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## A Life's Pursuit

Paul Ekman

### Introduction

I have spent so much of my life planning what I will do in the future that it is intriguing to look backward, attempting to account for what I have actually done. Leading a life of nearly full-time research has meant that almost every year I have had to propose formally what I would do for the next three, five, and sometimes ten years. These future projections have had to persuade those who periodically decide my fate, convincing them that I would be worth another investment.

I have been an idea peddler, selling possibilities, the chance for a discovery. I have been fortunate in that I have repeatedly been given the opportunity to indulge my curiosity. It has been a life filled with uncertainty about what I will find (that has been the excitement); uncertainty also about whether anyone will care enough to sponsor my search (that has been the worry). Over time I have become addicted to the uncertainties.

Uncertainty embedded in plans. 'Objectives' clearly stated, the 'specific aims' (in no more than the one page the application form allows) to meet those objectives, the 'methods' to pursue those aims, the year-by-year, and sometimes month-by-month activities to implement those methods. And the 'rationale'—the persuasive argument about why my hopes, my curiosity, my self, were worth the dollars. This was, like much else, not what I chose, but what was required of me. But I was ready and eager for such impositions.

Research fellowships and grant proposals explicitly require such planning. When I applied for a Career Development Award from the National Institute of Mental Health (NIMH) in 1965—an award which would pay my salary for five years so I would be less vulnerable to the uncertainties of depending on short-term grants for my salary—they required that I write not just a five-year plan, but a projection of what I would be working upon and thinking about ten to twenty years later. What I wrote then was anticipating and

developed methods, obtained data, and provided some theory, accomplishing all that my vision about nonverbal behavior made possible. Fortunately, I was given a grant I did not seek (I will explain how that happened later), to do cross-cultural research on nonverbal behavior. That opportunity raised questions I had not considered. A stunningly large, fundamental issue remained unsettled—are facial expressions of emotion and gestures universal or specific to each culture? A half dozen studies over fifty years had produced equivocal results due to fatal flaws in research design.

The other major influence which coincided to shift my attention from body movement to the face, from a theory of nonverbal behavior to theories of emotion, was meeting Silvan Tomkins, who showed me the rich information displayed in the face and whose theory of emotion challenged and inspired me. Though never my teacher in graduate school, Silvan has been my mentor, colleague, and intimate friend for the last twenty years.

Initially I was reluctant to study facial expression, for I knew how many eminent psychologists (Allport, Landis, Munn, Hunt, Osgood, Woodworth) had failed in their attempts to unlock the information contained in facial expression.<sup>1</sup> For decades only Schlosberg (1954) continued, and he told me once that he never looked at faces, only at how people use words to describe their inferences. There was then no method for objectively and comprehensively measuring the often rapid flow of complex, subtle facial expressions in any culture. No one knew the answer to such simple questions as: How many expressions can a human being make? How many of the expressions have anything to do with emotion? Studying the face kept me busy for fifteen years. By the mid-1970s, I had (with other investigators—see Ekman and Oster 1979 for a review) settled the universality issue, and I had just published the method for measuring the face that had taken me and Wally Friesen almost a decade to invent. Another shift in my interests was occurring.

I was no longer defining the focus of my interests as nonverbal behavior, nor my task as developing a full account of the information which could be derived from body movement and facial expression. Most of my research dealt with emotion. It concerned either the disguise of emotional expression (deceit) or the relationship among emotional signals, the subjective experience of, and physiological changes associated with, emotion. Looking back, I can see now how those themes first emerged in my

6 Paul Ekman

empirical studies twenty years earlier. But the issues that I now grapple with are far more complex and fascinating, and (amazingly) quite susceptible to testing.

The continuities in how I live my life are probably, to an external eye, most apparent. Mine has been and continues to be a life led in nearly full-time research, made possible largely by grants and fellowship support from the National Institute of Mental Health (NIMH). NIMH began supporting my research on nonverbal behavior in 1955 when I applied for and was awarded a predoctoral research fellowship. My research has been supported by NIMH since then, the only interruption occurring when I was drafted for two years into the Army. The staff at NIMH had confidence in me, and were willing to support high risk, long-term basic research. I am especially grateful to the late Bert Boothe, who was the director of the research fellowships branch, and to Lou Wiencowski, who was the director of extramural research.

Another unusual continuity is that, except for the Army years, I have spent all of my life since 1957 at Langley Porter, the psychiatry department of the University of California Medical School. A psychiatry department is an odd choice for an academic psychologist who does no clinical work, and who spends most of his time in basic rather than applied research. One of the reasons I went there was that Jurgen Ruesch and Weldon Kees had just published their book *Nonverbal Communication* (1956). I stayed because of the other reason: when I was ten years old I visited San Francisco on a family vacation and decided that this was where I must live.

In 1960, when my graduate training was over and my military obligation fulfilled, there were few places which would have given me a salary to study nonverbal behavior. It would be comparable to hiring someone today who intended to study ESP or graphology. Langley Porter did not give me a salary, but it provided a base from which I could try to obtain grants which would. That is how I lived, on 'soft money', until I was fourteen years post Ph.D., when for reasons having little to do with my research I was given an academic position.

The continuities in my life are also apparent in the links between what I study now and what I was studying twenty or thirty years ago. I continue to examine the issue of universals and cultural differences in expression, which I began in 1966. I continue my interest in deceit, which also began in

1966. My major effort continues work begun in 1967 examining the relationship between expression and autonomic nervous system activity. And I am still interested in the question of just what kind of information can be derived from expression, a question I started work upon in 1954.

### **Beginning**

I have often been asked how I became interested in studying nonverbal behavior, body movement, and facial expression. No one asks that question when I give a talk about emotion. That is what psychologists are supposed to study. Although many events and people influenced my career choice, my mother's suicide in 1948 when I was fourteen was the crucial event. I was painfully aware that no one had been able to relieve her anguish, neither her psychiatrist nor her family. I coped with my grief, loss, and anger by deciding that I would dedicate my life to trying to help people like her who suffered from mental illness. I thought then that I would either do research to find the causes of mental illness, give treatment to mental patients, or do both. I felt I had little choice to consider anything else; my grief (which lasted many years) compelled me to pursue these goals.

I was a rebellious high school student, thrown out a number of times for such offenses as telling my homeroom teacher she ran her classroom like a fascist. When I learned that the University of Chicago would accept students after their sophomore year if they passed an admission test, I applied. My high school history teacher told me, 'Paul, you have the audacity of a brass monkey to apply for college given your high school record'. I remember looking up audacity in the dictionary but being unable to find brass monkey. I was afraid to ask anyone exactly what she meant.

Entering college at fifteen was a wondrous experience. In those years, when that University was led by Robert Hutchins, my mind was awakened. It was a startling education, the intellectual atmosphere was both electric and dense, and I was charged by the excitement of ideas. The University of Chicago spirit led its students to believe there was no intellectual challenge which could not be met. I thought I might be able to find a cure for, or the cause of, mental illness. Reading Freud in a course on rhetoric, I found explanations for the inexplicable in my life, and decided to become a

psychoanalyst. His writings about lay analysis convinced me that medicine was not the best route for psychoanalytic training. Psychology would allow me to keep open the possibility of also doing research, and that choice had the fringe benefit of provoking my father.

My relationship with my father was extraordinarily painful and, like much pain, motivating. He had hoped that I would work with him in his pediatric practice. My father had not pursued his own love of scholarship—it was too impractical—and he expected me to make the same sacrifice. Psychoanalysis offended him, and my offensive choice was magnified by pursuing it through psychology rather than medicine, and talking about becoming a researcher. My father never accepted my career, nor gave any sign to me that he approved of what I did with my life.

The University of Chicago in 1949 was strictly 'great books', no electives, no classroom lectures, no major and no minor. Unfortunately, few graduate programs, including those in psychology, would accept Chicago undergraduates unless they went elsewhere and obtained a major. I went to New York University in 1952 to obtain one. I was beginning to be troubled about how individual psychoanalysis could ever cope with the amount of mental illness in the world. Group therapy offered a better hope; at least the therapist-patient ratio was better. I read all I could find about psychoanalytic group therapy, began to observe group therapy sessions, and began psychoanalytic treatment myself.

One of my last undergraduate courses at NYU required a research project. I devised a projective test intended to measure how patients would respond to group therapy. My test was not much, but my teacher, Margaret Tresselt, was great. She gave me the chance to see someone actually do research. Her study of verbal learning did not interest me, but I liked helping her do the experiments. My reading of the emerging literature evaluating the efficacy of psychotherapy convinced me that psychoanalysis was not yet proven to be the panacea I was searching for. I would have to do research examining psychoanalytic treatment, to try to help improve its efficacy, in order to realize my goal of helping people with mental illness. I still was determined to become a therapist myself, but research was becoming equally important. All I really knew about therapy was my experience as a patient, and it was often very difficult. By contrast, research was usually fun. I was nineteen when I applied to Ph.D. programs in clinical psychology.

## Early Years 1954-1963

### *Graduate School*

The Ph.D. program at Adelphi University was the only one which accepted me. Part of the problem was that no one else wanted a student who said he intended to become a private practitioner of psychoanalysis. And my grades were spotty until my last year at New York University, when I finally had learned that I could be just as rebellious by over-achievement as by under-achievement.

Adelphi emphasized training professionals to do clinical work, and sought students who had my goals. Nevertheless, research was required for both the master's thesis and the doctoral dissertation. Robert Berryman, a Skinnerian trained, I believe, with the Kellers at Columbia, taught the research methods course. I found his wit and intelligence enormously appealing, and I became his unpaid research assistant, helping him teach rats to press bars.

For my Master's thesis I wanted to validate the projective test I had begun to develop at NYU. To do so meant that I would have to show that my test would predict how people behaved in group therapy. The only method for measuring the behavior of individuals in small groups at that time was Bales's (1950) Interaction Process Analysis. Observing group therapy sessions, I saw that much of what went on was not spoken but manifest in expression and body movement; but Bales dealt only with the words. I decided I would have to develop objective methods for measuring the information provided by facial expression and body movement. I thought I could accomplish that in two years. It took ten before I made any progress.

Without realizing when it happened, I lost interest in my projective test and became fascinated by the difficulties of measuring body movement and facial expression. Berryman was largely responsible, but the shift in focus was also compatible with my other interests at the time. Since early adolescence I had had an avid interest in photography, and had been toying with the idea of having a second vocation as a photographer. Studying nonverbal behavior allowed me to merge my interests, using my knowledge and love of photography in psychological research. And a focus on body movement fit very well with my strong interest in and involvement with many friends in modern dance circles.

## 10 Paul Ekman

Halfway into the Master's thesis project I realized it would take more time than I had anticipated to develop useful research methods. I wrote my first grant application to the National Institute of Mental Health (NIMH), asking for funding to measure nonverbal behavior in psychotherapy sessions. Jerome Frank, sent as site-visitor, explained that although the research proposal was interesting, a first-year graduate student could not receive a research grant. Instead he suggested that I apply for a pre-doctoral research fellowship, using the same research plan. I did, and that fellowship was awarded in 1955, beginning my continuing association with NIMH.

My approach to nonverbal behavior was, through Berryman, Skinnerian. I was totally convinced that everything about expression and gesture was learned. I made very little progress over the next two years, although I did write my first article (Ekman 1957). I read Werner Wolff's book (1943) about expression and personality and went to visit him at Bard College. He was gentle in his distaste for my narrow-minded approach to what he had studied so inventively. He asked me what else I was doing apart from this research on nonverbal behavior. I described my first job working part time as a clinical psychologist at a mental hospital to help pay for my schooling. It wasn't research, and so I felt free to speculate about what I was observing. Wolff was quite interested in my ideas about the coercive nature of this institution. In many ways the patients were infantilized and punished for being sick. I thought that rather than being treated they were being taught to keep their illnesses to themselves. Wolff invited me to give a paper at the next Congress of the Inter-American Society of Psychology—not about nonverbal behavior, but about my views of this mental hospital! It was 1955, and I found it extraordinarily exciting to be an invited delegate presenting a paper at a scientific meeting.

But the research was not making much progress. Berryman told me that I wasn't likely to be able to complete a doctoral dissertation studying nonverbal behavior. It was too unconventional a topic and there were no methods yet workable. It was too much of a gamble to try to make such exploratory research meet the highly ritualized requirements of a doctoral dissertation. Instead he suggested that I take time out to do a very straightforward conventional experiment that would allow me to jump through the dissertation hoop quickly. Once I had my Ph.D. I could return to research on nonverbal behavior. I followed his advice, completing the

dissertation from start to finish in less than four months. I have never published my dissertation, which attempted to use conditioning to influence opinions about capital punishment. By nodding my head and smiling—the reinforcements—I was able to influence those who were undecided, but not those who held a strong opinion one way or the other, about capital punishment.

*Clinical Internship and the Army*

I chose Langley Porter Institute for my year's clinical internship (1957-58) for the reasons I mentioned earlier. I had already decided to settle in San Francisco, and Ruesch and Kees (1956) had just been published. Although many of the ideas in the book were interesting, others seemed totally wrong; most important, there were no data, just assertions. I was fiercely anti-theoretical as a result of my Skinnerian training. Ruesch was not willing to spend much time with me. I was only a graduate student, and he could not understand why anyone should want to do research on questions he thought his book had answered. The chief psychologist at Langley Porter, Robert E. Harris, befriended me, encouraging me to continue my research on nonverbal behavior. I spent most of my time doing psychotherapy and diagnostic testing, earned part of my living as a free-lance photographer, and planned to pursue a career divided among psychoanalytic psychotherapy, research on nonverbal behavior, and photography.

Those plans were interrupted by the U.S. Army—I was drafted the moment I finished my internship in 1958. I served as a first lieutenant-chief psychologist, at Fort Dix, New Jersey. The opportunities for clinical work were limited, but the research opportunities were extraordinary. I obtained a grant from the Surgeon General's Office to study attitude changes over the course of basic training, finding that externalization of blame and willingness to act upon impulse increased from beginning to end of infantry basic training (Ekman, Friesen, and Lutzker 1962). I thought this was alarming; the Army felt it was quite consistent with their mission to 'teach these boys how to kill, how to put a bullet in the enemy'. I also did a study of military delinquency, which showed that if men were given company punishment rather than a month in the stockade the recidivism rate



## 12 Paul Ekman

was much lower (Ekman 1961). Based on my findings, the Army changed its handling of AWOL offenders. Awed by the impact that research could have, I decided to obtain more research training rather than pursue clinical work when I was discharged, although I still planned to continue as a psychotherapist as well. I wrote an application to NIMH to resume my research on nonverbal behavior and received a post-doctoral fellowship to return to Langley Porter.

### *First Job and Post-Doctoral Fellowship*

Before I was discharged from the Army, Leonard Krasner, then at the Palo Alto Veterans Administration Hospital, offered me a research staff job working on his grant. Krasner was interested in my doctoral dissertation, since his grant involved the operant conditioning of verbal behavior in psychiatric patients. Though I was not interested in conditioning, his job offered twice the salary of the post-doctoral research fellowship. Taking it allowed me to pay off the debts I had incurred in graduate school and the Army. I planned to work there for only nine months and then start the post-doctoral fellowship. Krasner gave me one day a week to work on my own research on nonverbal behavior, but after a few months I felt it was not enough time. Kindly, Krasner and his colleague Len Ullman allowed me to leave after four months to begin my fellowship at Langley Porter.

While with Krasner and Ullman I continued an experiment I had begun in my last year of graduate school at Adelphi. I arranged and studied interviews in which a high-status scientist first criticized and then praised a graduate student. The photographs I took every fifteen seconds were shown to observers. Some saw just the face of the interviewer and his victim, while others were shown just the body. The observers had to guess when the pictures were taken: during the stressful criticism portion or the praise portion of the interview. If the observers were able to do better than chance in their judgments of when the photographs were taken, that would prove that the nonverbal behavior signaled accurate information about the quality of the interpersonal relationship. By comparing the judgments made by those who saw the face with those who saw the body I could determine

which was more informative. (My findings were published a few years later in Ekman 1964, 1965a, and 1965b.)

The most valuable experience I had while in Palo Alto was meeting Gregory Bateson, who was then on the staff of the VA hospital. He had begun his work with Don Jackson on the double-bind, and was intrigued by my interest in nonverbal behavior. He lent me the camera he had used in Bali, although he did not think much of the experiment in which I used it. I was proving the obvious; it did not matter much to him that there was then no proof, and few who believed as he did that nonverbal behavior was a rich source of information.

Gregory encouraged me when he didn't completely mystify me. He led me to think more seriously and usefully about communication and what it might mean to talk about nonverbal behavior in communication terms. Five years later, when I first received support for doing cross-cultural studies of expression and gesture, Gregory gave me a copy of the motion picture films he had taken in Bali in the mid-1930s. When I traveled to New Guinea for my first 'field trip' in 1967, I stopped en route in Hawaii, where Gregory was then living. We did not truly meet on the level of ideas, but his confidence that I could and should go to New Guinea helped me to overcome my fears of trying to do what my training in psychology had not in any way prepared me for. Five years later Gregory and I participated in a small, very exciting conference on human ethology organized by Erving Goffman and Tom Sebeok. Gregory was less friendly, not at all pleased by my proofs of universals in facial expression, and dismayed that I was becoming 'preoccupied' with emotion.

I was a post-doc for three years, working by myself in what was then a very fertile, active research environment. I was excited just to be in contact with full-time researchers. I remember how impressed I was by their ability to design research and spot flaws in experimental designs. There were many moments when I thought I would never become as skilled as they clearly were. Robert Harris, Daniel G. Freedman, and Enoch Callaway encouraged me to think that I could. They made me feel that I was onto something and could succeed. I continued my experiment in which high-status figures criticized and then praised a subordinate, and did another study which showed different patterns of nonverbal behavior in hypertensives as compared with rheumatoid arthritics. This collaborative study with Rudy Moos and George

14 Paul Ekman

Soloman was never published. I had not yet learned that almost everything I write is rejected by at least one journal before I eventually, with revision and new submissions, find someone willing to publish it. That article on hypertensives was the last time I accepted no for an answer from a journal editor or book publisher.

After a year and a half on the post-doctoral fellowship I wrote a proposal for a grant, including in it a full-time salary for myself. Alex Simon, then chairman of the psychiatry department, would not allow me to submit the proposal unless one of the senior psychiatry professors was listed as the author (principal investigator), with me listed as a staff research employee. I was told that this was necessary because my grant would be turned down if it was authored by someone so young (twenty-five) and inexperienced, and that this would injure Langley Porter's reputation. I refused, and instead negotiated to submit my grant through San Francisco State College, without any obligation on their part to hire me if the grant was not funded. It was approved, and the research director of Langley Porter, Enoch Callaway, let me use Langley Porter's research facilities to do the work. A year later Langley Porter's total grant funds were in danger of falling beneath the minimum necessary for receiving other federal grant money, and Dr. Simon invited me to transfer my grant back. I did so, but only after bargaining to obtain the promise of a year's salary support if any future grant proposal was rejected.

In the first year of that grant I obtained additional funds from NIMH to switch from rapid still photography to motion picture film to record nonverbal behavior, and to hire Wallace V. Friesen to work with me. I had met Wally in 1959 when he was an enlisted man assigned to me at Ft. Dix. Wally had done graduate work in clinical psychology at Kansas University, working primarily with Roger Barker, but had dropped out of school and been drafted. I gave him the responsibility for administering and executing all of the research I designed, and he soon began to contribute to the interpretation of findings and the planning of further research.

After the Army years, Wally had been a research associate with Jack Kounin at Wayne State University. We have worked together since 1964, when I obtained the money to pay his salary. Wally completed the graduate requirements and did his dissertation with me at the University of California, receiving his Ph.D. in 1971. Over the years his role has evolved from

assisting to full collaboration. On many of our research projects it would be hard to assign responsibility for each decision or idea. Much of what I have accomplished I would not have been able to do if we had not worked together; especially the work on facial measurement. After 22 years of full-time collaboration, we continue to work together, although now he is seeking his own support to direct independently work which we have jointly planned, on which my role will be much more limited than in the past. To give Wally proper credit for what he has contributed, I have been careful not to use the editorial 'we', instead using that pronoun deliberately to refer only to work or ideas to which Wally made a significant contribution.

#### Initial Studies 1963-1973

My initial grant-supported project involved filming psychiatric patients at time of admission and at time of discharge, and searching for nonverbal behaviors related to diagnosis or improvement with treatment. I was very anti-theoretical, and focused primarily on body movement, accepting the conclusions of academic psychology (e.g., Hunt 1941; Bruner and Taguri 1954) that facial expression was not worth much attention. I had difficulty making much sense of what I so carefully measured. I could not get beyond the level of individual idiosyncrasies. While we found specific bodily movements that indexed whether a patient was severely disturbed or improved, it was a different movement for each patient, nothing generalized across patients. I was greatly helped and challenged by George Mahl to take my first steps in developing a taxonomy of body movements. Using my theoretically guided classification of body movements, we obtained our first results, which differentiated neurotic from psychotic depressed patients and indexed the extent of clinical improvement over time (Ekman and Friesen 1968).

Through serendipity I received a grant from ARPA (the Advanced Research Projects Agency of the Department of Defense) in 1966 to conduct cross-cultural studies of nonverbal behavior. I had not sought that grant, but in the post-Camelot<sup>2</sup> furor ARPA had to spend research money overseas on noncontroversial topics. By accident, I walked into the office of the man in charge of spending that money in the behavioral sciences. Married to a

woman from Thailand, he was intrigued by how expression and gesture might differ across cultures. He proposed that I do such research. I was initially excited about the prospect, but in talking it over with Wally I became convinced that we were already busy trying to make sense of body movement, and were not ready to commit to such a large undertaking. The man from ARPA would not take no for an answer. He flew to San Francisco, persuaded us, and actually helped us draft the proposal.

In trying to decide exactly what to do, I read widely in anthropology and ethology and sought advice from Margaret Mead, Gregory Bateson, Edward Hall, and Charles Osgood. I was committed to a cultural relativist social learning view, expecting to find evidence of cultural differences in facial expression. I only read Charles Darwin after my data forced me to search for an explanation of what I found.

#### *Developing Theory*

Fortunately, through no doing of my own, I had a year off from doing research to think and develop theory. It came as a by-product of our attempt to develop better methods for handling the overwhelming amount of film we were trying to analyze. I obtained funds to build a system which would for the first time interface a small computer with video equipment for automated search and retrieval (Ekman and Friesen 1969a). Since it did not make sense to continue measuring behavior until this new equipment was operable, I had time to think and write.

I don't know how Tom Sebeok learned about me, but we met around 1967, and Tom invited me to write a theoretical article for the first issue of *Semiotica*, the new journal he was formulating. In preparing that paper I read in semiotics for the first time, and more in ethology. The paper I wrote reflected those influences, my earlier discussions with George Mahl, and my increasing interest in emotion stemming from my meeting with Silvan Tomkins (which I will describe shortly). 'The repertoire of nonverbal behavior' (Ekman and Friesen 1969b) offered a typology of nonverbal behavior, distinguishing among five different types of body movements, in terms of their origins (how they become part of one's repertoire), their coding (how meaning is represented or coded in a movement), and their

usage. This set of distinctions has been incorporated in many treatments of nonverbal communication since, and some of the terms suggested—such as 'illustrators' (for movements which are tied to and illustrate speech) and 'emblems' (a term of Efron's [1972] which we redefined to refer to symbolic movements which stand for a word)—are now widely accepted by people studying body movement.

### *Cross-Cultural Studies*

That first theoretical paper also developed our first account of the similarities and differences in emotional expression across cultures, an issue we continue to study. We distinguished which aspects of emotional expression are universal and which are culture-specific, formulating the concept of 'display rules' to denote cultural rules for the management of universal expression. That term now is so commonly used in psychology that it no longer appears in quotes, and no longer is accompanied by a reference. In an empirical study (with the aid of Dick Lazarus) we were able to show that, when alone, Japanese and Americans showed the same facial expressions in response to stress-inducing films, but when another person was present, because of differing display rules, the Japanese, but not the Americans, masked the universal expressions of distress. In that data set autonomic nervous system (ANS) measures were also collected, and collaboratively with one of Lazarus's associates, E. Malmstrom, we did our first study of expression and ANS activity (Malmstrom, Ekman, and Friesen 1972). We found a tight linkage between expression and ANS activity, and evidence of emotion-specific ANS activity. It was not until ten years later, in 1982, that I found another collaborator (R. Levenson) to resume this aspect of my work, which is now one of my central research projects.

In my work in a preliterate culture in New Guinea, in 1967 and 1968, I learned how to combine naturalistic observation and controlled experiments, and how to do the latter in a field setting. My first trip to New Guinea was frightening and exciting, and the amazing part was that I felt those two emotions nearly every minute of the day. The fear had many sources. I was living in a very strange setting, with people who were charming but who also, in the first weeks, were frightening. Not knowing their language,

working through schoolboys, some of whom had learned Pidgin, I did not always know what was happening to me. One day, for example, the village elder began to squeeze my thigh and smile at me. People quickly gathered around and began to jump up and down, shouting 'whey' again and again in what seemed to me a bit of a frenzy. I did not know what was happening or what to do. Later I learned that the elder who was squeezing my thigh was saying that if I died he would eat me. These people only ate those they respected, and everyone was jumping up and down with excitement about my having been accepted by the elder as someone he respected!

I was also afraid of contracting Kuru, a disease which I knew was widespread among those people. Kuru is fatal, with no known treatment, and was killing about half of the adults. When I was there no one yet knew whether Kuru was (or could be) transmitted by direct infectious contact or whether the transmission was by cannibalism.<sup>3</sup>

More than Kuru, I was afraid of failing, of giving in to my wish to flee and return home, having been unable to figure out how to do research in that very different setting. Even if I stayed I was afraid that my results would be inconclusive. But the fears were by no means all that I felt. More often I felt extraordinary excitement. Hiking through the New Guinea highlands, coming into villages where I was the first or second Caucasian to enter, I felt I was fulfilling my boyhood dream of living the life of Magellan! The results from my first trip were not totally conclusive (Ekman, Sorenson, and Friesen 1969), but I learned how better to plan what I would be able to do on a return visit. Wally accompanied me on my second trip, and we obtained solid evidence of universality in facial expression but cultural differences in symbolic gestures (Ekman and Friesen 1971; Ekman 1980).

I reported our findings of universality at scientific meetings of both the American Psychological Association and the American Anthropological Association in 1967, 1968, and 1969. I had not expected the outraged hostility I encountered from some of the anthropologists. Alan Lomax, Jr. tried to prevent me from completing my talk at one of the Anthropology meetings. He rose from the audience shouting that I should not be allowed to continue to speak; my claims of universality in expression, he said, were fascist. I could not see how showing the commonalities in emotional expression across all people could be interpreted as fascist or communist or capitalist. If there was any political implication it was one of unity and

brotherhood. On another occasion a Black radical activist who advocated separation accused me of being racist for claiming that Black facial expressions of emotion were no different from white expressions.

My unforgivable sin, it seemed, was support for Darwin, for an evolutionary basis for facial expression. It was not a creationist objection, it was not fundamentalist religion that aroused the passionate attacks. It was an equally strong commitment to the idea that everything important about social behavior is learned. I was challenging the domination of cultural relativism, environmental determinism, the Lockian view that we are *tabula rasa*. I, of course, had started from just that position myself—my research had simply proven me wrong. In that sense I had discovered something, rather than simply proving what I already knew. To many social scientists then, and still some today, acknowledging a biological basis for emotional expression is opening the door to claims that one race is superior, biologically, to another—a strange distortion of my belief that I was providing evidence of unity and brotherhood.

I was also crossing trade union lines, practicing anthropology without a license. Margaret Mead (1975) wrote a five-page attack on me as an example, in her words, 'of the appalling state of the human sciences'. She criticized me for doing improper anthropology and for having the gumption to disagree with one of her favorites, Ray Birdwhistell. It was true, at least, that I was repeatedly attacking Birdwhistell in print. I had shown his claim that all that is universal about facial expression is the anatomical equipment to be wrong. Many people had accepted Birdwhistell's assertion—without any systematic data—that the relationship between sign and significant (expression and emotion) varied widely from culture to culture. I enjoyed being assaulted; at least people were taking me seriously enough to try to demolish me and my work. Over the next ten years most of those working in the social sciences accepted our findings.

In 1965, just before we began these cross-cultural studies, I met Silvan Tomkins. Since then we have met once a year (it would have been more often, but we are on opposite coasts) and talked by telephone at least once a month. He is the major intellectual influence in my life. When very few others in academic psychology thought I was doing important work, Silvan gave me enormous encouragement. I do not know whether I would have



been able to pursue my life's work without the confidence he instilled in me.

Silvan made me realize how important theory could be in leading to discovery, overcoming my Skinnerian bent. He helped me plan my initial cross-cultural studies. The days we spent together in the mid-1960s convinced me that the face was an extraordinarily rich source of information, the most important of the nonverbal behaviors, although he did not convince me that expressions are universal—that took data. In talking with him, I was exposed to a barrage of ideas, some fascinating and others outrageous. The general view put forth in his theory of affect has provided the framework within which I think about emotion. Some of his specific ideas—particularly his concept of an affect program—are central to the theory I am developing. I do disagree with some other parts of his theory (facial feedback and the density of neural firing), but those disagreements have never been a source of friction between us. Silvan's love for ideas, his excitement about discovery, is congenial and contagious. There have been few experiences more enjoyable than the spinning of possibilities that occurs in our many talks over two decades. I have had great fun testing and exploring ideas, both his and mine, in research.

By contrast, neither I nor anyone else I encountered had many exciting ideas about nonverbal behavior. Nonverbal behaviors are difficult to quantify, and because of that were not measured even though they are important in understanding a number of phenomena, from psychotherapy to attachment. Once we and others developed techniques for measuring nonverbal behavior, there were no large issues or questions to address within the constraints of just considering nonverbal behavior. We are talking organisms! (Even those nonverbalists who sought to maintain the exclusivity of their specialization by studying infants found they had to deal with sound-making and, soon enough, the emergence of speech). By 1980 it was clear that the most fascinating questions about the face involved emotion, and that it was emotion itself which I wished to understand. My knowledge of facial expression would be an enabling route for its study, but not an exclusive source of information. But before that transition, I spent nearly a decade in methodological work on the face.

I was struck by what Tomkins could see in facial expression. There was no magic—he could point out exactly how he was reading expressions to

draw extraordinarily accurate inferences, not only about emotion, but about personality and culture as well. I remember very clearly the time Silvan opened my eyes. It was 1966. Wally Friesen and I had spent a few hundred hours examining films, loaned to us by Carleton Gajdusek, of two stone age cultures in New Guinea—the Fore, whom I visited the next year, and the Anga. We saw no expressions which were unfamiliar, and we thought that we could understand their expressions without difficulty. This was our first glimmering that expressions might be universal. Wally and I worried, however, that our interpretation of the expressions might be biased, for we had seen the films many times, and there were clues about what was occurring other than just facial expression. Our impression that someone felt happy at one point and afraid at another point might have been influenced, consciously or not, by what came next in the film. As a check, we asked Silvan to look at portions of the films. We did not tell him anything about either culture, and we did not let him see any contextual information, just isolated expressions.

Silvan judged the expressions correctly in that the emotion he saw usually fit with the context of the film. When we stopped the action in the film and held an expression on the screen, Silvan had no difficulty in pointing to exactly what it was in a face that indicated the emotion being experienced. When we asked for his impressions concerning these two cultures, he performed what seemed almost an act of magic. One group, he said, seemed quite friendly. The others, he suspected, were explosive in anger, highly suspicious if not paranoid in character, and homosexual. It was the Anga that he was describing (whom we did not study); his account fit what we had been told by Gajdusek, who had worked with them. They were still an 'uncontrolled' group, having repeatedly attacked Australian officials who tried to maintain a government station; they were known by others in New Guinea for their fierce suspiciousness; and they led homosexual lives until the time of marriage! A few years later the ethologist Eibl-Eibesfeldt literally had to run for his life when he attempted to work with them.

After that meeting with Silvan, I decided to devote myself to developing an objective way of measuring facial behavior to make available to anyone what Tomkins could see. I did not realize that it would take nearly eight full years to do so.

## 22 Paul Ekman

Another very significant event in shaping my career was the receipt of a Career Development Award Type II, in 1966. Uneasy, after eight years, about continuing to rely upon grants for my salary, I was just about to take an academic job. The five years' salary support offered by the Award was sufficient to risk staying off the academic ladder, free to continue full time in research. For the first time my own salary was not paid by my research grant; I was under a little less pressure to grind out findings to meet grant renewal obligations every two or three years. I could afford to take on research projects which would not produce a publication for three or four years.

### *Deceit and Political Behavior*

In this same time period I wrote another theoretical paper, titled 'Nonverbal leakage and clues to deception' (Ekman and Friesen 1969c). I cited Erving Goffman in that paper, not knowing then that he was the chief reviewer of the paper. We met two years later and, as I will describe later, became close friends. My interest in deceit arose from the reactions of both colleagues and students to my emerging findings on body movement and psychopathology. The psychotherapy researchers at the Third A.P.A. Research on Psychotherapy Conference, held in 1967, thought my results interesting, but did not plan to add measures of nonverbal behavior to their research until I could show it would provide information not readily available in verbal behavior. The psychiatric trainees I occasionally taught also wanted to know something not available in words. They feared making the mistake of trusting a patient's verbal account of feeling better, giving the patient a weekend pass, only to learn later that strong negative affect had been concealed in order to win freedom from supervision to commit suicide more easily. What should they look for in the nonverbal behavior, they asked, which would betray such a deceit?

The study of nonverbal behavior in interpersonal deceit intrigued me. It offered the opportunity to show the practical value of studying nonverbal behavior, and also to deal with a fundamental question basic to any account of nonverbal communication—the extent to which body movements and facial expressions can be controlled, inhibited, or simulated. We examined

closely a few of our psychiatric patient films in which, we knew from subsequent confessions, the patient had been concealing plans to commit suicide. Based on these observations I then developed a theory to explain why particular (not all) nonverbal behaviors would be likely to escape attempts to censor and disguise communication (Ekman and Friesen 1969c). We developed an experimental deceptive interaction, which attempted to model the situation of a patient concealing strong negative affect. The subject was motivated to conceal from an interviewer negative affect aroused by watching stress-inducing films.

In subsequent years we have confirmed a number of hypotheses about the particular types of body movement and facial expressions which provide leakage of negative affect or tip off the observer that deception is in progress (Ekman and Friesen 1974). Our theoretical paper in 1969, and subsequent empirical papers, may be responsible in part for the recent development of interest in studying nonverbal behavior during deceptive interactions. More than three dozen empirical studies on this topic have appeared in the last decade. My interest in deception continues. As I will explain later, my recent book *Telling Lies* (Ekman 1985) has further developed a theory of lying; I continue research on the differences between voluntary and involuntary facial behavior, and have begun to study lying in children.

From 1961 to 1966 I also worked on war and peace issues, as a scientist and as a citizen. Motivated by the threat of war—made palpable by the Berlin Crisis in 1961 and then the Cuban Missile Crisis—I became very involved in the peace movement, working simultaneously in the American Friends Service Committee, Committee for a Sane Nuclear Policy, and the World Without War Council. Looking back, I am amazed that I could work simultaneously in peace groups that had such different positions about the obstacles to peace. At the time I was quite naive, convinced that the problems between the U.S. and the U.S.S.R. were psychological in nature, explainable in terms of misperceptions, distrust, and misunderstandings. I failed to appreciate the fundamental nature of the conflict—political, social, and economic—in the world, and how difficult it is to preserve democratic values and institutions without resorting to organized violence.

Despite my naivete in those early days, I did believe that a scientific engagement could only be helpful if the scientists acted as scientists rather than as propagandists. Political values might be responsible for the choice

to do a particular research project, but once undertaken the project must be pursued objectively. In any research one cannot know in advance what the outcome will be, but one should never hide or distort findings to meet one's preconceived position, even if that position is to try to advance the cause of world peace. I organized, and for a few years led, a behavioral science peace research group—the Committee for the Application of the Behavioral Sciences to the Strategies of Peace. Its hallmark was what I called a 'balanced bias', recruiting scientists who disagreed about a political issue to work together, so that the possible influence of their political commitments would be balanced in the choice of methods and interpretation of findings. The group was consulted by both peace groups and government, utilizing the volunteered time of about two hundred scientists to do applied action research, which had some impact on the public debate about civil defense and other war/peace issues.

I seriously considered a career in politics or government, and was offered the financial backing to run for Congress. I was also offered a position in the newly formed U.S. Arms Control and Disarmament Agency. I was strongly drawn to these choices, but felt I could not walk away from the research I had begun. Having accepted grant funds, I felt an obligation to keep at it until I had made substantial progress in meeting my objectives. By the time I had, politics was no longer an option, nor that much of an interest.

Through my peace research I obtained a half-time appointment as a research associate at the Institute for Political Studies at Stanford University in 1963. I embarked upon a second research interest quite unrelated to nonverbal behavior. I pursued studies of public opinion about war/peace issues (Ekman, Cohen, Moos, Raine, Schlesinger, and Stone 1963; Ekman, Tufte, Archibald, and Brody 1966; Verba, Brody, Parker, Nie, Polsby, Ekman, and Black 1967). I also developed an experimental bargaining game to test my hypotheses about threats and conflict. In 1966, when I began the Career Development Award, Type II, I was urged by the NIMH review committee to close this area of research. It was becoming increasingly difficult to maintain two laboratories on two different campuses forty miles apart, on two unrelated topics, with work at both solely dependent on the soft money I had to raise. In the last few years, as I have written about how

my work on lying applies to international negotiations and espionage, I have reintegrated this early interest in political behavior.

My work in politics and peace research was at least partially responsible for my finally obtaining a faculty position on my campus. During the Cambodia crisis, some of the students and faculty on my campus wanted to close the university for a few days to protest President Nixon's policies. A campus-wide meeting to decide the issue was announced, to be held two days later. I was opposed to closing universities as much as I was opposed to the Cambodia and Viet Nam wars. Drawing upon my contacts through my earlier peace research days, I assembled a team which put together a public opinion questionnaire, gathered data on the attitudes of 600 San Franciscans, completed the analysis, and produced a report within twenty-four hours. At the meeting I handed out the results, which showed that closing the university would alienate some of those opposed to the war, without gaining any additional support. My proposal to keep the university open extra hours for teach-ins about the war won support. I arranged for a public debate between a supporter and an opponent of the Cambodia policy. A small student group threatened to disrupt the meeting if the Cambodia policy supporter was allowed to speak. The chancellor, when we met for the first time, told me that since he would be away that day I as the moderator would be responsible for handling matters, including the decision of whether to ask the campus police to remove the disruptive protestors. I managed to get the audience to quiet the disrupters, and an open debate occurred. Soon afterward the chancellor offered me a professorship. I had always thought that it would be my successful research on nonverbal behavior which would some day earn me a professorship; it seems my political skills played a role as well.

#### *First Books*

I spent much of the late 1960s abroad, studying expression and gesture in six cultures. I found much greater agreement among observers in their judgment of emotion than had been reported in the previous literature. My findings were often met with skepticism. How could I be correct in light of the negative findings of Hunt, Landis, Munn, Sherman, etc., reified for

more than two decades in every introductory psychology textbook? These venerated scholars had shown that the face was a noisy, inaccurate source of information. In part to answer such objections, I undertook a review of the literature from 1914 to 1970, reanalyzing the data from a number of these classic experiments. That work culminated in the publication of my first book, in 1972 (co-authored with Wally Friesen and Phoebe Ellsworth), *Emotion in the Human Face: Guidelines for Research and an Integration of Findings*. It showed that major methodological flaws invalidated many of the earlier studies, and reanalysis of results from old experiments, when the data were available, fit my own results. (That book was turned down by seven publishers before, two years after writing it, I found someone to publish it. Two years ago the book was honored as a 'citation classic' for being cited so often in the scientific literature.) Shortly thereafter, I edited another book, *Darwin and Facial Expression: A Century of Research in Review* (Ekman [ed.] 1973). That book brought together studies of facial expression—in nonhuman primates as well as in infants and children—and cross-cultural studies.

### Measuring the Face

#### *Developing the Facial Action Coding System (FACS)*

I was approached by the Surgeon General's advisory committee on television and social behavior in 1971. They were commissioning a crash research program to determine the influence of television violence on children. They asked me to examine the facial expressions of children watching television. We found that children who looked happy when witnessing a murder subsequently were more likely to try to hurt another child than children who looked disgusted or afraid when watching the same televised violent act. Those children who looked displeased were likely to try to help another child subsequent to viewing the television program (Ekman, Liebert, Friesen, Harrison, Zlatchin, Malmstrom, and Baron 1972). This evidence that facial expression could predict future social behavior fueled my motivation to develop a way to measure facial behavior directly

instead of relying on the inferences about emotion made by observers viewing expressions.

We (Ekman, Friesen, and Tomkins 1971) had developed a rudimentary technique for measuring facial movement. Though it was workable, I was becoming aware of its inadequacies, particularly in isolating signs of facial leakage in my videotapes of deceptive interactions. About that time I met Wade Scaford, an anthropologist who was knowledgeable about facial anatomy. He showed me some glaring omissions in our rudimentary measurement technique. For example, we had completely ignored the action of the mentalis muscle, which pushes the skin below the lower lip upward. I decided that I would have to learn thoroughly the anatomy of facial actions. I suspected this would take six months. Instead it took seven years.

I was reluctant to take on this task, because I knew that I would have to become expert in facial anatomy, a prospect I found thoroughly unappealing. Wally was initially opposed to our taking on this task, arguing that it would over-tax our resources and capabilities. Once we began, however, facial measurement became the project in which his role changed from collaborative but junior partner to equal participant. We thought we would be able to build a measurement technique based on the functional anatomy of the facial muscles. Instead we found that such functional anatomy work had not, for the most part, been done. The anatomists had named and distinguished the facial muscles not by their capability for independent action or their affect on appearance, but by location. We had to do a functional anatomy of the face.

Friesen and I spent two years learning how to move each facial muscle independently and studying the appearance changes each movement produced. I also resurrected Duchenne's (1862) technique of inserting needles into my own face and electrically stimulating my facial muscles to learn how each muscle changed the surface appearance. Over the next few years we examined over 5,000 different combinations of single muscle actions in our quest to provide a comprehensive tool for describing any facial movement. We photographed and filmed the changes in facial appearance produced by each single muscle action, each combination of two muscles (more than 300 expressions), each combination of three muscles (more than 3,000 expressions), and many of the combinations of four, five, and six muscles. We decided early on that the scores for an expression would be the muscles



which, singly or in combination, produce that expression. Having made a film and video record of the expressions, our next step was to describe and explain to an observer precisely how to infer from surface appearance which facial muscles had acted—how to perform a live dissection.

After four years, our new measurement technique—the Facial Action Coding System (FACS)—was complete. We trained six people, and determined that reliability was satisfactory (Ekman and Friesen 1976). FACS is a comprehensive facial measurement technique. Unlike other attempts to measure facial movement (see Ekman 1982 for a review of fourteen techniques for measuring the face), FACS can describe any facial movement observed, not just those on an a priori list. FACS scoring entails decomposing any facial expression into the elemental muscular actions which combined to produce the expression. Only a technique such as FACS could test our predictions about which facial actions signal which emotions, and only by having developed FACS, which taught us how many signals the face can generate, could we have developed the predictions. FACS allows study of any facial movement for any purpose, not just those movements relevant to emotion.

In 1975, when FACS was completed, I faced a difficult choice—to begin to use FACS to answer my own substantive questions, or to develop FACS into a self-instructional package so that others could learn it. Although I was tired of methodology, and worried that more methodology would cause me to lose my grant support (which was not awarded for such work), I nevertheless decided on the latter course. The decisive consideration was my judgment that the number of questions to be answered about how facial expression might be related to emotion and personality required that many investigators work independently. By making FACS available to others, I could increase knowledge much more rapidly than by pursuing substantive work myself. If many investigators were to use the same measurement technique, knowledge would accumulate quickly. It took two and a half years to develop self-instructional materials so that others could learn FACS. FACS was published late in 1978 (Ekman and Friesen 1978). It consists of a self-instructional manual, illustrative photographs, two 8 mm motion picture films which give illustrative and practice material for scoring, computer programs, and a final test which the learner can take to determine reliability.

*The Scientific Community's Reaction to FACS*

Since it was published in late 1978, more than 200 investigators (faculty members and graduate students) have learned FACS, using the self-instructional materials, and have obtained high reliability on the final test without any direct contact with us. The number of people learning FACS is steadily increasing; twice as many people learned FACS last year as learned it the year before. While most of those learning the system are psychologists, the group also includes zoologists, psychiatrists, neurologists, linguists, anthropologists, sociologists, and ethologists. Scientists from eleven countries have learned FACS; about half of those using FACS are Europeans. About one third of the current investigations deal with infants and children, another third deal with psychopathology, and the remaining studies cover a wide variety of problems, from facial syntactic signs in the deaf to facial clues to neurological lesions. FACS has proven to be, as I had hoped it would, a general purpose measurement technique, useful to anyone who wants to measure facial movement. It deals with nature in all its complexity, allowing the data to determine which facial actions should be treated as synonyms and which actions signal different matters.

Not everyone, of course, has been enthusiastic about FACS. Some psychologists have been appalled by the detail in FACS, by the time it takes to learn and to use it. A quick procedure for identifying a handful of emotions is what they want. Since we have only a few words for emotion, I have been asked, how can there possibly be so many different emotional expressions? Most of what FACS distinguishes, some of my impatient colleagues have asserted, must be synonyms. They don't realize that it is only with an instrument such as FACS that we can find out whether they are correct. For example, nature provided us with some eighteen different ways the facial muscles can combine into recognizably different expressions which people will label as disgusted. Are they synonyms? Should all eighteen expressions be collapsed into one scoring category? It is only by distinguishing among them, as FACS does, that an empirical answer can be obtained. In the past six years, we have found that various expressions of the same emotion mark the occurrence of different experiences (Ekman, Friesen, and Ancoli 1980).

The most convincing evidence of the worth of FACS comes from research obtained with it, in particular research obtained by investigators who (unlike us) have no vested interest in its utility. In the Fall of 1985, Europeans using FACS organized their second conference. Their first conference, two years earlier, had dealt with methodological problems in using FACS. By 1985 there were findings to report. The conference was attended by almost fifty faculty and graduate students from six countries. There were 21 presentations of research using FACS, in studies of infants, children, adults, and mental patients. Articles are just now appearing in the English-language scientific literature; a number have already appeared in the non-English literature, which does not have as long a publication lag.

The best evidence of the utility of FACS would come from a study which compared the yield from FACS to that from a simpler technique for measuring the face. Such evidence was provided in a recent study by Davidson and Fox (1982). Their purpose was not to compare methods, but because they did not obtain the expected results with a simpler method (the Max technique, developed after FACS by C. Izard), they rescored their materials with FACS. Their aim was to see whether their findings of greater left frontal EEG activity in positive emotions would be evident in ten-month-old infants. They scored videotapes with the simpler facial measurement technique and found no difference in EEG asymmetry between smiles to strangers and smiles to mothers. The predicted EEG asymmetry was obtained when they used FACS to distinguish, on a muscular basis, among different types of smiles. These ten-month-old infants showed what we have interpreted as both emotional and non-emotional smiles. More non-emotional or voluntary smiles were shown to the stranger and more emotional, involuntary smiles to mother. When these were compared there was more left frontal EEG activity during emotional smiles to mother than during non-emotional smiles to the stranger.

#### *Other Work*

While still in the midst of developing FACS, I began teaching medical and nursing students about facial expression and body movement. We (Ekman and Friesen 1975) wrote a practitioner's guide to learning how to recognize

facial expressions of emotion. It was based on what we learned in our cross-cultural work and in the process of developing FACS, simplified for use, in real time, by the practitioner. It also was rejected by many publishers, not accepted until the eleventh submission.

Although most of our time from 1971 through 1980 was spent in developing FACS, we also did some empirical studies, all of which were published in this time period. This included two studies of facial expression and body movement in deceptive interactions (Ekman, Friesen, O'Sullivan, and Scherer 1980; O'Sullivan, Ekman, Friesen, and Scherer 1985); studies of facial expression in infants (Oster and Ekman 1978) and in children with genetically based mental deficiency (Johnson, Ekman, Friesen, Nyhan, and Shear 1976); and a validity study showing that FACS could differentiate positive from negative emotions, differentiate within negative emotions disgust from fear, and determine within positive affect which of two experiences was most enjoyable. I also did a developmental study of the capability to voluntarily control facial muscles; this included a new test of the capability for voluntary facial action (Ekman, Roper, and Hager 1980). In addition, we brought to completion some earlier studies of body movement, including reports on the 'vocabulary' of symbolic gestures in five cultures (Johnson, Ekman, and Friesen 1975; Ekman 1976) and hand movements in psychiatric patients (Ekman and Friesen 1974).

During this time I became friends with Erving Goffman. We spent time together looking at small pieces of social behavior, examining in fine detail what transpired. A few times Harvey Sacks joined us. From our first acquaintance I found Erving's views challenging, provocative, and almost always on a different plane from my own. When we examined a social incident, our analyses and interpretations were not contradictory, but unrelated.

Once we arranged an interaction which we hoped would be useful for joint study. I suggested recruiting two students who belonged to opposing political groups to discuss their differences. As their interaction proceeded, I pointed out to Erving some of their more interesting expressions, which we would be able to dissect later when reviewing the videotape. He, however, was taken with the fact that serious people were willing to engage in such conversation in a laboratory setting, and decided to test how much interference they would tolerate. Dressed in his usual casual style, he posed

(quite credibly) as a janitor. He walked into their room, saying that he had to remove some of the furniture. He removed one piece of furniture after another while they continued their argument, until finally he took away the chairs in which they were sitting. They continued their argument standing up! For Erving, the videotape demonstrated that someone who was not a player—Erving—could not really interfere with the scripted interaction. For me, the videotape offered the opportunity to examine how disagreements and anger are expressed and controlled during argument. We were looking through different lenses.

Erving liked my interest in very close descriptions of behavior, although he was offended by the notion of universals in expression—loyal to his teacher, Birdwhistell. He changed his mind after reading the book I edited on Darwin, which laid out the argument and the evidence. With a twinkle he told me that my evidence meant nothing, it was my logical argument which changed his mind. Erving was committed to a disciplinary approach to a problem and thought I was mistaken in not caring enough about my discipline and being willing to use methods and to deal with phenomena which were not central to my discipline. Emotion was a useless concept for him because it was not, as he saw it, a social fact. I enjoyed showing him how many times he had used emotion terms in his writings. Over the years we became good personal friends, and a few months before he unexpectedly died we had arranged to both spend a month, with our families, in Paris. I was nearly finished writing my book on lying and had many conversations with him about it. I was in his turf, he said, and although we approached lying from opposite perspectives, he liked what I was doing.

His friendship meant a great deal to me. When we first met he was an awesome figure, renowned for his work and his sometimes insulting manner. Over the years he became a close friend, an older brother whose barbs were gentle and caring. While we never saw matters the same way, he helped me demarcate my level of analysis from that which he so brilliantly staked out. He was a wonderful writer, and I used his style and manner of writing as a model when I wrote *Telling Lies*.

In the late 1970s I also wrote three theoretical pieces. One further developed what I called a 'neurocultural account of emotion', my first attempt to answer the question of what an emotion is and how we can recognize it (Ekman 1977). Another was a semiotic analysis of the face,

distinguishing three sources of information linked to eighteen different messages the face can provide, one of which is emotion (Ekman 1978). A third made the distinction between emotional and conversational signals, with particular reference to the face, and also critically reviewed past speculations about the origin of facial expression (Ekman 1979). I began to write *Telling Lies*, and wrote (Ekman and Oster 1979) the first review of facial expression to appear in the *Annual Review of Psychology*.

**Recent Years: 1981-1986**

These have been extraordinarily exciting years. Projects which were underway for many years have been brought to completion. I have started entirely new research projects which are producing intriguing findings about emotion and its expression. My intellectual development has been influenced and intermingled with the careers of three extraordinary scientists: R. Davidson (University of Wisconsin); R. Levenson (University of California at Berkeley); and K. Scherer (University of Geneva). Working with them, I have been able to explore problems that would not otherwise have been approachable. These collaborations have taken the place of work with graduate students which would have occurred if I had been located in a psychology department at a major university.

*The Search for Cultural Differences*

I initiated another cross-cultural data collection in the late 1970s, enlisting the cooperation of scientists in eight countries and collecting data myself in two others. This research was funded only by the travel costs provided for lectures I gave, and so took many years to complete. Once again the evidence completely contradicted what I thought I would prove. I had selected the expression of contempt for study because I thought it was likely to be recognizable only in Western cultures. Instead the evidence showed that it is just as universal as anger, fear, or disgust (Ekman and Friesen 1986). We did find evidence of cultural differences in the judgments of how strongly an emotion is displayed (Ekman, Friesen, O'Sullivan, Chan,

Diacoyanni-Tarlatzis, Heider, Krause, LeCompte, Pitcairn, Ricci-Bitti, Scherer, Tomita, and Tzavaras, in press).

*Deceit*

I have continued my interest in deception, working on both a theoretical and an empirical level. Soon after my first article on nonverbal leakage in 1969, I met Klaus Scherer, who was then finishing his doctoral dissertation with Bob Rosenthal at Harvard. I knew I could not isolate the behavioral clues to deceit without adding his measures of the voice to our measures of bodily and facial movements. I interested Scherer in joining our study of the experimental deception situation I described earlier. The work proceeded slowly because he returned to Germany in 1972. Since then we have managed to spend at least a month every other year in each other's laboratories, but the geographical separation has stretched out the time required for data analysis. Scherer and I have co-edited two volumes, one on emotion (Scherer and Ekman 1984) and one on methods for measuring nonverbal behavior (Scherer and Ekman 1982), and have co-organized two international conferences; we also jointly played leading roles in founding the International Society for Research on Emotion.

Another delay in completing our joint work on deception was the nearly ten years Friesen and I had to spend developing FACS. But without FACS we could not identify the subtle signs of facial leakage. Nevertheless, every few years since 1969 we (myself, Scherer, M. O'Sullivan, and Friesen) have published new findings. After Maureen O'Sullivan (a psychometrist who got her degree with Guilford) joined my laboratory, we did several studies of observers' judgments of honest and deceptive interactions. We are now preparing reports which will draw that work to a close, integrating our measurements of face, body, voice, and speech content with the inferences observers draw from each of these sources.

I began a new series of studies relevant to deception which more broadly addresses the question of whether it is possible to tell from the facial muscular activity itself whether an expression was generated voluntarily or involuntarily. Some of this work was with one of my few doctoral students, Joe Hager. We found evidence in support of three markers which I

proposed would distinguish voluntary from involuntary expressions: the extent of bilateral symmetry, the specific muscles which act jointly, and the differential latencies of the component muscle movements. A half dozen other investigators have now replicated our findings, in particular our ability to distinguish, among happy expressions, involuntary signs of actual enjoyment from more voluntary social or masking smiles. This distinction appears to be very robust, applicable across a wide age range (including infants) and very diverse populations and settings (Ekman 1980; Ekman and Friesen 1982; Ekman, Hager, and Friesen 1981; Hager and Ekman 1985).

I spent nearly seven years writing *Telling Lies*, which is addressed both to the scientific community and to those who try to catch liars professionally. I decided to write this book because of my worries about the interest of the FBI and CIA in using my methods and findings in criminal interrogations, counterespionage, and diplomatic negotiations. My book warns the reader about the difficulties and pitfalls as well as the opportunities. It provides a theory to explain why people make mistakes when they lie, and why and when such mistakes should be more evident in nonverbal than in verbal behaviors. Emotions are central to my account of why lies fail, for it is the failure to inhibit completely signs of fear, guilt, and what I call 'duping delight' which most often betrays the liar or leads to confession. *Telling Lies*, however, reaches further afield than emotions in developing a general theory of deceit.

Since it was published I have been asked to give workshops on how to detect lying from behavioral clues to judges, lawyers, and members of the U.S. Secret Service. In most of these workshops I have also gathered data on how well these professionals can detect lies. I am working with my friend and colleague Maureen O'Sullivan on what has grown into a very interesting study attempting to account for why some people can detect deceit much more accurately than others.

I have just this year begun with Maureen a pilot study interviewing children of different ages to learn how they think about issues involved in lying and truthfulness. This work developed from some of the ideas about lying contained in *Telling Lies*, and also in part was stimulated by the questions asked of me by judges who have to evaluate the testimony of children.



*Expression and the Physiology of Emotion*

In 1982 I began collaborating with three investigators on different approaches to understanding how emotional expressions are related to central and autonomic nervous system activity. Many of the questions I have about the nature of emotion, and even those more limited to the nature of emotional expression, require that there also be information about what is happening inside the organism. Ideally I would like to consider both facial and vocal signals, the subjective experience of emotion, and central and autonomic nervous system activity on the same subjects. I have not achieved this yet, but I have been able to look at face, subjective report, and either ANS or CNS measures. Two central questions are: 1) how interrelated are these different aspects of emotion—the signals, the physiology, and the subjective sense? and 2) are there such distinctive emotion-specific patterns in the voice and in the ANS as I and others have found in the face?

With Luigi Pizzamiglio (a psychiatrist who is a professor of neuropsychology at the University of Rome), I have been studying patients with left and right hemisphere lesions. The patients' ability to make voluntarily each of the muscle movements involved in emotional expression is tested, and their spontaneous emotional expressions when watching emotion-arousing films are measured; their ability to recognize facial expressions of emotion is tested as well. This work has helped me to start thinking about the role of the cerebral hemispheres in the control and production of emotional expression. My role has been that of a Co-rather than a Principal investigator, as the material is gathered and scored (by people I have trained) in Rome. We will finish the data analyses this year.

I have worked more closely with Richard Davidson (University of Wisconsin), in the past four years, on a study of EEG and facial expression. Despite the geographical separation, we have managed to spend time at each other's labs piloting and then gathering data (in his lab). Although I do not agree with him about the role of the left and right hemisphere in the direction of emotion, working with Richie has been very rewarding. I have learned from him current thinking about hemispheric specialization, and I have been able to test my predictions about which facial expressions signal specific emotions and the differences between voluntary and involuntary

expressions using Davidson's EEG measures of differential hemispheric activity as the criterion. Together we have achieved findings on the interconnection between expression and EEG activity that would not have been possible for either of us to obtain separately. We have evidence of differential patterns of cerebral hemisphere activity associated with different emotions, associated with emotional and non-emotional smiling, and we are exploring how well expression and EEG measures account for subjective report. We expect to write articles this year on our findings. I have planned further work with Richie (which I hope to pursue if I can obtain support and find time) which would involve comparing voluntary and involuntary expressions.

As I described earlier, in 1970 I did a study on the relationship between expression and ANS activity (with E. Malmstrom) which showed different patterns of heart rate acceleration and deceleration accompanying disgust and surprise expressions. I sought to continue that work in 1972, but was not funded, and instead devoted myself to the problem of facial measurement. A few years later, as we were filming our own voluntary facial movements to discover how muscles, singly and in combination, change appearance, Friesen and I found that some expressions—the muscular actions which we had established as universal—produced strong bodily sensations. It was not until six years later, when Robert Levenson spent his sabbatical year at my lab, that it was possible to pursue this research.

Bob Levenson has spent three years at my lab, first on sabbatical and then on leave from Indiana University. In 1982, in a small study of sixteen subjects, we found that voluntarily making facial muscular movements does indeed generate ANS activity, and that the pattern of ANS activity was distinctive for four emotions. Published in *Science* (Ekman, Levenson, and Friesen 1983), our findings generated a lot of interest in the scientific community. In 1984 we obtained a grant to replicate and extend our findings, exploring the mechanisms which make it possible for voluntary facial action to generate involuntary emotion-specific ANS activity and a number of issues involved in understanding emotion-specific ANS activity itself. In 1986 we obtained another grant, to repeat our experiments in Western Sumatra, in order to determine whether the phenomena we discovered are limited to Western cultures. Again I had the chance to work

in a culture very different from my own. When I wasn't feeling lonely, missing my family, I was excited and grateful to have such a chance again.

Our findings have been extremely exciting. Each experiment has raised fascinating new questions and theoretical issues. It is certain that the phenomena are robust: voluntary facial action *does* generate autonomic nervous system activity, and there are different patterns of physiological activity for at least four emotions. But this is just a glimpse of one of the issues we will be addressing in the next few years. Collaboration with Bob Levenson on this work will be my main focus for the coming three or four years. While Levenson has completely separate areas of investigation which involve no collaboration with me, the work we do together occupies a central place on each of our research agendas. Working together is very enjoyable and rewarding. Our knowledge, technical skills, and conceptual stances nicely complement each other; our differences stimulate the design of experiments and make explicit alternative hypotheses.

#### *Theory of Emotion*

In addition to the collaborative work with Levenson, two other spurs to developing theory about emotion have come from two other empirical studies—one on Type A behavior and the other on startle reactions. In 1982 I obtained a grant from the National Heart, Blood, and Lung Institute to study emotional expression collaboratively with M. Chesney and R. Rosenman. More important than our findings (Chesney, Ekman, Friesen, and Black forthcoming) was being confronted by the need to distinguish among affective phenomena. Most Type A researchers and others in behavioral medicine use such terms as 'angry' and 'hostile' interchangeably, failing to consider that anger describes an emotion, which from an evolutionary perspective cannot be unhealthy, but must be adaptive. Hostility, on the other hand, could refer to an enduring character trait or attitude. I have recently attempted to describe the boundaries of emotion, proposing how emotions differ from moods, emotional traits, and emotional disorders (Ekman 1985). Another recent study, of startle reactions (Ekman, Friesen, and Simons 1985) helped me to distinguish another boundary of emotions, that between emotions and reflexes.

*Future Direction*

In the next five years I will pursue research as actively as I have in the past, but will spread my efforts over fewer projects. One set of studies will continue my collaboration with Levenson on expression and ANS activity. Another will continue my studies of the differences between voluntary and involuntary facial expressions of emotion, involving collaboration with Davidson. And (to a much more limited extent) I will continue some cross-cultural studies of facial expression.



Plate 1 Paul Ekman.

I plan also to begin a monograph on emotion, synthesizing the new findings about emotion coming from an evolutionary perspective, from studies of the brain, and from cognitive psychology, anthropology, and sociology. While I expect to produce at least one theoretical article during this period, the monograph will probably take more than five years. I write slowly, the issues I want to address are very complex, and the timing will

partly depend upon the pace of discoveries, my own and those made by the many other investigators now working on emotion.

I am a student of emotion, on an exploratory trajectory which has no end in sight.

### Notes

1. Goldstein (1983) has given an interesting analysis of why so many noted scientists worked so briefly and unsuccessfully on facial expression.
2. Camelot was the name given to what seemed to be a social science survey research project in South America funded by the Department of Defense. There was a scandal which received a great deal of media attention when it became public that the research was largely a cover for developing information on counter-insurgency.
3. Many years later, Carleton Gajdusek received the Nobel prize for his research on Kuru. Kuru provided the first model for slow viruses, which incubate for many years. Transmission, it turned out, was through eating the brain—without cooking it—of someone who had died of Kuru. Gajdusek kindly invited me to utilize his field resources in the Kuru area of the New Guinea Highlands. He had been working there for more than a decade. We started out working in the same villages, but his style and mine were too different, and I instead worked with E. Richard Sorenson, then a cinematographer, who later became an anthropologist. In the second trip to New Guinea I was aided by Neville Hoffman, an Australian physician who had spent two years working in the highlands.

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