

Anger and Approach: Reply to Watson (2009) and to Tomarken and Zald (2009)

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C. S. Carver and E. Harmon-Jones (2009) reviewed evidence consistent with the idea that anger arises from a behavioral approach system. Commentary on that article by A. J. Tomarken and D. H. Zald (2009) raised questions about the many elements involved in acts of approach and limitations on what information can be provided by electroencephalograms. Commentary by D. Watson (2009) raised questions about the extensive psychometric evidence linking the negative affects. This article responds briefly to these issues.

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Carver and Harmon-Jones (2009) reviewed evidence consistent with the idea that anger arises from a behavioral approach system. Commentaries on that article have been written by Tomarken and Zald (2009) and by Watson (2009). We appreciated the constructive approach of the commentaries. Both pointed to the need to move past prior models, and both pointed to issues needing examination in the future. This article responds briefly to the issues the commentaries raised. The issues have three main focuses. One concerns the need to reduce constructs to their basic elements because different elements will have different underpinnings in brain function. A second focus concerns what sorts of evidence are informative about brain function. A third focus concerns how to reconcile disagreement between two literatures bearing on affect.

What Is Approach?

Component Elements of Approach

Tomarken and Zald (2009) argued for the need to reduce the construct of approach, in particular, to its basic elements. They pointed out that approach has multiple component processes, which almost certainly are managed in different brain locations. Approach is indeed a complex phenomenon with many constituent elements (cf. Carver, Johnson, & Joormann, 2008). We do believe it likely, however, that diverse acts of approach, or states of being prepared to approach, recruit many of these component processes to form a coherent whole that organizes the effort. We agree that showing such recruitment of disparate processes is itself a complex research agenda rather than a summary of current evidence. But we doubt that Tomarken and Zald reject the idea. Their point was that

saying the left prefrontal cortical area is involved in approach says little about what that area is doing, or indeed how many different things different parts of it are doing. We agree. And yet electroencephalographic (EEG) recordings repeatedly show more activity there than in the comparable right prefrontal region, in circumstances consistent with approach motives being active. Is that not a useful, if coarse, first approximation?

Nonetheless, we do concur with Tomarken and Zald's (2009) broad theme that an understanding of how motives actually result in behavior will require a far better grasp of the subfunctions underlying approach (and avoidance) and how they are recruited. That, in turn, will require examination of more detailed questions than was the case in the studies we reviewed.

Approach as Precondition to Anger

Though Tomarken and Zald (2009) reported being unclear about what link we proposed between anger and approach, they did correctly describe our position: that anger follows from disruption of approach, and that engagement of an approach motivation thus is a precursor to anger. As they noted, we raised other possibilities, both because those possibilities are salient to many people and thus deserve consideration and because of ambiguities in some of the evidence.

With regard to approach engagement as a precondition, Tomarken and Zald wrote, "Ideally, an argument that anger is globally related to approach requires a demonstration that the varied contexts and triggers in which anger occurs consistently can be characterized in terms of approach" (p. 210). We certainly agree. Indeed, we raised and discussed exactly that issue under Further Questions, though we obviously did not and can not now settle it once and for all.

What Information Can EEG Provide?

Tomarken and Zald (2009) devoted much of their commentary to the neuroimaging literature, asking whether findings from it converge with those from EEG studies, contradict them, or neither. They concluded that a large number of imaging studies have little relevance at all: "Notably absent from Wager et al.'s analysis are studies that actually engage the participants with motivationally significant stimuli" (p. 212). Despite the scarcity of relevant studies,

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Tomarken and Zald (2009) noted that several (though not all) experiments that manipulated approach motivation failed to find greater left prefrontal activation using functional magnetic resonance imaging (fMRI) or positron emission tomography (PET).

Do these null effects call for dismissing the EEG, lesion, and repetitive transcranial magnetic stimulation (rTMS) findings? How else might the discrepancies be understood? Several methodological issues seem pertinent here.

One issue is that EEG and fMRI/PET may reflect different populations of neurons. EEG signals result from very selective areas of current source activity, often corresponding to small subsets of total synaptic action in tissue volumes and largely independent of action potentials. PET/fMRI measures, in contrast, result from action potentials in areas requiring much hemodynamic/metabolic activity. For example, cortical stellate cells occupy roughly spherical volumes and their synaptic sources provide a “closed field” structure, which make them invisible to EEG. On the other hand, although stellate cells constitute only about 15% of the neural population of the neocortex (Braitenberg & Schuz, 1991; Wilson, O’Scalaidhe, & Goldman-Rakic, 1994), they contribute disproportionately to cortical metabolic activity, because of their higher firing frequencies of action potentials (Connors & Gutnick, 1990). Thus, they have a large effect on fMRI and PET.

Other cases can yield strong EEG signals and weak metabolic (fMRI/PET) activity. EEG signals can be large when only a small percentage of neurons in each cortical column are “synchronously active” if a large-scale synchrony among different columns produces a large dipole in which individual columns tend to be phase locked in particular frequencies. In this case, because most neurons in each intracolumn population are relatively inactive, there is minimal metabolic activity.

EEG does not provide the spatial resolution of fMRI. But that does not wholly eliminate it as a useful tool. Observation of frontal hemispheric asymmetries has shed light on important issues in the study of emotion, motivation, and associated clinical problems, leading, for instance, to the development of neurofeedback treatments for depression (Baehr, Rosenfeld, & Baehr, 2001). Moreover, as we reviewed, the EEG findings do converge with results from lesion and rTMS studies.

Better spatial resolution can be obtained from EEG through source localization techniques of high-density EEG arrays. One source localization method that has generated much interest is called low resolution brain electromagnetic tomography, or LORETA (Pascual-Marqui, 1999). Its solution space (i.e., the locations in which sources can be found) is composed of cubic elements or voxels, which are 7 mm × 7 mm × 7 mm, roughly equivalent to fMRI spatial resolution. LORETA solutions have been cross-modally validated with studies combining LORETA and fMRI, structural MRI, PET, and intracranial recordings (see review by Pizzagalli, 2007).

Another methodological issue in comparing these literatures concerns the stimuli producing the reactions. Tomarken and Zald (2009) noted that some fMRI/PET studies failed to find that anger manipulations activated the left dorsolateral prefrontal cortex. A potentially critical point is that anger was manipulated in these studies by recall of past anger experiences. The same method also produced null effects in one of the first Harmon-Jones EEG studies. That failure prompted the use of more controlled and potent methods of anger induction.

A final methodological issue concerning lack of anger effects from fMRI and PET is that those measurement techniques require a supine body position. Preliminary data indicate that this body position itself reduces relative left frontal reaction to anger-inducing insults (Harmon-Jones & Peterson, 2008). This result is consistent with an embodiment hypothesis that lying on one’s back is antithetical to approach-oriented behavior (particularly behavior related to aggression).

The essence of Tomarken and Zald’s (2009) commentary was to question whether a co-localization hypothesis is tenable—that is, whether approach engagement with positive affect and anger activate similar neural circuits. We concur on the need for better localization methods and the use of multiple methodologies. In our view, however, the tenability of this hypothesis (which we note is actually a very complex one) remains an open question.

Psychometric Evidence Linking Negative Affects

Watson’s (2009) commentary focused on a different issue: the relation of the studies we reviewed to the extensive psychometric literature that links all negative affects to one another. He provided examples to make the point that mood ratings made on large numbers of items reliably yield a positive affect factor and a negative affect factor. This well-established pattern was noted in our article, though it was not emphasized. We also noted that the vast majority of this literature consists of ratings pertaining to fairly broad spans of time, or ratings of “now” in situations in which there was little reason for affect. This is also true of the distressed samples that Watson (2009) mentioned: their instruction was to indicate to what extent they had experienced each symptom during the past 2 weeks, including that day.

It is very difficult to know exactly what one is getting when a participant summarizes the affective experiences of a 2-week period. One may be getting an aggregation of responses to many discrete (and diverse) events; one may be getting a residue of mood that presumably was influenced by many events. One is almost certainly *not* getting an affective response to a discrete event. The latter, however, is the focus of our interest. Thus, much of the literature we reviewed involved situations in which it would be normative to experience a particular affect.

Affective responses to situations and accumulation of affects over a 2-week period are quite different phenomena. Watson (2009) agreed that when a situation creates a specific emotional reaction, such as anger, the two-dimensional space is likely to be significantly altered. However, he referred to these cases of specific emotion-inducing events as anomalous. We, in contrast, regard them as the main event.

Affect and Mood

Let us take this point a step farther. The target article addressed a particular affect, with *affect* defined as a valenced reaction to an event or object. In contrast, moods are affective states that are “not elicited by an external event or outlasting such an event, or disproportionate to such an event in intensity or duration” (Frijda, 1986, p. 252). Moods may be residuals of prior emotions, having become detached from the eliciting stimulus, or they may arise in other ways.

Watson’s extensive work on emotion-related experience has focused heavily on moods. For example, Watson and Tellegen’s (1985) highly influential *Psychological Bulletin* article was titled “Toward a Consensual Structure of Mood.” A focus on mood leads

naturally to research that asks participants how they have felt for the past 2 weeks, including that day. This focus on mood has less relevance to people's valenced reactions to specific events. Nonetheless, the positive affect (PA)—negative affect (NA) model has been widely generalized to reactions to specific events, both conceptually and empirically. For example, the Positive and Negative Affect Schedule (PANAS) is often used to assess the success of experimental manipulations of specific emotions.

A number of the remarks in Watson's (2009) commentary appear to suggest that he wrote it with mood in mind, as opposed to affective reactions to specific events. It may be time for all of us to be more pointed about the fact that these are not the same. If we fail to recognize that the structure of mood is a topic that differs in important ways from the bases of specific affects, we will continue to expend our energies in what may amount to talking past one another.

Anger and Positive Affect

Another important issue concerns whether to assume an isomorphism between approach and positive affect. Despite his disavowal of this idea, Watson occasionally took positions that seemed to imply it. For example, we suggested that anger and fear co-occur when impediments to desired conditions (implying the engagement of approach) and threats of punishment (implying the engagement of avoidance) occur simultaneously. (Solid evidence of this can be found in Carver, 2004, Studies 2 and 3, in which anger and fear occurred in response to the same event, with anger linked to approach sensitivity and fear linked to avoidance sensitivity). Watson (2009) responded that if both motivational systems are engaged, then positive affect should be strongly linked to anger and anxiety.

We see no other way to interpret that statement than as an affirmation of an isomorphism between approach and positive affect. Watson seemed to be saying that positive affect occurs—by definition—whenever the approach system is engaged. This could not be the case if valence depends on whether the approach system is doing well or poorly at approach.

On the other hand, the term *positive affect* attaches to diverse states, and circumstances do exist in which states that are called positive affect are indeed linked to anger. Harmon-Jones, Harmon-Jones, Abramson and Peterson (in press) found that anger manipulations cause increases in anger and also in PANAS PA (active, alert, attentive, determined, enthusiastic, excited, proud, inspired, interested, strong). PA correlated positively with both anger and happiness during these episodes, even though anger and happiness correlated inversely. Note that many of the PA items are actually rather neutral in valence, reflecting mainly engagement. Association of these items with anger would seem to reflect determination to make things right. Items reflecting happiness, in contrast, items that are more clearly positive in valence, are related inversely to anger.

Mixed Feelings

Finally, Watson (2009) asserted that the experience of mixed feelings is inconsistent with all of the structural models in our Figure 1. We welcome the chance to clarify this point. People often have multiple goals in mind simultaneously, with one at a time receiving the most processing resources and thus occupying consciousness. If humans are organized in such a way that progress on multiple goals is tracked simultaneously (cf. Simon, 1967), affect can exist with

regard to each such goal. A person can be happy about one thing at a wedding (e.g., "The bride is so happy"), sad about another (e.g., "I never was that happy"), and anxious about another (e.g., "The bride's parents hate the groom"). What apparently was not clear in our earlier statement was that the models in Figure 1, Panels C–E, were intended to portray affects that are related in principle to two classes of goals (incentive approach and threat avoidance) without regard to how many goals are in play. If there are multiple simultaneous goals, there can be multiple affect states, creating the clear potential for mixed feelings.

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