In closing, we hope our short discussion of dynamical mechanisms linking affect and recognition memory illustrates the potential of the dynamical approach for providing parsimonious explanations for specific empirical phenomena in the domain of emotion-cognition interaction.

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Author's Response

An emerging dialogue among social scientists and neuroscientists on the causal bases of emotion

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Abstract: The target article developed a dynamic systems framework that viewed the causal basis of emotion as a self-organizing process giving rise to cognitive appraisal concurrently. Commentators on the article evaluated this framework and the principles and mechanisms it incorporated. They also suggested additional principles, mechanisms, modeling strategies, and phenomena related to emotion and appraisal, in place of or extending from those already proposed. There was general agreement that nonlinear causal processes are fundamental to the psychology and neurobiology of emotion.

My response to the commentaries is organized in several sections. The themes of these sections progress from general agreement on the value of a dynamic systems (DS) reformulation of emotion science, to modeling strategies and mechanisms of emotion I did not employ in the target article, to arguments specific to a DS conceptualization, to fundamental questions about the nature of emotion in relation to cognition, and finally to developmental, clinical, and empirical considerations. The arguments of the commentators, with each other and with me, can be seen as bidirectional transactions that give rise to an emergent form – a dialogue that is still consolidating into a new scientific perspective on emotion.

R1. A new way to think about emotion

To take a scientific interest in emotion is a little like acquiring a giant squid for one's aquarium: it would be so much easier to kill it first. Emotion is unruly, powerful, strange, and complicated. It is intrinsically difficult to study. More than any other psychological phenomenon, it resists categorization, its function is not at all obvious, it does not correspond neatly to any subset of the nervous system, and it can be reproduced in the laboratory only in watered-down form. Yet emotion is at the core of being human, and to give up studying it would be to give up understanding human thought, experience, and behavior.

Unfortunately, the solutions arrived at by emotion theory have come quite close to killing it. Emotion has been hitched like a trailer to cognitive appraisal in a one-way causal sequence. How would we know what emotion to have unless cognitive appraisal preceded and directed it? In fact we wouldn't, and keeping emotion alive requires allowing its irrationality. Emotional effects on cognition have also been portrayed in a narrow, artificial way, as biases or distortions in an independent stream of thought, again in a one-way causal direction. The failure to link these two causal arrows, in a bidirectional process that shapes momentary experience as well as development, makes it difficult to capture emotion without killing it. And the failure to see emotion as complex and iterative robs it of its vitality, leaving an inert shell in its place.

In the target article, I highlighted these deficits in mainstream emotion theory and outlined DS principles that frame causality and part-whole relations in more realistic terms. I argued that the causality of emotion does not reside in cognitive appraisal; it resides in self-organizing processes that give rise to appraisal concomitantly. With DS modeling, it appeared that emotion would not have to be killed in order to be studied, and this provided new possibilities for a bridge with neurobiology. The intricate and recursive flow of current and chemicals in the brain, and the convergent synchronization of its rhythms, could instantiate the causality of emotion only if it too were seen to be intricate, recursive, and inherently dynamical. I went on to demonstrate that self-organizing neural processes, mediated by bidirectional and circular causal relations, give rise to emotion and cognitive appraisal at the same time – each a different aspect of an emergent unity.

R1.1. DS constructs and psychological realism

Most of the emotion theorists who wrote commentaries agree that we need to think about emotion in new ways, and most are enthusiastic about the utility of a DS framework and its facilitation of neural modeling. **Frijda** calls the approach taken in the target article "considerably more plausible" than traditional models, and sees it as a template for modeling appraisal processes in relation to emotion. He states that "both the temporal development and the appraisal-response-reciprocities should become elements of any standard account of emotion generation." Frijda has long argued against the conventional "linear model" of appraisal (e.g., Frijda 1993b). Although he has never fully developed a nonlinear alternative, his commentary outlines several points of agreement with my model: (1) appraisals evolve through feedback with emotional response processes, and trigger, self-amplification, and self-stabilization phases can be meaningfully distinguished; (2) appraisals stabilize through feedback with response options, action plans, and action-monitoring; and (3) dynamic systems approaches are useful for retooling emotion theory along these lines.

Izard, **Trentacosta**, **& King** (**Izard et al.**) also find the principles of self-organization useful for understanding the coupling of cognitive and emotional processes, and in recent theorizing Izard and colleagues have considered similar principles (Izard et al. 2000). **Buck** agrees that emotions and accompanying cognitions arise simultaneously and interdependently, and he endorses the notions of self-organi-

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zation, complexification, and emerging coherence for describing and explaining this process. For Buck, it is time to move away from traditional debates within emotion theory and concentrate instead on the nature of the constituents and their means of interaction. **Ellis** also advocates principles of self-organization and applies them in his own model of emotion and consciousness. If, as he suggests, some predictions in the target article are consistent with both models, their confirmation would support a self-organizational approach that spans multiple perspectives. **Potegal** emphasizes that emotion should not be considered an outcome of appraisal, and **Fabrega** finds my treatment of emotion "highly realistic" relative to conventional psychological theory.

R1.2. DS constructs and neural realism

Pizzagalli and **Thayer & Lane** think that the study of emotion can be much improved by compliance with the constraints imposed by the brain. They eschew the linear causality of conventional models of emotion, and they argue that the traditional compartmentalization of emotional and cognitive systems is untenable from a neural perspective. Pizzagalli and I agree that the definitional overlap between emotion and appraisal reflects functional and anatomical overlap among brain systems, and that this overlap befuddles cause-and-effect models. He likes my use of bidirectional causation, functional coupling or synchronization, and distributed emotional subcomponents that become assembled on-line. Thayer & Lane endorse my efforts to unify the cognitive antecedents and cognitive consequents of emotion, again as demanded by neural realism. They are also very explicit about the benefits of a DS analysis for linking emotion theory and neuroscience, and have pursued a similar course in their own modeling. **Tucker** thinks that contemporary scholarship should pursue general models that span psychological and neural processes of emotion. He reviews the explicit advantage of DS principles for elaborating reciprocal, iterative causal mechanisms as well as Haken's (1977) circular causality. He agrees that this approach to modeling gets at the complexity that is ignored by unidirectional (cognitive or emotional) causal accounts in psychology, resulting in the disconcerting (but revitalizing!) loss of one's definitional starting point.

R1.3. Richness and complexity

Realism is not the only thing sacrificed by linear models. As nicely captured by **Galatzer-Levy**, "the clarity and testability reached through the reduction of complex psychological phenomena is achieved at the price of the loss of the richness people hope for from psychological explanations." He notes that DS modeling allows for richness and innovation in the behavior of all kinds of systems and thus makes plausible what seemed inexplicable on the basis of linear assumptions. With these assumptions discarded, the infusion of richness back into theories of emotion can make them compatible, finally, with our actual experience of emotional life as revealed in psychoanalysis. Galatzer-Levy notes that this direction of theory development and its integration with neuroscience follow an agenda set out by Freud, but with conceptual and methodological tools that were unavailable in his lifetime. I agree with him that a more satisfying interface between emotion theory and neuroscience invites psychoanalytic considerations that have been avoided by mainstream psychology.

R2. Other models and mechanisms of emotion

Although many commentators saw the target article as moving in the right direction, just as many felt I had ignored or underplayed key mechanisms of emotion, and a few argued that I had missed important considerations for modeling these mechanisms. In response, this section moves from general criticisms of the modeling strategy, to alternative mechanisms of emotion, to psychological, neural, and social extensions compatible with the target article.

R2.1. Modeling issues

Barnard & Dalgleish make a case for systemic models of appraisal-emotion at the psychological level of description. They say that my mapping out of global appraisal components such as perception, evaluation, and attention, and my conclusion that the psychological level has little more to offer, ignore the existence of much more sophisticated psychological models that specify interacting parts. These models would presumably provide a more detailed platform for bridging psychology with neurobiology. Although this argument seems persuasive at first, it misses a substantive consideration and a logical step. I do refer to component-system models of emotion-appraisal in section 2.2, but I designate them as information-processing approaches and emphasize their disadvantages, perhaps too glibly. Indeed, the terms suggested by these commentators – "properties of processing resources, varieties of mental representation, and/or mental coding attributes" – fit the rubric of information processing. I argue that these models are mechanistic, hence lacking in realism, and that they remain at the level of interacting parts without explication of part-whole relations. I go on to review process models of appraisal, as a step toward greater realism from within emotion theory, and propose my own process-level account in section 3.3, based on an alternative set of (DS) principles. I can therefore be accused of giving information-processing models short shrift, but not of ignoring them. It would be helpful for these commentators to demonstrate the advantages of such models. Do they really provide a better basis for bridging the psychology and neurobiology of emotion? Are processing resources and mental coding attributes really translatable to types or locations of neural activities? On the logical side, my conclusion that the psychological level of description has little more to offer does not follow my discussion of these models. It follows the presentation of a detailed *systemic* model of my own. My point was that psychological detail can take us only so far, no matter what principles guide the modeling.

According to **Pascual-Leone**, my "failure to use organismic units of processing such as schemes or schemas makes the bridging attempt fall under a reductionist 'mereological fallacy.'" He claims that schemas are the macro units of choice for both psychology and neuroscience, and that they can be used successfully to analyze appraisals. There are two parts to this criticism. What Pascual-Leone calls a mereological fallacy is the blithe mapping of psychological functions onto brain parts. I do use appraisal and emotional

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"constituents" to fashion a map of neural systems on the basis of function. However, I go to great pains to demonstrate that each constituent/function corresponds with many different structures distributed across the brain (sects. 4.2 and 4.3), and I conclude (sect. 4.4) that the definition of these constituents is challenged by a neural analysis. Although Pascual-Leone is a masterful theoretician, he seems to confuse parts and wholes more than I do. Schemas are parts of several "organismic" theories, but importing this term does not make a theory organismic. In fact, Pascual-Leone's use of schemas seems rather mechanistic to me (or else organismic in too strong a sense). The fear reactions of the passenger in his example are explained by an "automatically" synthesized meta-scheme. According to my model, there is indeed a juxtaposition of events which together with emotion (arousal, action tendencies, and attentional orientation) yield a powerful and coherent appraisal. But it is the self-organizing stability of this appraisal that permits the learning (by synaptic shaping) of associations *over time*. Nothing is "automatic." Pascual-Leone is right that several subassemblies must be co-activated synchronously when emotional interpretations (EIs) complexify (i.e., contain more information), either in real time or development. However, this too must be enabled through synaptic shaping *over time*. Few theorists are interested in models that are organismic in the strongest sense, and DS models are often located somewhere in the border region of contextualist and organismic metaphors, or they can be said to rely instead on the fundamental concept of emergence (Lewis 2000b). In such models, outcomes are never completely specified in advance, and coherence must emerge through recursive system activity.

Sander & Scherer begin their commentary by claiming that I equate the psychology of emotion with narrow conceptions of appraisal theory more than a decade old and that I fail to recognize the contribution of cognitive neuroscience to emotion theory. This makes little sense to me. Appraisal theory hasn't really changed that much in ten years. A thorough reading of Scherer et al.'s (2001) handbook on appraisal processes (to which I contributed a chapter) shows that most of the traditional positions are alive and well. However, relatively recent process models of appraisal have garnered more attention, as thoroughly reviewed in section 2.3 of the target article. As for the cognitive neuroscience of emotion, **Thayer & Lane**, chief proponents of this approach, praise the thoroughness and relevance of my treatment. Indeed, I cite and incorporate the work of cognitive neuroscientists throughout the article. But Sander & Scherer's hollow criticism is the tip of an unfortunate iceberg. What is most disappointing is that these theorists, who hold a compatible view of dynamic emotional processes (see Scherer 2000), choose to inflate discrepancies rather than highlight our common vision and shared goals.

Let me address their four substantive complaints in sequence. First, **Sander & Scherer** state that I ignore appraisal dimensions and focus instead on cognitive structures and mechanisms. I do not ignore appraisal dimensions: I review this classical approach in the first paragraph of section 2.2. My emphasis on cognitive mechanisms is similar to Scherer's process orientation. I don't see the problem. Second, these authors say that my Figure 1 does not explain the unfolding of an emotion episode and want a more specific delimiting of its beginning and end. In fact, Figure 1 does not deal with the time course of emotional episodes. Rather, it sketches a feedback reconceptualization of appraisal-emotion processes. This sketch is soon followed by section 3.3, which is given to the explicit modeling of the phases of an emotion episode. Behind these misapprehended details, Scherer's model and mine share an emphasis on synchronization in emotion episodes. This should be the basis for congeniality, not dispute. The third criticism is that my model relies on constructs that are vague and lacking in concreteness, and it therefore provides no analytical advantage over, say, Scherer's (1984) model. I am chastised for appearing unwilling to "take the [appraisal-emotion] amalgam apart in order to understand its nature." One hopes that these commentators read beyond Figure 1, because that is exactly what the remainder of the article set out to do. A high degree of specificity in psychological modeling was provided in sections 3.2 and 3.3, and concrete neural structures and processes were presented in great detail in sections 4 and 5. Finally, Sander & Scherer discuss theory and findings concerning the amygdala to highlight a perceived lack of detail in my own account. Although the amygdala is referred to frequently throughout the target article, I don't disagree with any of their discussion, and I am happy with the notion of relevance detection (see sect. 4.2.2). More functional detail is always welcome in a discussion of multiple neural systems and their mechanisms of interaction. But the overarching goal of such efforts should be an integrated perspective in which neural details adhere to a coherent set of principles derived from the work of like-minded theorists.

R2.2. Alternative mechanisms

According to **Carver**, the meaning of emotion can't be gleaned without elaborating the nature of triggers. He sees triggers as events evaluated (i.e., appraised) according to their relevance for attaining or avoiding desired or undesired conditions. For Carver, dynamic mechanisms such as positive and negative feedback are unnecessary to explain the amplification and stabilization of emotion. Rather than positive feedback, "the mere passing of time creates a steady increase in the trigger's potency," resulting in the rise of emotion from baseline. Similarly, he claims that stabilization (to an attractor) happens sometimes, but usually action reduces emotion back to baseline, by changing the eliciting conditions. In Carver's account, rather than stewing in anger, "Mr. Smart acts to change the situation so his goals are being better met." But importantly, this can take time, *giving the appearance of stabilization* until the cognitive or behavioral response succeeds at reducing the emotion. Carver's account might be compelling in a world of robots busily reducing discrepancies between conditions and goals. But in such a world, emotions are unnecessary. In fact, Carver's account of what I call *triggers* misses the point entirely. A trigger is an event in which a discrete cause produces a disproportionately large (nonlinear) effect: the effect grows based on the properties of the system, not the properties of the trigger. Carver's linear model epitomizes the cognitivism that I am trying to get away from, so it is no wonder that his proposed mechanisms differ from my own. I will tackle his claims in order.

Do emotions rise from baseline simply by virtue of the passage of time? Assuming that one can define an emotional baseline (which I doubt), and that this baseline is somehow equivalent to numerical zero (which is even more

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troublesome), this would be like saying that a car accelerates because the engine speed catches up with the amount of fuel released into the carburetor. For cars this is roughly true in theory, although there are of course many nonlinearities where the rubber meets the road. But **Carver** seems to believe that emotion accelerates because a passive cognitive system appraises more and more of what's going on. On the contrary, organisms function in the world through changes in multiple internal systems that interact with each other and with the environment. These changes are constantly informed by environmental input resulting from *active* perceptual and motor processes. A DS approach highlights active adjustments on the part of the organism and characterizes this set of interactions as recursive and self-organizing. This is what explains the nonlinear profile of change.

Is emotional stabilization really a result of successful action whose impact is, again, delayed in time? Stepping on the brake stabilizes acceleration, and brings the vehicle to a stop eventually. But this simple system is a poor model for complex animate processes. In fact, **Carver** seems to misunderstand the requirements for successful action. He believes that action tendencies, which I agree are generated with emotion, are the same as coherent actions. But raw action tendencies are useless for effective behavior. Instead, the evolutionary advantage of emotion for a sophisticated brain is to constrain and guide cognition until it coheres around a plan. Frijda (e.g., 1993a) and others have made this clear at the psychological level. At the neural level, the prefrontal cortex transcends the "default mode" leading directly from stimulus to response, so that foresight and reflection can guide behavior (Mesulam 2002). According to my model, this sophisticated system achieves stabilization through vertical integration with limbic and brainstem systems, allowing intelligent action to be synchronized with attention and emotion.

Finally, how far-fetched is my portrayal of Mr. Smart stewing in anger? According to **Carver**, "In reality, that is not how such an episode typically ends." Would that it were so. In his commentary, **Potegal** describes the phenomenon of aggressive arousal, a centrally mediated and enduring state of low-level aggression commonly observed in animals. Like children's tantrums (or just a grumpy mood), it "persists well beyond the withdrawal of the provoking stimulus." This cross-species phenomenon is consistent with the kind of attractor state model proposed by the target article and antithetical to Carver's explanation.

In my view, there are some situations in which actions terminate emotional states quickly, when goals can easily be achieved or discarded. But on most occasions we are unable to fully achieve or fully discard our goals. Situations are usually not so accommodating. This results in enduring mood-like states in which emotions and interpretations continue for some time, and action plans are assembled, rehearsed, and discarded, as was the case for Mr. Smart. Elsewhere (Lewis 2000a) I have suggested that these enduring states contribute the most to developmental outcomes, because they foster ongoing synaptic shaping through LTP and related mechanisms. Consequently, the EIs that become entrenched in personality development are precisely those that maintain engagement with situations in which goals are not quickly satisfied.

Northoff identifies two major "neglects" in my treatment. The first is my neglect of his theory, which I actually find intriguing. It seems reasonable that the processing of self-referential stimuli, as opposed to non-self-referential stimuli, should contribute to an emergent self, and that emotional intensity helps to foster this distinction. But then to say that emotion is only present for self-referential events seems circular. I also object to the idea that cognition-emotion integration or unity is a special case of some kind. This implies that cognition-emotion disunities also abound. **Izard et al.** describe cognition-emotion disunities in various pathological conditions. But for me, emotion always fosters integration, because emotional "constituents" include the arousal and attentional focus necessary for cognitive activity to cohere and consolidate. My second neglect, according to Northoff, is my inattention to the role of the medial parietal cortex and posterior cingulate cortex. However, the high level of activity in these and associated regions during resting states (Northoff refers to them as a "physiological baseline") implies that they have less to do with immediate emotional responding than do other regions. In fact, activation is thought to switch from posterior to anterior cingulate cortex when animals are challenged, expectancies are violated, and new learning must take place (see review by Luu & Tucker 2002). This shifting of activation to the ACC appears to mediate action-monitoring in emotionally compelling circumstance (e.g., Luu et al. 2003), as I discuss in section 5.4 of the target article.

Barnard & Dalgleish say that I neglect other important neurobiological systems, including approach-avoidance systems, behavioral activation versus inhibition systems, reward-punishment systems, and appetitive and aversive systems. Many such parsings are possible, but the intent of the target article was not to elaborate every alternative for slicing the neural pie. It was to describe representative structures, believed to mediate key psychological functions, in order to demonstrate the mechanisms of integration most relevant to a DS analysis.

Van Honk & Schutter rightly claim that I do not go into very much detail on the neuroendocrine system. Indeed, endocrine processes constitute a critical mechanism for the stabilization of emotional states. This is not "absent" from my account, but is dealt with in summary form in my discussion of mechanisms of arousal and neuropeptide activity (sects. 4.3.1 and 4.3.2). These commentators propose that various kinds of balance between brain systems, and critically between competing endocrine systems, are necessary to arrive at "emotional homeostasis." But I question whether the term *homeostasis* works as well for lasting emotional states as for endocrine balance. Cannon's homeostasis means maintenance of a steady state, and this is achieved in biological systems by self-regulation following a perturbation. Stable emotional states may be better characterized as *dynamic equilibria*, a term used by dynamic systems thinkers such as Jantsch (1980) to describe the stability of systems that actively maintain their self-organization, or by the term *homeorhesis* as proposed by Waddington (1962). Stable emotional states are not necessarily resting states, as is evident in the commonplace phenomenon of low-intensity moods.

Potegal describes states of aggressive arousal that persist without the help of cognitive appraisal. Most interestingly, he identifies an amygdala-hypothalamic circuit that maintains aggressive states, through control of autonomic and motor functions. This mechanism is important and fits well enough in my overall treatment. Then why does Pote-

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gal imagine that our positions are diametrically opposed? I don't claim that Mr. Smart must first assign blameworthiness and then experience anger. The whole point of the target article was to move beyond this cognitivist idea. I do claim that, very often, a blame appraisal consolidates *with* anger, as it did for Mr. Smart. Anger generally focuses attention on obstacles, including features of other people. Focusing on those features of the other that obstruct one's goals is fundamental to the appraisal of blame. Potegal's account of the rapid rise and stabilization of anger corresponds neatly with my self-amplification and self-stabilization phases, as he acknowledges, and it argues against **Carver**'s assertions. But I would challenge Potegal to find a lasting state of high aggressive arousal in humans without the assignment of blame. To ignore the role of appraisal in the temporal extension of emotional states throws the baby out with the bathwater.

R2.3. Extensions

R2.3.1. Psychological mechanisms. The commentary by **Ainslie & Monterosso** develops the idea that emotions are motivated, not just motivating. They claim that evaluation of the rewardingness of an emotion helps to select it. If they are right, then, as they suggest, the self-augmenting phase of an EI should include this mechanism of emotion generation, and the nucleus accumbens should be featured in the motivated monitoring loop as well as the action loop. This is an elegant argument and I agree with parts of it. Indeed, if cognitive appraisal is concerned with what is most salient, the anticipation of an emergent or soon-to-emerge emotional state ought to occupy appraisal as much as any feature of the external world. Viscerosensory feedback could channel information about emergent emotions without conscious attention. But I would say that the anticipation of a certain emotion, as a phase of appraisal, must have its own emotional concomitant: anxiety or excitement. I am anxious about the likelihood of becoming angry, or excited about the onset of pleasure or vengeful satisfaction. I would further argue that anticipatory anxiety, and not just anticipated reward, can facilitate the *generation* of emotions (not just their minimization). Anxiety about imminent shame increases attention to the self, thereby accelerating shame, and anxiety about anxiety is clearly self- amplifying. Thus, anticipation could be considered a cognitive feature of many EIs, adding to the mix of other features in a self-organizing process already underway. The result, either enhancement or minimization of emotional states, is what many theorists refer to as *emotion regulation*.

R2.3.2. Neural mechanisms. Other extensions to the model are suggested by **Thayer & Lane**. They highlight the importance of inhibitory processes at both the psychological and neural levels of analysis. They argue that inhibitory neural processes are critical for all stages of an EI, not just the self-stabilization and learning phases, as I emphasize. They cite evidence that inhibitory processes among neurons facilitate phase transitions, and that states of psychological entrenchment, such as rumination, indicate a breakdown of inhibitory processes at the psychological level. The link between inhibition and sensitivity proposed by these commentators is fascinating. We agree on one mechanism of sensitivity and change: positive feedback, which implies a loss of inhibition. However, they propose an additional mechanism: the presence of inhibition, which, as I understand it, tunes the system and makes it more responsive. There is room for convergence here. I claim that inhibitory processes (in negative feedback) allow EIs to become focused, coherent, and organized. This consolidation process could be the condition for rapid transitions to alternate states, but only when these transitions are directed by intention or focused thought. Coherent EIs enable directed action, and switching one's focus from one state to another is directed action in the form of planned cognition (e.g., shifting out of rumination). I do not elaborate these ideas in the target article, and Thayer & Lane's modeling suggests that this is a gap that needs filling.

Schore asks many intriguing questions concerning core mechanisms of emotion. Does the developmental sequence of maturation of the amygdala (AM), anterior cingulate cortex (ACC), and orbitofrontal cortex (OFC), respectively, parallel the sequence of activation of these neural systems in an EI in real time? In the target article, I cite evidence linking trigger phenomena (at the start of the sequence) with AM activation, as well as later processes of consolidation and stabilization with the ACC and OFC. The order of activation of these latter two structures may depend on whether the appraisal sequence is initiated through the object-evaluation or monitoring loop, but I know of no evidence that bears on this question directly. Schore also suggests that distinct *representations* at the level of each of these structures may link up to form emergent wholes. In the target article, I claimed that vertical integration links *functions* at different levels of the neuroaxis and suggested a superordinate phase synchrony as a likely mechanism for this phenomenon. But I am not sure how one would identify distinct "representations" at different levels, given the assumption by most theorists that representations depend on cross-level integration. Finally, Schore proposes another sequence for systems involved in emotion regulation. Consistent with my discussion, but articulated beyond it, he suggests that early implicit appraisals mediated by the right OFC precede explicit appraisals mediated by (more dorsal aspects of) the left prefrontal cortex, with the latter feeding back to the right OFC. If these transmissions then modulate vertical integration within the right brain, as Schore suggests, they would provide an ideal mechanism for the consolidation of emotion regulation in the presence of explicit (dorsally mediated) self-monitoring. I like this modeling. My only complaint is that Schore describes an "emergent whole" within the right brain. This seems to ignore his own assertion that modulation by the left brain is necessary to regulate right-brain appraisals. The left hemisphere plays too important a role to be left out of Schore's emergent whole.

Freeman summarizes his highly innovative theory of self-organizing brain states that are both intentional and emotional. I take it as inspirational that a neuroscientist who has been in the fray for so long uses principles of self-organization to model emotion. I have been greatly influenced by Freeman's work, so it is not surprising to find a good match with many of his arguments: (1) Freeman and I agree that the rapid onset of emotional states is trigger-like, constituting, in his words, a "virtually instant reorganization" of brain states. (2) These changes can be modeled neurally as phase transitions leading to self-amplification. However, Freeman's *local* phase transitions occur many times a second, indicating discrepant time scales for the reinitialization of neural patterns and perceptible changes in emotional states. (3) According to Freeman, local state transitions are swallowed up by a *global* state transition about 200 msec after stimulus onset, integrating several sensory systems with the limbic system in a vertical integration. Here the time scale is more in line with my modeling of EIs: the completion of the self-amplification phase of an EI (the "swallowing") could feasibly take up one-quarter to one-third of the minimum time course I estimated for its stabilization (600–800 msec, based on error-related ERPs). (4) Freeman claims that large areas of cortex enter into synchronized oscillations corresponding to intentional behavior on the part of the animal. If all intentional behavior is indeed emotional, this suggests another plank of broad compatibility. (5) We both see self-organizing appraisal states in terms of the selection of one of a number of attractors (multistability). Freeman calls neural attractors hypotheses about the world, and he shows that trajectories of sensory activation select among competing attractors. This phase is followed by abstraction and generalization, consistent with the cognitive elaboration I impute to the complexification of an EI.

Freeman's neural mechanisms of emotion are highly detailed and quantitatively explicit, but they need to be integrated with constructs available to other neuroscientists and psychologists. One of my goals in the target article was to set out a comprehensive but global framework anchored by mainstream research findings in neuroanatomy and neurophysiology, and use it to make sense of emotional processes observable to psychologists. An important next step would be to forge connections between this broad-based framework and Freeman's unique theoretical and empirical contributions.

R2.3.3. Social mechanisms. **Buck** agrees that emotions are self-organizing and that cognition and emotion arise interdependently. His commentary goes on to emphasize the role of communication at all levels of a dynamic system. However, according to Buck, my modeling of emotional processes remains "inside-the-head," and he recommends moving beyond neural constituents to social constituents such as roles and norms for the analysis of higher-order social emotions. One could indeed say that emotions self-organize among individuals as well as within individuals, and various forms of interpersonal signaling become critical at this level of analysis. The emotions involved in riots or sports events appear to require interpersonal coordination, and Fogel (1993) identified emotions in infant-mother transactions as belonging to the dyad, not to either partner. However, this kind of argument can also muddy the waters. Buck refers to Mr. Smart's shame as a social emotion, even though the other driver may have been completely oblivious to Mr. Smart's presence. Emotions, after all, do occur within individuals, whether or not communication is going on between them. The motivational thrust of an emotion is felt by the individual, as mediated by neural and endocrine processes within the individual's body. This thrust may *also* express itself interpersonally and may couple with that emerging in another individual, in a self-organizing process at a higher level of analysis. Studies of interpersonally correlated brain activities (e.g., Hasson et al. 2004) may eventually concretize relations between these levels.

Fabrega has many positive things to say about the target article, but like **Buck** he worries that EIs are modeled within the (individual's) head. As he demonstrates, many heads acting in a shared environment (culture) produce individual differences. Thus, a system of emotional interpretations develops uniquely for each individual, though still culturally constrained, with the extreme being psychopathology and other syndromes. I agree wholeheartedly with this emphasis. Like Fabrega, I see short-term stabilization as tuning long-term appraisal habits through associative learning. Fabrega's clinical and societal emphasis complements my own interest in how the emotion/appraisal system shapes and consolidates individual differences. I regret that I could not devote more time to these issues in the target article.

I am guilty, as charged by **Downey**, of underplaying one of the most important influences on the shaping of emotional patterns. I demonstrate how action shapes and stabilizes EIs, but I neglect the impact of an important class of actions – learned forms of emotional behavior. Habits of emotional behavior (e.g., grief behavior, anger displays) should indeed exert a critical top-down influence on the self-organization of EIs, helping to crystallize interpretation and emotion. Downey's most interesting claim is that this top-down factor "is probably the most important avenue for cultural variation to affect neural architecture." What an excellent point! If culture constrains habits of emotional behavior, emotional behavior helps stabilize EIs, and stabilization sculpts the synaptic circuitry that provides developmental continuity, then cultural forms of action will select and stabilize highly distinct EIs entrenched in the neural architecture of members of that culture. I particularly like Downey's conclusion: "A DST approach to cross-cultural difference in emotional psychology offers the possibility of making physiologically testable hypothesis about emotional responses while recognizing that neural plasticity may be greater than we can imagine." I would only add that constraints on emotional behavior supplied by family members or temperamental proclivities should affect developmental outcomes as profoundly.

R3. Do's and don't's for dynamic systems modeling

R3.1. Matters of principles

The commentaries I have dealt with so far address substantive arguments in the target article. However, a few commentators raise formal issues regarding the conceptualization or presentation of a DS framework. According to **DeLancey**, I may have slipped into thinking that DS principles, in and of themselves, provide a theory or a set of testable claims. He goes on to caution that "there are something like substantive claims lurking in Lewis's account." I certainly hope there are substantive claims in my account. But I do not imagine that these derive directly from DS principles. As suggested by the title, the modeling is where the substantive claims lie, and the DS nomenclature indicates, as DeLancey agrees, a set of conceptual tools for analyzing relations of a particular sort (reciprocal, recursive, etc.). **Pascual-Leone** also asserts that dynamic systems theory is a metatheory, not a substantive theory of its own. I completely agree, and I have spelled this out elsewhere (Lewis 2000b). DeLancey is correct that it does not praise or damn a theory to say that it is a DS theory, as DS has no substantive value added on its own. And he's correct that

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the predictions themselves don't explicitly contrast DS with non-DS principles, though the early sections of the article should make it obvious that non-DS (conventional) principles lead to very different predictions.

Bakker claims that the notion of circular causality is meaningless and should be discarded. The behavior of wholes doesn't *cause* the behavior of parts; it simply corresponds to it. And the fact that wholes constrain parts is obvious for any system. With respect to my neural modeling, he suggests that vertical integration should not be seen as a within-system (levels) issue but rather as reciprocal causation (e.g., feedback) across neural systems, in which case circular causality need not be invoked. Bakker's argument is very clear, and it would apply to any model of self-organization. But is he right? Causality between wholes and parts is a novel construct introduced by Haken (1977) to help explain processes that had never been adequately explained. So the fact that circular causality defies conventional notions should not be surprising. Juarrero (1999) contrasts circular causality with conventional types of causality and argues for its appropriateness when self-organization gives rise to a "new 'type' of entity" (p. 129). She concludes that "self-cause" is necessary for explaining the continuity of complex adaptive systems.

Yet **Bakker** is not the only scholar to express dissatisfaction or at least confusion concerning circular causality. What exactly does it add? As I understand it, synchronization between two oscillating units (whether cuckoo clocks or cicadas) involves bidirectional signals that entrain their oscillations. Add another clock or cicada, and we have signals from each unit to two other units, making the job of entrainment a little more complicated. But with dozens or thousands of units, unit-to-unit signals would not be capable of establishing a single frequency to which the oscillations of all units correspond. Phasing would drift as the sequence of signals fans out from unit to unit. This is not what happens. A vast number of oscillating units remain tightly coupled in lasers as well as brains. Circular causality, in the form of a unitary frequency, provides a top-down influence that simultaneously entrains (or "enslaves") all units, while they simultaneously produce the global oscillation that embodies that frequency. The need for circular causality is perhaps most obvious in brains. Interneuronal transmissions involve large numbers of cell bodies, firing independently, and influencing each other through synapses that vary structurally and chemically. Hence, phase locking across neural assemblies requires something more than lateral forces. It may be for this reason that many scientists who model brain processes dynamically find circular causality indispensable, including **Freeman**, **Tucker**, and **Grossberg** among the commentators and others cited in the target article (e.g., Engel et al. 2001; Szentagothai 1993; Thompson & Varela 2001). If circular causality is a ghost in the machine, it is an *emerging* ghost, and that might be exactly right.

R3.2. Math chauvinism and neural network modeling

Other commentators had very little to say about the target article except that it missed the point entirely – not for reasons of inadequate substance or misplaced principles, but because it did not pay homage to mathematics or neural network modeling. According to **Kaup & Clarke**, all my verbiage means nothing without equations to construct a sample dynamical system. They admit that my modeling might be convincing to those with backgrounds in neuropsychology, but dynamicists require equations. They suggest that a sample dynamic system (a set of equations corresponding to the relations proposed between components of the model) should "model some simple feature of emotion theory, which could then be bridged to some feature of neurobiology." But that's the problem. A simple feature of emotion theory mapped onto a highly idealized neural system would do little to account for the complex processes that interest me. I am aware of mathematical and neural network models of emotion induction and cognition-emotion interaction, but their simplicity and idealization make them less convincing to me than a detailed model corresponding to biological data. Math modelers have an important role to play, but it is only one among many, and problems of realism don't go away just because you supply some numbers.

Grossberg's principal complaint is that I ignored 30 years of work bridging emotion theory and neuroscience – namely, his neural modeling of cognitive and emotional processes. Grossberg's theory and modeling have indeed been important, and I probably should have referred to them in the target article, but I am hardly reinventing the wheel that he has been constructing for many years. Grossberg has been a key figure in developing quantitative models that are strongly self-organizing. Indeed they contain many of the DS mechanisms that I mention. Some of these models (CogEM) also contain modules that have motivational functions (in terms of proximity to goals, etc.), and this makes the modeling slightly more realistic. However, Grossberg is not an emotion theorist, and emotion theory has paid little heed to his work. The converse is true as well: Grossberg is not concerned with arguments and findings in the province of emotion theory. Why this mutual disinterest? In part, because this kind of modeling simplifies "emotion" so much as to make it untranslatable to the variety and color of human emotional behavior. In Grossberg's model, actions are "released" and memory searches are "driven" in a major simplification of psychological and neural function. In turn, emotion is seen as a parameter, not a process, not even a psychological state, in a set of relations that are highly mechanistic despite being dynamical. This is not a fault intrinsic to the modeling; quantitative models require this simplicity. But neither does it provide a bridge that everyone wants to cross.

Winkielman & Nowak claim that my framework "specifies few concrete mechanisms that perform the postulated integration of cognition and emotion." In fact a great number of pages are given over to specifying exactly those mechanisms. The largest section in the target article, section 5, details five neural mechanisms of integration, each referring to data on the functional integration as well as temporal synchronization of brain systems. Winkielman & Nowak's neural network simulations seem useful for modeling cognitive phenomena and speculating as to their emotional concomitants. But they are, after all, simulations of neural processes. If these commentators are interested in real neural processes, they might reread section 5 to see how brains actually work. A handful of dynamically oriented theorists have arrived at the notion that simulations and mathematical models are the main road, if not the only road, to concreteness. Such models are useful, *as* models, for understanding various computational mechanisms.

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However, these models are not concrete; they are abstract. They are metaphorical representations of flesh-and-blood systems. Let's not confuse specificity with concreteness.

R3.3. Moving too fast?

A few commentators appear queasy about a DS makeover of emotion science. **Carver** rejects the need for any dynamical mechanisms for explaining emotional processes. For him, positive and negative feedback mechanisms are "creative" solutions to problems that could be more easily solved with cognitivist formulas. This position is relatively extreme, however. **Frijda** expresses a great deal of enthusiasm for DS principles of feedback, emergent order, and self-stabilization, which indeed support his stand against linear appraisal models. But he goes on to ask what phenomena make a self-organization analysis "desirable." Frijda seems happy with the intellectual parsimony of DS modeling, but like other emotion theorists he may lose track of the strain imposed on emotional phenomena by the Procrustean bed of traditional models. Features of emotions that don't fit the bed, but do fit with a self-organizational perspective, include their rapid emergence on the basis of minimal triggers, their initial sensitivity to context, their globality and coherence once formed, and their resistance to change for prolonged periods, giving way to global reorganizations in response to a subset of perturbations. These features simply cannot be modeled in linear causal terms.

Panksepp calls the DS approach to emotion "a compelling metaphor that raises more difficult empirical questions than substantive scientific answers. . . . Such theoretical views still need to be guided by linear cross-species experimental approaches . . ." He concludes that DS methods cannot hope to tackle the analytical chores of neuroscience. I don't agree. Linear methods, such as correlating single events in one system with single events in another system, have tremendous value for compiling a foundational corpus of data in neuroscience and other fields. But in a system of complex causal interactions, the synthesis of these observations into an overarching explanatory framework requires nonlinear modeling. It is important to study the relations among discrete parts, but we also want to understand the whole. Not only are DS ideas critical for theoretical integration, but they have also proved highly productive for neuroscientific experimentation. The second paragraph in section 4 of the target article lists more than a dozen empirical papers based on dynamical approaches to the brain. This is a representative sampling of an exponential trend facilitated by new methods for time-based analysis of scalp EEG, local field potentials, single-cell recordings, and so forth. Most of these studies rely on methods for assessing synchrony or coherence in the behavior of interacting neural systems, as advocated in the target article. Also, studies of phase synchrony not specifically informed by DS ideas (see **Pizzagalli** and **Kocsis** among the commentators) provide data consistent with these approaches and with models such as my own. Surely these empirical directions complement more traditional analytical approaches and lead to insights not otherwise available.

R4. Cognition and emotion: Two systems or one?

For a number of commentators, the arguments raised by the target article highlight a conceptual fault line running through the psychology and neurobiology of emotion. Should we construe emotion and cognition as two interacting systems or as a single integrated system?

To introduce the debate, let me contrast the views of two commentators. **Potegal** hammers home the point that emotion is not cognition. He cites physiological, developmental, and evolutionary arguments that pitch emotion as a phenomenon distinct from and independent of cognition. Then he goes on to dismiss an obvious role for cognitive appraisal in the temporal extension of angry states. This zealous segregation of emotion and cognition becomes extreme. At the other extreme, **Chella** argues that conceptual space modeling can already map out the cognitive mechanisms of appraisal. By extending the features (to include arousal, action tendencies, etc.) represented by elements in this space, he says it may also be able to map emotional processes. But I don't think that adding to the list of features represented by a point (knoxel) in conceptual space takes us from cognitive appraisal to emotion. This could work for a description of emotional events, but not for the emotion process itself. Even higher-order spaces merely re-describe lower-order interactions. There is something about emotion (the "what to do about it," not just the "what") that is fundamentally not a description. Potegal and Chella place themselves at opposite fringes of the unity debate, viewing emotion either as highly independent of cognition or as a category of cognition. But for many theorists, issues of independence, integration, and unity are more complicated, and a DS analysis brings these issues to a head.

R4.1. Parts and wholes

For years Izard and Ekman, both well-known figures in the field, have been champions of an independent emotion system. In their commentary, **Izard et al.** argue that "the concept of highly interactive emotion and cognitive systems seems a viable alternative hypothesis to the idea of systems integration." They recognize that cognition and emotion are designed by evolution to interact seamlessly in normal circumstances, but they reject the idea that this constitutes a single integrated system. The only integration they allow for is the "functional integration" between particular emotional and cognitive constellations for individuals with a given personality style. The idea that certain appraisals evolve and consolidate with particular emotions over development is the basis of Izard's theory of personality development, and it is a theory I have borrowed from liberally for many years (e.g., Lewis 1995). In fact, in the target article, my modeling of the fourth phase in the evolution of an EI specifies this very process (sect. 3.3.4) along with its likely neural underpinnings (sect. 6.3). However, "integration" means different things at the scales of real time and development. As I see it, integration in development means a *predominant tendency to couple* in real time. This difference in scales is central to a DS analysis, but it is conflated by the semantics used by these commentators.

The crux of the argument put forth by **Izard et al.** is that instances of cognition-emotion nonintegration or dissociation provide evidence that the cognitive and emotional systems are generally independent. They construe infants' inability to regulate emotions cognitively, autistics' and psychopaths' lack of emotional involvement, the disadvantageous decisions made by orbitofrontal patients, and even the responses of normal subjects early in the gambling task,

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as indicators of cognition-emotion independence. This independence is then replaced by interactive integration for normal adults as situations unfold. But I take issue with some of these arguments. Infants, autistics, and other individuals may not show disintegration between emotion and cognition as much as inadequate functioning (by normal adult standards) in the system as a whole. Emotional infants integrate whatever cognitive controls they have at their disposal, as is evident by their efforts to avert gaze or self-soothe when distressed. The inadequacies of autistic functioning are as much cognitive – particularly in the domain of social cognition – as they are emotional. These authors point to amygdala processing deficits in autistics and orbitofrontal deficits in brain-damaged patients as evidence for a stunted emotional system, compensated by cognition. But as I argue in section 4.4 of the target article, these structures are as much involved in appraisal as they are in emotion, and in fact the two roles are impossible to differentiate satisfactorily. Thus, orbitofrontal patients could as easily be described as incapable of certain kinds of appraisals – those based on previous or anticipated rewards or punishments.

My sense is that **Izard et al.** have built their position as a bastion against excessive cognitivism in developmental theory and emotion theory, and it has served its purpose well. But with the advent of new models, especially those that discard cognitivist assumptions at the outset, it may no longer be necessary to take so hard a line. Izard et al. want to see emotion as independent of cognition, because the alternative has always been to see it as a subordinate component of cognition. My emphasis on integration (yes, functional integration – it happens in real time, even though its contents are shaped over development) is far from cognitivist and it grants emotion and all its constituent processes their full status. Integration doesn't have to demean emotion; on the contrary, it makes emotion fundamental to all processing.

Echoing **Izard et al.**'s concerns at the level of neurobiology, **Panksepp** questions my synthetic modeling of emotion and cognition. "When we dissect the many 'organs' of the brain-mind, we see that cognitions . . . are vastly different species of brain activities than emotions." Panksepp, like Izard, goes to considerable pains to draw such distinctions. He has done a great deal of research on brainstem circuits and neuropeptides that point to emotional primes. This body of research and the vision of the brain it imparts have had a bracing effect on the field. But despite the importance of isolating circuits that mediate basic emotional response systems, we should not ignore the interaction of these circuits with other brain systems. Panksepp comes around reluctantly, by saying: "Only when we consider the intact organism, working as a whole, can we claim 'that cognition and emotion were never two distinct systems at all.'" Then let us consider the intact organism! Psychologists think best in terms of wholes, and quite a few neuroscientists care about wholes as well as parts. For **Tucker** and **Pizzagalli**, contextualizing part relations within meaningful wholes is the chief agenda for current theorizing.

Moreover, I doubt that cognition and emotion are distinct in the same sense as liver and kidney (**Panksepp**'s analogy). Brainstem response systems fundamental to emotion receive sensory modulation directly from hypothalamic and nearby brainstem circuits, as well as from higher up the neuroaxis, and they act directly and indirectly on these circuits simultaneously. In other words, they form integrated systems at a relatively local level. These sensory circuits would seem to be involved in mapping out the world in terms of primary appraisals. I am a believer in emotional primes, but without appraisal primes I don't see how they could operate (cf. Ekman 1994). Each of Panksepp's primes implies a basic interpretation: seeking implies resources to discover; panic implies the loss of an attachment figure. Thus, even in the neurobiology of simpler animals, we can say that emotion and appraisal are integrated in any coherent activity.

R4.2. The argument for unity

Other commentators worry about too much segregation. **Colombetti & Thompson** say that it is unproductive to differentiate appraisal constituents and emotional constituents at any level of the argument, for either neural or psychological systems. Feeling is no less part of appraisal than of emotion, so why classify it as an emotional component per se? We are in agreement on the importance of looking at parts in relation to wholes, but for these authors there is only one whole – the unitary brain, whereas I continue to use the language of two systems. For example, I describe appraisal and emotion as being bound in a "functional unity." But *binding* still implies duality. Yet even these commentators have to use phrases such as "constitutively interdependent" to describe the relation between perception and action. The use of "interdependent" must provide them with some leverage by thinking in terms of two as well as one. Similarly, if we do away with "cognition" and "emotion" at all levels, we may be left with a kind of soup. We can only characterize wholes by understanding their parts. And if the wholes are no longer classifiable by traditional functional terms (i.e., if a functional unity is really a unity), then we lose the benefit of these designators all the way down the hierarchy. The danger here is that the wholes will become opaque and unidimensional because the parts can't be adequately characterized. I am arguing now in a similar vein to **Panksepp**. Let's allow cognitive parts and emotional parts for the heuristic purposes of designation and mapping, and then let's notice at what levels of analysis it no longer makes sense to do so. In fact, the target article is intended as a bridge, and it wouldn't be a very good bridge if it did away with the categories psychologists (and some neuroscientists) find necessary and useful. Philosophers of science like Colombetti & Thompson are in a good position to guide the semantics of neuropsychology toward more radical ground, but certain heuristics are hard to abandon in the mean time.

Tucker is not surprised that, when examining neural circuits at all levels, we find no separation of cognitive functions from emotional functions, thus losing the functional categories with which the analysis began. In fact, much like **Colombetti & Thompson**, he suggests that these isolated functions are "psychological fictions," and losing them may be a necessary step in the development of more sophisticated neuropsychological models. Tucker goes on to suggest that the embedding whole in psychological terms is the self, and that this corresponds to the vertical integration of neural activities reflecting both past and present needs and demands. However, once again, by bridging psychology and neuroscience, we end up with a transformed psychological construct: the self as an occasional state "emerging only to the extent that the constituent mechanisms are recreated in the continual flux of psychophysiological processes." I address Tucker's thoughts about the occasional self in the next section. But his notion of embedding might help resolve ambiguities about the cognition–emotion nomenclature. For Tucker, these functions remain isolated in one context – psychological analysis – but become unified once they are embedded within a second context – a neurophysiological landscape. Thus, there is no right answer for the dualism issue. The embedding context in which phenomena are examined, whether psychological experiments, discretized neural experiments, or whole-brain approaches, will determine the most appropriate semantics.

R5. Developmental considerations

Walker-Andrews & Haviland-Jones as well as **Schore** scold me for not making development a central theme of the modeling and for not presenting more developmental data. As I often consider myself a developmental psychologist, this possibility certainly crossed my mind. However, it seemed necessary to pin down real-time, moment-to-moment processes fundamental to the psychology and neurobiology of emotion, before I could hope to analyze developmental processes at a satisfying level of detail. Despite this concern, I did not want to ignore development completely, and the target article deals with associative learning and synaptic shaping, both as a key mechanism of integration (sect. 5.5) and as the final phase in the consolidation of an EI (sects. 3.3.4 and 6.3). This discussion allowed me to analyze experience-dependent pathways of individual development in a manner consistent with Schore.

Schore and I concur that emotionally compelling experiences in early development lay down lasting patterns of interpretation at the scale of developmental self-organization, and **Fabrega** takes a similar view. I state in section 6.3: "Across several occasions, an accumulation of learning events would then be expected to narrow the degrees of freedom for interpreting any subsequent event of this class . . . consolidating individual styles of interpretation, feeling, and belief." From my perspective, what gets learned *is* the present appraisal (and action orientation), and the stabilization of an EI is necessary so that the contents of this appraisal can be consolidated through processes such as LTP. Schore emphasizes the role of emotion in individual development, and I also suggest "that events that are not emotionally significant may not maintain arousal or attention long enough for learning to take place" (sect. 5.5, para. 3). Both Schore and Fabrega propose continuity between personality outcomes and psychiatric disorders based on this kind of learning, as influenced by early attachment conditions. But what I am most interested in is the interplay between real-time and developmental processes, and the role of phase synchrony in facilitating synaptic change that leads to consolidating developmental forms. Although I do not cite a great deal of developmental literature, I do emphasize research that has some bearing on this interplay. I would like to hear Schore address these processes and mechanisms as well as those he has written about so extensively.

Walker-Andrews & Haviland-Jones point out that DS approaches in psychology have been particularly fertile in the area of development, and they model several normative acquisitions in early child development in terms of emergence, consolidation of wholes out of interacting parts, and even fractal-like patterns of self-similarity. I am very sympathetic to developmental DS approaches, particularly in the domain of emotional development, and these authors have indeed contributed to this area. However, one of the problems facing authors in this tradition is how to concretize their models, particularly with respect to temporal measurement, and thereby achieve a level of specificity that advances communication with other scientists. Phenomena in the domain of emotional development are so complex as to make this a major challenge. Mathematical approaches (van Geert & van Dijk 2002) and advanced statistical methods (Hsu & Fogel 2003) have just begun to be applied, and my colleagues and I have introduced our own temporal-statistical methodology (Granic & Hollenstein 2003; Lewis et al. 1999). These methods are suitable for behavioral data, but my approach in the target article was to concretize emotional processes with reference to neural events, such that spatiotemporal processes at the neural level can be related to descriptive phenomena at the psychological level.

A final developmental consideration relates to **Tucker**'s provocative thoughts on the self. If, indeed, psychological causation is emergent rather than unidirectional, and based in brain processes that are complex and self-organizing in the moment, then, as Tucker argues, we might view the self as an emergent form, corresponding to the vertical integration of neurophysiological events. Tucker views the self as an occasional state arising whenever the necessary neurophysiological conditions are recreated (cf. **Northoff**). I agree, but I would even go a step further. Because vertical integration emerges out of neural activities reflecting immediate environmental demands as well as the residue of past appraisals selected by present circumstance, there should be a variety of selves, any of which might emerge on a given occasion. The view that the self is multiple or polyphonic has cropped up in psychological theory (e.g., Hermans 1996) and it has attracted developmentalists with a DS perspective (e.g., Kunnen et al. 2001). I suggest that there are several highly familiar selves (e.g., a strong, confident self; a childish self; a critical self), each constituted by an anticipated, actual, or imagined dialogue with a predictable other, and this cluster of selves fosters a family of attractors for self-referential appraisals. I recently speculated about the neural underpinnings of some of these self-like "positions" based in part on their emotion-regulation and cognitive style characteristics (Lewis 2002). But, unlike Tucker, I think these forms do have a special organizing status. Their frequent re-emergence produces strong continuities over development and their emotional relevance provides consistent constraints on interpretive activities within occasions, each property feeding back to the other recursively. The result, as **Schore** might agree, would be selves that are highly robust despite their limited time on stage – selves that do provide an organizing principle for development, even though they are always instantiated in momentary neuropsychological processes.

R6. Clinical considerations

Galatzer-Levy reminds us that the emotional phenomena of conventional psychological theories are pale reflections of the difficult, irrational, and conflicting emotions of interest to psychoanalysis. Therefore, emotion theory has less to contribute to the understanding of character patterns and clinical syndromes than one might like. Galatzer-Levy recommends a kind of parallax perspective in which DS modeling and neuroscience can recapture this richness. I agree that the rapid switching of behavior in clinical syndromes, emergent phenomena such as generalized anxiety and depression, and the multistability of competing character organizations are now within the purview of scientific modeling. Beyond supplying the details for such models, neurobiology provides new methods for getting at self-organizing brain processes of particular clinical relevance. For example, **Pizzagalli**'s research concretizes the notion that depressed patients experience some kind of disconnect in self-monitoring – a disconnect that prevents adaptive appraisals from making sense of current emotional states (cf. **Izard et al.**). His findings reveal that depressed patients do not show the theta-band synchrony along frontocingulate pathways typical of normal controls (see also **Northoff**). Here, a DS-related prediction about the role of synchrony in functional integration appears useful for helping to explain the symptomatology of major depression.

Schore and **Fabrega** want to distill developmental aspects of DS-inspired neural modeling to explain continuities from difficult temperament and poor attachment patterns to problematic outcomes. These authors agree with me on the importance of habitual appraisals consolidating across emotionally compelling occasions en route to the consolidation of normal and pathological traits. Points of disagreement are confined to the details, and even there I see nothing significant to argue about. Fabrega concludes that "constructs in psychiatry and clinical psychology . . . are, like the psychology of emotion, dependent on a 'language of wholes.' Constructs that sharpen the way emotional behavior disrupts function in the short run provide a language for improving 'diagnosis' that could be more useful to clinicians." He says that my modeling moves in the right direction, but the neural account needs more streamlining and depictions of the self need more articulation in order to maximize the effectiveness of this communication.

The challenge of making this approach accessible and useful for clinicians is onerous, but it is certainly worth pursuing. The research reported by **Pizzagalli** provides a nice example of how the identification of a "disconnect" in neural synchrony can translate directly to clinical intuitions and observations. But perhaps the greatest value of a DS-based neuropsychological approach will be in providing constructs and methods for analyzing individual trajectories of problematic development, as emphasized by **Schore**. Clinicians are always concerned with individual variation, but their models, based on traditional approaches in psychology and psychiatry, highlight categorical syndromes divorced from developmental processes. The field of developmental psychopathology recognizes this constraint and offers developmental-systems accounts to correct it. These accounts are now beginning to include DS-informed models of neuropsychological development (e.g., Derryberry & Rothbart 1997; Post & Weiss 1997), making them far more precise and ultimately more powerful than would otherwise be possible.

R7. Empirical considerations

As noted earlier, **Panksepp** has little confidence in DS-inspired modeling and methods for neuroscience. But he also takes aim with some very specific challenges. These take the form of a list of questions that attempt to squeeze more juice from my admittedly global predictions. He asks: How do I know that cortical theta (measured at the scalp) is the same as the subcortical theta recorded from single cells in animals? Does my modeling of vertical integration suggest particular recording sites or other parameters that could differentiate predictions according to emotional primes? What neural changes measurable through scalp EEG differentiate conscious appraisal from precursor processes? I don't have complete answers to these questions at present. And I agree that they are the kind of questions that need to be articulated and then applied systematically in neurophysiological research.

Although I agree with the appropriateness of **Panksepp**'s questions, I challenge his suggestion that DS approaches are unequal to the task of answering them. In fact, we need not look very far to see answers beginning to appear. In his commentary, **Kocsis** presents "recent data on the relationship of rhythmic neuronal discharge in the supramammillary nucleus and the large-scale theta oscillations in the limbic system which provide support to many of [Lewis's] ideas regarding vertical integration in dynamic systems." Kocsis shows that oscillations of nuclei in the brain stem can drive septohippocampal oscillations on some occasions (induced by the potent stimulus of tail pinch) and be driven by them on others. He also shows that each can drive the other over the same time span, one showing up in the "background" of the other. Here we have not only the phenomenon of emergent phase synchrony across levels of the neuroaxis, but also two triggers of this synchronization, one of which is correlated with negative emotion (probably anxiety) induced by tail-pinch. In this research as well as that cited in the target article, Kocsis has gone a long way toward sorting out the source nodes of emotion-related phase synchrony across the brain stem and limbic system. This approaches Panksepp's ideal of selecting recording sites to match specific categories of emotional response.

As far as differentiating conscious appraisal processes through scalp EEG, it is encouraging that this too is underway, as exemplified by the neurophenomenology research program advocated by **Colombetti & Thompson**. Following principles laid out by Thompson and Varela (2001), Lutz et al. (2002) trained participants to report on subjective states of experience when anticipating and attending to challenging visual displays during EEG recordings. They found a correspondence between states of conscious attention and gamma-band synchrony across frontal cortical regions. As in other studies, gamma-band synchrony appears to tap featural consciousness related to perceptual focusing. Other investigators have looked for synchrony in the theta range corresponding to self-monitoring in emotionally loaded circumstances. **Pizzagalli** summarizes recent work by Luu et al. (2003) indicating theta-band synchrony across anterior cingulate and other cortical sites when subjects are engaged in a difficult process of action monitoring. Luu et al. (2004) extend this paradigm by decomposing action-monitoring ERPs into synchronous and nonsynchronous waveforms tapped at the single-trial level. It seems that guided self-report coupled with single-trial analysis of frontal theta could provide highly sensitive measures of conscious appraisal processes as requested by **Panksepp**.

These research findings are preliminary, and they represent new conceptual and methodological approaches that are just now gaining serious attention. One hopes they will lead to increasing convergence among investigators studying the properties of neuronal synchronization and those developing hypotheses and research strategies for analyzing self-organizing processes underlying emotion and appraisal. As pointed out by **Ellis**, evidence for neural self-organization as the basis of emotion would not necessarily authenticate my particular model. He is correct in noting that the predictions I propose are general enough to be shared by other models of self-organizing emotional states. Indeed, the validation of these and related predictions, and the convergence of findings from scalp EEG, consciously reported cognitive activities, psychophysiological measures, and detailed neural hypotheses, can lend credence to a family of models, all of which depict neural mechanisms of self-organization fundamental to emotional processes.

Organizing a response to 30 energetic and creative thinkers has not been easy. There were pools of converging opinion, but just as many diverging views. Yet it has been greatly informative, and very often fun, to join in a debate on so many fronts. My thinking has been challenged and hopefully advanced by revisiting old problems such as duality versus unity in cognition-emotion. Challenges concerning the use and abuse of dynamic systems constructs have refined arguments that I have left simmering for some time. Extending the discussion into developmental, clinical, psychoanalytic, social, and anthropological domains has provided a good deal of intellectual richness for me, and hopefully for the reader. And debating the significance of additional psychological and neural mechanisms has furthered the tension that generally leads to theoretical improvement. I am thankful to the commentators for their converging and diverging views, as both will move the dialogue forward.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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