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**Cognition and Emotion: A Plea for Theory**

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**Abstract**

Research on cognition and emotion during the past 30 years has made reasonable progress in theory, methods and empirical research. New theories of the cognition-emotion relation have been proposed, emotion research has become more interdisciplinary, and improved methods of emotion measurement have been developed. On the empirical side, the main achievement of the past 30 years is seen to consist in the reduction of the set of serious contenders for a theory of emotions. Still, several important issues are not fully resolved, including the computational implementation of appraisal processes, the nature of emotions, and the link between emotions and actions. Also, quantitative theories of the cognition-emotion relation need to be refined and tested, and improved theories of the link between emotions and bodily and facial expressions need to be developed. To counter the dangers of theoretical fragmentation and knowledge loss, more efforts should be devoted to the analysis, reconstruction, comparison and integration of important theories and hypotheses in the field of emotion, as well as to the systematization of arguments in favor and against these theories and hypotheses.

Although research on cognition and emotion during the past thirty years has perhaps not advanced as quickly and linearly as some of us may have hoped, I believe that the field has made reasonable progress in theory, methods and empirical research.

**Progress in Theory.** If I were to name the three most important books that have appeared in the field of cognition and emotion in the three 10-year periods since 1988, I would probably choose Ortony, Clore and Collins (1988), Scherer, Schorr, and Johnstone (2001), and Miceli and Castelfranchi (2015). To readers familiar with these books, this selection will make clear that I see the development of new and improved theories about the cognition-emotion relation as one of the most important tasks of the field; particularly of theories inspired by cognitive science. The reason is that the development of methods and the conduct of empirical studies are not aims in themselves but only means to the superordinate goal of science, the construction of valid theories about a domain of inquiry. And, counter to the claims of some empiricist philosophers of science, theories do not usually emerge from data by a process of inductive generalization. Empirical studies are indispensable for testing theories, but of limited value for devising them in the first place. Creating and improving theories is a task of its own.

So where do psychological theories come from? There are certainly several sources: Previous theories in the same field or related fields, creative insights, inferences to the best explanation of a phenomenon, and more. However, in the case of cognitive emotion theory (as well as many other psychological theories, including most theories of motivation), I would argue that the original and main source of theoretical inspiration has been the implicit psychology of common-sense. Common-sense psychology has often been disparagingly described by psychologists as a set of contradictory proverbs, but systematic research into its structure (e.g., Heider 1958; see Reisenzein & Rudolph, 2008a) has shown that, to the contrary, it consists of a highly sophisticated system of assumptions about the human mind.

These assumptions comprise, among others, a cognitive theory of emotions. This theory is the basis of the description, explanation, prediction and control of emotions in everyday life, and it is implicitly also relied on for the measurement and induction of emotions in scientific research (Studtmann, Otto, & Reisenzein, 2009).

Although behaviorist and neurophysiological emotion researchers have repeatedly argued that the science of emotion should ignore or even reject common-sense psychology and start from scratch (e.g., Watson, 1919; LeDoux, 2014), the reasons given for this eliminativist strategy are unconvincing (see already Arnold, 1960) and its prospects are, in my view, dim. The alternative approach, taken at least tacitly by most cognitive emotion theorist, is compatibilistic: The implicit common-sense theory of emotions is accepted as being correct at least in the outlines, and an attempt is made to erect a scientific theory of emotions on its basis.

To develop cognitive emotion theory within a scientific framework, at least three routes can and have been taken. The first, necessary route consists of explicating, clarifying, refining, and if necessary correcting, the emotion-theoretic postulates of common-sense psychology on a qualitative level of analysis (e.g., Green, 1992; Ortony et al., 1988). This project may culminate in a qualitative emotion model expressed in a formal language (see e.g., Adam, Herzig & Longin, 2009). The second route, which extends the first, consists of attempts to refine the qualitative theory into a quantitative model of the cognition-emotion relation (see e.g., Mellers, 2000; Reisenzein, 2009a). The third route consists of supplementing the resulting, qualitative or quantitative "intentional-level" theories by "design-level" assumptions (Dennett, 1971), i. e. assumptions about the representations and the operations on these representations, that underlie the intentional-level laws.[1] Design-level

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[1]A fourth route to the development of a scientific emotion theory, already taken by the pioneer of appraisal theory, Magda Arnold (1960), is to supplement the psychological

assumptions provide not only for a deeper understanding of emotion processes (Reisenzein, 2009a); they also provide the basis for constructing computational models of emotions, models that can then be simulated to study the dynamics of emotion processes (Scherer, 2009), and that can be implemented in artificial agents to make them more human-like and possibly also, more intelligent (Macedo, Cardoso, Reisenzein, Lorini, & Castelfranchi, 2009; Reisenzein, Hudlicka, Dastani, Gratch, Hindriks, Lorini, & Meyer, 2013). All three described routes to a scientific cognitive emotion theory have been pioneered, or advanced, in the three books mentioned at the beginning of this essay, as well as in several other books and many articles (for overviews see e.g., Scherer, Bänziger, & Roesch, 2010; Reisenzein, Hudlicka et al. 2013).

My own main contribution to this research consists of a sketch of a computational model of the cognitive-motivational, or belief-desire, theory of emotions (e.g., Reisenzein, 2001; 2009a; 2009b). This model integrates two previous theories: the belief-desire theory of emotions, initially developed by philosophers (e.g., Green, 1992), and a theory of surprise proposed by Meyer, Reisenzein and Schützwohl (1997; for a recent review see  Reisenzein, Horstmann, & Schützwohl, 2017). The belief-desire theory of emotions differs from the classical and arguably still standard version of appraisal theory (though see Moors & Scherer, 2013) in assuming that appraisals as understood in the classical theory (i.e., as evaluative beliefs; Arnold & Gasson, 1954; Lazarus, 1966) are in fact not necessary for emotions. Rather, emotions are assumed to be directly caused by factual (nonevaluative) beliefs and desires (motives). In the extended belief-desire model, a parallel assumption is made for surprise, which is held to be directly caused by the comparison of pre-existing to newly

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postulates with assumptions about the brain structures and processes that implement emotions. However, this route is in my view more important for testing theories of emotion than for devising them in the first place.

acquired beliefs. This extended belief-desire theory allows to account for a set of basic forms of emotion (happiness and sadness, pleasant, neutral and unpleasant surprise, relief and disappointment, fear and hope) in a parsimonious way: All that is needed are beliefs and desires, plus a time index (e.g., present belief–past belief). Furthermore, it is possible to formulate quantitative laws that describe how the intensity of these emotions is related to the intensity of their cognitive (belief) and motivational (desire) bases (see Reisenzein, 2009a).

Assuming that the belief-desire theory of emotion generation is broadly correct, how might it be computationally implemented? My answer to this question (e.g., Reisenzein, 2009a) is as follows: The emotions within the scope of the theory are generated by hard-wired mechanisms whose function is to support the monitoring and updating of the belief-desire system, the central representational system of humans. The emotion mechanisms are regarded as being similar to sensory transducers; however, instead of sensing the world, they sense the state of the belief–desire system and signal important changes in this system. More precisely, the emotion mechanisms continuously compare, at an unconscious level of processing, newly acquired beliefs with existing desires and beliefs, and generate outputs that signal the detection of actual or potential fulfillments or frustration of desires and the confirmation or disconfirmation of beliefs. These output signals, in turn, trigger a set of characteristic effects in the cognitive system that serve to prepare it to deal with the detected belief- and desire matches and mismatches (Reisenzein, 2009a). Note that this theory agrees with Frijda's (1994) proposal that the emotion mechanisms are at core "concern relevance detectors", but extends Frijda's proposal to the detection of "epistemic relevance".

You may call the proposed relevance detectors "appraisal mechanisms" if you want; but note that their outputs are not beliefs (e.g., "something good has happened" in joy or "something unexpected has happened" in surprise), but nonpropositional signals similar to sensations (Oatley & Johnson-Laird, 1987). Furthermore, these signals—the immediate

outputs of the proposed appraisal mechanisms—are *identified* with emotions in the theory; so if appraisals are the immediate products of appraisal processes, then emotions *are* appraisals. Specifically, it is assumed that, if the emotion signals exceed a threshold of intensity, they are experienced as feelings of hope and fear, pleasure and displeasure, expectancy confirmation and surprise, as well as feelings resulting from an unconscious integration of these signals, such as relief and disappointment.

Although the described emotion model is still a cognitive theory of emotions, it is clearly less cognitive than the original appraisal theory proposed by Arnold and Lazarus—it is, in effect, an appraisal theory without appraisal as originally conceived of (as evaluative belief). This change of assumptions allows the theory to rebut, at least in part, the central criticism raised against classical cognitive emotion theory, the objection that it "overintellectualizes" the emotions (e.g., Zajonc, 1980) (see Reisenzein, 2009a).

**Progress in Methods**

The theoretical advances in the field of cognition and emotion have been fostered by the gradual development of an interdisciplinary "affective science" (see Calvo, D'Mello, Gratch, & Kappas, 2015; Sander & Scherer, 2009), an important part of which is the computational modeling of emotions. The establishment of this interdisciplinary field is in my view the most significant methodological (if can be so called) advance of emotion research in the past 30 years. To be sure, affective science is still in an early stage; however, there is clearly an increasing exchange between the different disciplines interested in emotions, including, apart from psychology and cognitive science, in particular philosophy (see Scarantino & de Sousa, 2018).

Closely connected to the emergence of affective science is the development of improved and new methods for assessing emotions, such as the development of automatic affect detection methods (see Calvo et al., 2015). Most of the new measurement methods

focus on the inference of emotions from behavioral indicators. However, given that the most frequently used and most sensitive methods for assessing emotions are currently still based on self-reports, the improvement of these measurement methods is at least equally important. Some progress has been made in this area as well (see e.g., Pekrun & Bühner, 2014). As one example, I have developed, together with Martin Junge, an improved method for measuring the subjective experience of emotions that is based on an indirect scaling procedure (graded pair comparisons; Junge & Reisenzein 2013; 2015; 2016). We have provided evidence that the scale values of emotion intensity obtained with this method have a higher reliability than the usual rating scales (Junge & Reisenzein 2013; 2015) and even seem to achieve a metric scale level (Junge & Reisenzein, 2016). This makes this scaling method well suited for testing quantitative emotion theories. Indeed, substantially improved fits of quantitative cognition-emotion models for disappointment and relief were obtained with this measurement method, compared to rating scales (Junge & Reisenzein, 2013).

**Progress in Empirical Research**

On the empirical side, in my view the main achievement of the past 30 years consists, on a very general level, of the empirical reduction of the set of serious contenders for a theory of emotions. In a review of philosophical discussions of the mind-body problem after 1945, the philosopher Pauen (2002) concluded that, although the puzzle of the relation between mental states and physical/brain states has not yet been solved, progress has been made on two fronts: The different proposed theories of the mind-body relation have been clarified, and arguments have accumulated that make the majority of these theories unlikely, leaving only very few serious contenders. Somewhat analogously, I believe, emotion psychology has progressed during the past 30 years by the clarification of the assumptions of major emotion theories (or classes of theories) and the accumulation of empirical evidence which suggests that at least some of these theories are improbable. This is the case—in my view at least—for

the group of Jamesian and Neo-Jamesian theories of emotion (see Reisenzein & Stephan, 2014), including their physiological feedback, facial feedback, and cognition-arousal variants (e.g., Schachter, 1964; see Reisenzein, 2017); as well as for basic emotions theory, at least as originally proposed (e.g. Ekman, 1972; Izard, 1971).

**Bodily feedback theories**. The core assumption of bodily feedback theories is that feedback from the body, in particular from physiological reactions and/or facial expressions, is an essential component of emotional experiences, or at least contributes importantly to them. However, the empirical evidence provides only very modest support for these hypothesis.

Regarding first physiological arousal feedback, the accumulated data on (a) the emotional experiences of people suffering from reductions of physiological feedback (e.g., paraplegics) and (b) the effects on emotions of beta-blocking agents (drugs that reduce peripheral, in particular cardiovascular, arousal), suggest that physiological feedback is most likely not necessary for emotional experience (Reisenzein & Stephan, 2014). And although experiments on the effects of induced arousal—conducted mostly in the context of research on Schachter's (1964) cognition-arousal theory—suggest that arousal can have an intensifying effect on emotional experience, this effect has been found to be weak and context-dependent (Reisenzein, 1983; Leventhal & Tomarken, 1986; Parkinson, 1995).

Parallel conclusions can in my view now be drawn for the facial feedback hypothesis (e.g., Laird 2007). This hypothesis has meanwhile been tested in more than 130 experiments in which facial expressions were manipulated and the effects of these manipulations on emotional experience and (presumably) affect-based judgments of objects were measured. A recent meta-analysis of these studies concluded that the facial feedback effect is statistically reliable (Coles, Larsen, & Lench 2017). However, it also found that the effect is on average very small (about 1% explained variance), suggesting that is of very limited importance.

Furthermore, alternative, nonemotional explanations of facial feedback effects have been proposed (see Reisenzein & Stephan 2014).

Finally, all bodily feedback theories suffer from an unsolved theoretical problem: While feedback from the body is supposed to provide emotional experiences with "affective warmth" (James, 1890/1950), it appears that emotion-relevant bodily changes cannot be distinguished from nonemotional ones in a noncircular way (i.e., without referring back to emotions) (Reisenzein & Stephan, 2014).

**Basic emotions theory**. The central testable prediction of basic emotions theory—which has served as the background theory for most versions of bodily feedback theory—is that a small subset of human emotions, the basic emotions, are reliably associated with distinct patterns of bodily, in particular facial, reactions (e.g., Ekman, 1972).

In conflict with this prediction, laboratory and field studies of spontaneous facial expressions, conducted mostly during the past 35 years, have found that the facial expressions allegedly associated with basic emotions occur only rarely in individuals who have these emotions, and if they occur, are usually partial rather than complete—even if the conditions for the emergence of the expressions are favorable (in particular: solitary situations, strong emotions) (for overviews, see Reisenzein, Studtmann & Horstmann, 2013; Durán, Reisenzein & Fernández-Dols 2017). The empirical evidence for these conclusions is particularly good, I believe, for surprise (e.g., Reisenzein, 2000a; Reisenzein, Bördgen, Holtbernd, & Matz, 2006; Schützwohl & Reisenzein, 2012); but parallel conclusions are also suggested by the data available for other basic emotions (happiness, sadness, disgust, anger, and fear) although the data base is less strong in these cases (Durán, Reisenzein, & Fernández-Dols, 2017). High coherence between emotional experience and facial expression, at least in some situations, has to date been reliably documented only for amusement and smiling (Reisenzein, Studtmann, & Horstmann, 2013); but amusement is not usually regarded

as a basic emotion. Incidentally, these findings further support the conclusion that facial feedback does not play much of a role for the experience of emotions (see also, Reisenzein & Studtmann, 2007).

Finally, studies of peripheral-physiological changes in emotions have found even less coherence between emotional experience and bodily reactions (e.g., Mauss and Robinson 2009; see also Lang, 2014).

**Noncognitive emotion elicitation and affective primacy**. Similar "falsificationist" conclusions to those proposed above for bodily feedback theories and basic emotions theory can now also be drawn, in my view, with regard to the anti-cognitive hypotheses that were at the focus of the cognition-emotion debate (Zajonc, 1980; Lazarus, 1981): the affective primacy hypothesis (Zajonc, 1980), and the closely related hypothesis of a noncognitive route to the elicitation of emotions (or at least some emotions, specifically fear; Öhman and Mineka 2001; LeDoux, 1998). The main initial support for these hypotheses were studies that allegedly showed that emotional pictures can elicit pleasant and unpleasant affect (e.g., Murphy & Zajonc, 1993), as well as physiological reactions suggestive of an emotion (e.g., Öhman & Soares 1994), even if the stimuli are presented in ways (very briefly and masked) that prevent them from being consciously recognized.

However, more recent studies that have used a more fine-grained methodology, in which the presentation times of emotion-evoking pictures were systematically increased from clearly below to clearly above the detection threshold, have found that the affective valence of the pictures could only be detected (Lähteenmäki, Hyönä, Koivisto & Nummenmaa 2015), and physiological reactions only occurred (Peira, Golkar, Öhman, Anders, & Wiens 2012), if the depicted objects were at least partly consciously recognized. Furthermore, contrary to the claim that a noncognitive path to fear has an evolutionary advantage because the cognitive path to fear is slow (LeDoux 1998), it has been found that emotion-evoking and other objects

can in fact be semantically categorized extremely quickly (Nummenmaa et al., 2010; Lähteenmäki et al. 2015; Kirchner & Thorpe, 2006) and more quickly than affective valence can be detected (Nummenmaa et al, 2010). These findings are in agreement with conclusions from neurophysiological studies (see Lähteenmäki et al., 2015; Storbeck, Robinson, and McCourt, 2006).

Finally, it should be noted that, even if the noncognitive emotion elicitation and affective primacy hypotheses were true for the highly restricted range of feelings typically evoked by affective pictures (cf. Studtmann, Otto, & Reisenzein, 2009), they are a priori implausible for most paradigmatic emotions. Again, surprise is a good example: Contrary to noncognitivism, surprise is a paradigm of a cognitively evoked emotion: it arises if explicit or implicit beliefs are disconfirmed (see Reisenzein, Horstmann, & Schützwohl, 2017).

**A note on the replicability question.** Note that the preceding falsificationist conclusions are suggested by the evidence now available even if the validity of the data presented in support of the discussed hypotheses is not questioned: these data (e.g., on cognition-arousal theory, facial feedback, noncognitive emotion elicitation) can be interpreted in alternative ways (e.g., Lähteenmäki et al., 2015; Reisenzein & Stephan, 2014). The recent replicability crisis in psychology (see Świątkowski & Dompnier, 2017, for a recent overview) suggests, however, that if these data bases were probed for their replicability, the skeptical conclusions that I arrived at would become even firmer (for support in the case of the facial feedback hypothesis, see Schimmack & Chen, 2017; Wagenmakers, Beek, Dijkhoff, & Gronau, 2016).

From the perspective of cognitive emotion theory, these conclusions can actually be welcomed, because they reduce the set of empirical phenomena that a cognitive theory of emotion needs to explain. Finally, note that even assuming that the effects under discussion are robust, the fact that they seem to be quite small and context-dependent could be a

sufficient reason for cognitive emotion researchers to disregard them in their theories. After all, all theoretical models are simplifications of a more complex reality.

**Cognitive emotion theories**. Within the class of cognitive emotion theories, too, empirical as well as theoretical arguments have accumulated during the past thirty years which suggest, to me at least, that certain members of this theory class are more plausible than others (see Reisenzein, 2009a; 2012). In particular, arguments have been advanced that cognitive-motivational theories (see e.g., Green, 1992; Reisenzein, 2009; Miceli & Castelfranchi, 2015) that make do without the concept of appraisal as originally conceived of (i.e. as evaluative belief; Arnold & Gasson, 1954; Lazarus, 1966) are more plausible than the classical appraisal theory (see Miceli & Castelfranchi, 2015; Reisenzein, 2009a). Likewise, I believe that cognitive emotion theories that conceptualize emotions as unique kinds of mental states are more plausible than componential theories (Reisenzein, 2012).

## How to Counter Theoretical Fragmentation and Knowledge Loss

During the past 30 years, emotion research in general, but also the more specific field of cognition-emotion research, has been moving not in just one, but in several different directions. To the degree that diversification and the creation of variety is a mark of scientific progress, this is a positive development. However, to avoid fragmentation of the field, the centrifugal forces need to be held in check by contrary centripetal ones; and these, I believe, are currently too few, and too weak. This fact is in part responsible, I believe, for the impression of some outside observers that emotion psychology is going in cycles, coming back again and again to the same "perennial problems of emotion" (Arnold, 1970). However, another reason for the impression of some observers that emotion research proceeds somewhat slowly is that over the past 30 years, knowledge has not only be gained, but also seems to have been lost again. For example, claims that have already been disproven, or can at least be considered very unlikely given the available evidence and the associated history of

critical discussion, nonetheless reappear at regular intervals in the emotion literature, being sustained by a strategy of selective citation and the simple ignoring of valid objections that have been raised. This practice, which did not exist to the same extent 30 years ago, should be discouraged—it should be added to the list of questionable research practices that have come to the fore in the recent replicability debate (Świątkowski & Dompnier, 2017).

To counter the dangers of theoretical fragmentation and knowledge loss, we need more efforts devoted to the clarification, explication, reconstruction, comparison and integration of theories and hypotheses in the field of emotion, as well to the systematization of empirical and theoretical arguments in favor of and against these theories and hypotheses. I have tried, together with several co-authors—some of them philosophers or computer scientists—to contribute to these tasks in several reconstructions and critical discussions of important historical and contemporary emotion theories. These include James's theory of emotion (Reisenzein and Stephan, 2014), Wundt's theory (Reisenzein, 2000b); Meinong's theory (Reisenzein et al., 2003); Darwin's and Freud's theories (Reisenzein, 2018), Schachter's cognition-arousal and related theories (Reisenzein, 2017), Arnold's theory (Reisenzein, 2006); Weiner's attributional theory (Reisenzein et al., 2003), different versions of the belief-desire theory of emotion (Reisenzein, 2012), and computational models of emotions (Reisenzein, Hudlicka et al., 2013). While these theory reconstructions are for the most part informal (but see Reisenzein, 2000b), there are now also precise reconstructions of emotion theories in formal languages developed by logicians and computer scientists (e.g., Adam, Herzig, & Longin, 2009; see Reisenzein et al., 2013 for an overview). Computational emotion models implemented in agent architectures achieve the same goal by different means (see Reisenzein et al., 2013). In the same spirit, software tools for argument mapping (e.g., ter Berg, van Gelder, Patterson, & Teppema, 2013; Betz, Bohse, and Voigt, 2007) could be used to improve the systematization of important argumentations and debates of emotion research.

To support these efforts, *Cognition and Emotion* might consider opening up a regular special section *Emotion Theory*. Together with the journal *Emotion Review*, this could help to reach a larger goal: to formally establish a theoretical division of emotion science as part of a broader theoretical psychology. An official theoretical branch of psychology is in fact long overdue: Whereas most other scientific disciplines have such a theoretical branch (i.e. there is a theoretical physics, a theoretical biology etc.), there is still no institutionalized theoretical psychology.

**Questions that still need to be resolved**

Over a decade ago, I concluded a contribution to a symposium in honor of Magda Arnold with a list of questions about cognitive emotion theory that still needed to be resolved (Reisenzein, 2006). These questions were: (1) Can cognitive emotion theory explain all affective experiences or only a subset of them? (2) Are emotions really caused by evaluative beliefs, as assumed in classical appraisal theory (Arnold & Gasson, 1954; Lazarus, 1966), or are they directly caused by beliefs and desires (motives), as assumed by the belief-desire theory of emotion? (3) What, exactly, are emotional experiences, if the cognitive theory of emotion elicitation is right? (4) Exactly what kinds of appraisals (or beliefs and desires) are required for emotions in general, and precisely which appraisal patterns characterize different emotions? (5) How, exactly, do emotions influence actions?

Today, these questions are still not fully resolved. I have committed myself to particular answers to them in the computational model of the belief-desire theory described earlier: This theory explains "propositional" emotions (the paradigmatic emotions of common-sense, such as joy, sadness etc. that are directed at states of affairs), but not sensory feelings (those evoked by simple sensations); emotions are directly caused by factual beliefs and desires without intervening evaluative beliefs; and the emotional feeling is the direct result of the comparison of newly acquired beliefs to existing beliefs and desires.

Regarding the patterns of beliefs and desires required for specific emotions, I proposed such patterns—largely drawing on extant proposals—for a small set of emotions (pleasure and displeasure, hope and fear, pleasant, unpleasant and neutral surprise, disappointment and relief; Reisenzein, 2009a) and I discussed how social and moral emotions (e.g., pity, guilt) could be integrated into the belief-desire framework (Reisenzein, 2015a). Finally, regarding the question of how emotions influence actions, I did not have a complete answer but proposed that the outputs of the emotion-generating mechanisms, the affective signals, influence actions not just by the hedonistic route (see e.g., Baumeister, Vohs, deWall, & Zhang, 2007; Mellers, 2000; Zeelenberg & Pieters, 2007), but also by directing attention to the evoking events, by updating beliefs and desires, and—possibly mediated by these processes—by creating emotion-specific action desires (see also Reisenzein, 1996; 2015b).

Although I believe that there are reasonably good arguments for these proposals (Reisenzein, 1996; 2009a; 2012), this does of course not mean that they are correct. Thus, these questions remain on the agenda.

In addition to further clarifying the patterns of appraisals (or beliefs and desires) underlying specific emotions on a qualitative level, another important task for future research is the refinement and testing of quantitative models of the relation between beliefs and desires on the one hand, and emotions on the other hand. Such models are needed to account for the intensity aspect of emotions (e.g., Gratch et al., 2009; Junge & Reisenzein, 2013).

On the "downstream" side of emotion, there is the need to clarify not only the relation between emotions and actions (see above; and Moors, Boddez, & De Houwer, 2017), but also—given that, as argued above, classical basic emotions theory has become doubtful—to reexamine the relation between emotions and their bodily expressions (Reisenzein, Studtmann, & Horstmann, 2013; Scherer, 2009). For example, although the above-reviewed evidence suggests that the link between emotions and facial expressions is too weak to

support basic emotions theory, this evidence still supports the assumption that emotions and facial expressions are moderately related; hence, there is still something that needs to be explained. However, although several alternative theories of the emotion-expression link have been proposed (see Reisenzein, Studtmann, & Horstmann, 2013), none seems at present able to fully explain the data.

### Advice to Young Investigators

As suggested by the editors, I conclude my reflections on the state of the field of cognition-emotion research with a few suggestions for young researchers. It will come as no surprise that these suggestions echo my general predilections, already revealed before in this article.

My first advice is: Before you embark on an own research project, be it theoretical or empirical, try to digest and understand the literature and history of the issue in question thoroughly (hence, I definitely advise against "think of an interesting everyday phenomenon and then do an empirical study on it"). Your reading should, ideally, also include research from neighboring disciplines such as computer science, philosophy or economics. Thorough familiarization with a topic will often also include studying classical writings: The history of cognitive emotion theory from Aristotle to Hume and Spinoza up to and including the psychological literature of the late 19th century is very rich in ideas about emotions, many of which still await resuscitation.

My second advice: Focus on theory development; don't rush to the lab too soon. This includes: Think theories through, compare them with related theories and with your own prior knowledge to see whether they are consistent, complete, and plausible. In this context, keep in mind that, as a competent user of common-sense psychology, you already know—if implicitly—a great deal about emotions and about the mind in general; try to use that knowledge, as well as your ability to engage in mental simulation, for thought experiments

(cf. Heider, 1983). You may also wish to improve your skills in theory reconstruction and argument analysis by taking courses in logic and argumentation theory. Learning some formal representation tool, such as an agent logics or a simulation language for cognitive agents (see Reisenzein et al., 2013) will be helpful; even if you do not plan to use formal reconstructions or computer simulations in your work, familiarity with these tools will help you think more clearly about psychological issues.

Third, when selecting a research topic, consider that to understand the human mind, one needs first and foremost to study its common, everyday workings.

Fourth, because emotion theories—like most psychological theories—are formulated on the level of the individual, test them on that level whenever possible (and do not confuse individual-level with group-level effects).

Finally, take to heart the lessons learned from the recent replication crisis in psychology (e.g., Świątkowski & Dompnier, 2017). This includes: Be suspicious of extraordinary, surprising claims that are unlikely to be true a priori (Reisenzein & Rudolph, 2018b), avoid questionable research practices, make sure your studies have sufficient power (Schimmack, 2012), and focus on reasonably strong effects.

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