Academic careers, like most others, often begin in the strangest ways. We counsel our children and our students to set goals, make plans and determine how best they can be accomplished. This sounds so reasonable, so rational, so cognitive! I am sure it works that way for some. It did not for me. Looking back, my journey to this point was often unplanned with considerable improvising along the way. As students of human behavior, it is important for us to realize that there is often a chaotic dimension to individual development but it can all work out in the end if we remain alert to opportunities when they arise. So, let me tell you a bit of my story.

Except for my senior year, my early schooling was in Catholic schools in Southern California. School came easy for me and I thoroughly enjoyed it. Once I reluctantly realized that I was not good enough as an athlete to earn a college athletic scholarship, and because the military draft was on, I decided to “Join the Navy and see the World” as the recruiting poster advertised. For an 18-year old kid, that was the start of a series of eye-opening experiences and I
learned many things in fairly quick order that have served me well throughout my life. First, I was enthralled by the diverse cultures I encountered on the beautiful Islands of the South Pacific, sailing from the Marianas to the Eastern Carolines, to the Solomon Islands, to Samoa and Tahiti, as well as to Japan with its intriguing mixture of the medieval and the modern, the Philippines, Korea, that fascinating combination of ancient China and modern England, i.e., Hong Kong, and, of course, the ethnic diversity of Hawaii that was the home-port of the ship on which I was stationed. Second, I quickly learned that having special skills could lead to a more interesting and less physically-arduous life aboard ship. It also allowed for very quick advancement via Fleet-wide competitive exams. Therefore, in less than 18 months, I advanced farther than my father did in his 6 years in the Navy and I found myself supervising sailors who had been in the Navy four-times as long as I. Third, I discovered that I had the ability to lead several lives simultaneously. I was a skilled radarman and anti-submarine warfare specialist, a petty-officer, an athlete, and a carousing sailor “on the beach” all at the same time. This chameleon-like tendency both fascinated and puzzled me and was understood only years later.

So, from my Naval experience, I learned that possessing advanced skills offered special benefits. Therefore, I realized that I needed to go to College. Because my discharge from the Navy was delayed for several months due to an international incident, I only had time to apply for admission to Long Beach City College in my hometown, and even then, just one week before classes began. This was fortunate for several reasons: enrollment was free to California residents, the instructors were excellent, and I met a vivacious 19 year-old girl with whom I quickly fell in love. I also “loved” all of my classes but I did not have the foggiest idea what I wanted as a career. While I was in the Navy, I had spent several weeks at Camp Pendleton training with the
Marine Corps learning how to be a military policeman (a “shore patrolman”), so I thought maybe that is what I should do — become a policeman. On the other hand, biology fascinated me although I was unsure how I could pursue that as a career.

Then I took courses in psychology and sociology and realized that it was possible to study human behavior scientifically just as we do plants and animals. So, I decided on a dual degree in psychology and sociology. At the very least, I thought that would be good training for a career in law enforcement. And, there was the added personal benefit that came from learning about the sociological concept of social roles and the psychological concept of personality traits and how the same trait can be manifested in very different ways. I was now able to understand that chameleon tendency to which I referred earlier whereby the same person could at different points in life be a good student, an altar-boy, a seminarian, an athlete, a member of a rowdy car club, a skilled radarman, a military policeman, and a sometimes hard-drinking sailor who never backed down from a physical challenge.

By the time I selected my dual-major in psychology and sociology, I had married that 19 year-old girl and had gone on to Cal State, Long Beach to complete my undergraduate degree. Several months later, after the birth of our first daughter, we drove north to San Francisco to spend Thanksgiving with my aunt and uncle. My uncle was a senior scientist with the Stanford Research Institute and he gave us a tour of the Stanford campus. At one point, he drew our attention to what he identified as the Administration Building of the Graduate School. To show you how naive I was, I was not quite sure what he was talking about. So I asked him. After explaining it to us, he asked how my grades were and suggested that I explore with my professors the possibility of doing graduate work. Immediately upon our return to Southern
California, I did just that. The problem was, I was unsure which discipline, psychology or sociology, interested me the most. Consequently, I applied to schools that seemed to offer both simultaneously, e.g., Harvard and its Department of Social Relations and the University of Michigan and its Social Psychology program. To my surprise, I was offered admission into these programs as well as the University of California, Berkeley, Stanford University, the University of Washington, USC, and Duke University. Yes, that required a lot of applications to fill out. That I did so, is a good measure of my lack of confidence in getting accepted into a major graduate program.

My eventual choice was Washington University in St. Louis because it had a Sociology/Anthropology Department with an option in Social Psychology. At that time, the Wash U. sociology department was ranked third in the nation behind Harvard and Cal, Berkeley. It was truly a multi-disciplinary program and, importantly, they recruited me. This was an intoxicating time for a young and naive student. Wash U. had an internationally-distinguished faculty that provided exemplary training while generating breathtaking intellectual arrogance. This must explain the fact that, during just my third year in graduate school, I decided that I had learned all the faculty could possibly teach me and that it was time to begin my own professional career, having earned a Master’s Degree in Sociology/Anthropology and since I was about to receive a Doctorate in Social Psychology. The intellectual arrogance of my professors must have been contagious.

But, what was my field of study or academic discipline? This, the 60's, was such an exciting time in the behavioral and social sciences. It was a time marked by growing optimism that we could make the world better through society-wide interventions in health, education, and
welfare. It was also a period marked by intellectual ferment in all of the behavioral and social sciences. In psychology, Freudian and other personality theories were giving way to learning theory; in anthropology, culture and personality theory was beginning to give way to a recognition of the importance of evolutionary biology; in sociology, functionalism and symbolic-interaction were giving way to exchange theory. All of this fascinated me. My Master’s thesis was part of a large funded research project on juvenile delinquency in the notorious inner-city housing projects of St. Louis. The tradition in the 60's, at least at Wash U., was that it was acceptable to derive your Master’s thesis from the faculty’s funded projects. My part of the larger study drew upon the ethnographic methods of my anthropological training as well as my experience in the Navy as a shore patrolman where I had direct contact with many forms of deviant behavior and had come to realize the critical importance of the peer groups to which one belongs.

Master’s theses were one thing, doctoral dissertations, however, were another. The norm for dissertations was that they should be an original design of your own–from start to finish. In my case, it is important to understand that this was the beginning of the Golden Age of behaviorism and behavior modification and I had come under its spell. I had convinced myself that the complex subject matter of sociology and social psychology would benefit from the application of the tactics of experimental research developed and promoted by behaviorists. To test this assumption, my doctoral dissertation was designed to compare different organizational structures and communication patterns in four-person problem-solving groups. I also designed and built the electronic circuitry to run my planned experiments. This required drawing the
necessary schematics, purchasing the equipment that I would need from a Navy electronics-surplus store, and spending long hours disconnecting old wiring and then re-soldering the relays according to my design. Please do not assume that this was easy. There were many mis-steps along the way that required redesign, more soldering, many expletives, and entreaties to the electronic gods. The experiments, however, were a success and I was able to resolve previously contradictory findings in the research literature. The results were published in three successive peer-reviewed publications.

For the next ten years, while I was a faculty member at the University of Washington in Seattle moving up the academic ladder from assistant to full professor, I built upon that initial success and proceeded to experimentally analyze several other kinds of social behavior. I was in other words, a card-carrying social psychologist. Why? Because I was mainly interested in social behavior, social interaction, and group processes and it was social psychology that primarily dealt with these topics and saw them as the product of behavioral interdependence among individuals. This was a time when “small-group” studies and “group dynamics” were vibrant fields of study.

Social psychologist I may have been but, while I was pursuing my research, a dialogue had been going on in my mind that periodically thrust itself to the surface, for my early interest in biology was always lurking nearby. I felt that there was a common human nature that we needed to explain above all else, e.g., bi-parental care, long-term pair bonding, language, our lengthy childhood, deception, cooperation, competition, trust, jealousy, violence, sex-specific reproductive strategies. Yes, there are individual differences in these traits as well as in intelligence, personality, temperament, etc., but there are limits to these differences and the same
differences are found all over the world and in all societies and cultures. In taking this perspective, I was in good company. For instance, in 1871 in the *Descent of Man*, Charles Darwin wrote:

> As man is a social animal, it is almost certain that he would inherit a tendency to be faithful to his comrades, and obedient to the leader of his tribe; for these qualities are common to most social animals. He would from an inherited tendency be willing to defend, in concert with others, his fellow men; and be ready to aid them in any way, which did not too greatly interfere with his own welfare or his own strong desires.

In a manner consistent with Darwin’s assertion about the existence of a common human nature, social psychologists, especially after the end of WWII, had set about experimentally analyzing universal social processes such as the emergence of norms (Sherif, 1936), social conformity (Ash, 1952) and obedience to authority (Milgram, 1961) to give just a few examples. Throughout my ten years at UW, I worked within that tradition and proceeded to experimentally analyze several forms of social behavior, both in and outside of my laboratory and published studies on such topics as the factors influencing children’s decisions to cooperate or compete with their peers, the structure and function of imitation in child development, the conditions under which status hierarchies emerge in small groups, and how differences in power affect the nature of exchange relations in adult dyads and a person’s willingness or unwillingness to tolerate inequitable outcomes. In 1971, in recognition of this work, I was elected President of the West Coast Association for Small Group Research. My pleasure at presiding over the annual conference of the Association was enhanced because it required my return to Hawaii.

While I was carrying out these experimental studies, my internal dialogue continued as I
struggled with another nagging thought. In this case, it was a lesson that I learned in my very first year as a graduate student and from the first book I was required to read. This lesson was that science consisted of more than the obsessive gathering of data. Only doing that was referred pejoratively as mere “dust bowl empiricism” leading, all too often, to outcomes equivalent to the repetitive re-discovery of the wheel. This, I was taught, is science at its worst: overanalyzed measurement for its own sake. It is the kind of thing with which journals are stuffed, and which nobody reads. Good science, I learned, also consisted of exploring ideas and examining their logical power to explain why the data appear as they do. Indeed, the particular complexity of human behavior demands that our research be theoretically grounded. Theories are explanations of empirically established relationships and comprise an indispensable component of the scientific enterprise. The trouble with the behavioral and social sciences is not a dearth of information or data but a glut. We typically have more than we can handle. To be sure, data and theory must be in a proper balance; theoretical generalizations can be reached only from empirical evidence, but it is theory that gives value and interest to data.

Therefore, during this same period, drawing upon my Master’s thesis and behavioral principles such as The Matching Law, another young assistant professor at UW, Ron Akers, and I published theoretical articles explaining delinquent, criminal and drug-abusive behavior. Fifteen years later, and to my great surprise, one of these articles, A Differential Association-Reinforcement Theory of Criminal Behavior, resulted in an award from the American Society of Criminology for being one of the ten most cited articles in the field at that time. As I mentioned earlier, the die had been cast since graduate school and, from that point on, theoretical issues played a significant role in my thinking and had led, first of all, to the co-authoring of a book in
1969, *Behavioral Sociology*, with a former fellow graduate student, Don Bushell, examining the importance of the theoretical principles and research techniques of operant conditioning for understanding social behavior. Theories, of course, serve many functions. They allow us to make sense of empirical observations; they can lead to recognition of unexpected connections between seemingly unrelated phenomena; and, they can lead to previously unanticipated hypotheses resulting in new knowledge. Less appreciated is the fact that theories can also lead us to question anew what we think we know by encouraging us to look at data through different lens. But, I am getting ahead of myself and will return to this idea later.

Toward the end of my tenure at UW, I became increasingly interested in studying behavior in non-laboratory settings. I had grown a bit weary of trying to explain the relevance of my lab experiments to social scientists from other traditions. Frankly, I had come to the realization that social psychology experiments were often merely *demonstrations* of what was already known. Although, if we were honest with ourselves, we would have to admit that this criticism applies even today to much social science research. In any case, in 1974, as Director of the Center for Studies in Social Psychology at UW, I received a request for proposals (an RFP) from NIMH indicating that they were interested in encouraging researchers from outside family studies to investigate a problem that was increasingly on the front pages of our nation’s newspapers, i.e., child abuse. Given my long-held interest in aggressive and violent behavior, I jumped at the opportunity and designed a study drawing upon my earlier work on communication patterns, the observational methods of anthropology and primatology, and the theoretical principles of behavioral psychology. I was fortunate to have my application funded even beyond the years I requested. But before I could get the study underway, Penn State’s
College of Human Development invited me to give a lecture where I described my still-developing and exploratory ideas about the importance of evolutionary biology for understanding human behavior. A few months after my visit, I was asked to apply for a position in the Department now known as HDFS. Given that I was in certain respects a man without a fixed disciplinary identity, the multi-disciplinarity of the Department was and remains very appealing to me and, thus, here I have been for the past 31 years.

Upon arriving, I often felt as if I had returned to graduate school and that I had much to learn in a very short period of time. I found myself with outstanding colleagues who had been trained differently than I; colleagues who read professional journals I had never read; colleagues who went to conferences that I had never attended. It was challenging and exciting and I was permanently changed by the experience. I often felt as if I was hanging on by my finger tips and could fall at any time. I never felt “caught up” nor do I still. It was a very humbling experience. So at the same time that I was beginning my examination of abusive and neglectful families, trying to understand how parents could seriously and sometimes fatally harm their own children, I was also trying to understand the nature of individual development, individual differences, and my niche within HDFS. As part of this effort, Ted Huston, another social psychologist in the Department and I, organized a conference entitled Social Exchange in Developing Relationships. A guiding assumption of the Conference was that relationships have developmental trajectories just as individuals do. The Conference was a success and led to a 1979 book by the same title. The Conference and the book convinced me that I really did fit into this multi-disciplinary Department and College.

That aside, I still had a large research project to get under way where we were attempting
something that had never been tried before, namely the observation of abusive and neglectful families in their own homes. I do mean families because we recorded verbal and physical interactions between and among all family members. That, too, was unique. We were able to do so by using the focal sampling techniques developed by primatologists. Each family member would periodically and randomly be the “focal subject” and we would record any and all contacts between that person and other family members. Little did I know as a graduate student how valuable my training in anthropology would eventually be.

It is sometimes said that all scientists stand on the shoulders of giants. There is much truth to that; we all are influenced by the training we receive and, hopefully, by the trials, errors, and successes of our own scientific experiences. Certainly that was the case for me. For example, I apparently still possessed some of the intellectual arrogance of my professors at Wash U. Why else would I, who had never even had a course in the family, think I could possible contribute some original and significant ideas to a topic as important and perplexing as child abuse and neglect? More importantly, the approach I took to this difficult task was in part a product of my training in experimental social psychology. This is seen most notably in my assumption that we need to distinguish between words and deeds more than we do. I felt then that far too much of behavioral and social science research was based upon the use of questionnaires and interviews and too little upon the actual observation of behavior (i.e., deeds). Similarly, my work in behavioral analysis led to my assumption that critical “process” variables (as opposed to “marker” variables like poverty, social class, or personality) would be found in interpersonal contingencies of reinforcement and punishment operating within families. I also assumed that these contingencies would be evident in day-to-day social interactions that transpired between parents
and children and, given my anthropological training, I assumed further that these should be observed in the natural ecology of the families’ homes.

It was these patterns of interaction among all family members that were the focus of this study, which I called Project Interact, and that were examined in abusive, neglectful, and control families. The “control” families were matched on all relevant criteria to the abusive and neglectful families. These criteria were marker variables that had been found to be correlated with maltreatment in previous studies. An observational code was carefully designed and used to record who interacted with whom (verbally and physically); who was the initiator and who was the target; the emotional affect of the behavior; whether the interaction included a command and, if so, whether the command was followed by compliance or refusal. The intent was to test the hypothesis that there were patterns of day-to-day interaction that distinguished abusive and neglectful families from other families that were similar in all other respects but where neither abuse nor neglect had occurred.

Given these various assumptions and my principal hypothesis, it was necessary to determine the number of observational sessions that would be required to obtain a reliable and valid account of a family’s typical way of interacting with each other. To accomplish this, I constructed a computer program wherein simulated families were assigned different interaction profiles. These simulations were used to determine the minimal number of times each family member should be designated the “focal subject” and, therefore, how many observational sessions there should be. Finally, it was decided that there should always be at least two observers independently recording family interactions so that we could continually assess observer reliability. These observations were recorded with special equipment designed by the Primate Lab
Apart from the issue of reliability, these observer teams were randomly rotated because of the possibility of “observer drift”. To address this issue of observer accuracy, we videotaped several model families and developed a transcript of their interactions. Thereafter, project observers would at unexpected times every few weeks be required to score interactions from variable parts of the videotapes to test for observer drift. Wherever such “drift” was found, an observer would be given a refresher course in the code and re-tested.

Well, after all of this, what did we find? The results from these studies are found in various publications over the next several years. Briefly, when we compared family interactions in abusive and neglectful families with non-maltreating families living under similar circumstances, we found that there was a kind of *basic training* for mutually coercive exchanges within the maltreating families. These parents and their children were found to reciprocate each other’s negative behavior more than their positive behavior, leading to increasingly aversive behavior, escalating counterattacks, and domestic guerilla warfare. This, in turn, contributed to the parent perceiving the child as troublesome and as a costly investment and eventually rejecting the child. This was found to be true for the neglectful families as well as the abusive families. In fact, the largest difference between the maltreating and non-maltreating families was that the former were less contingently positive to one another and they interacted with one another less often. To be honest this was somewhat surprising and I did not fully understand it until later.

Overall, however, similar findings were reported by John Reid in 1984 in Oregon and, in Spain by M. Angeles Cerezo in 1997. Fundamentally, then, the principal hypothesis of Project Interact was confirmed and replicated in other studies.
Following these initial results, an intervention program was designed, implemented and assessed. In general the program was promising in the sense that the relative frequency of contingently positive parental behavior could be increased and coercive behavior decreased. Nevertheless, the gains were hard-earned, modest, and difficult to sustain over time despite the fact that the intervention program also addressed the multiple environmental stresses that emerge in these families and that exacerbated conflict and disaffection between family members. Unfortunately, intervention programs targeting maltreating families continue to this day to have only modest effects.

In summary, the behaviorally-oriented research carried out by Project Interact clearly did add to our understanding of child maltreatment. However, a number of questions remained unanswered. For example, why is it that coercive interaction and parental rejection develop so easily in some families? Why is it so difficult to intervene successfully with maltreating parents? Given that the correlates of child maltreatment are multi-dimensional, involving both marker and process variables, how do we link them all together? Are these various correlates of equivalent explanatory power or are some more important? Is there a theory that can help us answer questions like these? I suggested that there is but it requires that we look at child maltreatment in a new and more comprehensive way. We must temporarily set aside the wide-angle lens of anthropology and sociology and the micro-lens of psychology and behavior-genetics and take up the telephoto-lens of evolutionary biology that has a depth of field that allows us to examine the significance of our evolutionary past for understanding the nature of human nature. Doing so, directs us to a branch of evolutionary biology termed *behavioral ecology* that examines linkages between ecological conditions and adaptive behavior, including parental investment. A key mid-
level theory of behavioral ecology is termed *life-history theory*. In several publications, I have employed a modified version of life-history theory to explain how personal, social, and ecological factors, as well as interpersonal contingencies of reinforcement and punishment, combine to produce the family dynamics culminating in child maltreatment.

I cannot go into all of the details of this theoretical model other than to draw your attention to several of its key assumptions and characteristics. (1) It draws upon research from several disciplines. (2) Because of this, it recognizes that, cross-culturally, the maltreatment of children has a long and inglorious past. (3) It acknowledges that while parental investment is biologically influenced and culturally universal, historically it has also been variable and contingent on a variety of factors. (4) It stipulates that the level of parental investment, high or low, is a function of the ratio of perceived benefits and costs. It is important to note that as costs increase, benefits do not necessarily decrease. The relationship is more complex and takes the shape of a sine curve. This can be seen in Figure 1.

![Figure 1. Ratio of Benefits-to-Costs and Parental Investment](image-url)
(5) Because costs are likely to fluctuate and to do so in short spans of time, perceived costs are assumed to be more influential than perceived benefits in influencing levels of parental investment. This can be seen in Figure 2.

(6) It explains low-investment parenting and child maltreatment as products of a combination of contextual factors, individual and ecological, that impact parents’ perceptions of the benefits and costs of parental investment. (7) It concludes that low investment parenting and child maltreatment are not invariantly linked because proximate antecedents, such as coercive interaction and poor family management practices, mediate the relationship between parental investment and child maltreatment. This can be seen in Figure 3.
I realize that all of this looks rather complicated. Human behavior is complicated. This does not mean, however, that everything is connected to everything else—a view that is hopelessly vacuous. It does mean that the complexities of human behavior can be made more tractable when viewed through the lens of empirically-derived theoretical principles. Evolutionary theory and the mid-level theories associated with it such as life history theory are singularly well-placed to accomplish this task. Why? Because it is the most general theory we have in the life sciences and, therefore, has the greatest potential to unify the behavioral and social sciences. Theories are important because science, as I noted earlier, is concerned not only with establishing relationships between phenomena (empirical research) but also explaining why these relationships obtain. This is the primary function of general theories in science: to explain empirically established relationships. They add simplicity and parsimony to the understanding of how our complex world works. Simplicity and simplistic, however, are not synonymous. Even though general theories usually consist of a few general and simple principles, the derivation or deduction of complex
phenomena from these general principles is seldom a simple matter.

And, apart from its generality, there are other reasons why evolutionary theory can no longer be ignored by behavioral and social scientists. First, empirical support for the theory has been increasing at an ever-increasing rate. This empirical support has been accelerating ever since the breaking of the genetic code. Second, as a result of that breakthrough, we now possess what is essentially a molecular time-clock that allows us to accomplish such tasks as estimating the elapsed time since species split off from a common ancestor. Third, our genome is a sort of autobiography of our species recording when important events happened. It even permits reconstructing the history of human migrations in the last several thousand years. Fourth, it is the only scientific theory that has successfully explained pan-specific traits, i.e., those traits that are shared by all normal members of a species. In our case, these traits describe our common “human nature”.

A core assumption of my approach is recognition of the importance of different levels of analysis–different levels of generality. This was most elegantly explained by the Nobel Laureate, Niko Tinbergen. As he pointed out in 1963: In the life-sciences, explanation always occurs on four complementary levels of analysis. I have recently written that these different levels reflect the fact that the various behavioral disciplines are divided less by the theories they employ than by the problems they address. These four levels include: (1) the evolutionary history of a trait; (2), its adaptive function, i.e., how the trait affects survivorship and reproductive success; (3) the development of the trait in an individual’s life-span; and (4), the specific proximate mechanisms that cause a trait to be expressed at a particular time and place. At the level of theory, evolutionary history and adaptiveness are more general than developmental and proximate antecedents, yet a
common thread runs through each of these analytical levels. The reason for this is that in each case, a genetic process must be involved. The development of a behavior must involve genetic action in some way and the potential or capacity to exhibit a behavior must have been adaptive at some point in historical time. It is important that we understand, then, that we are not dealing with a continuum from nature to nurture: both are always involved. For example, the environment affects development by switching genes on and off that allow for our remarkable plasticity and our ability to learn from experience. Therefore, it follows logically that developmental and proximate mechanisms can be deduced from (i.e., explained by) the first two and more general levels under what philosophers of science call empirically-specified “given conditions”.

Recognizing evolutionary theory as the most general theory in the life-sciences does not lessen the significance of the allied disciplines of anthropology, economics, history, psychology, or sociology, nor their “middle-range” theories such as attachment theory, learning theory, exchange theory, or rational-choice theory, to name a few. The behavioral and social sciences have made many empirical discoveries, but the central intellectual problems of these fields are not analytic, i.e., discovering new and general theories. Rather, their problems are synthetic: showing how genes and environments, in accordance with evolutionary principles, combine to produce our common human nature and the diversity of ways in which that nature is manifested. Each of the behavioral and social sciences and their middle-range theories contribute a piece of the puzzle in our attempt to understand the nature of human nature.

There is nothing too surprising here. The concept of the phenotype, as a product of genotypes, communicates the flexible and variable ways in which individuals respond to differing environmental circumstances and developmental experiences. The ability to adapt to different
environments and to learn different things is a product of natural selection; hence, learning, development, and phenotypes depend on evolutionary history and principles. Tinbergen was right: There are four recurring and complementary levels of analysis. And, while it is certainly acceptable to restrict one’s work to only one or the other of those levels, at some point, and I believe this to be an important point, Humpty Dumpty has to be put back together again. This I have tried to do in the courses that I have taught, and in a variety of articles and chapters, most extensively in my 2005 book with Kevin MacDonald of Cal State, Long Beach. This book is entitled, *Evolutionary Perspectives on Human Development.*

To conclude, as developmentalists there is much that we can learn from biographies and case studies. In this brief intellectual autobiography, I have tried to describe as best as I can how I got interested in the subjects I have worked on and what influenced me. I am quite aware that the picture I have painted has little in common with what the picture of an ideal scientist says it should be. This journey I have taken was often intensely personal and at times a matter of chance. As I mentioned earlier, I was profoundly influenced by the very first book I had read as a graduate student. The book was *Social Behavior: Its Elementary Forms.* The author was the Harvard sociologist, and former President of the American Sociological Association, George C. Homans. I had the privilege of George writing the Prologue to my first book. He opened our conference here at Penn State on *Social Exchange in Developing Relationships* and he was the author of the Forward to the book that followed. I also had the privilege of being invited to his retirement ceremony at Harvard and contributing a chapter to a book in his honor. He was my intellectual mentor although I am confident that were he still alive, he would be dismayed at what I learned from him, which I am also quite sure is not exactly what he would have wanted me to get from
him.

Whatever the case may be, I have in this account come full circle. I began this autobiographical sketch in Long Beach, California; I described my early and enduring interest in biology; my commitment to a multi-disciplinary approach to explaining human behavior and its development; and, why theory is so important in a science as complex as ours.

Finally, let me leave you HDFS faculty and students with your commitment to understanding life-span development, with the following thoughts. In the beginning, each of us arrived here because of a chance encounter between a particular sperm and a particular egg. Had it been a different sperm or a different egg, the result would have been different. This is pure biology. And as an antidote to the intellectual arrogance to which I referred earlier, it is important to remember the melancholy fact that, in the end, all victories are temporary. This, too, is biology.