

## Jerome Kagan

- Born 02/25/1929 in Newark, NJ
- Spouse - Cele (Katzman) Kagan
- Ph.D. from Yale University (1954); B.A. from Rutgers University (1950)

### Major Employment:

- US Army Hospital at West Point - 1955-1957, Psychologist
- Ohio's Fels Institute - 1957-1964, Psychology
- Harvard University -1964-Retirement, Psychology

### Major Areas of Work:

- Temperament, Cognitive and Emotional Development

### SRCD Affiliation:

- SRCD - Member



## SRCD ORAL HISTORY INTERVIEW

### Jerome Kagan

Interviewed by Robert McCall  
1993

#### McCall: What got you into psychology in the first place?

Kagan: I think it is a combination of several things. First, it is like the unleashed historians; you got deep water and then you got the surface water; I started with the deep water. I think in the deep water there is a basic interest in how a child becomes an adult. I am sure of that; that is the deepest premise. But now as we go to the surface, what does every adolescent do? He is trying to figure out where his talents are: what is he good at, what is he bad at? So as I reflect on my college years, I was certainly interested in science and interested in, like most young people, challenging science, hard science and actually had a strong interest in chemistry. So I was weighing the attraction to the challenge of chemistry like most undergraduate students who major in math are interested in psychology, but they find psychology too easy. So I was like that undergraduate, but as the college years went by I said, "Well gee, you want to be the most talented right?" I wasn't feeling that I was going to be that creative in chemistry and sometimes anecdotal experiences occur, which you weigh too heavily, but they crystallize things for you. I was the quantitative analysis course that was the barium sulfate determination. Remember how I can remember it? You go to determine how many milligrams of barium sulfate are in the unknown vial; it is a six-hour determination. I bring my white precipitate to the balance and then I go back to my desk, and my partner, the guy next to me, is carrying his through the balance and he drops it. So there is some barium sulfate powder on the floor of the laboratory and he says, "I am not going to do it again." So there were eight of us left at about 5:30 and he says, "Would you estimate how many milligrams took the mean." He got an A and I got a B+.

#### McCall: And, you checked out of chemistry.

Kagan: I said to myself, "This is not for me."

**McCall: You are going to a precise science.**

Kagan: Aye, that is right. I said to myself, "This is not my field." It is experiences like that. Then there are other anecdotes I remember. As a junior, I remember taking a psych course and the instructor, who never did any research, says, "Wait after class. Let's walk across the campus. I have been watching you." He had no right to say this, but he said, "You would be a good psychologist." Well, I took that very seriously; I took that as a prescription.

**McCall: Did he say why?**

Kagan: He said, "You ask good questions, you are thinking good." It is little things like that; alone they are unimportant, but they are built in your heart as an interest in psychology. Without the interest in psychology those events are unimportant, but given the base, they move you. And you know this, I think I told you this, I was still ambivalent. When I was a senior I applied to both biochemistry and psychology, and I got into both. I applied to The University of Texas and got in, and I applied to Yale and you know the story; I have told you this. The single event for me was that I was still good at chemistry. Psychology seemed soft and just didn't have the same story we see today; it did not have the elegance or challenge. Then it is the spring of 1950 and I was reading Head's *Organization of Behavior*, which I took out from my local library (population 25,000). I had no right to have the *Organization of Behavior*, and I sometimes think about that because it would be like going to a small town in America and them having Patricia Churchland's new book on neurobiology. You wonder why should my local library have this, but it did. I am reading this, and I get a letter from Frank Beach at Yale saying that I have been admitted to Yale and, "If you come, I would like you to be my research assistant." So I turn to the bibliography of Head's book and the whole first page is a bibliography reference to Frank. I am only 21 years-old, so I said, "God, Jesus, this guy Head thinks that Mr. Beach is a pretty important scientist and this guy Beach wants me to work with him." I didn't have that back way; it was the admission letter. Now again, without a deepest interest in psychology, I decided to go to psychology.

**McCall: So the barium sulfate occurred in high school or college?**

Kagan: College.

**McCall: And you had the same sort of experience, I recall, when you decided to leave Fels and go to Harvard, same confluence of sort of happenstance circumstance?**

Kagan: What was that?

**McCall: You told me that you visited Harvard; you came back and had lunch at a wayside table on the road.**

Kagan: Aye, that is right. I couldn't decide because I liked Fels, and we were happy there; I could not imagine a better environment for my family or for me. I really didn't want to go to Harvard, and I held the Dean up for two years. Finally, Dave McClelland who was the Chairman said, "That's it; the Dean will not wait anymore." It was the summer, and Cel and I were vacationing in Maine. David has a summer home in Connecticut and he said, "I want you to stop by on your way back from Maine, and I want a decision that afternoon. It has been two years." So we are driving toward McClelland's house and we are an hour away and I haven't decided; I still have not decided. We stopped by a picnic table somewhere in Connecticut on our way to David's and there in an old picnic table carved up with hearts--you know MK loves CB--there in the middle of the table is carved Fels. I will never forget that.

**McCall: Why did you go to Harvard?**

Kagan: That's right. My story and the seal of myself was I am basically counter-phobic. That came from God, and I wasn't taking any orders. I will never forget that.

**McCall: And you went to Harvard?**

Kagan: I went to Harvard for the obvious reason. Let me see, it is 1964; I am 35 years-old; and I felt that if I didn't like it, I could go back; but if I don't do it, I may say, "Gee, why didn't I try it." That is what anybody would do, try it and if you don't like it, return to Fels.

**McCall: There aren't very many places you could return to.**

Kagan: That's right and notice with Fels gone, I made the right choice.

**McCall: I hustled in to fill your spot so you couldn't come back. You have spent your life studying womb to tomb, but you started telling me the roots of your interest in psychology started in high school. Was there nothing before that in your family life, in your childhood?**

Kagan: Oh sure, usually in psychology is not academic psychology. I think it is true of--maybe not today when it is a guild--but I think in my era and in era's before, the majority pick psychology because of a deep interest in their own development. So, that was also true for me. First born boy, my memory is that I was an anxious child; I am sure I thought about that. Two years ago, I read an interesting book called *Lonely Hearts of the Cosmos* by Dennis Overby. It is a brilliant book of the 14 leading cosmologists. What is nice about the book is he weaves beautifully modern cosmological theory with what these people are like. What comes through is that these young cosmologists, when they were 11, were staring at the sky all night; I never did that. What he is trying to say is that something mysterious happened to them; they were just interested in the sky. I was interested in people and how they develop. I don't think my upbringing was that much different from a neighbor a block away: Carl Sagan who chose astronomy, or another neighbor Ronald Breslow who chose chemistry. Those are mysterious things that have to do with family and temperament. I was interested in people/psychology, and I can't go any further. It is like saying to one of these cosmologists, "Why are you interested in the stars?" These are mysterious things, but I can say, "Yes, that was important, it remained important, and I think good scientists, good artists, good writers have to have something happen to them in their lives to make them interested in something."

**McCall: When you were at Yale you worked with Frank Beach on sexual behavior in rats and the modern field of child development is really populated with patriarchs and matriarchs, with people who are not trained in developmental. You got into clinical as well as running rats. How did you get into developmental?**

Kagan: Oh, that is the same thing. As I said to you, the attraction to psychology was not by sensation perception categorical means; it was a developmental question. What are the forces that make a child do well in school, not do well in school, worry, not worry, stutter, and not stutter? I stuttered for two years when I was in the first grade. Why did that happen? So my attraction to psychology was because of development. That is what I was interested in, not what is published in the *Journal of Experimental Psychology*.

**McCall: Even though you studied that at Yale?**

Kagan: When I went to Yale, I was more ingenious than most undergraduates. I went to my Chairman and said, "I am thinking of psychology, where should I apply?" The Chairman happened to be an older man, a graduate of Yale, and at the time Yale was good; it was that good. He said, "There is only one place to go, that's Yale." Remember, Neal Miller is there, Frank Beach, Carl Hubbeland is there, and Leonard Doob is there, some good people. He said, "You go to Yale." So, I applied to Yale. Now Yale was not that strong in developmental, but I went to Yale because I was authority-struck and he told me that was the only place to go. Remember that over at the Child Study Center it was Katie Wolf and Sybil Escalona. Bill Kessen got into developmental because of these people. Jerry Lesser, a man you haven't heard of, Martin Comb, a lot of developmentalists, and a lot of young graduate students chose

developmental because of that influence that came from the Child Study Center.

**McCall: Now who was at the Child Study Center?**

Kagan: At that time?

**McCall: All these people you just mentioned were students?**

Kagan: Aye, but they were influenced by Katherine Wolf and the entourage around her.

**McCall: People we don't remember in developmental.**

Kagan: Well, no; you have heard of Katie Wolf. She had the Wolf-Hexter Baby Test. It was an early Brazelton early Bailey. No, no one remembers Katie Wolf.

**McCall: Giselle --?**

Kagan: Well, Giselle exerted minimal influence in the 1950's. It was Katie, but you also have to realize that interest in developmental when we were graduate students was when Neal Miller and John Dollard published *Personality Psychotherapy*. It has a developmental harmony. You learn through conditioning. It begins at the very beginning. Neal never worked on children but the contrapathial harmony all starts with a Watsonian argument, which is a developmental argument.

**McCall: Ok. You get your degree in clinical?**

Kagan: Oh, no. In those years it was very easy. There were no different degrees; there was only one degree, not like Harvard or Yale psychology. Because I knew I was interested in children, I knew I would never be a clinical psychologist. I wanted to get some clinical experience though. What do mentally disturbed children look like? This is very common, and Yale had a very nice relationship with a child guidance clinic, Clifford Bears Guidance Clinic in New Haven. My predecessor was George Manwok who never became a child psychologist, but he did it for the same reason. So I went over and spent a year working; today we call it an internship. Not because I wanted to be a clinician, but because I wanted to see mentally troubled kids. That was a great year and that is why I did that. My degree was in psychology; my thesis was with rats, as you know.

**McCall: You are about to start out now on your career. What did you think at the time you wanted to do?**

Kagan: Very clearly you could tell it by my very first research project because my first job was at Ohio State. The key to understand is not about what happened to the child, but about the child's interpretation. My first publication on children--my first was actually on rats--but the first on children was a child's perception of his parents in the *Journal of Personality and Social Psychology*. What I did was make up TAT Light pictures, and you can imagine the study. You go to children and try to sneak behind their defensiveness and try to get at what their perception is of their home, family, and each parent. I knew that was what I wanted to do, and that was the work I did at Ohio State before I was drafted and went into the Army.

**McCall: You have very clear images of what you wanted to do. Was that ever a liability for you? You are a broadly talented person, did you leave something behind? Are there things you regret that you weren't able to follow because you were quite directed?**

Kagan: No, because I perceived that I had a very narrow talent profile. I was not good at music; I played the trumpet as a child, and I was OK. I remember thinking, "Why am I not better at the trumpet," really. I played athletics; I was OK. I once tried drawing; I was a terrible artist. There wasn't much left.

**McCall: Those were the only avenues?**

Kagan: No, it is not that. There are many people with multiple talents that say, "Gee, I really want to write novels." I perceived that I had a very leptokurtic profile and that I was good in psychology, so no. Later in my life there was a regret of not moving more toward neuroscience only because the advances are more aesthetic, but no, not a regret of choosing another.

**McCall: When you started in the profession, what was the value system, what did it value, what were you suppose to do, and did you do that?**

Kagan: Well, the value system for development was the one I choose. You are supposed to do the most important research as objectively as you can and work on important problems, which is what I did. It is not essential today and it was not the value system for all of psychology. The split just between experimental and socio-personality existed then and so I was not doing the very tight experimental manipulation that is so characteristic of what we call experimental psychology, but that is ok. My reference group was the developmentalists. So aye, I was not the original or bold; I was doing empirical work that a serious developmentalist should work on.

**McCall: How has the value system of the field changed over the years?**

Kagan: It has changed without making any comparisons. It has changed and there are good things about this that include much more concern with current contemporary applied problems, which was not true in the past. That is helping to balance the field.

**McCall: You went to Fels, why?**

Kagan: I went to Fels because my first job was at Ohio State, and I wasn't that happy there. I was only there for eight months because I went into the Army. There was too much interest in football; Woody Hayes was the coach. When you went to the faculty club, they wanted to talk about Saturday's football game. It was not an intellectual place and no other developmentalists to talk to; Jules Rhodes and Kelly were not interested in development. Columbus is not a very interesting city, so although I had a job to return to, I was not eager to return to Ohio State. So a happy accident occurred. Lester Sontag had received a big NIMH grant to follow-up the Fels population. I had met Lester when I was a last year graduate student at Yale. He came by and tried to hire me. My mentors at Yale said, "Don't go there, you'll never get out. It's a tiny little town and you will sink." So I thought about that and didn't go there, but Lester remembered the interview and so he found me at West Point where I had been stationed and said, "When are you getting out?" I said, "January of '57," and he said, "I have this grant from NIMH, would you direct it?" So I took the train from New York Penn Station to Xenia, Ohio, which was a long train ride. I spent two days at Fels in the library, you know, going through those black books and when I left two days later I said, "I am taking this job because I saw it." It's like when you take an archeologist and you take him to a new site in the middle of the desert and tell them, "There was a town here, a very important town. So find it." That is how I felt. There were these pages but I was really glad; I really felt this was it. This was what I always wanted to study. Here are these histories, and they are all written down. Tens of thousands of hours and they are giving me this present. I took the job, and of course I never regretted it. That was very interesting.

**McCall: Now Birth-to-Maturity, what was that for you?**

Kagan: Oh, that was very important. I would say two intellectual events have been important in my life in determining what I did: the research that lead to Birth-to-Maturity and the work in Guatemala, and I can tell you why. From Birth-to-Maturity I learned two or three things, which you can see in my future research. First, I saw that only a few things were stable. Remember, one of them was what Howard Moss and I call pacificity, which is what we now call inhibition. Those kids--we called them pacificity, a long word--those kids who were very, very frightened to restrained, kept that for the rest

of their lives. That is the one variable we quoted from zero to three that stuck in my craw. That is interesting; I thought about that. Second, one part of the battery was a test we made up for a different purpose. We were trying to get conflict, but actually, because the data didn't come out is how I invented reflection pulsatility. I found that certain personality types were taking a long time to do this task; it was a figure sorting task. We put out a lot of figures and we said put them together. We were interested in whether they put the aggressive figures together. That turned out to be totally worthless. Some people took a long time and some people were quick, and at Fels I began reflection in pulsatility. That was ten years of work I would say, and of course the collaboration with John, taught me how to record heart rate and ever since then, I have been recording heart rate. So the Fels experience was a very important one.

**McCall: What about Guatemala?**

Kagan: Well, Guatemala was--until then I am an environmentalist for deep reasons--not just my training, but my prejudices, my upbringing. And like most environmentalists, biology is unimportant. We are psychologists; we don't have to get into biology. Environment is everything and then in that year I was seeing Marazik babies retarded on everything because they are kept in this hut. And then four-year-olds in the same village (who must have been like that) were lively and bright; that did it to me. Everything I believe has to be fraud because how could retarded infants recover. If everything I believed was right, they should not recover.

**McCall: Why, the environment changes? Wouldn't that be just as much evidence for environment?**

Kagan: That is not what was being interpreted. The way it was interpreted at the time was you're learning all these habits and then you're never going to catch up. No, it turned me toward maturation and that was a preemie wardship.

**McCall: Help me understand. I can understand how that experience would dissuade you from the proposition of the indelible permanent importance of early experience, but I don't know how it necessarily drags you to biology. It simply says the environment is important and organisms react to the environment.**

Kagan: No, but the point is the environment was so simple, so homogenous. Why did these four-year-olds living in a dirt-poor village with no radios and no light look like Boston four-year-olds? I mean, I was seeing what every anthropologist sees. There are some profound maturational forces you can't deny; that is what I saw. For example, I give a piece of Play-Doh to a ten-year-old (a kid that has never seen Play-Doh) and say, "Why don't you make something." He makes as good a figure as any kid and the symbolism--I remember sitting on the edge of the wake and saying, "And give me some formal operational questions, like what would happen if the lake dried up?" And imagine having an 11 year-old unable to answer and a four-year-old being able to answer. I mean, I had a lot of experiences there. I said, "Look, you don't need schools, you don't need pencils." That whole year drove me to what I should have realized: the importance of maturational control.

**McCall: But you took an awful lot of flak for Guatemala? Why?**

Kagan: Well, because in the 1970's (we published that in 1973) the field was not ready for that; the field did want to believe that early experiences write their marks on a tablet, and you can't erase those. That was not only the theoretical belief. Now developmental is getting political and a lot of psychologists believe if that weren't true then the government wouldn't fund intervention research. So it was politically dangerous, not just against the theoretical exiguous. And I took a lot of flak; fortunately, I was right. If I had been wrong, I would have been very embarrassed. You know, I could have been wrong on that, but obviously I wasn't.

**McCall: Do you think you took a little flak because your arguments were more--?**

Kagan: They were beyond my data. Well, sure, but I think you have to go beyond your data sometimes.

**McCall: You once told me that data were fine but the important thing was what you believed in.**

Kagan: Right. Plumcara said, "Facts don't speak." Another person that paraphrased Plumcara said, "I have often been in a room full of data and listened very carefully; it was all silent." We need facts, but the things that science moves on are some inferences about those facts.

**McCall: But we have rules of evidence.**

Kagan: But Bob, if all I had were the Guatemala data, I wouldn't have published *The American Psychologist*. Let me just add two things. Upon my return from Guatemala, as I went through my mail there was a paper by Novak and Harlow in which they took these monkeys who had been what they call schizophrenic and placed them with infant monkeys. Now I don't remember the words, but the conclusion of that paper is--if you remember it is a '73 paper. "Well, there is a way this is not indelible," write Novak and Harlow. Remember, I am coming back from Guatemala thinking none of this is indelible; I pick up this paper on primate research and it says, "You're right!" These are back to back, plus I am reading a lot. I am not the first one to talk about maturation. So then after my tiny experience in Guatemala, suddenly I am reading the different literature and the literature says, "Of course, you are right." I had a lot of other things to go to. Then you feel strong enough. It isn't just Guatemalan research.

**McCall: What do you think you have contributed to this field?**

Kagan: In terms of results?

**McCall: Whatever.**

Kagan: Well, if you use what is in the textbooks, then I think--well, the textbooks now say what we said in the '73 paper.

**McCall: About Guatemala?**

Kagan: Aye, if you compare the textbooks of '55 to now, '55 textbooks say it's indelible; you can't change. Now the textbooks always have a section somewhere that says it is not indelible; it can change. Even if they don't quote Wohlwill, sometimes they will. Now someone would have discovered it; someone would have said that, so we just said it early. That is the contribution. I think the current research on temperament, although other people are working on it, is excellent. Mary Rothbart, Phil Goldsmith, I think our voice is part of that chorus. You know, I know there is a tendency to think that the work you're doing at the moment is the most important, controlling for that. I think the work on temperament, which I took up in the last part of my career, is probably the most important. I think that will have a profound influence on the field. Many people are in this field and that is good, not just our work.

**McCall: Jerry, you have spent your life lighting candles in dark forests, sometimes ahead of the rest of the developmentalists. What have been the frustrations?**

Kagan: Loneliness and feeling on the periphery, but obviously I wouldn't have done this had feeling on the periphery been a source of great tension; then I wouldn't have done it. My childhood, personality, and temperament made it easier for me than most to accept being on the periphery.

**McCall: What have been your rewards?**

Kagan: Oh, the rewards are all very private and are of a beautiful result. I remember beautiful moments when we plotted the four separation anxiety courses from the four cultures. Do you

remember the bushman Guatemalan daycare and they all had the same function? I remember like it was yesterday; it was late at night, the smile across your face. Or in last month's--I think it is a recent issue of *Developmental Psychology*. Karen Snyder Rosen was the Harvard student working with Dante, a real attachment theorist. I had been criticizing the strange situation. In this month's *DP*, she is at Boston College and has a paper with Fred Rothbaum. She says, "We didn't find a maternal sensitivity predictive of strange situation behavior." I remember it was about 10:30 reading it and I said, "Good, Karen. When you write, you smile when you write."

**McCall:** A lot of people who are pace setters, who do new things in advance of their time, who do things their own way, don't get recognized by the discipline in its traditional ways or rewards. Do you feel you have been passed by?

Kagan: No, as a matter of fact, and that has helped. I mean, got the Distinguished Science Award from APA, SRCD, and the members of the Institute of Medicine; I have gotten most of the awards. They aren't the most important thing in the world, but aye, those are satisfying. It says that some of your peers think you have been doing good work, so no.

**McCall:** Regrets? Things you would have done differently? Paths you should have taken/not taken?

Kagan: Regrets? I would say the only regret--which all my friends know, including you. For me, the joy is in discovering something about nature that is beautiful and elegant, and the neurosciences are doing that. So, I guess the only regret is that I didn't earlier move in that direction more, just to get closer to nature's secrets; I think that is the only regret.

**McCall:** You frequently voice frustration with your discipline.

Kagan: Yes, several frustrations. First, I think the younger generation (the generation that entered developmental psychology or personality after 1960 or 1965) entered at a time when all quantity publication became important. Research funds were beginning to become harder to get, the fast experiment, the long list of publications became important, and so I feel the sufficient number, not all, but a large number started, I think, to deny what they must have known. You can't use simple questionnaire techniques to get at profound issues of conflicts in society and self-esteem. I feel that the field has moved too far in that direction as I said in the December issue of *Developmental Psychology* in that paper "Yesterday's Premises, Tomorrow's Promises". There is a long critique of questionnaire studies of complex topics. I feel very strongly and I do believe that the future will validate that point of view, and history will say, "That was a waste of time." So that is frustrating. The second frustration is a kind of timidity about your data and your data analyses. There is a reluctance to look at new and different ways to analyze it. Look at the data first then see what happens. In the book I am writing I have a long section in which, after I looked at some heart rate data, I found that the peak response that other people had written about is much better than mean. There is a long criticism of the mean as a useful measure for some reason. But, of course, I will probably receive some criticism too. And, the third, which I have said in public, is I think we do need applied work. Science can't answer all questions and the best science has to be where these two vectors join. One vector is a theoretically significant problem that we uncover a secret of nature or help society, and the other vector is vulnerable to inquiry. You want to be where the two vectors meet. I think in the last 20 years, not all, but a large number of developmentalists as well as social psychologists and personality theorists only ask the first question--is this theoretically important, the effect of maternal abuse or daycare? Even though I don't have the measures, I am going to study that problem. I have said in public as I would say here, "If you don't have the available methods, you can do mischief." If you try to ask a question for which methods aren't available, you will get an answer--and I remember one study, I don't remember the quote, but it is about 15 years-old. Bert Brim, when he was President at FCD, funded it, went to New York to wealthy neighborhoods and poor neighborhoods, and they gave a self-esteem question there. "How good are you in school; how many friends do you have?" It turned out that the kids in the worst neighborhoods had the highest self-esteem. So, as I say to students, "Do you believe that?" That is a problem and, yes, I have criticized that.



**McCall:** You once told me—this was two decades ago--you wished psychology was more like physics where they had experimental physicists and theoretical physicists, but psychology wanted to cram everybody into a value system.

Kagan: I felt strongly about that but that was at the time; I don't feel as strongly now, so let me explain why. There was a time when I really thought incorrectly that you could be powerful theoretically in psychology just sitting in your room the way Chomsky tried or Beurock. I feel less strongly about that now and, indeed in my writing, I don't say that. I think that psychology is a young science, and in young sciences it is extremely rare and highly unlikely that you are going to come up with good theory sitting in your room; and maybe it is best we don't have a lot of theoreticians. So when I said that it was because I really thought sitting in your room, maybe you could come up with some good theories.

**McCall:** I interpreted it differently. My view of you is that you were crammed into a value system that did not quite fit and that for many years, maybe continuing, you were not appreciated as much as you should have been because you did not fit that narrow value system.

Kagan: Right. Oh, but isn't that because of the problems I picked and the fact that I went a little bit too far beyond my data in the view of my peers? That is why; I think those were the reasons why. In their view, I went too far in my inferences.

**McCall:** *American Psychology* has never said it valued conceptualizers or theoreticians despite the fact that we demand theory and concept and Piaget, the antithesis of American psychology, had as much or more influence and so forth, but it wasn't us. We demanded this dust-bowl, empirical hard-science; it wasn't you?

Kagan: That is right. Correct. That is right, but it is interesting; this is not meant to be self-interested. I admit that I went beyond my data, compared with my peers, and I took criticism for it, which was just because that was not the value system. But I was not an idea theorist. I have written very few purely theoretical articles in the way that a researcher would.

**McCall:** Let me ask you about that. You are very much a thinker, conceptualizer. You even once almost said--you know, I characterize you on occasion--you collect data as your own intellectual masturbation. It is a stimulus for you? It is not evidence as much for you as if it fits what you are thinking about, but you have never written a formal theory about anything.

Kagan: Right, this is not meant to be facetious, but I'm probably not smart enough.

**McCall:** That is facetious.

Kagan: Well, I think that is because I have taken big problems and not small problems, not small, but unimportant. That is, I never--let's take a very interesting area, color perception. I never pick problems at that level where brilliance and preservation could allow you to come up with what we now know to be opponent process theory. I picked, because that is me, larger problems and either no one can write a formal theory or those areas are not ready. That is my belief as a defense, or I just wasn't smart enough; that is not meant to be a joke. I picked areas where I don't think they are ready for a formal theory, so I know I have not written any.

**McCall:** Was personality ready for Freud? Was mental development for Piaget?

Kagan: No, but that may be, and I am being really honest, my timidity, probably timidity.

**McCall:** Kagan, timid?

Kagan: Oh aye, yesterday in this symposium I finally said publicly that I am actually timid. I know that sounds--I actually said publicly what I have thought about for about seven years. Why didn't I say it before? It is a wild idea. In a sentence, it is this. We now know that inhibited children, the prototype of the inhibited child is a northern European with blue eyes, light complexion, a very narrow face that goes with an equimorphic body build, and a very reactive synthetic nervous system; we know that. It just so happens that those four tissues--facial bones, align-a-sites, adrenal modula, and sympathetic ganglia--are all derivatives of the neurocrest cells that form four weeks after conception and migrate, and I really believe that a genetic mutation occurred that altered the chemical environment of those neurocrest cells; that is really the heart of it. It is a wild idea and I finally said it yesterday; that is timidity. Why didn't I say it eight years ago? I am aware of my colleagues and because I wish to persuade them. I hope this is not interpreted as immodest, but that is what Darwin did. Why did Darwin wait for so long? If your goal is to persuade your colleagues with your ideas, then you can't be too crazy or you will be dismissed. I mean, you do research first for satisfaction, but then you are trying to persuade people. So it is obviously a truth that you don't persuade people if they think you're crazy. The timidity is in the service of persuading them, which is why I have not sat down or written a formal theory; I actually believe that if I sit down most of my work will be wrong as Freud's was. I mean he had ego, super ego, or bad ideas, didn't he though? So here is this brilliant guy who wrote a theory influencing a lot of people, and actually he misled most people. He was very influential and primarily most of it was wrong; I don't want to be wrong, and that is why.

**McCall: -- has it turned up?**

Kagan: This may be an illusion, and this may be one of my few immodest comments. Occasionally, I reflect on what I have written. There is very little I have to take back, and that has reinforced an attitude of timidity. If you think about what I have written, most of it is right; that is amazing, Bob.

**McCall: And I would say, "They don't remember who said that."**

Kagan: So the restraint has been reinforced. I don't have to take back very much.

**McCall: There is one other question. One might say there are some of your peers, like Joe Pesman, who have produced a whole wrath of students. For those of us who remember their students anyway, one might say you never had a lot of students. Do you perceive it that way?**

Kagan: I perceive that I haven't had--I've had a lot of students, but not a lot of students who made major contributions; I don't know why.

**McCall: Shortage of talent?**

Kagan: I think it is because of my areas of interest. I worked in complex areas where the students--the students that came to me were a lot of students interested in clinical psychology. They became outstanding clinical psychologists and not researchers because they were interested in the problems I took on. I think for the more experimental students who would go to the academy, my work is not amendable to experimental manipulation and so that is why. I think it is a function of the problems I took; it may also be a personal thing. When students are honest with me--and this always surprises me because I perceive myself as a gentle person--they say, "You intimidate students." So if I intimidate students by being more demanding then obviously students are going to say, "Why should I put myself in this?" Maybe I do intimidate students; that is possible.

**McCall: You are a very good man who gets very enthused about what you are doing and there is very little left in the room after you are gone.**

Kagan: Very little left in the room, meaning?

**McCall: People are blown away. It takes only someone who is brash enough to think there is**

**another thought on the subject.**

Kagan: So that intimidates the student, you are saying.

**McCall: I don't think it is a fault; that is the way you are.**

Kagan: Aye, you may be right.

**McCall: Your supporters talked about girls and your regret about them was that they won't stand up and argue with you.**

Kagan: Aye, right. I think the second factor is important; I do.

**McCall: Suppose you're being paternal wisdom to a new student. What does it take in this field to make it a profession today? What do you have to have?**

Kagan: Well, it has two answers. They do want to have the joy of creative work or do they want to get promoted as soon as possible and have secure tenure. What question should I answer? That is two different sets of advice. Well, the first one is the most important. I would say to the first, consult your heart and determine first, as best you can, what you are really curious about. What is it you would love to know if a Genie appeared, and you could ask that Genie any question and go pursue that problem? Because the affect that comes from a deep curiosity is absolutely necessary to make an important discovery and that is the only prize. Make an important discovery and the world will take care of you; it will be gentle and kind to you. So that is the advice: find out what you are really interested in. Most students have no idea. My experienced students come ready for their thesis and they are not deeply curious about any one thing, or they have a problem figuring out what they should do. So that is the advice to that student. While the pragmatist who recognizes that science is becoming a guild, I say that with sadness, a guild in the renaissance sense chose control over quality and didn't let anybody in who was not playing by the rules; that wasn't true 30 or 40 years ago. Then it is pay now; take the stuff off the shelf, do clean antiseptic studies with the right manipulation, and the proper statistical analyses do the guild work.

**McCall: That is a tragedy.**

Kagan: It is a tragedy, but you can't change history. There are two or three things in my life and for me one of the most profound changes; I am sure it happens to everyone with aging and is not unique to me. You're young and you really feel you're agenic; you're an agent. You can do it and what is happening around you is really quite irrelevant and unimportant. If you really persist and are smart enough, you can move this field, which is obviously wrong.

**McCall: You don't think your career has moved this field?**

Kagan: Oh, absolutely not. Look at the field.

**McCall: But you just told me that all of these ideas have come.**

Kagan: But think, take all of developmental psychology and take this program at this meeting, my influence is infinitesimal. Oh, come on now, you don't move a field.

**McCall: Well, then, is this whole thing just intellectual masturbation?**

Kagan: No.

**McCall: If there is no influence, it is just spinning wheels, academic even within academics.**

Kagan: Societies have institutions; you need these institutions in case, rare cases, society needs them. We have the military, and we support the military with enormous amounts of money. There hasn't been a war in a while and there may not be one, but just in case there is a war we need to have whoever is in the military at that time. I don't remember having any influence, but if chance calls them we need them. So, let's take science and the academy. Ninety-five percent of science is the premise that we got to keep science going because we won't know when we will need it, and occasionally we need it. Occasionally, someone discovers a drug that helps somebody. Ok. Occasionally, we need a psychologist to testify in *Brown vs. the Board of Education* and this is it. There is a fire station, and occasionally there is a fire; you can't suddenly build a fire station and that is all. That is not being derogatory of science. Each of us lives our lives and occasionally the world calls on us and since it is rare and the odds are you will never be called on, I feel that all of us who have chosen the academy are damn lucky; we have a privileged life. Why? We can pursue our interests, our curiosity with minimal restriction on us. When I entered academic psychology (38 years ago), it was a good life concerning what has happened to medicine and war and business; it is a privileged life. I feel enormously privileged. As you know, doctors now have to sign-up that they took courses; you and I have to sign-up when we go to a colloquium. We live a great life, and I am very grateful for it. But influence, forget it because that is not what you ask for. The influence of any one person is so small and occasionally, when you are a big influence, you are wrong. Look at Freud's influence; look at Baldwin's influence. They were wrong on most things. As a matter of fact, I would suspect there is an inverse correlation between the amount of influence and correctness; don't ask for influence.

**McCall: You are a student of temperament? What kind of temperament does it take to be a good academic in development?**

Kagan: No special temperament for development. I think you have to have a temperament to not get-- which is probably true of all professions--to not get anxious easily. It doesn't help to have an inhibited temperament. You have to occasionally be bold, tolerate whatever uncertainty or apprehension that is associated with being bold, and you can't get depressed easily because rejection both from funding agencies and referees is very high. I would say I have had as many grants turned down as accepted. If I had a temperament that would lead me to be self-accusatory and being in a depression, then I would be down-in-the-dumps a lot of the time. You read a referee's letter for a journal publication and if you are temperamentally prone to enormous self-flagellation and sadness, then you are going to be hurt by it. So, it does require certain temperamental qualities.

**McCall: You once captured us with a sentence, "You have to be able to stick your neck out one day, get your head chopped off, and stick it out again."**

Kagan: Right, that is right.

**McCall: You told that to me.**

Kagan: I forgot that. I mean, it is common for young people to not want error and to avoid criticism, which is natural and human. And even I have told you I have been timid, but you can't carry it to an extreme.

**McCall: I suppose I need to ask you a few questions about SRCD. How have you seen this organization change, what contributions has it made, what contributions hasn't it made, where should it go?**

Kagan: I think SRCD should feel self-celebratory. My first SRCD meeting was in Iowa City in 1957 and you could fit the whole thing in a hotel called the Jacktar Hotel; it probably doesn't exist now. I bet you there were only 200 people at that meeting. I bet there are 4,000 people at this meeting, so that is an enormous advance. It is broad; I mean, at that meeting there won't have been anything specific on the social problems of divorce or abuse, so it is broad in scope. It is a loyal organization; the same people keep on coming. The senior people, even if they are not speaking, come. It is a very loyal

group, I think, in science. Maybe it is an advantage to have it every other year instead of every year. It seems like a family reunion. So, I think SRCDC gets a great deal of credit; it is a great organization.

**McCall:** It started out interdisciplinary; it became dominated by psychology, although I would assert that all of behavioral science is dominated by psychology. It has tried to become more interdisciplinary, but continues to feel and is not as interdisciplinary because of that. Do you feel that way?

Kagan: Sure, that is the one sadness, but all science is going toward specialization. There are so many meetings and so few people trained in an interdisciplinary strategy; that is history and is no one's fault. No one at SRCDC is to be blamed for that. History took over and, unfortunately, has become purely psychological. But take a worse tragedy: the Society of Neuroscience began with a premise in its constitution that it would have both behaviorists and neuroscientists. Now go to a meeting, which has 10,000 people; it is very hard to find a behavioral paper. It is all neuroscience now. They began with a strong conviction to be interdisciplinary and it is even worse there. I am afraid that in science there are fewer generalists and everyone is moving, retreating into their sphere of inquiry. It is sad we are not more interdisciplinary.

**McCall:** You have played a variety of roles on national academy panels, advisory groups to the government, etc. What do you feel about those? Do you have any influence?

Kagan: I have no influence; the roles must be filled. You know, when you take this and the work and the time in good cognitive dissent theory, you persuade yourself that you are going to have some influence. I am not distorted; I have felt I have no influence. In fact, I can laugh at myself. When I was on Council for the NICHD, probably a decade ago, I put my neck out because it is a trip and it is three days out of your life. Why am I doing this? And I would criticize grants and try to change grants. I lost every time, and I only made enemies. So at the end, I made many enemies by taking a grant that the study section had approved that I thought should not be funded, and I would get up and make a speech for a grant that I felt should be funded and was not. All I did was make people in the Institute angry, not my peers on Council, and I accomplished nothing, absolutely nothing; I had no influence.

**McCall:** What about the policy groups like The National Academy of Science panel?

Kagan: Ok, let's take an example. There was a three-year panel on RC Violence in America and I was asked to serve, and mainly sociologists as you know, to represent adult mental along with Len Eron and Tony Earls. So, I made a pitch for the importance of temperament. The book is out; it is 400 pages; there is one paragraph in 400 pages. So where was my influence for all those days in Washington? No influence, as I said earlier to you, is what I have learned. We must--and this is just human--you must exaggerate how much influence you will have in order to take the job on because if you thought you didn't have any influence, you wouldn't do it; that is human. That is hope, Orval Hobart Mowrer's hope. And it turns out, of course, that the hope is an illusion because you have very little influence. The forces are much bigger than you, which is what I have learned.

**McCall:** So, then what is your satisfaction? Why do you do it?

Kagan: But I do it much less.

**McCall:** But why do you do this job entirely since you said you don't have much influence in contributing to knowledge?

Kagan: Because I am reading Sartre's biography by a man named Hayman; it is very good. I don't mean to flatter myself, it is just that I am reading his biography and he was torn all his life between dissatisfaction from him writing alone and then feeling guilty that he wasn't doing anything useful for society; and that was why he got mixed up so much in politics. Hayman, the biographer, captures it beautifully. He would start to write for four days and then feel, "What am I doing? This is selfish," until

he would leave and start to be active. Of course, at that time, communism was the answer, pro-communism movements, and antifascist's movements. I do much less of this than I did when I was younger and had the illusions. Occasionally, someone will say, "Look we really need you," then, for the moment, you let yourself be captured by, "Well, maybe, this time I can be useful." You're not; it is a flattering illusion. You might be useful this time. Now that, of course, is my perception. Suppose you call Al Reese who chaired the committee and said, "Al, I want the truth. Was Jerry helpful on the committee?" I would be very pleased if Al said to you, "Oh boy, I am sure lucky we had him." It would make me feel good. My subjective interpretation is anybody could have been there; I didn't need to be there.

**McCall: Do these committees and policy groups in general, regardless of your role, have any influence?**

Kagan: I am sure memory is distorting, but as far as I am concerned and of all the groups that I have served on, I think that the influence has been very hard to detect.

**McCall: Let's be paternalistic again. It seems to me you go through a sequence of rewards and accomplishments in your career. I remember I learned how to publish and got a few rejections, but I learned how to publish. I went to Fels, and I asked Lester in the job interview what was the criterion as somebody told me to ask that. He said, "To make a contribution." I said, "Oh s\*\*t, I just learned how to publish, how do you learn to make a contribution." Now you are telling me that you don't make much of a contribution. Now here is a man who, and you correctly said, that had an enormous impact, and sometimes the field doesn't recognize that it was you that lit the first candle and pushed it that way. No, you don't have much influence; once in a while a fire alarm rings. What gets you through the day with this perception? What do you tell a new kid? Where are your satisfactions?**

Kagan: You asked me that question in different words before. It is discovering something true about nature and hoping that sometimes another person will pick up that small truth and move it. Science is very interesting; it is not like the novel. It is cumulative and I am going to say something you may know. Alfred North Whitehead and Bertram Russell made their fame with Principia, Principia Mathematica. Principia Mathematica had absolutely no influence on western mathematics--none. I think it was ten years of their lives in world fame and everybody remembers them. You say, "Principia Mathematica" and I say, "Oh, that is Russell and Whitehead," but no influence on mathematics.

**McCall: So is this just intellectual masturbation?**

Kagan: Is love just f\*\*\*\*\*g? I mean the use of the mind to discover a secret of nature is a beautiful thing; there is not much beauty in the world. That is not masturbation; that is a wonderful thing to do.

**McCall: But should society be paying for this?**

Kagan: Should society be paying for it? Occasionally, society wants some truths that it needs and so it should put in a little bit. If you took all the research on this program, I bet less than 15 % is paid for by society. Most of this research is unfunded by the government.

**McCall: Well, but they pay us to teach and we don't teach very much.**

Kagan: But you need the university. The university does an important role in providing to the next generation what the beliefs are, what the truths are, and what the falsehoods are. Of all the institutions in our society, it is clearly one of the more important ones. I don't think you need a defense of universities and science; it is just wasted.

**McCall: You do after you have just told me this wisdom about what influence and contributions there are.**

Kagan: No, but that is what I said about science. The field has an influence; a single individual has minimal influence on the fields.

**McCall: The grain of sand, but the beach is bare.**

Kagan: I will give you someone you know who I regard as brilliant his whole life, Raymond Cattell. Raymond Cattell was brilliant and he spent his whole life on important problems; Raymond Cattell's influence is zero, but I don't think that was a wasted life. He thought he was doing something; he published some beautiful articles. We don't want to say that Raymond Cattell wasted his life.

**McCall: Where is the field going?**

Kagan: I don't know.

**McCall: What is your prediction for what SRCD meeting themes will be important ten or twenty years from now?**

Kagan: Well, I hope that there is more of a balance between what I regard as fundamental problems and social problems. I am glad that many developmental psychologists are working on the effects of divorce, abuse, and daycare. The messages aren't yet ready, but someone has to be working on these problems and that is good. But the balance between what a baby perceives and what a child's memory is about, if you look at this program, it is a bit out of balance.

**McCall: More applied than basic?**

Kagan: If you looked at the program 16 years ago at New Orleans in 1977, you would see a more balanced program. I think it will correct itself. I do believe in cycles of self-correction as the program in 1957 corrected itself. I come back to the deeper questions regarding the development of new methods. The serious problem is no one is working on these efforts. Where are the people in our field, or aligned to our field, that are making those vacuum tubes? I am going to physics. I mean, physics without the vacuum tube, forget it. The microscopes we have video cameras and VCRs was a big advance. You couldn't do attachment research and you couldn't do aid man research without video and VCRs; that is a big advance even though you might not think that is a major method because without new methods Whitehead said, "You can't discover new facts, and without new facts you can't see what is wrong with your theory." Since all theory we have is flawed, you need new facts. You can't gather the same facts. We should talk to children and we should observe children, but if that is all we do then we will discover nothing new. We will just keep on discovering children behave this way and children give these answers.

**McCall: What is something that you can do and most of us can't? You have probably seen 90% of psychology, 95 % of developmental psychology. What have we learned? What things have we contributed to society because we tend to not appreciate ourselves in the same field?**

Kagan: Enormous. Let us start with development and let's take as a comparison the development of 1930 or 1935 or even 1940. Go read a child development textbook; read Elizabeth Hurlock, any of those. I mean, look at what we learned about infant perception. Take Vance and Hayes, we now know about exactly the course of perceptual development in the first year. We have more to learn, but none of that. We now know about the maturation changes at two to three months, tone at eight to twelve months, growth of recall memory, emergence in the second year, and the work on language by Brown, Snow and Bates. God, we now know about language! Read the section on language by Hurlock; it is primitive. The advances are stupendous in our knowledge. There are now textbooks on infancy like the ones by Marc Bornstein and Campos or Lamb and Bornstein. Could you write a textbook on infancy in 1930? You would have had nothing to say, so it would take hours. And take perception, who would have thought that color, movement, and shape are initially processed separately and then put together

down in the interior temple cortex; that is so counter-intuitive. That looks like a unity to me; the Gestalts were wrong. It is not perceived as a unity; it is all taken apart. That is counter-intuitive. Look what we have learned, a pointed process theory in color perception. Schachter's work on implicative memory that in the phasic, you said it was a phasic apple, apple, onion, fox--what did I say?

**McCall:** I don't remember.

Kagan: Can't remember. Then you show them a fragmented word \_p\_le. "What do you think that is?" He says, "Apple." Come on, that is incredible! Who would have believed that in a fascicle he can't recall anything still but can recognize a fragmented form of what he saw? And so, psychology has much to be proud of, and really someone should write a short book. Take a textbook of the '30s as the referent, as the boot, they will make everybody happy. That would be great therapy; we have learned a lot.

**McCall:** We often say to ourselves, "We don't have any laws in psychology." Well, sure we do, more or less. What are the five laws in psychology? Define one-word concepts or principles or movements we contributed to society?

Kagan: To society or to knowledge?

**McCall:** Both

Kagan: Let's take knowledge. I am not sure we have influenced society. We now appreciate more than we ever did. And here the Gestapo was on the right track with the role of context that it is the stimulus in the context. You look at the papers here that are on cognitive approach every one is saying are like the papers here on infant perception. That the context, like Helen Ruby Caller's work, is very important; it is not just stimulus. The stimulus is in the context; that is a very important principle. The rules of generalization are new; they weren't the work originally of Hubbeland and then Roger Shepherd. Stimulus and generalization are very important principles. The power of discrepancy that is never absolute glucose whams never absolute luminous level. It is always change, which as a matter of fact in von Békésy's book on sensory and inhabitation, a thin 200 page book, he says, "That is a very important advance because in the '30s everyone assumed it was the absolute level of glucose in your blood, the absolute amount of light." To say the nervous system responds to change and not absolute levels may seem ho-hum to you and me, but that was not the belief a half-century ago. And that holds for every domain of psychology from the hippocampal discharge to our behavior here.

**McCall:** Terry Helson's attitude?

Kagan: Exactly, that is a very important principle. It is not a wall, but as Ernest Meyer said in his book on biological thought, "By and by, the life sciences don't come up with many thoughts because there are too many exceptions." And so, we are not like physics yet; Ernest thinks we will never be like physics. Let's say we are not there yet. We don't have any laws, but we have a lot of important principles that we learned in 50 years.

**McCall:** Reinforcement?

Kagan: Yes, and probably we don't know what reinforcement is. Reinforcement, aye, I left out reinforcement, of course. Skinner's work on the effect of various schedules is a very important principle.

**McCall:** Testing?

Kagan: Well, that is a methodological advance. We know how to make good tests. I won't call that a basic contribution to knowledge. That is like, say, how to make a better microscope.



**McCall:** Sure as hell has a lot of influence in society.

Kagan: Oh, testing has an influence on society, oh sure! Now, let's get to your question about society. What influences society? Oh, testing movement has influenced society enormously, more than any other I would say. That is where the influence is most salient. But I am not sure you can take developmental psychology and determine whether it has influenced society very much because the message is, and finally society says, "Curse on both your houses."

**McCall:** If you had three minutes to talk to the next generation of our discipline and give them some advice, what would you tell them?

Kagan: I would say, "Begin with phenomena, not with words." That is not empty advice because most young students begin with words. They say, "I think I want to study anxiety," or "I think I want to study love," or "I think I want to study intelligence." I say to them, "Don't start with words; the words don't have reference out there. Start with phenomena as Newton argued life. You want to study children who seem to be frightened in school? Ok, why don't you explore that? What makes them frightened, not with anxiety?"

**McCall:** What else?

Kagan: Start with phenomena. Second, don't pick problems where the methods are not ready. So in my humble opinion, if there is a set of phenomena out there that are covered partially by the term self-concept, there are no good methods of self-concept. That is an interesting problem, but don't pick that problem unless you want to devote five years to developing the proper methodology. Don't pick a problem that is not ready for inquiry. Third, be patient; be prepared for frustration; pick a problem you are willing to work on for five or six years; examine your data before you even put it into the computer; those are mine, and again, of course, pick a problem you are really interested in; those are the four things.

**McCall:** Good, you're a great man.

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**Those who inspired and were influenced by Jerome Kagan:**

**Mentors**

Frank Beach  
Katie Wolf  
Sybil Escalona

**Colleagues**

Frank Beech  
Leonard Doob  
Carl Hubbeland  
Katie Wolff  
Sybil Eskowoti  
William Kessen  
Jerald Lesser  
Martin Comb  
John Dollard  
Neal Miller  
Lester Sontag  
Howard Moss  
Melinda Novak  
Harry Harlow

Mary Rothbart  
Phillip Goldsmith  
Karen Snyder Rosen  
Fred Rothbaum  
Joseph Pesman  
Al Reese  
Raymond Cattell  
Elizabeth Hurlock  
Timothy Vance  
Bruce Hayes  
Roger Brown  
Catherine Snow  
Elizabeth Bates  
Marc H. Bornstein  
Michael E. Lamb  
Joseph Campos  
Paul Schachter  
Helen Ruby Caller  
Roger Shepherd  
Georg von Békésy