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Prolegomenon to the Structure of Emotion: Gleanings from Neuropsychology

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This article presents a model of the structure of emotion developed primarily from a consideration of neuropsychological evidence and behavioural data which have bearing on neuropsychological theories. Valence is first considered and highlighted as a defining characteristic of emotion. Next, the use of facial behaviour and autonomic nervous system patterns as defining characteristics of discrete emotions is questioned on empirical and conceptual grounds. The regulation of emotion is considered and proposed to affect the very structure of emotion itself. If there is an invariant pattern of biological activity across different instantiations of the same emotion, it is likely to be found in higher-order associative networks of central nervous system activity, the very same networks that subserve goal-directed behaviour and other cognitive functions. Drawing upon evolutionary considerations, it is argued that what is basic about emotion are the dimensions of approach and withdrawal. The nature of the linkage between such action tendencies and emotion is discussed.

INTRODUCTION

The purpose of this article is to begin to sketch a model of what we know about the structure of emotion. A sizeable portion of the evidence from which I draw is neuropsychological or psychophysiological. The analysis I present also makes liberal use of non-biological evidence, but considers the relevance and import of this corpus of data for biological theories of

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emotion. I first consider the role of valence in emotion and highlight this as a defining characteristic of emotion. I then consider the ways in which emotions differ from one another. Specific questions are raised about the utility of using facial behaviour and autonomic nervous system activity as defining characteristics of specific emotions. The issue of emotion regulation and its relevance for understanding the structure of emotion is considered next. Here, I ask the question of *whether* and *what* invariance is preserved across different instantiations of the same emotion when different regulatory processes are invoked. In other words, is there a basic emotion structure that is preserved irrespective of the nature of the regulatory manœuvres called into play to cope with the emotion? Although we clearly do not have the requisite evidence at this time to firmly answer this question, I introduce a variety of data which suggest that at least for expressive behaviour and autonomic physiology the answer is likely to be no. The possibility that certain components of central nervous system functioning are invariant across different instantiations of the same basic emotion is considered and the level at which such invariance might occur is described. Finally, drawing upon evolutionary considerations, I argue that what is basic about emotion are the dimensions of approach and withdrawal. The role of these action tendencies in different emotions is considered and the nature of the linkage between the action tendency and the emotion is described.

I wish to note at the outset that I am focusing upon data and theory which bear upon the actual production of emotion. Evidence relating to the perception of emotional information, although interesting and important in its own right, is in most cases irrelevant to a consideration of emotion production. Moreover, we know from an extensive literature that the neural circuits implicated in the perception of emotional information are different from those required for the production of emotion (see Davidson, 1984, for a review).

The Role of Valence in Emotion

Many theorists have suggested that emotion must be valenced, that a given emotion must either be positive or negative in hedonic tone (e.g. Ortony & Turner, 1990; Woodworth & Schlosberg, 1954). It is instructive to consider the emotion surprise because it raises critical problems for the valence criterion and suggests a related, but alternative view. As Ortony and Turner (1990) have noted, surprise appears to be an emotion that is positively valenced in certain situations (e.g. "John was surprised to learn that he won the award") and negatively valenced in others (e.g. "John was surprised to learn that his tumour was malignant"). As a consequence of this, Ortony and Turner (1990) question whether surprise should be considered an emotion.

In general, I agree with those theorists who suggest that affective valence is a characteristic of emotion. However, I prefer to describe this dimension as an approach/withdrawal one rather than as positive/negative. The reasons for this will be elaborated in a later section. For the present purposes, let me underscore that the fact that approach and withdrawal are, in an important sense, the fundamental psychological decision an organism will make in relation to its environment. Approach and withdrawal behaviour are seen at every level of phylogeny where behaviour itself is present. The over-arching importance of approach and withdrawal was clearly underscored by the pioneering syntheses of the great comparative psychologist, Schneirla (1959), who illustrated its fundamental importance for promoting adaptive responses in situations of survival relevance.

In this context, and in general agreement with Ekman's (this issue) intuition, I regard the emotion of surprise as an approach emotion. Surprise is often associated with an orienting response (Donchin, 1981) and many theorists have suggested that an orienting response reflects a "call for further processing" (e.g. Ohman, 1979). In this sense, then, surprise would arise in contexts where the organism requires additional information prior to a final decision about appropriate action. Because of this, surprise is probably an emotion which is more likely than most to be followed by another emotion, as soon as the organism processes the additional information required to resolve a discrepancy. According to this analysis, at the *moment* of its occurrence, surprise is an approach emotion. It is associated with an accentuation of information processing to resolve a discrepancy. However, it is likely that in certain contexts, it is quickly replaced by a negative emotion if the additional information warrants such a response.

This analysis highlights the dynamic temporal unfolding of emotions. It also implies that retrospective self-report, the primary source of evidence for many emotion researchers (e.g. Ortony & Turner, 1990), is simply incapable of resolving the rapidly shifting, often preconscious changes in emotion that occur in response to real world challenges. Just as new methods were developed in cognitive psychology to address processes that are automatic and inaccessible to conscious awareness, so must new methods be utilised in the emotion domain for the very same reason.

In What Ways do Emotions Differ?

Like any other natural phenomenon, the topic of emotion has attracted a variety of taxonomic schemes designed to parse the terrain into meaningful units. A major impetus for this Special Issue is the continuing controversy over so-called "basic emotions". A number of theorists have proposed that complex emotions are comprised of different combinations of elemental or basic emotions. This colour palette scheme has been a dominant one in the theoretical writings of a number of discrete emotion theorists, most notably Izard (1972). Ekman (this issue), in his most recent statement on the topic, suggests that basic emotions share nine distinguishing characteristics. The first four, which Ekman regards as the most robust, include: (1) distinctive universal signals; (2) distinctive physiology; (3) presence in other primates; and (4) universal, distinctive antecedent events. Different instantiations of the same basic emotion are said to preserve the invariant characteristics of distinctive signals, distinctive physiology, and distinctive antecedent events.

Although aesthetically appealing, I wish to raise several questions about this general approach and suggest that nature does not conform to this idealised scheme. I will argue that there is considerable within-family variability in the response systems which specify emotion. Furthermore, I will suggest that different emotions may have differential access to certain response systems. If this is so, it further complicates the notion that each discrete emotion is associated with a relatively invariant pattern of expressive behaviour and physiological activity.

It is instructive at the outset to consider the sources of evidence that have most often been used to argue for the presence of discrete emotions. The most convincing and substantial evidence is from studies on facial expression where Ekman (e.g. Ekman & Friesen, 1971; Ekman, Sorenson, & Friesen, 1969) and others (Izard, 1971) have demonstrated that observers clearly choose the "appropriate" facial expression when asked to pick the one that displays a particular emotion. Moreover, if told a story with a universal theme (e.g. loss) and asked to choose an emotional face that depicts the most likely emotion experienced by the story's protagonist, subjects throughout the world choose the correctly matching facial expression (e.g. sadness). Finally, if subjects are asked to show how their face would appear if they were the protagonist in an emotion episode, New Guineans pose expressions which are judged moderately accurately by US college students. Ekman (1972) reported that the percentage of correct judgements ranged from a high of 73% for happiness to a low of 18% for fear. The mean level of accuracy across six emotion situations was 47.2%. This corpus of evidence establishes that certain facial expressions are associated with particular emotions and that this association is indeed universal. What such studies do not address, however, is whether such prototypical expressions invariably mark the occurrence of the emotions they denote when such emotions occur spontaneously. Even though Ekman (1972) has studied the spontaneous display of happiness and disgust expressions in the United States and Japan, virtually nothing is known about the differential incidence of displaying other facial expressions in contexts where those emotions are reported. For example, when subjects report experiencing fear, what is the likelihood of them expressing a fear face compared with the expression of a disgust face when reporting disgust?

A recent study performed in my laboratory addresses this issue and raises a fundamental question about whether emotions may differ in their likelihood of being expressed on the face. In this study, we (Tomarken & Davidson, in prep.) unobtrusively videotaped subjects' facial behaviour while they viewed short emotional film clips designed to elict happiness, disgust, and fear. The film clips were carefully selected based upon extensive prior ratings from several hundred subjects. I will focus here on the responses to the fear and disgust clips. These clips (two per emotion) were chosen to be matched on self-reported intensity of the target emotions (M = 6.30 and 5.39 on disgust for the two disgust clips; M = 6.44 and 5.40 for the two fear clips, on 9-point scales). The intensities of the non-target emotions in response to these clips was also matched. For the disgust clips, the emotion reported more intensely after disgust was fear; similarly, for the fear clips, disgust was the second most intense emotion reported. The mean fear rating for the disgust clips was 3.31, and the mean disgust rating for the fear films was 3.10. In addition, the variance of the ratings in response to the disgust and fear film clips was matched. A total of 86 subjects were exposed to these clips in counterbalanced order. Subjects viewed the clips alone in a darkened room, while physiology was recorded. The facial behaviour was scored from the videotapes with Ekman and Friesen's (1984) EM-FACS system. This system is designed to code the facial actions which comprise emotional expressions and allows for the derivation of the frequency and duration of specific discrete expressions. We simply tabulated the percentage of subjects showing facial signs of disgust in response to the disgust clips and facial signs of fear in response to the fear clips. We found that 62% of the subjects showed at least one disgust expression in response to either of the two disgust film clips. The mean number of disgust expressions displayed by subjects (who had at least one expression) to the disgust clips was 2.58, with a range from 1 to 8 expressions. In response to the fear clips, ***not a single subject showed a facial expression of fear or questionable fear*** according to the EM-FACS criteria. If we just use the most common element of the facial prototype of fear, eye widening, 8.5% of subjects show this action unit [either alone or in combination with any other action unit(s)] in response to the fear clips. If we adopt the most inclusive possible facial criteria and determine the percentage of subjects who show any action unit (either alone or in combination with any others) that is part of the fear prototype (i.e. eye widening, lips pulled back, eyebrows raised), a total of 18% display such behaviour. It is important to underscore the fact that subjects' reported experiencing fear as intensely in response to the fear clips as they reported experiencing disgust in response to the disgust clips. Thus, using self-report criteria, they were experiencing the relevant target emotion in each case. However, the facial responses to these stimuli differed dramatically.

One objection that might be raised about this evidence is that film clips may simply be incapable of eliciting "genuine" fear. The film clips we selected to elicit fear did contain vivid depictions of threats of bodily harm, an antecedent event proposed by a number of theorists to be universally associated with fear (e.g. Lazarus, 1991). However, the subjects viewing the videoclips clearly were not threatened in this way.

To address this issue, we performed another study with small animal phobics (Tomarken & Davidson, in prep.). Such subjects report intense fear to their phobic object. In the context of an exposure treatment protocol, we exposed phobics (who met DSM-III-R criteria for simple phobia) to their phobic object *live* (either a boa constrictor or a tarantula) while their facial behaviour was again videotaped unobtrusively. All of our phobic subjects in this context reported being threatened with imminent physical harm. Here again, despite their self-reports indicative of intense fear, we rarely observed facial signs of fear. However, we did see a large percentage of subjects displaying facial signs of disgust to the phobic stimuli. Thus, in both a normal and a clinical sample, we found that when subjects are exposed to stimuli which they rate as eliciting moderately to extremely intense fear, little evidence of facial signs of fear are observed. Disgust, however, is much more likely to be displayed on the face.

Another objection that might be raised about this body of evidence is that it has been obtained from adults who have been socialised to mask their expressions, even in situations where they believe they are not being observed. What about fear expressions in infants? The literature on this point is again consistent with that obtained in adults. When young infants are exposed to classes of stimuli that are expected to elicit fear (e.g. placement over the deep side of visual cliff), they show behavioural signs of wariness (e.g. lack of approach) and autonomic signs generally believed to be consistent with fear (e.g. high heart rate), but they show little evidence of facial signs of fear (see reviews by Camras, this issue; Campos et al., 1983; Camras, Malatesta, & Izard, 1991).

These findings in diverse populations suggest that certain forms of fear exist that are not associated with the prototypical facial expression of fear. Although not readily testable, one obvious hypothesis which presents itself is that there may be adaptive advantages to not displaying fear on the face because its expression may signal vulnerability to a predator. It must also be noted that full, protypic facial expressions of fear probably do occur,

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but rarely in the laboratory. When the intensity of the frightening stimulus reaches a certain level, all available response systems may be recruited. The notion that different response systems may become engaged at different levels of affect intensity is consistent with recent findings of Cacioppo (see Tassinary & Cacioppo, 1992). At low levels of stimulus intensity, he observed differentiation between positively and negatively valenced stimuli in recordings of the electrical activity of muscles (but a relative lack of differentiation for discrete expressions). These findings suggest that facial differentiation of emotion varies directly with the intensity of the elicited emotion. Interestingly, the differentiation between positively and negatively valenced stimuli at low levels of stimulus intensity was not observable in recordings of autonomic activity.

In future research, it will be important to ascertain whether the lack of recruitment of particular response systems at low levels of affect intensity is a function of different gain thresholds present in the different response systems, or rather different patterns of efferent signals to the various effector systems at different stimulus intensities. This question is very important theoretically because if it is found that the pattern of efferent commands to the various effector systems maintain their differentiated activity and specify different discrete emotions, the relative lack of emotion-specific patterning in particular instances would be attributed to effector-specific (i.e. local) threshold and sensitivity factors. According to this view, different effector systems have different gain requirements and only become recruited when the input gain reaches a certain level. Lack of consistency across response systems may arise as a function of variability among effector systems in the gain requirement parameter.

An alternative view holds that the very pattern of efferent outflow to the effectors varies with intensity. Such a view implies that as the "same" emotion at different intensity levels is associated with a different pattern of efferent responses, the central physiology is not invariant across different intensities within an emotion. In fact, for certain components of physiology, the variability within a family may be as large as the variability between emotion families. The empirical question must be to specify precisely what is invariant across the many different instantiations of an emotion within a family. Of the first four characteristics which Ekman (this issue) has proposed are shared by each of the basic emotion families, only the first two, expressive signals and physiology, have been extensively evaluated. Certainly, the question of distinctive appraisals has received a considerable amount of research attention, but as Ekman (this issue) has noted, this research is mostly based upon the retrospective recollections of subjects about processes that occurred during prior emotional states, or that might occur during hypothetical emotional episodes. In the light of the automatic

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and often unconscious nature of appraisal, it is highly unlikely that retrospective self-report methodology is capable of addressing this question.[1]

For the first characteristic expression, I have tried to raise questions earlier about the degree of isomorphism between expression and emotion and to suggest that the package is indeed not a tidy one. There are many other questions of a similar nature that might be raised about expression, but an exhaustive cataloguing would go beyond the purpose of this paper. I wish to note only one other complication in the view that different discrete emotions have distinctive facial expressions. The complication has to do with the question of whether there are different types of positive emotion which may be as different from one another as negative emotions are among themselves. It seems that there is only one basic form of positive facial expression. Thus, if there are different types of positive emotion and if each is marked by the same expression, then the first characteristic shared by emotion families is violated. Ekman (this issue) clearly acknowledges this point and asks how crucial it is that each basic emotion share every one of the characteristics predicted to define such emotions. However, more important than the taxonomic question is the issue of meaningful differentiation within the positive emotions. We have suggested that one important distinction within the category happiness may be the degree to which it is associated with an approach action tendency. We have therefore distinguished between approach and non-approach happiness and have proposed that each is associated with distinctive central nervous activity (Davidson et al., 1990). Contentment and amusement may represent positive affective states that contain no approach component. Unambiguous "approach-happiness" is observed when 10-month-old infants observe their mothers walking toward them and smiling. Such infants show frank signs of approach action (e.g. reaching toward mother) and dramatic changes from baseline in brain activity (e.g. Fox & Davidson, 1988). The wrinkle is that each of these different forms of positive affect appears not to have a distinctive universal facial expression.

Consideration of the question of whether different emotion families are associated with distinctive patterns of physiology underscores the complexity of this problem and further highlights the substantial variability we can

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[1] In a landmark paper on the nature of verbal reports as data, Ericcson and Simon (1980) specifically delineate the conditions in which verbal reports might provide veridical information about ongoing mental activity. They clearly note that automatic processes are relatively unavailable to verbal report. If such processes are rendered conscious, it is likely that significant transformations of the "heeded" information take place. In most contexts where subjects are asked to introspect about automatic processes, the verbalisations they produce reflect their a priori theories or knowledge about the information requested (Nisbett & Wilson, 1977).

expect to find *within* emotion families. I will address several issues as examples, but do not intend for this to be an exhaustive review of the problem.

In considering the physiology associated with emotion, it is essential to distinguish between peripheral and central physiology, as well as among different components of the latter. The autonomic responses associated with emotion presumably occur to facilitate the action associated with the emotion. Thus, Levenson, Ekman, and Friesen (1990, p. 379) suggest that autonomic changes associated with different discrete emotions "produce patterns of activity that will support the behavioral adaptations and associated motor programs that are most likely for that emotion". Levenson et al. (1990) ask but do not answer the question of whether the same emotion accompanied by different action tendencies is likely to be associated with an invariant or differing pattern of autonomic activity. Fortunately, other relevant data may be brought to bear on this problem.

Kalin and Shelton (1989) studied different behavioural manifestations of fear in infant rhesus monkeys and found that certain fear-related behaviours (e.g. freezing) are selectively reduced by administration of a benzodiazepine, whereas other fear-related behaviours are selectively reduced by administration of morphine (e.g. vocalisation). These findings underscore the differentiated nature of the various subcomponents of fear and suggest that different patterns of central neurochemistry underlie distinct behavioural components of fear.

Unfortunately, there is a paucity of literature reporting on research with humans that has direct bearing on this important question. However, there are other animal studies which are relevant, although they certainly were not originally designed to address this question. Almost 20 years ago, Goesling and Brener (1972) reported on a study that was designed to address a critical problem in the literature at that time on operant conditioning of heart rate. The problem was whether heart rate control was "somatically mediated". Goesling and Brener (1972) trained one group of rats to run (in a running wheel) in response to a particular discriminative visual stimulus and to remain still in response to a second visual cue. In another phase of the experiment, the animals were administered curare, a peripheral neuromuscular blocking agent. Thus, while under the influence of curare, the animals were paralysed: They received one of the two discriminative stimuli that had been previously paired with either running or remaining still. Then, the animals received training to either increase or decrease their heart rate using standard operant techniques. They found that when the rats were exposed to the stimulus previously paired with running, they showed large magnitude increases in heart rate compared with exposure to the stimulus previously paired with remaining still, even though the animals were unable to move at all under the influence of curare. This suggests that the cardiac-somatic linkage was central and did not depend upon the overt expression of the action. Interestingly, the operant training failed to override the massive effects of the discriminative stimulus on heart rate.

Although this study did not involve emotion in any way, it does underscore the potency of autonomic changes which are action-specific. From this study, one would certainly expect that a fear state accompanied by freezing would differ from a fear state associated with fleeing; just as the rats exposed to a stimulus previously paired with running showed much higher heart rates than animals exposed to a stimulus previously paired with freezing. Of course, this is only a prediction and the requisite empirical studies have not been performed. However, if the predicted finding did emerge, it would seriously call into question the idea that a *single* pattern of autonomic activity remains invariant across different manifestations of the same emotion.

In this discussion, I have addressed the first two characteristics that Ekman has suggested are shared by basic emotions—distinctive universal signals and distinctive physiology. Of course, it should be apparent that signals other than the face are likely to be important and that failure to differentiate among emotional states on the basis of facial expression does not imply that such states do not have distinctive, universal signals of another type (e.g. vocalic expression). Also, the fact that different forms of the same emotion are likely to differ on certain parameters of physiology does not indicate that other aspects of physiology will not be found which are invariant across different types or expressions of the same underlying emotional state (e.g. fear associated with fleeing vs. freezing; anger associated with striking out vs. withdrawing; happiness associated with approach vs. non-approach, etc.). My comments are meant more to raise questions than to provide answers, to underscore where future research must be directed and not to draw firm conclusions on the basis of extant data. I have not considered the third and fourth characteristics that have been proposed to be shared among the basic emotions because of the paucity of available data in the case of primate emotion and the methodological limitations of the evidence that is available on the issue of appraisal.

The empirical challenge that confronts us is the specification of reliable patterns of coherence across multiple response systems. If a 10-month-old infant exposed to the deep side of the visual cliff reliably displays a cardiac acceleration and behavioural signs of withdrawal in the absence of a protypical fear facial expression, it would seem that such a state would have a status equal to that of any other emotion. The name we assign to that state is not the issue. In fact, it is likely that the very act of assigning a name to the state obfuscates rather than clarifies, as we tend to unwittingly assume that states given the same name are indeed the same state (see

Kagan, 1988). What is important is the establishment of repeatable patterns of multi-systemic coherence in response to well-defined incentive conditions. Only through such efforts will it be possible to identify which, if any, components of the emotional response remain invariant across different instantiations within an emotion family.

It is instructive in this regard to consider other characteristics which some theorists propose as the basis of the invariant features for different emotions. Stein and Trabasso (this issue) underscore the importance of higher-order goals associated with different emotions and propose that such goals remain invariant across different instantiations of basic emotions. If they are correct, then it is likely that the patterns of central physiology which might retain their invariance within an emotion family reflect activity in brain circuits associated with the formulation of goals and plans. Such a possibility is consistent with our emphasis on the importance of frontal cortical function in emotion (e.g. Davidson, in press), as the dorsolateral frontal cortex has been directly implicated as the critical structure in the mediation of such behaviour (Fuster, 1989; Nauta, 1971). This issue will be considered in more detail in a subsequent section.

Emotion Regulation: Can it be Disentangled from Emotion Structure?

Regulatory influences on the various components of emotion begin early in development and continue throughout the life-span. Some theorists (e.g. Izard, 1990) have postulated a core emotion process that remains relatively invariant under the pressure of these influences. Contextual factors are held to influence display rules and coping, but the core emotion remains unchanged, a Platonic essence which exhibits deep universality. Although aesthetically elegant, I wish to take issue with this position and to suggest instead the inevitability of the core changing as much as the shell. The evidence that I will bring to bear in support of this position is derived from studies on emotion, as well as studies on neighbouring phenomena, such as cultural variations in pain sensitivity, as many of the most important questions on the psychobiological consequences of emotion regulation have not been asked, let alone answered.

I will consider two forms of emotion regulation, borrowed from the cross-cultural work of Shweder (1985). The first he termed the *communication question*. This refers to cultural differences in emotional expressivity. Some cultures reinforce the overt, full-blown expression of certain emotions whereas others place constraints on the expression of some emotions. The relevant question in the context of this article is the impact of such regulation on the structure of emotion. Where do such cultural influences exert their effect? Do they simply amplify or attenuate the motor expressive component? Are they associated with differences in the subjective intensity of the experienced emotion? Are they associated with different magnitudes of physiological response?

There have been a number of studies of ethnic differences in pain responsivity. Although some of the differences observed are undoubtedly in part genetic, it is also likely that experiential effects play some role. In a classic experiment, Sternbach and Tursky (1965) demonstrated ethnic differences in pain tolerance levels, which were paralleled by differences in autonomic responsivity. Women of Italian descent tolerated less shock and showed heightened electrodermal responses compared with women of Old American origin.

The data on ethnic differences in pain responsivity are relevant to the emotion domain for they suggest that cultural differences in the reinforcement of expressivity[2] will likely have both behavioural *and* physiological consequences, particularly if such influences are long-standing. If cross-cultural differences in the physiology of emotion are found, it would again pose challenges for the proposition that there are unique, invariant, and universal patterns of physiology which specify each discrete emotion. If certain action tendencies varied in their association with particular emotions across cultures, it would unquestionably be associated with different patterns of physiology.

The claim that culture can importantly modify the physiology of emotion is simply an extension of the more basic proposition that experience influences the brain. Since the experiments of Rosenzweig and his colleagues nearly 30 years ago (e.g. Rosenzweig, Krech, Bennett, & Diamond, 1962; see Diamond, 1988, for a review) on the effects of environmental enrichment on basic brain organisation, anatomy, and neurochemistry, we know that even at the level of gene expression, experiential determinants abound (see e.g. Kandel, 1983 for a more modern statement on this issue). On this view, biological processes are no closer to the genotype than are behavioural processes. And, therefore, it should come as no surprise that considerable variability may exist in the

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[2] It is also conceivable that such cultural differences in physiological reactivity to pain are, at least in part, a product of cognitive differences in reaction to the pain challenge between the ethnic groups. The Italian women might ruminate more in response to such stimuli compared with women of Old American origin. Such differences in rumination might amplify or attenuate both expressive and physiological responses to the painful stimuli. In the depression literature, individual differences in ruminative tendencies have been proposed to account for differences in the duration of depressive symptoms (Nolen-Hoeksema, 1991). Individuals who engage in ruminative responses and focus attention on their symptoms are likely to have longer depressive episodes than people who distract themselves from their symptoms. Similar regulatory influences are likely to occur in both the pain and emotion domains.

biological concomitants of emotion and be determined, in part, by the differing action tendencies with which the emotion is associated. Having said this, I must hasten to add that I view it as likely that certain emotions have hard-wired action tendencies that emerge early in ontogeny, but may be modifiable with experience. Also, the possible pairings of action tendencies with emotion is not random, but is likely to be biologically constrained in much the same way as has been demonstrated for preparedness in classical conditioning (e.g. Seligman & Hager, 1972). Thus, it is very unlikely that a strong approach action can be coupled with the emotional state of fear. Attempts at such pairing would likely change either the emotion itself, or the nature of the action tendency.

Another form of emotion regulation considered by Shweder (1985) is what he termed the *management question*. This refers to techniques and/or strategies utilised to deal with emotions that cannot be directly expressed. The issue most directly relevant to the theme of this article is the level at which such management occurs and the question of whether long-term habitual use of certain management strategies changes the very core of the emotional response. More specifically, do prohibitions on emotional expression change the physiological concomitants of emotion? As will be described below, the answer to this question depends upon the level at which the management occurs. The level, in turn, reflects the duration of time the management strategy has been practised.

I will again borrow here from the literature on pain, as the requisite studies in the emotion domain have not yet been performed. Several studies have been conducted to evaluate the claim that the adoption of certain meditative states can dramatically attenuate pain responsivity (e.g. Anand, Chinna, & Singh, 1961). Another, related literature (e.g. Hilgard, 1973, 1977) concerns the effects of hypnotic analgesia on behavioural and physiological responses to pain. Both meditation and hypnotic analgesia have been proposed as effective strategies for managing pain. However, it appears as if the level at which such management effects are exerted differs substantially between these two forms of self-regulation. Meditation has been found to significantly attenuate certain physiological responses to pain (e.g. Anand et al., 1961). Although hypnotic analgesia results in profound decreases in reported pain and objective increases in pain tolerance (i.e. subjects can withstand a painful stimulus for a longer duration of time), it has not been found to reduce the autonomic concomitants of pain (Hilgard, 1973, 1977). Thus, it appears that hypnotic analgesia in contrast to other forms of self-regulation, such as long-term meditation, change those components of pain responsivity over which we have the most direct voluntary control. Other, more automatic response components, such as physiological changes, remain unaltered. This is consistent with the findings on signal detection and pain responsivity under hypnotic analgesia where it has been found that alterations are produced in measures of response bias (β), but not in measures of sensitivity (d') (Clark, 1974). One important question raised by these data for the emotion domain concerns the differences between phasic and long-term regulation strategies.

Pennebaker and his colleagues (Pennebaker & Chew, 1985) have examined the effects of a short-term management strategy on one component of autonomic physiology—skin conductance. They tested subjects in a standard guilty knowledge paradigm (Lykken, 1959) where they had to deceive an experimenter. One group of subjects was instructed to specifically inhibit all overt expressive signs, and another group was not given specific instructions about the inhibition of expressive behaviour. Pennebaker and Chew (1985) found that the inhibit group displayed significantly higher skin conductance activity compared with the control group. The very act of inhibition exacted a biological cost reflected in increased skin conductance activity. An identical pattern of results was obtained in a recent study by Gross and Levenson (1991), where they found that when subjects were asked to suppress expressive signs of affect in response to a disgust-producing film clip, greater sympathetic activity was observed compared with a non-suppression control group.

These findings suggest that a short-term management strategy may actually accentuate certain physiological manifestations of emotion. What we do not know from such experiments is whether the long-term deployment of such strategies results in changes in physiology over time. Based upon the very limited corpus of available evidence, I offer two related hypotheses concerning the effects of expression management on the underlying psychobiology of emotion. The first hypothesis states that short-term (i.e. those which are only occasionally deployed for short periods of time) prohibitions on expression may not attenuate physiological changes that normally accompany emotion. In fact, for certain emotions, they may accentuate such changes (Pennebaker & Chew, 1985; Gross & Levenson, 1991). Such short-term changes are also likely not to have much effect on the neural activity which normally accompanies the emotion. The efferent patterning to various effector systems is likely to be intact in such cases. Inhibitory strategies are overlaid on and may potentially mask such effector changes, but the input to the effector systems is thought to be largely unchanged. In contrast to this is the second hypothesis which holds that prohibitions occurring early in life and persisting for a long period of time are likely to modify the very pattern of efferent commands normally associated with that emotion. In other words, long-term exposure to a cultural context requiring certain forms of management and inhibition of emotional expression will modify the underlying structure of the emotion (see also Shweder, 1985).[3] To evaluate these hypotheses, longitudinal research in different cultures which differ in prohibitions on expressivity is needed. Early in life, substantial universality in expression and physiology is expected. However, with development, significant departures from universality are predicted. On this view, the structure of emotion reflects an irrevocable mix of innate and experiential influences.

Carving Nature at its Joints: Approach/Withdrawal as the Basic Emotion-relevant Dimension

The analysis just presented questions the special status of emotions that putatively possess distinguishing characteristics proposed to denote basic emotions. Both expression and physiology were shown to be quite variable within an emotion family. Rather than characterise any particular emotion or set of emotions as basic, I wish to argue that approach and withdrawal are two dimensions along which emotions differ and these dimensions should properly be regarded as basic. They are basic principally because of their phylogenetic primacy. Organisms approach and withdraw at every level of phylogeny where behaviour itself is present. To approach or to withdraw is the fundamental adaptive decision in situations or conditions that have recurred during our evolutionary past (Tooby & Cosmides, 1990). In very primitive organisms with simple nervous systems, rudimentary forms of approach and withdrawal behaviour occur in the absence of any emotion. The very structure of the nervous system in such species precludes any reasonable ascription of emotion in the presence of approach and withdrawal behavioural states (see Schnierla, 1959, for examples) as they contain none of the circuitry found to be essential for the generation of emotional responses in vertebrates. Thus, it is important to distinguish between approach and withdrawal action *per se*, and approach and withdrawal behaviours that occur in the context of emotion. Over the course of evolution, approach and withdrawal action emerged prior to the appearance of emotions to solve adaptive problems in simple species.

As the nature of adaptive problems became more complex (for examples, see Tooby & Cosmides, 1990) and co-ordination among perceptual, cognitive, and action systems was required, emotions evolved and became associated with already established approach and withdrawal action sys-

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[3] It may be that long-term exposure to particular cultural constraints on emotional expression exert their effects indirectly through modification of certain cognitive processes such as appraisal and coping mechanisms, rather than by (or in addition to) direct effects via the inhibition of emotional expression. Indeed, certain technologies for emotion regulation, such as cognitive therapy for depression, are predicated on this view.

tems. The co-ordination and integration among these various systems required a convergence zone (Damasio, 1989) in the brain. According to current accounts of convergence zones, these brain regions contain codes to "bind" together information from widely distributed neural networks. In the case of an emotion arising in response to an external stimulus, perceptual information about the stimulus must be combined with the output of various specialised processing regions in the brain, including those that code valence, formulate action plans, and generate the requisite autonomic supports. This integration of multiple components of emotion depends upon "phase-locked coactivation of geographically separate sites of neural activity" (Damasio, 1989, p. 127). As Damasio (1989) notes, more complex combinatorial codes are inscribed in more anterior cortical zones. In the light of the extensive multi-system coherence apparent in emotion, the frontal lobes are likely the major site of the emotion convergence zone (for supporting evidence, see Davidson, in press, Fuster, 1989; Nauta, 1971).

Elsewhere (e.g. Davidson & Tomarken, 1989; Davidson, in press), I have dealt with the evolutionary significance of separating approach and withdrawal systems in each hemisphere. I have suggested that one effective way in which competitive interactions between response systems could be minimised is to separate them geographically in the brain. Hemispheric specialisation is perhaps the most effective geographical separation which has been achieved in vertebrate nervous system evolution. And, recent evidence of hemispheric specialisation in a number of different species consistently confirms the direction of the effects found in humans and indicates that lateralisation for approach- and withdrawal-related emotion is considerably more robust across phylogeny than is cognitive lateralisation (e.g. Denenberg, 1984). In fact, some investigators have implied that cognitive lateralisation in certain primate species, including humans, has emerged from the more basic lateralisation for emotion (e.g. Kinsbourne, 1978).

We (Sobotka, Davidson, & Senulis, in press) have recently completed an experiment in which we attempted to disentangle approach/withdrawal action components from the positive and negative emotional states with which they are usually associated. According to the model of the emotion convergence zone I just sketched, only during the actual production of emotion should distinct activation patterns in the frontal region be present. We had subjects play a video-type game in which they could win and lose money as a function of their performance. At the start of the experiment, they were given $5 and told that they could add to this amount or decrease the amount, depending on their performance. On certain trials they received a cue (an arrow in the up position) that denoted a potential reward trial. Four seconds following the cue, they received an imperative stimulus to which they were to respond as quickly as possible. If they responded sufficiently quickly, they received a monetary reward. If their response was slow, there was no change in their cumulative earnings. On other trials, the initial arrow was in the down position. On such trials, if they responded too slowly money would actually be taken away from them. If they responded quickly, there was no change in their earnings. We confirmed that such reward and punishment contingencies indeed produced marked changes in their emotional state during the playing of the game.

In addition to manipulating reward and punishment contingencies, we also manipulated the nature of the motor response subjects were asked to make in response to the imperative stimulus. On half the trials, subjects were instructed to make approach responses—their index finger was held above a button and they were instructed to make a button press when they saw the imperative stimulus. On the remaining trials, subjects were instructed to make withdrawal responses—their index finger depressed the button prior to trial onset and they were instructed to lift their finger off the button when they detected the imperative stimulus. The reward and punishment contingencies were fully crossed and counterbalanced with the movement instructions. Brain activity was examined in the four second period just prior to making the motor response.

The results indicated that approach and withdrawal responses (averaged across reward and punishment conditions) did differ in the lateralised pattern of brain activation that preceded the response, but only in the temporoparietal region and not in more anterior regions. Approach responses were associated with more relative left-sided activation in this region compared with withdrawal responses. When the reward and punishment conditions were compared, we confirmed what we had found on many previous occasions—reward trials were associated with more left-sided frontal activation compared with punishment trials, and the latter were associated with more right-sided activation than the former. We expected that the movement condition might interact with the reward and punishment contingencies. We did not find this to occur, largely because the effects of the reward and punishment contingencies were so large that they swamped any additional variance contributed by the rather subtle manipulation of movement.

The results of this experiment suggest that as simple a manipulation of approach and withdrawal responding as a finger press vs. a finger lift is enough to produce differential lateralised effects in posterior processing regions. This finding suggests that approach and withdrawal action plans themselves, stripped of their usual pattern of associated emotion, are segregated into different hemispheres. However, recruitment of the frontal convergence zone occurs only in response to the incentives that produce emotion.

Although within an experimental context we can dissociate rudimentary action plans from the emotions with which they might normally be associated, naturally occurring emotion is likely to be more tightly coupled to specific action tendencies. And conversely, when approach and withdrawal action occur, they usually are associated with an emotional state. As I noted earlier, the nature of this coupling is not rigidly fixed,[4] but rather probably biologically constrained in a fashion similar to that observed in the literature on biological preparedness and learning (Seligman & Hager, 1972).

What is the utility of highlighting approach and withdrawal as the fundamental basic dimensions relevant to emotion? Below I will briefly summarise the powerful analytical advantages afforded by this conceptual approach. Following this summary, I will then consider where discrete emotions fit within this model and how research on their neural substrates should proceed.

We have proposed that those negative emotions, affective traits, and psychopathology that include a strong withdrawal component—distancing the organism from the source of stimulation—will be associated with right-sided anterior cortical activation. Both fear and disgust protypically involve withdrawal. Certain anxiety disorders, such as phobias, also include a strong withdrawal component. In our research, we have consistently found that the experimental arousal of these negative emotions is associated with an accentuation of right-sided frontal activation compared with either a non-emotional baseline or a positive emotional state (e.g. Davidson et al., 1990). Moreover, in three separate studies we found that those individuals with tonically elevated right-sided frontal activation report more intense levels of fear and disgust in response to short film clips designed to elicit these emotions compared with subjects who show left-sided activation (Tomarken, Davidson, & Henriques, 1990; Wheeler, Davidson, & Tomarken, in press). Among social phobics, the anticipation of making a public speech is associated with pronounced right anterior activation (Davidson et al., in prep.). In contrast to these withdrawal-related negative emotions and psychopathology is depression, a negative

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[4] Although some theorists argue for a more isomorphic relation between action tendencies and emotions (e.g. Frijda, 1986), their own data do not support such a strong linkage (cf. Frijda, Kuipers, & ter Schure, 1989). Using self-report measures of action tendencies, these investigators found that an average of only 40% of the emotion names used across two studies were correctly predicted from descriptions of action tendencies.

affect state or trait that we have proposed is predominantly associated with deficits in the activation of an approach system. This suggestion is based upon numerous and diverse sources. The phenomenology and symptomatology of depression specifically includes references to approach-related deficits (see Depue & Iacona, 1989, for a review). For example, psychomotor retardation and loss of interest and pleasure are all symptoms we would logically expect to follow from deficits in the activation of an approach system. Moreover, factor analytical studies of mood in depressed and normal subjects indicates that the most pronounced difference between these groups is not an increase in negative affect among depressives, but rather a decrease in positive affect in this group compared with normals (Watson, Clark, & Carey, 1988). In comparisons of baseline levels of frontal activation in depressives and controls, we have consistently found that depressives show decreased activation in the left frontal region (Henriques & Davidson, 1990, 1991; Schaffer, Davidson, & Saron, 1983). We have also found that toddlers who have a temperamental style characterised by reticence to approach novel and unfamiliar people and objects show decreased left frontal activation compared with their uninhibited counterparts (Davidson, Finman, Straus, & Kagan, submitted). Most recently, we have found that the administration of diazepam, a benzodiazepine that increases approach-related behaviour in novel and unfamiliar situations, also increased left-sided frontal activation in rhesus monkeys (Davidson, Kalin, & Shelton, submitted).

Within the positive affect category, we have distinguished between approach and non-approach forms of happiness. In response to short film clips which depict amusing positive events, subjects report increases in the intensity of happiness and amusement and also show facial signs of felt happiness (Duchenne smiles). However, these positive affect states rarely include an approach component. Contrast this with a situation where a 10-month-old infant is exposed to an episode of its mother approaching and smiling. In addition to showing a Duchenne smile, the vast majority of infants of this age show unambiguous and frank signs of approach behaviour, e.g. reaching out toward the mother. We have observed different patterns of brain activity in these situations. During approach positive affect, we find increases in left frontal activation above baseline (Fox & Davidson, 1987, 1988), whereas in the film clip example, anterior asymmetry does not differ significantly from that found at baseline (Davidson et al., 1990). Thus, we have two positive affect states, both of which are associated with the same pattern of facial expressive behaviour and of self-reported emotion. However, behaviourally these states differ and they also differ in patterns of frontal brain activity.

We believe, although we do not yet have the requisite data, that a similar distinction between approach and non-approach forms of the emotion can be made for anger. When anger is elicited within the first year of life it is typically associated with a strong approach component. For example, one of the most reliable procedures for the experimental elicitation of anger in the first year is the moderate restraint procedure (Stenberg, Campos, & Emde, 1983). In this procedure, the infant is placed on its back while the wrists of the infant are held to the ground. Infants will typically struggle against this restraint (presumably to overcome a goal blockage) and will show facial signs of anger. During this procedure, when facial signs of anger are present in the absence of crying, infants show left frontal activation (Fox & Davidson, 1988).

We have not yet directly compared the experimental arousal of approach and non-approach forms of anger in the laboratory. We are currently performing such a study by comparing brain activity during the experimental arousal of anger in subjects who habitually express their anger and in those who habitually withdraw from anger-producing situations. Only through such a comparison, or by experimentally manipulating these different forms of anger will a more definitive answer emerge.

It is my hope that this discussion underscores the salience of the approach and withdrawal dimensions for understanding the structure of different emotions. As I tried to illustrate, for at least some emotions (e.g. anger, happiness) the magnitude of engagement of the approach and withdrawal systems is not fixed but rather varies with the context and across different individuals. Certain emotions may have a high probability of being associated with either approach or withdrawal (e.g. disgust), although one may imagine scenarios in which the opposite action tendencies may be associated with even these emotions. It is clear that approach and withdrawal are dimensions which are basic to emotion, appear throughout many levels of phylogeny, emerge early in ontogeny, and are mediated by separate brain systems found in all vertebrate species.

This analysis should not be taken as inconsistent with a focus on discrete emotions. The fact that certain forms of anger and happiness both include an approach component does not imply that they are the same emotion. They may share certain important neural substrates in common, but it seems clear that they also must differ in important respects too. The dimensional perspective articulated above should be regarded as complementary to the discrete emotions perspective. They are simply addressing different levels of a complex process.

If physiology and expressive behaviour vary within emotion families, then what remains invariant across different instantiations of the same discrete emotion? In agreement with the theoretical position articulated by Stein and Trabasso (this issue), the invariance is carried in the higher-order goal with which the emotion is associated. Thus, for example, Stein and Trabasso argue that fear *always* is associated with "the desire to prevent

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the onset of an aversive state or the desire to prevent the loss of a valuable goal". According to their analysis, each emotion has its own unique higher-order goal which is *necessary* for the experience of that emotion. A similar emphasis on higher-order goals is implicit in the analysis of basic emotions proposed by Johnson-Laird and Oatley (this issue). Given that different discrete emotions are associated with different higher-order goals and these goals represent the invariant core of the emotion, I would expect there to be central states which subserve these mental events. However, it is likely that the neural networks which subserve these core goals will be constituted in cortical sites that participate in the generation of other, non-emotional beliefs and desires. Thus, the patterns of neural activity that might reflect the core invariance within emotion families and differentiate among different emotion families will likely be found to occur in those brain regions—such as the dorsolateral frontal cortex—that are not specifically implicated in the emotion generation process, but rather participate in the integration of basic cognitive and emotional operations associated with belief and desire states.

SUMMARY AND CONCLUSION

In this article, I have attempted to raise questions about some of the characteristics of emotions given special status as basic. I have focused here on expression and physiology. The strength and isomorphism of the linkage between expression and emotion was considered and several examples were brought to bear to support the conclusion that the expression-emotion package is neither simple nor tidy. Similarly, considerable variability within an emotion family in physiology was suggested. Most physiological patterning was claimed to be more action-specific than emotion specific, although it is clear that the requisite studies to fully address this question have not been performed. Invariance across the many different instantiations of an emotion within a family may be found centrally, but are likely to be found in neural circuits that subserve the basic cognitive processes associated with beliefs and desires because it is at this higher-order level that the best case for invariance within emotion families can be made. Finally, the dimensions of approach and withdrawal were proposed as basic, fundamental building blocks of emotion. The application of these dimensions to a number of different emotions and to individual differences in affective style was illustrated.

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