Searching for Evidence, Not a War: Reply to Lindquist, Siegel, Quigley, and Barrett (2013)

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Lindquist, Siegel, Quigley, and Barrett (2013) critiqued our recent meta-analysis that reported the effects of discrete emotions on outcomes, including cognition, judgment, physiology, behavior, and experience (Lench, Flores, & Bench, 2011). Lindquist et al. offered 2 major criticisms—we address both and consider the nature of emotion and scientific debate. Their 1st criticism, that the meta-analysis did not demonstrate emotion-consistent and emotion-specific changes in outcomes, appears to have been based on a misunderstanding of the method that we employed. Changes in outcomes were coded according to predictions derived from a functional discrete emotion account. Their 2nd criticism, that the findings are consistent with a psychological constructionist approach to emotion, is partially supported by the data and our statements in Lench et al. (2011). However, only 1 meta-analytic finding is relevant to this hypothesis, and it does not offer unequivocal evidence. Further, we contend that no modern discrete emotion theories would make the claims described by Lindquist et al. as representing a “natural kind” perspective and that viewing a scientific debate as a war has negative implications for the ability to evaluate evidence.

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Lindquist, Siegel, Quigley, and Barrett (2013) offered two major criticisms. First, they argued that the meta-analysis did not demonstrate emotion-consistent and emotion-specific changes in outcomes. This conclusion appears to have been based on a misunderstanding of the method that we employed (described below). Second, Lindquist et al. argued that the findings are consistent with a psychological constructionist approach to emotion. We also made this point in Lench et al. (2011), but we believe that only one finding in the meta-analysis is relevant to this debate, and it does not offer unequivocal evidence. Generally, many of our methods and interpretations are mischaracterized by Lindquist et al., including minor criticisms they raise in footnotes, and we refer readers to Lench et al. for the statements that we actually made. Below, we respond in detail to the two major criticisms and, along the way, consider the nature of emotion.

1 Lindquist et al. (2013) also stated that we assume emotions precede changes in outcomes and are thus separable from those changes. However, we are neutral on whether emotions represent the coordinated changes themselves or something separate. Discrete emotions were considered separately from outcomes in the meta-analysis because they were empirically elicited as independent variables in the studies under consideration. Whether emotions are those changes or something separate is not a question that can be clearly addressed in a review of experimental emotion elicitations.
**Effect Coding Must Be Emotion Specific**

Lindquist et al. (2013) claimed that we coded any difference among different discrete emotions as positive (i.e., consistent with predictions) in our meta-analysis (pp. 256–257). This would mean that, for example, greater self-reported sadness after a happiness elicitation versus a sadness elicitation would have been coded as positive. This type of coding would have been, at best, uninformative and certainly would not have fulfilled the stated intent of the meta-analysis. In the meta-analysis, we coded effect sizes according to a functional discrete emotion account, whereby emotions are considered to arise from specific situations and are associated with responses that meet the challenges of those situations (see Lench et al., 2011, pp. 834–835, for this idea and examples of predicted effects). We did not examine brain and hormone measurements because it was not possible to derive specific predictions (Lench et al., 2011, p. 838; but see Vytal & Hamann, 2010, for meta-analytic evidence that discrete emotions activate unique brain regions). Often reports of meta-analyses include a table that lists each outcome for each study and the coding for the associated effect. In the case of our large meta-analysis, however, doing so would have necessitated a table with nearly 5,000 rows of data. We could not locate any statements that would suggest all differences were coded as positive, as claimed by Lindquist et al. This is especially confusing, as Tables 2, 3, and 4 of Lench et al. (2011) reported several average effect sizes that are negative, and Lindquist et al. used some of these negative effects in their table derived from our findings. From where would these negative values have emerged if all differences were coded as positive?

Lindquist et al. (2013) also stated that only reviews examining specific outcomes are useful (p. 257). In our meta-analysis, we combined, for example, all physiological outcomes into one superordinate category of physiological measurements. As stated in Lench et al. (2011, p. 838),

> Each of these broad categories includes multiple specific outcomes and measures that could each serve as the focus of a separate review. We chose to focus on the fundamental issue of whether emotions elicit changes across systems, and we therefore classified outcomes within relatively broad categories.

A review at this broad level was necessary to address the broad questions of whether discrete emotions elicit changes and if those changes are correlated. We agree that more specific reviews will be useful in addressing more specific questions and outcomes, and we have several reviews using the meta-analytic data in preparation that examine only one type of outcome or elicitation. We welcome contact from anyone interested in collaboration to use the data to examine specific theories, outcomes, or elicitations.

Lindquist et al. (2013) went on to state that we claimed to have “ruled out valence and arousal as alternative explanations for their [our] findings” (p. 259). Again, we cannot identify the source for this statement. What we actually said was the following:

> The effect sizes associated with all of the comparisons among discrete emotions were greater than zero, although the effect sizes associated with the comparison between anger and anxiety and anger and neutral conditions were quite small. The small difference between anger and anxiety may be due to an underlying similarity, perhaps on an arousal dimension, as suggested by valence and arousal accounts of emotion.

It is also possible, however, that emotion elicitations have not effectively targeted one emotion. (Lench et al., 2011, p. 849)

We later stated that the large difference between happiness and all negative emotions as well as the small effect between anxiety and anger suggest that valence and arousal models capture important information about the differences among emotions (Lench et al., 2011, p. 850). But we noted that the difference between sadness and anxiety was also small, which is not clearly accounted for by valence/arousal models. We also stated that some conflicts with a valence/arousal model were resolved by including whether outcomes were self-report or not but that some findings remained inconsistent with a valence/arousal model. Lindquist et al. (2013) misconstrued these statements as claims that valence and arousal were ruled out as contributors to emotion. Perhaps this confusion arose from the fact that the Lench et al. (2011) review started with discrete emotion theory. We began with a discrete emotion account because, to our knowledge, it is the only theory that offers predictions about how specific emotions should affect outcomes. Other emotion models could be evaluated in terms of how well they account for differences among emotions (Lench et al., 2011, p. 835). However, valence/arousal models generally are descriptive and only make predictions about how similar various emotions should be, failing to predict what changes would be expected with different emotions or different levels of valence and arousal (for a review, see Barrett & Russell, 1999).

In Lench et al. (2011), we were not seeking to support or refute individual theories but rather to evaluate evidence for broad theory classes that make similar assertions. We included detailed tables in the hope that individual theorists could evaluate their theory in terms of the evidence—and we were thrilled that Lindquist et al. (2013) did so. In their Figure 1, they map effect sizes from our meta-analysis onto valence and arousal dimensions. Valence/arousal models are often silent about the size of the differences among emotions (although they are often mapped with self-reports of emotion, Barrett & Russell, 1999), and we were excited by the possibility that meta-analytic effects could provide this information in dimensional maps of emotion. Future attempts along these lines should also incorporate the distance from each emotion to the neutral point on the dimensions. Such a process may offer refinements to models that plot emotions along valence and arousal dimensions. However, additional evidence is necessary to determine whether the valence and arousal associated with each emotion predicts the differences in outcomes. To do so, one would need to gather data, in either an empirical study or a review, on valence and arousal ratings, plus differences in cognition, judgment, experience, behavior, and physiology. Tests would then focus on the extent to which the intensity of valence and arousal predicts changes.

**Evidence for a Psychological Constructionist Approach**

The main argument of Lindquist et al. (2013) centered on their perspective that emotions are psychological constructions rather than natural kinds. Although the surrounding debate is interesting, we believe only one finding in the meta-analysis is relevant—that effects were largest for self-reported emotions versus other outcomes—and therefore we discussed this view only briefly (and did not discuss issues of volition). Lindquist et al. argued that this
finding supports the idea that emotions are psychologically con-
structed. We agree with this statement, having made this point several times in Lench et al. (2011). However, we believe that this is the only part of the meta-analysis that can speak to this debate, and we note that whether this finding supports a constructionist approach “remains to be directly tested” (p. 850). We made this qualifying statement because the finding does not offer unequivocal support for a psychological constructionist perspective. First, the difference in effect size between self-reported outcomes and outcomes that are not self-reported might reflect the fact that self-reports are more subject to demand effects than other outcomes, and thus, the difference might reflect attempts to meet experimenter expectations rather than construction of emotions. Second, we would caution against overinterpreting the finding that effects for physiological and behavioral outcomes for negative emotions did not significantly differ from zero (Table 3 in Lench et al., 2011). Several of these comparisons are based on a very small number of studies, as few as five. The effect sizes are small, but other effects of a similar size reached significance when they were associated with more studies. As with other analyses, the magnitude of an effect in a meta-analysis must be larger to reach significance when the degrees of freedom are small. We and Lindquist et al. are likely to agree that additional studies examining the effects of discrete negative emotions on physiological and behavioral outcomes are needed. The lack of such studies is driven in part by a focus on elicitions of negative affective states (such as by presenting a mix of negatively valenced pictures that elicit sadness, anger, and disgust) rather than discrete emotions. Perhaps Lench et al. will prompt more researchers to examine physiological and behavioral outcomes associated with discrete emotions.

We went on to note that the results suggest anxiety in particular did not “influence outcomes other than self-report in ways that differed from other negative emotions” (Lench et al., 2011, p. 850). Although most discrete emotion theories focus on fear (as pointed out by Lindquist et al., 2013), very few laboratory elicitations included fear (the experience of an impending threat), with the exception of a few older studies in which participants’ chairs suddenly dropped backward. Most elicited anxiety (the anticipation of a threat), and thus our review included anxiety rather than fear. It is possible that evidence for a discrete emotion perspective would be stronger with fear manipulations, but it is difficult to imagine how fear might be elicited and still allow participants to complete self-reports, cognitive tasks, or behavioral measurements. Fear conditioning might be useful in this regard by pairing some stimulus with the experience of a threat and then exposing participants only to the stimulus while they complete other tasks.

To support their psychological construction interpretation using other evidence in the meta-analysis, Lindquist et al. (2013) presented two additional arguments. (1) They presented evidence that the method carried more weight than discrete negative emotions. This was an interesting application of a multitrait (emotion), multimethod analysis. However, one must be cautious when examining effects broken out this way because, as noted in the tables of Lench et al. (2011), some of these differences were based on a small number of studies. Also, the fact remains that the effect sizes associated with comparisons among negative emotions, though small, all significantly differed from zero. (2) They argued that all outcomes must be perfectly correlated, accounting for measurement error, to support a discrete emotion account. The outcomes were certainly not perfectly correlated in our analysis of the relationship among changes in outcomes (Lench et al., 2011, p. 844). However, we do not believe the lack of perfect correlation supports a psychological construction account. Rather, there are likely to be multiple situational and individual variables that moderate the influence of any experimental elicitation of emotion on any outcome. We know of no modern discrete emotion accounts that are so inflexible as to claim that emotions are deterministic, such that, for example, anger always results in aggressive behavior. Rather, the proposition is that discrete emotions increase the likelihood of particular responses, such that anger increases the likelihood of aggressive behavior (whether this tendency is fulfilled depends on a number of moderators; e.g., Frijda, 1987; Izard, 2007; Lerner & Keltner, 2001; Panksepp, 2007). Lindquist et al. might also argue the opposite hypothesis for a psychological constructionist view (not explicitly stated, but suggested on p. 261; see, however, Barrett, 2006)—that changes associated with each discrete emotion should be arbitrary and uncorrelated. The results of the meta-analysis do not support this hypothesis, instead revealing that changes associated with each emotion that are consistent with predictions derived from a functional discrete emotion account, and those changes are partially intercorrelated.

The Discrete Emotion Perspective

We did not imply and do not now suggest that Lench et al. (2011) provided evidence for emotions as natural kinds. The issue of whether discrete emotions predict changes in outcomes consistent with the theorized function of each emotion is separable from whether these emotions are natural kinds. We also do not believe that most discrete emotion theories would make the predictions described by Lindquist et al. (2013) as representing a natural kind view of emotion (see also Barrett, 2006, and comments by Izard, 2007, and Panksepp, 2007).

Discrete emotion theories often posit that emotions evolved to meet the challenges of particular situations with significance for survival and adaptation (e.g., Izard, Levinson, Ackerman, Kogos, & Blumberg, 1998; Mauss, Levenson, McCarter, Wilhelm, & Gross, 2005; Pinker, 1997; Plutchik, 2000). However, the idea that emotions have evolved (and thus have some biological basis that can be passed through genes) does not necessitate completely unique physiological profiles for each emotion (e.g., Panksepp, 2007). Consider, for example, anxiety and anger. According to a discrete emotion perspective, both emotions motivate people to deal with threats and are thus likely to be associated with physiological arousal and may have similar physiological profiles on some measures. One might expect anxiety to increase arousal more than anger, but this would necessarily depend on the intensity of the emotion elicited.

Further, as eloquently described by Gould and Lewontin (1979), general adaptations that have an evolutionary basis can be realized through specific responses that are not necessarily biologically based. Discrete emotions may have evolved as coordinated responses to certain states of goals often encountered in the environment, including successes, failures, and threats. However, the emotions felt in those situations and the ways in which those emotions influence behavior, physiology, cognition, and experience, may differ based on experiences within a particular environment and culture. Thus there may be a genetic basis for certain
types of emotions that arise in particular situations, but the phenotype—how that genetic predisposition is realized in observable reactions—would depend on experience, with certain types of responses likely to be adaptive to deal with certain types of situations. Accordingly, one would expect some similarity across individuals and cultures. Similar arguments have been made for gender differences that may have an evolutionary basis. For example, men report greater concern for sexual infidelity (e.g., Larsen & Buss, 1991; Wilson & Daly, 1992). This does not mean, however, that these differences have a biological basis. Rather, they might result from an evolved tendency to detect cheating behavior of all types (e.g., Pinker, 1997). How this general tendency is enacted depends on experience within particular cultures and environments, with some similarities arising across cultures and individuals due to similar constraints on men and women (Levy & Kelly, 2010).

Lindquist et al. (2013) stated that to suggest emotions are natural kinds (which we did not suggest in our meta-analysis) is to hypothesize one of two things: “that each category has a biological essence that causes it” or “that instances of emotions have some cluster of properties . . . that recur with sufficient consistency and specificity as to be diagnostic for that category” (p. 255). The meta-analytic evidence of Lench et al. (2011) can be interpreted in light of this second hypothesis and suggests that discrete elicited emotions are associated with moderate changes across systems in ways predicted by a functional discrete emotion account and that these changes are partially correlated. However, we disagree with Lindquist et al. about whether these findings can be interpreted as evidence of “natural kinds.” This line of argument requires tautological reasoning in which we are not willing to engage; namely, discrete emotions predict changes in outcomes, therefore there are discrete emotions. Rather, we believe that the findings support the idea that emotions represent coordinated adaptations to problems. This coordination may arise because emotions are natural kinds in a philosophical sense or because emotions represent a set of responses that are generally effective in the type of situations that give rise to each emotion (but experiences within particular environments matter). The question raised by Lindquist et al. regarding where the differences among emotion come from is certainly interesting, but, again, cannot be clearly addressed by the evidence in Lench et al.

Lindquist et al. (2013) asked whether Lench et al. (2011) should be regarded as the “definitive word on the matter or merely another battle” (p. 256). We suggest that our meta-analysis did resolve one issue—the extent to which discrete emotions predict changes in outcome—which had to be resolved before the next set of questions about why emotions predict changes can be answered.

Conclusions

Lindquist et al. (2013)—admittedly tongue in cheek—characterized the debate over the nature of emotion as a war accompanied by pain and misery but also by great progress. One consequence of viewing this debate as a war is that it may result in the misinterpretation of evidence. We do not believe that Lindquist et al. intentionally mischaracterized our statements, but we contend that strong opinions alter how information is interpreted. In a classic study, Vallone, Ross, and Lepper (1985) showed pro-Israeli and pro-Arab participants the exact same news coverage of a Beirut massacre. All participants, regardless of affiliation, viewed the coverage as biased against their side and hostile to their views. Findings such as these suggest that strong supporters of a particular ideology may view neutral statements or information as biased against them. The main outcomes of war are misunderstandings and casualties. The outcome of a scientific debate should be a logical resolution that incorporates evidence from all sides, based on the strength of that evidence. We believe such a resolution is possible both in terms of whether discrete emotions predict outcomes and whether discrete emotions represent psychological constructions (see Izard, 2007, for one such attempt), but evidence is required for progress to be made.

References


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