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TOWARD INTEGRATION IN THE SOCIAL PSYCHOLOGY OF EMOTIONS

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Abstract

Contemporary research on emotions has been increasingly demarcated into four self-contained segments: the cultural, biological, inner, and outer aspects of emotional processes. A parallel trend is the increasing hegemony of the perspective of those who study only the cultural and outside segments. Research on cultural variation, the causation of emotional states, and cultural universals is reviewed and evaluated. This review suggests that the evidence does not support the exclusion of cultural universals from research designs. It is proposed that integration of these four approaches is urgently needed if we are to understand the interactions between culture and biology, between inner and outer, and therefore what is distinctively human about human beings. Some research that integrates these elements into single designs is described as pointing the way for future exploration.

INTRODUCTION

For this chapter, I have chosen an issue somewhat broader than a review of recent work in the sociology of emotions. A recent review is already available in Steven L. Gordon's (1981) "The Sociology of Sentiments and Emotion." In that article, Gordon examines the social processes relevant to emotions under three headings: *differentiation*, "the establishment and maintenance of qualitative distinctions among sentiments;" *socialization*, "by which a cultural vocabulary of sentiments becomes an interpretive resource of individuals"; and

management, “the normative regulation of expression and feelings by individuals and groups.” Gordon gives brief, appreciative expositions of some of the most prominent work in this field, including descriptions of the work of Averill (1974, 1975, 1976, 1980), Hochschild (1975a, 1975b, 1979), Kemper (1978), and Schachter & Singer (1962). Shibusani’s (1961) analysis of the social control of emotion, although published over 20 years ago, is still the definitive statement of the sociology of emotions, because of its breadth, complexity, and systematic nature. (See Chapter 10, “Sentiments and Interpersonal Roles”; Chapter 11, “Conventional Norms and Sentiments”; and pp. 548–56, “The Development of Sentiments.”) In addition to these sections, sentiments play a prominent part in Shibusani’s entire social psychology, as in his treatment of reference groups as perspectives, since he postulates that sentiments play a key role in the transformation of perspectives. I refer the reader to these two studies, since they complement this one. Here I review some of the conflicts among disciplines and paradigms involved in the study of emotions and suggest ways to reduce these conflicts.

Contemporary approaches to the study of emotions are characterized by great diversity and conflict. The current reawakening of interest in emotions in social science and psychology shows no sign of decreasing these differences. Although there are other dimensions of conflict, I limit my attention here to two of the most prominent: (a) between those who see emotion as largely culturally specific, and those who see it as largely universal; and (b) between the objective approach, in terms of outward appearance, and the subjective approach, in terms of inner experience. Underlying these two issues is a third, which is only implied by the other two, concerning how important emotions are in human affairs.

One of the architects of the culture-specific position was Durkheim (1915):

Mourning is not a natural movement of private feelings wounded by a cruel loss; it is a duty imposed by the group. One weeps, not simply because he is sad, but because he is forced to weep. It is a ritual attitude he is forced to adopt out of respect for custom, but which is, in a large measure, independent of his affective state (p. 397).

Notice how emphatic this statement is, and how little qualified: “Mourning is *not* a natural movement of private feelings. . . it is duty imposed by the group.” Even today, there are still insufficient grounds for a tone of such utter finality. I return to the question of evidence below.

The idea that emotions are universals that occur in all cultures and historical eras also has a long history. Its most illustrious advocate was Charles Darwin (1872). He believed that basic emotions such as fear and anger are biologically determined universals for the entire human species. He also thought that similar emotions were shared with other mammals, such as the primates.

C. H. Cooley (1909), the social psychologist, also believed in universal

emotions, although he did not appear to subscribe to biological determinism. He argued that the universal situation of the primary group (the intimate group of parents, children, friends, and neighbors) generates similar emotions in all cultures. These intimate groups give rise to: "sympathy and the innumerable sentiments into which sympathy enters, such as love, resentment, ambition, vanity, hero-worship, and the feeling of social right and wrong Always and everywhere men seek honor and dread ridicule, defer to public opinion, cherish their goods and their children, and admire courage, generosity, and success" (p. 28). Like Durkheim's statement, Cooley's strikes the note of complete and exclusive certainty—"always and everywhere." When framed in such strong language, the two positions seem mutually exclusive—an idea that I contest later in this paper.

Paralleling the culture specific-universal controversy is a closely related one concerning cognitive and biological emphases in the conceptualization of emotion. Most universalists believe that the seat of emotions is in the center of the body, in the viscera (in vernacular terms, in the "guts"). Like Darwin, they see emotions as biological, genetically determined reactions that are universal in the human species. Most of those who take the culture-specific approach, on the other hand, take the opposite position, that emotions occur in the mind and the reactions in the mind to the immediate environment, particularly the social environment.

The current cultural-specific perspective does not eliminate biology completely. Physiological reactions play a part in this scheme, also. But these reactions are introduced in such a way as to considerably decrease their importance, in comparison with the crucial role they play in the universal approach. According to the cultural-specific perspective, there is only one type of physiological arousal, and this one type plays a role in all emotions. This might be called the postulate of the unitary nature of all arousal. According to this point of view, the differences among the various emotions experienced—grief, fear, anger, shame, and so on—is not physiological but is caused by differences of *interpretation* of the same bodily arousal. In a context interpreted by self and/or others as one of danger, and/or when one is expected by self and/or others to feel fear, then autonomic arousal occurs, and this arousal is usually interpreted as the feeling of fear. In a context of loss, exactly the same type of autonomic arousal occurs, but in this context, because of the difference in interpretation, it will be experienced as grief.

Another conflict is related, but overlaps only partially. This is the argument between those who view emotions as largely objective, outer phenomena and those who view them as mostly subjective, inner ones. The positivist approach conceptualizes emotions as phenomena objectively observable from outside of the person experiencing the emotions. The subjective approach views emotions in terms of inner feeling: An emotion is the subjective experience, not reducible

to objective events. Most current empirical research uses the objective approach. The subjective view is found largely among clinicians, and, in philosophy, with the phenomenologists. This dimension has strong implications for the methods used in investigating emotions: Most researchers holding the objective view observe only outer signs of emotions, and most who hold the subjective view elicit only subjective reports. For this reason, and for conceptual ones, it is difficult to relate the studies done by one group to those of the other.

The outer-inner dimension is related to the culture specific-universal conflict, particularly at the culture-specific end of the continuum. At least in psychological investigations, there is a strong tendency for those who see emotion as culture-specific to also use outer, positivist measurements. At the other extreme, the relationship is less predictable. Universalists may tend to think in terms of emotions as felt inner experience, but many exceptions come to mind. Nevertheless, for discussion purposes only, it will be convenient to reduce the dimensions of conflict to two poles: the culture-specific one, which uses positive methods, and the universal one, which may favor experiential methods.

Implicit, but not usually discussed in this argument is still another dimension of conflict, between a position that assumes that emotions are relatively unimportant in human affairs and one that assumes that they are of overwhelming importance. The culture-specific proponents usually seem to assume that emotions are fleeting, intermittent, and mild. For this reason they see them as unimportant in the causation of human behavior, or, in a less extreme formulation, as no more important than other components of behavior, such as perception, cognition, and social processes such as control. The universalists, on the other hand, usually seem to assume that emotions are extremely intense, continuing, and ubiquitous in human behavior. For this reason, emotions are seen as of predominant importance in the formation of behavior. Especially in clinical thought, other components of behavior are often seen as subordinate to emotions—thoughts, verbal statements, and rules being seen as rationalizations or defenses covering or resisting emotional ends.

An empirical aspect of this conflict has recently surfaced in the work of the psychologist Robert Zajonc (1980). He notes that cognitive psychologists usually assume that cognitions hold primacy over emotions in the sense that they occur first: Emotions are seen as reactions to thought. Reviewing a number of studies, and adding his own study, however, he reports that the evidence actually shows the reverse order: the emotion comes first, followed by a thought. For this reason, he argues, emotions should be considered at least as important in behavior as cognition, and perhaps even as primary, in the sense that they occur first in the formation of behavior.

THE NEED FOR INTEGRATION

When these conflicting ideas first arose, there was a tactical division of labor. Since the complexity of emotions and their effect on behavior seemed overwhelming at the time, researchers elected to study one dimension at a time. Some looked to universals, others for culture-specific emotions; others for positive outward signs, and others for feelings as experienced from within. This division was probably useful at the time but has now hardened into separate institutions. What was temporary and tactical has become permanent and ideological. The behaviorists study the outside, the phenomenologists the inside, each largely ignoring the work of the other. Until recently, the issue of culture-specific vs universal aspects of emotion divided the social scientists from the psychologists, the social scientists looking at the culture-specific aspects, the psychologists at the universal ones. Unless some changes occur, the quest for the truth about emotions may be more and more subordinated to jurisdictional disputes between the various disciplines and professions.

The division of scientific labor into self-contained schools of thought encourages the study of emotions at the zero-order level of analysis, to borrow a concept from multivariate analysis. If, for the moment, we consider emotions to have four basic components, the culture-specific, the biological, the outer and the inner, then the segmentation of outlook discourages the study of interactions among these four components. The link between outer and inner is a centuries-old puzzle—the dualism of mind and body, flesh and spirit, appearance and reality. What is distinctively and elusively human about human behavior may reside largely in these interactions, between culture and biology, between inner and outer, and so on, permuting each of the four component relationships. The present separation of inquiry is probably a grave error.

In addition to the lack of integration of the ongoing studies of emotion, a new threat to the integrity and balance of the field has arisen recently. The popularity of the culture-specific/behaviorist position is growing so rapidly that the other positions may be virtually discarded. Many leading researchers in psychology—traditionally a stronghold of the universalist point of view—are now adopting the culture-specific position. A recent example is provided by psychologist James Averill (1980), who advocates the idea that emotions are social roles: “An emotion is a transitory social role (a socially constituted syndrome) that includes an individual’s appraisal of the situation and that is interpreted as a passion rather than as an action” (p. 312). This sociological definition of emotion exactly illustrates the trend I have been describing. It suggests that emotions are culture-specific, (since they are roles), that they are fleeting (transitory), and involve cognition (interpretation, appraisal) rather than biology. Such a definition virtually eliminates physiology from considera-

tion, as well as the interactions between culture and physiology, inner and outer, and so on.

How can these interactions be studied? Before attempting to answer this question, I first assess the evidence in the argument between the culture-specific advocates and the universalists. I then return to the question of what kinds of investigations might lead to integration rather than fragmentation of the study of emotions.

CULTURAL SPECIFICS AND UNIVERSALS: THE STATE OF THE EVIDENCE

The culture-specific argument rests on two principal grounds: observations of the great variability in emotional behavior from culture to culture, and experimental studies in social psychology. Since the argument from cross-cultural variability is the earlier one, I discuss it first.

For many years, social scientists have been reporting immense differences in emotional reactions among cultures. According to these reports, displays of basic emotions such as grief, fear, anger, and shame seem to be frequent and intense in some cultures and almost totally absent in others. For example, the duration and intensity of mourning after loss vary so much, according to these reports, that the culture-specific advocates have argued that any universal biological process of grief should be ruled out. Similar arguments have been or could be made about the other basic emotions.

One can make two objections to the argument about cultural variation. The first is methodological. The evidence on cultural variation in emotions is unsystematic and anecdotal, for the most part; it may overstate the amount of variation. The one systematic study in this area (Rosenblatt et al 1976) indicates considerable uniformity in mourning practices in cultures in the study. For example, weeping at funerals is reported in 78 of 79 of the societies studied.

Some doubt even exists about the single exception Rosenblatt found in the Human Relations Area Files, the Balinese society. When I reported this exception to two anthropologists familiar with the Balinese culture, they both replied that the HRAF were misleading on this point. Both stated that the Balinese have the ideal value that one should not cry during mourning, but in practice they weep as much as members of other cultures.

A pattern of considerable uniformity emerges from Rosenblatt's study. It appears that most cultures in the history of the human species have practiced carefully designed, elaborate, extensive procedures for mourning the dead. Furthermore, these rites appear to have obtained virtually complete involvement from all members of any given society. Future systematic studies of other emotions cross-culturally may show that the amount of variation has been greatly exaggerated.

However, mourning practices do vary somewhat. The single society in the Rosenblatt study in which weeping appears to be absent at funerals is the Balinese society, as already indicated. Benedict (1934) noted the curtailment of mourning in the Zuni society. And of course, Gorer (1965) and many others have commented on the inadequacy of the mourning rites in modern industrial societies.

However, it is a non sequiter to use these cultural variations as evidence that mourning is not necessary; to do so is to subscribe to the belief that whatever is, is right. A body of evidence now suggests that some cases of bereavement give rise to psychological and physiological impairment, independently of cultural context. The clearest case for this position has been made by Averill (1968), on the basis of his review of numerous studies of grief and depression. More recent studies of physiologic impairment have been published by Bartrop et al (1977) and Hofer et al (1979). The Bartrop study demonstrates that bereavement disrupts the immune function; the Hofer study found, at least for persons undergoing severe grief reactions, that bereavement was associated with an increase in corticosteroid excretion, which presumably indicates impairment.

A second line of research that should be considered in this connection concerns the recently discovered phenomenon of pathological or prolonged grief. Several investigators, among them Volkan (1979), Parks (1972), Paul (1965), and Ramsey (1977), have independently defined a new psychiatric syndrome, which involves the inability to mourn. Lazare (1979) has presented a concise description of their findings and described the psychological and social sources of the syndrome. All of these clinicians agree that the inability to mourn, whether its source be individual psychopathology or cultural inhibition, damages the individual.

These studies of the effects of bereavement are relevant to the issue of cultural variation in mourning practices. The absence or curtailment of mourning in a society would show that mourning was unnecessary only if it were also shown that no deleterious effects followed. None of the studies of cultural variation even addresses this problem, much less conducts research on it. An analogous argument would be that because some societies do not define fruit and leafy vegetables as edible they are not required in the diet. Unless one could show that scurvy, rickets, and other diseases of poor nutrition were absent, the argument about cultural variation in diet would not be creditable. Because of this lapse, the cultural-specific position on the arbitrariness of emotional expression should probably not be given much weight.

EXPERIMENTAL EVIDENCE

The second ground for the current surge of support of the culture-specific viewpoint comes from laboratory studies in experimental social psychology. I

do not attempt to review all these studies here; rather I critique a single representative study. This study, by Schachter & Singer (1962), is the best known of this genre. It is widely cited by advocates of the culture-specific position as providing strong empirical support, if not proof, of their point of view.

The Schachter & Singer study is important, but I think its relevance to the culture specific-universal controversy is extremely limited because of two aspects of the design of the study. [For a comprehensive look at other conceptual and methodological problems with this study, see Kemper (1978) and Scheff (1979).] First, like many other laboratory studies of emotion, it does not employ genuine emotions in its design. It substitutes autonomic arousal caused by ingestion of a drug. Second, like almost all other laboratory studies, the level of arousal is at best moderate, rather than intense. The relatively low level of arousal makes it difficult to apply the study's findings to the culture-specific hypothesis, but this is not as serious a problem as the use of drugs. Before discussing the implications of the use of drugs in Schachter & Singer's design, I briefly describe the study and its results.

The study examines the interaction between physiological changes, information, and social environment. The experimenters injected some subjects with epinephrine and others with an inert substance. Some subjects were informed what kinds of reactions they should expect from the drug, others were uninformed, and others were misinformed. The final variable was the social environment. Subjects were placed with another person, who they thought had received the same treatment they (the subject) had. Actually the other person was a confederate of the researcher's, who displayed happy behavior in half of the cases and angry behavior in the other half. If the universal position were correct, the information and social environment conditions should have little effect on the emotions felt by the subjects; only the injection of epinephrine or placebo should matter. If the culture-specific approach were correct, the subjective feelings of the subjects should be strongly influenced by the information and social environment.

Schachter & Singer's findings actually provided support for both sides of the controversy. The subjects' evaluations of their emotional states were influenced by all three variables. However, since it was found that under certain informational conditions (misinformed subjects) there was a correlation between social environment and subjective emotion (subjects reported feeling more happy with a happy confederate, more angry with an angry confederate), the study is usually interpreted as decisively supporting the culture-specific argument. Does this study constitute a test of the culture-specific hypothesis, or is that hypothesis built into the design as an untested assumption? In my opinion, the latter is the case.

As indicated at the beginning of this article, the unitary nature of autonomic

arousal is a key assumption in the culture-specific hypothesis: Differences of feeling are generated not by physiological differences in the state of arousal but in differences in interpretation of states of arousal that are physiologically identical. If you assume the culture-specific hypothesis, there is a further implication concerning the arousal caused by drugs: Since there is only one kind of autonomic arousal, there is no difference in arousal caused by drugs and arousal caused by emotions. Making this assumption, Schachter & Singer substituted a stimulant drug in place of emotional arousal. They didn't arrange to have the real emotions of joy and anger as their source of arousal. Instead they produced arousal by the use of epinephrine. The subjects found themselves in a state of arousal (rapid heartbeat, rapid breathing, etc) of unknown origin. When the social situation suggested that they were experiencing anger, a small but statistically significant number of subjects indeed interpreted the arousal as anger. A similar result was found to hold with the suggestion of joy.

In order to generalize the results to the study of emotions, Schachter & Singer had to assume a key postulate of the culture-specific position, the unitary nature of arousal. The study is not a test of the culture-specific hypothesis at all, since it assumes it in its very structure. If my argument is correct, the use of this study to support the culture-specific position involves circular logic.

A second difficulty with the Schachter & Singer study concerns the issue of the intensity of arousal. For ethical and other reasons, this study, like almost all other laboratory studies, uses fairly low levels of arousal. The experimenters, for obvious reasons, were reluctant to cause high levels of arousal in their subjects. Even if one accepts the validity of their findings, can one generalize the results of findings about low intensities of arousal to high intensities? This question would arise even in a study using genuine emotions as the source of arousal. Is a subject as likely to confuse intense anger and joy as weak? I think not. Under the conditions of the experiment, where some of the subjects were subjected to confusing suggestions and misinformation, and were experiencing mild autonomic arousal of an unknown origin, they might well mislabel some of their feelings. As the intensity of the emotions increases—e. g. to the level of nightmare fear or rage—the likelihood of such mislabeling probably decreases. The experiment seems to have been designed to provide the weakest rather than the most severe test of the culture-specific premise.

The issue of the intensity of arousal in emotion states may have an important bearing on the entire culture specific—universal dispute, going far beyond the question of the validity and relevance of the Schachter & Singer study. There may be no real conflict between the two positions, since each may refer to different phenomena. The examples used in the culture-specific arguments seem to indicate that low levels of arousal are being assumed, as in the Schachter & Singer study. Often the references are to sentiments—cognitive

events with an affective tone—rather than to intense emotions. Gordon's (1981) review of the sociology of emotions, already cited, makes this tendency explicit. He suggests that the concept of sentiments be made the focus of social scientific concern in this area, echoing an earlier suggestion by Shibutani (1961) to the same effect. The concept of sentiment certainly connotes a low level of arousal.

In this context, it might prove useful to revive a distinction made by William James (1891) between "coarse" and "subtle" emotions: "I shall limit myself in the first instance to what may be called the 'coarser' emotions, grief, fear, rage, love, in which everyone recognizes a strong organic reverberation, and afterwards speak of the 'subtler' emotions, or of those whose organic reverberation is less obvious and strong" (p. 449). Following James's distinction might help reconcile the culture specific-universal conflict if the culture-specific advocates are describing the subtler emotions, or sentiments, and the universalists the coarser ones. I return to the issue of reconciliation and integration after a brief discussion of the evidence for the universal position.

EVIDENCE FOR CULTURAL UNIVERSALS IN EMOTION

So far I have examined some of the principal evidence for the culture-specific hypothesis and found it weak and flawed. Does the evidence for the hypothesis of cultural universals fare any better? In attempting to answer this question, I rely heavily on a recent article by Ekman & Oster (1979) that focuses directly on this issue. They cite two kinds of evidence in direct support of the cultural universality of emotions: cross-cultural studies and developmental studies.

To quote Ekman & Oster, there is "unambiguous evidence for universality" for the expressions of five basic emotions: happiness, anger, disgust, sadness, and combined fear/surprise, based upon six studies (Ekman et al 1969; Izard 1971; Ekman 1972; Boucher 1973; Ekman 1973; Saha 1973). These studies show a high degree of consensus within and between various cultures on the names of emotions shown in photographs of facial expressions. One of these studies (Ekman, 1973) included an aboriginal group from New Guinea that appeared never to have had contact with any aspects of Western culture. That the judgments of the photographs by members of this society agreed with those of members of the literate cultures tested seems to rule out the possibility that concurrence arose through culture contact rather than from the true universality of the expressions.

The evidence of an unlearned, biological source of emotional expression from developmental studies of newborn and very young (e.g. 3-4 weeks) infants also provides tentative support for universality. The observation in newborns of crying, smiling, startle, and an expression resembling disgust in adults in response to unpleasant tastes (Steiner 1973) strongly suggests that at

least these expressions are innate. The presence of the disgust expression in even brain-damaged newborns (Steiner 1973) further supports the genetic origin of this expression.

How are we to compare the strength of this kind of evidence for universality with the culture-specific evidence cited earlier? As Ekman & Oster point out, the existing studies, particularly the developmental studies, leave many key questions unanswered. Crying, smiling, startle, and disgust are fundamental expressions, but many others have not yet been documented. Precise longitudinal studies of all the basic expressions from the day of birth will be needed to firmly support the genetic position. With respect to the cross-cultural recognition of the facial expression of the fundamental emotions, the evidence is much more impressive. Even with this material, however, the evidence is far from conclusive. One issue is the use of posed and static photographs. A stronger case for the validity of the findings might be made if spontaneous expressions in motion pictures were used.

Taken all in all, however, I judge the evidence for universality to be at least as strong as and probably stronger than the evidence for cultural specificity. For me the implication of this review is that it would be premature to discard the universal viewpoint at this time, or even to discourage equal time to research on universal aspects of emotion.

STEPS TOWARD INTEGRATING THE CONFLICTING VIEWPOINTS

In criticizing the culture-specific position, I am by no means suggesting that this position has never been useful. It is still a good strategy for many purposes. In my opinion, some of the current investigations based on the culture-specific assumption are invaluable. One such study that comes to mind is Hochschild's (1983) investigation of the emotion work done by airline stewardesses. The biology of the false smiles employed by the stewardesses is not very relevant to Hochschild's thesis, which involves the emotion work done by women, work usually unnoticed and undervalued. There is nothing wrong with a scientific division of labor, as long as it does not rule out interdisciplinary, interparadigmatic effort.

In the remaining section of this paper, I review some of the studies I consider integrative of the seemingly conflicting viewpoints so far discussed. The most outstanding work in this area is that of Paul Ekman, (a psychologist at the University of California, San Francisco) and his collaborators. Some of their work has been cited above in connection with the evidence for the universality of emotions. Ekman and his group have also produced studies that bear directly on the integration of the currently fragmented study of emotion.

The first contribution of the Ekman group is not a piece of research in the

conventional sense, although it is based on a program of exceedingly precise and diligent research. It is a system for coding facial actions that Ekman developed in collaboration with Wallace Friesen (1978). In my opinion, it is the first reliable and practical guide for the objective study of facial expressions. It is therefore of enormous importance for social science and psychology. The system is called FACS (pronounced "fax"), which stands for Facial Action Coding System. Because this system may be very valuable to researchers, I briefly describe its development and features.

When Ekman and Friesen began their study of facial expressions, they soon found that no functional anatomy of the muscles in the human face was available. Structural anatomy, which showed muscles that were physiologically distinct, did not show which were capable of independent action. Ekman and Friesen devised a functional anatomy by studying the movement in their own faces in mirrors, over a period of two years. They found 44 distinct functioning muscles, each capable of independent action. Since each is also capable of action in conjunction with one or more others, a staggering number of facial movements is possible. This finding agrees with our intuitive sense of the complexity and variety of facial expressions.

The identification of these 44 units established the foundation for the coding system. Each independent muscle unit is called an Action Unit and is assigned a number. For example, the zygomatic major muscle causes the ends of the mouth to curl up as part of a smile. In FACS, it is called AU 12 (Action Unit 12). For convenience, it is grouped with the other lower face AUs that cause oblique actions, such as the nasolabial furrow deepener, AU 11. Other lower face groups of AUs are those involving up/down actions and horizontal actions, such as AU 20, the lip stretcher, and orbital actions of the mouth—e.g. tightening of the lips, AU 23. The other major AUs involve the upper face, especially the forehead and the eyes. There is also a miscellaneous group, involving the jaw, tongue, and nostrils. Finally, a group of AUs that do not involve facial muscles but are nevertheless significant in facial expression: those involving head and eye positions. The total number of units is 66.

Each of these 66 actions is visible to a trained observer, singly or in combination. This method is objective, and it is precise enough to catch almost all the nonverbal facial actions that give human interaction its unique flavor. To illustrate the precision of the method, I refer to one of the many studies Ekman and his co-workers have already completed. This study concerns the simple smile. Using videotape, Ekman, Friesen & Ancoli (1980) studied the naturally occurring smiles of persons viewing brief comic films. They rated muscle movements, frame by frame. (There are 30 frames per second in video.) Even in a few seconds, many subtle facial movements occur. They found in addition to the simple smile 19 other types of smile, caused by various combinations of the zygomatic major, zygomatic minor, buccinator, risorius, and caninus

muscles. By asking the subjects what emotions they felt during the viewing of each film, the researchers were able to specify which feelings were connected with each type of smile. The only smile correlated with the experience of joy was that involving AU 12, the simplest one. None of the other smiles showed any association with joy. These other 18 smiles probably involve simulation or control of joy, or blending with, masking of, or commenting on some negative emotion.

Another paper, "Felt, false, and miserable smiles" (Ekman & Friesen 1982), extends the definition of an authentic smile beyond AU 12. In this paper they show that in addition to AU 12, the muscle groups around the eyes must contract—orbicularis oculi, AUs 6 or 7, which raise the cheeks, bag the skin below the lower eyelid, narrow the eye aperture, or produce crow's feet at the corners of the eyes. Without the action of these muscles, the smile produced by raising the lip corners alone looks and is inauthentic. Ekman & Friesen call this expression a "false smile."

One immediate implication of this study, which the culturalist would be quick to point out, is that most of the smiles we see are not spontaneous expressions of an inner feeling but rather are communicative in nature. Simulation, control, masking, and commenting would usually serve some social requirement. If other facial expressions are similar to smiles in this respect, and there is no reason to believe they are not, then the culturists have an important point: Most of the expressions we see in social interaction are contrived for communication in a particular culture. Ekman's work seems to have prepared the ground for a precise and systematic study of a basic source of nonverbal communication, movements in the face. The Ekman group reports having already observed some 4000 different facial expressions, most of which seems social in nature. Many of the expressions appear to serve linguistic ends: emphasis, punctuation, and so on. In order to understand nonverbal communication, a grammar and dictionary of these thousands of expressions may be necessary.

The Ekman et al findings indicate a complexity that goes beyond the culturalist purview, however. All of the smiles they observed were communicative except one. The smile signaled by AU 12 and 6 or 7 appears to be a spontaneous sign of an inner feeling. Again, assuming that other expressions are similar to smiles in this respect, this finding suggests that two different systems are involved in facial expression: one intentional, the other spontaneous. Apparently these systems may act independently, but they also may interact. For example, a speaker may intentionally express surprise at an unintentional smile, thus commenting on his own spontaneous gesture in an intentional, nonverbal way. Alternatively, the expression of surprise, like the spontaneous smile to which it is a reaction, may also be unintentional. The participant in a social transaction is faced with the complex task of sending and receiving messages in two

languages, one intentional, the other unintentional, and of trying to be aware not only of the two languages themselves, but of the interactions between them.

The Ekman et al study puts new emphasis on the frequently repeated strictures concerning the importance of nonverbal elements in communication. It has often been noted how basic nonverbal gestures are to social interaction. The fundamental human feelings that Cooley referred to—sympathy, trust, attraction, and antipathy—are probably largely reactions to nonverbal expressions, especially spontaneous and unintended gestures. Can ordinary people in social interaction detect the difference between the AU 12-6, 7 smile and all the others? I would guess that under certain conditions they can. At least they usually try to make this discrimination and often think they have. Is my conversational partner genuinely pleased, or is he merely trying to make me think so? Or is he trying to suppress or disown a feeling of pleasure? Another primitive concern in social interaction is the issue of threat. Within the first few seconds of every social interaction most people probably make a preliminary judgment about the threat level of the other person or persons involved. Is this person a threat to me in any way, whether physical or emotional? Such judgments probably determine the nature of the subsequent interaction, and they depend largely on the spontaneous nonverbal gestures observed. In a fairly direct way, the most basic social bonds and antipathies may proceed from spontaneous nonverbal expressions (such as those investigated by the Ekman group) and the other's perceptions of these expressions. Future studies of the bases of social conflict and cohesion may require knowledge of the language of the face if they are to succeed.

Another paper in the group of studies of the difference between spontaneous and controlled expressions by Ekman, Hagar & Friesen (1981) compares symmetry between the left and right side of the face. This study required precise measurement techniques, since the left-right differences are usually subtle. The authors reported that felt expressions are more symmetrical than posed ones. Apparently there is a factual basis for distrusting the crooked smile. The precision of the FACS technique allowed this and the other two studies cited above to be completed.

This group of studies offers an outstanding example of the way to reconcile one of the two primary conflicts discussed in this paper. These studies show the relationship between an inner fact (i.e. joy) and an outer one (a smile involving AUs 12 and 6, or 7). Studies of this type offer the hope of restoring the vital link between outer appearance and inner reality, the link that is lost in the division of research labor between behaviorists and phenomenologists. These studies deserve an award from any group interested in the unity of human experience.

Another study, this one by Ekman (1972), provides a model for reconciling the other primary conflict in the study of emotion, between the culture-specific

and universal position. In this study 25 Japanese adults (in Japan) were shown a stressful film (an extremely bloody eye surgery), and 25 Americans were shown the same film in the United States. A hidden TV camera recorded the facial expressions of each subject. In the analysis, the expressions of the Japanese and the Americans were compared, frame by frame. The first stage of the study was conducted when each subject was in the room alone, watching the film. In the second stage, an interviewer joined the subject, ostensibly to ask him questions about his responses but actually as an experimental intervention. In this part of the study, the hypothesis was tested that the Japanese subjects, because of the display rules in the Japanese culture, would mask negative emotions with smiles more than the Americans. The authors report that the subjects, when alone, displayed extraordinarily similar facial responses to the film. The correlations between the responses of the Japanese and the Americans was .72, when a particular area of the face was compared (such as the eyes and lids) and .96 when the movements of the whole face were compared. I assume these figures mean that although there were differences between the reactions of the two groups, the between-group differences were no larger than the within-group differences. When alone, Japanese and Americans exhibit a similarity of emotional reaction to distressing scenes that supports the universality of emotions.

In the presence of another person, however, the Japanese showed more positive emotions as they watched the film than the Americans did. This study subsumes and expands the culture specific–universal controversy. When they are alone, the Japanese and Americans are brothers under the skin; their emotional responses do not differentiate them. In the presence of another person, however, their culture differentiates them: The Japanese hide their distress behind a polite smile more than the Americans.

This study suggests that the culture-specific and the universal positions are not in competition; instead, there is a lawful relationship between them. Whether one or the other is the best predictor of behavior is dependent on a situational variable—in this case, the presence or absence of another person. The design of this study neither assumed nor excluded one viewpoint, and so allowed the reality of the human condition to show in the results.

It is instructive to compare the design of this study with that of the Schachter & Singer study. The latter suffers in comparison. The Ekman study is precise and fine-grained, directly examining hundreds of frames of spontaneous and intense facial behavior. By contrast, the Schachter & Singer study deals with moderate levels of arousal indirectly through paper-and-pencil questionnaires. Most important, the Ekman study does not preclude or assume one viewpoint; both culture-specific and universal questions are explored in the design. For these reasons it seems to me that the Ekman study, and not the Schachter &

Singer one, provides a model for future research. Researchers in the field of emotions will be indebted for many years to Ekman & his collaborators for the comprehensive sweep and the precision of their work.

Even though the program of research initiated by Ekman is extraordinarily broad in scope, covering as it does many of the basic unanswered questions about the nature of emotions and their relationship to other spheres of behavior, there are still many questions he has not yet raised. His work to this point has been pragmatic and inductive, rather than theoretical, and has described, for the most part, static emotional states rather than dynamic ones. The very comprehensiveness of his attack on the basic nature of emotional expression has precluded the intensive exploration of theoretical questions, or the delineation of fundamental sequences of emotional states. At this time the descriptions of actual emotions are called "predictions" (in the FACS manual, Part 2, pp. 142-43) and are static.

In my own research, I am seeking to test theoretical hypotheses involving dynamic sequences of grief or anger. Building on Ekman's ground-breaking efforts, I am trying to discover the interrelationships among cultural, psychological, and biological components of emotion. My collaborators and I believe emotional catharsis is the link between activities as disparate as rituals of mourning and healing, classical drama, and certain kinds of children's games. In earlier studies, we tried to test the theory of catharsis in collective settings: the effects of laughter on audiences of film comedy (Scheff & Scheele 1979; T. J. Scheff, D.D. Bushnell, in preparation). We found that laughter in this setting leads to relaxation, as measured both by the participants' subjective report (an Adjective Check List) and by a physiological measure (heart rate). However, the effects were small, being just on the border of statistical significance.

From my observations of the participants' reactions and from the debriefing sessions, I formed the opinion that the effects were small because our research interfered with the audience's emotional responses. Laughter is a delicate response; self-consciousness deflects it. This effect was especially prominent in substudies that employed measurements of EKG and finger temperature. It seemed to me that the sensors from this equipment interfered with the participants' enjoyment of the film by restricting their movement and making them self-conscious.

I interpret the results of a classic study of audience response to be an even more extreme illustration of the intrusion effect. Averill (1969) carefully monitored the physiological responses of audiences to tragic and comic films, using 14 different indicators. In order to measure stomach motility, Averill had even asked some subjects to swallow an instrumented balloon. He found that very few physiological changes were statistically significant; but given the elaborate wiring of the members of the audience, is this finding accurate? Internal evidence in the report suggests that the measurement procedures

interfered with the effect they were designed to measure. Averill reports that only 11 of 18 subjects who saw the comic film laughed, and he does not report on sobbing and tearing for the tragic film. Perhaps there was little physiological reaction because the research design intruded on the effect it intended to measure. Averill was candid enough to include some of the comments made by the subjects during debriefing. Although there were different reactions, one of the responses published should alert us to the intrusion effect: "I was tired and uncomfortable during the movie so I became bored and indifferent about the action on the screen" (p. 405). It seems to me that his study, like our earlier ones, was not designed to allow strong emotional responses in the audience.

My next step was to seek a research setting that would generate strong emotional responses and a measurement technique that would not be intrusive. I settled upon a classroom for the setting and a TV camera and recorder for the measurement technique. In this first stage of the research, I have focused upon individual rather than collective reactions. With refinement of the theory and method, I plan to return to collective studies.

In each class I ask for one volunteer to join me in front of the camera to narrate an emotional episode from his/her life. After the telling, I usually coach the subject to change the "distance" from his/her emotion. In another place, I have defined this distance as the balance between observation of self, the objective view of oneself as from outside, and participation, especially attending to one's inner feelings (Scheff 1979). Thus I try to have each subject feel and express more emotion if her story seems flat and detached or be more objective and detached if he seems overinvolved in emotions from the past. With this technique, intense cathartic reactions sometimes occur. The tape recording is then replayed, for comment by the volunteer, myself, and the rest of the class, a small group of 15–20 students. The tape becomes a research protocol and is viewed in real time, in slow motion, and in freeze-frame, frame by frame, in subsequent studies.

The primary hypothesis is that cathartic processes, as signaled by laughter or by a certain kind of crying, lead to a resolution of emotional distress, and that this resolution should result in changes in the facial actions of the subject. The theory is that there is a grief response cycle and an anger response cycle, analogous to the sexual response cycle, and that these types of laughter and crying signal climax of an inner process of emotional tension, somewhat as an orgasm signals climax of sexual tension. In a preliminary analysis of interviews with a subject who'd had a "good cry," we found substantial reductions in the frequency and intensity of facial and other actions indicative of tension (e.g. licking or pursing lips; swallows; deep breaths; shoulder movement and hand gestures) and in verbal content indicative of sadness or unhappiness (Scheff, Gleberman & Parker 1982). These changes were significant at the .05 level of probability.

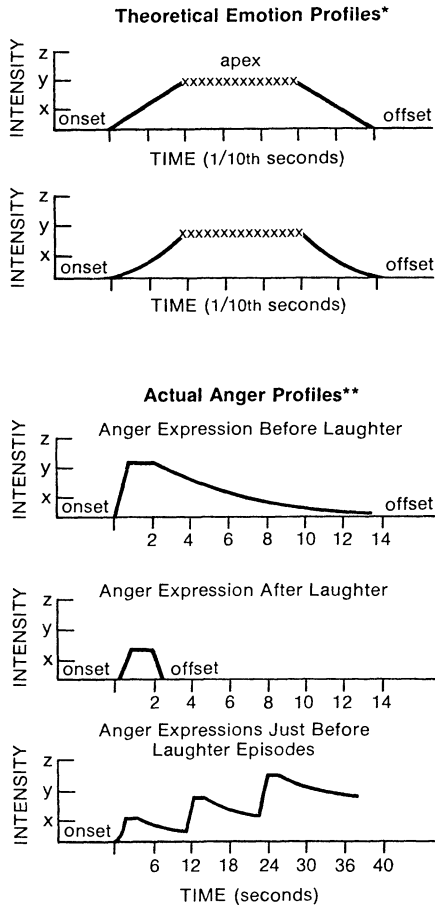
In a second preliminary study, we examined changes that occurred after laughter in two different interviews with the same subject (Scheff & Retzinger 1982). For this analysis, we rated the frequency, duration, and intensity of anger expressions, as well as the verbal indications of anger. Again we found sizeable reductions in all of these measures, significant at the .01 level.

Since the raters were aware of the hypotheses, we needed a way of testing for correlated bias in the ratings. For this purpose we empaneled two groups of naive raters who were unaware of the hypotheses, one group for the crying interview, the other for the laughter ones. The first group was asked to rate a randomly scrambled sequence of slides randomly chosen from before and after the cry. Their task was to rate the level of tension or relaxation in the face of the subject. The second group was asked to rate a randomly scrambled set of facial expressions in excerpts from the videotape, including expressions of anger from before and after the laughter. Their task was to name the type of emotion and its intensity.

The results from both panels corroborated the initial ratings. The crying panel saw a reduction in the level of tension after the cry, significant at .001. The laughter panel saw a reduction in the frequency and intensity of anger expressions, also significant at .001. Since there was no way the raters could have known the hypothesis or discerned the sequence of excerpts even if they had guessed, correlated bias in the ratings seems to be ruled out.

The measurement of duration in the laughter interviews produced a dramatic, unexpected finding. Duration involves an onset (beginning), an apex (level of highest intensity), and an offset (end) of a particular facial action. (See the theoretical profiles in Figure 1, taken from Ekman & Friesen 1978.) In the first laughter interview, many of the expressions of anger before laughter were of unusually long duration: 4–10 seconds. (Most felt expressions are brief: 1–2 seconds.) The most unusual thing about these expressions, however, was their shape (see Figure 1). Instead of the rapid offset that occurred in the expressions of anger after the laughter, and in all of the other expressions we had ever seen, these expressions had a brief apex and a long slow decay, making it difficult to decide where the offset occurred.

This finding seems to provide initial support for an idea that had previously been only theoretical: the concept of the asymptote. In my version of the theory of catharsis, I have argued that emotional states not resolved by catharsis linger for a long time. This phenomenon is analogous to the slow dissipation of sexual tension when orgasm has not occurred. The expressions of anger in the second laughter interview are somewhat longer than in the first, ranging from 6–15 seconds. For this one subject, the concept of the asymptote seems to be confirmed, especially since this phenomenon virtually disappears after the laughter episodes. In the two interviews combined, there are 23 anger asymptotes before the laughter and only one afterwards.



* Ekman and Friesen, Facial Action Coding System 1978.

** Scheff and Retzinger, "Facial Expression and Catharsis: A Preliminary Study of Laughter and Anger" 1982.

Figure 1 Theoretical and actual emotion profiles.

I have drawn a profile of the anger seen in the face of the subject immediately before the laughter episodes (see Figure 1). The profiles for both the interviews are very similar except that in the first a small laughter episode occurs before the main one. In both cases, three spikes of very long anger expressions precede the main laughter. None of the expressions ever returns to baseline; there is no offset. Each becomes the base of the next expression, building ever higher intensities, until the third expression dissolves into the smile that precedes the laughter.

This finding illustrates another aspect of the idea of the asymptote. I have argued that asymptotic emotions go on forever. They never quite return to baseline, though their intensity may ultimately become vanishingly small. This could be an important effect, however, if many episodes of emotion (in this case anger) remain unresolved. The mounting intensity of the expressions of anger immediately before laughter, each asymptote building in intensity because it is riding on the tail of the asymptote before it, seems to illustrate this effect. If there had been no laughter episode, the asymptotic tail would presumably have been thicker and longer because of the build-up. These results are all preliminary, until we have analyzed a sizeable number of cases and refined our rating techniques further.

Videotapes, in conjunction with FACS, offer an extraordinarily powerful new method for studying emotions, perhaps comparable to the introduction of the microscope in the study of microorganisms. If some of our paradigmatic conflicts can be reconciled, perhaps we will be able to move to a new level of understanding emotions. The time has come to pool our knowledge of culture and biology rather than continue our ideological debates. We now have the concepts and methods to allow us to formulate and test specific hypotheses. With a spirit of cooperation we may be able to proceed rapidly in this vital field.

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Literature Cited

- Averill, J. R. 1974. An analysis of psychophysiological symbolism and its influence on theories of emotion. *J. Theor. Soc. Behav.* 4:147-90
- Averill, J. R. 1975. A semantic atlas of emotional concepts. *J. Suppl. Abstr. Serv. Cat. Sel. Doc. Psychol.* 5:330
- Averill, J. R. 1976. Emotion and anxiety. In *Emotion and Anxiety*, ed. M. Zuckerman, C. Spielberger. NY: Wiley
- Averill, J. R. 1968. Grief: its nature and significance. *Psychol. Bull.* 70:721-48
- Averill, J. R. 1969. Autonomic response patterns during sadness and mirth. *Psychophysiology* 5:399-414
- Averill, J. R. 1980. A constructivist view of emotion. In *Emotion: Theory, Research, and Experience*. Vol. 1: *Theories of Emotion*, ed. R. Plutchik, H. Kellerman, pp. 305-39. NY: Academic
- Bartrop, R. W., Lazarus, L., Luckhurst, E., Kiloh, L. G., Penny, R. 1977. Depressed lymphocyte function after bereavement. *Lancet* 1:834-36
- Benedict, R. 1934. *Patterns of Culture*. NY: Mentor
- Boucher, J. D. 1973. Facial behavior and the perception of emotion: Studies of Malays and Temuan Orang Asli. Presented at Conf. Psychol. Related Disciplines, Kuala Lumpur
- Cooley, C. H. 1909. *Social Organization*. NY: Scribners
- Darwin, C. 1955. *The Expression of Emotion in Man and Animals*. NY: Philosophical Library
- Durkheim, E. 1915. *The Elementary Forms of Religious Life*. NY: Macmillan
- Ekman, P. 1972. Universals and cultural differences in facial expressions of emotion. *Nebr. Symp. Motiv.*, pp. 207-83
- Ekman, P. 1973. Cross cultural studies of facial expression. In *Darwin and Facial Expression*, ed. P. Ekman. NY: Academic
- Ekman, P., Friesen, W. V. 1982. Felt, false and miserable smiles. *J. Nonverb. Behav.* 6:1-24
- Ekman, P., Friesen, W. V. 1978. *Facial Action Coding System*. Palo Alto, CA: Consulting Psychologists Press.
- Ekman, P., Oster, H. 1979. Facial expressions of emotion. *Ann. Rev. Psychol.* 30:527-54
- Ekman, P., Friesen, W. V., Ancoli, S. 1980. Facial signs of emotional experience. *J. Personal. Soc. Psychol.* 39(6):1125-34
- Ekman, P., Hager, J. C., Friesen, W. V. 1981. The symmetry of emotional and deliberate facial actions. *Psychophysiology* 18(2): 101-6
- Ekman, P., Sorenson, E. R., Friesen, W. V. 1969. Pan-cultural elements in facial displays of emotion. *Science* 164(3875):86-88
- Gordon, S. L. 1981. The sociology of sentiments and emotions. In *Social Psychology: Sociological Perspectives*, ed. M. Rosenberg, R. H. Turner, pp. 562-92. NY: Basic
- Gorer, G. 1965. *Death, Grief, and Mourning*. Garden City, NY: Doubleday
- Hochschild, A. 1983. *The Managed Heart: The Commercialization of Human Feeling*. Berkeley: Univ. Calif. Press
- Hochschild, A. 1975a. The sociology of feeling and emotion. In *Another Voice*, ed. M. Milman, R. Kanter, pp. 280-307. Garden City/NY: Doubleday
- Hochschild, A. 1975b. Attending to, codifying, and managing feelings. Paper presented at the annual meeting of the Am. Sociol. Assoc., San Francisco
- Hochschild, A. 1979. Emotion work, feeling rules, and social structure. *Am. J. Sociol.* 85: 551-75
- Hofer, M., Wolff, C., Friedman, S., Mason, J. W. 1972. A psychoendocrine study of bereavement. *Psychosom. Med.* 34:481-504
- Izard, C. 1971. *The Face of Emotion*. NY: Appleton-Century-Crofts
- James, W. 1890. *The Principles of Psychology*. NY: Holt
- Kemper, T. D. 1978. *A Social Interactional Theory of Emotions*. NY: Wiley
- Lazare, A., ed. 1979. *Unresolved Grief. Out-patient Psychiatry: Diagnosis and Treatment*. Baltimore: Williams and Wilkins
- Parks, C. M. 1972. *Bereavement: Studies of Grief in Adult Life*. NY: International Universities Press
- Paul, N. L., Grosser, G. H. 1965. Operational mourning and its role in conjoint family therapy. *Commun. Ment. Health J.* 1:339-45
- Ramsey, R. W., Happee, J. A. 1977. The stress of bereavement. In *Stress and Anxiety*, ed. C. D. Spielberger, I. G. Sarason. London: Wiley
- Rosenblatt, P. C. et al. 1976. *The Expression of Emotion in Bereavement*. New Haven, CT: HRAF Press
- Saha, G. B. 1973. Judgment of facial expression of emotion—a cross-cultural study. *J. Psychol. Res.* 17(2):59-63
- Schachter, S., Singer, D. 1962. Cognitive, social and physiological determinants of emotional state. *Psychol. Rev.* 69:379-99
- Scheff, T. J. 1979. *Catharsis in Healing, Ritual, and Drama*. Berkeley: Univ. Calif. Press.
- Scheff, T. J., Bushnell, D. D. 1981. The physiological and psychological effects of humor and laughter. Presented at Int. Conf. on Humor, Washington, DC
- Scheff, T. J., Gleberman, L., Parker, R. 1982. Sadness, crying and the resolution of grief.

- Presented at Ann. Meet. Am. Psychol. Assoc., Washington, DC
- Scheff, T. J., Retzinger, S. 1982. Facial expression and catharsis: a preliminary study of laughter and anger. Presented at Ann. Meet. Int. Sociol. Assoc., Mexico City
- Scheff, T. J., Scheele, S. 1979. In *Catharsis in Healing, Ritual and Drama*, ed. T. J. Scheff. Berkeley CA: Univ. Calif. Press
- Shibutani, T. 1961. *Society and Personality*. Englewood Cliffs, NJ: Prentice-Hall
- Steiner, J. E. 1973. The gustofacial response: Observation on normal and anencephalic newborn infants. In *Fourth Symposium on Oral Sensation and Perception*, ed. J. F. Bosma. Bethesda, MD: USDHEW
- Volkan, V. D., Josephthal, D. 1979. Brief psychotherapy in pathological grief: re-grief therapy. In *Specialized Techniques in Psychotherapy*, ed. T. B. Karasu, L. Bellak. NY: Brenner/Mazel
- Zajonc, R. B. 1980. Feeling and thinking: preferences need no inferences. *Am. Psychol.* 35:151-75