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

KLAUS KRIPPENDORFF

interviewed by

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December 20, 2016
January 18, February 22, April 12, and May 17, 2017

Philadelphia, PA

BIOGRAPHY

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

ABSTRACT – Session Three (January 18, 2017)

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.

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 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 Shackleford 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.[1] 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

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

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].[2] But it's interesting, these politics of loyalty, etc., etc.

[2] 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 [gutteral 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.[3] 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.

[3] 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.[4] 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.[5] But this was Randy Harrison

[4] 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.

[5] 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.[6] And it was published later.[7] 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

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[6] 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.

[7] 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*.[8] 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

[8] 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.[9] 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

[9] 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.[10]

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.[11] 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 havef 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

[10] 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.

[11] 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