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The science of emotion as a multidisciplinary research paradigm

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Abstract

I discuss the emergence of a *science of emotion* and argue that research in this domain requires an appreciation of the organization of emotional processes at different levels as postulated by *social neuroscience*. Emotions cannot be understood without relying on a program of multidisciplinary research. Local multidisciplinarity cannot be achieved without a programmatic framework that takes three issues into account (1) the *relationship of multiple levels* of emotions and connected processes, (2) the *mutually informative study of humans, animals, and artificial systems*, and (3) the dynamic nature of emotions in a *dynamic systems approach*. Illustrations for my arguments are provided relating to facial expressions of humans.

Keywords: Emotions; Evolution; Facial expression; Psychology

Most scientists working on affective processes identify the publication of Charles Darwin's The Expression of the Emotions in Man and Animals (1998) in 1872 as the corner stone of modern emotion research. Darwin's impact is twofold. The general impact is that behaviors were seen as functional properties of species that play a role in adaptation and as such can be interpreted regarding their adaptive value. Emotions were no longer seen as dysfunctional, in the sense as something to reject, to control, or even to conquer, but instead as something being functional and ultimately essential for the survival of the species.

As regards the specific impact of Darwin's book on emotion research, it is assumed that observable behaviors are interpreted by members of the same—and potentially other—species as indicative of mental states or predictive of readiness to act. In other words, these 'expressions' may serve an interindividual regulatory function. Darwin also hinted at the fact that voluntary control of these behaviors would have an impact on the states as such, i.e. management of expressions might serve intra-individual regulatory functions. Obviously, the idea that behaviors, and particularly facial actions in humans, would reflect underlying mental states was not new (see also Fridlund, 1994). However, Darwin's arguments clearly encouraged subsequent cross-cultural research as

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well as comparative studies across species. This approach made sense within the larger framework of the theory of evolution and particularly the latter would not have occurred to most scientists before Darwin's eloquent arguments.

The emergence of a *science of emotion* has been non-linear over the last century and the associated theoretical development has been characterized by some as being contradictory and confusing. In fact, it appears now, with hindsight, that there was less confusion regarding emotions *per se*, but that rather that different researchers focused on different components of the emotional reaction, such as on expression or on visceral physiological changes. Particularly, those who were mainly interested in the subjective experience of emotion, such as James (1884) were interpreted as having expressed very different ideas than many later theorists. The fact that James used the term *emotions* essentially as being synonymous with feelings created a condition that caused interpretations concerning subsequent discussions, e.g. the debate between Walter Cannon and William James, to be, to a certain degree, *incommensurate* (Kuhn, 1962), in that different theories used the same word for different processes or concepts. James argued in fact that the subjective experience of *some* emotions depends on a perception of bodily changes. Clearly, these changes themselves were the consequence of information processing that would occur in the brain. James was simply not very explicit about this process and hence later discussions of James' theory simplified the process to the point where it would appear that the brain was involved only once the bodily changes were perceived—this is of course nonsense and does injustice to James' ideas. The core difference between Cannon and James boils down to whether the subjective experience of emotion, 'how the emotion feels', does or does not require the perception of bodily changes. In fact, this debate has not ended and is very current, for example in the guise of Damasio's *somatic marker hypothesis* (Damasio, 1994). In fact, there have been multiple instances of important confusions of terms in emotion research over the years and still there are issues arising when terms such as *cognition* are used as being synonymous with *thinking* (Kappas, 2001) or *intentional* with *voluntary* (Kappas et al., 2000).

Undoubtedly, the key factor in the current emerging understanding of emotions has been the widely accepted notion that emotional reactions are the consequence of some sort of information processing of an individual. This processing is now commonly referred to as *appraisal* (Scherer et al., 2001). As Arnold (1960) presented her ideas originally, appraisal is 'not the result of reflection. It is immediate and indeliberate (p. 172)'. According to her, the difference between 'mere' perception and appraisal is that appraisal refers to the fact that an event appears to have a personal relevance. That is, the situation is perceived as affecting the individual and it is relevant to its goals. The distinction of things being relevant to the individual, or not, is according to Arnold so basic that many species are capable of that distinction. In fact, preferences or dislikes that are the consequence of classical or instrumental conditioning can be interpreted as one of the simple (and possibly phylogenetically early) bases of appraisal *sensu* Arnold. The event that elicits the conditioned reaction has acquired the capacity to trigger a motivation to approach or avoid an object (Rolls and Treves, 1998).

All emotional reactions, from the 'simple' conditioned response to the human emotion, elicited at times by conscious reflection, can be seen as functionally freeing the individual from hard wired and innate responses (Arnold, 1960). Scherer has described one of the key functions of emotions as being able to decouple the individual from the necessity to respond unconditionally (e.g., Scherer, 2001, 1984). Emotional reactions prepare the organism for potential action and provide the individual with information regarding the meaning of the current state of the environment. It is plausible to assume that the emergence of emotional reactions is associated with the emergence of consciousness, but this argument is likely not to be resolved by means of empirical research (but see Cabanac, 1999). However, what is critical to current interpretations of emotional reactions is that *self-consciousness* is not considered a prerequisite for emotional processes. While there are researchers that argue that animals and human

infants cannot experience emotions (Cornelius, 1996) it is important to transpose these views into the integrative language of emotion science simply as there being a difference, possibly qualitative, in the pattern of the emotional response as shown by human adults and infants and perhaps between humans and other species. This does not imply that there are not components of the reactions that are highly similar and that, in the context of a *science of emotion* framework, the study of the one would not be informative for the other. It is important not to get side-tracked by the individual preferences of some authors regarding whether or not to reserve the term emotion to human adult experience (for a discussion on species-centrism in emotion research Panksepp and Panksepp, 2000; Mealey, 2001).

The science of emotion today is necessarily multidisciplinary in nature. Of course this is not surprising, as many, if not most, current scientific endeavors are multidisciplinary. As Stegmann remarked '[a]ny discipline must fully consider, and be consistent with, other, related disciplines, to produce credible results '(2001 p. 20). In fact, there is formal recognition of this multidisciplinarity, such as the foundation of the *International Society for Research on Emotions* more than 20 years ago that affirms in its mission statement that ''[t]he interdisciplinary interests of researchers and the international and cross-cultural scope of much of the recent work on emotions has created a need for a society where researchers from various disciplines can come together to discuss issues of mutual concern'. There is also empirical evidence in that researchers in many different domains, such as psychology, biology, anthropology, sociology, ethology, medicine, cognitive science, political sciences, economics, or philosophy study emotional processes. In some cases, related terms such as *affect*, *mood*, *feeling* are used and at other times a researcher will embed her own work in a different context, such as *motivation*, *learning*, *cognition*, *perception*, or even *behavior linked to specific situations* (e.g. nurturing or play behaviors) and yet, clearly, the phenomena under study are all arguably linked to emotional reactions as we have come to understand them today. If there are problems to such interdisciplinary endeavors, they are more likely to be linked to practical 'sociological' issues in how researchers define themselves within a domain (Segerstråle, 2001) than scientific.

Regardless of whether it is the nature of emotions itself or the historic fact that Darwin played a decisive role in spurning a new wave of scientific research on emotions in different disciplines, it is clear that the science of emotions today is decidedly multidisciplinary. While Tooby and Cosmides (1992) criticized what they referred to as *The Standard Social Science Model* that denies or underplays the importance of the coherence of different domains that are in fact linked due to the evolutionary nature of phylogeny, it appears in fact that the study of emotions has followed already for an extended period of time the credo that without a serious attempt at multidisciplinarity, including the phylogenetic heritage, it will not be possible to understand emotions. Clearly, this understanding predates formal emergence of the domain of research that is now referred to as *Evolutionary Psychology*. Perhaps it was historically easier to see a link between the study of emotions and an appreciation of the phylogenetic dimension because there is a naive association of emotions with the human 'animal' whereas cognition seems, equally naively, to be a very human enterprise—a misconception that has been clearly amplified by centuries of philosophical and religious dogma. Whatever the reason, Darwin's ideas helped to pave the way for a comparative study of emotion *and* cognition.

1. Is emotion research truly multidisciplinary?

While it is true that there are few research programs that actually involve multiple disciplines concurrently, the current multidisciplinarity in emotion research can be seen as *holistic* (Von Eckhardt, 2001). Von Eckhardt (2001) distinguishes between a *localist* conception of multidisciplinarity that implies that a 'field is multidisciplinary if the individual research efforts of its scientists are, typically multidisciplinary (p. 454)', as opposed to a *holist* conception of multidisciplinarity that implies that a 'field is multi-

disciplinary if it is characteristic of the field that multiple disciplines contribute to the execution of its research program (p. 454)'. In the case of emotion research clearly the latter is a more accurate description of current research activities. What needs to be addressed urgently is (a) how can findings from one discipline inform or impact research and theories in another discipline and (b) how can a truly multidisciplinary *science of emotion* begin to integrate findings and theories into a *standard theory of emotion* that crosses the boundaries of disciplines? The present contribution is an attempt to underline the necessity to increase the multidisciplinary cohesion by moving from an implicit formulation of how this could be accomplished to a meta-theoretical framework that states explicitly how science of emotion can move forward.

2. How fuzzy can a definition be?

The lack of a consensus definition of *what emotions are* seems to be the first stumbling block in integrating different theories and findings in different domains of scientific inquiry. In fact, in an often cited article, Kleinginna and Kleinginna (1981) attempted to compile emotion definitions and argued that there was no consensus but rather groups of categories of definitions that emphasize specific emotional phenomena or theoretical issues, such as physiological mechanisms, expressive behavior, or disruptive vs. adaptive effects. While there had been a long history of suspicions that there existed little agreement among emotion researchers, the Kleinginna's article in its meticulous attempt of categorization made it even the more obvious that emotion research seemed to be a conceptual quagmire. One could be tempted to ask how emotions could be studied if the respective researchers did not even agree what emotions are? However, it would be superficial to assume that a definition must be either *right* or *wrong*. Consider that in some cases emotion definitions are incommensurate in that they use language that uses different terms for the same process or the same terms for different processes. Without translating definitions to a meta-language where definitions are axiomatic defined and agreed upon, it is difficult to compare some of these without the proper background knowledge. It is unfortunate that such differences in technical terms have existed in the past and, in fact, continue to thrive at times in the present, but this issue can, I argue, be overcome through a proper process of interpretation.

Another issue in evaluating differences between emotion definitions requires consideration of *who* defines emotion, *why*, and in *which specific context*. Some of the cited definitions in the Kleinginna and Kleinginna (1981) article for example were drawn from introductory texts, others originated in articles or chapters that dealt with a specific issue. Naturally, researchers tend to emphasize their own focus as regards the components of the emotional process or the aspect of the concept they are interested in. A one-line or two-line definition does not necessarily reflect the theoretical position of the respective author. What is important, is not that a particular printed short version of a definition corresponds to another but how the corresponding larger descriptions of the emotional process and of the implicated components compare. Taking more elaborate statements of the authors' position into account, there appears to be less confusion. Personally, I consider the issue of confusion a *myth* that is propagated over time by a tendency to focus on differences, rather than on similarities between theories.

Lastly, a definition often serves to identify boundaries between concepts when those boundaries are arguably arbitrary. Assume the following example: In many cases emotional reactions occur when an event is processed as being relevant for the organism in a particular way, e.g. the sudden appearance of a conspecific with which the individual had previously several aggressive confrontations. Knowledge representations—mostly learned contents—play a key role here. Without memory it is difficult to prepare for action because the organism cannot predict the meaning of an event for its well being and its goals and needs (e.g. LeDoux and Phelps, 2000). Thus, memory processes are important if not crucial for the information processing required to elicit emotional

reactions in many situations. Yet, we have no reason to assume that memory is *necessary* for all emotional reactions to occur. In fact, the absence of any relevant knowledge representation, learned or hard-wired, might be sufficient to trigger emotion related responses in some cases. Should memory processes be included in a definition of emotion? Inversely, it becomes increasingly apparent that emotion related processes are important for the establishing and the retrieval of knowledge representations (e.g. Parrott and Spackman, 2000). Should the role of emotional processing be included in definitions of memory or cognition? In fact, most if not all information processing plays a role in emotions and emotions can play an important role for processes such as perception, attention, etc. And why not! We can interpret the functioning of organisms as being characterized by the action of various subsystems, but in many cases, these subsystems can only serve a meaningful purpose in interaction. Clearly, accounts that focus on the procedural issues of emotional responses will be more productive than structural models that appear static representations of interlinked modules. In these cases, a different level of analysis, such as meaningful groupings of subsystems (e.g. an organismic level or even interindividual level) is more adequate. Of course, modularity of cognitive processes is one of the issues that will provide as much discussion for the near future as it has in the recent past. Issues relating to modularity are linked to an understanding of what emotion is—or is not. But, our understanding of emotions requires that at times research be focused on one level of organization, at others on a different level, and yet at others on how these levels interact (see below the section on social neuroscience). There are good chances that appraisal processes should be viewed as functional modules that are being served by somewhat redundant and parallel systems that might or might not be isolated in the interaction of systems of specific subcortical and cortical structures. We have to look at a functional analysis that interacts with, and is informed by, our knowledge of brain structures, of the brain in its internal interactions, of the functioning organism, and of the interactions of organisms in their social and cultural environment (see also Panksepp and Panksepp, 2000, 2001).

In summary, it would be shortsighted to conclude from the multitude of emotion definitions that have been offered that the current state of emotion research is confused. It is not necessary to have a consensual definition to construct a coherent research framework that can be called *science of emotion*. It is this framework that deals with questions such as what brings emotions about, which subsystems of single or multiple organisms are involved or not involved and under which conditions, and how are ontogenetic and phylogenetic levels of analysis informative for a coherent understanding of the emotions? Similarly, questions such as whether there are essential emotion components (e.g. Averill, 1982; Solomon, 2000), or whether there are basic emotions and if so how many (Averill, 1994; Ekman, 1999; Ortony and Turner, 1990) must be part and parcel of the research program and not necessary components of *a-priori* definitions of emotion that are a prerequisite to defining the research program as a whole.

3. Multiple levels of analysis: the case of social neuroscience

It is frequent in emotion research, as it is in other domains of scientific inquiry, that there seem to be contradictory empirical findings that challenge currently prevailing theoretical positions. For example, many researchers argue that smiles are automatically and necessarily elicited when humans are happy or amused. In this case a simple causal chain of events is assumed: An event *A* (e.g. watching a comedian on TV) occurs that elicits an emotion *B* (e.g. amusement) that leads to display *C* (e.g. smiling and laughing). There are reasons to make such assumptions based on very reliable links between stimuli and facial actions (e.g. taste elicited facial actions in neonates, Steiner, 1977). However, there are numerous studies that show that under some circumstances, particularly when people are not alone, the observed facial displays differ in intensity or in quality (type) from those predicted. Ekman and Friesen (1969) introduced

the term *display rules* to describe a type of process that modulates nonverbal behavior in different social contexts. They argued that individuals might exaggerate an expression, such as a smile in response to a gift in order not to offend the giver, or they might inhibit expressions, such as signs of anger in response to unjustified accusations of a higher-ranking individual to avoid further confrontation. While there is no consistent theory of display rules that would actually predict the types of interactions and contexts that would elicit specific modulation of displays (Kappas, in press), the notion of display rules has been widely accepted as an explanatory device for negative empirical findings regarding predicted concordance of internal state and expressive behavior (Kappas, 1999). In consequence, many researchers trying to show concordance between internal state and expression have tried to eliminate the influence of display rules by studying participants who are presented with emotion eliciting stimuli when they were alone and did not know that they were being observed. However, Fridlund (1991) could show that facial displays of amusement of participants in isolation could be modulated without actually changing the physical social context. In his study, participants smiled at different intensities, depending on whether they were told before that a friend in a different room would watch the same funny movie they were watching or that the friend would engage in some unrelated behavior. Based on these results he argued that it is not emotion, but rather the social context that determines facial behavior. However, Hess et al., (1995) could demonstrate in an extended replication of Fridlund's study that in fact both, emotion and social context, physical or imagined, would impact facial displays. Clearly, it seems impossible to truly understand facial displays only at an organismic or only at a social level of analysis. Neither level alone can account under all circumstances for the observed facial behavior. The resulting theoretical stale mate follows from the fact that on the one hand, emotion theorists that endorse the concept of display rules have failed to produce a theory that would allow clear predictions as to when which emotion will be shown in what form, because, they argue, display rules are not part of the emotion generation process itself, and that on the other hand, those researchers who endorse the behavioral ecology view *sensu* Fridlund (1994) have denied any meaningful relationship of facial expressions and emotions, but have not provided a theory that could predict which expression will be shown under which circumstances and to whom. The effect of social context is not only obvious in cases in which an expression would be controversial or 'against the rules'. Take for example taste or odor elicited facial actions. Firstly, the results of the classic studies are in the best of cases not clear-cut even without the effect of social context. Specifically, Fridlund critically discusses the results of Steiner's study (1977) in which 'neonates were exposed to five odorants: butter, banana, vanilla, shrimp, and rotten eggs (. . .) [a]lthough the modal responses were in the predicted direction for all odors, the variability was considerable. At least 40% if the neonates departed from prediction on four of the five odors; only the rotten eggs produced 'rejection' faces in all the neonates' (Fridlund, 1994, p. 155). Since Steiner's study, similar experiments have been conducted with adults. In one such study Kraut (1982) had participants sniff and evaluate a series of odors either alone, or in the presence of a stranger (behind a translucent screen). The participants' facial expressions were filmed and later judged by a second group of participants who had to guess using the 'sniffers' faces the pleasantness of the odors. Kraut found that the presence of the strangers reduced the expressiveness of the participant sniffing the odors—another example of the interaction of spontaneous expressions and social context (see also Wagner and Smith, 1991; Wagner and Lee 1999) that is difficult to account for with a display rule approach.

Clearly, the current state of empirical evidence seems to suggest that emotions and social contexts play a role (e.g. Hess et al., 1995) so now we need to provide a theory that takes both levels into account. I have argued previously (e.g. Kappas, 1996; Kappas 1999, 2001) for such a theory in the context of my developing *Dynamic Appraisal Theory of Emotion*. I have suggested to consider facial expressions the product of multiple source streams that involve mental states *sensu* Darwin

(i.e. emotional states, motivational states, attitudes), voluntary management that includes not only a filtering of displays but also the use of emblems and other patterns that help to structure dyadic interactions (Ekman and Friesen, 1969), various functional elements, such as respiration, mastication, and speech, as well as automatic processes that couple and decouple source streams as a function of social contexts. For example, it is not necessary to invoke cultural rules and social reinforcements to provide an account for inhibiting an externalization of mental states. Based on an increasing self-organization of an infant in early development (Messinger et al., 1997), it appears plausible that there is an emergent and universal process to decouple the tendency to externalize any state from the actual display. It is by now established that there are intra-individual feedback processes that link displays and underlying states ('facial feedback', see Adelmann and Zajonc, 1989; Kappas, 1991; McIntosh, 1996). In the light of such feedback processes one could expect that attempts for affective self-regulation that are obvious from a very early age, in the form of attentional and behavioral regulation processes (e.g. Rothbart, 1994), will be aided by the reinforcing nature of decoupling. Very loosely put, if an infant is in a state of high distress that should be terminated, all means of changing the causal input (attention and behavioral regulation) as well as the reinforcing feedback loop (expression regulation) will lead to the acquisition of emotion regulation strategies that become automatic with practice and repetition. It is not necessary to assume that 'culture' tells humans not to show emotions when in fact a simple and general principle can predict a universal tendency not to show how we feel most of the time. In fact, the current state of knowledge would concur that we are not displaying how we feel most of the time (Kappas and Descôteaux, in press, Kappas, in press). In contrast, if we are together with friends, we seem to be very effective encoders of affective states (Hess et al., 1995; Wagner and Smith, 1991). I hold that we automatically inhibit in these situations the decoupling of mental states and expression without the requirement of volition. The decoupling is a simple consequence of reinforcement—success in regulating high arousal states that would interfere with other actions. This mechanism is assumed to be similar to that proposed by Rizzolatti and Arbib (1998) for the inhibition of motor action induced by mirror neurons. I assume the decoupling of expressive behavior to be a natural process that emerges in the interaction with the environment, like other motor control skills, such as grasping, or walking.

My short sketch of how some aspects of effects of social context impact on the expression–emotion relationship is to serve mostly as an example of how to try to account for expressions at a neural level, at an organismic level (emotions), as well as at a social level. Now we need data. The issue is that it appears not fruitful to restrict the scope of emotion theory to purely emotional processes. We will not understand the emotion–expression relationship if we define other relevant determinants of expressions as outside of the scope of the theory!

In fact, there are many behavioral processes that can and should be analyzed at different levels. In recent years, there has been increasing attention to approaches in Evolutionary Psychology that try to link behavior and cognitive processes by taking recent evolutionary history into account. However, the question is often, at what level brain processes and/or structures are really part of the behavioral analysis. Panksepp and Panksepp (2000, 2001) criticized eloquently what they call 'the seven sins of Evolutionary Psychology'. They argue that much of the current discussion in this domain neglects the reality of cortical and subcortical organization. While one of the goals of Evolutionary Psychology is not only to describe the development of behavior as a function of 'recent' (i.e. past few million years) evolutionary pressures, but also concurrent cortical development, the Panksepps argue that these specializations are not really informed by recent neuroscientific approaches and evidence. And they are potentially right. There is a difference in acknowledging the existence and importance of other levels of organization and actually developing theories that will 'make sense' when seen from either level. The debate on how to best understand behavior cannot be argued on which level is the

most important, the best, or the most elegant. However, a debate can be held as to how true discourse on the relationship between levels of analysis should inform, structure, and guide individual research efforts. Such a concerted effort does not depend on agreements of all elements of the underlying emotion theory.

Originating from an attempt to clarify the role of psychophysiology in the understanding of human behavior, Cacioppo and Tassinary (1990) underscored that anatomy, physiology, and psychophysiology share 'a collective aim to elucidate the structure and function of the parts of in interrelated systems in the human body in transactions with the environment (p. 5)'. Based on the axiomatic assumption that psychological processes (such as emotion) are bodily phenomena[1] and that consciousness can be explained in a materialistic fashion they can be studied at multiple levels, such as chemical, cellular, at the level of tissues, organs, whole bodily systems, the organism itself, the organismic-environmental transaction, and the organismic-environmental transaction in a social context. The issues relating to multiple levels of investigation have been further discussed under the heading of *social neuroscience* by Cacioppo and his colleagues (e.g. Cacioppo and Berntson, 1992; Cacioppo et al., 1996). According to these authors, there are three general principles that characterize the relationships of organization at multiple levels:

(1) the *principle of multiple determinism*—a target event at one level of organization (e.g. neuroeffector response, evaluative response predisposition) may have multiple antecedents within or across levels of organization; (2) the *principle of nonadditive determinism*—properties of the collective whole are not always predictable from the properties of the parts until the properties of the whole have been clearly documented and studied across levels; and (3) the *principle of reciprocal determinism*—there can be mutual influences between microscopic (e.g. biological) and macroscopic (e.g. social) factors in determining brain and behavioral processes (Cacioppo et al., 1996 p. 73).

Many issues are covered/implied in these three principles but in the present context I would like to point out two issues that seem important for an appreciation of the structure of the research endeavor required to understand emotions. Firstly, while there is an intuitive hierarchy as to the scale of the process being studied (e.g. cellular vs. social), there is no level that is necessarily fundamental in that it determines or completely describes behavior at a different level. Because we are dealing with mutual interactions—and the body of data supporting this view is impressive—we have to understand that, as far as the research strategy goes, each level is as fundamental as the other and hence the study of any level is as basic as any other. This implies that for example neuroscience is as basic as is ethology if we want to understand emotions[2]. This is not to say that there are inherent aspects to the levels as such that could be interpreted as basic. Clearly, the organization of molecules has produced societies and molecules as such are not created by societies (at least outside of laboratories). But if we acknowledge, for example, that the systematic use of drugs for medical and recreational use is a function of social and cultural processes then it becomes clear that for some questions research on the one level requires an understanding of the other level. The conceptually closer levels of 'body' and 'mind' should require even less argument as to how they are related. Secondly, we should never focus only on one level of organization but at the interrelations of levels of organization instead.

Clearly, theories are needed that bridge different levels of analysis of emotional processes (Dalgleish and Power, 1999), but the first step is a definition

[1] The issue of materialistic vs. dualistic views on emotional experience and consciousness in general is complex and still object of debate in some circles (LeDoux, 1996; Panksepp, 1998; Solomon, 2000). Psychophysiological or neuroscience approaches are essentially by definition materialistic because otherwise there would be no sense in studying within or across species the interaction of peripheral or central physiological processes and psychological processes.

[2] Cosmides et al. (1992) elaborate on this argument and contrast the implicitly hierarchical notion of vertical integration of disciplines with conceptual integration that is assumed to be less judgmental or more democratic.

of a multilevel, multidisciplinary research program[3]. This view is emerging but it is not yet perceived as the standard model of the science of emotion. One important element of such a model is the inclusion of non-human species and artificial systems as an integrated part of the research program. The multi-level approach outlined by Cacioppo et al. can and should be understood to include non-human species and artificial systems as a dimension that is orthogonal to the dimension of complexity of organization, as can be seen in Fig. 1. Thus, it is possible to perform comparative analysis within the analogue level across different species, or different species at different levels.

It should be noted that the inclusion of machines, programs, or artificial life-forms into a larger research framework seems radical but there have been advances at many levels of organization, some aimed at simulating human or animal performance but not the underlying architecture, whereas others try to emulate specific elements, such as neurons or neuron groups to better illustrate the effects of learning or damage to nerve/brain tissues (see distinction between neuronal network approaches and connectionism in Rolls and Treves, 1998; also Picard, 1997). The issue here is rather to be explicit in the assumptions as to how our understanding of other levels of organization and across species can be informed by this research. In some cases there is simply a reality check as far as the plausibility of certain processes is concerned. If a very simple architecture can perform a task as an emergent property of learning that has previously been argued to depend on complex processes, including executive functioning, there is a challenge as to the need of the complex assumptions. Is it a logical consequence of such a finding that the complex theory is always worse than a simple one? No, obviously not. This holds particularly true when we consider that there are possibly layers of development embedded in human emotion systems that have changed their function in interactions with later developments. Consider the brain and its functions as a complex building that has evolved through several generations including annexes, new wings, and changing uses of previous features for new purposes. Clearly, if we were to design a new house today—for the current inhabitants and uses—we would come up with a much simpler structure. The problem is that looking at that simple structure would not necessarily elucidate how the old, architecturally heterogeneous house 'works'. There is much to be said for simple and clear models and the use of Occam's razor is good practice. However, we cannot be blind to the fact that at times the simplest solution does not describe reality.

[3] It is important that the notion of a multilevel approach must not degenerate into a fuzzy holistic idea. See Panksepp and Panksepp (2001) critique on Stegman (2001) strong position on multiple layers of analysis.

4. Caveat analogias: usefulness and limits of comparisons of emotional behavior across levels of analysis and domains of inquiry

One of the many challenges of research that spans not only levels of organizations, but also species, perhaps even extinct species, and artificial organisms is how one domain can be informative for the other. Misuses of analogies or misinterpretations abound, such as the transposition of the concepts of *natural selection* and *adaptive functions* into day-to-day interpretations of behavior in modern society (see e.g. Symons, 1992).

Fridlund (1994) points out persisting problems regarding the confusions of homologies and analogies in the area of expressive behavior. Specifically (a) like actions do not imply homology, (b) commonalties do not imply genetic control, and (c) the phenotypes of extant species do not necessarily inform about phylogenesis. Based on a comparison of anatomy of several species he argues, for example, that the 'human brow-*knitting* is homologous with nonhuman earflap (pinna) *protraction* (i.e. raising and bringing forward)' (p. 41). Issues such as these underline the difficulty, particularly with regard to the interpretation of expressive behavior (also for example regarding the origin of the smile), of evaluating comparative arguments that are simply based on observation and not on a proper multidisciplinary approach.

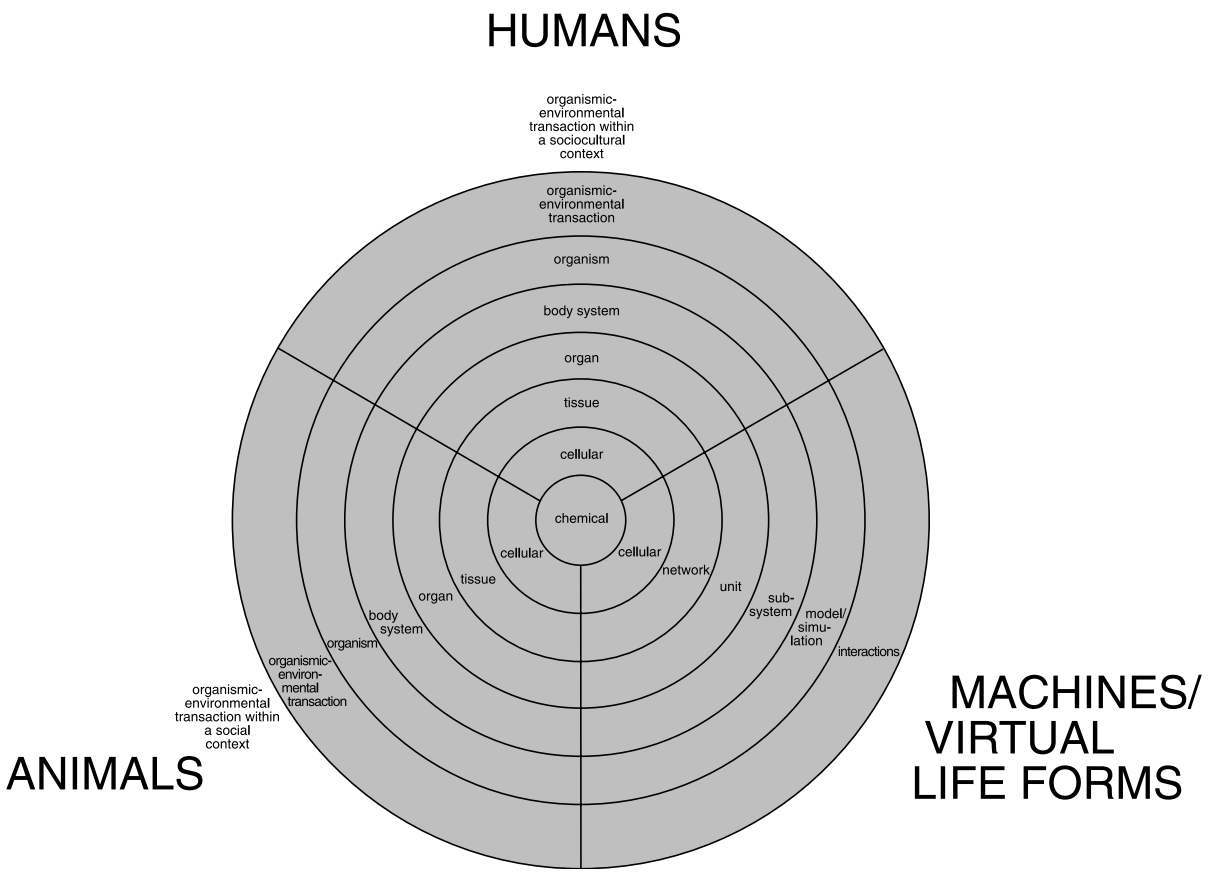

Fig. 1. Levels of organization of behavior for humans, non-human species and artificial systems. Adapted from Cacioppo and Tassinary (1990).

5. The science of emotion as basic and applied science

One of the great challenges to the science of emotion is the interest of emotional phenomena in many different 'applied' contexts. Oftentimes, emotions are seen as an indicator of other processes, such as interpersonal relationships or intra-individual dysfunction. In these contexts, the researcher, or clinician, wants to use a simple measure of emotion, such as facial expressions to get a better grasp on other constructs. If facial expressions were indeed 'a simple measure of emotion' that would be fine—unfortunately, we do not know today for almost any specific case whether a particular pattern of facial expressions is reliably linked to a specific emotional state. I am choosing facial expressions here as an example to illustrate the issue of uses and abuses of emotion science, because I am familiar with it and because the same issues apply to other behaviors or reactions (e.g. peripheral or central physiological changes).

A summary of the current state of knowledge of the relationship of human facial expressions and other components of emotional reactions is necessarily subjective in nature. I believe it is a fair description to say that (a) there is some relationship but (b) there are so many factors that moderate this relationship that it is not possible to use individual expressive episodes as markers of emotional state (Kappas and Descôteaux, in press). We might reach a point where a probabilistic estimate could be given for individual facial

actions or for patterns of expressive behavior that include also other components such as posture and gaze (see e.g. Keltner, 1997). However, we are not at that point yet.

In my perception, this state of affairs has created a schism between some applied areas (e.g. clinical diagnosis, development, detection of deception) and basic research. The answer, I believe, is not withdrawal from applied questions but to state clearer why there are conceptual problems with the notion of markers of affective state given our knowledge today. We need to sensitize the applied areas to the nature of the difficulties that emotion research has encountered and that needs to be informed by a clear demonstration of the interaction of multiple layers of organization.

There is currently a surge of interest in machines that try to diagnose how we feel for implementation in information and surveillance systems, as well as in simple and complex tools, such as computers and cars. Are we really at the point, where emotion research would provide sufficient knowledge to tell my word processor how to find out whether I am upset because I have difficulties with the automatic correction feature or with the company whose acronym I am trying to type at that moment?

This particular example leads me to consider one more issue that emotion research is only beginning to grapple with—the dynamic nature of emotions and its manifestations. Much of our naive understanding of the world and much of our scientific research is based on a simple chain of cause and effect as outlined above ($A \rightarrow B \rightarrow C$). However, a practical consequence of the architecture of emotional responses is that they are linked to multiple levels of interacting systems with within-level and across-level feedbacks. These are dynamic systems, which do not allow from the observation of A and B at a single point in time to predict C. I have argued previously (Kappas and Descôteaux, in press) that in nonverbal communication there is reason to believe that 'small' events can lead to 'large' changes in nonverbal behavior because of the instability of the self-regulating systems involved which might tip in one or the other direction. For example, whether or not we manage to avoid or not avoid a bout of hysterical laughter if our chairman drops in an important meeting for the third time in a row all overheads might depend on the perception of a colleague whose corner of the lips twitch, at that very moment (Banse et al., 1992).

The application of findings in the area of emotion (as in other domains) is limited by the predominant use of experimental paradigms that try to exclude sequential and feedback effects as much as possible. Whether we look at the responses of a human in isolation, observing disgusting slides in a comfortable chair, or a rat in isolation, in a radial maze undergoing a learning procedure, we have to consider whether some of the factors that we are excluding because they are 'noise' and 'merely' intervening variables, are also those whose exclusion will prevent us from understanding how emotions in 'natural situations' differ from the processes we study in isolation.

Obviously, the nature of empirical research requires the identification of key variables and their study in a controlled environment. However, if for example social context is not just a nuisance but a necessity to understand complex behaviors, such as playing, then we will have to grapple with that complexity rather than simply restrict the scope of inferences to artificial situations.

There is no reason to believe that the 'usefulness' of emotion research, whether on humans, animals, or on machines can be increased by a stricter adherence to multiple level analysis and by taking the dynamics of systems into account. The fact that emotion science has so far only limited applications is a direct consequence of the research paradigms that have been and are typically used.

Obviously, it would be naive to assume that it would ever be possible to achieve a full deterministic prediction of the world without modeling it completely (which of course is absolutely nonsense because of the implied modeling of the model and not taking into account whether god or quanta play dice). However, this is not the goal of my argument and reasoning. Instead we can approach a better understanding of emotions in interacting systems that might at least lead us to a state of affairs where we are able to state with some confidence *when* we can make predictions or inferences and *when not*.

6. Conclusion

The fact that my discussion of emotion science includes many current buzzwords, such as *social neuroscience*, *multidisciplinarity*, and *systems dynamics* should not lead to extreme reactions of enthusiasm or rejection. Clearly, there are issues here that suggest a reframing of the programmatic foundations of emotion research. This is not about clamoring for a paradigm shift but simply an attempt to describe emerging developments in how emotions are understood and studied.

Emotions have become a serious focus of interest for many researchers. These researchers have often grappled with the complexity of the issues on the one hand and a rumored state of confusion on the other hand. I hold that there is a myth regarding how confused the field is. I have argued that there is a current state of holistic multidisciplinarity that is pervasive and that has lead to impressive developments of our understanding of emotions over the last 100 years. We need to increase local multidisciplinarity by acknowledging that we need to reflect on how to go about emotion research taking into account three elements. Firstly, the multiple levels on which emotions are organized and how an understanding of these levels depends on the applications of the social neuroscience approach. Secondly, we need to reconsider the degree to which emotion research is species specific. Research on humans, animals, and on artificial systems at different levels needs to be coordinated and mutually informative. Lastly, we need to take the dynamic nature of these processes into account and use notions from system dynamics to understand development at a phylogenetic, ontogenetic and actual genetic time scale. Obviously, this is a big puzzle and many of us have a piece or two to contribute to tell the story on emotions that is yet to emerge.

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