

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 Two (January 18, 2017)

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.

RESTRICTIONS

None

FORMAT

Interview. Video recordings at the home of Klaus Krippendorff, 510 South 24th Street, Philadelphia, PA 19146, USA.

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.

BIBLIOGRAPHY AND CITATION FORMS

Video recording

Bibliography: Krippendorff, Klaus. Interview by Jefferson Pooley. Video recording, January 18, 2017. Communication Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania. **Footnote example:** Klaus Krippendorff, interview by Jefferson Pooley, video recording, January 18, 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, January 18, 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, January 18, 2017, Communication Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania, pp. 34-35.

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 adaption 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.¹ 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 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,² 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

¹ Bernard Berelson, *Content Analysis in Communication Research* (Glencoe, IL: Free Press, 1952).

² Klaus Krippendorff, *Content Analysis: An Introduction to Its Methodology* (Beverly Hills, CA: Sage Publications, 1980).

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 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.³ 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 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?

³ 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.

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 1969⁴—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.⁵ 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 impressive 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, 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

⁴ 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).

⁵ Ithiel de Sola Pool, ed., *Trends in Content Analysis* (Urbana, IL: University of Illinois Press, 1959).

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.⁶

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.⁷ 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

⁶ 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.

⁷ 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/.

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.

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.⁸ 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.

⁸ 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/.

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 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.⁹ 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

⁹ Sol Worth, "Pictures Can't Say Ain't," *Versus* 12 (1975): 85–108.

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 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.¹⁰ And the second one was published the next year which was on data—on generating data in communication research.¹¹ 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 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:

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

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

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.¹² And you talk about a general law of communication process, that—you know—communication generates structure. And it sort of downplays intentionality and purpose. And so I had a question—if you know about where that paper came from—remember? But also of all of this cybernetics-oriented work, how would you place it in the rest of your trajectory as you went along, you know?

KRIPPENDORFF: Well, I 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 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

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

analysis.¹³ And if you could just talk about how the period in the late sixties, you know, gestated through to that book. How did it come about? It obviously got published to huge acclaim and interest and so on. What was the through-line from this period we're talking about and that book?

KRIPPENDORFF: I don't know how to answer the through-line issue. 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 communication, and I remember I drew fantastic diagrams which are also preserved.¹⁴ And that

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

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

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.¹⁵ 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.

¹⁵ 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 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.¹⁶ 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

¹⁶ Herminia Corazon M. Alfonso, *Socially Shared Inquiry: A Self-Reflexive Emancipatory Communication Approach to Social Research* (Quezon City, Philippines: Great Books Trading, 1999).