, what people see and all agree that this is it, that is information. And when they don't agree that is noise. So that was my first paper, actually, on reliability—was on information theory.

And then I used that but it turns out to be not workable because of—I don't want to go into technical details, but it was not workable as a method. But the idea was there. And then I went to other kinds of things and I found a coefficient or a beginning of it. But we had no time and the way we did it, actually, is we assembled—in a big room with a big table—we assembled students, six. And everyone had to do one thing. The first had to summate this, the next had to tabulate this, the next had to average this. And so we went around and this is the way the first reliability was computed—very primitive.

But then I said I have to learn computer programming. And I took a course in electrical engineering on Fortran IV. I was the only social scientist over there. And I learned how to program and then I programmed, indeed, the first version of the Krippendorff's Alpha, and it was implemented. It was working. We had to punch cards and they were submitted at the computer center, and then we got some results and that is the way it was.

But there was a very interesting controversy with Gerbner. Gerbner didn't understand that. And Gerbner said—and he was correct—he said, The industry is deliberately making things ambiguous so that there is no definition. Reliability is not a measure that should be applied because that is a sign that the industry is successful if we are unreliable. Well, we could not convince him—in fact I have still some correspondence. I said—everyone else said, I mean the three of us said, Well, if that is so we should have some coding, some way of describing the ambiguity and with that it should be also reliable. We cannot say that is—the reliability is—actually giving credit for the industry [laughs]. We need to defend our findings even if we say it was ambiguous. So that was kind of a struggle, a constant struggle with Gerbner. But he realized that, indeed, when he made his presentation in the Senate, he had to say something about the reliability and that was, I think, for us the important—for me, I'd say—the important contribution.

There are lots of other stories. For example, Marten Brouwer, he did an amazing experiment, which I tested—I wrote much about—in order to find out what is reliable, and when can we say it is reliable and when not. Well, what he did is he wrote a coding instruction with very complicated Dutch words—and saying, These are the characters you have to code for these television characters. Now they were chosen to have no English equivalent and, one could say—kind of an analogy or something—they were also very difficult to pronounce, with [Dutch inflection] in it [laughs]. And then he asked coders, Do the best you can, categorize these people in these terms. Well, surprise, surprise. It was not zero reliability. It was 0.44. Then we said, If this is 0.44, people would not—analysts would never know what the viewer saw, the coder saw, and these Dutch words are meaningless. So this is the absolute minimum where one could say is totally unreliable. It's not zero, it's 0.44. Well, one can argue about the 0.44, but then we came to the conclusion that it must in fact be like 0.8. And that was republished, and it is now increasingly the norm. But this was the kind of early beginnings of the reliability measure. I mean, 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.¹⁰ 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 a presentation and it kept being a theory. And I mean, for me, it was instrumental because I defined content analysis in this communication sense. I didn't want—I mean, I acknowledged the possibility of other kinds of models—I have no objection to that—but it was not very interesting. And, particularly, I was always concerned with the discipline of communication, instead of going into the Osgood psychological approach. I was interested in making contributions to communication and thereby pushing the communication model. But actually this paper is widely cited and has had lots of influence in other areas, in communication areas and other disciplines also. But I don't think it informed the violence study. This was such a rush job and it had to be done—I forgot now the time frame—but in a minimum amount of time. We had to hire students and the students have other things to do. They couldn't just stay on the job. We had to train them. That's the reason I had this training film. This was really a big operation and it was difficult, and I'm not so sure if I'm so happy with the results. But it was something that was presented to the Congress—I don't know if it makes a difference.

But there were, later on—and I have the books upstairs—several things were published including—this was also interesting—including, for example, the contribution that Marten Brouwer did about the characters of violent people and victims, which Gerbner didn't like but it was at least an appendix. And so, I think, it generated a lot of attention. It should have been better. It was for me, actually, a very important period of learning, because I was kind of—I was a coder as a student. I was not a content analyst per se and I learned just a lot in planning this. Later on I was a consultant to so many different content analyses and also as an advisor of students. I mean, I've done a lot of content analysis and I'm responsible for that. But this was my first real, big project, let's say. And I still cite experiences from there and I think that was very important. And I think also the epistemological issues, like what I mentioned to you—the controversy with Gerbner—and I still have that letter where he said, This is all bogus, you know [laughs]. He didn't use the word but he said, This is not meaningful to have for reliability measures when the intent of the industry is precisely to have ambiguity. And he's right in his own—methodologically he's wrong but conceptually he's right.

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

¹⁰ 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/.

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. Before Gerbner went to Urbana, and we all went to Philadelphia, he told me, I have one PhD student, and that is Sol Worth. Sol Worth wanted to be an academic—he was a painter and filmmaker—and he saw the need to be respectable. But he never never did any PhD work. But he was involved in these—with an anthropologist who hired him to be doing the films with Navajos, and he drew a lot from the experiences of the very different kinds of perceptions that Navajos have about what a good film is. Which is actually showing the landscape, and nothing happens. And that was really a very different way of thinking. And so Sol Worth was very enamored with it, and these experiences, I think, made a great difference for him. He wrote also a very interesting paper and that still, I think is: you can't say no visually.¹¹ This is not possible. To me that is very profound. You can make a bar, but that is already a symbol which you have to learn, no? But you can't say no. So that's one of the things [laughs] that he did. So he tried to be an academic and he made in fact a lot of academic contributions of this nature.

Charles Wright, I don't know exactly when he came. He came later, '69. Well, when I was a student I actually read Charles Wright's book, *The Sociology of Mass Communication* [*Mass Communication: A Sociological Perspective*, 1959] and that was part of the staple that we did in Urbana. So we were very happy to have him. I mean Gerbner was constantly—I wouldn't say constantly—asking whom should we invite, but he was certainly on the list from the beginning as someone who would certainly 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.

But there were people—one important person was Hiram Haydn. Hiram Haydn was actually the editor of *The American Scholar*, and he was a writer, and he promoted important novelists. We had Barbara Herrnstein [Smith] who was a—I don't know how to categorize her—but an important literary scholar, and teaching writing. We had a sociologist named [Rolf] Meyersohn, who was very influential in shifting towards sociology. Shel Feldman was there and he was more an Osgood-type of person—psychological—and, yes, there were a lot of exciting people besides what I mentioned earlier—the television and graphics, and they were slowly phased out—the radio very soon, because there was nobody interested in it.

The graphics stayed for a while and Sam Maitin was in charge of [that] ultimately. And I don't know exactly at which year it was abandoned—it must have been in the '70s or end of '60s because—well, I don't want to get into this, but Oscar Gandy was a student at that time. He took, actually, the course with Sam Maitin and he made a famous poster that I have in my office on revolution. And so that must have been '72. So Sam Maitin continued, the television continued also for quite some time, but we changed. That was, I think, important. That was Gerbner's contribution—not only Gerbner, but it was increasingly clear that the equipment we had, the television equipment, was always out of date relative to the industry. And so we said that it is not feasible to teach people how to make movies—television production—when the equipment is so outdated that when they go to the industry they have to learn anew. But what we can do is to teach them the principles. And that was really the mode of operation of using

¹¹ Sol Worth, "Pictures Can't Say Ain't," *Versus* 12 (1975): 85–108.

media by saying, We're not interested in teaching you how to press buttons, but the principles underlying it. And when people took television courses that's what they were emphasizing and that was, I think, pretty good. So that was the justification to maintaining media labs, to say, There is something in the production—translating ideas, if you want, into something producible. And that was really the operating procedure that Gerbner introduced and made manifest.

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 much later. But the sociologist Meyersohn, 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 assigned to make the Annenberg School academically acceptable. That was the University of Pennsylvania's demand. Maybe one should go further back. It was founded by Walter Annenberg, who was the owner of *The [Philadelphia] Inquirer*, and his idea was, basically, that he wanted to have an institution that teaches people to work at *The Inquirer*. So it was staffed with writers, journalists, that could help make professional journalists for *The Inquirer*, and that's what—precisely what—the first dean [Gilbert Seldes] wanted to get away from by making it a general media-philosophy type of school. And he succeeded, actually, quite well in introducing different media, and I mentioned Mary Ellen Mark—photography, graphics, radio, television—that was not really *Inquirer*.

There were some strange happenings. There was, for example, a teacher of writing before Hiram Haydn and he had at some point led a union strike against Annenberg in his *Inquirer* and Walter Annenberg managed to get him fired. Well I'll—let me say so, I shouldn't say that, and don't quote me—but he was let go, and Walter Annenberg didn't like it, so—. But it was also—the relationship between [Walter] Annenberg and the Annenberg School was fascinating. When I came, Walter Annenberg always said that this is my school. He owned it. Now we had fascinating parties. Once a year the whole Annenberg School was invited in Philadelphia in some place, at the Barclay, at the—I forgot now what it is—anyway, big places. The whole Annenberg School was there—faculty was there, Walter Annenberg, all the trustees came, and there was an Italian band and dancing and so on. That was his school and he treated them as his school.

So it was a very different kind of atmosphere and Gerbner had really the difficulties of balancing that. This was a difficult balancing act between satisfying Walter Annenberg, and the University of Pennsylvania, and then the third element—that Gerbner was a Marxist. And that was a taboo topic, and so it was no doubt difficult. And in 1973—oh no, let's get into this, in terms of his style. You asked about that. He wanted to make not just the School but also the

discipline of communication moving in an academic direction. Actually the discipline moved already in an academic direction, but he had a certain conception of what communication is, and that is what he wanted to push through. In the Annenberg School he could do that because it was smaller and it was his school. And it was sometimes dogmatic, and students were not always that satisfied. And there were lots of occasions where people protested or were just not very satisfied.

And I remember there was—I did one thing about functionalism. And I went to the encyclopedias and looked at all the different words, uses of the term function. There is not just one function. There are so many different functions—and so I, in the proseminar I presented these, well, I think 15 different ways of looking at functions. But that was besides the point. There was [to Gerbner] only one function and that is the industry dominating the mass communication. So these kinds of tensions did exist. So Gerbner was a bit dogmatic, and that played out later—also became increasingly important. But he had the idea of making communication the center of both the ICA, and the Annenberg School, and in general. He was actually more involved in the IAMCR [International Association for Media and Communication Research] than the ICA, because that was international and he had Marxist friends. And so it was complicated for him—no doubt very difficult—for him to balance that. But when I said “dogmatic” is probably not so clear to say that. But when I compare, for example, Gerbner, with Michael Delli Carpini, Gerbner knew what is to be done and informed the faculty. Michael Delli Carpini raises an issue and there’s lots of discussions—maybe more discussions than one can tolerate. But there is discussion and there comes some, slowly, a consensus out of that. Gerbner was not that way.

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.¹² And the second one was published the next year which was on data—on generating data in communication research.¹³ 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 say something about the “Values” paper. That came actually out of a collaboration with the political science department. They wanted to make—what is his name [Willard D. Keim] now? I have to think about it—but he was a political scientist and he wanted to make an international study of democracy—what the conceptions of democracy are, for example, in Poland, in the Soviet Union. And that was really his bag. And he assembled an international team of people that would contribute—Philip Stone was one of them. Philip Stone came and he said, I can solve this all with the [General] Enquirer. Well, and I thought that is a little bit too much. As much as I liked him personally, as much as I appreciate his work, but the

¹² 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>.

¹³ Klaus Krippendorff, “On Generating Data in Communication Research,” *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

claims were kind of too much. And at that time, actually, I wrote this paper—for the political science department—not department, for this project—that one should look at different ways of conceptualizing values and political discourse. That came out of that. I forgot now his name [Keim]. He retired and then went to Hawaii, and died over there and then [the] project kind of disappeared—fizzled away.

I remember that I was invited to give, in the political science department, a big paper in conjunction with that. And there I had talked about systems and so [on], and that was, I think, well-received because I said, You know, values—you can deal with it on a psychological level, but it really is a normative phenomenon of larger systems, and that's where it is located. I think that was well-received, and I don't think I wrote it up.

But the other paper of generating data. Well, this was my critique of—well, let's start a little differently. [Paul] Lazarsfeld—no, [Harold] Lasswell formulated a formula of communication: Who says what to whom with what effects? And then he said the “what” is content analysis, the effect is—well what is this?—effects research. “Who” is communicator research. And he divided, basically, the area of communication, the field of communication, in different disciplines with separate methodologies and said, This is what communication is about and content analysis is the “what.” And I thought, That is precisely what one shouldn't do. It kills the idea of communication because communication is that—links all of them. And he had it in one sentence, no doubt. And I mean the idea was of course correct, but the idea of parceling these—“who says what to whom,” etc., etc.—out into separate areas of studies destroys the communication discipline. And then I came to the idea that one should really—no, let's say, if everyone collects data for their own little somethings they never get it together. And that was the idea of that paper. And the idea of that paper was that we have to see that in connection, over time. That had something to do with information theory, with the notion of changes over time, and the connections over time—past history determining in part the present. And I wanted to introduce that into the communication literature, to say, It's a big mistake to study pieces and not [be] looking at the communication process.

And that paper I wrote and I submitted it to the *Journal of Communication*. The *Journal of Communication* was at that time a tiny, tiny thing published by the ICA and the editor's name was [Paul D.] Holtzman, I believe, and he had sent it to various reviewers and they said, No—no good. But he thought, There is something to that, and he said, I'll publish it. And it turns out it made the best paper of 1970. And since that time it has been quoted everywhere. In fact, just now I met someone at the conference of the NCA [National Communication Association] in Philadelphia who writes a book based on that. I mean it's an important paper. But it's also interesting how it goes: Reviewers don't often recognize this. And I don't really know why it was selected as the best paper, but this was in the ICA voting on that and they made it the best paper of 1970.

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 a lot of people. I mean, I don't like to see the world, let's say, separated between quantitative and qualitative people. And in some sense I overcame that myself because I became increasingly a qualitative person. But I have these concepts behind. I will not give up the idea that past history determines how it changes, structures emerged as a result of communication—I mean, that is part of me. And I think the verbal part is increasingly accepted. That this can be formalized, to some extent—and I'll have to emphasize, to some extent—because lots of it is missing when it's formalized, and that is actually the whole notion of language. But that was an innovation and I think people still talk about it. And it has befuddled a lot of people. And as I said the reviewers didn't have any place for it. And I understand that but, luckily—and I have to say “luckily” because I'm sure there are a lot of people that have good ideas, submit it to a journal and they say no. And I hope they don't give up, you know [laughs]. I tell to my students, Don't give up. Just find a better journal. It's difficult. In this case I was lucky, that the editor overruled the reviewers. And actually at that time the journal was very small. 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.¹⁴ 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 mean you mentioned that it came from Ashby and so, indeed, I think many of these conceptions came from cybernetics. And it's fueled a lot of the—and I think it still has so much potentials that are not explored. Now the paper on communication generating structure without intentionality. In fact the very process of communicating—it doesn't matter what it is—it structures. Now, I think there is, of course, something missing then with the intentionality—some structures are intentional. But even if you don't have any intentionality, structures inevitably emerge. And this paper had something to do with also defining structure—that is probably also important. That the structure had an information-theoretical flavor, namely that there are certain kinds of constraints on the relationships between various kinds of components and they are—that is one definition of structure which is probably not so generally talked about in the current communication literature. But it is fundamental that any human communication—I mean, that's [Gregory] Bateson later on, Bateson said always that there is a content—I mean, he described—there is a content aspect and a relationship aspect. And any

¹⁴ Klaus Krippendorff, “Communication and the Genesis of Structure,” *General Systems* 16 (1971): 171–85, http://repository.upenn.edu/asc_papers/225.

communication implies relationships, and establishes relationships or ruins relationships or transforms relationships. That is fundamental. That's fundamental to communication. But I think many people don't recognize this. It's a shame, but what can you do?

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.¹⁵ 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. The book came out, actually, and Sage asked me to write it. There was no content analysis book other than Berelson, and I had actually written, you know, this piece of three models and co-edited that book. So there was something there. And I became increasingly known for what I was teaching, so Sage asked me to write it, and I did. That was in 1981 [*sic*: published in 1980] I was finished. But before that, actually, there was in Annenberg a lot of interesting developments. For example I had one student, Charles [*sic*: James] Taylor. He was Canadian and he later on went to Canada. You know him?

Q: Is this the philosopher Charles Taylor?

KRIPPENDORFF: No, but he [James Taylor] was a student, Annenberg student. He was older than me a few years and he was actually in the television laboratory. And so we made experiments, actually, about the difference between hearing and seeing. For example, we delayed their own voice for a few seconds so that they couldn't hear their own voice except a few minutes later—and what kind of effects that had. It turns out, one cannot talk anymore. So then we had made experiments, for example, cybernetically informed: What if the camera of a television gets on the television screen, focuses on the television screen, what happens? When people made a hand movement, sometimes it becomes smaller or disappears, sometimes it becomes bigger. And all of these kind of cybernetic experiments we did with television, and that it was actually Charles [*sic*: James] Taylor who was working in television at the time. But he was also interested in organizational communication, and I was also interested in the kind of the systemic aspect. And he had a summer job working at the Philadelphia Museum of Art. And the task was actually to find out, why do people not go to the museum [laughs]? It changed, but, anyway, that was a task. So then we talked, actually, endlessly about organizational communication: the struggles he experienced within working within the museum, the authority structures. And that informed me in many ways, also, about the social construction of the kind of authority, how it's maintained, etc., etc. So that was work with Jim [James] Taylor. And he wrote a dissertation with me and later on became the chair in Montreal. And now there is a second generation—no, a third generation. The second generation is François Cooren—you know him? He always says that I am his grandfather, and his students, you know [laughs]—he has a sense of the lineage, and he always relates it to me and to Ashby, etc., etc. So there was a lot of things happening, but it didn't necessarily result in papers. I wrote a lot of papers, actually—I wrote, for example, a paper for a book on the recursiveness of

¹⁵ Klaus Krippendorff, *Content Analysis: An Introduction to Its Methodology* (Beverly Hills, CA: Sage Publications, 1980).

communication, and I remember I drew fantastic diagrams which are also preserved.¹⁶ And that is frequently cited. This is a book that was edited in Canada and that is frequently cited also about the recursiveness of communication. And I think that is what many people know me—and that is the cybernetic influence.

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 thing because I knew so much about it, but soon, very soon, I taught a course that I taught for many years called *Models of Communication*. And the *Models of Communication* course was basically a cybernetics course. And I talked about—well, not necessarily information theory, but about the whole notion of relationships between people, and looking at that from a systemic point of view. And I think that's where I used Ashby as a textbook, and people got a lot out of that. I think, when I talked to earlier students, they were always like, *Models of Communication*, that taught us for a long time. I taught also a course at some point about information theory, just information theory. And that was when I wrote, actually, a book on information theory.¹⁷ And I put lots of things together, including what I contributed that was not [Claude] Shannon and not Ashby. And so I made a course out of that—that's one other kind of thing.

I'm thinking of students, early students, that for example—and I don't know about the year—but a Filipino student Alfonso, Herminia [Corazon M.] Alfonso. She was, one could say, a kind of community worker in—she was actually a professor, but she was interested in community work, and she wanted to write a dissertation. She took, actually, my *Information Theory* course. And she wanted to see what can one do in the Philippines about doing research, communication research, and particularly communication research that enables development. That was her mission. And I said, Well, if you look at like what Americans do, they only measure what is, but not what comes to be. So she related to me—which is of course obvious—that most development in the Philippines takes place, governed by the mission of a government, of a state government. They send their commission to a village and look at it and say, You need water, you get a well. You need a street, you get a street. And, for example, she always said, You know, one of the wells, where is it placed? In front of the mayor's house, no? So there is a collusion between the government feeling the obligation of supporting development, the interests in the community to improve their own well-being at the expense of the community. So, actually, I worked with her. I'm very proud of her dissertation. I worked with her on what is a community—what is it, how can one find out what a community wants, does, finds desirable? And so she developed a way of doing research and she called it “socially shared inquiry.” And that was totally against the Western notion of inquiry, where you are an objective scientist and you verify your data and you make a theory and that's it.

¹⁶ 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.

¹⁷ Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

And we said, Well, a community that is competent about its own being, they should be developing themselves. And one needs to help, sometimes, the community, but helping is different from determining. So we developed—actually, there were several strategies. For example, we called, or she called, actually, the scientist that would come to a village an enabler—not a scientist, not an observer—an enabler. And the enabler has to find a way of getting the community together to find out what is happening there—what the problems are, if there are any. And so the community developed, basically, a questionnaire. The questionnaire was filled out by the members of the community. The community afterwards evaluated them themselves and then came to a conclusion as to what is to be done.

Now from a behaviorist point of view this is an absolute no-no [laughs]. You never asked the people that are to be studied to participate in the studies. Well, there are several other dissertations that I did—that I supervised later—where we made that much more formal. But that was a way of enabling the community—the communication, introducing different kinds of communication among members of the community—that would enable them to be confident about their own being, and then they can communicate to the government and say, We want to do this. And so that was written—actually, it was a dissertation, and it was then a bit rewritten in the Philippines and it came in a book.¹⁸ I have it upstairs. I was asked to write a foreword, and everyone quotes the foreword [laughs]. But it is amazing. I meet—often in ICA, frequently—students and they say, You were the advisor of Herminia—she’s dead unfortunately, she was much older than me. So I had, actually, many students that were older than me. But she died, unfortunately. But she had a major impact on the issue of development. So that came out of the notion of cybernetics, embedding it in social phenomena.

Q: That has design implications, too, and I know you elaborated some of those later, but—
KRIPPENDORFF: Yeah, that is true. But that—as I said earlier, that’s kind of my design background. It is carried through a lot of things that I’m doing or thinking.

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

¹⁸ Herminia Corazon M. Alfonso, *Socially Shared Inquiry: A Self-Reflexive Emancipatory Communication Approach to Social Research* (Quezon City, Philippines: Great Books Trading, 1999).

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

Philadelphia, PA

Interviewed by Jefferson Pooley

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 was a media school—media philosophy but also had numerous labs: writing, television, graphics, radio. And so that was very practical. And the university had, actually, the idea of making that a more academic institution and Gerbner was actually hired to do so. And so we inherited several faculty—I think there were five or six. But they were largely lab people in television and so on. And so Gerbner hired, or Gerbner came, Shel Feldman came, Wendel Shackelford came from the university—co-student with me—and I. So we were the beginning of it. Then came very soon Bob [Robert] Shayon, who didn't have a PhD, a BA only, and the catalogs always mention BA. But he was a major television critic and made major contributions in terms of activism, awareness of television structures, etc. So he was one. The other one was actually [Bob Sayer?]. He was the writing lab person, and he was replaced by Hiram Haydn. Hiram Haydn was a major scholar in the scholarly community. He was an editor of the—I forgot now, the scholarly—

Q: *American Scholar*.

KRIPPENDORFF: *American Scholar*—yes—and he was also an editor for a publisher and promoted poets, writers, historians, etc. So he was, I think, a major force for the writing lab. And then we hired a sociologist, for example: [Rolf] Meyersohn, who introduced the sociological perspectives. And it slowly moved towards a more academic discipline, if you want, or area of studies in the University of Pennsylvania. I don't know whether I should talk about details but—let me just give you a story that I mentioned earlier. The whole thing was very unstructured, but Gerbner was really in charge. He wanted to define the discipline of communication, and also the Annenberg School. He had the idea that there are three major areas of studies, one he called “codes and modes,” thinking about—well, I would translate it into language, meaning, and study of content. The other one was “institutions,” that means

looking at the mass media as an institution in society, and not just merely a delivery mechanism. And the third one was called “behavior,” or looking at effects. And he introduced this notion in the Annenberg School, but also tried to do that in the communication field in general.

I think it served us well to enforce some sort of a spread over the different areas so that people who came here, they had to be a little bit knowledgeable about all of them. But it was also a bit of a tour de force because many people—for example, Rolf Meyersohn, he was a sociologist. He was not really very comfortable with that and he didn’t last that long, and then he disappeared. And then, actually, Charles Wright was hired—also a sociologist—and I had studied, as a student, his book on sociology of mass communications [*Mass Communication: A Sociological Perspective*, 1959], so I knew his approach. And so it slowly moved towards more academic things.

There is one other person that I need to mention, that’s Ray Birdwhistell. Ray Birdwhistell was a bit of an unusual character. He had never had a faculty position. He was in various institutions as a temporary contributor. And he was interested, what he called, in “kinesics”—that means non-verbal communication—and he prided himself to have created that field. And it was in fact alive until he died, and nobody talks about it anymore. So this is kind of unfortunate, but anyway. As requirements, we had, actually, a proseminar which was for credit. We had a colloquium that everyone had to attend—but it was not for credit—and then two other courses. At the colloquium there was a [practice of] inviting important people, and I heard [Paul] Lazarsfeld, I heard [Harold] Lasswell, lots of people that came, that made, in fact, the Annenberg School much more academic. For example, one person that had great influence on me—and in fact I was the one who suggested it—is Gregory Bateson. He came also, and he came into my seminar, and so it was very stimulating.

But that was the colloquium. The proseminar was, actually, organized by the faculty as a whole. Gerbner was in charge and he orchestrated, basically. Often it ended up in a kind of debate between various kinds of faculty members. And I don’t know if I should now get into some of the, shall we say, complaints. I think it must have been in 1973, shortly after Hiram Haydn left. There was a new writing teacher, a very famous literary theorist, Barbara Herrnstein Smith, who took over the writing lab. And there was a lot of struggle among the faculty. And students were befuddled because it was—they saw all these kinds of struggle between territories, and they were forced to make loyalty pledges to one faculty or the other, and that created just a lot of, well, dissatisfaction. In addition, the proseminar had to be graded, and everyone had to write some sort of a statement as to what they got out of it—it was a good idea—but Gerbner gave most of them, or many of them, a D to start out with, and that created of course so much dissatisfaction.

Later on they got better grades, but this was kind of a philosophy of teaching that I personally think is just discouraging, and that’s created lot of revolts. And there was, in 1973, a major, one can say, revolt of students saying, What is the relevance of all of this? We don’t get what we want to hear. And, well, students don’t always know what is good for them but still, one had to

keep within the domain of what people would be willing to study. So that created a lot of difficulty, and it showed Gerbner as kind of a more dogmatic figure imposing his own view of the field.

I don't know if I should—I mean the catalogs say, actually more about the kind of people that we hired and fired. And, for example, actually, Sol Worth is a very interesting example. And I remember when I came Gerbner said, The first PhD student that we have will be Sol Worth because he is an artist and he's interested in communication, has lots of aspirations to work with anthropologists and so, and he will be the first PhD student that we graduate. Well, that didn't come to be. Sol Worth stayed the way he was and taught the [Documentary] Film Laboratory until he died. So—but there was a lot of struggle in the faculty, and I was relatively junior and I didn't really want to get too much involved in this, but one could not entirely help that.

First of all, in 1970 there was a major opposition, and Oscar Gandy, who was a student at that time, he made a poster among others, and I kept it in my office, and that is "Support Revolution." He was a black student and he was—as many of them were—against the Vietnam War, and Gerbner wanted not to promote that or allow students to protest. And I was actually surprised. And I recently asked Oscar Gandy to interpret that poster again, and he reminded me—which I didn't quite recall—that indeed Gerbner was against the demonstrations, surprisingly. And so Oscar Gandy was one of the many students who protested this kind of intrusion, and he made that poster and I have it in my office now.

But in 1973—it must have been around 1973—Gerbner's continuation as dean came to an almost end, and he invited the faculty to a retreat in North Philadelphia for a weekend retreat. And he said, You know, I'm soon no longer dean. You should help me finding another one, and that should be a collective decision. And so we talked about the future of the Annenberg School. Incidentally, it's very similar to what we had on Monday with our current dean who had also the same idea. But it went very different.

Anyway, so we were all prepared for a new kind of dean, but then came the demand of Gerbner to declare loyalty to him, and it turns out that, actually, Walter Annenberg influenced the university to say, I want to keep George Gerbner as a dean. And so that the replacement was, kind of, decided in ways that I don't quite know. But he continued to be the dean and that created a lot of tension within the faculty. There was Hiram Haydn and Bob Shayon and I was also—Shel Feldman, etc.—we said, Well, you know, we don't want to interfere with that decision, and that should be the university's decision, and it's OK to have him as a candidate. But we didn't want to blindly declare loyalty.

So it created a lot of tension, and I think the students realized this and the students saw it more, like, as loyalty to the different kind of faculty. And they revolted very overtly. And I remember a session in the proseminar, when everyone was together and the students—it was very articulate, how miserable the Annenberg student is despite what the document that they wrote, the presumed leadership of the Annenberg School. Well, I don't want to get too deeply

involved, but it was not always a smooth ride. But the three divisions that I mentioned, what transformed slowly and turned into so called “buckets,” where everyone had to take a few courses of that. That was slowly undermined by more recent developments and we have now a very different kind of division in areas, like more quantitative or qualitative studies, or behavioral and cultural. 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. We had for the longest time mimeo [mimeograph] duplication and that had wax mattresses. One had to type on it and the type produced an opening for ink to come through and then we had to grind it through a mimeo machine, hand-cranked. And so at some point I remember I had lunch with Shel Feldman, the psychologist. And Gerbner had also lunch with someone else and I, Shel and I, we talked and we said, We should have a better machine—that is so old-fashioned. It’s not old-fashioned but so cumbersome. And so I wrote on a red napkin, We should have a Xerox machine. And I didn’t want to really disturb Gerbner—who was in conversation with someone else—and I simply took this red napkin and gave it to him, and he wrote down [gestures writing motion], If you can make a copy of this, you get one. So the problem was, that was on a solid red background and it turns out that the Xerox machine, actually, is based on blue light, and the red and the black is almost indistinguishable. So I went, actually, downtown to the Xerox people and said, Please make a copy of this. And they somehow made it, and we got a Xerox machine [laughs]. That’s kind of a vignette of how that was done. And it was actually in the library, so we could work together with reading, Xeroxing, and the office [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 making a—selecting a dissertation topic, content analysis was one of them. And I thought that it would be a good topic because content analysis was kind of an underdeveloped methodology and I thought it is a key to communication research. In fact, I still insist why communication research has borrowed so many different other methodologies, such as survey research or making experiments, but there were two areas—methodologies—that are unique to communication: One is the analysis of messages—of content—and the other one is actually networks—the whole notion of networks, that communication is not taking place just from A to B but rather it is networked in organizations, administrations, etc. So network analysis and content analysis were, to me, disciplines—methodologies that were unique to communication.

So content analysis was kind of underdeveloped, and I wrote my dissertation, largely conceptual, from the literature and seeing what one can do [to] develop it as a methodology.

And we move in the information theory, and lots of things—and we will talk probably about this at another point. But it was for me, largely, one should say, conceptual—theoretical, based on literature. And I have to also say, based on a different kind of epistemology. And I was very early on, during my dissertation period, I was realizing that the idea of content, the idea that messages could contain something that you could simply take out, is just the wrong conception. And it had the consequence that when you conceive of content being inside the message, then taking it out—it doesn't allow you to take different kinds of things out of the same message. Recently I've had to write something saying, Well, a container contains either wine or milk but not both at the same time, nor can it be for one person milk, and for the other person wine. So the whole notion of container metaphors, as I later on described it, is mistaken.

So I developed, actually, a notion of content analysis that dealt not with content rather than making interpretations of messages of text, interpretations, and making inferences to other things. So, much of my approach to content analysis is actually the issue of making interpretations to the context in which messages are being used. I remember when I was asked in 1978 or something by SAGE [Publications] to write a book on content analysis—there was none really available—I said, This would be better to name it differently. But they said, No, you have to write on content analysis because this is the established term. So I decided to do so, and the first chapter is actually to undermine the notion of content, or to redefine it in terms of the kind of inferences that people make—the interpretations, etc. So that was kind of the conceptual basis, and then—in my 1980 book, although I'm going a little bit ahead—I enumerated different kinds of methods and I also talked about the issue of reliability.

But, coming before that, in 1967 Gerbner got an invitation from the Surgeon General. At that time there was a big debate in the U.S. Congress about violence in television. And there [were] very little studies of violence on television. So he was invited or asked whether he could make some sort of analysis of violence in television. He invited three people at the Annenberg School to join him and he said—well, one is Marten Brouwer, who was a visiting professor from the Netherlands—he was an opinion researcher. And then Cedric Clark, who was a postdoctoral fellow and he had written, actually, an interesting dissertation. He was black, and wrote about the role of blacks on television, and that they were often the butt of jokes and he unraveled actually a whole ethnic representation of television. And then it was me. I was teaching already—I didn't mention that. When I came to the Annenberg School, I taught actually three courses, one is *Content Analysis*, the other one is *Models of Communication*—and we can talk about that later—and the other one is *Cybernetics and Society*. So it made sense that I would be part of the team.

And Gerbner said, basically, This is too much for a single person to do. Unless we make it as a team it won't be done. So we agreed to work as a team and we worked very hard. We transformed the whole Annenberg School—not the whole Annenberg School—but many students to that project. We trained coders, that means students, but students could not just be hired and stay on the job. There were many students and they kept—became part of it and then they dropped out. So it was relatively complicated. And we had also lots of epistemological disagreements among us. Actually, more like Gerbner and the other three.

Maybe we'll talk about reliability a little later, but I remember that we found that there was often very little agreement as to whether violence was there or not. And Gerbner wrote a memo, and I still have it, saying, Well, that is a strategy of the industry: to make it ambiguous so that you do not know for sure what it is, and leaving it open, and this is a kind of a back door to not being blamed. And he was right, of course. But when we want to study it, that itself needs to be reliably described.

And so there was a kind of philosophical disagreement, but we developed these various kinds of instruments. Marten Brouwer had kind of his own perspective: He wanted to look at the personalities of victims and perpetrators, and so he described their characteristics. Gerbner was more interested in the quantity of violence, and so, then, we wrote a big report—actually we wrote it—but then it was rewritten by George Gerbner, and published.¹⁹ And he testified also in Congress. But I have to say, many of the experiences that I, later on, wrote came out of this heavy involvement with the violence study.

I don't know—should I talk about the Krippendorff's Alpha? Well, one of the things was, actually, we observed that there was so little agreement among coders. And Marten Brouwer said, Well, we have to measure this in some form, and quantify, because if we don't demonstrate that there is reliability, this can be easily debunked. And it is correct. So, now it was the question of how to do that? In retrospect there were actually some coefficients to measure that, but none of us knew about them. So I had to start from scratch.

Initially I was interested in analysis of variance, and analysis of variance deals, actually, only with scaled values, and I found, actually—and I wrote a paper on—oh no, let me go back. Before that it was on information theory. And I thought that the idea of noise in information theory is actually a sign of unreliability of coders. So I wrote—the initial version of, if you want, reliability assessment was to use information theory and to measure the amount of noise in their coding.

It turned out that there was something odd about that, mainly because information theory deals with probabilities and not with actual coding things. And I didn't know that. And so, then, I went to analysis of variance and that solved some of the problems of generalizing the notion of noise from the individual categorizations to a large sample—that this problem existed in information theory but not in analysis of variance. So we used analysis of variance—I used it—and I defined some sort of a coefficient to start out with, but it required this scaling [gestures]. And we didn't have, actually, scales. We had categories of different kinds of things. So, I remember it. I was flying to East Pakistan, and that was an endless flight. And I had a lot of

¹⁹ George Gerbner, Marten Brouwer, Cedric C. Clark, Klaus Krippendorff, and Michal F. Eeey, *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/.

paper with me and [thought], I have to solve this problem [gestures emphatically] of going from the analysis of variance to [a] more categorical type of reliability issues.

And then it occurred to me—which I had not learned, or nobody talks about it. But analysis of variance is always a measure of how much an observation deviates from a mean. But when you go analyze it very carefully, it's actually a collection of pairs, individual pairs, that are then averaged in an unusual way. Once I had that, then, I said, OK, now the pairs—when we change that towards “yes” or “no,” “matching” or “not matching,” then we deviate from the analysis of variance but preserve the idea of pairs. And that was the beginning of another coefficient, and I called it Alpha for agreement. And we used that.

I should also say when we said we used that, we had very little time, number one, and number two, we were not very sophisticated in terms of statistical processing. We had card-sorting machines, and we didn't have that much capability of processing as we know now. So I remember that we organized, on a big table, five or six students, the first one had to do one thing; the second had another thing; the third one had, etc., etc. So we had, basically, a machinery of human beings that would, in the end, produce some sort of an agreement coefficient.

Well, then, I decided, This is outlandish. And so I started going to the physics laboratory and learned Fortran IV, and wrote a program for that. And that was pretty successful—although by modern sense limited. But this was all on punch cards. And we had a computer center that was on 34 hundred [34th Street] and Market Street. And the number of times I went there from Annenberg—endless, almost daily went there and submitted cards. And then we got some numbers on print out. But that was, I think, the beginning of this Alpha.

Subsequently it—first of all, I could link it then to other kinds of coefficients. And it turned out that, actually, this Krippendorff Alpha is far better than others. There is, for example, Cohen's kappa. And it turns out that it's so biased in numerous ways, and that [Krippendorff's] Alpha doesn't do that. And then there is Scott's pi, which is limited to two. And then there is a guy named [Joseph L.] Fleiss, who was a student of [Jacob] Cohen, and he tried to generalize Cohen's kappa. But [he] didn't know that he actually generalized Scott's pi. And so this was all very, very unclear.

At some point, actually, someone from sociology sent me a rejection letter by someone who was the editor of *Biometrics*. And he submitted a critique of the Cohen's kappa and was rejected. And I thought, He is actually really right. And so I decided I'll write to this editor, and simply say, He is correct, and we should just publish this. It turns out that the editor was Fleiss, a student of Cohen, and he allowed me to say something that he then discounted as editor. This is still published [laughs].²⁰ But it's interesting, these politics of loyalty, etc., etc.

²⁰ Klaus Krippendorff and Joseph L. Fleiss, “Reliability of Binary Attribute Data,” *Biometrics* 34, no. 1 (1978): 142–44, <https://www.jstor.org/stable/2529602>.

Anyway, since that time I have been actually working to generalize this Alpha to other areas. One important area was actually—because many of the texts have an extension, a linear type of extension, and you have to make units out of it. And the unitization that we, for example, didn't solve in the violence study—where does a violent act begin, where does it end?—there was a lot of disagreement even on that, much less on the coding of it. So I developed a coefficient, and I made many mistakes. I have to say that I had to discount and improve upon what it means to take a continuum and cut it into units and then code them. Then there's another extension because many of the codings are multi-codings—that someone, an individual, like a perpetrator of violence, is not just a male but they may be a male, an honest person, and a professional, but multiple codes would be applicable—that could not be computed with anyone.

Another thing that was lucky, I think: Most of these coefficients that we know, they require that every coder that is hired has to provide a code, a judgment, a categorization. But we had the problem that people dropped in and out, so in some cases we had more, in some cases we had less. And there was also the other notion of [unclear] the very conception of reliability, namely my conception was, Reliability? What do you rely on? Can you rely on the data as opposed to, Are the coders good? Now, we had to hire good coders, no doubt, but the idea of reliability, to me, is still, Can we rely on the data to be representative of something that coders had seen, or that was in fact on television? So this relationship is very different.

So we were forced, in a way, to consider that there were sometimes many and sometimes few, and this allowed Alpha to be much more general. I got recently a request—actually it's now in the process—someone who crowd-sourced the internet, actually, to code images from Darfur, about human habitation in a one square mile thing. So there was something like, I think, it was one and a half million of squares to be judged, and it was simply everyone could participate. He had seven million of judgments ranked by—or categorized by—between two and fifty-six different coders, and he said Krippendorff's Alpha is the only one who can cope with this. But there is a problem that so many data require so much computation that we can't do it—there is no software, right now, available. So, actually, I was lucky and I solved the problem. But I don't want to get too deeply into that.

The point is, actually, that Krippendorff's Alpha, even if it was developed from this very primitive stage, and in ignorance of existing history, which had the advantage that I was not bound by this. I think I say that's always—sometimes innocence is a good start for something different because if I had built things on top of existing methods, I would have probably not come to all of these kinds of things.

I should also mention one thing that I am frequently quoting to say how good reliability has to be. Marten Brouwer, he was Dutch, and he said also, What are the criteria? Up to when can we accept something? And this is a big question. I mean, there are numbers from zero to one, but what is good? And of course the more modern notion is to ask, If there is unreliability—there is noise in the data—will that affect correlations, findings, etc.? That is another question. But we were also asking, What does it mean in terms of what people see?

So he designed a very interesting—just a, you know, off-hand experiment. He developed, or defined, Dutch terms for characters—for different kinds of characters or personalities. Now, these Dutch terms were complicated. There were no English equivalents, intentionally. The coders did not know anything Dutch. So, I mean, if they were accurate then there must be a relationship between what the researcher means by these terms and the characters. Well, it turns out that the reliability alpha was 0.44, so that—and I'm arguing—there must be something in these peculiar pronunciations—you know, when something is [guttural sound], you know. That there is something bad, or something good, or something warm, but this is not the basis of making inferences from the data to something else. That would preclude it. So 0.44 is definitely uninterpretable.

Now, this is an example that one should do probably more frequently, but it is also a big debate among statisticians. For example, Fleiss said at some point, Well, if it is 1, of course that is what we want. If it is 0.7, that is good, but if it is 0.2, that's not so good. But it's not just "not so good." It is totally misleading. So I think making experiments of that kind is important to validate the numbers that one gets. So there's lots of controversy in the literature and I'm constantly asked to make comments on something—or misconceptions.

For example, there is a Chinese scholar from Hong Kong who decided that the real issue is how difficult it is to code. Experientially, yes—if something is difficult to code, then it's likely to be chance or it's arbitrary. But then he said, Well, the difficulty is what we should measure, not the consequences of the difficulties. So when something is easy, let me give him one, well that's fine. But when something is difficult that should not be zero, it should be better. So he wants to change the standard, which in my version is simply randomness or noise in the data—full noise. He wants to change it into the difficulty. So we have to first measure how difficulty it is, and then measure based on the difficulty. If it's difficult and they are good, then it's—anyway. So he wrote the paper for the *Communication Yearbook*, and I was a reviewer—I was asked to review—I said, This is not publishable. This is irresponsible. Because difficulties is a challenge of the designer to do better, to define it better. We don't measure the bad qualities of a designer, rather you want to have the reliability of the data. So there's a fundamental, epistemological difference.

Now, this editor, he was kind of committed to publish that paper. But he said—actually to protect himself, I guess—Klaus, why don't you write a rebuttal? So I did.²¹ And I have to say I have never, in my life, written something that was really against something. I'm a constructivist, and I'm always interested in doing something new and justifying it, and trying it out, but to be against it was really not for me. But I did it, and this school is still going. And I had, in the meantime, several of the same school to continue that. But there is something fundamentally wrong in misconceiving reliability as the difficulty.

²¹ Klaus Krippendorff, "Commentary: A Dissenting View on So-Called Paradoxes of Reliability Coefficients," *Communication Yearbook* 36, no. 1 (2013): 481–99, <https://doi.org/10.1080/23808985.2013.11679143>.

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 this.

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.²² 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.²³ But this was Randy Harrison

²² 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>.

²³ Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

who encouraged me, and in 1970 I was elected to be the chair of the Information Systems Division. And I stayed, actually—I believe that was four years that I was 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, a *Systems Letter*—I mean, actually it was Information Systems. It was not just information theory, but it was also computationally the issue of systems that develop as a consequence of technology.

And so this division thrived, I think, quite a bit. I had a lot of support. Someone wrote and edited a newsletter, a *Systems Letter*. We were the only division that had that. I remember also, I designed a t-shirt. That was the first t-shirt [laughs] that any division had made. And I remember Ed [Edward L.] Fink, who succeeded me as the chair the Information Systems Division, he was very aggressive in selling it 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 me—and it was personal—there was another division that arose out of it and that is the Technology Division [Communication & Technology Division]. And it was unfortunate; it was not necessary, but nevertheless that's split in that way. 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 actually they organize a conference. That was in San Francisco. I decided that we should be a more academic orientation. Even though we called it a conference, but lots of people said, There's a convention. And I was very much against that. But, anyway, we had, for the first time, a topic. I asked, actually, several people to be on a committee to look at the future of communication. I realized that there are so many technological developments that change the nature of studying of communication. So the conference was called "Communication in Transition." And it was, as I said, the first ICA conference that had a name, a topic.

Since that time, always we had a topic. As a designer—well, one of the biggest tasks was actually to organize that conference. Now, we had an executive director, Bob [Robert] Cox, but to distribute the paper submissions into buckets of sessions fell to the elected future president. And I thought, That is a phenomenal task to deal with so many papers. So what I did is I wrote a computer program, and that was the first time that this was ever done. And the computer program looked into which division it comes from, who are the authors and co-authors—and distributed them into sessions so that the number of co-authors would be minimized. And in fact I managed to get every paper that had co-authors not [to] conflict with anyone else, with one exception, and that was Ev [Everett] Rogers. He was on everyone's committee [laughs], and so that was the only exception. But the result was, actually, that people were very happy that they could go to the sessions that didn't conflict. I think that was a major success—this

program. And later on others wanted to have this program, but it was written in Fortran IV, and it was not easily transferable or whatever. But now we have that, of course, much more mechanized. I don't know to what extent the same criteria are applying, but we had, actually, fewer divisions, so it was a little bit easier. Then I developed, actually, being a designer, a catalog, and the catalog had several innovations. One is a numbering system for all of the sessions. And the first one, the first number, digit, was 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 of the day, then came a period, and then came a room number. And the room number was actually correlated with divisions. So, also divisions, that people who were staying within a division, they could stay simply in the same room. So that was just a way of numbering the various sessions. And I also introduced, what had never been done before, who are the contributors. So there was, in the end, a list of names and which sessions they would be in. Now, this numbering system has somehow changed, but it is still there, and the idea to allow people to look for names and say, Where could I find them?, is still there. Another thing is, as I was saying, we had this conference theme, and the conference theme could apply to all divisions, and so as a consequence I asked every division to contribute something to the theme for what the future looks like, and so they got an additional session for that conference theme. And I think that was a pretty successful conference, that was—actually, this was because of San Francisco—it was the biggest one up to that moment. But this had something to do more with San Francisco than with the program. But we had a movie continuously showing avant-garde communication movies, and so there were lots of interesting developments.

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

KRIPPENDORFF: Well, I could say, then, the next year came, actually—what's her name now? I'll get her later, but she—oh, one of the things that I did at that time—actually I was the founder of the feminist study committee. I wanted to have a gender division or something. So there was one woman, she was a feminist, and I asked her [Rita Atwood?] to organize the divisional interest group for gender issues, and she did this very successfully. There were only two men in this meeting, this was someone—I forgot now his name—and me. But this then transformed into the Gender Communication [*sic*: Feminist Scholarship] Division. So that was one aspect.

The next president [Brenda Dervin] was actually trying to undo everything that I did. Which is very unfortunate, and she has since really lost much of contact with the ICA. That was in Hawaii and I was still deciding, or I could make the logo. And I have in fact, here, that is the logo [holds up booklet]. And I had lots of Chinese students, and I thought, We have to find a single character that represents all of it. And I learned from my Chinese students, you cannot use a single character—that is not possible. In Chinese you have at least two for an idea, if not more. But then I went with him through the Chinese dictionary, and I found one character—namely this one [points to booklet]—which had the quality—now, this is my Western, inadequate, I

would say, understanding of Chinese. But it [the Chinese character] had the ability to change something moveable. So, to come together, to be close, to meet, to join, unite, to mate, have intercourse, intimacy, a friend—this was all connected with this character. And so that became—in fact, I made again a t-shirt—and it became the symbol of the next conference. That was in 1985.

At that time, in the ICA, the outgoing president gives an address. Everyone credits me for that. I was the first one who gave an academic address. Other ones made some kind of a, How good we are as the ICA, and what we should do—whatever—but never academic. But I wrote, actually, a paper proposing five imperatives of what communication research should be.²⁴ And it was published later.²⁵ So that was the end of my presidency.

Actually, I should also mention, at that time, well the ICA was struggling with the word “international.” It was originally entirely an American association, and the international component was minimal. And there was a disagreement, I think, in the ICA. On the one side they said, Well, we have to get international people in there. On the other hand, I mean, I was on the side is that we should join an international federation and leave other associations their own identity. So I was actually convening in Hawaii, at the end of my presidency, a group of different kinds of associations—national associations, you know, Chinese, Japanese, whatever—and we asked whether that would be useful to make a federation that would simply allow people to exchange journals, exchange ideas, information about conferences. And that ended up with an International Federation of Communication Associations. And I, together with some Canadians, we decided we should not make it an American association. So it was registered in Canada, and we had several meetings at different kinds of associations. And it was actually pretty successful in providing newsletters of the various associations, allowing access, or publishing, in fact, the various journals, etc., etc. But after me someone else took over in Holland—in Germany—and then someone in Poland and then someone in Croatia, and now it has kind of fizzled out. And so it had a short life and the ICA has become increasingly international, which is fine. But this was another initiative that I pursued after my presidency in ICA.

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, I mean, actually my programming started in 1967 or something— ’66 or something. I mean, the last program I wrote was in 1973. And at that time programming was

²⁴ 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.

²⁵ 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.

pretty simple. It was Fortran IV, then Fortran H, and I have not advanced beyond that. But I have always had an interest in understanding algorithms. And so for me, I think, computation is a very important part of communication research. And I would say, increasingly: The whole notion of algorithmic forms of communication, starting with calling someone and having a machine answering you, and allowing you to come to some sort of an answer, to solving problems on the stock market, the fast exchanges within seconds responding to a change in prices, etc. So I have been always interested in that, but I'm not really involved anymore with actual programming. But I think this is important for communication researchers, I would say, is to have a sense of what programming does, what you can do and what you can't do, in order to look more carefully into what it does socially.

My interest now is more like the social consequences of these kind of phenomena. So I have not done any programming since '73 or something. And it had initially to do only with Alpha, and then with the issue of information theory—about which we will probably talk at another time. These are the kind of very practical issues of computation. And the other one is the ICA. So I think computation is a skill that one has to at least, in my opinion, have an inclination by actually writing something and not just talking.

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*.²⁶ 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] when I was a designer. And there was one guy named Horst Rittel, who became kind of my mentor in Ulm. And he introduced all kinds of strange conceptions—among others cybernetics and information theory. And I remember distinctly that we had an information department at Ulm. And that was actually informed by someone who was a philosopher in Stuttgart [Max Bense], and who had, actually, quite amazing ideas about information theory, but in general philosophical terms. And he said, for example, that—he wanted to understand art—he said, When new kind of art comes, it is challenging, it's rejected, but as it is duplicated it becomes increasingly accepted and at some point is taken for granted and is no longer interesting. And that was, kind of in a nutshell, his approach to information theory. Because that is the same thing also: The more redundancy emerges, the less it is informative. And he retired or went away, and at that time this Horst Rittel was hired. And I remember distinctly, he was a mathematician, and he gave a lecture in Ulm to the information department—they were all writers, art critics, and so on—and he gave a lecture on information theory proper, with probability theory. And I see him still writing on the blackboard. And

²⁶ Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

nobody could understand a thing. But he was actually an amazing character. He was adaptive. He could understand very quickly what was needed. But he left behind, to me, an opening to look at the world from a slightly different perspective, namely, What is plentiful? What is informative? etc., etc.

So, with this very superficial background I came to [Ross] Ashby in his cybernetics class [at the University of Illinois at Urbana-Champaign], and there he introduced us to information theory proper. And not just in [Claude] Shannon's sense, but also in a sense of the complexity that one has to cope with. The more information there is, the more difficult it is to sort things out. And so that was, I think, his approach. And that is all quantifiable or at least one can deal with it in quantifiable terms. My dissertation in content analysis included a chapter which is still, to me, conceptually, I think, a key—namely, that data that have to be made have to be informative about the phenomena that we want to study.²⁷ That means that the information that the data have, have to be carried through to the end, through the conclusions that one wants to draw about this.

So I think that is kind of fundamental. But it is not entirely Shannon. But it is based on the notion of Shannon, namely that information can be somehow measured, and that one has to see what happens in the process of analysis, where, usually, an analysis takes a lot of complexity, makes it simple in the direction of finding a communicable conclusion. In the process much of it is lost—complexity but also redundancy—hopefully also noise, which is more difficult. So that was kind of my approach in my dissertation. Actually I developed an information theory, a qualitative information theory, which could be nevertheless measured in bits—namely, how many bits are coming from the outside world, which you don't know, it's too complex—come into the data, and then are slowly transformed into conclusions.

So that is one aspect. But I also, at the University of Illinois—I don't know if you know that but, Wilbur Schramm was actually given—he came from [the University of] Iowa, where he wanted 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 that one needs now—radio, television, and other things. He was not successful there. He got an appointment in the University of Illinois Press, and was also given the possibility of developing a department of communication research [a Division of Communication, including the Institute for Communications Research (ICR)]. That is where I actually graduated from.

One of the things is that he [Schramm] was also enthused about information theory. Shannon wrote that information theory, or developed it, during the war—but wrote it in 1948. In 1949 Schramm published Shannon's information theory [*The Mathematical Theory of Communication*], together with a foreword or interpretation by Warren Weaver. It's also interesting how Weaver got into it: There was a biology lab in New York, and they have kind of an advanced scientific discussion group. And someone said, Information theory, that should

²⁷ 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.

have an implication of other sciences. And the guy, I forgot his name, talked to Weaver, and said, Why don't you report on that? And he was struggling through that, but he reported that and that was then the basis of that book by Shannon and Weaver, and published by Wilbur Schramm.

So Shannon was part of the discussion in the University of Illinois. But to me, and in fact lots of people said, It is a kind of a one-dimensional thing, going from a sender to a receiver. And it is in fact correct—nothing wrong with that: That a message that is sent is received in a different way. Noise interferes. One cannot easily, from the sender's point of view, see what is actually sent. And so these ambiguit[ies]—that is inherent in one-way communication. So, actually, we learned that from Ashby, through Ashby, and Ashby connected this more to the issue of complexity. That means he was interested in kind of what the brain does with complexity—the brain as an adaptive system to an uncertain environment. And so he was focusing not so much on the transmission issue but on how one can cope with complexity.

And he developed, actually, several measures, among others a measure of complexity—that means a multiplicity of interaction between different kinds of dimensions of information. And that could be easily calculated with. But it turned out to be wrong. Wrong, in the sense that it was odd, it sometimes became positive, sometimes negative. And that was disheartening and I had no good solution for that. Except at some later point I realized that it had to do with circularity. And this was again Ashby's influence, but he didn't make that connection. He talked about the Q-measure, and he did not do that. Now, that came, maybe my interest, or experience now with programming through loops, etc., etc. And I realized that the difficulties have to do with ignoring the feedback loops that can be constructed within the information theory.

So I developed, actually, an algorithm, to start out with, to get at the interactions. And I wrote a book that was again—SAGE asked me to write something on information theory, and I reviewed, of course, all the classical information theory including the feedback loop that will get us at the complexity. And this resulted, actually, in a whole, one could say, analytical framework for looking at complexity—how to compartmentalize different kinds of complexities into each component, the components that can be explained by theories, whatever. And this book provides that method.

That is another area that I computed—I used computer programs to do that. And actually someone in computer science at the University of Pennsylvania, who became a student of mine, he wrote a dissertation describing this also. And he did write a computer program but it was not portable. Then I hired, actually, someone, at the end of my capabilities, to write a more general program and he didn't succeed. So, the last one that I wrote is still valid. It still works, but it has been taken off by other people, namely a guy named [David A.?] Swick who looks at this complexity from that point of view. And he is analyzing also complexity, by decomposing it into various components, including mine.

You mentioned [George] Klir. Klir was a systems theorist, and he was—where was it?—anyway, Albany, I believe [*sic*: Binghamton University]. And he presented at some point a paper at the General Systems Society, in which he also decomposed things, but he decomposed it only linearly. And I said, There is something wrong with that. Of course, I had already a background with this, and so I challenged that, and developed what he was missing. And that was then published in a yearbook of the General Systems Society, the decomposition of complex systems, including feedback loops.²⁸

Actually, the last big paper I wrote on information theory was to follow up, actually, on something that Ashby, at some point, proposed or looked into. He asked the question, How complex is the world? And he said, at some point, Well, suppose we can take the whole mass of the Earth, make it into the most sophisticated computational hardware. How much can we compute? Now that's an interesting question. First of all, it's a highly theoretical question—but it is an important one. I mean, there are limits. He said there are limits to what we can compute, practically and materially. And so I wrote, actually, a paper developing that further and asking, What is the capacity of the Internet? How much information could possibly be made available on the Internet? And where are we now? It turned out that we are very small by comparison to what we can do. But what we can do is just a theoretical concept. It's actually two to the power of 100 bits—that's kind of a maximum. There is nothing more because this is the limit when all mass of the earth is turned into the most sophisticated computer technology possible. And there are some limits—[Werner] Heisenberg's uncertainty principle and several others that say, you know, what are the limits that one can observe, and what are the limits, etc. So that is what I was weaving into this paper that I wrote on the capacity of the Internet, etc., etc.²⁹ 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 there was a conference at Oxford, England, and there I presented, actually, these decomposition in terms of information theory. And there was a guy named [William] Grey Walter, a Britisher who had actually developed an automaton to find [its way] through mazes and so [on]. And he gave a paper, and he said, Ashby is as good as dead; he has a brain tumor. And I was just shocked because he was kind of my teacher. And there was another guy from Switzerland, named [Christof] Burckhardt. And he was also an Ashby student, that I didn't know before. So we decided we have to go to see Ashby. And so we took a train from Oxford to—where was it? Anyway, we went there and we wanted to see Ashby. We made an appointment by telephone, talked to his wife, and then as we came, the wife came out and

²⁸ 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.

²⁹ 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.

said, Look, you have to be very careful. He does not know that this is the end. He's diagnosed. We don't want to rock the boat, and so on and so on.

And it was disheartening to see a brilliant scholar who was my teacher in Illinois, and then retired. And then he got the brain tumor and it was fatal. So we didn't have very much time to talk with him, but I gave him my paper about information theory, which is his—you know, continuing his work. And he said, Thank you. It has to wait until I am better to study it. That was kind of it. And so we talked very simple talk. It was disheartening. But I was probably one of the last scholars that saw him together with his friend Burckhardt. And it was actually, for me, it was very good to find closure, to see someone—at the moment when he was no longer really the creative scholar that he always was, but to connect at least superficially. It was disheartening, but it was a good experience. And I was really glad that we decided to take that time and visit him. But this was actually, as I said, it was a paper that I presented about the decomposition issue which he introduced to us. It's a sad moment in the history of this. But, to me, I think it was an important closure.

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, when I came in 1964 I was actually not a professor—I was a research associate. Then I became whatever the next kind of stage was, and I taught three courses: One is *Content Analysis*, the other was *Models of Communication*, and *Cybernetics and Society*. The *Models of Communication* was actually, basically, cybernetics, because I thought, you know, the simple idea of mass communication—actually, I never was a mass communication person. I was always interested in relationships between people, between institutions, how they are made up. And so my *Models of Communication*, which many people took, was an introduction to communication theory, as well as also an introduction to cybernetics. And later on I decided to go towards the more social phenomena and decided—actually, already in 1965 I had this course on the books, *Cybernetics and Society*. So there I looked, actually, in 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 a course that many, many people took as an introduction to information, communication, and cybernetics at the Annenberg School. Because many of the courses that were initially offered, they had to do with media, with television, with writing, with graphics, etc. And there was very little in terms of a more general theory of communication, which this course introduced.

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—that came much later and I forgot exactly when—I think in 1968 or something—That could be there in the catalogs—then we started the PhD program. Although Gerbner was assigned, in the beginning, to develop a PhD program. But we didn't have the faculty to do that. Many of the teachers that we inherited—there were six of them—they didn't have a PhD, could therefore not be advisors of PhD dissertations. And these were the television guy [Paul Desard?]-who was a brilliant guy, came from New York—[Lou Glassman?], graphic artist, came also from New York. And he was then later on replaced by Samuel Maitin. But these were all not PhD kind of people—all writing. There was, first, someone that was replaced by Hiram Haydn. And we were not ready to have a PhD program. But then when we had, for example, [Charles Hovan?]-he had a PhD. He came from education. But then we had the sociologist Rolf Meyersohn—he had a PhD. So then we could start a PhD program.

But in terms of students or involvement I think my course was trying to generalize communication in many ways, and that was the *Models of Communication*. And I think this pushed the student to think differently about the media. I should also say, there was a dissatisfaction among students to say, We don't learn what we really need. Well that was a somewhat naive conception, because as an academic institution we could not have the latest technology that industry had. So we redefined the Annenberg School to say, We don't teach you to press buttons on the camera, but we can teach you principles: what it means to write something, to translate it into a medium, and then communicate it. So that was, I think, the shift in emphasis in the Annenberg School, and my courses were, actually, precisely trying to do that. So I had, actually, good resonance with a lot of students, and also including cybernetic notions, for example Jim [James] Taylor.

He was an early PhD student, but he started, actually, to teach in the television laboratory. So he was interested in organizational communication, which is part of what I was teaching in *Cybernetics and Society*. But he was also interested in the circularity. So, for example, we made experiments: What if you see yourself seeing yourself? So we had a camera that focused on the image itself, and then you are part of it, then you disappear or you become big—and all of these kinds of things. That was kind of the fun experiments that came from the notion of feedback. But that was more like fun. But Jim Taylor was actually someone who was interested in organizational communication. He was at some point hired by the museum to find ways of making the Philadelphia Museum [of Art] more attractive in the city. And he looked into the various media through which the museum communicates with the public, and what that all means. And so he developed a lot of things out of that course in *Cybernetics and Society*.

So I think the *Models of Communication* I taught for many many years. And that at some point I thought that there are other things that I have to do. But the *Cybernetics and Society* stayed alive for many years. Actually, I have to say, also, the *Content Analysis* seminar developed into a *Message Systems Analysis* seminar, as an addition, to look not just at content but also the

system of relationships between industry, sources, etc., etc. And that moved, also, into the issue of *Cybernetics and Society*. So that is very much connected. Then I developed, also, out of content analysis, another kind of approach, namely semantic analysis. And I looked at different kinds of meaning systems, anthropological approaches to studying different meanings—[Ward] Goodenough and lots of people that contributed, actually, different kinds of approaches. Actually right now Lisa Henderson is at the Annenberg School. She took my semantics course and mentioned it recently as having major influences on the way she approached things. So, I mean, there are lots of things left over of that teaching.

END OF SESSION THREE

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

Philadelphia, PA

Interviewed by Jefferson Pooley

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 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 1959 summer, and I bought two books at Blackwell, in the bookstore. And without knowing, they determined really my trajectory. One was [Ludwig] Wittgenstein's *Tractatus [Logico-Philosophicus]*, and the other one was Ross Ashby's *[An] Introduction to Cybernetics*. Because I vaguely knew his name, but [Wittgenstein] had the advantage of having German and English text, and my English was miserable, and therefore I thought that is a good way [laughs]—certainly one cannot really learn English from a philosopher.

However, when I came to the United States one of the incentives of coming to the University of Illinois [at Urbana-Champaign] was that Ashby happened to be there, and taught a one-year course on cybernetics. And I took that. And actually I was surprised. Well, first of all, the Institute for Communications Research [ICR] was very interdisciplinary, and my advisor, hearing about the course, about my intention to take it, he was very enthusiastic and got in fact other communication students also there. So I was the first but not the only one. We went to Ashby's *Introduction*, but in the meantime—that was written in 1956—in the meantime he had progressed in many ways.

But I think what are the basics of his course were, first of all, complex systems; second, the issue of circularity; and, third, that circular causal systems have a behavior that is not just linear. It converges, either to stability or it explodes. And he had a notion of not just exploding in a negative sense, which of course often happens, but that something new emerges—a new system emerges as a result. And so that was one dominant theme. And the other one was, I

think, perhaps even more consequential, is the issue of variety, as he called it, or diversity of variance or information. And so he was always interested in the number of choices or the alternatives that were buried in a system.

So he developed, actually, before he came to Urbana, a notion of the ultrastable system. His aim was, actually, to explain the adaptation of the human brain. And he mapped kind of two levels: On the one side, when one interacts with the environment one is automatically shifted, adjusts. But then one comes to a point where what he called the essential variables of the organisms are not met, and to avoid a breakdown, one has to shift the behavior. And he said, That's ultimately what a brain is doing, constantly shifting to a new alternative. And he called that ultrastability. And that was, actually, again related to the issue of, How much choices do we have to change? And so he proposed a law of requisite variety, suggesting that, well, in his terms, any regulator has to have at least as much variety as the disturbance it has to compensate. So there was again the issue of variety—was part of it.

And then he developed the notion of information theory—and I have to say, that was actually a generalization of [Claude] Shannon's Tenth Theorem, which dealt only with the issue of noise in channels and how one can compensate that. So, this, to me, and the law of requisite variety, is simply a generalization or one could even say a universalization of adaptive systems—systems that could respond to disturbances from the outside. But his notion of variety also entered a more general analysis of complex systems in terms of the variety they had available. So he developed a whole calculus, one could say, of information or entropy, and how one could look at large systems as to where the variety is, what it does with it, etc., etc.

And, actually, much of at least one branch of my interest in cybernetics was to expand that, and to write about it, and study, particularly, complex systems. At the same time I also got involved in the General Systems Society [*sic*: Society for General Systems Research]. And there was a guy named [George] Klir. I saw his paper, listened to his lecture, and he also had the idea of decomposing complex systems into smaller ones and asked, What are the kind of the dominating things? But I quickly realized that all of his components were linear components, and as a cybernetician I was looking for circular components. So I criticized him.

And I had to, of course, do something with that, and I developed, actually, a way of looking beyond—and, I have to say also, Shannon was strictly speaking linear, and he was criticized for that, rightly so. Although I think sometimes unjustified because not everyone really knew the implications of it. But he had this linear notion of a communicator and a receiver, and Ashby was interested in the larger system. But [Ashby] made the mistake when it came to complex interactions among at least three kind of components. He had a measure which he inherited from, I forgot now, someone else. And that didn't work, in my opinion. And I found out it didn't work because there were circular constructions behind it.

So I think one of the contributions I made to information theory was to capture the circularity of information flows within complex systems. And I think in 1980 [*sic*: 1986] I wrote a book on information theory which captured that notion, and to my surprise it is still being sold and

widely accepted.³⁰ But to me I think the idea of calculating this was very important, and I had fun with playing with that, no doubt. But I wrote several papers on these issues of circular flow of variance.

But let me go to Ashby again. I think one of the interesting things he demonstrated to us is that when one observes systems—and he designed some—and asked an observer to predict what will happen next, which would demonstrate their understanding of the system. There are systems that are so complex that you can't do that. So there is a limit to observation. And he said, But there is not—not that there is no limit but instead of observation one could also design a system, when one knows what is in there because one designed it, and then study the implication and look at whether it corresponds to what one wants to understand. Ashby was, I mean, very peculiarly designing things and then trying to understand them, and seeing whether it corresponds to what one wants to explain.

And of course his aim was, actually, coming from psychiatry originally, was the brain—the intelligence of the brain, what the brain does as an adaptive system. I myself think that this law is much more generally applicable, and also to society and larger discourses. But that is a later issue. The point is that there is a limitation to observation and prediction. And that's one thing he plowed into us by, for example, exposing us to a complex system, asking us to predict, and this naturally failed. And then explaining the mechanism that allows the machine to do something that, while being deterministic, but allows us to go beyond observation.

So in fact this kind of attitude is uniquely cybernetic—that one is not just describing or modeling the observations, rather than building something that one can then explore. It has other limitations but that's not the issue right now. The point was, there is a difference between observability and constructability or designability. And with this in mind, actually, I have to say, Gregory Bateson recognized that Ashby's epistemology—and Ashby never used the word epistemology—but Ashby's epistemology was entirely new and had something to do with the evolution of epistemology. And he said Ashby's explanations are negative, which is true—that means you can say what it doesn't do but not what it will do. And [Bateson] linked that to the issue of evolution, and so he celebrated Ashby's epistemology, and saying this is a totally new kind of thing and is translating Darwin's evolutionary theory from biological kind of evolution to knowledge, to epistemology. [Bateson] was one of the few people that recognized and published this widely.

When I came to the Annenberg School in 1964, besides *Content Analysis*, I taught one course on *Models of Communication*. And the description of *Models of Communication* was, actually, introducing students to the cybernetic alternatives and different kinds of models, and how one can describe them, and what one can learn from that. As a follow-up to, kind of as second semester, I taught a course on *Cybernetics and Society* in which I tried to get these principles

³⁰ Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

applied to large social systems. For example, issues of information overload, information flows in small groups, in large groups, in social organizations, etc., etc.

And I think this course was very instrumental for a lot of people. I had excellent students that carried cybernetics in other areas. I don't want to name them right now. But this was, to me, I think actually elaborating on Ashby in the direction of social sciences. I also joined the American Society for Cybernetics [ASC] at the time when it was founded—I don't know whether it was the year afterwards. Anyway. The very first of several annual conferences were in Gaithersburg [Maryland] and I went there. And I remember three things that was exciting or interesting. One is I had been among cyberneticians in Urbana. I knew some of the literature, all the other people. But there were some—all of the cyberneticians were there. That was one exciting experience.

The other thing is it was during the Vietnam War and there were people that were upset with a lot of things. And I remember people shouting and walking out in part because, or at least one person, because cybernetics was funded, or among others, by the CIA and other kind of governmental organizations in the fear that the Russians are actually far quicker developing cybernetics than in the United States. And that was a correct fear. There were in fact some Russians at this conference. So that was the second one, the kind of politics behind the cybernetics—not that I understood it fully because I never, even now, I really don't know to what extent the government was involved. But I know some of the conferences were sponsored by the U.S. government.

And the third important influence, or what I recall, was the keynote speaker, which was Margaret Mead. And Margaret Mead, she was part of that Joshua [*sic*: Josiah] Macy, Jr. Foundation [Conferences] on Cybernetics, I believe from 1946 to '53 or something. And she reviewed what happened there. And she described among others that she was so excited that she, I mean, even didn't notice that she lost a tooth while just being stunned with what happened. And she described for us the excitement of discovering something new. And then she was somewhat critical of Norbert Wiener, who was a mathematician, and as a mathematician he was interested in formal systems. And formal systems, when you materialize them, end up with causal systems or with mechanisms. And so she criticized him—not making it a big point, but if you read between the lines, it was very clear that she said this is the wrong way to go, that it limits cybernetics.

And then she said, you know, cybernetics is being implemented everywhere, an increasingly computerized phenomenon. And I would like to say in 1967 we are far not where we are now. But she recognized that many of the processes, international processes, trading issues, Cold War, have something to do, or at least their end was the use of computers, and we do not know what they do. And so she said we have to understand the implications of cybernetics, not just doing things, computerizing mechanisms, but the social implications. And now, being an anthropologist, that was of course natural to look at the cultural implications of cybernetics.

And she said that we should really re-focus cybernetics, recognize that cybernetics is a language. It's not the things, the cybernetic mechanism. And I should say that at the same time there were lots of histories of cybernetics, and the histories of cybernetics were actually largely looking at the antiquity—when the first kind of circular control systems were built, like water clocks, etc., or oil lamps that automatically regulated. And that is all correct—or the steam engine, which had a feedback in it, the regulator, the Watt steam engine. But she said, We have to look at the consequences of that.

And then she said, actually, we have to have a cybernetics of cybernetics. That means where the cybernetics is applied to the practices of cybernetics, and that includes the cyberneticians in the object of cybernetics. That, to me, was a major shift. Now I have to say I listened to the address and I was stunned. I can't say that I fully understood the implications. But this came later, however—that came with Margaret Mead. So that was, I think, to me also a milestone, being first a member of this association. And I was at that time more like a student and listening. But that shifted, also, my thinking in terms of the courses that I was teaching. And that was in 1967.

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. And I presented, actually, then a paper, an analysis of communication theories, in particular [Harold] Lasswell's, who said that communication is 'who says what to whom with what effects.' And then he said that each of these things,—'who' is a different kind of exploration. It's the analyst of the sources. 'Says what' is content analysis. 'To whom' is receiver or audience research. 'With what effect' is other things.

And I thought that is just—to parcel communication out in these separate components with separate methodologies is precisely avoiding the systemic, and the dynamic, consequences of communication in society. So I wrote this paper, still very mathematical, trying to approach or find a way of collecting data for these connections that would not be separated into content and whatever. And that I presented, and then I published it in 1970.³¹ And actually, I have to say I had difficulties: I submitted it to the *Journal of Communication* and there were several reviewers that said, It's no good. And then the editor said, I'm overruling the reviewers and I'll publish it. And it got an award for the best paper in 1970, from the ICA. And so these were kind of the struggles of—the difficulties of—making cybernetics, understanding—and actually I don't think I mentioned even the word cybernetics in this paper. But the ideas of it, looking at larger systems, were a major focus of my attention.

³¹ Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, https://repository.upenn.edu/asc_papers/273/.

Q: Well, you know, that 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, that was when I was strongly working with information theory, basically. And I presented a paper on algorithms, of how one could look at complex systems, and that was in Oxford, in England. And there came a presenter—maybe I should say one thing beforehand. 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 University of Illinois the Biological Computer Laboratory, which was headed by Heinz von Foerster, became in disfavor, one could say. And it was in the process of being disbanded, and so it never became anything—nobody was replacing Ashby.

But I was an Ashby student. And there was [William] Grey Walter, a British cybernetician who is famous for his mouse, an intelligent mouse that finds its way through a maze and recalls the paths that didn't work. And he said, basically, Ashby is as good as dead. He has cancer and there's nothing to be expected. And I was standing next to someone who was from Switzerland [Christof Burckhardt] and he was also an Ashby student. And we talked about how sad that is. And then we decided we should visit Ashby in Birmingham. And we took a train and went there. And the sad part was we were greeted by his wife in front of the house, and she said, Look, he is going to die. It's terminal, he doesn't know, and I don't want you to remind him on it. But you can certainly talk to him and he would certainly like that.

But now that was the sad part. And I gave him my paper proudly, because it was building on his own work. And he just looked at it and said, I will look at it a little later. But he was, already, I wouldn't say incoherent, but he started talking about his experiences when he was a soldier a long time ago. So he was not really there anymore. And it was sad. And I think the two of us were probably the last ones, at least the last cyberneticians, that saw him. And it was important for me to say, in a way, goodbye. But it was a sad moment for me personally and maybe cybernetics in general. Because Ashby continued even after—well, let me say it differently. He maintained a log of all his thoughts during his whole career. And this log later on was kind of recovered by one of his relatives. And I think it is being published. But it is very complicated because he related all of his explorations to his personal struggles. For example, in the military where he didn't fit, and all of these kinds of things. But he continued, and he told us that he had a small room, a closet actually, where he was working, continued to work on cybernetics. Well nothing ever came of it anymore. But, anyway, all the promises of his 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: Well, the annual conferences of the American Society for Cybernetics had ceased, stopped. And cybernetics, in my opinion, was a bit in disarray. There was one, the

president, [Roy] Hermann was his name, and he was in Washington. And the American Society for Cybernetics had shrunk almost to the board of members that met socially in Washington. And I thought, That is just unfair. And at the same time in Philadelphia I had actually developed a relationship with others that had similar interests. Someone at Drexel, someone at the Decision Sciences [department of the Wharton School at the University of Pennsylvania], etc., etc. So there was, actually, a momentum of interest in cybernetics, and we had occasional meetings and talking about our own approach.

So I went to the American Society for Cybernetics and said I wanted to organize a conference. And they were very skeptical. But then I managed—the Annenberg School provided the rooms and the American Society for Cybernetics was supposed to publicize it and see to it that lots of people came. The Annenberg School was for free, so we had very little costs. We asked a small fee for the program and some of the recordings, but it was actually from the American Society for Cybernetics. They were not really capable, very much, of getting people. However, I managed to get a group of people from the University of Pennsylvania that were interested together, and we met frequently and decided what we should do and whom we should invite. And we invited mainly people that had something to do with social phenomena.

The Department of Decision Sciences was particularly instrumental. They decided on business decisions, political decisions. And so this was a conference—we had about thirty-five presenters, from numerous areas talking about knowledge creation, about the effects of circular relationship in international relationships, the issue of war and peace. And so there are a lot of fascinating areas. And that was the conference.

But just a minor thing: The American Society for Cybernetics was supposed to hire someone who recorded that. And we wanted to publish the results. And for whatever reason this guy Hermann said that the papers, don't record them—only the discussion afterwards. And I have the tape, and the tape is such that—applause, next speaker. That's all I had. And so it was amazing, actually, and extremely disappointing. And so from that to make a book was very difficult.

Luckily someone had actually taped Anatol Rapoport's presentation. He didn't want to write it, so I wrote it from what he said and he made minor additions. But the book was published. Again, I wrote to many publishers, I forgot now how many, but there was one, Gordon and Breach, that wanted to publish it. And it has become now, I wouldn't say a standard book, but it was a snapshot at this time.³² The issue of control in society was of course central, but control not in the sense of forcing people to do things not to do, but in order to develop equivalences, balances, etc., and imbalances, understanding imbalances that come.

So that was, I think, an important book. For me it was also important to put that on the map. I should like to say after that, the ASC attracted, for whatever reason—actually I know the reasons—family therapists. So we had several conferences in which family therapists came.

³² Klaus Krippendorff, ed., *Communication and Control in Society* (New York: Gordon and Breach, 1979).

Now that has to do with Gregory Bateson. Gregory Bateson was dissatisfied with individual therapy and said that most of the mental illnesses are the results, actually, of communication. And in order to understand the communication that drives people into some sort of insanity—actually he wrote a paper on pathologies of communication. And the pathologies of communications are precisely those that bring people into untenable positions.

For example, when the mother says, I love you, and I punish you for your own good. Now that creates a disconnect that a child has difficulty to reconcile. And so [Bateson] made, I think, a wrong speculation that schizophrenia could come from that. It has been debunked. But the point is, actually, that in many families, families usually decide who is the so-called, later called, identified patient—someone who was a little bit more crazy or doesn't fit, and he is being told by the family to be the black sheep or the mentally ill. And then that person is given to individual therapy and the therapist tries to "cure" that.

And Bateson, I think, was correct to say this is not the way to look at therapy. We should look at the communication. There is dysfunctional communication in the family that produces, actually, mental illnesses. And so he created or proposed the notion of family therapy. In Philadelphia there was actually, in the Child Guidance Clinic, a big group of family therapists. And many of them came also to the ICA [ASC?] and presented papers. But there was also Humberto Maturana, who was embraced by many family therapists, I think, wrongly, because he was a kind of biological determinist, and didn't really get into the issue of language.

But let me then shift back to what I remember that you wanted me to talk about. At this conference there was a big dinner. And I invited Heinz von Foerster to give the keynote address. And he said about Humberto Maturana, who always said, Everything said is said by an observer. And he—Maturana—had the idea, Yes, there is always an observer who observes reality—and language, the role of language, we have to tie it to the observations by the observer and not to anything objective.

That made a lot of sense, but then he—Heinz von Foerster—said, I take this to be theorem number one. And I offer a theorem number two: Anything said is said to an observer. Now that is an important shift, because in some sense he recognized that language is a social phenomenon, that it is not just the observer describes, but it is a social phenomenon. And then he did something that, later on I think, that was a bit unfortunate. He said, We have to have a cybernetics of cybernetics. And, you know, with Bertrand Russell's theorem that you have to have a logically different type when you talk about 'of,' he said that cybernetics of cybernetics is second-order cybernetics.

And he did not mention in one word Margaret Mead. And that was disappointing, because he was actually rephrasing, and in my opinion limiting, the ideas that Margaret Mead espoused and talked about, or created, second-order cybernetics. Second-order cybernetics was immediately, as a concept, embraced. And family therapists, others talked always about second-order family therapy, etc., etc. And the point was, actually—and that part I do agree with—and it's what goes back to Ashby, who always said the experimenter is part of the system

that he's experimenting with. And so the observer has to be part of the system observed. You cannot separate them.

And the family therapist has to be part of the conversations that a family engages in. In fact, when a family therapist invites a family in at a session, he interacts with them not as a member of the family but as now a different system with the family plus therapist. And the therapist reacts in the context of that family dynamics and plays a role in it. So that was, I think, very important to recognize. But in my opinion it psychologized cybernetics, in the sense that now, in fact, many people still talk about second-order science—suggesting, again I agree, that one should not separate the scientist from the object that he is describing. But the scientist is not a lone observer. And in fact, one can say, the second theorem of Heinz von Foerster said—it's said to someone else—but he was not developing that in any way. And many people didn't develop that in any way.

So it is now being increasingly talked about, second-order science, recognizing that any scientist comes from somewhere, has perceptions that he brings from literature or whatever, and what he describes is in part their own history. Second-order cybernetics, going back also to Humberto Maturana—all of them say the aim of science is to find descriptions and to describe something. And that gets into the issue of representational language. An observer, as an observer, is always an outsider, like a spectator. And the aim of describing buys into a representational notion of language.

That's precisely what, actually, Ashby was against—not explicitly but implicitly. And what Margaret Mead pointed out is not the way to go—that description ties the observer to what exists and not what could happen. So that was, is still, my objection. And so I think many of my subsequent papers actually criticized the effort to describe things as they are, and rather look at what I call social consequences.

There is, for example, one paper—I don't remember when I wrote this—but I was invited in a dialogue on comparative communication theory. And so people compared communication theory. Well, I did this too. But I was kind of surprised, there was never any consideration as to what the communicators about which one theorized, what they were thinking of it. So I said—this was kind of tongue-in-cheek criticism, by saying, When you do this then you are actually conducting intellectual imperialism. By saying that those who compare the communication theorists, they are superior to those who communicate. And I, at that time, said the alternative is conversation—that the communication theorist should engage in conversation with those they are theorizing.³³

There's another element. And I forgot now which paper I wrote this. But I said, All social theories stem from observations—whatever. But they are also published, and they are published and made widely available. Now there are some effects: What about if those who are

³³ 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/.

theorizing, theorized, if they hear what they are theorized about? They have the choice of saying, This is not me, and change their behavior in opposition to the theory. But more often it is so because the great authority of scientists, many of them say, Oh that—I didn't know that. Now I'm buying into these conceptions. And so that is the result of a self-fulfilling prophecy or self-validating theories.

And to me, again, that is a cybernetics of theorizing that people don't—theorists still think they are observer describing objectively, but it has the consequence of either amplifying what they theorize, or opposing, or, in the worst case, being irrelevant. When it's irrelevant then it stays within the academic discourse. But if you want to have an impact or if you want to even enlighten someone as to what happens and so on, then one has to look at the consequences. And that—this paper—was I think also critical.

At this point I have to say I didn't really know the implications of conversation. Let me also say there are three people, three students, who nudged me in that direction very early on. One was John [Henry] Clippinger [Jr.], and he wrote actually a dissertation on conversation. The other one was Chuck [Charles] Goodwin, and he became, in fact, a major conversation analyst. And I remember, also, I had difficulties getting him to the Annenberg School because conversation was not mass communication and it had no connection. But in this particular case I got Bill [William] Labov from the linguistics [department] involved, and Ward Goodenough, who was in anthropology. And so he [Goodwin] passed with flying colors, and he wrote his dissertation, in a book. And he is teaching linguistics now in UCLA. So, I mean, conversation was not really my central theme, but it was in the making. And the students in some ways had nudged me in that direction.

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: Well, there is also my presidential address in ICA [International Communication Association]. And I was president up to, well, 1984—'83, '84 [*sic*: 1984–1985]—and this is always a long involvement. You become elected, then you become the vice president, and then you become president. And at the last conference that is already organized by the successor, then you give the major address. And I am attributed to be the first to give an academic lecture. And I remember, also, I couldn't complete it because the previous speaker had been taking too

much time and I was not given enough. But at that time I proposed several imperatives.³⁴ I would like to see if I could get them [consults book]. These imperatives are still not really yet in the issues of conversation, but in the beginning of constructivism.

And in part, actually, I got some ideas from Heinz von Foerster. And the first imperative is what I call the aesthetic imperative, and that is, Construct your own reality to see. There comes again Ashby, who not explicitly but he said, We can only see and understand what we do. So, constructing a reality that you can see. If you don't construct it you don't understand that. So that was the aesthetic imperative. Then the next one was the empirical imperative, and this is: Invent as many alternative constructions as you can, and enact them to experience the constraints on their validity—Ashby. It's saying, basically, Develop a system and see what it cannot do.

That goes back into Gregory Bateson's acknowledgement that Ashby's epistemology is actually defining what cannot be understood or not happen, as opposed to what can happen. So, this is my empirical imperative: Just construct as many and then see what works. To me that shifted the whole emphasis not just on describing, having one version of reality and test it. It goes also in the direction of Gregory Bateson who at some point said, There's never a single description. There's always more than one. And so that, I think, has to be recognized. I think the tradition in the sciences in general is to have one theory and not alternatives. When there are two different theories of the same phenomenon, Oh, that's pretty bad. Like, for example, in physics, the wave and the particle theories and their struggle to have a united theory. And Gregory Bateson said, This is the part of the health of any epistemology to tolerate alternative constructions. So, that is what I was proposing as an empirical one.

Then the next one is this self-referential imperative, and that is: Include yourself as a constituent of your own constructions, and recognize that you are doing the constructions. And I elaborated on that with graphics—that one cannot really separate the observer from the observed. And that comes from Ashby and also from Margaret Mead and, if you want, also second-order cybernetics from Heinz von Foerster.

Then comes an ethical imperative and that is: Grant others that occur in your constructions at least the same capabilities that you employ in constructing them. See, that was another version of being against this academic imperialism by saying, You are the superior observers and we describe these people as causal mechanisms. And that is still going on. I mean, if you think, for example, much of the quantitative research that reduces the subjects to response mechanisms—that there is a stimulus and there is a response. And that's what they are theorizing. And I'm saying, If the theory of communication would be correct, it should apply to you too. If you are a response mechanism, I wonder if you could come up with these [laughs].

³⁴ 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.

So the issue of superiority is something that is to be questioned. And then that goes into the whole kind of recursiveness—that you have to describe yourself as part of a recursive system.

And then came the social imperative. And the social imperative reads: When communicating, preserve or open new possibilities for others. That means, if you describe other people, like in marketing research—you know, responding to something to increase sales and so on—if you describe them as responding mechanisms to improve one's sales, then you reduce them to some sort of robots or whatever. And to me a good communication theory should open the possibilities that were not there before. And that's an ethical issue. And that's, to me—as again, this goes in numerous directions. "Preserve or open the possibilities to others." And that is the social imperative.

I presented that in ICA, and actually I'm very proud of that. But it still does go not directly into the issue of conversation. So, I think it came later, the issue of conversation. That was in my terminology, but I didn't really develop that further. And then—I forgot now the year—but then I started talking about, What is conversation? Is there such a thing as authentic conversation? And I, actually, wrote about this and asked, What can we say about authentic conversation as opposed to constrained conversation? And I had several propositions: One, which is a very cybernetic notion, is it's self-organizing. It is a closed system of people that converse. There are always speakers and listeners, and you can't just have monologue and everyone listens. That's not a conversation. So there have to be people, have to be senders and receivers, if you want, or speakers and addressees. It is self-organizing—that whatever happens a conversation develops its own rules. A conversation doesn't repeat itself. Everything that's said is in a sense new.

Now one can, of course, refer back and use the same vocabulary, no doubt, but it's not a repetitive phenomena. It's producing something new. And it has to be genuine by not letting someone managing it or someone from the outside saying, This is what you should talk about. And I asked, What is the motivations—do we have, should we have, in fact, a motivation? And I found one cannot do that. The whole motivation is actually to stay in the conversation—to the idea that—let me say it so: In practice, of course, every communication comes to a physical end. Someone dies. Someone has to go do other things. But the point, actually, of a good conversation, is that it can be continuing in principle. If it ends in violence that cannot be a good conversation. If it ends by saying we have solved the problem, that was not a conversation. So the whole purpose of conversation is to stay in conversation.

And so I have developed, I think, nine different propositions. I don't want to describe them. But then I asked—this is of course an ideal—how often do we have that? And in fact, we don't have it that often. But to me the interesting part is that when we recognize that something is different from that ideal, we recognize the difference. So it is always, in my opinion, always implicit in when we judge. When someone, for example, talks too much—well, against what? Because there is the ideal, is actually that everyone has the same possibility of contributing. It doesn't mean, necessarily, that everyone has to talk equal amounts. But as long as you have the

possibility of contributing you can be part of the conversation. If someone talks too much, that is measured against that concept of equal dialogue or dialogical participation.

If someone comes—and I'm thinking of now of, for example, faculty meetings, of committees, and so on—and someone said, Well, I cannot make a decision here, I have to go back to my department. Well, he is not a participant in the conversation. He has the whole department behind him and has to get permission to say whatever was supposed to be said in this conversation. So the reference to outside is not part of the ideal conversation. And when you do this, in fact, my experiences with that is that you cannot come to conclusions. If you constantly have representatives of others who cannot take the position of others but have to refer back to them.

Now the phenomena is that you speak for others. When, for example, in politics nowadays, you know, "all Americans"—and then you come a statement. How can we say this? Well, if one believes it, if the people believe it, then they are duped into a conception that is outside conversation. We don't know what "all Americans" mean. Or: "I'm for the poor people." One can be for the poor people, but who are they if they don't have a voice in conversations? So, there are a lot of kind of practices, communication practices, that deviate from the ideal of conversation.

There came a very important influence for me, and that is actually accountability. This came, actually, from John Shotter. I spent a year with him in New Hampshire. And he wrote about accountability, saying that one can always ask someone, Why do you say that? Why do you do this? And one gets an answer. And that's part of a good conversation—that when something goes odd, let's say, then you can ask, Why or what do you mean? And there are three kinds—four, actually, four kinds of responses, roughly. One is explanation. When you ask someone, What do you mean? Then, in a way, not that there is an accusation, but there is a recognition that what the person says is not really resonating with the receiver, and the receiver is eager to find out and wants to understand. So you ask for explanations.

The other one is excuses. Now, an excuse for being late—then you deny the agency of the speaker and blame someone else, something else. That is, again, coming from the outside. And you can't do anything about it. You were forced, someone gave you command to do so. These are excuses. Then comes justifications, and justifications are the opposites. Justifications acknowledge the agency of the speaker but say there is some virtue to that and you should rethink it. And the next—the last one—is an apology. Now, an apology means, actually, that you acknowledge the agency—you acknowledge that it was something hurtful or something bad, and you promise never to do this again. Now, that is a good apology. I think in a political discourse, nowadays, that is rarely done. You apologize for other people being hurt. Now that means you don't [laughs] take responsibility for having done so. You express some sort of a feeling that other people shouldn't be hurt. But he is right to say what it does now.

But that gelled with another area which, to me, was very important. C. Wright Mills wrote, at some point [1956], 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. But he said, What is power in the power elite? And he went to board meetings and participated, and he came up with the idea—he didn't call it accountability, but he said motivation. In every board meeting, when there's something proposed there is motivation provided—these are, kind of, justifications. And so he preceded, in a way, the whole notion of accountability, by saying, That is where power is exercised—that someone convinces a group of the positive things that had been done and they comply. The point is not to exert power in one direction. The point is actually to accept the account given by that man.

And, actually, John Shotter made an interesting statement, saying, We never speak without having the possibility of being held accountable in mind. And we know already that if someone asks us, Explain, we can explain. Justify it, we can justify it. That is, basically, built in, in any sort of communication. And to me that was really, also, eye-opening. And it had to do with understanding what happens in conversations and what is, actually, among conversation analysts called repair. Unfortunately, most conversational analysts think repair is only looking for explanations—that one is not clear in what one said. And that is kind of unsatisfactory, because it's far more broadly speaking.

So I wrote this one paper in which I both elaborated on the criteria of what is a genuine conversation. And I said that all genuine conversations can easily erode into something else—by, for example, accepting someone as an authority: He's an authority, I'm not. Introducing some imbalance, if you want. Considering that someone brings data from the outside and they have to be accepted. That is not a conversation. And also, for example, like we are sitting here. You are asking me questions. That's an unequal relationship. If I give a talk, I'm assigned to be the speaker, and I'm sitting, you know, singly there and presenting a monologue, even though I might just ask for questions. That's quite possible, and it's often formally acknowledged to do that. But it's not a conversation.

So there are lots of phenomena that move away from conversation. And then there is the whole notion of formal communication. But then comes an important category that came for me, is discourse. Discourse is actually a constrained conversation. Speaking as a physicist to other physicists limits the vocabulary that is to be used, limits the kind of arguments that are acceptable. And they are not shared by, let's say, mathematicians, who have a different type of discourse. They also engage in interactions—not in conversations per se, maybe sometimes, but it is more constrained. So I was just fascinated by the notion of discourse. And I developed several papers on what is a discourse—what happens in a discourse.

And, again, I couldn't help taking cybernetics into that discussion. And, I think, if you go to the literature and talk about discourse, they say, Oh, what's talked and written. And to me that is very unsatisfactory, because I think that's what I described there from a cybernetic point of view. Discourses have consequences. Discourses, and I now increasingly say, constructs artifacts—every discourse constructs artifacts. Physics designs theories. Scientists propose theories of sorts. Engineers produce objects that can be produced. So every discourse has, actually, its own artifacts that it controls, recreates, protects. And that is kind of the material

dimension. And even though I think physicists would not say that we deal with material. But they describe, in fact, an experiment—a physical experiment that is being made. And their description is connected with what they have been doing.

There is one example that I like to use, you know, speaking about physics. And I have to say I, at some point when I was in England I presented something like this. There came a physicist that said, You have it all wrong. Well, I don't think I have it all wrong. For example, the famous Hadron accelerator [Large Hadron Collider]: That is the biggest experimental set-up in the world. It is a circular structure underneath, crosses the boundary between Switzerland and France. It cost millions of dollars. It has attracted many, many scientists to support it, many workers employed. It's very, very costly. Why has it been built? Because someone had a theory of the so-called God Particle, and this God Particle doesn't exist on Earth. It's theorized that it has only a lifespan of a few seconds. And physicists wanted to show that.

Now, from my point of view, for all reasonable things, this is an artifact—particularly if you don't find it on Earth. And it is theory-driven or discourse-driven. And it costs an enormous amount of money. But it's an artifact, and much of physics is an artifact. And if you look at the history of certain kinds of theories—I'm using in my course, actually, Ludwik Fleck, who preceded Thomas Kuhn. And I think Thomas Kuhn stole many of the ideas from Ludwik Fleck. And who described, actually, the history of syphilis from a scientific point of view—or not necessarily a scientific—I don't even know what the scientific point of view is. But he started, actually, with an astrological explanation. I'm not an astrologer, I can't say. But there was a configuration, and one configuration has to do with sex and the other one with fighting or something. And this came together, and in 1400-something syphilis was created. Now if you have this kind of construction then, of course, you can't do anything about—stars, they do this all by themselves.

Then came the religious kind of explanation, saying it's a sin, a carnal sin, because it's related to sex. But then came the issue of, what can we do about it? And the first "knowledge" was to find reliefs. And that was a pharmaceutical kind of response. What could reduce the pain? Well, the answer was, actually, mercury cream. Now, I don't know what that is. I don't know to what that does, its promises. But that was not the end of it. And then, at some point, came all kinds of explanations, one of which was very traditional—prejudices, if you want, in biology. It must be blood, bad blood. Now blood is historically always an explanation. For example, nobility have blue blood. And so bad blood—it was a natural thing to do. And then biologists started to look into the blood and found, indeed, some differences between people that had syphilis and those who had not.

But then came also the 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 are ahead of Germans in syphilis research, and he picked out a guy named [August] Wassermann. And Wassermann was a biologist. And he said, Here is lots of money. Just do something to outwit the French. Now, you see, it is political [laughs]. And Wassermann found something that was, later on, found wrong. But he developed a whole area in biology to look at serums in blood.

Well, and so it went on, and I don't want to get into—Oh, I might want to mention one thing. Namely, Ludwik Fleck was Jewish. He was working in Poland. He had difficulties getting it published, this book, because in Vienna there was the positivists that say, Science is a question of objectivity. The idea that something develops historically, culturally, is not part of science. It would not be published. He had difficulties publishing. But he published it in 1934, by a small Swiss publisher. Only seven hundred copies were published in the world. But then, in 1939, the Nazis came to Poland and arrested him, putting him in a concentration camp. But they realized he's a scientist. He has to be special treatment. So they asked him, Would you be able to develop something against syphilis for Aryans, because you are Jewish, but can you do it for Aryans?

Now this is, of course [laughs], from our current conceptions, ridiculous. But he said, Yes. And he survived the war. After the war, he became head of a biology department in Poland again, and then he retired to Israel and died. But this is the social dimension—construction. And I argue that every discourse has some material focus and constructs it, and it's constantly reconstructed. And I'm telling my students, What you think is—everyone thinks, We are now so much better. We know everything. In the past it was inferior and primitive. And it was different. But in a hundred years they'll say, well, we didn't understand what we're doing. So I think that there is a constant change, a reconstruction—that discourse is alive to the extent it reconstructs its own objects.

And there is another element. I mentioned earlier that the traditional focus in this course is only on the written text or talk. But there's always a community. And a community of scientists or physicists—they are the ones who are the housekeepers of the discourse. A discourse does not exist in a vacuum. It is spoken, enacted, preserved, deliberated by a community of discursive practitioners. And any discourse also defines its own boundaries. There is, for example, in physics, you know what is a physicist. And you have to have, maybe, a PhD, or you're a trained technician, whatever. But you have to comply with what physics demands. And you are certified to do so. Medical discourse. You have to be a doctor in order to practice. And the medical discourse practice community decides who is and who is not. It's not decided by the legal system or by politics. Every discourse designs their own membership. And then, drawing the boundary means also to say what they are not—and justifying the boundary in view of what they contribute to others.

So that was, in a nutshell, my interest in discourse and asking myself, What happens with the conversation once a conversation is eroding into formal communication with rules and monologue and references to other things? And then comes a discourse in which that is very strongly organized. Then, I think, is another further reduction. And that is what I am saying into computation. Now a discourse—oh, one of the elements that I didn't mention, but it's very important—that every discourse institutionalizes its recurrent practices. That means if you do it again and again you develop a methodology. You write about it. That's it. And when you become a student in the social sciences you have to acquire statistical arguments. This is very much organized and institutionalized that when you use this methodology you are demonstrating that you are part of the discourse.

And the discourse normatively spells out what is an institutionalized practice and what is not. But institutionalized practices somehow eliminates, actually, human agency. That means statistics—you can learn it and it doesn't matter who uses it. It's there, it's a mechanism. And, I think, all discourses have a tendency, more or less, but to erode further into computation. Now, again, if you think of, for example, statistics. When I started very early on, I remember I was working with a psychologist who could do factor analysis. And he had a desk calculator. And he was the one who just went for hours to compute his factor analysis. And nobody else could. But now nobody does this. It is mechanized. You get a software and you get the results. But that is the recurrent practices, which are standardized and institutionalized. They can be turned into algorithm.

But now going much more—not just within a particular discourse, every discourse does this. For example, physics has regular ways of instruments to measure certain things, that everyone knows what they measure and employ it, etc. And in behavioral sciences, what is an experiment: You have to know the rules and so on. But in society more generally anything that is clearly institutionalizable can ultimately be turned into algorithms. For example, bank tellers. Now, a bank teller is actually a human being and you interact with him. You say what you want from which account, in what kind of money, currency you want—whatever. There is a conversation, but the conversation is really not essential for the issue of banking. You can actually extract from that the recurrent practices, and you design a MAC machine, a money access machine. That is mechanization of the institutionalized practices of having access to your account and getting the money out.

Airplane reservations. Well, you know, a long time ago you went to an agent, and the agent, with telephone calls and so on. And that was very impractical because the airline never knew how many people were coming, and they had to tabulate that and then decide whether they should use a big airplane or small airplane. It's now totally mechanized, and you go to the internet and you put your things in there, and you pay and then it's registered. And you get your confirmation and that you take to the airport.

Or even any communication with institutions, like the city hall. You have a question, you get an answering machine and you go to the binary thing. And very rarely, or I would like to talk to a real person. And that's often very difficult, if not impossible. So now we are moving very much in the direction of mechanizing, algorithmizing, social practices. And coming back to Margaret Mead, she did not live to see these in these details, but she recognized that even, you know, foreign policy decisions—they became already mechanized, and we don't know the consequences. That's what she said. And therefore it's very important to me, is not to study language by itself, or communication separate from making—through a content analysis, if you want. But to ask, What are the consequences? And in the case of this kind of continuum that I depicted between ideal conversation eroding into struggles, if you want—rules intruding and becoming real full discourses, and ultimately the institutionalizing of recurrent practices becoming mechanized. That is a process in which communication is playing a role. And cybernetics is the basis of that.

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? Well, speaking of doctoral students, as I as saying, Charles Goodwin had very great difficulties. He had to have three members of the faculty from Annenberg. They didn't like it. And the fact that I managed to get others in from the outside on it, that were powerful and respected, etc., that made him go through. Similarly, I had another student, Mariaelena Bartesaghi, who was, actually, to me, also very interesting. She wanted to understand what happened in therapy. And she worked with a family therapist at the Child Guidance Clinic. And she wanted to write a dissertation about that. And so she went to them saying, Can I observe? And they said, Well, you can observe but you have to pay back. I mean, not literally. So she was given the privilege of being behind the one-way mirror with a therapist, observing therapists, observing the ongoing therapy in families. And what she had to do, free of charge, is to have exit interviews with the patients.

And the psychologists were interested, actually, Will they come back? That was the only purpose. And she had the benefit of getting these dual views of what the therapists—or triple view [laughs], one could say—what she observed happened in the therapy, what the therapist behind the one-way mirror said is happening, and what the patients were in fact reporting. And she wrote the dissertation on that and was also badly received. Someone—and I don't want to name the person—said, You are not a therapist. You are not a psychologist. You cannot write about this. But she didn't write about the psychology. She wrote about conversations, or interactions, in which one of the important features was, first of all, if someone goes to therapy, that group, family, or person needs to have some sort of a belief that the therapist can help. There is an attribution of authority.

But the therapist has to establish their authority by imposing—Mariaelena Bartesaghi—imposing a psychological view that may not have fitted the description of the patients. And always making it so that, or explaining it in such a way, that the therapist could “help” them. So that means imposing a psychological theory that was beneficial for the institutionalization of therapy. And so she wrote about this, and it was difficult to get it through in Annenberg.

But now coming to that course that you mentioned. At some point I thought I should emphasize the making of things. And, again, I'm not always relating it to Ashby, but in retrospect he was the one who said that at least one way in cybernetics is design something, study it because you know how it was, and then you see whether it matches what you want to have explained. So,

thinking about earlier what I said about discourse, I think that is what is being done, in some ways—that every discourse designs artifacts, whether these are theories, models, or solutions to problems. Cities, if you want, bridges, and then the explore—what they did wrong. Whether they did something wrong. And this is why this one empirical imperative is important: Design as many as possible and see just what are the limits of what you can do. And that is strictly Ashby or, later on, Gregory Bateson recognizing this.

So I wanted to teach a course on that, which actually derived out of the *Cybernetics and Society* [course]. And I wanted to make that more general, the social construction of reality. There's also an important book, [by] [Peter] Berger and [Thomas] Luckmann, who described, well, the social construction of realities [*The Social Construction of Reality*, 1966]. But he was kind of limited to the social realities, which is fine. He described, for example, how people categorize each other and how the categories get stuck. And even though there is always an originator of the categories, they are soon forgotten and the categories develop their own existence. And so this was, in a nutshell, Berger and Luckmann.

But I thought that it's much more general. And so I proposed this course. And this was—you mentioned—actually the year after George Gerbner had stepped down as dean. And I proposed it in the faculty—it had to be voted on. And Gerbner wrote a two-page opposition. And he could not get it, or was opposed to it, by saying that this is just one way of looking at it. And that's true. It's one way of looking at it. But he was actually, implicitly, more interested in his own way of looking at it as, kind of, media being dominated by industry and ruining public discourse and all these types of things. That was not—he couldn't handle it.

Luckily, it was approved and I taught it since. And I'm increasingly fascinated and embrace many more topics in my class. And it's a more open discussion of several kinds of things, and my students bring their own concerns into it. And I have certain standard topics. For example, issues of discourse is very important to me, so is physical reality, how it's constructed, and then racism, sexism, how that is coming about. And I'm using there, for example—I'm very influenced, actually—I didn't mention it earlier—[by] Gregory Bateson's notion of information. Now he said at some point, information is a difference that makes a difference. Now that seems to be a very simple formula, but it is, I think, an important one. He, in a nutshell, said information theory in which the observer defines the elements—the characters, if you want—that are either information-bearing or not. But, ultimately, not everything makes a difference.

As an example, at that time he used a chalk, and he said, I can break this chalk in two pieces. Now this is difference. And I can break in many differences. There are millions of differences. But not everyone makes a difference. So the difference that it makes has something to do with human beings. The difference that exists, so he says, may have not. And one has to balance that. Now I was critical about that for another reason, namely, I think, what he didn't do—which came also out of cybernetics—is the issue of distinction. In cybernetics there was, actually, laws of form. Spencer Brown, who developed a logic of distinctions, and he said everything has to be distinguished, and a distinction is an act. It's not there, it's an act. Francisco Varela wrote about this and others. And I think when Gregory Bateson broke the

chalk, that is an act. And it is an act that created the differences. So what makes the difference is actually that you act on it, that you make a distinction.

And this one paper that I think you mentioned earlier, about the epistemology of communication, did this precisely, saying we're making distinctions, thereby creating differences.³⁵ And now we have to understand, What is the difference between them? And then we construct relationships between them, and that is what we then report: correlations, etc. 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. Not every culture makes this distinction by color.

Actually I have a lot of Chinese students and I have fun saying, Are you yellow [laughs]? Or had American students, Are you red-skinned? Now, I mean, it is crazy when you think of it. But you make this distinction. And I think you have to be accountable for making a distinction. But scientists do not. And then you describe the differences and you always find a correlation—for example, intelligence. There is this famous bell curve and it suggests that people that are black have a lower intelligence, and the intelligence is inherited because the next generation of blacks have also a lower intelligence. Now once you have this finding, you made a distinction, you find correlations. You actually make this a bigger complex. It's no longer just merely color of the skin but it is intelligence, etc., etc. And then you impose that, or you can publish it.

And, for example, one of my students, actually in *Content Analysis*, she made a study of the effect of knowing that someone is black when being hired. And she used the same kind of job qualifications vita, but in one case it was a white person and in one case it was a black person. The black person was rarely ever hired, because one knows that they are lower intelligence and whatever. And there are so many racial prejudices come out of the distinctions that someone makes, and you find always correlations. But in the case of intelligence, interestingly, because it ignores completely the fact that black people do not have the educational opportunities, that most of the intelligence tests are developed for suburban white students who have a long history of knowing to write, etc., etc. So that there is a major bias, but it is described in objective terms, and has then these consequences. So I say, always, you know, distinctions are made actively, reported as differences, when enacted has the consequence of increasing the differences that are initially observed.

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

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

³⁵ Klaus Krippendorff, "An Epistemological Foundation for Communication," *Journal of Communication* 34, no. 3 (1984): 21–36, https://repository.upenn.edu/asc_papers/538.

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

Philadelphia, PA

Interviewed by Jefferson Pooley

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 the more I re-read sometimes—and I rarely ever read something that I wrote a long time ago—but my thesis was very instrumental, and I see many, many kernels of statements that I didn't really—couldn't back up—but they stayed with me. And sometimes I'm wondering what [laughs] one really learns in the meantime. But this is of course just, kind of, a theoretical question. No, but Ulm, as I said previously, it was an avant-garde school. It dealt with topics that nobody else dealt with. At some point I had to write this thesis, and they had two kinds of theses. One had to make a practical work—and I should have brought a picture of what I did. I did a motor grader. I don't know if you know that: They plow the roads.

And then I made the theoretical one.³⁶ And that was interesting because I was a bit in opposition to some of the things that was done in Ulm, mainly because I had an engineering background. I think they took me because I had an engineering background, and they thought that I would make, probably, major contributions. In the end I did, but for the wrong reasons. So I decided to look at, in this thesis, the issue of the meaning of artifacts, and the first step was—actually there was a guy named, the director actually, Tomás Maldonado. He was a South

³⁶ 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/.

American, originally a painter, and he had actually introduced the notion of semiotics into the curriculum. But he was a very traditional semiotician, and for him signs have referents, and the issue of, for example, visual communication means you make reference to what you want to tell by depict, etc.

So I went to him to say, I'd like to write about the meaning of objects, artifacts, designs. And he said, Klaus, that is a categorical mistake. Objects are referred to but they don't have any meaning. So I decided I will not work with him. And I worked with another person who was always my mentor, that is Horst Rittel, who later on became also in Berkeley [University of California] a professor.

Anyway, so my basic idea was that one has to look for the artifacts as to what they tell the users to do. I thought that many of the interpretations of objects have to do with pointing what one could do. For example, a switch invites, if you want, switching, and one may not know what it does, but that is the meaning of objects—it's not a reference, it is what one can do. I think that was an important shift which I think this Tomás Maldonado, as much as I liked him, but he didn't understand. This was a different kind of semiotics that had to do with actions—the actions that are invited by the interpretation of objects.

Now Ulm was dedicated, I would say, to so-called functional objects, and a function means that you know what the use of it is. A screwdriver is something that you tighten the screw, or a heater is something only to heat, etc., etc. That was something very good about it. In fact, it was in opposition to the kind of design that made ornaments, everything beautiful, and cover over everything. And I dug out one object from Ulm and that is a heater [presents heater]. This was kind of a minimalist object, it shows the so-called honesty to show what the technology in fact does. You see the plastic, you see the the fan going, and you can turn it, and you can put it on the wall and it blows in your face, whatever. And that's a typical example of Ulm design.

That was also another element which often Ulm is made fun of, and that is the preference for gray. Not to make it, you know, crying colors or whatever, just gray, decent, maybe black, maybe white. So that's a really a good example of functional design. Well, coming as an engineer, I said the engineers can—and I could do that—they can do the technical parts very well. I mean, like making this motor run, and even finding out the shapes of this—this is all engineering. But design should not get sidetracked by the technological parts. It should focus on other things. So I wrote a kind of a very challenging article in our student paper at the end, before I left, and I said, Engineers design functional objects that have a technological function and work. Designers should focus on the communication of artifacts.

Now that's a little bit, you know, tongue-in-cheek, but the point is actually what I condensed in this one word, was that one should look for the meaning of things. For example, here is a switch. You know how to switch that. There's a number and there is "hi-low," etc., etc. And you might not know that you can turn it, but maybe at some point you do it and then you know. The point is, actually: That is what designers should do to make it usable from an individual point of view.

I had this note—my whole thesis was about the symbolic and sign characteristics of artifacts. But this article that I said, you know, Designers should deal with communication of artifacts—that was actually a naive way, at that time, to say it, but it directed me ultimately to study communication. When I came to the United States—I mentioned that previously—I couldn't find a good place, but communication was really, to me, a key issue, and I found, in the end, a communication department and we did talk about that.

The last connection with design I had, actually, when I was looking for a university, among others, at the University of Illinois, I went to the design department. And that was, one could say, not comparable with Ulm. Ulm was such open and new ideas. This was really a traditional design department. And that's the last time I talked to the designer for a long, long time. But I mentioned the word cybernetics, and then he pulled out a paper saying, We have here, in University of Illinois, someone who has written this paper. And that was Heinz von Foerster, and he told me that he has a Biological Computer Laboratory. So, I went to him and then he told me that [Ross] Ashby was teaching a course in cybernetics at the University of Illinois, and that made the decision.

So, that's where I went to study communication and cybernetics. But between 1964, when I came to the Annenberg School and, ultimately, 1984, when I wrote a key article on design, I didn't really do very much in design, except for one thing, namely Reinhart Butter, with whom I studied. He was two years behind me in Ulm—he had become a professor at Ohio State University. And the way it went, a long time ago, you had to buy a thesis. So he bought my thesis in Ulm for the reproduction costs, and he had it always with him, and he said, Klaus, you have to come give a lecture.

So I went several times to Ohio State and gave a lecture. In fact, at some point I made a long workshop. And at that time I was interested in content analysis and so I combined that. In fact, I wrote a computer program to analyze the responses to people, and so I asked the students to look at particular objects, and asked them to rate them, to say what they find from it. And that was then analyzed. So that was kind of also an early thing. But the point is, Reinhart Butter, he was keeping me in touch with the rest of the design community. I have to say also—it must have been in 1967 or something, that was one year before Ulm closed—I got a letter from Ulm, the director, and they wanted to invite me to become a professor. But then at that moment I was already on the track here.

That's another element where I still was a designer, namely Horst Rittel, who was a supervisor of my dissertation. And I have to say the Ulm professors were relatively young. I didn't even know he was just three years older than me. So he became later a professor at Berkeley, in the architecture department, and there was a need for a chair for the design department. So he asked me, Would you come? I said, I don't know whether I can contribute. But I decided to go. And I made a presentation and I remember I made, I think, a good presentation about the design of toys, and not making toys so that children can really learn something, as opposed to plastic things that are imaginary or something.

It was, well, reasonably well-received, and I remember the interview—I was the one actually to ask all the faculty members who they are. And it turned out to be they were semi-artists. They were getting high on making individual mugs, so that every individual has a different kind of mug, and so on. So when I thought that is backward, very backward. And I was not hired. But who was hired, an aerial photographer who had nothing to do with design. But so in fact, it was probably a good thing. I was already on track in the Annenberg School. I don't know if I would have actually taken it. But I did it in part for Horst Rittel, who invited me. And who knows, you know.

But then in 1984, in fact, I know, April 1, a few days before, Reinhart Butter had a sabbatical, and he said, Klaus, we have to write this down. I'm now asked to edit a journal of innovation magazine for the IDSA, Industrial Designers Society of America, *Innovation* magazine. And I [Butter] would like to put together—actually, I had worked with him a little bit earlier—to just put several, what one could say, have something to do with a focus on meaning. So I wrote an article with him, and he came to Philadelphia and, you know, you can't just write an article in two days. But anyway, I did whatever I could.

I know April 1st because I made a big joke. I had an open car at that time, and I gave him a ride to the airport. And I had this paper in there, and I told him afterwards, It was flown away [laughs]. And he believed it, but it was April 1st. Anyway, so in May this issue came about, and that proposed basically what I had said in Ulm, maybe with better knowledge of communication. And I said, Well, one has to see the designer as someone who communicates, well, with the user, and providing something that they can use.³⁷

I no longer believe it in this simplicity, but still that was it. The designers association—the IDSA—found that so fascinating that the very same year, in fact in August, they invited all the designers that could come, in the United States, to a big conference, a week conference, at the Cranbrook Academy of Art. And so there were the four people—that means Reinhart Butter, John Rheinfrank—who was already also a professor at Ohio State, and with whom I had a good connection. And he was kind of a systems person and was also very good. And myself, and there was another one. So we organized a one-week seminar on product semantics and that caught on. I mean, it was really like a wildfire going on.

The next year we were invited to Philips in Eindhoven. Philips is a worldwide company and has designers all over the world. And so they got all the designers to come to a one-week workshop in Eindhoven, in their headquarters in the Netherlands, and to introduce, basically, this new perspective to not look at technology so much, but look at the meaning of technology. To me that was interesting, because many of them were accomplished designers, and they were forced to come to the headquarters. And there came these three or four people—and I was not

³⁷ 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.

really a designer per se, I had done a lot of things and more thinking. And Reinhart Butter was a designer, clearly, and John Rheinfrank, he was working with a consulting firm in Ohio State.

So we asked them to do certain things, practical projects and with the criteria that we set. And so they were at first resistant, but then, in the end, they were so excited and a lot of interesting things came out. There was, for example, one guy—and I have to say, this is the kind of thing that I was actually promoting—he was trying to develop a radio, a portable radio. And then he was also a drummer, and he said, You know with drums, maybe we have to replace the drummer, the drums, with a loud speaker. And so they were first this way and then this way, and there came out a very famous design, the Roller Radio. On the side, that had lots of things to do with that, but the head of the design department at Philips, he took this design, the Roller Radio, and wanted to have it produced by Philips. So he went the traditional way, going to the marketing department, and they made a study and saying, It can't be sold.

So then he had a friend in Italy, a producer, and he saw that and he said, I'm making a thousand. So he went past the marketing—marketing is so traditional and so conservative, and not understanding, really, innovation or new things. And so he bypassed it and it became a major success. So that is another thing that we were kind of on the edge of something really new. Following that there were meetings in Amsterdam of the IDSA. There I met someone from Finland, Yrjö [Sotamaa], who was the head of the design school—now called Aalto University—in Helsinki. And he said, You have to come to Finland. So we began to go to Finland, and there were three conferences in sequence about product semantics. And so a lot of people from Europe came there, and it was quite remarkable.

At the same time or shortly after, intermittent, we had a conference in Germany, one in Taiwan, one in Japan—that was alone by myself. But in Taiwan I was with Reinhart Butter. It had spread very quickly. But there is something odd about the design community. As designers you are always interested in innovation, and anything that is from yesterday is no longer good. And so I read, for example, something like in 1990, they said, Product semantics—that was a decade ago. But I kept going, and so I wrote, actually, many papers.

Maybe I'll mention one where I actually got into issues of discourse, and I had not written about discourse before. But one of the things that puzzled me is the profession of design. Historically designers were simply professionals that were hired by a company, put in an office and make things beautiful. That had both changed, as well as also it's something I just think is too demeaning. And I observed that many disciplines try to capture design as their own—there is, for example, marketing says, The only purpose of design is to increase sales. And economists say, Well, it has to be value added. And the ergonomics people say, Well, it has to be beautiful. Everyone had his own reasons for saying design is actually us.

So I realized there is a struggle going on, and the struggle is really conceptual. But it had to do with discourse. For example, marketing saying, The only purpose of design is to increase sales. Well, that is because they want to use their own terminology, and redefined design in their own terms. So I wrote a paper, saying, One has to properly design the design discourse, and making

the design discourse an autonomous entity that other people have to respect, but not being colonized.³⁸ And I'll note that I developed the notion of the colonization of one discourse of another. And I again, on the side, of course, I lived through this in the communication area where, for example, in the Annenberg School, very early on, when communication was introduced as a school.

One has to also say [Walter] Annenberg had a really kind of very practical aim to educate his journalists for his *Inquirer*. The university never wanted to do this, but then came sociology [and] said, Communication, we do this much better; we have a sociology of knowledge. And then psychologists came and said, As social psychologists, this is a social psychology problem. So I saw there are a lot of parallels between the different kinds of discourses—in fact the making the distinction between discourses and how they struggle to identify themselves, how they incorporate other areas. Currently is also interesting. Like psychology is being colonized by cognitive science, and very soon maybe psychology disappears, because cognitive scientists have been “better computer models,” etc.

I don't want to get too deep in that, but the point is, actually, I was doing this, my exploration, in the one paper that I wrote for Helsinki. I looked into the conflict of discourses. But again, these designers didn't really understand that. In fact, the editor of that volume didn't have a place for my paper, and I had in the end other kind of things, you know [laughs]. But it turned out to be, I think, very critical for me. And then I decided, or many people said, You have to write a book. And so then I wrote *The Semantic Turn: New Foundation for Design*, and summarized many of the papers that I had written before.³⁹

And maybe I should just mention a few things. One is that I said, When we deal with artifacts, all artifacts are designed or made, not necessarily by designers, but by people who do something new. So there are these four levels. One is the meaning of objects for a user. And then I looked very carefully and said, Well, what does a user know of the artifacts they are using? And it's actually very minimal. If you think what you know when driving a car, you don't really know how the engine works, the piston, and how the gas is converted into pressure. You have no clue. And so I came to the notion that, actually, we know only the interfaces. Maybe we should just forget about the artifact as a physical object, rather than the interfaces that human beings have. And that goes back again to [the] Ulm thesis—that you ask the question, What can you push? What are the consequences?

And so the notion of an interface generalized from the notion of computer interfaces, where there is kind of a very well defined—and there is interface design and so on. But I think every object in and of itself is known to people only through the interfaces. I said designers ought to understand the interfaces. And the interfaces is not physical, the interface is interactive, and it has to do with meaning, as the recognition of it, etc., etc. So, I think that was one important

³⁸ 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.

³⁹ Klaus Krippendorff, *The Semantic Turn: A New Foundation for Design* (Boca Raton, FL: Taylor & Francis, 2005).

part—on one level, the artifacts in use. And one kind of principle, and I said, always, human beings never react to physical qualities of things, but to what they mean to them. By physical qualities I mean the kind of things that physics can measure. It's always translated into what people understand—meaning.

So that was kind of the first level, and then the second one was actually artifacts in language. And in this book I showed many examples where the language that you use is critical in how you perceive things. This goes back to in Ulm when I was in the Institute for Visual Perception. I even then recognized that how we talk about things has very much to do with how we see things. And so there are lots of examples, for example, of artifacts. There was in Germany a small car, at some point, and this was immediately ridiculed publicly and it didn't go anywhere. It was actually driving quite well, but it was a small car, and it was called the breadbox because it was kind of odd. But the point is, actually, you can really kill products by using language, and we know this now in politics, you know, that language is critical. And then it's a question of what kind of language—this comes the issue of methodology for designers. Ethnography is, for example, a discipline that deals specifically with the language of ordinary people, and so I wrote a paper that ethnography is important for designers to understand the language of the users they can use, to make use of the artifacts.

So that was kind of the language level—and I could say much more about this issue of categorization, for example. There are good theories of categorization. There are super categories, and categories and subcategories. Like, for example, a chair is a very general one and then there come the subcategories—children chair, leisurely chair or whatever, but they're all under the category of chair. And then there is, on top of this, furniture. Furniture has no clear image, and that definition, in fact, of the basic category is that you can draw it or that you have a sense, like a chair. Anyway, so I connected that with these kind of things, and I think that had also something to do again with my teaching at the Annenberg School. I taught a course on semantics, on *Semantics of Communication*, in which we dealt with this not from the point of view of design, but from the point of view of perception, of political categorizations, etc.

So that is the second level, and the third one—I said that one has to look at design in the process of its making. And there I developed—actually, I can say I stole that concept from management science. I knew Russell Ackoff well. I had a secondary appointment in the Social System Sciences [Department] with Russell Ackoff, and they talked often about stakeholders. And I looked at, What are the people that designers design for? And the traditional notion is that's a user, and they focus only on the user. But that seemed to be so limited. Actually, Reinhart Butter at some point said, Well, you know, I don't know about this user concept. He was working for Caterpillar, and I was actually consulting with that. Well, Caterpillar is a big company, a construction company. The drivers of that equipment have absolutely no say in what kind of equipment is being bought by a corporation. So the client, if you want, is the corporation and not the user. And then, parents buy toys for children, the children don't buy it, you know.

So the notion of user is very questionable, when one looks more carefully. In order for something to be, let's say, bought and sold by parents for children, there is a whole system of institutionalized actors. And I called them stakeholders—borrowed from the management science—and a network of stakeholders. I developed this notion of network of stakeholders where each stakeholder has a very different kind of stake in the process of getting something done. For example, someone who finances a project, only he's not interested really in how it looks, or something, as long as there are short-term and long-term benefits, and then he would invest. And so that's one thing.

Engineers—they are interested, actually, in finding the great solution to a problem. Then they are proud of it and pass it on. So I think there is something to the understanding that process—and then as the designer you ought to understand the network of stakeholders in which something is happening. So I taught that. For example, I was in Sweden many times, and I was, I think, very successful in introducing that concept. There was, for example, one interesting one about a museum exhibition. Now, who is the user here? Of course, we can say the visitor of a museum, but they never talk to the visitors, they talk to the director. And the director had a board meeting. And there were people working in it. And so they outlined the whole network of people that are interested, and then they asked what is their interest, what is their interest?—and how that cannot connect. What can we do in order to get our design through?

There's one other thing I was challenging designers with that, namely the issue of motivation. How do you motivate stakeholders to be part of it? Now, again, in the industrial area designers were employed with the company, and when they had a design the CEO said, This is what we do, write the specification, and everyone follows. But this is, nowadays, no longer possible because there are so many different agencies, institutions involved in this. So I said, What is really motivating everyone is design. Design is, I think, to me, a very human activity. It happens everywhere. When you design your—rearrange your furniture—when you buy your clothes—you have aspects of design. You want to know how you look and you select something in order to look good or whatever. So engineers are proud to have found a fantastic solution, and then they push it. Or finance people, they say, Well, I can make a lot of money and I've made some good here—

So everyone has different interests, but the key is actually that one allows these stakeholders to make a contribution that they can say, I made this contribution. So I said, Design needs to be delegated. Now, that was very controversial because then they say, Well, what about if we delegated—what's going on with the profession? And I said, Well, you can elevate the design profession by delegating that which you either don't know or don't want to do. So, for example, I said other poets, avant-garde poets, they are designers of language. They are far ahead of everyone else, so that it trickles down after some time. People are recognized first, they are not understood, and later on they are recognized and become celebrated poets. The same kind of thing can happen with design. That one has to observe how something gets produced and allow everyone to make a contribution. And I said, in some qualifications, Well, it's not just delegating your profession, but at least give enough space for other people to make their contributions. So that's another kind of challenge that I posed.

Then the fourth level is actually what I call the ecology of artifacts. That an artifact that you design as a designer is an object, ultimately—I mean, like going through this process—Oh, now, maybe I should also say, before I get to that, in this network of stakeholders an artifact takes simply different kinds of shapes. What leaves the design department is maybe sketches, drawings of something, video or something. Then it comes to the engineer, and then he figures out with equations, and figures out—so each time it has a totally different shape. When it comes to the salesperson, it's an object, maybe boxed in or something, on a shelf. Again, a different kind of thing. In the house it's different, etc.

Then comes afterwards—which I didn't mention but—after use there is usually so many different kinds of things that happen. I was recently talking—I didn't know that, but ships, when they are decommissioned—what one does with that? Someone has to do something with it. It turns out, which I didn't know, that in Bangladesh they make a business: An old ship that is no longer useful or something, they take it to Bangladesh and that they take it apart. But it has to be done. Then after doing that—I mean, the ship is probably an extreme example, but all objects end up either in the garbage, or recycling, or repairing. There are a lot of industries interested in when an object is retired from use. So this network is really something that the designer has to cope with. That is a big challenge because it's not just making a beautiful drawing—that's what I have been always arguing—but making a drawing in such a way that every stakeholder sees a possibility, contributes something, and then gets it through the chain.

There's also another—maybe I should mention that—about the change of the nature of the artifact. I was in the Philadelphia Museum [of Art] in the armament part—I don't know if you have been there—with my children and they were interested in that. So there was one armament, and that was actually black, and it said, From the Count of Brunswick—give a date. There's a tiny description, It was worn when he got married. Now, I'm German. There is a mythology of a knight of Brunswick who came at the night and made things right when there was injustice. So he was a mythical figure, and I said to myself, Nobody in this whole museum knows that. Not that I—that's knowledge—but it is a myth.

Then I decided to write a paper on it and I ask myself what happened to this armament? It must have been produced at some point. That is a very complicated thing, lots of craftsmen hammering on this thing, and then probably the count came often saying, Is it done? Does it fit? I mean, it's a different kind of artifact. And he was wearing it, actually, supposed to be, at his wedding. It's not just the wedding, but there were tournaments and he had to ride horses and push the opponent over now. He was a count, so probably nobody wanted—they are putting him off the horse. But in other cases an armament is really a protection against being killed, so there is lots of fear in it. That's all in this armament. So now, when you look at how this armament moves through various kinds of what I then call bricolage, from the making, to the ceremony, to war.

And what happens after this? Well, in Germany, many, many high houses, they kept the armament of the ancestors and said, This is from my grandfather or whatever. So it became a family heirloom, a demonstration of the importance of something. But then there came the guy

who actually collected this. They were collectors. He and the Hertz family were competitors, and they were traveling to Europe and buying off these various kinds of things. And there is, in fact, in the museum a picture of his living room with all the armaments in it. Now these armaments became something totally different, not related to family rather than to value or whatever—exhibition of wealth and so on. Now it's in the museum, and there's just this label, and you never know what it is [laughs]. So, I'm saying that all artifacts, they go actually through these various phases—if they are durable. With decay lots of things disappear, but when they are durable, they go through various kind of phases, and that one has to look into.

Now comes to the ecology, which is part of that, actually. I think when an artifact is bought and put in use it's always done in connection with other things. If you buy tableware you have to have a table, you have to have plates, etc., and that goes together. They are always connections made. The connections are not made by the designers, but they are made by users or whatever. Or when you buy a new drive for your computer you plug it in and it connects it to something else, or the computer is connected with the cloud, ultimately, on other systems. So there is a lot of connections that are outside the immediate control of designers, but they are made by ordinary users or institutions, whatever. So they form an ecology, and the ecology is to me interesting. I mean, in ecology we talk about competition, and survival, and it's interesting because the artifacts really don't compete with other artifacts. They compete with the attention of users.

So when you see something new then you think, Should I replace the old one? It is you who make the decision, not the artifact. So there is a different kind of ecology that is operating, and that is creating, if you want, culture. So I always said designers are not just producing objects. Designers are introducing something in the culture, and, I would say, designers keep culture viable. The viability—ultimately, that is really what design is all about, and not a particular type of product. In another paper, to do the same kind of thing, I said there is a whole trajectory of design problems or design focus, concerns. Traditionally, it was products. In 1950 the products became actually goods, the issue of selling.

Then, afterwards, I think it was the interface issue, because it became so complicated. Like a computer, nobody knows what's in it. A user doesn't know what's in it. Even engineers, most of them don't know. They know more than maybe a user, but they don't know everything. So there are always different kinds of interfaces, but then is the issue of designing multi-user systems like a library, like an election campaign, with lots of people, and you have to put them together so that something is happening. Like the Internet—the Internet is a technology, but the technology is really very minimal. The code of the Internet you can state in a few words. But the connection with many, many people, that makes a difference, that makes the Internet. Then there's the issue of projects, that you have to get people together to solve particular type of problems. And then there's the issue of what I said earlier: discourse, designing the discourse. So that, to me, is kind of the highest level—I don't know—but it is also an important design project, to design the discourse to cope with certain kinds of things.

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.⁴⁰ So at the 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 an interplay. But a small correction—the 1984 paper didn't talk about stakeholders. Well, I think in 1984 I was a little bit naive and I talked actually about communication. That was really my main focus and not too much the other levels—that came later. But there's one important thing, when you mentioned the connection with cybernetics, and that's the epistemology. Cybernetics has a lot of contributors, but there's one that—my mentor is Ross Ashby, and he developed a set, in fact, a definition of cybernetics is the study of all possible systems which is informed when some of them cannot be built, cannot be found in nature, or when you interact with them it's impossible. Now, that is a negative kind of argument, and the positive one, the study of all possible systems, allowed one to not just fix on what is, but what is possible.

That to me was, I think, a very key, one could say philosophical, approach that seemed to be very plausible. Actually, Gregory Bateson focused on that, and he said, citing Ashby, this is a new kind of epistemology. It is exactly like Darwin's evolution, but on the level of epistemology, of knowledge. Darwin dealt only with organisms but this has to do with knowledge, or with information, whatever. So I think that gelled very much with that. The notion of affordance is, to me, also very critical in design—that certain objects allow you to do things, and there are numerous—to Ashby, infinite number—ways of using something—but not all. And one ought to, according to Ashby, be aware of what one cannot do.

Now in design the notion of affordance comes actually—the word affordance even comes from [James J.] Gibson, who was a perception theorist, and he had actually an ecology of perception. He was not dealing actually with the negative parts, unfortunately, but he said, We see not objects, but ability to use them. So when we say we see a chair, we see sit-ability. When we see steps we see step-on-ability. And so he turned it all into the issue of actions—that means what we see is the potential of action. And that goes back to my earlier—that was gelling with my thesis in Ulm. But it also connected me in design and cybernetics. So cybernetics, to me, was

⁴⁰ Klaus Krippendorff, "An Epistemological Foundation for Communication," *Journal of Communication* 34, no. 3 (1984): 21–36, https://repository.upenn.edu/asc_papers/538.

both informative of design, but also the other way around—that one has to look at how one can create possibilities. That, to me, I think was a key element of the mutual interaction between the two.

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 that, I remember. In 1984 I was president of the ICA [International Communication Association] and I had the privilege, as all presidents do, to give a big speech.⁴¹ I think I must have mentioned it earlier. But that is very much influenced by the combination of both design and cybernetics. I don’t think I can get them all together, but I proposed several principles. The first one is an aesthetical imperative and say, Create the world to see. So that means—actually, it goes back to Giambattista Vico, who said, We can only understand what we do, and not what is given to us by God. So, that was the number one, and the second one was empirical—that is Ashby, literally, saying, Explore until this cannot be done anymore, and not get stuck with what currently is, but rather explore. Then there was a kind of a social one, namely that one should always increase the number of options for someone, but not harm people in participation. I don’t want to get into this, but there the design definitely entered my communication interest.

But now you’re coming back to the issue of my sabbatical. That was in Ohio State, Reinhart Butter wanted me to be there, and I wouldn’t mind doing that. In fact at the same time—that was also interesting—I was also invited, while I was there, to India. In India they had heard about product semantics, and they organized a huge conference on—now what is this is called? I don’t know the Indian name, but it had something to do with meaning. I’ll probably find it out. But that was also a great conference in which the Indians recognized—or I recognized—that it’s not just functional design a la Ulm, but you have to make design that has to do with culture in which it is taking place. Arthaya is the name. So that was a huge conference [at the Industrial Design Centre of the Indian Institute for Technology in Bombay].

I was actually, at the same time, in Ohio State when that happened, but in Ohio State I got an appointment with the design department, with the systems engineering department, and with the consulting firm. I had an office in the consulting firm and they roped me into their products.

⁴¹ 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.

And this was also interesting. In the beginning, they said, Well, we'll give him an office. And they let me be, but then they asked me at some point, Can you help me with this? I looked at it and I made some suggestions, and they said, My God. And they invited me from that moment on in all the projects, and that was a so-called experimental design laboratory. That means they really wanted to [work on] advanced type of projects.

Before I was there, there was a fascinating project by a furniture company, and they wanted to have office furniture. So they designed modular systems that you could put in a space, whether it is a room for making conference, to sit together, not just desks. And so there are different kinds of things, and you can buy them, and put them combining in there. So that was really very much avant-garde design experiments, etc. One of the projects they got, actually, was for Philips Eindhoven—coming back to Phillips. They had heard of the design laboratory. They sent two Phillips designers to come there and to learn from how we did things. But this was actually not that systemic, so I don't know what they got out of it.

Then there was a project in Eindhoven, and we were flown in there and helping develop it. To me that was eye opening because, for the first time, I had to work with stakeholders. There were numerous people—there were programmers, businesspeople, someone from the CEO—and we made suggestions, and we listened, and there was interaction. Actually, it's a long time ago, but it was to design something for insurance companies to have casework available. And so there was a study made by someone in Philips, saying, What do they need? What do they do? And there were videos made—what is interrupting their work?—telephone calls, etc. And how one can get access to these various kinds of things—how one can trace problems with the insurance company. So that was the beginning of it. We basically orchestrated a meeting of stakeholders in Philips, and we were also exposed to the latest technology, which was fascinating—although by now common.

But this was, to me, I think, a major impetus to understand stakeholders. After my sabbatical I was actually, for maybe three years period, I was constantly flying to Eindhoven and work with them. I learned an amazing amount. But it had also to do with communication. I mean, in this case communication with ordinary people, with different stakeholders, totally different conceptions, different discourses, and how they put together. And so that was quite fascinating and eye opening.

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: Well, it's a mixed thing. First of all, one disadvantage I had is I'm a professor of communication, so in some sense I'm not teaching in a design university. And I think many, many people like the ideas, and many of them copied them, used them, and use them in their

own way. But I think it has shifted radically towards issues of meaning, one conference after the other. Sometimes I'm mentioned, sometimes I'm not. I was recently, actually, also invited in Amsterdam. There is a huge conference on interaction design, and so they asked me to give a keynote address and I did. I wouldn't say I was a failure, but that was a huge conference, I was one of the first to do this, and I talked, actually, about different kind of languages and how discourse—how we could change the education towards a different kind of discourse that allows interfaces to be designed, developed, and evaluated. But afterwards there came other people, they were actually TED Talk types, and with huge sound and visuals, and so—I mean, I made a lot of friends, but I don't think I was on that level of presentation. I'm just not a performer in that sense.

But I think that is now common to talk about these interactions and interfaces, and looking at the ethnography of interfaces—that has gotten stuck. The other thing is in participatory design. Well, I don't know if I mentioned the issue of working with stakeholders. That has to do with conversation. And I think I go much further. Most designs are actually taking place in teams. When I was in Ulm there were big heroes, they knew everything, and one had to follow their lead or something. But important design decisions are never made by individuals. So I said from the beginning, one has to look at the kind of conversations that designers can engage in, or do engage in, that bring a new phenomena.

Actually, shifting to communication. There was an interesting conference, and actually it was in San Francisco from ICA. And that was actually called Comparative Communication Theory [Workshop, 1989], and I was part of it. I got really annoyed with these theoreticians that talked about theories without looking at what happened, and just comparing them. I wrote, actually, a paper and that was entitled "Conversation or Intellectual Imperialism."⁴² And I said what they are engaged in is intellectual imperialism, instead of looking at the kind of conversations within which communication theories emerge. And I said also—and that comes to the reflexivity—I think communication theorists ought to be part of the conversation that he or she describes. That is the second-order cybernetic notion that you cannot really, or you should not, separate the observer from the observed—and in the case of communication, the communication theorist from the communication that is going on.

Then, much later, I wrote a paper on conversation theory and asked the type of things that ruin conversation.⁴³ I was more interested in that, because there is something like genuine conversation—conversation where you respect someone else. You don't have an agenda. But in this process of interaction something new happens. In fact, I would say that no conversation repeats itself. It's all innovation. Everyone responds to the other with either adding something, elaborating something, or not responding, and there is an evolutionary aspect to it. I said always that conversation is the most efficient evolutionary mechanism that we know, because

⁴² 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/.

⁴³ 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.

things happen within seconds. Good things emerge, bad things drop out, and creativity is encouraged. That's part of a good conversation.

But then of course it can be ruined. It can be ruined by, for example, when you don't say something that you believe but you represent someone else. For example, in faculty meetings at the University of Pennsylvania, very often I was in meetings and there we discussed things, and then when we came [to] the decision: Oh, I cannot make a decision, I have to run it by my department. Well, then this constrains conversation. So, I mean, as soon as you are representative of someone else, or responsible for someone outside, then it is not a genuine conversation. Not that it's bad, but it's not a genuine conversation, and some of the creativity is gone.

That comes back now to another cybernetic concept—that of self-organization. Much of cybernetics is actually dealing with, maybe, different degrees of self-organization. A feedback loop is actually closed, and within the feedback loop certain things happen that are only marginally influenced by the outside—like the thermostat does it by itself. Of course, when the outside temperature change, then they change that too, but it is basically a self-organizing system. To me, a conversation is a self-organizing system, but it can be ruined when it has to respond to other things. When someone says, I told you this has to be accomplished by that time, it's not a conversation. But it can be, of course—it can be still doing something.

But I think much of good design emerges in conversations. In my case, my experiences, not just in conversations among designers. We prepared all kinds of presentations in advance. But we had to be open to the input of these many stakeholders. So that, again, combines both my design interest as well as my cybernetic interest and my knowledge of communication. And in fact I don't want to summarize too simplistic, but if you look at the history, yes, I came from design. I studied communication. I went back to design and introduced, basically, communication ideas to design. And then I took the design ideas into communication and asked, We have to see what is being constructed.

You didn't ask me about this whole notion of constructivism. That came, actually, from, let's say, the attitude of design, saying that we do things, maybe unintentionally as opposed to intentionally. There comes the issue of language—[Ludwig] Wittgenstein. Wittgenstein had several lives, one could say, but the last one was actually getting rid of the representational notion of language, and saying—well, the part that I like most is: language is a collection of games. You have a game like asking questions, getting answers. This is a game, everyone conforms to that whether in an interview or—asking questions, getting answers. Then there are also lots of other kinds of games, and I think in discourse that there are a lot of games, methodological games, what one does with data, etc. So that is a very different kind of notion of language, and what is interesting, it is a productive one. He, Wittgenstein, used actually an example of a language game, while he used the master and his apprentice building a house. What is going on between them, that, in fact, in the end the house comes about.

Now, I think, much of traditional science—natural sciences—are inherently dealing with an object that can't talk. But the social sciences deals with people that can talk. Communication theorists ought to be communicating. So I think there has to be made a connection in the social sciences between the language as a game, even though you might play the game of truth. But it changes certain kinds of things. So, I think, to me, there was a profound influence of translating, actually, the kind of second-order cybernetics, being part of it, issues of language, and lots of other things that I could have said. For example, speech act theory, but more importantly the issue of accountability, C. Wright Mills, and others who introduced the notion—you look puzzled—but he was a sociologist and he wrote a book on the power elite in the United States. What he did is, actually, he wanted to know, How is power enacted? And so he went to lots of board meetings and observed, and observed, and he said, Well, there is no power per se, it has to do with accountability. When you ask someone to explain—there are several kinds of things. You can have an explanation, if you don't understand. You can have an excuse, saying, I did it, but I won't do it again. You can have justification, saying, This is a good plan and you better get on board. Or you can have excuses, saying, It happens badly, but it's not my responsibility. I was not an actor.

I think, in this accounting, the notion of an actor is always defined, and it is not power—the traditional notion of power—but it is actually the yielding to a particular kind of argument or account—that makes it. So I think that is C. Wright Mills, and I have still since celebrated that, and in my class that I'm teaching that is part of it. So, it is, again, the notion of feedback on the level of language, and also the notion that language does certain kinds of things. I had just yesterday lunch with someone, a journalist from Israel, who took my seminar in part. And he is always focused on the issue of truth and lying. I said, Well, that is very fine. As a good journalist you have to sort out what is true and what is not. But ultimately it's a question of the consequences.

If someone says something without any basis, you don't know whether it's true or false, it has consequences. They may be unintended but they could be also intentional. Design deals, actually, with intentions. There is always an effort to improve on something. There are a lot of unintended consequences. My complaint against journalism is that they don't really focus on the consequences. They say, Truth is it, objectivity, source's name, etc. I tried to get him to recognize—and it is very difficult to think of another way of quality of journalism. My point is, more generally, language is not just a description. And that goes back to what I said earlier, when I wanted to write about artifacts and the semiotician said, Artifacts have no meaning. That's where it started, that saying, to me, everything has consequences, and particularly now on language. And I know so much more about this—language is consequential. When you think of the current political situation, language is consequential. To me, that combines, if you want, my design background with cybernetics and communication.

I wanted to say something. Very recently I wrote a paper, maybe eight years ago or something, on the cybernetics of design and design of cybernetics.⁴⁴ That is now selected by someone who wants to write also in design, and asked me whether that can be contributed. I didn't like it anymore and I rewrote it. But there comes again the epistemology. Second-order cybernetics, as much as important is to keep the cybernetician in the object of his interest, or the observer is part of the observed. But there is a bad influence, namely the focus on observers. I said, That's fine, but by putting the observer a part of the system, there is some sort of a self-reference—there is maybe some subjectivity, maybe there is self-preservation. But there is not much in terms of change. I said, If you focus on observation, observation of observation, even of describing observations of observations, describing including yourself, but to others, that's all fine. But they're still descriptions. That's not design. I said, somewhat tongue-in-cheek, Well, designers ought to change the world, and change it in the way that is very difficult to predict.

Even though—I'll come back to this—but one of the fascinating things is that technological and cultural change are genuinely unpredictable. Most scientists who have tried to predict technology failed. Only in outlines—one knows that computers go faster. We know that, that we can predict. But we cannot get the the principles in it. And the reason for that is very simple. Because there are the designers, they'll ruin all the predictions. And so that is to me fascinating. And from that point of view I associate myself with the designer. As soon as you predict, you actually don't give a choice to change something because it's always an extrapolation of what is in the past. So I said, in response to the second-order cybernetic orientation of finding descriptions of observations, saying, Well, you leave one foot still in the Enlightenment. The point of design is to change your observation, to make obsolete the descriptions of observations [laughs], at least in part—not in everything, but in part—and that is the point of design.

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 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 thing, because precisely what I said. First of all in terms of discourse—most discourse establish their recurrent practices—methodology. You can teach something because it's applicable to here, and there are statistics, you can map packages. And so that is the recurrent practice, and it is mechanized, if you want, and can be taught. But it gets

⁴⁴ 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/.

only to that what is recurrent, not the new things. So, design is really in a kind of a dilemma. On the one side you want to teach something, but you want to also keep open the possibility of changing it. So it's a tricky thing. What can you teach and what is it that you need to be open with?

So I outlined several things in *The Semantic Turn*, but in all cases I ended up—you mentioned desirable futures. Desirable futures don't exist. You kind of have observations. But you have literature, and literature often depicts idealist futures, and as soon as people read it and think, My God, this is interesting—even though it maybe can never get there, but it's interesting. That is evidence, if you want, of the potential acceptability of an innovation. There are methodologies that can look at you, but in most cases it has to do with with acceptance of dreams, of future visions, of stories. I mean if you look at Star Wars and something like this, it's probably not happening. But there is something of it that can be translated into everyday life. Not just in terms of video games, but in terms of practices of living, and so I think there is a lot of methodology there.

Maybe content analysis [laughs] can be fed in there. But there is something to explore what is possible. I don't know if I said this already in *The Semantic Turn*, but I said it in other contexts, when I talked about design research. Actually, there's another twist. I was asked to write a paper on design research and I said the title is, "Design, the Undisciplinable Profession."⁴⁵ So that means you cannot discipline it, because then as soon as you do this, then you kill it.

One of the things that I said was that one has to look at possibilities. The first thing is this empirical question, What is possible? Now, there are many kinds of possibilities. One is new kind of technologies that have been developed in one little area—it could be combined with something else. So that is on a technological level. Many designs actually have to do with doing the same thing but with new kind of methods or new materials. And not always very innovative. But the point is, actually, to investigate the possibilities you have—this is an empirical question. But science doesn't do that, never looks at possibilities rather than they look at what is.

So the other thing is that looking at language. Maybe I'll mention that now. I met at some point, long time ago, maybe four or five years ago, in Basel in the conference and also on semantics. There was a Turkish student and she made an ethnography of Turkish tea drinking. I thought that is—well, it's interesting. But I had a long conversation with her, and she was fascinated with what I had to say. Later on she worked towards her PhD in Istanbul, and then she asked me, Can I come to study with you? And I said, I'm in a communication school. You may be just misplaced. But I want to do that [she said]. So then I said, OK, come. Luckily there was an office available at the Annenberg School. She got an office—the best office, in my opinion—better than mine. She was there, and she had also her husband. And what she did is, actually, she

⁴⁵ Klaus Krippendorff, "Design, an Undisciplinable Profession," in *Design as Research: Positions, Arguments, Perspectives*, edited by Gesche Joost *et al.*, 197–206 (Basel: Birkhäuser Verlag/De Gruyter, 2016), https://repository.upenn.edu/asc_papers/628.

wanted to—well, again she had an ethnographical bent, and she wanted to see what is the possibility of designing a new ironing board.

I said, An ironing board is such an old-fashioned thing, and there is not that much social in it because you do it by yourself. Yes, it's not that it is not a design problem, but it is not very rich. Why don't you think of other things? So, I gave her lots of things, one of which is health issues—health is a big issue. So she picked that and she joined several discussion groups: Fitbit, Weight Watchers, etc., and she joined as a participant. I told her also, Join—you cannot deceive that you are actually a designer and interested in it. Say it, very clear. But after she said it and everyone knew, it was insignificant. She was part of it, and she asked questions, and she got answers. And she transcribed them, and then she looked into, What could designers get out of that? What possibilities get out of it?

So then I said, Well, maybe we should make an experiment. We have a design department at Drexel University. So she made these huge transcripts and thought, Let's give them these transcripts. But I know designers don't read—actually most people—students—don't want to read endlessly. So I said you have to condense it, and you can't ask them to write things down. This was really an amazing thing. We gave them two felt pens and said, Underline what is surprising to you. And the other one is, What is it that you got a good idea from? Then we asked them to later share these good ideas. And so I was actually in this so-called experiment, it took maybe two, three hours, and the students were interested initially, without really knowing what was going on. But later on it was quite amazing. They outlined, they gave her data that she could analyze, content analyze—what is yellow, what is red. So she got in feedback from the designers, what they could get out of it, what was not there before, what they had never heard of. And so that is a methodology that is geared towards not what is but what could be done.

She wrote two articles, one of them is just now published, and she actually defended her dissertation—I went to Istanbul for that—and she was with flying colors, so she made it. And this was also amazing—normally they write maybe three years on their dissertation. She did it in one year because she was here and she was isolated from the obligations in Turkey. This is a very different kind of methodology, and that is what you're asking me. In fact, in this new rewrite that I made about the cybernetics of design and design of cybernetics, I said, Focusing on the description of what is—even ethnography is a description of how people really live. That keeps, actually—if designers take this as a link—keeps them confined. And so that is really unfortunate, that the more description the more it limits innovation.

That's the reason why I focus—for example, with these Drexel students, we developed an ethnography of possibilities. For example, asking people, What is boring for you? Where do you spend too much time that you don't want to? Where do you see something that you have to be paying so much attention to that you wouldn't want to? That comes from the ironing board. For example, when you iron—I don't know if you have ever ironed, but you know that you cannot have the iron for a long time on a shirt, then you burn it, and then you have to go on knobs, buttons, etc. So there are lots of things that you have to pay constant attention to. And there

are lots of things that are bothersome, and maybe not. But people have to pay attention to, even though they think that's normal—that's what skilled ironing is. When you have a list of all of these things, that gives designers possibilities to change.

There are also issues of correlations. I'm always fascinated—for example, when I learned to drive, actually, with the Volkswagen, in Germany. And the Volkswagen had a blinker on the side that went out like this, when you wanted to make a left turn, and then you had to put it back. But many people of course didn't [laughs]. That was always a problem. How come that we have this very useful thing that, after you made your turn, you come back, because it's correlated, when you make a left turn you have to at some point becomes straight. So these are correlations that give possibilities for design to shortcut something—mainly because someone would tell you, I forgot to put it, I always forget to. And then getting into trouble.

So this is a different kind of research that gives possibilities. As I said, one of the most important parts is probably—what came out in much of these health communications. What are ideal futures? With health, you want to be slim, and you want to be healthy, whatever. And that comes out in numerous ways, and people can give you criteria—why that is. Actually, I had a student, you know her. She is now in China interviewing women about their role. In China women don't play such a big role. If you look at the [Communist] Party Congress, there's not a single woman. So there are lots of problems. So she is interviewing them precisely to elicit what is possible, and why what is possible cannot be achieved? Or why they see it cannot be achieved? Then you get problems that the designer could solve. She is not interested in design, but maybe women could realize that what keeps them in a trap, and what could be done about it.

So that goes, actually, also in my whole notion of—I don't know if you want to get into this now or another time—the issue of constructivism. I don't like, personally, the word “ism,” because that looks like a commitment to an ideology, and I don't have that. But I do think there is something to understanding that language and actions do things, and maybe one should make a difference between the kind of things that are done automatically, without knowing. I mentioned earlier, journalists say, Just describe the facts. But even the facts have consequences. Or in everyday conversation, describing the facts, and describing it accurately—well, that is maybe the aim of much of science, but it has consequences.

For example, racism. I think I'm making myself perhaps unpopular, but when I say, As soon as you ask an interview question, What race? Well, then you create it. If it's not there—I don't think by not mentioning it it goes away, but by mentioning it, it says it's important. When you have these data, then you find correlations. You find, for example, correlations between blackness and low income, or blackness and low intelligence, and then you say, Of course, they are low intelligence. But this is not understanding that the very act of mentioning it is creating it. I had a Wharton School [at the University of Pennsylvania] student, and she did a fantastic experiment before she came to my class. They had job applications and they were uniform, but they changed the picture. One was a white person and a black person, and the black person, not never, but most of the time didn't get the job, and the white person did.

So the categorizations are extremely important and make a difference. And I think if you really want to make a difference, change something, then one has to not find these correlations as of now—like intelligence and blackness, which has to do much with cultural situation that someone simply not have the opportunity to go to college or to advance in some form, or the social situation prevent them from doing anything. So it's not really genetic, it's not really a racial issue, it is the cultural issue. So these kind of things to me are very important. Or the other thing is—that goes back maybe to discourse. I think a discourse is, again, a self-organizing system. When you are in the discourse, you don't easily see what's outside the discourse, and you can be entrapped. To me, I think, one of the most important parts is actually, again, the issue of construction, is to realize that when you are in a discourse and you are entrapped, it is you who entrapped yourself by adopting certain kinds of premises, certain kind of ideologies, certain kind of procedures, subscribe to certain kind of institutions, that you could do otherwise. That is the whole issue of ideology. Ideology is always closed.

I have one student right now who is a Kurd from Syria, and he describes the whole notion—he wrote a paper about the identity of Kurds. They are an interesting crossroads. On the one side they are Muslims, largely, but it was the Muslims who attacked them, bombed them, genocide. Then they have a long history, which is in a different kind of religion that is completely opposed to Islam. Then there is the issue of geography. As you know Kurds are distributed over—so that's very complicated. But as soon as you back on one—for example, the issue of being Muslim—once you do this, then you close certain things out. So, to me, much of construction has to do with the negotiation, in this case about identity, that is taking place, and hopefully it's for the better. You can also let it be, and say it will happen or not. From a design perspective, you might be able to think of doing it somewhat more deliberately, and ask what are the consequences of using certain kind of terms, etc.

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: Well, you are right. I'm always saying that I have three hats—that is 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 learned anthropology, language, and communication, and I learned to combine them. And to me—I mean, as a person I'm combining them. And when I'm going to designers, well, I will be a communication person and a cybernetician. When I'm at the Annenberg School I'm also a

designer and a cybernetician. So I think what I'm teaching at the Annenberg School is, as you say, very much influenced with all of them.

Actually, I feel sometimes sad because I'm not presenting myself at Annenberg as a cybernetician. Most people do not know, unless they do know me personally. And I didn't want to do that because it brands me in one way or the other, and I like to be open. Some people know that I was a designer and I did a lot of things—but I don't make a big fuss of it because I think, again, it is the issue of the discourse. If you say that you are really a designer [laughs]—which I am not—then you're easily outcast. I have made, I think, a lot of contributions to the communication field by not mentioning that, but by proposing ideas that clearly come from elsewhere, but that are revolutionary within the communication discourse.

I mentioned at some point in my presidential address, I think, to me, that is very important to make this connection, not as connection, but also changes—introduce changes that maybe other people don't recognize. I personally think I'm blessed by having—by being actually recognized and competent in all three discourses. Recognized—I got an honorary doctorate for design from the university in Kalmar [Linnaeus University in Sweden]—actually, I got it in design. I got numerous awards in cybernetics, a Norbert Wiener Medal and something else. I'm recognized there, and I have been president of the ICA, and I have been in best paper awards, and been nominated in honorary societies, and being a Fellow at ICA.

Because I could jump from one to the other, that made me, I think, a productive contributor to all. If I want to give a lesson to someone else, I would say that is probably the most important one—is to be versed in different kinds of discourses. In terms of students, I always, when I have a preference—and I don't have, by the way, I have a preference to take someone who wants to have a permission—for example, undergraduate. I have now an undergraduate, or several times. The first thing I asked her, What languages do you speak? I don't mind that you only speak English, but by speaking a different language you have access to different culture. Where have you traveled? That was also important.

For example, right now I have an undergraduate, she traveled all over the world, and so she had a sense of different kinds of cultures and could combine that in numerous ways to her own culture. And to me that is very important. To make creative contributions, as I said earlier, conversation. But in conversation you have to be open. Actually this undergraduate that I had just now, she wrote a fantastic paper—although from a social science point of view there are lots of things to be criticized, and I will talk to her—but about tourism. What do tourists see when they take a trip in a foreign country, and where did they go? She made a difference between kind of tourism—camp tourism, like taking a tour—as opposed to backpacking. I have the suspicion, I have to ask her, she was probably a backpacker [laughs], because it was very clear that backpackers are going to places where not everyone goes. They are open to new experiences, and actually, to tell you the truth, it gelled with me, because I was a backpacker when I was in Germany—we hitchhiked all over the world from Lapland to Yugoslavia, France, you name it. It was minimal because we couldn't travel very much outside, we didn't have much money as a student. But I was very much associated with that. I think the point is we

need to be open to differences, and to me that is critical to design. It's critical to make any contribution to anywhere.

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. That was also, for me, enjoyable to rethink some of the things that you asked me.

END OF SESSION FIVE