Gilbert Gottlieb: Developmental Psychologist and Theorist

[Taken from: http://icube.uconn.edu/GGvideo.mov. The original interview was done by Dr. David Miller and the video recording was made at the Gottlieb home by Marc Gottlieb. The interview was transcribed from the *GGvideo.mov* file into a Word file by Edyta Eansor and then edited by Dr. Douglas Wahlsten and Dr. Miller.]

(Foreword by David Miller)

DM - Hello my name is David Miller. I'm a professor of psychology at the University of Connecticut. Back in the 1970's, I had the good fortune of working as a post-doctoral fellow under the supervision of Gilbert Gottlieb at the North Carolina Division of Mental Health in Raleigh, North Carolina. On June 10th, 2006, I visited Gilbert in his home in Raleigh, to reminisce about his scientific career. We recorded this primarily as an audio podcast, but we also had two video cameras taping this session, so what you are about to see is the edited feed from one of those cameras. So join me now, as Gilbert and I discuss his amazing scientific career both in terms of his incredible theoretical contributions as well as empirical contributions to developmental science.

June 10, 2006 (The interview)

DM- Hello Gilbert. Thank you for doing this podcast with me today. We would like to elaborate on your career. Going all the way back, why did you first become interested in psychology when there were so many things you could have chosen?

GG- In 1951 I was drafted into the US Army, and I was lucky enough to be chosen to go to counter intelligence. The Korean War was going on then. After I went through basic training, I went through 14 weeks, 8 hours a day, 5 days a week, of counter intelligence school. I loved counter intelligence school. They told us that we were the top one to two percent of the people that had been drafted into the army. I worked very hard to be in the top one or two percent of anything that came along. I was very, very good at working hard, and it all paid off when I finished. I think I was second or third in my class of forty. When we came up to assignment day, we were special personnel because we had gone through this course. They didn't just hand us a piece of paper that said, *You're going to Korea, You're going here, You're going there*. They called us in for an interview, to make sure we were satisfied with our assignment. Because I had a smattering of German, they sent me to Austria instead of Korea. They did the interviews in alphabetical order. I was paying close attention to where everybody was being sent, and everybody was being sent to Korea. So, when the officer called me in and said, "*Well, I guess you know where you're going*," and I said, "*Yeah, Korea,"* He said, "*Gottlieb? Korea? You speak German, don't you?"* And I said, "*Yavohl*!" And off to Austria I went. I was very happy, generally, and that turned out to be a very valuable experience. I wasn't that young, I was going on 22. Austria was filled at that time with what was called displaced persons. They had come from Eastern Europe and many of them had held exalted positions,

were well figured, or had held government positions. It was very striking to me that some of them adapted extremely well to their new conditions. They somehow were making the best of it, while others just sort of gave up. I thought that was a very interesting phenomenon, and I thought what would explain that, probably, is psychology, individual psychology. So from that day on, I made up my mind that when I got out of the service, I would go back to school, major in psychology and continue my professional career in psychology, if it was possible.

DM- When you went back to school, were you still an undergraduate?

GG- Yes, I was still an undergraduate. I had started college when I was 16 and I was way too young. I didn't know how to study, and I botched it, so I left college in second year and started on a career trying to figure out what it was I was supposed to do. My father was a businessman, so I felt maybe I was supposed to be a businessman, but I wasn't very good at that. The best thing that ever happened to me, although I didn't think so at the time, was I was drafted into the army. After leaving the Army, I went to the University of Miami, because that's where I could get in. I was a Florida citizen and they made breaks for Florida citizens. I also studied at Miami for a Masters degree, and then I was fortunate enough to be accepted at Duke. Duke decided that they had not been accepting enough graduate students because they were not graduating enough people at the other end. They decided to enlarge their class to 15, and under that situation, I got accepted to Duke. I was still on fire and pretty young. I entered the clinical psychology program and then got interested in imprinting. Imprinting is a phenomenon whereby waterfowl become attached to the first objects they see shortly after hatching. I started to do research on imprinting while I was also doing some human research. Being in a clinical program, I needed to be doing some human research, so I did both. I decided to do my Ph.D. dissertation on imprinting, and Duke was willing enough to accept me doing that. Usually there's a conflict between the experimental side, which is the animal side, and the clinical side, which is the human side, and they fight fiercely over students doing their thing, so I was very lucky. Duke was very generous to me.

DM- Have you ever come upon another person who was trained as a clinical psychologist and ended up really doing animal behavior as their primary research?

GG- No, I never thought of that actually. I guess off hand I really don't know of anybody.

DM- Some of the people that are going to be listening to us are undergraduates that are interested in a career in science. What do you recollect were the most valuable aspects of your education that contributed most toward your development as a research scientist?

GG- Undergraduate education at Miami was very good because some of the professors were very broad in their orientation, and they encouraged me to come and talk to them after class. We talked about things that could extend into other things that

we had heard of. They encouraged me to read more lively things on my own, things that were not assigned. They put up with a sort of a wise-guy attitude that I had, that I was really pretty smart, and they managed to get me on the right track. But, they allowed me to dissent, and they would argue with me, discuss things with me and treat me in a way that I thought was very nice.

DM- Do you remember specific books that you were reading on your own that would have been important?

GG- I got interested in a book called 'Knowing and the Known' by authors Bentley and John Dewey. It was tough, because it was philosophy and psychology. An English professor introduced me to that book, and he would discuss that book after class with me. He would bring it into class as appropriate, in his lectures. You could tell he was a pretty broad-ranging person. The main theme of 'Knowing and the Known' was that there was a new view coming into philosophy and science called interactionism. Another new view that was coming into science went beyond interaction; it was called transactional. I got really excited about transactionalism - the idea that you didn't just have interactions going in one direction, but you had them going in both directions so you needed a new word. You needed transaction, so you can go across, and that just excited me.

Some of our professors were not as excited as I was about transactions, so I got plenty of practice. We would argue about it and discuss it. Then I ran across a book by Egon Brunswick. He talked about psychology as a whole, which was unusual, and he did it in a way that was commensurate with transactionalism, even though I don't think he used that word, but he was sympathetic with that point of view. I think those two books were the most influential that I read. I did not understand them right away. I didn't just read through them, I had to study them, and I studied them, and studied them, and studied them. Really, and I'm just saying this in case there are any students who run across the same dilemma if they are interested very much in some intellectual point of view but they are having trouble grasping it. If you stay with it, and you have sympathetic professors, you may eventually master the material. Some people say I never mastered it (smiling).

DM- Well, I'm certainly not one of those people who would say that. And in fact, the Bentley and Dewey book is still influential in many circles today. I discovered that economists are interested in this book. The American Institute for Economic Research in Massachusetts invited me the last two summers to give talks about inter-relationships, about the kind of research that you and I have done and how that fits the concept of what Bentley and Dewey were talking about in terms of the differences between interactionism and transactionism. Like yourself, I had a real tough time wading through the Bentley and Dewey book. I waded through it just enough to try to make those distinctions between our research area and that philosophy, and how that might relate to economic theory. I don't know anything about economic theory, but those folks were able to make those links, which I found fascinating. So, Gilbert, most Ph.D.s interested in pursuing a career in research typically end up in academic institutions. You did not, you actually ended up at the North Carolina Division of Mental Health in Raleigh, North

Carolina. Can you tell us something about how that came to be and how you established the Psychology Laboratory at their Research Division?

GG- Yes, my first job was as a clinical psychologist. I was finishing my Duke degree at Dorothea Dix Hospital, a mental hospital in Raleigh, North Carolina, which was not very far away from Durham where Duke is located. They were somewhat short-handed in the clinical realm and they offered me the opportunity that, if I would do clinical work for four days a week, they would let me do my duck work one day of the week and even have a laboratory. They had a ward called the old X-ward, which was a ward where they formerly kept the more violent patients before the use of psychotropic drugs. It was perfect for my purposes because I was doing auditory work with ducklings. It had about ten rooms, each divided by two-foot walls, and it was pretty sound proof. I called it the Psychology Laboratory because I didn't want to call it the animal behavior laboratory. I wanted to say that this phenomenon I was studying was psychological, not just an animal behavior. That one day a week I worked very hard, and of course I worked on weekends to keep my lab going. And lo and behold, an opening came in this new research division in the mental health department. Back then they didn't have to advertise nationally for openings. I applied for the opening. I had already been at Dorothea Dix a few weeks and knew people and was publishing in the clinical realm as well as the basic research realm. They hired me, and I was able to give up my clinical work in favor of doing the animal work five days a week, with money for a full-time research assistant

DM- I remember how we used to have weekly visitors, many of them as speakers, and others would just come to the lab. They were always in such enormous envy of your situation there. There were no committees to be on, and you were doing some teaching on a volunteer basis at local universities, but you had control of that and basically it was a full-time research position. What a luxury.

GG- I was always very grateful. We were a very small division, and the Director of the mental health department stayed in touch with us, which was very important. Once I was invited to go on WUNC-TV and talk about my duck research. I went to see Dr. Hargrove, who was head of the university. I was really concerned that I might get the wrong advice. So I went to see him and I said, "*Is it alright if I go on television and talk about my basic research with ducks?"* He said, "*Oh, that would be wonderful."* Whewww.

DM- You said that a lot of your own conceptual and theoretical contributions came because you were standing on the shoulders of many people who came before you. Who were some of these people, and who do you think among them are the most influential in terms of your own conceptual and theoretical development?

GG- I was extremely lucky to have made contact with Zing-Yang Kuo, a Chinese scientist who studied at [the University of California at] Berkeley in the 1920's. He had gone back to China and become the head of several universities. He was known for not accepting the idea of instinct, and he believed that all behavior

develops, by which he meant that you have this transaction, no matter what the outcome, it's always a transaction. He had to leave China in 1949 when the Communists took over because he just wasn't sure how things were going to go politically. He and his wife went to Hong Kong. I discovered in 1962 or 1963 that he was in Hong Kong and was looking for people to correspond with. We started corresponding, and I mentioned to him that I wanted to do prenatal work with ducklings. I sent him some reprints of my work, which all pointed to the fact that I was studying instinctive behavior, but in my mind, everything pointed to it having its roots in the embryonic phase of development. Fortunately, it suggested the same thing to Kuo. I asked him to come and teach me how to get into the egg, because I had never taken any biology courses. At first, he was reticent to do that; after all, he was a very senior person and was in his 60's. I was a very young person, and he was a little bit stuffy, but his wife said, "*Look you're not doing anything at this time, and this would be a really good thing for you to do."* So she talked him into coming over. I got a grant from the National Science Foundation for six months to pay his salary and expenses while he was here. While learning how to open up the egg, we decided to make a film called 'Development of Behavior in the Duck Embryo'. That gave me experience of all phases of embryonic development. We devised a procedure to make a transparent window in the egg. Kuo didn't mind that I actually wanted to pull the embryo out of the egg in the late stages and listen to it vocalize. He helped me learn how to do that without harming the embryo. So we did our film, and Pennsylvania State University put it in their rental program for people who were interested in using it in their teaching. Kuo gave me great confidence because he was not only an experimentalist, he was also a theoretician. We shared that transactional view, and that really gave me a lot of confidence because such a senior person had the same point of view

GG- The other person was T.C. Schneirla, Theodore Schneirla, in the Animal Behaviour Department at the American Museum of Natural History in New York. I began corresponding with him, and I invited him down to give a couple of lectures. He brought his wife Leone. My wife Nora, his wife, Ted and I got along extremely well. Once again, I was very encouraged by someone who was very much my senior. He liked my research, and my research was just beginning at that stage, so it wasn't really well known. I was taking a different view about imprinting than others were, and Ted and Leone liked that very much. Having those two senior people encourage me, and in a sense share my point of view, made all the difference in the world to me.

DM- You were obviously very passionate about the importance of field work in your own research, to the extent that you built a field station on your own property and had access to a field station on the property owned by the State of North Carolina. Why do you view naturalistic observation as an absolute necessity in understanding your own laboratory work, or just to broaden the question, laboratory work in general?

GG- When I came along, imprinting was strictly a laboratory phenomenon. People incubated eggs in the lab until they hatched and then they exposed the young

ducklings or chicks to the people who walked around the lab. The chicks and ducklings would follow the people and develop a preference for them as their filial figure. During my doctoral dissertation examination, I was asked about this. I had done my major work on the critical period for imprinting. One of the professors was fortunate enough to say, "*I noticed that even at the height of the critical period not all of the ducklings followed the model."* He said, "*That wouldn't happen in nature, would it?"* I said, "*No, I guess that wouldn't happen in nature, but we wouldn't know what would happen in nature because even Konrad Lorenz* (one of the discoverers of imprinting) *didn't make naturalistic observation."* He would observe chicks, ducklings and goslings that were hatched in incubators in the lab. After I got the job at Dorothea Dix Hospital I went out in the field to see what was actually going on. We went out with tape recorders as well as cameras, and what we discovered is that the ducklings started to peep very weakly but they started to peep before they ever hatched; and the hen, in a reciprocal way, the transaction way, answered them, so they were already doing the vocal, auditory interchange before hatching. Thereby, I thought, if imprinting developed a preference for the visual side of the human object that was first found and if the human object also vocalized, then the duckling would also develop a preference for the vocalization. It seemed to me that the animals were becoming auditorally imprinted to the mother during this dialogue that was happening in real life. The first thing I did was to get recordings of wood ducks with hole nests in cavities in trees and mallards which nest on the ground. We collected eggs from these two species and incubated them in the lab, wood ducks in wood duck incubators, and mallards in mallard incubators, and then we tested them with the recordings of the mallard and the wood duck call in the lab and gave them a simultaneous auditory choice test. We placed them in a situation where there were two speakers; one speaker broadcast the mallard maternal call and the other speaker broadcast the recording of the wood duck. Lo and behold, all the wood ducks went to the wood duck maternal call, and all the mallards went to the mallard maternal call. And these of course were ducklings that never heard these calls before. It wasn't like nature. If I had played them the mallard call beforehand, or played them the wood duck call beforehand, while they were being incubated, then you would expect to get auditory imprinting. That experiment started me off on a very long quest, It also showed that there was a fruitfulness in doing naturalistic observation Then we did various manipulations. I was trying to show that there was an influence of hearing the embryo's own vocalizations on the embryo's responsiveness to the hen. The only way I could do that was by measuring the latency of response of the duckling to the hen's call, under various conditions. We first tested 25 birds, and there was a wide range of the latency. We then raised ducklings of each species in individual sound proof incubator compartments so that the only thing they could hear was themselves. It wasn't the ideal experiment; it would have been better if they couldn't even hear themselves. When we compared latencies to the maternal call of birds that had been reared communally with other ducks or with other mallards, the communally reared birds showed a much shorter latency. That indicated, but didn't prove, that hearing their own vocalizations might facilitate their response to the maternal call.

DM- What actually led you to consider that their own vocalizations might be important? It is what you would call a non-obvious form of experience. How did that non-obvious experience become obvious to you initially?

GG- Hearing their own voice was the only vocal auditory experience that the ducklings had. I thought, well, it's not the fan motor or the hum of the electronics or the motor in the incubator that's likely teaching them to prefer characteristics of the maternal call. Maybe it is something about their own vocalizations. The non-linear, non-obvious thing was that the duckling's embryonic calls didn't sound anything like the maternal call. But they sounded more like the maternal call than the incubator fans, certainly. We considered non-vocal things that have rhythm or rate, such as turning over the eggs, and asked how that might influence their preference for certain rates of calling. My student Marvin Simner even considered heart rate We sped up the heart rates of the mallards, which was normally 3 beats per second, to 3-4 beats per second, and the mallard call became 3-4 notes per second, but we couldn't get it to go any faster. Then, with the wood duck call, normally 6 or 7 notes per second, we turned the temperature down in the incubator of the wood ducks, and their heart rate became slower. Then we tested them, and there was no effect. That's why the study was never published (laughing), and you are hearing it for the first time today. I had a big hint from a zoologist named Hester, who had wood duck boxes up at his dad's farm in Wendell, North Carolina. Wood ducks nest inside a box. When the hen gets ready to get them to come out of the box, she drops down below and calls them out. So we had that hint. We had no idea about the mallards, because the mallard mother can just step off the nest, and the ducklings can see her and they don't have to hear her. Just in case the mallards were vocalizing, we brought along a tape recorder.

DM- Thank goodness you did that.

GG- Yes, that's right. It made a career of very, very intriguing research.

DM- Only within the past decade or so, have some scientists even began to appreciate the role of very specific environmental factors and experiential influences in switching on genetic activity and in some cases switching off genetic activity. Indeed more credence has been given to the importance of experience in behavioral development and in some cases very, very specific experiences. There has been some very interesting work in bird vocalizations, for example, where hearing a particular conspecific (a member of the same species) song switches on genes in auditory pathways, such as work on zebra finches and European blackbirds published within the last decade or so. When I came upon this literature, one of the first things that came to mind is that if I were in your shoes at the time that this work was starting to come to the forefront, I would have been tempted to say, "*I told you so."* How did you react when all of a sudden people began doing research showing that very specific experience can switch on and switch off genes

GG- I'm glad to have influenced a few to my way of thinking, but the battle is still uphill

DM- Gilbert, we see statements all the time both in the scientific and popular press that a gene has been discovered for a particular trait. A gene for schizophrenia, a gene for homosexuality, you name it, a gene for alcoholism. I think that you and I both know what's really meant by such shorthand statements, especially when they make their way into the popular media, but with the continued proliferation of such claims, coupled with the mapping of the human genome, it's easy to see why people are misled into a false sense of genetic deterministic thinking. How do scientists, such as yourself, grapple with claims like these and the impact they have on the way people think about genes?

GG- We do see some inroads on genetic deterministic thinking, but it is very hard to get across what I call the developmental point of view. You understand it very well, and some others understand it very well, of course. But it's very hard to get that point of view across in a consistent way. People seem to understand it in one context but not another. I find that getting across the developmental point of view has been the largest failure of my career. I haven't succeeded, except with close colleagues that I've worked with and who have been sympathetic with that point of view, and therefore came to work with me in the first place.

DM- You find yourself in a very good situation here in North Carolina, especially at UNC Chapel Hill because you do interact with a lot of people who are sympathetic and who have been sympathetic for quite some time with this approach.

GG- You're right. The people at the Center for Developmental Science have been extremely generous, intellectually generous. They're sympathetic to my approach. The four-level analysis is hard to get across - going from the environment to behavior to the nervous system to genetic activity and back again. That reciprocity is so hard to get across. I think many people are more understanding of that approach, even though they may not actually practice it themselves.

DM- It's safe to say that any form of extreme determinism whether genetic or environmental is doomed to fail, yet such views persist. For example, there are so-called interactionists who wear a genetic deterministic cloak, beneath which lies a point of view in which only lip service is paid to environmental or experiential influences and usually then only in terms of such influences being supportive of development, rather than constructive in terms of development. Why do philosophies such as these still persist, and why are they doomed to fail?

GG- These points of view persist because they offer simplistic answers, and that is also why they are doomed to fail.

DM- Will the nature/nurture problem ever be resolved? What's prevented it from being resolved, and what might it take to resolve it once and for all? Or is this an unanswerable question?

GG- I believe it won't be resolved until a fully developmental point of view has been widely adopted and widely implemented. It will require the triumph of a fully developmental point of view.

DM- What long range impact do you think your conceptual and theoretical contributions will have to the field as a whole, to this developmental approach that you've been advocating for so long? Or at least, what are your hopes in terms of the long range impact?

GG- Adopting a non-obvious, non-linear developmental point of view about human development will solve the problem eventually. But it's extremely difficult to think in a non-linear, or non-obvious way about how some phenomenon developed. Something did not happen just because event A happened or event B happened. Instead, it happened because C happened, and then A happened, and then B happened, or some more complex relationship like that. This is not the way we're typically taught to do science.

DM- Kuo, in probably his last article, called for the establishment of multi-disciplinary centers for studying development. You've been fortunate enough to have been working in such an environment at the Center for Developmental Science at UNC, Chapel Hill, I guess about 10 years. Can you comment on the benefits of the approach that Kuo was advocating, and, if there were any drawbacks, what the drawbacks might be?

GG- What I have seen at Chapel Hill has been very encouraging. We started off approximately 15 years ago with the Center for Developmental Science, which was separate from the psychology, biology, and sociology departments, but we tried to integrate people from those disciplines into the Center, and we got grants together. Most importantly, we actually did the same research from our different perspectives, not just by studying some finding from the different points of view. The Center for Developmental Science was co-founded by Bob Cairns and some other people, including myself. Bob had a particularly strong link with a group in Sweden, and the group in Sweden became attached to us. We had traffic flowing back and forth, and that widened the scope so that we had anthropologists, sociologists, psychologists, and biologists all studying human behavioral development.

DM- What was it about the Swedish group that got them thinking about the importance of this kind of thing?

GG- It was Bob, in particular, who was already working with Lars Bergman and David Magnusson in what they called a person-centered view of psychology. That was why that transition was made.

DM- I wonder if UNC is unique in this respect. Do you know of other centers comparable to the Center for Developmental Science?

GG- I hope I'm not doing someone an injustice, but off-hand I don't. I think you have to have Deans and people at higher levels, and department heads who are sympathetic. I really do think it is essential to get away from so-called 'turf' wars. You can't have turf wars and also have this kind of developmental approach. Here, other departments shared their resources with us, and other universities shared their resources with us.

DM- Gilbert, what invitations, in terms of spreading the word about the Center, in particular to this approach about studying development, might have occurred as a result of the Swedish group getting involved?

GG- Well, certainly the main one was having a Nobel Symposium, a very prestigious event that went on for several days, involving people from Sweden, the United States, France, England, the Netherlands, and Belgium. People had, and they still do have different notions of exactly what is developmental science. My point of view is linked to the theory of probabilistic epigenesis, which involves those four levels of analysis. Other people see it more as a unifying enterprise involving sociology, anthropology, biology and psychology.

DM- What is the most important empirical finding of yours and why?

GG- I think it is the embryonic vocal contribution to the identification of the maternal call, both in wood ducks and in mallards. I think it was important to demonstrate in at least two species. It supports the idea of non-obvious, non-linear development.

DM- What about the most important conceptual or theoretical contribution of yours?

GG- I have just recently written a chapter for the second edition of Byron Jones' and Pierre Mormède's book on Neurobehavioral Genetics. The name of my chapter is, *Developmental Neurobehavioral Genetics: The Idea of Development as Explanation.* That's what I think is my most important contribution.

DM- My favorite that I think has influenced a lot of people in different disciplines is *The Roles of Experience in the Development of Behavior in the Nervous System*. What about your favorite publication?

GG- It's called *Normally Occurring Environmental and Behavioral Influences on Gene Activity,* published in the Psychological Review.

DM- Are there any other thoughts that you would like to share with our listeners today?

GG- If something inspires you, even though it seems like it might be a little off the beaten track, and if you're lucky enough to be able to pursue that off-the-track work, it can be very rewarding because at the end of the trail, you will have

discovered something - a new way of doing research, a new finding that hadn't been anticipated. I've been lucky enough to be able to follow my nose. Earlier on I got the research support that I needed. I didn't have to get a grant right away. I had that full time research assistant and therefore gathered data right at the start, then later got a grant to expand the lab further. Unfortunately, we are today very dependant upon financial support from NIH, NSF and other sources. Winning grants means you must first convince senior colleagues who may not be sympathetic to your approach, and try to persuade them that what you want to do is important and is deserving of support.

DM- Well, thank you, Gilbert, so much for joining me today in this podcast. I hope our listeners have learned something about science, about what it takes to be an excellent scientist and an excellent theoretician. So, thank you again.

GG- Thank you for inviting me and giving me such a warm welcome.

The End.