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How Did Fear Become a Scientific Object and What Kind of Object Is It?

WHEN ON SEPTEMBER 11, 2001, terrorists killed more than three thousand people in their attacks on the World Trade Center in New York City, for Americans in particular the world suddenly became a much more frightening place. Insecurity became the norm as the Bush administration’s new Department of Homeland Security used its color-coded terror alert system to orchestrate and manipulate the public’s fears. Among the many consequences of 9/11 has been the flow of federal funds to scientists committed to finding ways to identify terrorists before they can act. One of those is Paul Ekman, a psychologist who has devoted his career to studying facial expressions and is working on methods of surveillance designed to read the telltale involuntary facial signs that betray the potential terrorist’s deadly intentions. Ekman’s goal is to reassure us that we don’t have to be frightened by the tendency of human beings to dissimulate, because science can be counted on to reliably distinguish authentic facial expressions from false ones, genuine from feigned.1

In this paper, I examine the theoretical assumptions and methods informing Ekman’s approach to the emotions and facial expression. I offer my analysis as a contribution, from the perspective of the history of the human sciences, to our understanding of one major strand of research in the United States that has helped shaped the science of fear. Ekman’s is not the only game in town; in fact, as we shall see, there are signs today of a growing opposition to his research program from within psychology. But for the last thirty years he has exerted, and indeed continues to exert, a powerful influence. Not only do psychologists and neuroscientists routinely cite his experimental findings; as a consequence of the striking recent growth of interest in the emotions and affect in the humanities and social sciences, his work has begun to attract the attention of scholars in philosophy, political theory, cultural studies, literature, and related fields. Among those who share many of Ekman’s

ABSTRACT This essay provides an analysis of the role of posed facial expressions and of the facial image in the development of the science of fear. / REPRESENTATIONS 110. Spring 2010 © The Regents of the University of California. ISSN 0734–6018, electronic ISSN 1533–855X, pages 66–104. All rights reserved. Direct requests for permission to photocopy or reproduce article content to the University of California Press at http://www.ucpressjournals.com/reprintinfo.asp. DOI:10.1525/rep.2010.110.1.66.
presuppositions is the celebrated neuroscientist Antonio Damasio. In the
course of my paper I shall be discussing an important case, investigated by
Damasio, Ralph Adolphs, and others, of a young woman who, it has been
claimed, is unable to experience fear because she has an abnormal brain.
More precisely, the patient suffers from a genetic disorder that causes bilat-
eral calcification of her amygdala, a subcortical group of neurons widely held
to be implicated in rapid emotional responses, especially fear. Crucial to
Damasio’s interpretation of the case has been his use of methods developed
by Ekman to test a person’s ability to judge emotional expressions. In partic-
ular Damasio has employed a set of pictures of people intentionally present-
ing, that is, posing such expressions drawn from Ekman’s portfolio of such
items, in order to evaluate the deficits in his amygdala-damaged patient’s skill
in judging threatening faces.

I find Ekman’s pictures at once interesting and puzzling (figs. 1 and 2).²
One of my aims in this paper is to make such images historically intelligible
while also bringing out what seems to me their sheer strangeness as scient-
ific documents.

**Emotions as Nonintentional States:**
**Tomkins’s Affect Program Theory**

To begin: When in the 1960s Ekman began studying nonverbal
behavior, including facial expressions, the emotions after years of neglect
were just beginning to become a topic of renewed concern among scientists.
Ekman started his investigations at a time when one psychologist in particu-
lar, Silvan S. Tomkins, was proposing a new way of thinking about the affects.
Influenced by several important trends in the human sciences, especially
a resurgent interest in Darwinian evolution and the rise of cybernetics, Tomkins
turned his back on the then reigning orthodoxies of psychoanalysis to advocate instead a Darwinian-inspired biological theory of the emotions.

Tomkins argued that there exists a small number of basic emotions defined in evolutionary terms as universal or panchural adaptive responses of the organism. He proposed that there were eight or nine such basic emotions, namely, fear, anger, distress, disgust, interest, shame, joy, and surprise. (He later added contempt.) Tomkins described the basic emotions as discrete, hard-wired, reflex-like “affect programs” located in the subcortical parts of the brain. He believed that each discrete emotion manifested itself in distinct physiological and behavioral responses, and especially in characteristic facial expressions. Central to Tomkins’s approach was the idea that although the emotions can and do combine with the cognitive and other information-processing systems in the brain, they are essentially separate from these. In other words, he posited a disjunction between our emotions and our knowledge of what causes and maintains them by treating feeling and cognition as two separate systems. He thus argued that there is a “radical dichotomy between the ‘real’ causes of affects and the individual’s own interpretations of these causes.” In short, according to Tomkins the affects have no inherent knowledge of, or relation to, the objects that trigger them—which is why, he thought, we are so liable to be wrong about our feelings and ourselves.

By separating the affects from cognition Tomkins broke with the paradigm that had previously governed much of the work on the emotions. According to Tomkins’s predecessors, emotions are intentional states: they are directed toward objects and depend on our desires and beliefs about the world. In Sigmund Freud’s case of “Little Hans,” to take one famous example, the five-year-old Hans becomes terrified of horses and refuses to go anywhere near them. The immediate, precipitating cause of his fear was his experience of seeing a horse fall down, as if dead. But what interests Freud is not the contingent role of the event of the falling horse, but the meaning the horse has for Hans, especially the meaning it has for him as a substitute object for his conflictual desires, wishes, and beliefs. In the course of Freud’s narrative of the case it emerges that the little boy not only fears horses but also unconsciously identifies with them, to the point of wanting to be the very object of his terror. According to Freud, the reason the horse became so salient or important for the child is that it had once been an object of interest and desire for him: “What is today the object of phobia must at one time in the past have been the source of a high degree of pleasure.” The child had liked horses and enjoyed playing at being a horse before the onset of his phobia. By identifying with or becoming the horse in play, he can “bite” the father whom he (loves and) dreads. As Freud would later argue, the child’s unconscious yielding to, or identification with, the powerful, aggressive father solves the problem of the child’s conflictual wishes and intentions toward him, albeit at the price of
the child’s experiencing the incorporated paternal aggression in the form of a guilty, anxious conscience. Hence Freud joins together fear, the phobic object, identification, guilt, anxiety, and the subject in a single explanatory complex. And of course for Freud the wishes and intentions that underlie the phobic reaction can only be brought into the subject’s awareness through the transferential-narrative process of the “talking cure.”

I am not concerned here with the validity of Freud’s analysis of the case of Hans, which has been widely dismissed in scientific circles ever since J. Wolpe and S. Rachman in 1960 critically reviewed it and reinterpreted phobia as a conditioned response. My purpose is simply to indicate that psychoanalysis—and more generally “cognitivist” or “propositional” or “appraisal” or “phenomenological” approaches to the emotions—operate within an intentionalist paradigm that makes questions of meaning and belief of fundamental importance. The intentionalist approach contrasts strikingly with that of Tomkins, for whom the affects are, crucially, nonintentional states. His account of fear goes something like this: If I run from a snake I do not do so because I believe there is a dangerous object in front of me and desire or intend not to be bitten by it. I run because I am terrified of snakes. The threat of the snake lies out there in the object: snakes make me frightened because they were once terrifying to our ancestors in evolutionary history. The snake on this model does not function as an object of my beliefs or desires but as a “trigger” or trip wire for an involuntary, hard-wired response that is rapidly discharged without the affect system’s knowledge of the object that triggered it. As Donald Nathanson has recently stated in support of such an approach to the emotions, the affects

bear no intrinsic relation to any triggering source. . . . If we are frightened, some other mechanism will have to tell us what has become not just too much, but more too much. . . . The affects are completely free from inherent meaning or association to their triggering source. There is nothing about sobbing that tells us anything about the steady-state stimulus that has triggered it; sobbing itself has nothing to do with hunger or cold or loneliness. Only the fact that we grow up with an increasing experience of sobbing lets us form some idea about its meaning.

Tomkins’s affect theory thus suspends or displaces considerations of intentionality and meaning in order to produce an account of the emotions as discharges of facial behaviors and related autonomic responses that are fundamentally corporeal in nature. On this model of fear, the nervous system is understood to be “wired directly to the onset of the danger.”

For Ekman in the 1960s the most intriguing aspect of Tomkins’s affect theory was his claim that emotions are universal categories or “natural kinds” that are accompanied by distinct facial expressions. Tomkins’s views were at odds with the prevailing consensus among psychologists and particularly anthropologists, who doubted that facial behaviors had the same meaning.
in all cultures. The contradictory results of more than five decades of experimen-
tal research on the recognition of facial expression reinforced their
skepticism on this point.\textsuperscript{13} But Tomkins was determined to prove them
wrong.\textsuperscript{14} According to Ekman’s “neurocultural” version of Tomkins’s theory,
socialization might determine the range of elicitors that can trigger the affect
programs and can moderate facial movements according to social norms or
“display rules,” but the underlying emotions might nevertheless “leak out.”\textsuperscript{15}
He was therefore convinced it ought to be possible to find ways of distinguishing
or separating out the involuntary, biologically determined facial expressions
from those that are influenced by learning and culture. With Tomkins’s
couragement and help, Ekman in the late 1960s embarked on a series of
highly influential cross-cultural judgment studies aimed at determining
whether the basic emotions, as manifested in photographs of posed facial
expressions, can be universally recognized by literate and preliterate people
alike.

\textbf{Photographing Emotions}

The use of photographs for the scientific study of expression goes
back to the pathbreaking work of the French scientist Duchenne de
Boulogne, and of course to Charles Darwin’s \textit{The Expression of the Emotions in
Man and Animals} (1872). Early on in \textit{Expression}, Darwin—among the first to
use photographs for scientific purposes—describes the challenges con-
fronting anyone wishing to study the face in an objective way. In a brief but
telling passage he observes:

\begin{quote}
The study of expression is difficult, owing to the movements being often extremely
slight, and of a fleeting nature. A difference may be clearly perceived, and yet it
may be impossible, at least I have found it so, to state in what the difference con-
sists. When we witness any deep emotion, our sympathy is so strongly excited, that
close observation is forgotten or rendered almost impossible.\textsuperscript{16}
\end{quote}

One implication that was drawn from this state of affairs was the need for
“instantaneous” photography because, it was thought, only high-speed pho-
tography would be capable of capturing objectively—that is, of arresting and
“freezing” in time—the rapid play of facial muscles in a single image.\textsuperscript{17} It
might be objected that Darwin’s statement has rather different implications.
For instance, one might want to argue on the basis of his remarks that ex-
pressions are all about movement, especially the subtlety and volatility of the
movements of the muscles of the face, and that these movements—perhaps
\textit{actions} would be a better word—are what we ordinarily mean by emotion.
From this point of view any technology for objectifying the movements of the
face in a freeze-frame is likely to be useless, because by freezing an expression
the camera isolates a single, static moment in an overall flow of events that alone gives that moment its meaning. Another implication of Darwin’s remarks would seem to be that there is an ineluctable sympathetic-identificatory dimension to our perceptions of other people, a dimension that will inevitably be ignored or suppressed by techniques of observation based on the assumption of an absolute separation between subject and other, between the viewer and the object of his gaze.

For the photographers to whom Darwin turned for help to illustrate the expressions of emotion, however, the solution to the difficulty was to be found in the use of “instantaneous” photographic techniques capable of capturing and fixing once and for all the subtle, transient movements of the face invisible to the naked eye. But as Phillip Prodger has observed, although advances in photochemical technology had reduced exposure times, thereby making it possible for the camera to capture relatively small movements in time, by contemporary standards “exposure times were still long: in the order of tens of seconds.”¹⁸ This meant that the subject was required to remain completely still for several seconds while his or her picture was taken. One consequence of this, of course, was that the subject was fully aware of being observed. In its fidelity to the scene, the photograph inevitably registered this fact, which was perceived in terms of an aspect of artificiality, conventionality, exaggeration, or “theatricality” in the sitter’s expression as he or she self-consciously held the desired pose.¹⁹ Darwin accepted the need for posing and indeed for photographic manipulation and editing in order to produce convincing portraits of the emotions. Figure 3, for example, shows a picture of the Swedish photographer, Oscar Rejlander, well known for his mastery of photographic manipulation, himself posing the emotion of surprise in one of the thirty photographs Darwin included in his book. Figure 4 shows Darwin’s picture of fear or terror. The latter is not a photograph; rather, it is a slightly altered engraving after a photograph. The original was taken by Duchenne de Boulogne (fig. 5). In it one can see that the muscles of the man’s face have been artificially stimulated by electrical currents to produce the facial movements allegedly expressive of terror; in the engraving he commissioned for his book Darwin had the electrodes removed in order to give the face a more natural appearance.²⁰

Prodger has remarked that Darwin “considered the fleeting nature of expression to be the principal difficulty in observing facial movements,” as if for Darwin the problems he encountered were merely technological.²¹ This seems to be Prodger’s view as well, since he observes in this connection that “the limitations of photographic technology precluded objective recording of facial expressions.”²² There is an important sense, of course, in which the limitations Darwin confronted were not merely or only technological, but had a deeper, more philosophical or, say, ontological cause. A passage in a text by Johann Caspar Lavater, a famous eighteenth-century predecessor in the

physiognomic tradition to which Darwin, Tomkins, and Ekman can all be seen to belong, bears directly on this point. “I want to discover in the mild-mannered person the traits of this manner, in the humble person the signs of humility,” Lavater observed in 1772, exactly a hundred years before Darwin. He goes on:

But my observations must be exact, they must be repeated and tested often. How can that be possible if I have to make these observations on the sly? Isn’t it presumptuous to analyze faces? And if a humble person notices that she is being observed, won’t she turn away and hide her face? Indeed, it is here that I encountered one of the greatest obstacles to my studies; anyone who notices that he or she is being observed either puts up resistance or dissimulates. How can I get around this problem? Perhaps in part in the following way.

I retire into solitude; I place before me a medallion or a piece of antique sculpture, the sketches of a Raphael, the apostles as depicted by Van Dyck, the portraits of Houbraken. These I can observe at will, I can turn them and view them from all sides.  

On the one hand, in this passage Lavater recognizes that the spectatorial gaze of the observer is itself the problem, since it inevitably makes the subject aware of being seen. From this it follows that the risk of a certain posing or conventionalizing of expression inheres in the very situation of being beheld, which is to say that the problem of posing can’t be solved by technological means, such as the invention of the high-speed camera. On the other hand, and this is the point I especially want to stress, Lavater’s belief
that the truth shows itself on someone’s face when the person believes he or she is alone is what might be called a theoretical or ideological claim. It does \textit{not} follow from the fact that the face is a privileged site of expression for us that if only it could be seen in solitude it would tell us the deep truth about ourselves. To believe this is to succumb to the view that a distinction can be strictly maintained between authentic and artificial signs, based solely on the state of awareness of the subject in question. More precisely, it is to surrender to a physiognomic ideology that claims we can draw an absolute differentiation between the masks we allegedly put on for others and the genuine faces we have when we are alone and no one is watching. (One limit case here, I suppose, would be to photograph persons when they are asleep. The expressions of blind people have also been imagined as free from all social taint, which is why they have figured in discussions of the Tomkins-Ekman affect program theory.) It’s as if we were to imagine that we cannot find out the truth about people in the course of interacting with them in daily life, or by discovering how they act in an emergency, or on the basis of intimate conversations with them, but only when they are entirely removed from human intercourse. But if someone is untrustworthy, or inconsistent, or indeed honorable, is it plausible that the truth about that person will emerge simply because he or she is alone?

And of course if you think this way, if you believe that the answer to this question is yes, then you will have to worry about whether the expressions you have recorded by photographic means for scientific observation really are natural and unposed. You will find yourself continually haunted by the suspicion or charge that the images you are studying are not truly spontaneous because you have not adequately isolated them from the contamination of others.\textsuperscript{24} As we have seen, Lavater in the pre-photograph era thought one solution to the problem of the subject’s consciousness of being beheld was to study artistic illustrations of expression in the privacy of his study. His idea seems to have been that, since the subjects of such artist’s representations were not physically present to him, the problem of posing could not arise. Lavater’s solution could hardly satisfy Ekman, and it is not surprising that in the latter’s discussion of methods for recording emotional expressions he ruled out of consideration the results of \textit{any} experiments that had made use of artists’ drawings and illustrations, precisely on the grounds that such pictures were likely to be too stereotyped or subjective for scientific purposes.\textsuperscript{25}

But Ekman \textit{was} committed to the project of distinguishing between nature and culture, between the “natural signs” of emotional expression that might be so small or rapid as to be easily overlooked and the culturally coded displays or “artificial signs” that he thought tended to mask or disguise them.\textsuperscript{26} The whole point of his Tomkins-inspired “neocultural” theory of the emotions was to reconcile the warring sides in the dispute between the
universalists and the culturalists by developing a framework capable of explaining the occurrence of both universal and culturally specific facial expressions. According to Ekman, the display rules of any given culture are the social norms that govern the way a person manages and controls his or her emotions. Thus when a person is fearful he or she might exaggerate, constrain, or neutralize feelings and expressions in conformity with the felt requirements of the social context. (Ekman had been reading Erving Goffman on the performance of social roles.) But Ekman believed that it should be possible to identify the pancultural or universal signs of emotion behind the culturally determined display rules and even outright dissimulations (“lies”) that mask the truth. For these purposes, photographs of persons posing expressions were for him an indispensable research tool because, by fixing expressions in still images, they made possible the testing of people’s ability across the world to recognize and identify particular emotions. Still pictures were also crucial if Ekman was to realize his project to identify and code the component muscle movements responsible for the facial expressions supposedly linked to each of the hypothesized basic emotions—not an easy project, as he recognized, since facial movements seemed to lack natural divisions and the selection of variables was unclear.

**Ekman and the Problem of the Pose**

Starting in the late 1960s, then, Ekman set out to test the idea that the emotions could be correctly judged by everyone, literate and preliterate people alike. On the basis of studies purporting to demonstrate that American, Brazilian, Japanese, and other observers, including observers from among the last of the isolated, preliterate cultures of New Guinea, could all successfully distinguish the primary affects as represented in photographs of people posing expressions, he concluded that such facial expressions must contain universal or pancultural elements linked to the hypothesized “basic emotions.” In order to uncover the pancultural elements, he needed photographs of facial movements that, as he put it, were free of cultural differences because of learned evokers, display rules, and consequences. He therefore attempted to select such photographs in order to prove that observers from different cultures recognized the same affect from the same photograph. In other words, to test the validity of the universality thesis, Ekman preselected photographs of posed expressions that he theorized were already free of cultural influence. The Facial Action Scoring Technique, or FAST, which he and Tomkins developed at this time as a system for measuring facial expressions held to be characteristic of the hypothesized affect categories, played a role in this preselection process. For, partly on the basis of FAST (and partly on the basis of personal intuition), the two men chose...
only those photographs, from among more than three thousand such pictures of posed expressions employed by themselves or others in previous experiments, that had obtained the highest agreement about the emotion portrayed and that seemed to them to exhibit the affective expressions in their specific (“unmixed”) and unacculturated form. Since posed facial expressions were critical to establishing FAST in the first place, posing was institutionalized in Ekman’s first system of measurement from the start. His procedures were therefore highly recursive, to say the least.

I can sum up Ekman’s situation by saying that photographs of posed emotional expressions were central to his research enterprise, even as he was also committed to maintaining that such photographs were unconventionalized, unacculturated representations of the universal truth of the primary emotions. It was especially urgent for him to argue this point, since the status of still, posed expressions had been a topic of much contention during the previous decades of emotion research. Ekman’s concern with this theme is especially evident in his book Emotion in the Human Face: Guidelines for Research and an Integration of Findings (1972), in which he and his co-authors reanalyzed many of the experiments on the judgment of facial expression performed between 1914 and 1970 and presented their own findings in order to argue that, contrary to the opinion of the majority of his contemporaries, the preponderance of the data yielded consistently positive evidence in support of the universality thesis. But the problem of posing never really disappears from his work. Although Ekman moves with seeming confidence in this terrain, his arguments are marked by considerable tension and even, at times, incoherence. In effect, he adopts two different, apparently competing responses to the problem of posing:

Ekman’s first response to the problem of posing was to argue on the basis of the empirical evidence that, since people all over the world regardless of their cultural origins and background consistently labeled photographs preselected to depict the basic emotions the same way, facial expressions must be universal. “Observers in both literate and preliterate cultures chose the predicted emotion for photographs of the face, although agreement was higher in the literate samples,” Ekman wrote. “These findings suggest that the pancultural element in facial displays of emotion is the association between facial muscular movements and discrete primary emotions, although cultures may still differ in what evokes an emotion, in rules for controlling the display of emotion, and in behavioral consequences.” In other words, the fact that, as his cross-cultural experiments seemed to demonstrate, literate and preliterate people alike could agree when looking at photographs that certain facial expressions connoted the same emotional categories argued against the idea that posed expressions were determined by conventions, as the cultural relativists claimed. Rather, posed expressions must contain pancultural
elements and must be in some way “based on the repertoire of spontaneous facial behaviors associated with emotion.”

Ekman granted that posed and spontaneous expressions were not completely identical. Rather, as he explained, posed expressions were similar in appearance to “that spontaneous behavior which is of extreme intensity and unmodulated, although it may differ in onset, duration, and decay time.” Thus, according to Ekman, if posed expressions do look a bit strange, odd, or extreme, this is just because they are what spontaneous expressions would look like if they were not fixed in time, or not blends, or not altered or modified (“modulated” or “moderated”) by culturally inflected display rules. His idea seems to have been that social constraints suppress or damp down or deflect our facial expressions from conveying the inward “truth” of our feelings, and that the emphaticness and single-mindedness of posed expressions (that is, their exaggerated, caricatural, even comic-book quality) are the outcome of the successful avoidance or outflanking or overcoming of that suppressive effect, though it is not clear how that feat is managed. For him, the tendency of such expressions to look exaggerated is not the result of convention but exactly the opposite: the absence of those conventions that in everyday life constrain or moderate or mask behavior. In effect, Ekman weirdly claimed that posed expressions in their very caricatural intensity are among the best examples we have of what we would look like if we were entirely alone. “Our view is that posed facial behavior is similar to, if perhaps an exaggeration of, those spontaneous facial behaviors which are shown when the display rules to deintensify or mask emotion are not applied. . . . Posed behavior is thus an approximation of the facial behavior which spontaneously occurs when people are making little or no attempt to manage the facial appearance associated with intense emotion.”

It follows for Ekman that when an investigator asks someone to pose an emotion, all he is doing is implicitly requesting the subject to show that emotion without attempting to “deintensify, mask, or neutralize his facial appearance” as the display rules would normally require. The subject simply interprets the instruction as an “occasion to display an uncontrolled version of the emotion.” I repeat: How this is supposed to work is a mystery; it’s as if Ekman imagined that the poser ordinarily follows a set of explicit rules and conventions about how he or she is meant to act but is able to suspend these at will when asked to do so—or indeed when asked to pose!

But Ekman also dealt with the problem of posing in a different way. According to what I am calling his second response, and in an apparently contradictory gesture, he admitted that the subject’s consciousness of being beheld when posing for the camera inevitably introduced what was, according to the logic of his project, a problematic component of conventionality. Ekman not only knew and accepted this, he even used the issue of the subject’s awareness of being observed in certain experiments as a reason for
rejecting some of his predecessors’ findings in the field. He also recognized that so-called “candid” photos, widely understood as solving the problem of posing because the subject was assumed to be oblivious of the camera, might not be so candid after all. In other words, he conceded what he elsewhere denied, that posed facial expressions are conventionalized behaviors.

We can track Ekman’s arguments on this point by noting that in 1940 a psychologist named Norman L. Munn had objected to some experiments by Carney Landis on the grounds that what the latter had taken to be spontaneous emotional expressions of his subjects responding to various situations created in the laboratory, such as witnessing a rat having its head cut off, might not have been so spontaneous after all because the subjects had been filmed and therefore might have been “self-conscious, and hence not so spontaneously expressive as under more natural circumstances.”39 This is an objection with which Ekman agreed, and he did so precisely on the grounds that Landis’s subjects knew they were being photographed. As Ekman wrote:

A number of aspects of the experimental setting indicate the operation of display rules either to neutralize the facial responses or to mask them with a positive affect. All of Landis’ subjects knew him; most were psychologists who had had other laboratory experiences. Not only did they know they were being photographed, but, because Landis had marked their faces with burnt cork in order to measure the components of facial behavior in his other use of these records, they knew Landis was interested in their facial behavior.40

In short, Ekman here endorsed the idea that in posed expressions the subject’s awareness of being photographed was likely to induce the operation of masking conventions or “display rules”—the very idea that according to his first response to the problem of posing he appeared to reject.

In order to meet the criterion of “spontaneity without conventionalization” Munn had decided that “candid” photographs might do the job.41 Ideal material of this kind, he felt, would comprise candid shots of the same persons in a variety of emotional situations, together with verbal reports of the emotions they had experienced in each case. Munn remarked, though, that to obtain many different expressions of the same individual under such conditions would be practically impossible because “the subject, aware of the photographer’s purpose, would be no more naturally expressive than are subjects in the laboratory.”42 He therefore resorted to the use of “candid-camera” images published in Life and other journals of the day. He characterized such candid shots as “obviously unposed emotional expressions” and reported significant agreement among experimental subjects concerning the emotions those candid pictures were thought to express.43 But Munn’s solution to the problem of posing was soon held to be no solution at all when in 1941 Hunt complained that most of Munn’s “candid” photographs involved social situations where conventionalization could be expected. “Even the man holding the drowned person’s hand,”
he commented, “seems aware of the camera.”\(^{44}\) He therefore dismissed findings based on the use of posed expressions that appeared to support the thesis of the universal recognition of emotions. In Hunt’s view, emotions were not defined by distinct universal facial expressions or even distinct physiological responses, but were situational reactions involving a complex, learned relationship between the individual and his environment.

To Ekman, Hunt epitomized everything that was wrong with the culturalists’ approach to the emotions and facial expression. Nevertheless—and this is what I want to emphasize—he did not question the validity of Hunt’s criticisms of the candid shot. On the contrary, Ekman acknowledged as a methodological limitation in Munn’s experiments the fact that, as he observed, “the behavior studied (candid photographs taken from magazines) may not all actually have been spontaneous. The person shown in the photographs may have been aware of the photographer, or, even worse, might have completely re-enacted or staged the behavior for the press.”\(^{45}\) With this admission one might have expected Ekman to reconsider his use of posed expressions in his research. But he did not. Instead, he sidestepped the difficulty, essentially by changing the topic. That is, he proceeded as if after all the real problem was not that of posing \textit{per se}, but the merely methodological-technical one of the representativeness of findings based on the use of posed expressions. The result was a tendency on Ekman’s part to divert attention from the problem of posing, and to justify the continued use of posed expressions in experiments on emotion as long as the latter were carried out with the appropriate methodological safeguards.\(^{46}\) Above all, he returned to the (by now familiar) argument that “posed behavior is not a specialized, language-like set of conventions” unrelated to authentic emotional behavior on the grounds that he had shown that facial expressions could be accurately judged across cultures. “For these findings to emerge,” he wrote, “the behaviors occurring during posing must have developed in the same way across cultures. One reasonable explanation of such development would be that they are in some way based on the repertoire of spontaneous facial behaviors associated with emotion.”\(^{47}\) In other words, Ekman’s conclusion once again was that posed expressions are simply slightly exaggerated because they are unmodulated or uncontrolled versions of expressions of the kind that occur naturally when subjects are alone.

There would come a moment when Ekman had to respond to criticism on precisely this point. In 1975, in a scathing review of Ekman’s work, the anthropologist Margaret Mead, no lover of the universalist thesis, protested that all Ekman appeared to have demonstrated with his cross-cultural studies was that “\textit{simulated}” expressions of emotion—by which she meant pantomimed, or highly theatricalized, facial movements—could be recognized across cultures.\(^{48}\) This was not a trivial rebuke in light of Ekman’s commitment to distinguishing between

How Did Fear Become a Scientific Object and What Kind of Object Is It? 79
natural and artificial expressions and hence to isolating authentic faces hidden behind the codes of convention. Nor did Ekman dismiss her objection.⁴⁹ On the contrary, he acknowledged as a drawback of his cross-cultural studies precisely that the expressions he had studied were not authentic, or the real thing.⁵⁰ But Ekman had a solution to the difficulty. The physiognomist Lavater in the passage I quoted earlier had remarked: “But my observations must be exact, they must be repeated and tested often. How can that be possible if I have to make these observations on the sly? . . . Anyone who notices that he or she is being observed either puts up resistance or dissimulates.” In response to the problem of posing raised by Mead, Ekman now pointed to the results of an experiment he had already carried out using a device for recording facial expressions “on the sly” that had been unavailable to Lavater: *the device of the hidden camera. By secretly recording the emotional expressions of subjects as they watched stressful films, Ekman felt he had discovered the key to detecting authentic, spontaneous expressions. As he replied to Mead’s objection to his use of posed expressions: “The next type of research design answered this criticism.”⁵¹ Or as he also stated: “We avoided the display rule pitfall by videotaping when the subjects thought they were completely alone and unobserved.”⁵² It is to Ekman’s “next type of research design” involving the use of a hidden camera that I now turn.

**Spontaneous Expressions**

In the 1960s, in an interesting and important series of research studies, psychologist Richard S. Lazarus began using films in a systematic way to evaluate and measure the response of various subjects to stress. One film used extensively by him for this purpose was called *Subincision*. It was a short, silent, black-and-white anthropological film (with a running length of seventeen minutes) that showed naked Aborigines in the Australian bush undergoing puberty initiation rites involving scenes of extensive cutting of the penis with sharp stones, of bleeding wounds, and of the adolescents wincing and writhing in pain. The boys appeared to volunteer for the initiation procedure, which was carried out by older men.⁵³ Lazarus demonstrated that the film induced subjective and autonomic signs of stress in a variety of student and other viewers. But by varying the sound track of the film in ways designed to minimize or, alternatively, to enhance the threatening content, Lazarus was also able to show that the film became less or more disturbing to watch. He argued on the basis of these results and related considerations that the film’s threat could not be considered simply “out there as an attribute of the stimulus” but depended on the viewer’s appraisal process, which is to say, on the “person’s beliefs about what the stimulus meant for the thwarting of motives of importance to him.” Thus the same stimulus could

80 **Representations**
be threatening or not, depending on the “interpretation the person makes concerning its future personal significance.” In terms of the opposition between intentionalist and nonintentionalist accounts of the emotions with which I began this paper, Lazarus was on the intentionalist side.

In spite of their apparently different theoretical orientations, Lazarus lent Ekman considerable research help when the latter decided to make use of Subincision and other stress films to test his ideas about the universality of emotions. In a variant of Lazarus’s experiments, first published in 1972 and carried out with his collaborator Wallace V. Friesen, Ekman used a hidden camera to secretly videotape the facial expressions of American and Japanese students as they watched brief sections of Subincision and three other stress-inducing or “neutral” films when each student was alone in the viewing room. Japan was selected for comparison with the United States, Ekman explained, because of the common belief that Japanese facial behavior was sufficiently different from that of Americans to mitigate against finding universals. Ekman reported on the basis of his findings that the emotional responses of American and Japanese students to the stress films were very similar. But according to him, when an “authority figure” from the student’s own culture (actually a graduate assistant dressed in a white coat) was introduced into the room and interviewed the student about his feelings while the latter was viewing additional stress material, the facial behavior of the Americans and Japanese diverged. Ekman stated that the Japanese students masked their negative feelings about the stress films more than did the Americans when in the presence of the authority figure. Slow-motion videotape analysis allowing for direct scoring of the facial movements, he declared, demonstrated at a microlevel the occurrence of the Japanese students’ spontaneous negative emotional expressions before these were covered over by the culturally determined display rules controlling for false, polite smiles (unfortunately, no video frames showing this transition were published). Ekman thus claimed to have proven that the universal, biologically based feelings remained intact behind the culturally determined behavior, and hence to have demonstrated the validity of his neurocultural theory of the emotions.

Ekman’s study of apparently spontaneous expressions in American and Japanese students and their inhibition by display rules, presented by him as an answer to Mead’s worries about the problem of posing, has become canonical in the field. It is routinely cited today in works on emotion as decisive evidence in favor of the Tomkins-Ekman approach to the emotions. Yet in the last few years the study has been called into question. In particular, Alan Fridlund—Ekman’s former student—in a powerful critique published in 1994 demonstrated convincingly that the account given by Ekman and Friesen over the years of their Japanese-American experiment was inaccurate, and that their interpretation of the results in terms of the opposition

How Did Fear Become a Scientific Object and What Kind of Object Is It?
between genuine emotional expressions versus culturally coded display rules was unsupported. Fridlund made a number of methodological-technical criticisms of Ekman’s experiment. Although a thorough discussion of Fridlund’s critique lies outside the scope of this paper, I want to emphasize that central to Fridlund’s damning assessment was his rejection of Ekman’s fundamental assumption that the faces people make when they are alone are readouts, or authentic signs, of the truth of their inner emotional states. Ross Buck has put Ekman’s position this way: “When a sender is alone . . . he or she should feel little pressure to present a proper image to others, and any emotion expression under such circumstances should be more likely to reflect an actual motivational/emotional state.” As Fridlund has observed, Ekman made this same assumption in his crucial experiment on the differences between Japanese and American facial displays. Thus Ekman stated of the results of that investigation: “In this experiment we had shown how facial expressions are both universal and cultural. In private, when no display rules to mask expression were operative, we saw the biologically based, evolved, universal facial expressions of emotion. In a social situation, we had shown how different rules about the management of expression led to culturally different facial expressions.” Or as Ekman declared, “expressions do occur when people are alone . . . and contradict the theoretical proposals of those who view expressions solely as social signals.”

Ekman’s assumption that the truth shows itself on the face when someone believes he or she is alone is the same assumption that, as I earlier remarked, motivated Lavater’s physiognomic project. It is an assumption that depends on the claim that a distinction can be strictly maintained between authentic and artificial signs, between nature and culture. But Fridlund rejects that claim, offering instead a “Behavioral Ecology View” of faces that stresses the implicit sociality of even so-called solitary facial movements. He treats the differences observed between the facial displays of the Japanese and American students in Ekman’s well-known experiment as cultural differences in the management of facial behavior. He points out in this connection that “the experimenter himself is always an implicit audience, and his or her laboratory is always the stage for the directorial effort known as an ‘experiment.’ Thus the ‘alone’ phase of the study was implicitly social, and the . . . interview phases were simply more explicitly social. Thus contrasting the facial behavior in the ‘alone’ versus the interview phase as authentic versus managed [or cultural] is over-statement at a minimum.” In short, Fridlund regards Ekman’s Japanese-American experiment as a study of “audience effects,” arguing that “just because a viewer is alone physically does not mean that he is alone psychologically.”

The same year, in a masterly assessment of the cross-cultural facial judgment or recognition experiments reported by Ekman and his colleagues, James A. Russell demonstrated that the results were artifactual, depending
on forced-choice response formats and other problematic methods, such as within-subject experimental design, lack of variability in the order of the stimulus presentation, and the use of posed expressions, which beg the questions to be proved in ways that fundamentally undermined Ekman’s claims for the universal nature of the emotions. The net result of Fridlund’s and Russell’s analyses has been to dramatically challenge the empirical and theoretical validity of the Tomkins-Ekman research program. Fridlund has gone on to propose instead that facial movements or displays should be viewed, not as expressions of hard-wired, discrete internal emotional states leaking out into the external world but as meaningful behaviors that have evolved in order to communicate motives in an ongoing interpersonal or interindividual context or transaction. As Fridlund has put it: “Displays are specific to intent and context, rather than derivatives or blends of a small set of fundamental emotion displays. . . . Instead of there being six or seven displays of ‘fundamental emotions’ (e.g., anger), there may be one dozen or one hundred ‘about to aggress’ displays appropriate to the identities and relationships of the interactants, and the contexts in which the interaction occurs.” From this perspective, facial behaviors are relational or communicative signals that take other (real or imagined) organisms into account.

Recently, building on the work of Fridlund, Russell, and others, Lisa Feldman Barrett has published an impressive series of reviews of the growing empirical evidence that is inconsistent with the idea that there are six or seven or eight basic emotions in nature. She has come to the conclusion that “fear” and the other emotional categories posited by Ekman do not have an ontological status that can support induction and scientific generalization or allow for the accumulation of knowledge. The consensus among this group of formidably well-informed critics is that a new scientific paradigm for research on the emotions is needed. All the indications are that, whatever model or paradigm gains acceptance—if it ever gains acceptance (more on this in a moment)—it will be based on the same general intentionalist assumptions embraced by Freud in the sense that it will make the question of the affects’ meaning to the organism (or subject) of the objects in its world a central issue and concern.

As I have already begun to suggest, a constant motif in recent critiques of Ekman’s work has been the question of posing. It is sometimes said in this connection that the expressions on the faces of people who have posed for the camera are artificial or exaggerated and don’t appear in real life the results of experiments based on such pictures lack “ecological relevance” or “ecological validity.” The argument is that such posed expressions are caricatures, which, unlike “prototypical” expressions that are close to the average set of features for a naturally occurring emotion, are exaggerated expressions designed to maximally distinguish each hypothesized emotion.
category from the other. Evidence indicates that such caricatured expressions are easier to categorize than prototypical ones when the categories in question are otherwise hard to distinguish. The ways in which these posed expressions are produced has also come under scrutiny. Fridlund has offered a dramaturgical analysis of experiments involving the use of posed expressions, suggesting that those experiments are social scenarios in which the investigator implicitly functions as a director and the subject as a Stanislavski actor who “slips into role.” In a somewhat similar fashion, Russell has observed that Ekman’s universality thesis is not, or at least not directly, about posed faces, and states: “Posed faces do not express the emotion of the poser, but what the posed chooses to pretend and in a manner most likely to be understood by the observer.” Russell here puts an emphasis on deliberate feigning that may be somewhat misplaced, for he implies that actors or posers are never caught up in the emotional feelings they are trying to represent on their faces. The philosopher Ian Hacking may have come nearer to the truth about this matter when he recently suggested that what is often involved in posing is a subtle form of unconscious compliance. In a review of Ekman’s edition of Darwin’s The Expression of the Emotions in Man and Animals (3rd edition, 1998), he remarks of Darwin’s and many of Ekman’s choices of illustrative faces that they are quite extraordinary social documents. I am not sure I have seen anyone in real life looking like any of these people. One can wonder if all three groups, the American students, the wild men of New Guinea and the investigators, were not collaborating to generate the phenomena. These experiments may serve as instances of another human need, namely, wanting to please, wanting to get along. This is not simulation: if you are an experimental subject, you do not behave as you do because the scientist is boss, but because you are in a situation where you feel good about accommodating to his wishes, just as he feels good about accommodating to yours.

In spite of these incisive criticisms—others could be cited—Ekman’s approach to the emotions continues to thrive. Why this should be so raises interesting questions about scientific influence, authority, and power that cannot be addressed here. Instead I will turn in closing to the case described by Damasio and Adolphs of the patient with the damaged amygdala who cannot experience fear, in order to raise some final questions about the way the Tomkins-Ekman paradigm is being used today to underwrite the scientific investigation of the emotions.

“Have No Fear”

In 1976, in Pictures of Facial Affect, Ekman and Friesen made available in slide form a set of black and white photographs of posed facial expressions. Confident that emotions are universally recognizable, they presented
these pictures as prototypical expressions of six discrete affect categories. Two years later they published a new coding system called the Facial Action Coding System (FACS) for measuring and analyzing facial movements. This coding system, which replaced the earlier FAST coding system, was designed to provide an atheoretical, anatomically based, standardized scoring system of the movements of the face that researchers could use to test their hypotheses about the relationship between emotion and facial expression. Ekman’s pictures and FACS have since been used in hundreds of research studies. One scientist who has deployed Ekman’s work to great effect is Antonio Damasio, arguably the best-known neuroscientist in the emotion field today. Damasio accepts the Tomkins-Ekman paradigm, at least for the basic emotions. He regards affective responses as biologically determined, adaptive processes that depend on innately set devices with a long evolutionary history. According to him, although culture and learning introduce individual variations, emotions are fundamentally stereotyped and automatic responses of the body and face that can occur automatically, without conscious deliberation. That is why, he explains, Darwin was able to catalog emotional expressions in humans and animals, and that is why, in different parts of the world and across different cultures, emotions are so easily recognized. “The thing to marvel at, as you fly high above the planet,” he says, “is the similarity, not the difference” in facial expression among people, a view he sees as having been given “immeasurable support” by the work of Ekman. The biological function of emotions is thus to produce specific reactions to specific events in a fast and exquisitely reliable, automatic way. “For certain classes of clearly dangerous or clearly valuable stimuli in the internal or external environment,” Damasio writes in this connection, “evolution has assembled a matching answer in the form of emotion. This is why, in spite of the infinite variations to be found across cultures, among individuals, and over the course of a life span, we can predict with some success that certain stimuli will produce certain emotions. (This is why you can say to a colleague, ‘Go tell her that; she will be so happy to hear it.’)” One might be tempted to protest that a colleague won’t be happy unless she understands what she has been told, and if she understands it, she’s doing cognition. Damasio seems to think, however, that her feeling of happiness will automatically follow from the “stimulus.”

In 1994 Damasio teamed up with Ralph Adolphs to study the case of a young woman, SM, whose brain scan revealed that she suffered from an extremely rare genetic disease producing in her case complete, bilateral damage to the amygdala. Since the work of Joseph LeDoux and others, the amygdala has been implicated in rapid emotional responses, especially the emotion of fear. Adolphs and his colleagues reported that SM was normal in every way except for one strange symptom: she was unusually—that is, inappropriately,
even excessively—forthcoming with people. Indeed, she so lacked normal
reserve and reticence that she had often been taken advantage of by those she
trusted. When tested on her ability to judge prototypical facial expressions
of six basic emotions in pictures taken from Ekman and Friesen’s *Pictures of Facial
Affect* (1976), SM was unable to identify the expression of fear, as was demon-
strated by the fact that her ratings intensity for fearful faces was low compared
to those of normal subjects, although she had no difficulty identifying familiar
faces. In a subsequent study Adolphs and his colleagues showed that SM was
unable to draw a face representing fear; she complained that “she did not know
what an afraid face would look like, and that she was unable to draw any depiction
of it,” even though she could depict other emotions. Moreover, tests also
showed that, although SM did not lack the concept of fear, she seemed to lack
the capacity to experience fear in a normal way, since she did not appear to feel
frightened given the appropriate stimulus. Damasio stated in his commentary
on the case that SM’s fearlessness, the result of bilateral damage to her
amygdalae, had prevented her from learning “the significance of unpleasant
situations that all of us have lived through. As a result she has not learned the
telltale signs that announce possible danger, especially as they show up in the
face of another person or in a situation.”

That this was true appeared to be demonstrated in yet another study.
The experiment called for SM and two other patients with complete bilat-
eral amygdala damage to rate one hundred slides of facial expressions, again
selected from Ekman and Friesen’s collection of such posed expressions,
slides that had previously been rated by a group of normal individuals as
indicating various degrees of “trustworthiness” and “approachability.” As
Damasio reported, these normal subjects had been asked an apparently sim-
ple question: “How would you rate this face on a scale of one to five, relative
to the trustworthiness and approachability that the owner of the face
inspires? Or, in other words, how eager would you be to approach the per-
son with this particular face if you needed help?” Fifty faces that were
judged by normal individuals as inspiring trust and fifty faces that were
judged untrustworthy were then shown to SM and the two other patients.
The experiment demonstrated that although the latter were quite capable of
judging trustworthy faces normally, they were severely impaired in their abil-
ity to judge untrustworthy or dangerous faces. As Damasio reported, they
“looked at faces that you or I would consider trustworthy and classified
them, quite correctly, as you and I would, as faces that one might approach
in case of need. But when they looked at faces of which you and I would be
suspicous, faces of persons that we would try to avoid, they judged them as
equally trustworthy. . . Immersed in a secure Pollyanna world, these individ-
uals cannot protect themselves against simple and not-so-simple social risks
and are thus more vulnerable and less independent than we are.”
Several comments are in order. The first point to notice is that in Damasio’s narrative of the case the concepts of “trustworthiness” and “untrustworthiness” have been stripped of all context in order to treat these traits as objective, identifiable features of persons that are immediately, universally, and unambiguously readable in the human face. The aim of Damasio’s and his colleagues’ experiment was not to assess the meaning certain facial expressions might have had for SM but to establish the difference between abnormal and normal subjects on the basis of an ostensibly objective standard. The posed facial expressions drawn from Ekman’s *Pictures of Facial Affect*, with which SM was tested, were taken to provide that objective standard because they came with a predetermined “correct” estimation of danger. The assumption underlying the research project was that dangerousness or what is frightening inheres unambiguously in certain facial expressions that everyone has the competence to decipher, because that competence is an evolved skill shared by everyone with normal brain function. Russell has shown that when forced-choice response formats and other problematic methods advocated by Ekman are abandoned, the results obtained fail to support the Tomkins-Ekman position on the universal recognition of facial expressions of emotion. Ignoring such criticisms, Damasio employed the Tomkins-Ekman paradigm in order to argue that SM can’t feel fear because she has an abnormal amygdala and therefore makes errors when judging the dangerous faces she encounters. Her emotional deficit is that she cannot conform to a fixed standard or norm.

What interests me here is the kind of scientific object fear is imagined to be when analyzed in these terms. The issue is not whether the amygdala plays a role in the fear response—it clearly does—but what kind of role it plays and how that role is to be conceptualized. Among the questions that arise are: Did Damasio and Adolphs make unwarranted assumptions in their analysis of the case? To what extent did their commitment to Ekman’s taxonomic approach to the emotions and their use of the latter’s pictures of emotional expression predetermine their findings? How specific is the amygdala’s role in the fear response? Does other work on the amygdala show that its part in fear processing is somewhat different from what Damasio and Adolphs first proposed?

The answer to the last question now appears to be yes. Subsequent studies of SM by Adolphs in collaboration with Russell (the same investigator who has queried the validity of Ekman’s cross-cultural judgment studies) and others now suggest that the amygdala is involved not in making categorical fear judgments but in something different, a judgment of arousal levels based on attention to features such as wide-open eyes. Tests have shown that SM fails to look normally at the eyes in all facial expressions and that if she is explicitly instructed to look at the eye region when performing an emotion detection...
task her recognition of fear is normal. Her selective impairment in identifying fear thus appears to be due to the fact that her defective amygdala is unable to direct her attention to the wide-open eyes that are thought to characterize the expression of fear. Furthermore, neuroimaging studies have revealed that humans presented with pictures of fearful faces do not report feeling “afraid,” yet amygdala activity is nevertheless altered, suggesting that reported emotion and amygdala activation should not be equated. The strong inference from these and related experiments is that, rather than functioning as the site for the production of discrete emotional states, such as fear, the amygdala modulates the vigilance and arousal levels required to attend to especially ambiguous stimuli of relevance to the organism (for example, fearful faces that provide information about the presence of a threat but not the source of that threat). At the very least these experiments suggest that the amygdala is not the locus of a discrete fear “entity,” but that both vigilance and emotions are processes set in motion by amygdala activation.84

On the basis of these newer findings, some scientists involved in research on the human amygdala have begun to question the general taxonomic-categorical approach to the emotions associated with the Tomkins-Ekman paradigm.85 But this is not a direction Damasio and Adolphs seem to want to pursue. In line with the newer findings about SM they have revised their understanding of the mechanism by which bilateral amygdala damage compromises the recognition of fear. But they do not appear to have altered their general commitment to the Tomkins-Ekman paradigm.86 I can think of several reasons for the continued success of that paradigm. Its ostensibly objective approach to the affects; its solidarity with evolutionary theories of the mind; the agreement between its assumptions about the independence of the affect system and cognition and contemporary presuppositions about the modularity and encapsulation of brain functions; the congruence between its image-based approach to the emotions and neuroimaging technologies such as PET (positron emission tomography) and fMRI (functional magnetic resonance imaging); the promise it holds for surveillance experts keen to find ways of detecting liars as easily as a blood test can detect DNA—all these and other factors help explain why the “basic emotions” view is so entrenched in contemporary thinking.87

Especially important in this connection is the convenience of Ekman’s methods in facilitating research. His photographs of posed expressions are so easy to use that even his critics continue to employ them in their own experiments—an extraordinary fact, when you think about it. Moreover, I sense that Ekman’s critics face another difficulty, which is that the moment one abandons the basic emotions approach in favor of some kind of intentionalist interpretation of the kind associated with Freud and appraisal theorists, one finds oneself forced to provide thick descriptions of life experiences of the
kind that are familiar to anthropologists and indeed novelists but are widely held to be inimical to science. At the same time, one is obliged to engage with an array of tremendously difficult questions about the nature of intentionality, including the intentionality of nonhuman animals, which have traditionally belonged to the domain of philosophy. Quite apart from those considerations, however, it is precisely Ekman’s non- or anti-intentionalism that makes his work particularly attractive at the present time, at any rate in certain quarters. For, once you imagine that emotions are nonintentional states that are simply triggered by various stimuli, and once you imagine that, as inherited patterns of response, under the right conditions they will inevitably express themselves on the face—which is what it means for them to be universal—you are likely to conclude that the inner truth about a person will be detectable by properly trained observers, which is to say, you will conclude that there is an important sense in which the body cannot lie. That is why since 9/11 Ekman’s research program has been of interest to American intelligence and security agencies. Whereas for critics of Ekman’s approach to the emotions such as Fridlund, it is not true that what is hidden in deception is destined to “leak out” from unmanaged behavior. Rather, humans and nonhuman animals produce facial behaviors or displays when it is strategically advantageous for them to do so and not at other times, because displays are dynamic and often highly plastic social and communicative signals. Deception is thus omnipresent in nature and potentially highly advantageous for the displayer, not something that covers over the hidden truth of authentic feelings. But this way of thinking introduces a degree of complexity and uncertainty that contrasts to its disadvantage with the reassuring idea that the truth of our emotions is bound to reveal itself.

Notes

My thanks to Jennifer Ashton, Colin Klein, Michael Fried, Robert B. Pippin, Jan Plamper, and James A. Russell for their helpful comments on my paper.

1. “In recent years and with the intensification of anti-terrorism efforts, there has been a clear desire to improve or replace the polygraph technology with something better. There is a real need for administering real-time, highly automated veracity tests inside the country and around the globe, where human intelligence is being collected. At the same time, similar technology may be useful for quick screening of potential suspects in ports of entry”; P. Tsiamyrtzis et al. (Paul Ekman is one of the six authors of this paper), “Imaging Facial Physiology for the Detection of Deceit,” International Journal of Computer Vision 71, no. 2 (2007): 198. For a popular presentation of the work of Paul Ekman and others on terrorist surveillance and lie detection see Robin Marantz Henig, “Looking for
the Lie,” New York Times Magazine, 5 February 2006. Ekman has recently served as scientific consultant to the Fox Broadcasting Company’s TV series, Lie to Me, featuring a leading deception expert who studies facial expressions and involuntary body language to discover not only if someone is lying but also why. In its Time 100 issue for 2009, Time magazine selected Ekman as one of the world’s one hundred most influential people.


3. The emotion field has benefited by inviting Charles Darwin’s writings on facial expression, but it has frequently misinterpreted his ideas. Darwin is typically thought to have proposed that expressions are selected and adapted for the communication of emotion. But as Alan Fridlund has pointed out, Darwin’s conclusion was the opposite: he believed that most facial displays were not evolutionary adaptations, but vestiges or accidents. Alan Fridlund, Human Facial Expression: An Evolutionary View (Cambridge, 1994), 15. For a valuable, nuanced discussion of this point see Thomas Dixon, From Passions to Emotions: The Creation of a Secular Psychological Category (Cambridge, 2003), 159–79.

4. Silvan S. Tomkins, Affect Imagery Consciousness (New York, 1962–63), 1:248. The recent rise of interest in Tomkins’s work in the humanities can be credited to the late Eve Kosofsky Sedgwick who, in Touching Feeling: Affect, Pedagogy, Performativity (Durham, NC, 2003), made the case for the value of Tomkins’s nonintentionalist (or materialist) ideas for theorizing affect. I offer a critique of Tomkins’s and Sedgwick’s ideas about affect in From Guilt to Shame: Auschwitz and After (Princeton, NJ, 2007), chap. 4.

5. Sigmund Freud, “The Analysis of a Phobia in a Five-Year-Old Boy” (1909) in The Standard Edition of the Complete Psychological Works of Sigmund Freud, trans. and ed. James Strachey (London, 1953–74), 10:149–289. For Freud, it is because Hans fears the powerful father’s retaliation against his own unmasterable hostility that he (the child) identifies with him to the point of incorporating the father’s aggression. That incorporated aggression fuels the child’s superego, so that the aggressivity of the scenario is played out in the mode of the subject’s experience of a relentless self-reproach. Aggressivity, fear, and guilt on this interpretation thus have a deep connection, one that is lost in modern interpretations of fear, such as those by Tomkins and Ekman.


7. I use the word “intentionalist” here to describe the position of Freud and all those in the emotion field who call themselves “appraisal theorists” or “cognitivists,” because the term “intentionalist” captures what I consider to be the most fundamental aspect of their position, namely, the idea that the emotions are to be understood as states of mind that are directed toward objects and that include cognitions, judgments, and beliefs about the world. “Cognitivism” has been criticized by philosopher of science Paul Griffiths, and others, for two basic reasons: (1) Cognitivism is held to be captive to a particular picture of cognition according to which it involves making propositions or holding “propositional attitudes,” thereby tying cognition to the human capacity for producing linguistic propositions; cognitivism thus appears to create a sharp divide between humans and nonhuman animals. (2) Cognitivism is accused of
ignoring developments in the life sciences, especially the neurosciences. See Paul E. Griffiths, *What Emotions Really Are: The Problem of Psychological Categories* (Chicago, 1997), 1–3. Both criticisms seem to me to miss the mark. There is no reason to deny some sort of capacity for cognition and intentionality to nonhuman animals, and, as I try to show in this paper, the reproach that philosophers tend to ignore the findings of the life sciences can serve to distract attention from the inadequacies of the scientific evidence on offer. The conflict between intentionalist and nonintentionalist accounts of emotion, including crucially the nonintentionalist views of Tomkins, Ekman, and others, is the topic of my recent book, *From Guilt to Shame*. See also Ruth Leys, “Navigating the Genealogies of Trauma, Guilt, and Affect: An Interview with Ruth Leys,” in *Models of Mind and Consciousness*, Special Issue, *University of Toronto Quarterly* 79, no. 2 (Spring 2010): 42–65, forthcoming.

8. As Tomkins wrote in his vision of the “humanomaton” or emotional “animal-machine”: “There must be built into such a machine a number of responses which have self-rewarding and self-punishing characteristics. This means that these responses are inherently acceptable or inherently unacceptable. These are essentially aesthetic characteristics of the affective responses—and in one sense not further reducible. . . . If and when the humanomaton learns English, we would require a spontaneous reaction to joy or excitement of the sort ‘I like this,’ and to fear and shame and distress, ‘Whatever this is, I don’t care for it.’ . . . There must be introduced into the machine a critical gap between the conditions which instigate the self-rewarding or self-punishing responses which maintain them, which turn them off, and the ‘knowledge’ of these conditions. The machine initially would only know that it liked some of its own responses and disliked some of its own responses but not that they might be turned on, or off, and not how to turn them on, or off, or up, or down in intensity”; Silvan S. Tomkins and Samuel Messick, *Computer Simulation of Personality: Frontier of Psychological Theory* (New York, 1963), 18–19.

9. Donald Nathanson, *Shame and Pride: Affect, Sex, and the Birth of the Self* (New York, 1992), 66. Griffiths likewise remarks: “The affect program phenomena are a standing example of the emotional or passionate. They are sources of motivation not integrated into the system of beliefs or desires. The characteristic properties of the affect program system states, their informational encapsulation, and their involuntary triggering, necessitate the introduction of a concept of mental state separate from the concepts of belief and desire”; Griffiths, *What Emotions Really Are*, 243. Or as he also states: “The psychoevolved emotions occur in a partially informationally encapsulated modular subsystem of the mind/brain. The processes that occur therein, the ‘beliefs’ of the system and the ‘judgements’ it makes, are not beliefs and judgements of the person in the traditional sense, any more than the ‘beliefs’ and ‘judgements’ of the balance mechanisms fed by the inner ear”; Paul E. Griffiths, “The Degeneration of the Cognitive Theory of Emotions,” *Philosophical Psychology* 2, no. 3 (1989): 298.

10. Because the affect programs are said to be located in primitive, subcortical parts of the brain that function independently of knowledge, they are held to bypass the slower cognitive pathways of the higher cortical centers and respond instead in a quick and mindless way to the innate or learned triggers that elicit them. Much recent work on the emotions has been devoted to validating such an approach to the emotions, which has also found its way into the popular

How Did Fear Become a Scientific Object and What Kind of Object Is It?
media. For example, in a recent article in *Newsweek* about how people respond to terrorist threats and other situations we find this statement: “The strongest human emotions are fear and anxiety. Crucial to survival, they are programmed into the brain’s most primitive regions, allowing them to trump rationality but not for rationality to override them”; “When It’s Head Versus Heart, the Heart Wins,” *Newsweek*, February 11, 2008, 35.


12. For Ekman’s account of the origins of his research on the emotions see his “Afterword: Universality of Emotional Expression? A Personal History of the Dispute,” in Charles Darwin, *The Expression of the Emotions in Man and Animals*, 3rd ed., with an introduction, afterword, and commentaries by Paul Ekman (Oxford, 1998), 363–93. (I shall be using this edition of Darwin’s *Expression* throughout this paper.) Ekman here states that at the start of his career he did not have a strong commitment as to whether or not the emotions are universal, but that his goal was to “settle the matter decisively. Margaret Mead later wrote that it was outrageous for me to have such a goal” (365).


14. In “What Are the Primary Affects?” (1964), reprinted in *Exploring Affect: The Selected Writings of Silvan S. Tomkins*, ed. Virginia Demos (Cambridge, MA, 1995), 217–62, Tomkins and his co-author Robert McCarter reported the results of their own experiments on the judgment of selected facial expressions, experiments carried out in order to test the thesis of universality of the emotions. This paper served as a blueprint for Ekman’s subsequent research. As the latter stated: “Tomkins . . . provided a theoretical rationale for studying the face as a means of learning about personality and emotion. He also showed that observers could obtain very high agreement in judging emotion if the facial expressions were carefully selected to show what he believes are the innate facial affects. . . . Tomkins greatly influenced myself and [Carroll] Izard, helping each of us plan our initial cross-cultural studies of facial expression. The resulting evidence of universality in facial expression rekindled interest in this topic in psychology and anthropology”; Paul Ekman, “The Argument and Evidence About Universals in Facial Expressions of Emotion,” in *Handbook of Social Psychopathology*, ed. H. Wagner and A. Manstead (London, 1989), 145.


19. “With exposure times in the tens of seconds, a ‘true’ rendering of emotional expression would have required the subjects to achieve naturally a representative emotional expression, then hold it unadulterated for the photographer to record on film. This could not be done reliably or with precision”; ibid., 177.

20. Ibid., 169–70.

21. Ibid., 177. However, Prodger continues this passage in a remark suggesting that Darwin was aware of the risk of theatricality or conventionalizing inherent in the situation of being beheld: “Darwin was concerned that an observer might taint a subject’s actions through his or her own reaction to the emotion displayed, and that an observer might read into his or her observations because of the circumstances of the occasion” (177).

22. Ibid.

23. Johann Caspar Lavater, Von der Physiognomik (1772), cited by Richard T. Gray, About Face: German Physiognomic Thought from Lavater to Auschwitz (Detroit, 2004), 49–50. In his book, Gray makes many illuminating observations about the ideals of immobility, stasis, and repose in Lavater’s physiognomic-observational strategies, ideals that likewise inform the Tomkins-Ekman paradigm—as if only the human form stripped of vital, intentional actions and movements can reveal the authentic, primordial essence of the natural human being.

24. Darwin justifies his study of expression in nonhuman animals by stating that “in observing animals, we are not likely to be biased by our imagination, and we may feel safe that their expressions are not conventional”; Darwin, Expression, 24. I take Darwin to mean not that animals are unaware of being seen, but that they are not aware of our specifically human interests and motives in observing them, a point caught by Prodger when he observes of Darwin’s statement that “animals are unaware of the scientist’s purpose, and are unlikely to modify their behavior in reaction to experimental observation”; Prodger, “Illustration as Strategy,” 149.

25. “Until a systematic means of coding facial behaviors had been widely accepted and such drawings as well as a large number of actual photographs and films have been scored, there is no way of knowing to what extent they represented fantasy or reality”; Paul Ekman, Wallace V. Friesen, and Phoebe Ellsworth, Emotion in the Human Face: Guidelines for Research and an Integration of Findings (New York, 1972), 50–51.

26. For Ekman’s denial that his approach bears any relation to the older physiognomic tradition see his “Duchenne and Facial Expression of Emotion,” in Duchenne de Boulogne: The Mechanism of Human Facial Expression, ed. and trans. R. Andrew Cuthbertson (Paris, 1991), 271. But Ekman’s fundamental assumption that we can draw a distinction between natural and cultural facial expressions is the physiognomic assumption. In a recent study, François Delaporte credits Duchenne with being the discoverer of the means of distinguishing the authentic from the feigned emotional sign—for example, the simulated versus the genuine smile—and appears to defend that distinction in terms similar to those of Ekman, whose work he cites. See François Delaporte, Anatomy of the Passions, ed. Todd Myers, trans. Susan Emmanuel (Stanford, CA, 2008). For a superb historical discussion of Duchenne’s work that critiques Delaporte’s interpretation of Duchenne’s project, see Stéphanie Dupouy, Le visage au scalpel: L’expression faciale dans l’oeil des savants (1750–1880), PhD thesis, Université Paris 1 Panthéon-Sorbonne, 2007. My thanks to the author for allowing me to read
her thesis, and to Lorraine Daston and Andreas Mayer for alerting me to its existence. The distinction between “false” versus “true” (or “Duchenne”) smiles, a favorite topic of Ekman’s, is the target of Alan Fridlund’s criticisms in his Human Facial Expression, 115–18, 152–55.

27. It is worth noting that Ekman tended to exaggerate the conflict between nature and nurture in pre-Tomkins thinking about faces. He painted his predecessors as extreme cultural relativists, but that portrait was somewhat of a distortion that served his ends.

28. “We believe that some of the impressions of cultural differences in affect display have arisen from a failure to distinguish adequately the pan-cultural elements from the circumstances governing the display of affects which are markedly influenced by social learning and vary within and between cultures. We believe that, while the facial muscles which move when a particular affect is aroused are the same across cultures, the evoking stimuli, the linked affects, the display rules and the behavioral consequences all can vary enormously from one culture to another”; Paul Ekman and Walter V. Friesen, “The Repertoire of Nonverbal Behavior: Categories, Origins, Usage, and Coding,” Semiotica 1 (1969): 73.

29. See Ekman’s early discussion of this issue in “A Methodological Discussion of Nonverbal Behavior,” Journal of Psychology 43 (1957): 141–49. Ekman recognized that still photographs “freeze” expressions by slicing them out of what is naturally a sequence of movements in time. But in response to complaints by Jerome Bruner and R. Tagiuri, who criticized the use of such photographs because, as Ekman quoted them as stating, “judgment based on a frozen millisecond of exposure is not representative of the type of judgment made in naturally occurring conditions,” Ekman argued that if the purpose of the study did not require a judgment of sequential behavior, then still photographs “may be useful for some research questions”; Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 49. He especially defended the use of posed still photographs for their savings in cost and ease of use, remarking that in the case of posed facial behavior, where the poser holds the pose for the camera, “a still will provide the same information as five seconds of film or videotape of the frozen position. Certainly, there is considerable evidence that the frozen few milliseconds of a still photograph can provide quite a bit of information” (49)—which was of course precisely the point at issue.

30. Tomkins’s theory of “blends” plays an important role here. He and Ekman assume the existence of a core set of affects whose combination (“blends”) produce more complex emotions, with the result that much of the time the face expresses an admixture of two or more of the basic emotions. Theirs is an atomistic, combinatorial approach that conceives of the emotions as made up of a limited number of distinct categories or units (or “kinds”) out of which the “higher” affects are built up. The approach has the advantage of explaining away any negative results in judgment studies as “confusions” on the part of observers owing to the existence in posed photographs of facial expressions of blends resulting from local, cultural conditions. For the argument about “confusions” see Tomkins and McCarter, “What Are the Primary Affects?” For a discussion and criticism of the applicability of the theory of blends to the whole domain of emotional phenomena see Griffiths, What Emotions Really Are, 101–2.
31. Paul Ekman, Wallace V. Friesen, and Silvan S. Tomkins, “Facial Affect Scoring Technique: A First Validity Study,” *Semiotica* 3 (1971): 37–53. Thus if we follow Ekman’s writings during these important early years when he was attempting to lay the foundations of a science of emotion, we find him arguing that the facial expressions in the photographs he employed in his experiments must have been free of cultural taint because they were universally recognized. At the same time he suggested that those facial expressions were universally recognized because they were free of cultural taint. Similarly, Ekman presented FAST as if it were consequent on the finding that photographs of posed expressions could be distinguished correctly by literate and preliterate observers. But there is an important sense in which observers from literate and preliterate cultures were capable of distinguishing these affects correctly because FAST (plus subjective intuition) had already predetermined the “correct” or representative posed facial expressions to be used in his judgment tests.

Ekman, Friesen, and Tomkins decided from the outset to develop FAST in terms of emotion categories (happiness, sadness, surprise, anger, disgust, and fear) rather than emotion dimensions (pleasantness-unpleasantness, active-passive, and so on). This decision was based on several considerations, including the claim that the cross-cultural research carried out by Ekman and Izard had suggested the existence of universal categories of facial expression. To develop FAST, Ekman, Friesen, and Tomkins divided the face into three areas (brows-forehead area; eyes-lids-bridge of nose; lower face, consisting of cheek-nose-mouth-chin-jaw). Lists of facial components within each of the three facial areas for each of the six emotions were then compiled. The authors based these lists on the past literature as well as on their own observations and intuitions. To specify the appearance of the relevant components, they obtained “visual-photographic” definitions of each item by requesting several actors to pose the facial movements within each facial area (not the emotions) after they were told and shown what to do with the face. FAST thus consisted of three sets of photographs, one set for each facial area.

The faces to be scored by FAST were selected on theoretical grounds to show only one of the six hypothesized discrete emotions from photographs of posed expressions used by investigators in the past (including most of those used by Ekman and Friesen in their cross-cultural studies), each of which had obtained at least seventy percent agreement among observers about the presence of a single emotion. These selected photographs—the aim was to obtain ten pictures of each of the six emotions, although this proved impossible for disgust and fear—were projected life-size onto a screen, which was masked to show only one facial area at a time, and trained scorers, using the relevant set of FAST items, scored them by selecting the FAST item that best matched the area in question. The results for reliability and validity were found to be “very encouraging” for predicting the recognition of posed faces, although FAST had relatively poor success with fear faces as compared to the other emotion-category faces, owing to the problem of blends. FAST’s applicability to spontaneous expressions was justified on the grounds that, since it had been shown by Ekman and Friesen that posed expressions could be judged as showing the same emotions across different cultures, posed expressions must have some intrinsic relation to spontaneous ones. “In light of this evidence,” the authors concluded, “it seems reasonable to assume that posed behavior differs little
from spontaneous facial behavior in form” (53). For a further discussion of FAST see also Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 114–19.

32. See for example Paul Ekman and Erika Rosenberg, eds., What the Face Reveals: Basic and Applied Studies of Spontaneous Expression Using the Facial Action Coding System, 2nd ed. (Oxford, 2005), where the issue of posing comes up frequently.


34. Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 106.


37. Ibid., 106.

38. Ibid.


40. Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 81.


43. Ibid.

44. Hunt, “Recent Developments in the Field of Emotion,” 261.

45. Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 94.

46. For example, Ekman insisted that it was important to make sure that adequate sampling procedures had been followed in selecting photos for use in experiments of this kind, and that the investigator did not rely on an atypical group of persons to pose expressions, such as those who, because they were “highly extroverted” or specially trained, were particularly good at producing what he continued to call “spontaneous” facial behavior—as if the problem of posing could be solved by avoiding the use of professional actors in experiments of this kind; ibid., 95.

47. Ibid., 106.

48. “For this is what Ekman has demonstrated. Given a limited range of semantically designated emotions—grief, happiness, anger, disgust, it is possible to persuade members of different cultures to produce simulations which are mutually intelligible between these cultures,” Mead wrote in “Margaret Mead Calls ‘Discipline-centric’ Approach to Research an ‘Example of the Appalling State of the Human Sciences,’ ” review of Darwin and Facial Expression: A Century of Research in Review, ed. Paul Ekman (New York, 1973), Journal of Communication 25, no. 10 (1975): 212. This left open the question why, if Ekman’s experimental findings could be trusted, everybody, regardless of cultural origin, could be “persuaded” to recognize such “simulated” or theatricalized expressions. Mead was prepared to entertain the idea that there must be some universal, presumably
innate, element involved, perhaps of the kind Ekman had postulated between the nervous system and specific facial muscles. Recent criticisms by James A. Russell and others of Ekman and Friesen’s methods in such cross-cultural studies have now cast doubt on Ekman’s universalist claims (more on this later in the essay). Ekman for his part expressed surprise that Mead was willing to suggest that “what is innate would be apparent only in simulation and not in actual emotional experience” and suggested that the answer must come from data on spontaneous expressions of the kind he had first reported in his hidden camera study of American and Japanese students. Paul Ekman, “Biological and Cultural Contributions to Body and Facial Movement,” in The Anthropology of the Body, ed. J. Blacking (London, 1977), 69.

49. For Ekman’s account of his difficult relations with Mead see also his “Afterword: Universality of Emotional Expression?” where he characterized Mead’s review of his 1973 book as a “denunciation” (364). Mead herself adopted a communicative analysis of facial expression, with a debt to cybernetics and kinesics, an analysis that appears closer to recent “paralanguage” theories of communication of the kind advocated by Fridlund than to Ekman’s basic emotions approach. Mead’s position on this topic was closely aligned with the work of the anthropologist Ray Birdwhistell (1918–1994), who denied the existence of universals in facial and other bodily gestures and movements. Birdwhistell emphasized the need to study body movement as a “language” or form of communication between people, using film and videotape as methods of recording in naturalistic settings and without experimental intervention or manipulation. Birdwhistell’s concern with methodology, especially the sensitivity of subjects to being observed, led him to rule out having the cameraman present when they were being filmed. As reported by Martha Davis, a member of Birdwhistell’s research circle, by the end of the 1970s, though, Ekman’s attempts to apply traditional experimental methods to the study of nonverbal behavior had gained traction, not least because the microstudies of behavior of the kind Birdwhistell advocated took enormous lengths of time compared to Ekman’s quicker, more economical procedures. Birdwhistell’s criticisms of Ekman’s methods, assumptions, and findings ended in the former’s defeat when Ekman replaced Birdwhistell in the decisive role of arbiter of the National Institute of Mental Health grants for research on nonverbal communication. Ekman’s triumph over Birdwhistell—a triumph at once methodological, intellectual, and institutional—sealed the destiny of emotion research in the United States for the next several decades. What is new today is that criticisms of Ekman’s approach are being raised by researchers who have been formed by and trained in Ekman’s presuppositions and research methods, and it will be interesting to see whether these critics will have any better success than did Birdwhistell in challenging and dislodging them. See Martha Davis, “Film Projectors as Microscopes: Ray L. Birdwhistell and Microanalysis of Interaction (1955–1975),” Visual Anthropology Review 17, no. 2 (Fall/Winter, 2001–02): 39–49.

50. “A limitation of these cross-cultural experiments is that the facial expressions presented were not genuine but posed by subjects instructed to show a particular emotion or to move particular facial muscles. One interpreter of this literature [referring to Mead] suggested that the universality in judgments of facial expression might be limited to just such stereotyped, posed expressions”; Paul Ekman and Harriet Oster, “Facial Expressions of Emotion,” Annual Review of
Psychology 30 (1979): 530. Again in 1987 Ekman acknowledged as one of three limitations of his cross-cultural studies the fact that “the facial expressions were posed, and Mead (1975) argued that establishing that posed expressions are universal need not imply that spontaneous facial expressions are universal”; Paul Ekman et al., “Universals and Cultural Differences in the Judgments of Facial Expressions of Emotion,” Journal of Personality and Social Psychology 55, no. 4 (1987): 713.


52. Ekman, “Biological and Cultural Contributions to Body and Facial Movement” (1977), 69. This statement and Ekman’s discussion of Mead’s criticisms were omitted when in 1980 Ekman republished his paper in Rorty, Explaining Emotions, 73–101. In his 1980 version he placed his criticisms of Mead in note 7, 100.

53. A few years earlier, Subincision had been used to measure the level of castration fear and anxiety it aroused in viewers; in Lazarus’s work the emphasis on castration largely disappeared and the film was treated instead as one among others (such as workshop accidents) depicting “bodily mutilation” that could be used to measure stress; in Ekman’s work the emphasis shifted yet again from the study of stress to the study of emotions. It is a sign of another cultural shift that when I recently attempted to locate a copy of this film through the Internet, I discovered that the term “subincision,” which had formerly referred to a somewhat esoteric Aborigine adolescent initiation ritual held to be deeply upsetting if not traumatic for Western viewers, now chiefly refers to a popular form of penile tattooing.


55. For a discussion of the use of this film by Lazarus and Mardi J. Horowitz to study stress and of the question of the image, violence, and appraisal in the development of the diagnosis of Post-Traumatic Stress Disorder, see Leys, From Guilt to Shame, 93–122.

56. Ekman, Friesen, and Ellsworth, Emotion in the Human Face, 163.


98 Representations


65. Fridlund, Human Facial Expression, 128.
66. Fridlund characterizes his position as a “paralanguage” theory of facial expression in order to emphasize that in humans the role of facial movements is not to express inner emotions but to accompany and supplement speech. Studies of “audience effects” by Fernández-Dols and his colleagues showing, for example, that Olympic gold medalists produce many facial expressions during the medal ceremony but smile almost exclusively when interacting with the audience and officials, have been held to confirm the situational-transactional character of facial expressions. Fernández-Dols and Ruiz-Belda, “Spontaneous Facial Behavior During Intense Emotional Episodes: Artistic Truth and Optical Truth,” 255–74. Fridlund also situates his analysis of facial expression in the context of the post-Tinbergen and post-Lorenz “new ethology,” which likewise emphasizes the communicative value of nonhuman animal displays. From the perspective of the new ethology, displays do not necessarily provide “readouts” of internal motivation because it would not be strategically advantageous for animals to automatically signal their future actions. In a large literature on animal displays, whose role in the vicissitudes of emotion research will have be taken into account in any future history of post–World War II theoretical and emotional approaches to the affects, see especially J. R. Krebs and R. Dawkins, “Animal Signals: Mind-Reading and Manipulation,” in J. Krebs and N. B. Davies, eds., *Behavioral Ecology* (Oxford, 1984), 380–402; R. Hinde, “Was ‘The Expression of Emotions’ a Misleading Phrase?” *Animal Behavior* 33 (1985): 985–92; and R. Hinde, “Expression and Negotiation,” in G. Zivin, ed., *The Development of Expressive Behavior: Biology-Environment Interaction* (New York, 1985): 103–16.


69. Fridlund, *Human Facial Expression*, 179. Fridlund observes that his performative analysis may be especially applicable to experiments by Ekman in which many
of the subjects were actors, and the directed facial actions were obtained using a “coach” (that is, a director) and a mirror (179 n. 6).


71. Ian Hacking, “By What Links are the Organs Excited?” Times Literary Supplement, July 17, 1998, 11. Hacking here points to the role played by unconscious demands and expectations in the production of facial expressions in terms that are compatible with Fridlund’s dramaturgical interpretation of facial displays as determined by (conscious and unconscious) social motives. Seen in this light, Ekman’s forced-choice labeling procedures, which oblige experimental subjects to identify emotions in photographs of posed expressions by selecting one label from a limited set of emotion terms, can be understood as methods designed to suppress the social-relational dimension involved in their making by “objectivizing” the emotions in the image and in the “correct” judgment of the objective truth of those representations. Russell has demonstrated that if subjects are encouraged to give a description of such pictures of expression in their own words, they don’t use categorical emotion terms but tend to produce a narrative account in terms of the situation they think or imagine is being enacted in the image. Russell, “Facial Expressions of Emotion: What Lies Beyond Minimal Universality?” 378–81.

72. FACS was modeled on an earlier coding system made by the anatomist C. H. Hjortso. FACS is a “sterile,” noninterpretive system that focuses on the visible changes (action units) caused by the underlying facial muscles. The “theoretical” part of Ekman’s coding system is EMFACS (Emotion FACS), which makes “predictions” (or pronouncements) about which emotions are expressed by which concatenation of action units. The risk has always been that the “predictions” in EMFACS will tend to get folded into FACS training, since Ekman believes that the truth of EMFACS has been definitively revealed. It is worth noting that the iterative process used by Ekman to select representative portrayals of emotions has not in fact yielded a specific physically characterizable signal for each emotion. Instead, for each emotion a range of signals achieves varying degrees of agreement. For example, in Ekman and Friesen’s Pictures of Facial Affect, sixty-five different facial signals for anger are specified, yet “no theoretical rationale for this variety has been offered”; James A. Russell, Jo-Anne Bachorowski, and José-Miguel Fernández-Dols, “Facial and Vocal Expressions of Emotion,” Annual Review of Psychology 54 (2003): 333.


76. R. Adolphs, D. Tranel, H. Damasio, and A. Damasio, “Impaired Recognition of Emotion in Facial Expressions Following Bilateral Amygdala Damage to the Human Amygdala,” Nature 372 (15 December 1994): 669–72. The patient was originally identified as “SM,” and this is how she is referred to in most subsequent publications, although in his book, The Feeling of What Happens, Damasio calls the patient “S.” I shall refer to her as SM.
79. Ibid.
80. Ralph Adolphs, Daniel Tranel, and Antonio R. Damasio, “The Human Amygdala in Social Judgment,” *Nature* 393 (June 1998): 470–73. See also Ralph Adolphs and Daniel Tranel, “Emotion Recognition and the Human Amygdala,” in *The Amygdala: A Functional Analysis*, 2nd ed. (Oxford, 2000), 587–630. In these experiments on SM, ratings of approachability and trustworthiness were analyzed for the fifty faces to which normal controls had assigned the most negative ratings, and for the fifty most positive faces. Subjects with bilateral amygdala damage rated the fifty most negative faces more positively than did either normal controls or patients with unilateral amygdala damage. All subject groups showed similar ratings to the fifty most positive faces. On the basis of the results, the researchers suggested that “the human amygdala triggers socially and emotionally relevant information in response to visual stimuli. The amygdala’s role appears to be of special importance for social judgment of faces that are normally classified as unapproachable and untrustworthy, consistent with the amygdala’s demonstrated role in processing threatening and aversive stimuli”; Adolphs, Tranel, and Damasio, “The Human Amygdala in Social Judgment,” 472.
82. As two recent critiques of Damasio’s report of the case of SM (or “S”) and other neuroscientific studies of the relationship between facial expression and emotion make clear, when they emphasize the bracketing or loss of the social-interpretive dimension of the emotions in Ekman’s standardized, image-based approach: Daniel Gross, *The Secret History of Emotion: From Aristotle’s Rhetoric to Modern Brain Science* (Chicago, 2006), 28–39; and John McClain Watson, “From Interpretation to Identification: A History of Facial Images in the Sciences of Emotion,” *History of the Human Sciences* 17, no. 1 (2004): 29–51. The latter rightly observes that FACS images “enable researchers to introduce reliability and consistency into their investigation but it is important to remember that these advantages are procured only by bracketing the conceptual quandaries so thoroughly detailed by earlier researchers” (45).
83. Already in 1997, in a discussion of the existing literature on the amygdala’s role in the evaluation of emotional stimuli, Elizabeth Phelps raised questions about some of the more extreme claims being made about amygdala-damaged patients, pointing out that such patients are different but that their social deficits are subtle and not as dramatic as had been suggested. She proposed that the amygdala seemed to be activated whenever stimuli were arousing. E. A. Phelps and A. K. Anderson, “Emotional Memory: What Does the Amygdala Do?” *Current Biology* 7 (1997): R312–R314. See also Adam K. Anderson and Elizabeth A. Phelps, “Is the Human Amygdala Critical for the Subjective Experience of Emotion? Evidence of Intact Dispositional Affect in Patients with Amygdala Lesions,” *Journal of Cognitive Neuroscience* 14, no. 5 (2002): 709–20; and Elizabeth A. Phelps, “Emotion and Cognition: Insights from Studies of the Human Amygdala,” *Annual Review of Psychology* 57 (2006): 27–53.
84. In a large and rapidly expanding literature, see esp. Ralph Adolphs, James A. Russell, and Daniel Tranel, “A Role for the Human Amygdala in Recognizing

85. See especially Paul J. Whalen et al., “Masked Presentations of Emotional Facial Expressions Modulate Amygdala Activity without Explicit Knowledge,” *The Journal of Neuroscience* 18, no. 1 (1998): 411–18; M. Davis and P. J. Whalen, “The Amygdala: Vigilance and Emotion,” *Molecular Psychiatry* 6 (2001): 13–34; Paul J. Whalen et al., “Human Amygdala Responsivity to Masked Fearful Eye Whites,” *Science* 306 (2004): 2061; and Paul J. Whalen, “Fear, Vigilance, and Ambiguity: Initial Neuroimaging Studies of the Human Amygdala,” *Current Directions in Psychological Science* 7 (1998): 177–88, where Whalen observes with reference to Phoebe Ellsworth’s appraisal views that “vigilance and emotion are not so much entities that reside within the amygdala as they are processes set in motion by amygdala activation” (183) and concludes: “Most theories of amygdala function, and thus discussions of amygdala neuroimaging results, highlight its role in the production of fear states. Clear animal and human experimental evidence supports this view. Yet such a view may unnecessarily compartmentalize amygdala function, leading us to a categorical understanding of what may be a continuous system. The utility of invoking a process such as vigilance rather than an entity such as fear to explain amygdala function is that it might speak to a greater portion of our daily experience. . . . The present theory offers a more pervasive and generalized role for the amygdala in vigilance evoked by associative ambiguity to
explain its response to a wide variety of biologically relevant stimuli” (185). In a recent paper, Whalen draws attention to new experiments suggesting that the stimuli to which the amygdala responds do not even have to be biologically relevant to the organism, but that uncertainty or unpredictability is enough to activate the amygdala. Paul J. Whalen, “The Uncertainty of It All,” Trends in Cognitive Sciences 11 (2007): 499–500.

86. I am not sure where Adolphs now stands on these issues. For a recent statement of his position see Ralph Adolphs, “Perception and Emotion: How We Recognize Facial Expressions,” Current Directions in Psychological Science 15, no. 5 (2006): 222–26, where the author accepts the idea that the amygdala’s role in recognizing facial expressions encompasses a broader, more abstract, or perhaps more dimensional rather than categorical aspect, of the emotions, of which fear is only one instance. But this leaves open the possibility that he is still committed to the idea of the existence of a set of “basic emotions” according to the Tomkins-Ekman paradigm. Cf. Ralph Adolphs, “Fear, Faces, and the Human Amygdala,” Current Opinion in Neurobiology 18 (2008): 166–72. For a relatively recent statement of Damasio’s general position on the affects see his Looking for Spinoza: Joy, Sorrow, and the Feeling Brain (New York, 2003).

87. The compatibility between Ekman’s hypostatization of the facial image and the hypostatization of the image in new imaging technologies such as PET and fMRI is striking. In this connection see Joseph Dumit, Picturing Personhood: Brain Scans and Biomedical Identity (Princeton, 2004), in which the author suggests that PET and fMRI reinforce the notion that we are what our brain images tell us we are.

88. In his novel, Your Face Tomorrow, vol. 1, Fever and Spear, trans. Margaret Jull Costa (New York, 2005), Javier Marias offers a fascinating fictional account of what might be involved in the reading of faces and facial behavior. Indeed, it is striking that this topic is thematized in his book at a time when, post 9/11, these issues are of such urgent concern in contemporary society and psychology. Suf- fice it to say that Marias’s position is entirely on the side of narrative thick description and the observation over time of multiple aspects of human behavior, not just the observation of isolated faces, much less static depictions of these.

89. Fridlund, Human Facial Expression, 137–39. I leave for discussion on another occasion the general question of the role of deceit versus veridical signaling in animal communication. Such a discussion would have to assess the importance of Amotz Zahavi’s “Handicap Principle,” according to which animal signals to be effective they must be reliable (that is, believable because truthful), and to be reliable they must impose a cost or handicap on the signaler. Thus, according to the Handicap Principle, when a gazelle sights a wolf and jumps high into the air before fleeing it is signaling that it is in top condition, easily able to outrun the predator, and that therefore the wolf should attack another animal. Zahavi suggests that for the gazelle to jump high in this way costs the animal significant energy and as a signal of fitness it is not one that can easily be faked. Zahavi extends the Handicap Principle to many aspects of human behavior in ways that seem to me too simplistic. See Amotz and Avishag Zahavi, The Handicap Principle: A Missing Piece of Darwin’s Puzzle (New York, 1997).