

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 Four (April 12, 2017)

The session focuses on Krippendorff's lifelong engagement with cybernetics, beginning with his exposure to ideas at Ulm through to his 1980s turn to second-order, social constructionist cybernetics. He revisits his graduate school encounters with Ross Ashby, and his ongoing importance for his (Krippendorff's) thought. His involvement in cybernetics-related conferences and scholarly societies, like the American Society for Cybernetics and the Society for General

Systems Research, are recounted. Considerable attention is paid to Krippendorff's organization of a 1974 Annenberg School of Communications conference, on Communication and Control in Social Processes, and the 1979 book that emerged from the conference. Krippendorff traces his constructionist turn to Margaret Mead's paper at the 1967 Gaithersburg American Society for Cybernetics gathering, though he explains that his full engagement with what he called the cybernetics of cybernetics occurred in the early 1980s. His Annenberg teaching on cybernetics-related themes is discussed. Krippendorff describes the cybernetics implications for communication theory and ethics, through to publications appearing in the late 2000s.

## 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, April 12, 2017. Communication Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania. **Footnote example:** Klaus Krippendorff, interview by Jefferson Pooley, video recording, April 12, 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, April 12, 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, April 12, 2017, Communication

Scholars Oral History Project, Annenberg School for Communication Archives, University of Pennsylvania, pp. 34-35.

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

Philadelphia, PA

Interviewed by Jefferson Pooley

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

KRIPPENDORFF: Well, as I was saying at some point my first hearing about cybernetics was at my design school [Ulm School of Design, Ulm, Germany], where one teacher, Horst Rittel, tried to move designers away from designing little products to looking at larger systems, and cybernetics was part of it. And then, as I mentioned, at some point I was in Oxford in 1959 summer, and I bought two books at Blackwell, in the bookstore. And without knowing, they determined really my trajectory. One was [Ludwig] Wittgenstein's *Tractatus [Logico-Philosophicus]*, and the other one was Ross Ashby's *[An] Introduction to Cybernetics*. Because I vaguely knew his name, but [Wittgenstein] had the advantage of having German and English text, and my English was miserable, and therefore I thought that is a good way [laughs]—certainly one cannot really learn English from a philosopher.

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

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

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

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

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

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

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

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

widely accepted.<sup>1</sup> But to me I think the idea of calculating this was very important, and I had fun with playing with that, no doubt. But I wrote several papers on these issues of circular flow of variance.

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

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

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

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

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<sup>1</sup> Klaus Krippendorff, *Information Theory: Structural Models for Qualitative Data* (Beverly Hills, CA: Sage Publications, 1986).

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

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

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

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

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



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

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

I think my mission was really to apply cybernetics to my own work, and also to communication as a discipline. I joined the International Communication Association [ICA], I think, in 1966 or something—that was before I joined the American Society for Cybernetics. And I presented, actually, then a paper, an analysis of communication theories, in particular [Harold] Lasswell's, who said that communication is 'who says what to whom with what effects.' And then he said that each of these things,—'who' is a different kind of exploration. It's the analyst of the sources. 'Says what' is content analysis. 'To whom' is receiver or audience research. 'With what effect' is other things.

And I thought that is just—to parcel communication out in these separate components with separate methodologies is precisely avoiding the systemic, and the dynamic, consequences of communication in society. So I wrote this paper, still very mathematical, trying to approach or find a way of collecting data for these connections that would not be separated into content and whatever. And that I presented, and then I published it in 1970.<sup>2</sup> And actually, I have to say I had difficulties: I submitted it to the *Journal of Communication* and there were several reviewers that said, It's no good. And then the editor said, I'm overruling the reviewers and I'll publish it. And it got an award for the best paper in 1970, from the ICA. And so these were kind of the struggles of—the difficulties of—making cybernetics, understanding—and actually I don't think I mentioned even the word cybernetics in this paper. But the ideas of it, looking at larger systems, were a major focus of my attention.

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<sup>2</sup> Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication* 20, no. 3 (1970): 241–69, [https://repository.upenn.edu/asc\\_papers/273/](https://repository.upenn.edu/asc_papers/273/).

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

KRIPPENDORFF: Yes. Well, in 1972, actually, that was when I was strongly working with information theory, basically. And I presented a paper on algorithms, of how one could look at complex systems, and that was in Oxford, in England. And there came a presenter—maybe I should say one thing beforehand. When Ashby retired to England he was asked to nominate someone that could replace him, and that was me. However, at that time in the University of Illinois the Biological Computer Laboratory, which was headed by Heinz von Foerster, became in disfavor, one could say. And it was in the process of being disbanded, and so it never became anything—nobody was replacing Ashby.

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

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

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

KRIPPENDORFF: Well, the annual conferences of the American Society for Cybernetics had ceased, stopped. And cybernetics, in my opinion, was a bit in disarray. There was one, the

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

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

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

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

Luckily someone had actually taped Anatol Rapoport's presentation. He didn't want to write it, so I wrote it from what he said and he made minor additions. But the book was published. Again, I wrote to many publishers, I forgot now how many, but there was one, Gordon and Breach, that wanted to publish it. And it has become now, I wouldn't say a standard book, but it was a snapshot at this time.<sup>3</sup> The issue of control in society was of course central, but control not in the sense of forcing people to do things not to do, but in order to develop equivalences, balances, etc., and imbalances, understanding imbalances that come.

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

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<sup>3</sup> Klaus Krippendorff, ed., *Communication and Control in Society* (New York: Gordon and Breach, 1979).

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

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

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

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

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

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

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

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

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

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

There is, for example, one paper—I don't remember when I wrote this—but I was invited in a dialogue on comparative communication theory. And so people compared communication theory. Well, I did this too. But I was kind of surprised, there was never any consideration as to what the communicators about which one theorized, what they were thinking of it. So I said—this was kind of tongue-in-cheek criticism, by saying, When you do this then you are actually conducting intellectual imperialism. By saying that those who compare the communication theorists, they are superior to those who communicate. And I, at that time, said the alternative is conversation—that the communication theorist should engage in conversation with those they are theorizing.<sup>4</sup>

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

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<sup>4</sup> Klaus Krippendorff, "Conversation or Intellectual Imperialism in Comparing Communication Theories," *Communication Theory* 3 no. 3 (1993): 252–66, [https://repository.upenn.edu/asc\\_papers/257/](https://repository.upenn.edu/asc_papers/257/).

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

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

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

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

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

much time and I was not given enough. But at that time I proposed several imperatives.<sup>5</sup> I would like to see if I could get them [consults book]. These imperatives are still not really yet in the issues of conversation, but in the beginning of constructivism.

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

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

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

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

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<sup>5</sup> 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](http://repository.upenn.edu/asc_papers/275).

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

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

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

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

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



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

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

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

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

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

But that gelled with another area which, to me, was very important. C. Wright Mills wrote, at some point [1956], a book on *The Power Elite* in the United States. Unfortunately, he died very

young and he didn't really complete, so to speak, his work. But he said, What is power in the power elite? And he went to board meetings and participated, and he came up with the idea—he didn't call it accountability, but he said motivation. In every board meeting, when there's something proposed there is motivation provided—these are, kind of, justifications. And so he preceded, in a way, the whole notion of accountability, by saying, That is where power is exercised—that someone convinces a group of the positive things that had been done and they comply. The point is not to exert power in one direction. The point is actually to accept the account given by that man.

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

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

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

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

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

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

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

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

But then came also the political dimension. In Germany the minister of—I don't know if it was science—but a Prussian minister [Friedrich Althoff] realized that the French are ahead of Germans in syphilis research, and he picked out a guy named [August] Wassermann. And Wassermann was a biologist. And he said, Here is lots of money. Just do something to outwit the French. Now, you see, it is political [laughs]. And Wassermann found something that was, later on, found wrong. But he developed a whole area in biology to look at serums in blood.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

And this one paper that I think you mentioned earlier, about the epistemology of communication, did this precisely, saying we're making distinctions, thereby creating differences.<sup>6</sup> And now we have to understand, What is the difference between them? And then we construct relationships between them, and that is what we then report: correlations, etc. Now comes the issue, it doesn't stay with reporting. It has to do also, when these differences and relationships come into the public domain or are enacted. People act on the differences that they have been told, and that has an influence on whatever you observe. Now in, for example, racism, a good example. While there is a physical difference between blacks and whites, skin color, but now, how do you explain it? First of all you have to make a distinction. Not every culture makes this distinction by color.

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

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

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

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

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<sup>6</sup> Klaus Krippendorff, "An Epistemological Foundation for Communication," *Journal of Communication* 34, no. 3 (1984): 21–36, [https://repository.upenn.edu/asc\\_papers/538](https://repository.upenn.edu/asc_papers/538).



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

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

Q: OK, good.

KRIPPENDORFF: Thank you.

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

KRIPPENDORFF: Thank you.

**END OF SESSION FOUR**