

For God's Sake, Margaret

Conversation with Gregory Bateson and Margaret Mead



This following article is of a conversation between Stewart Brand, Gregory Bateson and Margaret Mead and was originally published in the CoEvolutionary Quarterly, June 1976, Issue no. 10, pp. 32-44. With very many thanks to Stewart Brand for his permission to reproduce it in this web page.

*Margaret Mead and Gregory Bateson were married in 1936. They had met and fallen in love in 1932 while both were doing anthropological fieldwork on the Sepik River in New Guinea (Margaret was at the same time with her second husband, Reo Fortune). In New Guinea Gregory's unusual sense of theory met Margaret's improved field methodology and sparked much of the quality in Gregory's opus on the latmul tribe, **Naven**.*

Newly-wed in Bali, they spent two collaborative years in the most intense and productive fieldwork of their lives, developing, among other things, a still unmatched photographic analysis of the culture.

*Their daughter Mary Catherine, Margaret's only child, was born in 1939 in the United States. Gregory and Margaret worked together on the result of their Bali fieldwork, **Balinese Character - A Photographic Analysis**, and then were separated increasingly by World War II and their own diverging interests.*

After the war they both were involved in starting the somewhat famous Macy Conferences (1947-53) that invented cybernetics. This interview begins with their joint recollection of that critical period.

*Margaret Mead is one of the world's most remarkable women. she got a full mixture of praise and notoriety (notorious in that day because women weren't supposed to talk about sex) with her first book, **Coming of Age in Samoa** (1928). Since then there have been ten other books and numerous honors and positions, including President of the American Anthropological Association (1960), and of Scientists' Institute on Public Information, and (this year) the American Association for the Advancement of Science, and a Curator of the American Museum of Natural History, which continues as her headquarters. In public affairs she seems to have taken over the Eleanor Roosevelt niche.*

*After Bali and the Macy Conferences Gregory Bateson went on to work with schizophrenics, alcoholics, artists, dolphins, students, and a steadily more general set of understandings of what they have in common. He co-authored a book, **Communication: The Social Matrix of Psychiatry** (1951-68, Norton), with Jurgen Ruesch, and edited **Perceval's Narrative - A Patients Account of his Psychosis, 1830-1832** (1961, Stanford). Mary Catherine, his and Margaret's daughter, wrote a book about one of Gregory's conferences, **Our Own Metaphor** (1972, Knopf). His collected papers appear in **Steps to an Ecology of Mind** (1972, Ballantine), a book that wowed me out of my shoes. If Gregory lives long enough he will get his Nobel for the Double Bind Theory of Schizophrenia.*

Margaret is now 75, Gregory 72. They meet seldom though always affectionately. Gregory has a son John, 23, by his second wife, and a daughter Nora, 9, by Lois Bateson his present wife. This meeting with Margaret took place at Gregory's home near Santa Cruz, California, in March of this year.

Stewart Brand: I need a little background, if it's all right, on how this whole Macy thing got rolling, why, and when, and what the sequence was.

Gregory Bateson: There was this Macy meeting in what, '42? ¹

SB: Who started it, and what was it about?

Bateson: This was a meeting called 'Cerebral Inhibition,' which in fact was a meeting on hypnosis.* 'Cerebral inhibition' was a respectable word for hypnosis. Most of what was said about 'feedback' was said over lunch.

Mead: Well, I know that's what you always tell people, but I didn't sit at the same place at lunch, and I heard what was said at that conference. But at that conference, which is the one where Milton Erickson hypnotised that Yale psychologist, it was at the end of that conference that you really had the design of what needed to be done. And then you were caught up in war work and went overseas and there was that long period.

I think that you actually have to go back to that earlier meeting that was held in the basement of the old Psycho-Analytic building on the West Side the day of Pearl Harbor.

Bateson: They didn't on-go from year to year, those early ones. Larry Frank was chairman I bet.

Mead: No, Larry never was chairman, you know. He always sat on the sidelines and made somebody else be chairman. Kubie was a very important person at that point.

B: Yes. Kubie was an important bridge because Kubie had respectable-ized Milton. There's a whole series of papers which are jointly Kubie and Erickson. Now, in fact, they were Erickson's papers.

M: And Kubie didn't know what was in them. That's the truth.

B: But Kubie did get right the energy problem. He was the first person that really took Freud's 'energy' and said, 'Look, look, look, it makes no sense.' There is a very good paper by Kubie on the errors of Freudian energy theory. [*Goes to find the references*] Huh. Kubie, 'Fallacious Use of Quantitative Concepts in Dynamic Psychology.'

M: Now when was that?

B: That was ... guess.

M: No, I don't guess that one.

B: Published in '47. **Psychoanalytic Quarterly**. For which I suspect he very nearly got read out of the church. He never said it again.

M: It was very hard to read Kubie out of the church because he had once been a neurologist, and that was the thing that they were all scared of. Now, where is the Rosenblueth, Wiener and Bigelow paper? The first great paper on cybernetics.**

B: Rosenblueth, Wiener and Bigelow. 'Behavior, Purpose and Teleology,' **Philosophy of Science**, 1943.²

M: That's it, you see.

B: It could just have been published at the time of the Cerebral Inhibition conference.

M: It was just coming out or just had come out.

SB: What was the experiment that that paper recorded?

B: It didn't record an experiment, it reported on the formal character of seeking mechanisms, essentially. Selfcorrective mechanisms such as missiles. The missile measures the angle between its direction and the target it's seeking, and uses that measure to correct itself.

M: But using some very simple psychological experiments that Rosenblueth had been doing at the University of Mexico.

SB: Do you recall what they were saying that you overheard that got you excited?

B: It was a solution to the problem of purpose. From Aristotle on, the final cause has always been the mystery. This came out then. We didn't realize then (at least I didn't realize it, though McCulloch might have) that the whole of logic would have to be reconstructed for recursiveness. When I came in from overseas in '45 I went within the first two or three days to Frank Fremont-Smith, and said, 'Let's have a Macy Conference on that stuff.'

M: You and Warren McCulloch had an exchange of letters when you were in Ceylon.

B: We did?

M: Yes. You told me enough about it in some way. I talked to Fremont-Smith. McCulloch had talked to Fremont-Smith.

B: Fremont-Smith told me, 'Yes, we've just arranged to have one, McCulloch is the chairman, go talk to McCulloch.'

M: And McCulloch had a grand design in his mind. He got people into that conference, who he then kept from talking.

B: Yes, he had a design on how the shape of the conversation would run over five years - what had to be said before what else had to be said.

M: He wouldn't let Ralph Gerard talk. He said, 'You can talk next year.' He was very autocratic.

B: Yes, but an awfully good chairman in many ways. It's very rare to have a chairman who knows what it's about at all.

SB: What was his grand design?

B: Who knows?

M: Well, I think more or less what happened was.

B: How did the first meeting differ from the second meeting?

M: There wasn't even any usable terminology. At first we called the thing 'feedback,' and the models that we were presented with at that point were the guided missile, targetseeking. Now there had been another even that's worth considering here. That is that Wiener had written an article in the **Atlantic**, or **Harper's**, refusing to give the war data on guided missiles. Remember that?

B: Oh, yes.

M: He'd worked on them all through the war, and of course they had the material if they had hunted for it, but they made the mistake of asking him for some, and at that point he said that he would not give it to

them, the war was over, and this was data that could only be used for war-like purposes. He would not give it to them.

B: That's right, it was the **Atlantic**.

M: They were talking almost entirely of negative feedback. By this time, Wiener and Bigelow and Johnny von Neumann of course, were members of the group, and Rosenblueth, Kurt Lewin, Molly Harrower, Evelyn Hutchinson, Leonard Savage, Henry Brosin and that Hungarian who always knew who was sleeping with who and it was the only thing he was interested in, I've forgotten his name. Well, the lists survive all right.

There were three groups of people. There were the mathematicians and physicists - people trained in the physical sciences, who were very, very precise in what they wanted to think about. There was a small group of us, anthropologists and psychiatrists, who were trained to know enough about psychology in groups so we knew what was happening, and could use it, and disallow it. And then there were two or three gossips in the middle, who were very simple people who had a lot of loose intuition and no discipline to what they were doing. In a sense it was the most interesting conference I've ever been in, because nobody knew how to manage these things yet.

SB: So you had one group of people that was to another group on a level they were not used to.

M: Yes, and shifting back and forth between these levels and keeping everything straight was very interesting. So we used the model, 'feedback,' and Kurt Lewin - who didn't understand any known language, but always had to reduce them to concepts - he went away with the idea of feedback as something that when you did anything with a group you went back and told them later what had happened. And he died before anything much else happened. So the word 'feedback' got introduced incorrectly into the international UNESCO type conferences where it's been ever since.

B: In the small group cult, feedback now means either telling people what they did, or answering.

M: Yes. 'I don't get any feedback from you,' or 'I can't go on with this without some feedback.' It wouldn't have survived if Kurt had lived. He would undoubtedly have got it right.

SB: I would like a little more detail back at the initial time when you knew you had hit something.

B: We knew we had, well, for me, I had analysed the latmul of Sepik River in **Naven**³ and I had analysed out the fact there were interactions which must stockpile.

SB: This was your schismogenesis?

B: This was schismogenesis, yes. We named it in '36.

M: It hadn't been named yet. You're starting back before you named it schismogenesis.

B: Well, **Naven** was published. I'm talking about the state I was in when this stuff appeared.

M: In '43.

B: Yes. The next thing that followed that was 'Generalised Foreign Policies.' L.F. Richardson.⁴ I went back to England in '39. Hitler had invaded Poland. Bartlett said, 'You might be interested in that,' throwing it across the room in contempt.

M: I'm glad I have another count against Bartlett, I didn't know he had contempt for Richardson.

B: For Richardson and for me, you see. It was contemptible that I would be interested in the contemptible. So I ran off with that and kept it (probably it's Bartlett's copy of his files that we now have), and brought

it back to this country.

SB: What was in that paper?

B: This is the mathematics of armaments races. How do you build the mathematics of a system in which what I do depends upon what you do, and what you do depends upon what I do, and we get into a thing. Richardson set a limit by invoking 'fatigue.' He started with a simple pair of differential equations in the premise that my rate of armament could be a linear function of your strength; and vice versa. That led immediately to an exponential runaway. He added a 'fatigue' factor representing the drain on your and my resources. The question then was whether the system could settle. Are we going to settle a mutual ... there's a word in international relations for slapping the other people's aggression back by threat. ...

M: You mean deterrence?

B: Yes, mutual deterrence. That word hadn't been invented then. Then in the appendix, he had some revised equations in terms of what is your strength and what is my strength, but what is the difference between our strengths. He worked it out in terms of the relation of two nations where each is stimulated by the amount the other side is ahead. This was obviously symmetrical - latmul Sepik River schismogenesis - right?

I then wrote to him at that stage, and said, 'What about the other case, when you are stimulated to aggression by the weakness of the other side?' Which is the complementary schismogenesis, right? He worked out the algebra for that, and said, 'It's very unpromising. I don't recommend nations to get into that all. The orders of instability they get into are then very serious.'

SB: Because that one would accelerate the difference rather than reduce the difference?

B: Accelerate the difference, yes.

SB: A large amount of this strikes me as being the war. Would cybernetics have begun without the war? Richardson's armaments race, and Wiener's missiles ...

B: Wiener without a biologist wouldn't have done it.

M: Wiener was working on Rosenblueth's stuff. Now Richardson is a very peculiar character. He was a Quaker school teacher of mathematics. He did all the basic work on weather prediction. It was used in World War II and he was never told how it worked, because of security. He died without knowing about it.

B: Richardson was responding to World War I. As a Quaker he refused to bear arms in World War I, and he became an ambulance man. He sat in the trenches waiting for the next call for the ambulance working out the mathematics of armaments races. Because he was sure that if only this could be got straight, the whole mess wouldn't have to happen, which indeed might be true.

M: Now, there were some other things like this that were being talked about, and one was what was called a vicious circle. Milton Erickson had written a paper on a girl who quarrelled and had headaches and got alienated from people, which led to further quarrels, and so on.

B: Yes, all the positive feedback stuff was ready. And that presented the problem: why don't these systems blow their tops? And the moment they came out with negative feedback, then one was able to say why they don't blow their tops.

SB: This was a word and an idea you heard about in '43?

B: That's when *negative feedback* came in.

M: We had things about reversals of sign ...

B: That was another story, that's before Richardson, even, and way before feedback. Already in **Naven** there is a statement that complementary schismogenesis neutralises symmetrical, and vice-versa. If you get into too-long a contrast between the bosses and the workers (which is complementary schismogenesis), you put them all out on the cricket field and make them play cricket, which puts them in a symmetrical situation. And it doesn't matter who wins the game of cricket, you know.

SB: As long as they're in that mode ...

B: Or if they're too far in symmetrical rivalry, such as a quarrelling husband and wife, when one of them sprains in his ankle, in comes them complementary with dependency. They suddenly feel much better.

SB: It doesn't matter who sprains?

B: It doesn't matter who sprains his ankle, of course not.

SB: So you had some notion that all of these various pathologies were structurally the same?

B: No, structurally related, that there was a subject matter of inquiry defined by all these. You see the fantastic thing is that in 1856, before the publication of the **Origin of Species**, Wallace in Ternate, Indonesia, had a psychedelic spell following his malaria in which he invented the principle of natural selection. He wrote to Darwin and he said, 'Look, natural selection is just like a steam engine with a governor.' The first cybernetic model. But then he only thought he had an illustration, he didn't think he'd really said probably the most powerful thing that'd been said in the 19th Century.

M: Only nobody knew it.

B: Nobody knew it. And there it is, still in the text. Nobody picked it up. Well, there was the machinery, the governor itself. There was the mathematics of the machine with the governor, which was done by Clerk Maxwell in 1868, because nobody knew how to write a blueprint for these bloody things - they would go into oscillation. Then there's Claude Bernard about 1890 with the *milieu interne* - the internal matrix of the body, control of temperature, control of sugar, and all that.⁵

SB: Which later became homeostasis?

B: Which later became homeostasis in Cannon.⁶ But nobody put the stuff together to say these are the formal relations which go for natural selection, which go for internal physiology, which go for purpose, which go for a cat trying to catch a mouse, which go for me picking up the salt cellar. This was really done by Wiener, and Rosenblueth and McCulloch and Bigelow. And who really put the truth through, I don't know, do you?

M: No. Wiener and McCulloch were first partners in this thinking, and then became rivals when McCulloch went to MIT. As long as McCulloch stayed at Illinois and Wiener at MIT they were working right together. With both of them at the MIT they became totally alienated, and then Walter Pitts got involved. He was the youngest member of the group.

B: Oh god, he was so clever. You'd set him a problem, you know, and he would reach up to his hair and take a couple of strands, and he would say, 'Well, now, if you say that, you see, um, no then, you see,' and he'd work it all out with his hair.

M: He was a very odd boy. Now, one of the important points at this stage was one that Gregory kept making, that a possible cross-disciplinary mathematical language was available. We never got very far with that because all you could ever get out of people like Wiener was, 'You need a longer run.' We used to drive them absolutely out of their minds because they were not willing to look at pattern, really. What they wanted was a terribly long run of data.

B: Of quantitative data, essentially.

M: Quantitative data, and we never got them really to look at the problem of pattern. Von Neumann came the closest to it.

B: Yes, he was in games theory, you see.

SB: How many of you were thinking you had some kind of a general solution?

M: Gregory thought so, and Larry Frank thought so, Evelyn Hutchinson; we had Ross Ashby over, how about Savage?

B: I don't think so, no. You see, one of the essentials, Stewart, for understanding it, was to have been brought up in the age when it wasn't there, when purpose was a total mystery. **Naven** is a disciplined book, written without teleology. The rule was that you must not invoke teleology. Now, people like Savage, who was a mathematician, for one thing he never faced biological data, you see. He didn't know what a mystery it is that you have a nose between two eyes, and you don't have noses on the outside here, you know. All that sort of mystery wasn't a question for him. Now, if you say to somebody like that, 'Why is the trunk of an elephant a nose?' they can't tell you without an awful sweat that it's because it's between two eyes. The formal-puzzle has never been presented to them.

M: I remember Robert Merton saying once that there wasn't a person in the country who was thinking hard about problems who didn't have a folder somewhere marked something like 'circular systems.'

Horney's book, **The Neurotic Personality of Our Times**⁷ discusses the vicious circle, and interventions in the circle, and the effect of intervention. Milton's paper on that girl with migraine headaches and quarrelling with her friends, there was lots of stuff around ...

B: On positive feedback.

M: But also about possible intervention.

B: But the essence of the other thing is that it's not an intervention.

M: Yes, but an intervention is a precursor of thinking of ...

B: Yes, yes. All cybernetic entities are displaced small boys.

M: Displaced small what?

B: Boys. They're jacks. You know what a jack is? A jack is an instrument to displace a small boy. A boot jack is a thing for pulling off boots 'cause you haven't a small boy to pull it off for you.

M: I'll remember that next time. This is an English joke that no one will understand.

B: I can't help it. On the first steam engines, you've got a pair of cylinders and you've got valves, and you pull this valve to run the steam into this one, close it, let it drive the piston, pull it - this is done by hand. Then they invented the idea of having the flywheel control the valves. This displaced a small boy.

SB: The governor displaced another one?

B: And the governor displaced another small boy, who was to keep the engine going at a constant rate, that's right. Now then, the John Stroud stuff is the study of the psychology of the human being between two machines.

In any device such as an ack-ack gun you've got a whole series of small boys in the situation of being between a machine and another machine. What John Stroud worked on was the psychology of that situation. He found what I still think are some very interesting things, namely that the orders of equations (you know, equations in X , or in X^2 , or X^3 , or whatever) are discontinuous in the human mind, as well as

being discontinuous in mathematical paper work. Where is John Stroud now, do you ever see him?

M: He is retired, teaching at Simon Fraser somewhat, and he's been brought back by Gerry O'Neill into discussions of space colonies.

SB: Good lord.

M: He was very much interested in space colonies. He told me all about them twenty-five years ago, and I was interested in all the problems then, the selection of people, and what not.

B: Stewart, you should get hold of John Stroud.

M: Now Gerry has John Stroud's manuscript and he's not going to read it until he's finished his own. I said, 'I think that's unscientific and childish.'

B: He wants credit for inventing anything that John Stroud had invented.

M: Well, he did invent it separately, that's true, and he wants to prove it, because after all, what does a physical science have in the world except priority? I don't blame them you know, because they haven't got anything else. All they're interested in is priority. They spend weeks and months discussing priority. It's so boring. Somebody mailed a letter three days before somebody else did, and they made a whole meeting about it.

SB: Margaret, what was your perception at the time of the early Macy meetings as to what was going on?

M: The thing that cybernetics made the most difference to me, aside from all the things that you know, in the social organisation field, was the interaction between the mother and child. There had been too much emphasis that there were temperamental differences among children, so that you responded differently to a hyperactive baby than you did to a quiet baby. But the extent to which there was a system in which the mother was dependent on what the child had learned as the stimulus for the next position wasn't well articulated until we got the cybernetics conferences going.

B: The link-up between the behavioral sciences spread very slowly and hasn't really spread yet. The cyberneticians in the narrow sense of the word went off into input-output.

SB: They went off into computer science.

B: Computer science is input-output. You've got a box, and you've got this line enclosing the box, and the science is the science of these boxes. Now, the essence of Wiener's cybernetics was that the science is the science of the whole circuit. You see, the diagram ...

M: You'd better verbalize this diagram if it's going to be on the tape.

B: Well, you can carry a piece of paper all the way home with you. The electric boys have a circuit like that, and an event here is reported by a sense organ of some kind, and affects something that puts in here. Then you now cut off there and there, then you say there's an input and an output. Then you work on the box. What Wiener says is that you work on the whole picture and its properties. Now, there may be boxes inside here, like this of all sorts, but essentially your ecosystem, your organism-plus-environment, is to be considered as a single circuit.

SB: The bigger circle there ...

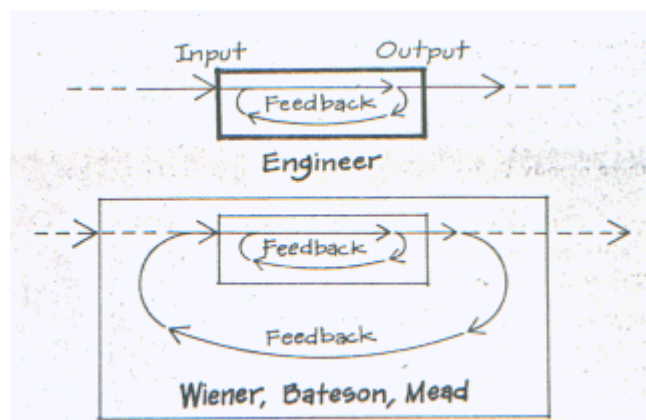
B: And you're not really concerned with an input-output, but with the events within the bigger circuit, and you are part of the bigger circuit. It's these lines around the box (which are just conceptual lines after all) which mark the difference between the engineers and ...

M: ... and between the systems people and general systems theory, too.

B: Yes.

SB: A kind of a Martin Buber-ish breakdown, 'I-it', where they are trying to keep themselves out of that which they're studying. The engineer is outside the box ... and Wiener is inside the box.

B: And Wiener is inside the box; I'm inside the box ...



M: I'm inside the box. You see, Wiener named the thing, and of course the word 'cybernetics' comes from the Greek word for helmsman.

B: It actually existed as a word before Wiener - it's a nineteenth century word.

M: Yes, but he wrote the book **Cybernetics**⁸ and sort of patented the idea to that extent. And then he went to Russia, and was very well received. The Russians were crazy about this right away - it fit right to their lives. But one of the big difficulties in Russian psychology is that they have great difficulty learning that anything's irreversible. So cybernetics spread all over the Soviet Union very rapidly, and in Czechoslovakia, whereas what spread here was systems theory instead of cybernetics.

SB: How did that happen? It seems like something went kind of awry.

M: Americans like mechanical machines.

B: They like tools.

SB: Material tools more than conceptual tools.

B: No, because conceptual tools aren't conceptual tools in America, they're not part of you.

SB: How about McCulloch? He loved machinery. Did he also see himself as inside the box?

M: Well, one of the things he spent a great deal of time on was perception machines, separate sensory apparatus for the deaf or the blind.

[After reminiscence about other meetings following the Macy period the subject 'feed forward' comes up.]

B: As far as I was ever able to make out 'feed forward' was implicit and more or less explicit in the original Wiener paper. The feed forward process is what you get by using not the primary variable, but the derivative of the variable. You've got a machine for steering a ship, an automatic steerer, and you set her loose in the Atlantic, and you want her to go to London or some such place: she's to sail east. You have a compass card, and you measure the error between the compass card and the direction the ship's pointing, and you use that angle to control the steering machine, which pulls a rudder this way and that, right? Depending on the error. So, when the error is northward, the machine tells the rudder to swing it across southward, right? When it is going due east, the ship has way on, rotational momentum, and is going to go

way over to the south of east, and now it's going to yawing all across the Atlantic, right?

SB: This is hunting, technically.

B: This is technically hunting, and if you want to cut that down, what you've got to do is to have a machine on top of that machine, another machine which measures the rate at which the ship is correcting its error. The faster it is correcting its error, the slower you have it correct its error. It will then, you see, actually hold itself before it gets to due east. If you've ever handled the tiller of a small boat, you know the problem.

SB: It's a double layer in other words. The first machine is treating the boat as something which needs to be given negative feedback, and then the second machine is treating the first machine as something which needs to be given negative feedback.

B: That's right. You've got a hierarchy of logical types there, and their various complexities.

SB: Is feed forward a kind of a discounting of part of the corrective signal?

B: It's a discounting of the corrective signal in terms of the error which the corrective signal will generate if allowed to continue.

M: Now, Stewart, what we thought we were going to talk about, but you didn't let us say what we wanted to talk about, you started something else under the pretense that you wanted to start the tape recorder (I want to point out I followed all those manoeuvres) what we had said we were going to talk about was the need of having 'some data flowing through the system.'

SB: Some data flowing through the system?

B: Yes. I set my classes an assignment. If they can, they will handle it purely abstractly. And they then get off into an awful mess of ill-drawn abstractions which act upon other ill-drawn abstractions. But if you can make them fool around with data of any sort, while they're playing with the abstractions, then you get something. I keep a fish tank going there, because a fish tank is a nice thing, really, to have in the back of your mind while you're thinking about ever it might be. Norbert Wiener, when he had a problem, used to sit with the wind blowing on a curtain.

M: I thought that was von Neumann.

B: It could have been von Neumann. Pitts did it by disturbing his hair.

Now, this goes along with: 'always the multiple approach.' Any Hebrew poetry is like this. 'The candles are white, as translucent fishes,' you know. 'Lilies for joy, and lilies for funerals.'; 'How are the mighty fallen and the weapons of war perished.' You get away from the pure verbalism by double-phrasing.

You make two statements, and what is true of both of them is the formal truth. This is what is called explanation.

SB: It's not that it's a repetition of the message, it's different derivations of the same message from different sources.

B: Often. In psychoanalysis if you can recognize the same formal pattern in a dream and in a childhood memory and in how you're treating your analyst, you will say, 'Aha, it's true.' You've got it.

M: and when your studying a culture.

SB: What would be an example there?

M: Well, you find the same pattern recurring in different aspects of the culture. You find, for instance, a

house in which there's no ornamentation inside, all the ornamentation's on the gate. You find a people who are preoccupied with the external aspects of their skin and believe that any breakage will impair them so that they're imperfect for something else, and so forth. With that kind of understanding, if you're told something, you can tell whether it fits or not.

For instance, the Balinese told us that they had marriage by capture, which didn't suit anything we'd understood about their culture. Our cook was going to carry off a girl by capture, so Gregory went outside the gate with him early in the morning, and the girl was there waiting. They looked around and there was nobody else there, so she trotted off with him. If there had been another group there, she would have pretended, she would have screamed and been carried off, because that was correct etiquette.

Then we had the case of a very stupid boy who thought it was true. He carried off a girl who had already planned to elope with somebody else. It took the society months to sort that out.

From a complex culture like Bali you take a lot of chunks - birthday ceremonies and funeral ceremonies, children's games, and a whole series of things, and then you analyze them for the patterns that are there.

B: In latmul they have flutes. The flutes are long hollow bamboo, an inch and a half thick, five feet long, and one hole here, which you blow across. And from that you can get about five notes by overblowing, by harmonics. All right. You have a flute, you're blowing with me, and yours is tuned one tone higher than mine. So your harmonics fit between mine, right? Between us we've got quite a bit of scale. If we blow alternatively we can make a tune. Well, now this is how the generations are arranged. The grandparents go with the grandchildren, and the initiation grades are like seniors - juniors - sophomores - freshmen, in which when you get a fight over initiation, the seniors and the sophomores go together, and the juniors and the freshmen go together. And so on.

SB: So what is the truth?

B: The truth is that latmul like to make this pattern. This is a pattern of organization that they think is nice.

M: When Gregory and Waddington talked to each other, I learned what I know about the way English biologists think, by listening to the two of them. They would pick their illustrations right across the field. One minute from embryology, the next from geology, the next from anthropology, back and forth, very freely, so that the illustrations from one spot illuminated, corrected and expanded the one from another. This is the thing that Americans are not taught to do. In our school system you have one year of chemistry, then you're through with chemistry probably, and then you have one year of physics. Whereas in the English system they took all of them at once in smaller doses that went along.

Nora Bateson: Goodbye.

M: Goodbye. Are you going to school now?

Nora: Yes.

M: Well, it's been lovely to see you, Nora.

Nora: It's been lovely to see you, too. Bye bye.

B: Goodbye.

Nora: Bye Daddy.

SB: Margaret, an old student of yours told me you have a list of reliable sources of insight. What's the list?

M: I used to say to my classes that the ways to get insight are: to study infants; to study animals; to study primitive people; to be psychoanalyzed; to have a religious conversion and get over it; to have a psychotic

episode and get over it; or to have a love affair with an Old Russian. And I stopped saying that when a little dancer in the front row put up her hand and said, 'Does he have to be old?'

SB: How many of those have you done?

[Blank here while cassette was changed. Dr. Mead said she had studied infants and primitive people. When she got to animals in the list, the conversation swerved to Konrad Lorenz.]

M: Watching Konrad Lorenz be simultaneously a bird and a worm, is one of the really magnificent things in the world. You've seen that, haven't you, when he's describing a bird catching a worm, and he's both? Talk about the whole system, there it is.

B: One of the things I've already regretted is that I didn't film him lecturing in Hawaii. One could've, I think. Lorenz is an Aurignacian.

SB: How do you mean?

B: I mean that he is identified with animals. Aurignacians are the people who did the cave paintings, the good ones. Lorenz goes to the blackboard and there is a live dog, hesitating between attacking and retreating. He takes the eraser, and he wipes the tail off, changes the angle by ten degrees, and flattens out the hair on the back of the neck, and he says, 'That dog's going to run.' He sticks it the other way, and 'that dog's going to attack.' And he is the dog while he's talking about it. And this goes for cichlid fishes, bees, any goddamn thing. And then, in the final lecture he gave in Hawaii, he got all mixed up, you know, the way scientists do, with physics and the Einsteinian universe, and his body got twisted, as he started to talk about the Einsteinian universe where the straight lines are not straight anymore. That's what I wish I had on the camera. The others all think that this is very unfair you know, he has all of this information that they simply don't have.

M: He contributes tremendous zest. If Lorenz is in a meeting, I can retire and take notes and think and have no responsibility to keep it going; whereas if he isn't there, I very often have to keep it awake....

Gregory, have you any ideas on the subject of the harm that is done by television because of the rigidity of the body of people watching TV? Sartre discussed at one point what happened when you peek into a keyhole. When you look through a keyhole, the whole body is focussed to try to use this very small aperture, and he described what happens if you touch somebody who is looking through a keyhole. They jump. I have a big set, now, of comparative pictures of family groups (they weren't taken for this, they were taken for family albums) reading and looking at TV. When the family is reading, they're a thousand years away from each other, their eyes are all down, but you get a sense of community and relaxation. Their bodies are very loose, and undoubtedly there's movement going on as they read. But when they're watching television, the same people sit like this, they don't touch each other, and they're very rigid.

We have lots of material that if you move in your mind, your muscles don't get stiff. For years we had this very funny problem with catatonics, such as a man who would stand all day long in a ward with his eyes up and his hands together in prayer, never moving. They'd pick him up at night, tip him into a bed, feed him artificially, and then after five years or something, there'd be a fire. He'd walk across the ward, pick up a telephone, report, 'Fire in ward five,' help get all patients out, and then when the fire was out, back he went to his position. But he was not stiff. Whereas if you take the ordinary person and put them in bed for three months, they have to relearn how to walk. All the data we now have on monitoring muscles with tiny transistor monitors shows, if you think about skiing or exercising, the muscles that you use to ski will respond.

If you inhibit movement, as one does watching TV, with no empathy, no muscular involvement at all, I think this is the thing that's doing harm.

B: I was wondering about looking through, for example, a camera.

M: Remember Clara Lambert and when you were trying to teach her? That woman who was making photographic studies of play schools, but she was using the camera as a telescope instead of as a camera. You said, 'She'll never be a photographer. She keeps using the camera to look at things.' But you didn't. You always used a camera to take a picture, which is a different activity.

B: Yes. By the way, I don't like cameras on tripods, just grinding. In the latter part of the schizophrenic project, we had cameras on tripods just grinding.

M: And you don't like that?

B: Disastrous.

M: Why?

B: Because I think the photographic record should be an art form.

M: Oh why? Why shouldn't you have some records that aren't art forms? Because if it's an art form, it has been altered.

B: It's undoubtedly been altered. I don't think it exists unaltered.

M: I think it's very important, if you're going to be scientific about behavior, to give other people access to the material, as comparable as possible to the access you had. You don't, then, alter the material. There's a bunch of film makers now that are saying, 'It should be art,' and wrecking everything that we're trying to do. Why the hell should it be art?

B: Well, it should be off the tripod.

M: So you run around.

B: Yes.

M: And therefore you've introduced a variation into it that is unnecessary.

B: I therefore got the information out that I thought was relevant at the time.

M: That's right. And therefore what do you see later?

B: If you put the damn thing on a tripod, you don't get any relevance.

M: No, you get what happened

B: It isn't what happened.

M: I don't want people leaping around thinking that a profile at this moment would be beautiful.

B: I wouldn't want beautiful.

M: Well, what's the leaping around for?

B: To get what's happening.

M: What you think is happening.

B: If Stewart reached behind his back to scratch himself, I would like to be over there at that moment.

M: If you were over there at that moment you wouldn't see him kicking the cat under the table. So that just doesn't hold as an argument.

B: Of the things that happen the camera is only going to record one percent anyway.

M: That's right.

B: I want one percent on the whole to tell.

M: Look, I've worked with these things that were done by artistic film makers, and the result is you can't do anything with them.

B: They're bad artists, then.

M: No, they're not. I mean an artistic film maker can make a beautiful notion of what he thinks is there, and you can't do any subsequent analysis with it of any kind. That's been the trouble with anthropology, because they had to trust us. If we were good enough instruments, and we said the people in this culture did something more than the ones in that, if they trusted us, they used it. But there was no way of probing further material. So we gradually developed the idea of film and tapes.

B: There's never going to be any way of probing further into the material.

M: What are you talking about, Gregory? I don't know what you're talking about. Certainly, when we showed that Balinese stuff that first summer there were different things identified - the limpness that Marion Stranahan identified, the place on the chest and its point in child development that Erik Erikson identified. I can go back over it, and show you what they got out of those films. They didn't get it out of your head, and they didn't get it out of the way you were pointing the camera. They got it because it was a long enough run so they could see what was happening.

SB: What about something like that Navajo film, 'Intrepid Shadows?'¹⁰

M: Well, that is a beautiful, an artistic production that tells you something about a Navajo artist.

B: This is different, it's a native work of art.

M: Yes, and a beautiful native work of art. But the only thing you can do more with that is analyze the film maker, which I did. I figured out how he got the animation into the trees.

B: Oh yes? What do you get out of that one?

M: He picked windy days, he walked as he photographed, and he moved the camera independently of the movement of his own body. And that gives you that effect. Well, are you going to say, following what all those other people have been able to get out of those films of yours, that you should have just been artistic?

SB: He's saying he was artistic.

M: No, he wasn't. I mean, he's a good film maker, and Balinese can pose very nicely, but his effort was to hold the camera steady enough long enough to get a sequence of behavior.

B: To find out what's happening, yes.

M: When you're jumping around taking pictures ...

B: Nobody's talking about that, Margaret, for God's sake.

M: Well.

B: I'm talking about having control of a camera. You're talking about putting a dead camera on top of a bloody tripod. It sees nothing.

M: Well, I think it sees a great deal. I've worked with these pictures taken by artists, and really good ones...

B: I'm sorry I said artists; all I meant was artists. I mean, artist is not a term of abuse in my vocabulary.

M: It isn't in mine either, but I ...

B: Well, in this conversation, it's become one.

M: Well, I'm sorry. It just produces something different. I've tried to use 'Dead Birds,'¹¹ for instance ...

B: I don't understand 'Dead birds' at all. I've looked at 'dead Birds,' and it makes no sense.

M: I think it makes plenty of sense.

B: But how it was made I have no idea at all.

M: Well, there is never a long-enough sequence of anything, and you said absolutely that what one needed was long, long sequences from one position in the direction of two people. You've said that in print. Are you going to take it back?

B: Yes, well, a long sequence in my vocabulary is twenty seconds.

M: Well, it wasn't when you were writing about Balinese films. It was three minutes. It was the longest that you could wind the camera at that point.

B: A very few sequences ran to the length of the winding of the camera.

M: But if at that point you had a camera that would run twelve hundred feet, you'd have run it.

B: I would have and I'd have been wrong.

M: I don't think so for one minute.

B: The Balinese film wouldn't be worth one quarter.

M: All right. That's a point where I totally disagree. It's not science.

B: I don't know what science is, I don't know what art is.

M: That's all right. If you don't, that's quite simple. I do. *[To Stewart:]* With the films that Gregory's now repudiating that he took, we have had twenty-five years of re-examination of the material.

B: It's pretty rich material.

M: It's rich, because they're long sequences, and that's what you need.

B: There are no long sequences.

M: Oh, compared with anything anybody else does, Gregory.

B: But they're trained not to.

M: There are sequences that are long enough to analyze ...

B: Taken from the right place!

M: Taken from one place.

B: Taken from the place that averaged better than other places

M: Well, you put your camera there.

B: You can't do that without a tripod. You're stuck. The thing grinds for twelve hundred feet. It's a bore.

M: Well, you prefer twenty seconds to twelve hundred feet.

B: Indeed, I do.

M: Which shows you get bored very easily.

B: Yes, I do.

M: Well, there are other people who don't, do you know? Take the films that Betty Thompson studied.¹² That Karbo sequence - it's beautiful - she was willing to work on it for six months. You've never been willing to work on things that length of time, but you shouldn't object to other people who can do it, and giving them the material to do it.

There were times in the field when I worked with people without filming, and therefore have not been able to subject the material to changing theory, as we were able to do with the Balinese stuff. So when I went back to Bali I didn't see new things. When I went back to Manus, I did, where I had only still photographs. If you have film, as your own perception develops, you can re-examine it in the light of the material to same extent. One of the things, Gregory, that we examined in the stills, was the extent to which people, if they leaned against other people, let their mouths fall slack. We got that out of examining lots and lots of stills. It's the same principle. It's quite different if you have a thesis and have the camera in your hand, the chances of influencing the material are greater. When you don't have the camera in your hand, you can look at the things in the background.

B: There are three ends to this discussion. There's the sort of film I want to make, there's the sort of film that they want to make in New Mexico (which is 'Dead Birds,' substantially), and there is the sort of film that is made by leaving the camera on a tripod and not paying attention to it.

M: Who does that?

B: Oh, psychiatrists do that. Albert Schefflen¹³ leaves a video camera in somebody's house and goes home. It's stuck in the wall.

M: Well, I thoroughly disapprove of the people that want video so they won't have to look. They hand it over to an unfortunate student who then does the rest of the work and adds up the figures, and they write a book. We both object to this. But I do think if you look at your long sequence of stills, leave out the film for a minute, that those long, very rapid sequences, Koewat Raoeh, those stills, they're magnificent, and you can do a great deal with them. And if you hadn't stayed in the same place, you wouldn't have those sequences.

SB: Has anybody else done that since?

M: Nobody has been as good photographer as Gregory at this sort of thing. People are very unwilling to do it, very unwilling.

SB: I haven't seen any books that come even close to **Balinese Character**.¹⁴

M: That's right, they never have. And now Gregory is saying it was wrong to do what he did in Bali. Gregory was the only person who was ever successful at taking stills and film at the same time, which you did by putting one on a tripod, and having both at the same focal length.

B: It was having one in my hand and the other round my neck.

M: Some of the time, and some not.

B: We used the tripod occasionally when we were using long telephoto lenses.

M: We used it for the bathing babies. I think the difference between art and science is that each artistic event is unique, whereas in science sooner or later once you get some kind of theory going somebody or other will make the same discovery.¹⁵ The principal point is access, so that other people can look at your material, and come to understand it and share it. The only real information that 'Dead Birds' gives anybody are things like the thing that my imagination had never really encompassed, and that's the effect of cutting off joints of fingers. You remember? The women cut off a joint for every death that they mourn for, and they start when they're little girls, so that by the time they're grown women, they have no fingers. All the fine work is done by men in that society, the crocheting and what not, because the men have fingers to do it with, and the women have these stumps of hands. I knew about it, I had read about it, it had no meaning to me until I saw those pictures. There are lots of things that can be conveyed by this quasi-artistic film, but when we want to suggest to people that it's a good idea to know what goes on between people, which is what you've always stressed, we still have to show your films, because there aren't any others that are anything like as good.

SB: Isn't that a little shocking? It's been, what, years?

M: Very shocking.

B: It's because people are getting good at putting cameras on tripods. It isn't what happens between people.

M: Nobody's put any cameras on tripods in those twenty-five years that looked at anything that mattered.

B: They haven't looked at anything that mattered, anyway. All right.

SB: I have a question that maybe relates to that, maybe not. What about field workers that join the tribe? Frank Cushing with the Zuni, and Carlos Castaneda, and such.

M: Castaneda hasn't joined the tribe.

SB: He hasn't joined the tribe, he's tried to join the practice.

M: No, only intermittently. We have examples. Edmund Carpenter's been making a study of these people.

SB: I've got some, too. Everyone that knows anthropologists knows someone who's a little wiggy because of some overzealous participation in something. And I wonder about that.

M: It's the temptation for another culture. We also have a case of a man who was studying the Chinese, and he married a Chinese girl. Which he then thought was enough anthropology; for quite a while he couldn't do anything else. It's a lot easier to study the culture where you can't marry people, where there's such a gulf that that kind of over-identification doesn't occur. The minute you study a culture where you might marry them, or adopt their children, or be adopted by them, you get complications. Extreme ones.

SB: Sometimes the ones who lose track of where they are, they find a place to be confused between, and proceed to be confused between it.

M: I think that's a function of people who are confused wherever they are, anyway. One difference between pre-World War II anthropology and post-World War II is that most of us who did the pre-war work grew up in reasonably coherent cultures, and we knew what a pattern was when we saw one.¹⁶

Remember that paper that I wrote that you invented the word 'quizbits' for? (But the paper is called 'Customs and Mores.'¹⁷) It was a discussion of the extent to which information was being broken up into meaningless bits and fed to people. All your experience is chopped, everything out of scale. The news over the radio - one event is of world significance, the next is nothing. Contemporary young people have had the things that are presented to them so chopped up.

SB: You mean just the speed with which they change, or the lack of integration?

M: The lack of integration. You get it on radio in New Guinea: 'Kruschev has been deposed, there was a jewel robbery in the American Museum of National History, two small boats off Port Moresby have sunk,' that's the news.

[Lois Bateson leans in on her way to an errand. Margaret will be gone by the time she returns. 'OK, you people. See you in a while. I'm really glad you came, Margaret. Come again.']

M: Well, it's been lovely to see you.

SB: You mentioned Gerry O'Neill a while ago, as though you're somewhat involved in the space colony business.

M: Well, I've been interested in them, because of the possibilities of diversity. You see, I've always lived the Pacific islands, because they have such high degrees of diversity. When John Stroud first told me about space colonies, the picture was that you could have an area about the size of Los Angeles, and they would be undisturbed for 1500 years, so they could vary.

SB: I have a question that goes back to the Macy conferences, and it also relates to projects that Gregory's getting interested in now. What is the history of the failure of conceptual cybernetics to become public knowledge? You said that the later Macy meetings were starting to get a sense that you had something that everybody ought to know.

M: It wasn't quite as deep as that. We thought we had something that would be cross-disciplinary language. The meeting we had with the Academy of Sciences was to include more of the scientific community. Now, I worked a lot on that idea. **Continuities in Cultural Evolution** deals with the fact that in social science, unless you can carry the public with you, you can't use your findings. I think that we could trace part of the lack of response to the American preference for linear sequences, which is very high. It's like the Manus, too. They're both moral cause-and-effect societies. You do this, and that happens.

A problem I was going to raise, to Gregory, is why do you think the United States has more run-away positive feedback than most cultures?

SB: Such as?

M: Such as, gasoline taxes that can only be used on roads. With the tax you build more roads, which makes it possible to have more cars, which uses more gasoline. It's a perfect endless runaway. We have hundreds of them in this country.

B: I think one of the things that's serious in this country is using the value one can catch hold of, rather than the real value. Such as, catching gangsters for their income tax returns. I had a whole series of examples of this at one stage. The actual feedback circuit runs upon a collateral variable and you don't, from the circuit, get insight into the whole structure.

SB: Still, you say that's good when it comes to something like oxygen or carbon dioxide being the controller of breathing rates.

B: This is exactly the problem. When do you want to do this, and where do you want not to do it? At one

stage I was saying, the thing to do is never to use the lethal variable to control the feedback.

SB: The lethal variable?

B: This was in asphyxia. The rate of breathing is not affected by the lack of oxygen but by the surplus of carbon dioxide in the blood. If you try to regulate by the lack of oxygen, it's already too late.

M: Well, we said the same thing, that if you're teaching children nutritional habits, don't do it with something that's related to nutrition. It's much safer to say, 'We'll take you to a circus if you eat your spinach,' than to say, 'If you eat your spinach, you can have ice cream.'

SB: Safer means?

M: Safer socially when you're bringing up children and you want them to learn to eat nutritionally good diets effortlessly. When you tie the eating to the food itself, if a child wants to fight about it, they fight about it by not eating the right food. So that you put all the trouble in the system. The Balinese say to a child when dressing a cut, 'Listen to the gong, listen to the orchestra, go and see an orchestra!' There's no orchestra. She's just presenting something pleasant. Italian mothers do that. They say, 'Ice cream! Ice cream! Lovely ice cream! Lovely ice cream!' while the child's having a cut fixed. Americans say, 'But she didn't mean it, she didn't give him any ice cream.' Watergate, of course, was an outstanding example. We didn't have to get Nixon because of Watergate. We were using Watergate because he was taking the country apart and had delusions of grandeur. I think if we'd known about the income tax, that would have done it.

I've wondered, Gregory, whether all of these things go together - the non-recognition of cybernetics in this country, as compared with the Soviet Union. I figure that they have about a hundred times as many people that understand the whole thing.

SB: Understanding it creatively and coming up with new thoughts?

M: That one doesn't know. We do know that they are using it for purposes of social organization, especially in Czechoslovakia.

B: At that first cybernetic meeting we had a Russian talk. He hadn't much idea what it was all about, I thought.

M: I don't remember what he said at all.

B: He had fifteen slides of circuit structures that would do various sorts of things like pattern recognition, or control temperature, or something. It was a sort of about the level of the McCulloch and Pitts papers. Without, as far as I could make out, the enormous theoretical spin-off that those papers had.

M: And evidently I wrote him off so I can't remember.

B: I think you wrote him off. I wrote him off very quickly.

M: Well, we had a period where I thought we could take cybernetics and use it as a language for communicating with the Russians, and then somebody in this country decided that the Russian cybernetic were very dangerous, and we had a big intelligence report on cybernetics. It ceased to be politics-free and was no longer useful. I wrote a discussion to that, and decided anyway, that instead of having a methodology or conceptual scheme for communication, it was much better to have agreed-on sub goals for communication between two systems as antithetical as the Soviet and U.S.¹⁹

SB: Well then something funny seemed to happen with the whole general systems bunch. I've never understood that.

M: Well, there are a dreadful lot of systems people in the Society for General Systems Research. Then

von Bertalanffy died. Anatol Rappaport runs a very isolated group. Now, when the Society for General Systems Research was formed in Atlanta, and Anatol was in the chair (I had never met him), and Ross Ashby was there on the front row, and there were about twenty people there, I went back to the correspondence, Gregory, where you had proposed that we plan an organization in relation to its purposes. This was before the cybernetics meetings, while you went overseas. When the Society for General Systems Research was formed, I proposed that we apply general systems to our society. Nobody knew who I was and I was feeling like the little old lady in tennis shoes. I went up at the end of it and talked to Ashby, and he said, 'You mean we should apply our principles to ourselves?'

B: I what tone of voice?

M: He was repudiating it, in a light playful voice that was appropriate, but he was repudiating it.

SB: So it was stillborn.

M: So now, the Society for General Systems Research, which is proliferating, is proliferating by the standard methods that are used in this country - regional chapters. I said to Dick Erickson, 'I don't think we should be so conventional, we ought to think of something better.' We can't get anybody to use any kind of constructive thinking on the problems of organization. And, of course, there's no place where you can get a well rounded degree in General System Theory. Rand has a school that is almost entirely military.

One of the most crazy situations - I was asked to speak at a dinner of the Air Force celebrating their fifth decade of Air Force intelligence. I talked about the fact that they weren't paying attention to the whole; the Air Force was modeling the Soviet Union as a system, and the Army was modeling the United States as a system, using different units, and they were both ignoring the fact that China existed, and therefore were making hopeless mess when you knew you had a universe to deal with. What I was telling them was to use cybernetic thinking as it had developed into general systems theory. The next morning I was on a chartered plane bringing me back, and there was a man on it who said, 'You left me way behind. I couldn't understand a word you said.' I said, 'What are you?' He said, 'I'm an electronic specialist.'

Americans are always solving problems piece-meal. They're always solving them *de nouveau* and artificially because they're all newcomers and they don't have decisions grounded in a culture.

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** I am told a paper by W. Ross Ashby predated this by a year but we didn't know it. - Mead

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