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EDITORIAL

The Future of Affective Science: Introduction to the Special Issue

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Abstract
Modern affective science—the empirical study of emotional responding and affective experience—has been active for a half-century. The *Future of Affective Science* special issue considers the history of this field and proposes new directions for the decades ahead. Contributors represent diverse theoretical perspectives, methodological expertise, and domains of study, and the special issue includes both literature reviews and new empirical studies as illustrations. This introductory article synthesizes the contributions, articulating the broader context of the current status of our field and highlighting common themes across articles as well as gaps notable even in this special issue. Sections of the article address theoretical and conceptual issues, research methodology, the questions we ask, and translation of basic affective science to applied domains. We conclude that much has been learned from the first 50 years of affective science, and it is now time for new theories, new research questions, and innovative methods for the decades ahead.

Keywords Emotion · Affect · Research methods

The Future of Affective Science: Introduction to the Special Issue

The seed of affective science can be found in Charles Darwin's (1872/1998) analyses of human and nonhuman animals' nonverbal expressions of emotion and his proposal of their evolutionary origins. Its roots lie in the theories of William James (1884), Carl Lange (1922), and Silvan Tomkins (1962/2008) and studies of physiological aspects of emotion and stress by Walter Cannon (1915), Philip Bard (1934), Hans Selye (1955), and others. Largely ignored by social and behavioral scientists in the mid-twentieth century, research on human emotion was revived in the 1970s by Paul Ekman and Carroll Izard's studies of how people around the world interpret facial expressions of their proposed "basic" emotions (Ekman et al., 1969; Izard, 1971). Since then, modern affective science—the empirical study of emotional responding and affective experience—has blossomed. A field that did not exist 50 years ago now boasts multiple dedicated journals, including this one, professional societies, doctoral programs, undergraduate courses and textbooks, and an evidence base in millions of empirical articles. The impact of emotion is increasingly recognized in applied domains and public policy, as well as the basic behavioral sciences (see Fig. 1). A 2021 consensus statement by dozens of prominent researchers predicts an "era of affectism" in the decade ahead (Dukes et al., 2021).

It is thus an ideal time to reflect on the body of work built over the last five decades and to consider future directions for the field—including potential course corrections—with care. Early studies served as a template for decades of science that followed, investigating aspects of emotion such as peripheral physiological reactivity (Kreibig, 2010), various channels of nonverbal communication (Tracy et al., 2015), appraisals of emotion-eliciting situations (Moors et al., 2013), and effects on cognitive processing and decision-making (Lerner et al., 2015). Building on Ekman and Izard's methods and theoretical assumptions, most research has both conceptualized and operationalized emotions as functionally discrete categories and emphasized relatively objective aspects of emotional behavior along with self-report measures. However, questions and debates about the nature of subjective affective experience, about emotion concepts, and about emotional language leapt to prominence in the 1990s. This

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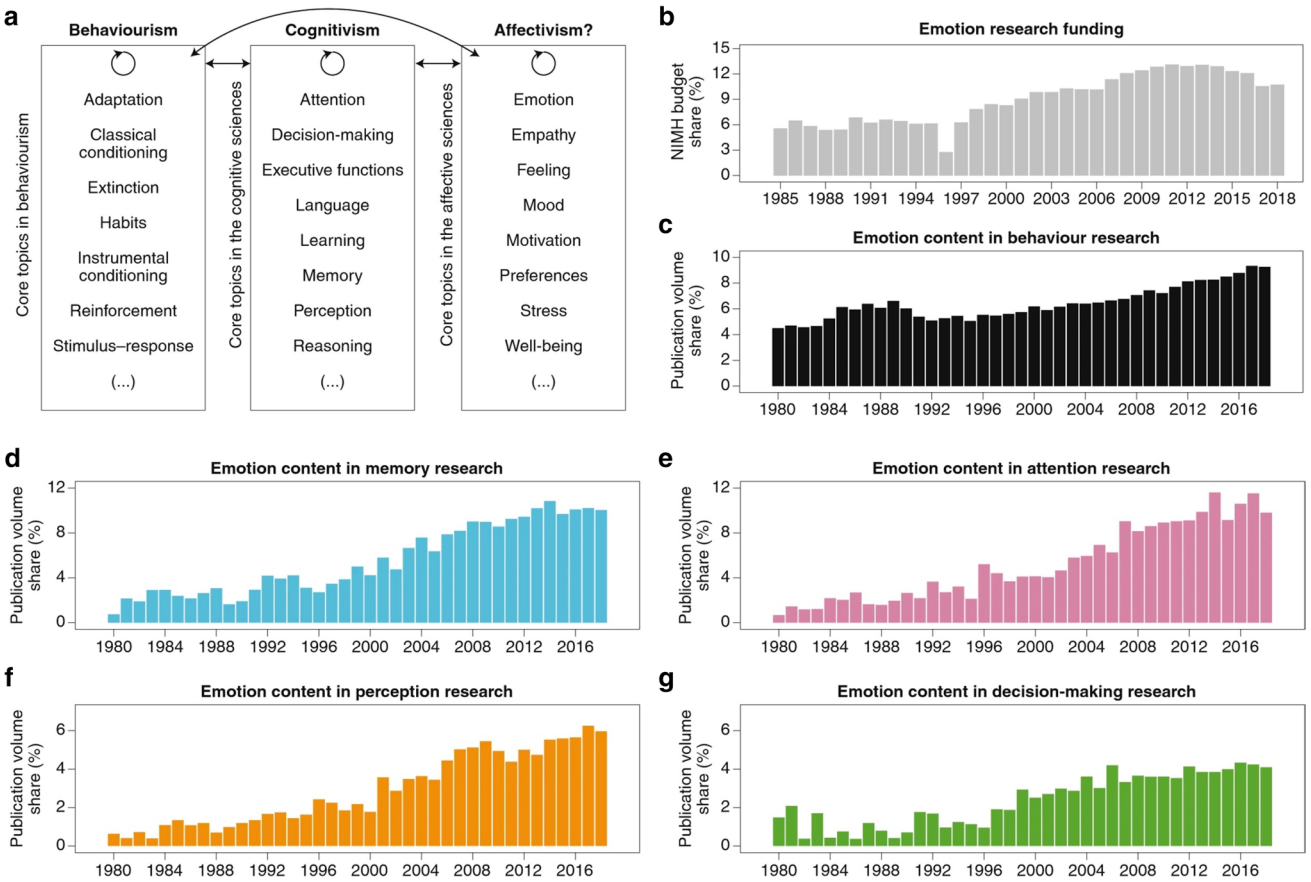

Fig. 1 The scope and increasing impact of affective science. From "The Rise of Affectism" by D. Dukes et al., 2021, *Nature Human Behaviour*, *5*(7), 816–820

movement began with Jim Russell's comparisons of "emotion" vocabulary and concepts across languages and cultures (Russell, 1991) and initial studies of *core affect* as the feeling space defined by dimensions of valence and arousal (Russell, 2003), and work on these topics has expanded dramatically in the last decade (Barrett and Lida, in press).

Much knowledge has been gained in a half-century of affective science. It has greatly informed our understanding of psychological, biological, and sociocultural aspects of emotional experience and demonstrated implications of affect and emotion for consequential outcomes such as physical and mental health, judgment and decision-making, and interpersonal behavior (Ferrer & Mendes, 2018; Lerner et al., 2015; Parkinson et al., 2005; Williams & Evans, 2014). However, we still lack agreement on answers to major theoretical questions about the nature and structure of emotion (e.g., discrete emotion vs. constructivist perspectives), as existing data do not unequivocally support any one of the competing positions (e.g., Cowen & Keltner, 2017; Elfenbein & Ambady, 2002; Kragel & LaBar, 2016; Kreibig, 2010; Lench et al., 2011; Mauss et al., 2005; Siegel et al., 2018; Touroutoglou et al., 2015), which may not be mutually exclusive to begin with (Nesse, 2014; Shiota, in press). Affective science has answered many important questions it sought to address, but needs to re-interpret the questions themselves and identify a next generation of questions, which have become increasingly clear.

In this special issue of *Affective Science*, the latest ideas and illustrative examples of directions for the future of our field are offered by scholars representing diverse disciplinary perspectives, theoretical assumptions, and methodological expertise. In this introductory article, we briefly review where we are now as a field; highlight themes across special issue articles in sections on theory and conceptualization, methods, the basic questions we ask, and translation to applied contexts; and offer additional thoughts on future directions for the decades ahead. Our aim is not to endorse any particular pathway, but rather to offer a wealth of possibilities (some of which may ultimately be rejected) and to stimulate a conversation about the future of affective science that should engage the whole field.

Future Directions for Theory and Conceptualization

Emotion Theories: Where We Are Now, and What Is Needed

After decades of debate, two major theoretical "camps" still strive for primacy in shaping the direction of affective science. As noted above, one is rooted in theorizing by Charles Darwin (1872/1998) and Silvan Tomkins (1962/2008), and the groundbreaking cross-cultural studies conducted by Carroll Izard, Paul Ekman, and their colleagues (Ekman et al., 1969; Izard, 1971). The latter concluded that their findings of similarity across cultures in the inferred emotional meaning of certain human facial expressions could best (and perhaps only) be explained by a set of innate, universal neural programs generating "basic" emotions, such as fear, anger, sadness, and happiness (Ekman, 1971). Recent proponents of this perspective have proposed modified, less rigid (e.g., less locationist) views of the nature and neural mechanisms of categorically "discrete" or "distinct" emotions, while still emphasizing the value of grounding hypotheses in theories of different emotions' distinct adaptive functions and anticipating and researching universal aspects of emotion (see Shiota, in press, for a review).

The other major camp is more recent, sparked by Russell's (1991) analyses of cultural variability in emotion concepts and represented most prominently by Lisa Feldman Barrett's theory of constructed emotion (Barrett, 2017). In general, constructionist theories abandon the idea that emotions reflect activation of discrete emotion-specific neural programs. Embedded instead in a predictive processing model of brain functions, this family of theories proposes that emotional episodes are assembled from diverse domain-general biological and cognitive processes that are not specific to emotion. While these processes sometimes result in co-occurring response elements that people may categorize as an emotion episode, either universally or within a particular sociocultural context, constructionist theories posit that the experience of "an emotion" is primarily a subjective interpretation constructed within the mind (a "conceptual act"; Barrett, 2017), rather than reflecting the objectively measurable effects of a dedicated neural program's activation.

For decades, the basic/discrete and constructivist camps have been "at war" (Lindquist et al., 2013). But is a war necessary? In this special issue, Wood and Coan (2023) offer a thought-provoking effort to reconcile the basic/discrete and constructionist perspectives. Using concepts derived from a dynamical system framework (see also Camras, in press), Wood and Coan argue that the two theories are not mutually exclusive. Both perspectives acknowledge that emotion involves biology and culture, and innate and learned elements. In fact, both theories acknowledge the possibility of emotion universals—although they explain these in different ways. One implication of Wood and Coan's argument is that compatibility between theories that initially seem opposed becomes apparent if one considers that they differ primarily in focusing on different levels of analysis (i.e., evolutionary function vs. ontogenetic mechanism) in explaining human emotional experience.

While a third major theoretical perspective—the Component Process Model (CPM; Scherer, 2009)—has remained on the sidelines of the "war," it too should be taken seriously in efforts to reconcile alternative theories and generate new options. Building on the early work of Magda Arnold and Richard Lazarus, appraisal theories including the CPM emphasize the central role people's appraisals of eliciting events, context, and coping expectations play in generating a rich and dynamic emotional experience (Moors et al., 2013; Scherer, 2009). All three major theoretical frameworks have contributed valuable knowledge to affective science, and each likely provides important pieces of the puzzle of human emotion. The trick will be to put these pieces together in the years ahead, and see what image is revealed.

Emotion-Related Concepts: Where We are Now, and What is Needed

Perhaps because the major theoretical perspectives actually agree on a number of points, a considerable amount of empirical affective science has been conducted without explicit commitment to one or another framework. These areas of agreement are increasingly the focus of innovative research, illustrated by articles in this issue. For example, Vishkin and Tamir (2023) highlight unique features of emotion norms that may account for a substantial amount of the cultural variability observed in emotion functioning (e.g., Tsai, 2017). At the same time, several special issue articles challenge assumptions behind commonly invoked concepts in affective science. Gasper (2023) argues for conceptualization of neutral affect as a distinct form of affect in its own right, not reducible to the absence of positivity and negativity. Walle and Dukes (in press) consider the multiple meanings of "valence" in affective science, going so far as to ask whether the construct of valence is useful at all. Zhang and colleagues (2023) investigate neural activity during instructed emotional reactivity and regulation and end by questioning whether the two can legitimately be distinguished. Becker and Bernecker (2023) challenge the widely held assumption that hedonic goal pursuit is necessarily bad, hindering the pursuit of long-term goals.

As exemplified by these contributions to the special issue, reconsidering some conceptualizations that have provided popular research frameworks may prove beneficial to the field. Widely accepted concepts may easily morph into assumptions that restrict our vision; challenges to their validity and utility can help prevent theoretical complacency. We anticipate that many, if not all, of the conceptual challenges offered by articles in this special issue will be controversial. Whatever the final resolution may be, addressing theoretical and conceptual challenges of this kind will help scholars sharpen their thinking and encourage researchers to formulate new hypotheses to test.

Additional Thoughts on the Future for Emotion Theory and Conceptualization

In contemplating the future of affective science, additional considerations emerge, some of which characterize scientific endeavors in any field. One is the role of consensus. Theoretical consensus simplifies the research process by providing a straightforward and accepted investigative path, with the goal of confirming (or disconfirming) a single theory. Consensus regarding appropriate methods (e.g., measures, procedures, statistical techniques) makes it easier to formulate concrete research plans and integrate findings across laboratories and distinct programs of research. However, expecting and relying on consensus also have some obvious disadvantages. In seeking consensus, scholars may invest excessively in the kinds of either-or battles that have been prominent in twenty-first-century affective science; battles we believe have become unnecessary and distracting. Once established, consensus may impede scientific progress by discouraging creative thinking and innovation, including the development of alternative theories, constructs, and measures that may prove to be improvements over their predecessors. If we accept the idea that scientific progress requires both innovation and replication, perhaps, a balance between debate and consensus can be achieved.

Still, it is incumbent upon scholars to articulate a strong and clear rationale for proposals that conflict with or reach beyond current norms. For example, in recent years, the pantheon of states called emotions has expanded greatly beyond the relatively small, original set of basic emotions, e.g., to include gratitude (Algoe et al., 2013), awe (Shiota et al., 2014), confusion (Rozin & Cohen, 2003), and hubristic vs. authentic pride (Holbrook et al., 2014; Tracy & Robins, 2014, as well as states named in other languages such as schadenfreude (e.g., Greenier, 2021) and kama muta (Fiske et al., 2019). Recent papers have implied as many as 27 or 28 distinct emotion states (Cowen & Keltner, 2017, 2020). While the criteria behind the initial list of basic emotions (Ekman, 1992) may well be in need of revision, simply ignoring those criteria without seeking some kind of agreement on new ones is not the answer. Scholars should consider the implication of expanding the pantheon for emotion theories in general (e.g., whether and how they can be accommodated within each of the current theoretical camps), as well as advancing criteria to guide these kinds of conceptual decisions.

Also, scholars should avoid the temptation of overgeneralizing their own and other researchers' findings in testing and building theory: what is true for some emotions, or aspects of emotional responding, is not necessarily true for all others. For example, emotion regulation strategies that are effective for downregulating sadness do not necessarily work as well as for anger (Southward et al., 2019). Similarly, data-driven findings regarding the dimensional, categorical, or other similarity structure of any one aspect of emotion—such as subjective affect (e.g., Russell, 2003), emotion language (e.g., Jackson et al., 2019), emotion-linked physiological reactivity (e.g., Kragel & LaBar, 2013; Wormwood et al., 2019), or interpretation of others' nonverbal expressions (e.g., Cowen & Keltner, 2020)—are not necessarily informative about the structures of the others. After 50 years of searching, we have yet to find a simple answer to the question of how and why different emotions and aspects of emotion are connected. In the next wave of affective science, an open-minded, nondogmatic reading of the data and analysis of what it might mean will be needed for the field to advance.

Future Directions for Methods

The bulk of affective science to date has been gathered through laboratory experiments in which participants respond to carefully selected emotion stimuli under highly controlled circumstances. Emotional/affective experience in real life is rarely as categorically pure, static, or tidy as assumed in these studies, yet can be far more intense and meaningful for the person having that experience. A central challenge for the years ahead will be to study emotion as it exists "in the wild": messy, dynamic, idiosyncratic, and richly modulated by context (Greenaway et al., 2018).

Populations Represented in Affective Science

Given the widely accepted assumption that affective and emotional experience are rooted in species-typical adaptations produced through evolution, yet powerfully shaped by cultural context and through socialization (Barrett, 2017; Ekman et al., 1972; Mesquita & Frijda, 1992; Scherer, 2009; Shiota, in press), the extent to which affective science has been limited to Western, Educated, Industrialized, Rich, Democratic (WEIRD) societies and populations (Henrich et al., 2010) is a serious problem. Although a considerable amount of research has examined emotion in East Asian cultural contexts, typically with an emphasis on collectivism

as a driving cultural factor, shockingly little research has been conducted with populations beyond North America, Western Europe, and the Pacific Rim. This needs to change.

The special issue article by Brady et al. (2023) illustrates both the value and challenge of research beyond the WEIRD world. This multi-method study of display rules in children's responses to the disappointing gift task, conducted in indigenous Yucatec Mayan villages in Mexico, has important strengths. The research team included a native member of the community; procedures were conducted in the local language; both objective (physiology, behavior) and subjective (self-reports of feelings and display rules) measures were collected; and the target task was culturally relevant for the sample while also used extensively in prior research. The challenges are also apparent. This kind of field work is expensive and labor-intensive, and requires an "in" with the local community. Procedures often necessitate translating English-language emotion/affect words into the local language, and a leap of faith that the same (or a sufficiently close) concept is invoked (Russell, 1991). The sample size is small and likely underpowered for the hypothesized effects. Findings are not notably different from those seen using the same task in the USA and Europe, possibly because the small sample size precluded detection of subtle cultural differences. As a result, the novel theoretical impact of the findings may not be readily apparent to reviewers and action editors. Moving forward, we all need to decide as a field how to evaluate this kind of research—what standards to apply given its distinct contributions and challenges.

Affective science also needs to decide how to integrate research conducted with humans and nonhuman animals (NHAs). Much research on the neural mechanisms of emotion-like behavior is conducted with rodents and other NHAs, and the techniques that can be used with NHAs are far more sophisticated and precise than those currently possible with humans. Researchers are extremely cautious about generalizing findings to human emotional phenomena, however, often to the point of using distinct, non-emotional terminology to refer to such states (e.g., Kennedy et al., 2020; LeDoux, 2012; Panksepp, 2004). In their special issue article, Michon and colleagues dare to take this leap, making the case that rodents demonstrate an ability to empathize with conspecifics' positive emotion states. Human emotional experience and behavior clearly inform the research agenda with NHA subjects; but under what conditions and constraints is generalizing findings from NHA research back to human emotional experience valid? A key challenge arises from the discrepancy in dependent measures used as ground-truth for inferring an emotion: typically, self-reports in humans, but nonverbal behaviors in NHAs (LeDoux & Pine, 2016). The field needs to tackle these challenges for the promise of animal neuroscience to be fulfilled.

Measurement of Affect and Emotion

Affective science has historically been a multi-method field, rich with measures of nonverbal behavior and physiological reactivity as well as cognitive tasks, questionnaires, and other forms of self-report. Behavioral coding and psychophysiology assessment are extremely resource-hungry, labor-intensive methods, however, and obtaining high-quality data has usually meant running studies in the lab. Several papers in this special issue highlight growing movements to collect behavioral and physiological data "in the wild" and/or to automate the processing of such data to reduce human workload. Park and colleagues' (2023) study using ecological momentary assessment and smartwatch/phone-based cardiovascular measures to examine age differences in real-life emotion illustrates one groundbreaking approach. Shore and colleagues' (2023) study integrating self-reported and partner-perceived facial expression regulation with automated coding of positive and negative affect to predict decisions in an iterative cooperation task (the prisoner's dilemma game) illustrates the valuable role automated behavioral coding may eventually play in elaborate social interaction studies.

While promising, these measurement innovations are still in their youth, and some challenges must be addressed before widespread use is advisable. First, automated and real-world measures are often much noisier than human-processed data from lab studies. Phenomena beyond emotion can influence the variables to be measured (e.g., non-emotional influences on autonomic nervous system activity, as discussed by Park et al., 2023), as will loss of measurement fidelity under non-ideal conditions (e.g., reduced accuracy of automated facial coding based on lighting, head orientation, and participant physical characteristics such as skin color; see Cross et al., 2023). While one might expect that automated coding would be less biased than coding by human researchers, this is not the case—worse, the biases inherent in automated algorithms are often unknown, and trustworthiness of these approaches is a major topic in current machine learning and AI. Is automated facial expression coding sufficiently accurate and valid for research use at this time? It depends—on what exactly is coded (which action units or expressions), by which software, and under what environmental conditions. Second, in the river of ongoing experience that can be captured in real life, how should researchers decide which epochs to study? Any option has benefits and limitations; as nicely discussed by Hoemann and colleagues (2023), this decision must be made with care. Third, how to decide which data should be retained in analyses and which should be excluded? This decision is also tricky. Park and colleagues' (2023) multiverse analysis offers a promising new tool for documenting robustness of findings across such researcher degrees of freedom. We look forward to seeing best practices for these innovations emerge, along with the technologies themselves.

Research on affective and emotional language, conceptualization, and subjective feeling has grown a great deal in sophistication as well, with recent studies comparing the structure of emotion vocabularies across languages as seen in large databases of text (Jackson et al., 2019); examining children's development of emotion vocabulary and conceptual understanding (Nook et al., 2017); and creating "maps" of affective, cognitive, and somatic space (Nummenmaa et al., 2018). We are eager to see this empirical work continue to grow. Moreover, such work may have applied utility, as proposed in the special issue article by Nook (2023) on affective language modification as a potentially powerful approach to treating psychopathology involving affect and emotion.

Approaches to Data Analysis

The move of affective science "into the wild" is an enormous opportunity, already bearing fruit in self-report ecological momentary assessment (EMA) and diary studies, with physiological and behavioral data not far behind. While lacking in experimental manipulation and tight control over extraneous variables, such datasets have the benefits of very large volumes of data with a longitudinal element. As journal editors, however, we have noticed an all-too-common approach that misses the full value of such data: many analyses simply aggregate all this rich time-series data into person means capturing individual differences, and then use regression or path modeling to examine hypothesized causal associations.

Fortunately, a wealth of analysis techniques that recognize, and can explicitly model, dynamic features of real-world affect and emotion is increasingly available. Within this special issue, Teoh and colleagues (2023) highlight the potential of dynamic system analysis techniques to advance conceptualization of affective experience, as well as increasing ecological validity and measurement sophistication. The empirical article by Rocklin and colleagues (2023) illustrates this potential, examining the emotional stimuli people encounter in their digital lives, and impact on their affective experience. Both Lange (2023) and Lin and colleagues (2023) analyze roles that machine learning and network modeling might play in advancing research and theory on affect/emotion. All these techniques are evolving rapidly, and each comes with limitations, constraints, and choices that must be made with care. Fortuitously, many of these same techniques are already strongly developed in a related field: the analysis of neural time-series data. While the timescales are quite different, the dynamic system algorithms can in many cases translate quite easily from neurons to people. Though the time investment needed to develop expertise in these advanced analysis techniques is non-trivial, we urge researchers to learn enough to at least be an informed consumer of this kind of work, as we anticipate its prevalence and influence will grow rapidly in the years ahead.

The Context of Affective and Emotional Experience

While laboratory-based research offers well-known advantages—standardization of the research environment, control over extraneous variables, ability to isolate mechanisms of interest, and confidence that research participants are actually paying attention—the costs are substantial. Most laboratory tasks elicit relatively weak emotional responses relative to peaks of real-life experience (Levenson, 2014), and stimuli aim to elicit or communicate far more "pure" experiences of particular, discrete emotions than characterize typical experience "in the wild." We expect that collecting and analyzing data on affect and emotion as people go about their daily lives, as discussed by Hoemann and colleagues (2023) and illustrated by Park and colleagues (2023) in this special issue, will thus become a major focus in the decade ahead. Other articles in the special issue push boundaries on the kinds of situations in which affect and emotion are studied, including sleep (Sikka and Gross, 2023), in virtual reality environments (Kako et al., 2023), and in robotics and artificial intelligence (Kappas & Gratch, 2023).

Additional Thoughts on the Future of Methods in Affective Science

While affective science methods typical of the last half-century are valuable for isolating and understanding mechanisms of emotional experience, the gap between affect/emotion in the lab and in real life is huge. In the decade(s) ahead, we expect a surge in data collected "in the wild." Given the leap in complexity of the phenomena being studied, this is likely to be a period in which bottom-up observational research is much more prominent than it has been for some decades; researchers will need to agree on new norms for rigorous exploratory affective science. In moving beyond the lab, we also caution against over-relying on cheap, easy-to-recruit online samples and questionnaire measures (e.g., via MTurk and Prolific). Such work has its place, but is arguably even more impoverished than laboratory research at capturing real-life experience, and should be used with care. As emotion and affect are increasingly recognized as potent forces in health, behavior, economic decision-making, and other societally consequential outcomes, we hope that funding agencies will again recognize the value of investing in basic affective science, and provide support for the resource-intensive work often needed to get it right.

Future Directions for the Questions We Ask

New science typically "stands on the shoulders" of prior scholarship, relying on previously published theoretical analyses and empirical work to provide justification for

new studies. This has many advantages: when built on the same foundation, programs of research conducted by independent laboratories can grow rapidly, be synthesized more readily, and accelerate theory building. When this goes on for a long period of time, however, there can also be costs. Noam Chomsky famously accused science of being like "the drunk who is looking under a lamppost for a key he has lost on the other side of the street, because that's where the light is" (Barsky, 1998). In deeply investigating those phenomena on which prior research has already shined a light, we risk neglecting vast areas of darkness.

The studies that define new fields—innovative, asking fresh questions, and high in rigor—both launch and constrain the burst of work that follows. For example, Ekman and Izard's early studies of facial expression recognition have had enormous influence on 50 years of affective science: fear, anger, sadness, and disgust are by far the most extensively studied emotion states; and Ekman's methods in particular have been copied in many thousands of studies of nonverbal behavior. Here, we highlight three topics for which an initial study's approach has proved highly fruitful, but the time has come to look beyond the light it cast on the street. These are not the only such topics within affective science, but serve as examples illustrating the importance of considering future directions in the questions asked by our field.

Emotion Regulation

Emotion regulation is now a major focus of affective science. As noted in the special issue article by Petrova and Gross (in press), more than 30,000 papers on this topic were published in 2022 alone. This wave of scholarship was arguably launched by two 1998 papers by James Gross: a review article introducing the Process Model of Emotion Regulation (Gross, 1998a) and an empirical study comparing effects of cognitive reappraisal and expressive suppression on facial expression, peripheral physiological change, and subjective emotional feeling in response to a disgusting film clip (Gross, 1998b). The impact of these papers on subsequent research cannot be overstated. The Process Model has proved extremely useful, organizing many varied emotion regulation/coping strategies into a coherent single model and generating a wealth of predictions about their effects, not only on immediate emotional responding, but also on social interaction, behavior, and well-being (Gross, 2015). The contrast of cognitive reappraisal—specifically detached reappraisal—with expressive suppression is the focus of a large proportion of emotion regulation research. When synthesized, the take-home message of this work is that reappraisal is good, and suppression bad, reflecting a long-standing emphasis in psychology on differentiating generally beneficial/adaptive from ineffective/maladaptive approaches to managing one's emotions (e.g., Aldao et al., 2010; Webb et al., 2012).

In their special issue article, Petrova and Gross (in press) call for dramatically expanding the questions asked in the next generation of emotion regulation research, highlighting three particular future directions. Because we received several articles on emotion regulation for this special issue, nicely illustrating these recommendations, the December 2023 issue of *Affective Science* will include a special section on the future of emotion regulation research. First, Petrova and Gross recommend increased attention to emotion regulation "tactics"—specific techniques for implementing broad strategies such as distraction, reappraisal, and response modulation. The article by DiGarolamo and colleagues (in press) illustrates this, examining age-related variability in tactics that downregulate negative affect, upregulate negative affect, or upregulate positive affect. Second, Petrova and Gross advocate for broadening theory and research to include interpersonal emotion regulation, as well as the self-regulatory processes that have dominated prior research. This refers primarily to scenarios in which one individual intentionally regulates another's affect or engages in social interaction as an emotion regulation tactic. However, the special issue article by Shore and colleagues (2023), on implications of self-reported and partner-perceived upregulation of positive affect expression for subsequent decisions in an iterative prisoner's dilemma game, also illustrates the kinds of new questions that arise when the focus expands to include the social context of emotion regulation.

Third, Petrova and Gross call for increased recognition of emotion regulation as a dynamic process, measuring and modeling change over time. Through what processes do people select, deploy, and modulate emotion regulation tactics? What implications do these dynamic features have for consequential outcomes, such as health behavior, relationship quality, and well-being? Though not explicitly about regulation of emotion, the special issue article by Lange (2023) proposing a network model of emotion duration suggests intriguing possibilities. If the duration of an emotion episode is determined in part by the strength with which different emotion components are interconnected, as Lange suggests, does this help account for the differing effectiveness of emotion regulation strategies/tactics that modulate different emotion components (e.g., appraisal vs. physiology vs. expression)? Recent work on emotion regulation flexibility (Aldao et al., 2015; Bonanno and Burton, 2013) and dynamics (Kuppens & Verduyn, 2015) is beginning to offer options for quantifying intrapersonal variability in emotion regulation in response to context and across time. We look forward to seeing future scholarship examining implications of these dynamic features for people's emotional lives.

Nonverbal Communication

Countless investigations of expression recognition/emotion communication have been conducted in the past five decades (Matsumoto et al., 2008). Most of these studies have presented participants with a set of predetermined, prototypical/stereotypical, exaggerated facial configurations or nonverbal vocal bursts and required perceivers to make judgments about those stimuli in isolation, devoid of any meaningful social context (although there are noteworthy exceptions; e.g., Aviezer et al., 2008; Jack et al., 2014). Yet in the real world, perceivers may not spontaneously process such cues in the same way they do when following instructions in an experimental study. In the real world, emotion communication is a dynamic process involving rich influence of situational context, as well as agency and mutual influence on the part of sender and receiver (Keltner et al., 2016).

Future studies should investigate how people spontaneously respond to emotion cues encountered in naturalistic contexts, including situations that may be simulated (to some degree) in the laboratory (e.g., Camras et al., 2017; van Kleef and Côté, 2018). Exemplifying this approach are studies of social referencing showing that infants (and sometimes even adults) will respond to ambiguous situations in accordance with others' emotional facial expressions, if they themselves are uncertain about how to respond (e.g., when confronted with an unfamiliar but potentially threatening object or event, e.g., Sorce et al., 1985). The special issue article by Shore and colleagues (2023) also offers a nice example of the impact of our inferences about an interaction partner's nonverbal expression on our own subsequent behavior. Other factors may also play a role, influencing whether perceivers attend to others' emotion cues at all, and how they integrate such cues from various sources in order to make emotion judgments—and decisions about how to act—in particular situations. Studies that examine perceivers' visual scanning of emotion stimuli represent a step in this direction, but integrating additional contextual information (e.g., situational cues; Reschke & Waller, 2021) into such studies will make them even more informative.

Development of Affect and Emotion

Extant developmental theories of affect focus on documenting presumably universal age-linked stages in the emergence of emotions, mainly in infancy and early childhood (Camras, in press). This approach harks back to the hey-day of Piaget's highly influential stage theory of cognitive development. In the post-Piagetian era, however, this approach has been largely abandoned in favor of documenting developmental changes in specific domains, which may vary depending upon both temperament and experience. As applied to emotion/affect, emotion understanding and emotion regulation are examples of such domains. Studies taking this approach (e.g., Nook et al., 2017) may eventually support integration of domain-specific developmental trajectories into a more comprehensive overarching theory.

In addition, further studies that bridge the gaps between the child development literature, the (young) adult literature, and the literature on emotion and aging will be particularly useful. Some developmental researchers are beginning to investigate the origins of emotion processes that are widely studied in the adult literature (e.g., emotion suppression; Gross & Cassidy, 2019). In addition, emotion socialization processes that have already been studied in children may operate in adulthood as well (e.g., processes that involve observing others' emotional behavior, contingent responding by others to one's own emotions, discussion about appropriate or inappropriate emotions; Eisenberg, 2020; Eisenberg et al., 1998a, b), meriting the attention of adult-oriented researchers.

Future Directions for Application of Affective Science

As noted earlier, basic affective science is of growing interest to policy-makers and increasingly used in applied fields (Dukes et al., 2021; Ferrer & Mendes, 2018; Lerner et al., 2015; Parkinson et al., 2005; Williams & Evans, 2014). Inevitably, applied work lags behind basic science while also pulling for premature application. As noted in special issue articles by Simmons and colleagues (2023) and Ferrer and Gillman (2023), however, the need to accelerate the translation of basic affective science into real-world impact is profound.

Where Are We Now, and What Is Needed

As of now, the main domains of application for affective science are mental health, behavior change, and artificial intelligence (AI), including human–computer interaction. Even here, translation of the basic science is severely limited, for a number of reasons. First, the lag of application behind basic science means that translational efforts often rely on theoretical assumptions and empirical research that are already out of date—perhaps by decades. As an example, the machine-learning algorithms behind automated emotion recognition programs—the kinds considered for use in security and law enforcement as well as in AI—continue to presume a classification problem in which images of people's faces must be assigned to one of Ekman's six basic emotions. Alternative frameworks for emotion space, likely cultural moderators, and effects of situational context are generally ignored, despite a growing basic literature showing them to be absolutely critical (Barrett et al., 2019).

Similarly, much of health psychology has carried forward a historical focus on negative affect as a discrete, categorical phenomenon.

Second, real-world applied contexts are far more complicated than their controlled laboratory counterparts. For example, there is wide acknowledgement that the great majority of psychiatric disorders in the Diagnostic and Statistical Manual (DSM) feature dysfunctional affect/emotion, often as a primary symptom (Kring, 2010). However, psychiatric disorders are complex in their biology and incredibly diverse in how they manifest—within a single person over time, let alone across individuals. Fractionation of one disorder into subtypes, or a spectrum, is expected (and in some cases has already happened, as for the various anxiety disorders and the autism spectrum). We have already noted the modest ecological validity of much prior affective science research. Linking emotional and affective processes to symptoms, and emotional/affective mechanisms to potential treatments, is made all the more difficult by the mismatch between the simplified emotion space covered by most existing laboratory research and the complexity of real-world disordered emotion.

Third, basic affective science typically employs emotion stimuli of moderate intensity at best, in contrast to the powerful emotional experiences that may characterize psychological disorder. Confident application may require basic science that captures these more intense emotional episodes, raising the question of how to ethically evoke strong emotions in human research participants—especially strongly negative ones. This topic will require discussion among all stakeholders, including ethics review boards, and also stresses the importance of affective science research in non-human animals (where validity and ethics are no less important, but often easier problems). Finally, the many challenges and limitations of affective science often are a surprise to non-experts in our field, who are generally impatient for tangible real-world applications. Policy-makers and the general public alike are vulnerable to overselling by those who are most confident in their beliefs, willing to overlook inconsistencies in the basic science, and may literally have something to sell. We must learn to communicate more effectively the state of the science—to the popular press, policy-makers, and even students in our classes—in a way that supports real-world application while acknowledging the challenges and limitations of the research.

While this list of challenges for application is by no means complete, it can be mapped to both near-term and longer-term solutions—as illustrated by several articles in this special issue. Several papers articulate and/or take up a core challenge identified above: collecting basic affective science data that are valid with respect to the complex, messy real world in which application will occur. For instance, Rocklin and colleagues (2023) analyze an intriguing dataset of screenshots of what participants saw on their cellphones, offering a window into the emotion stimuli they encountered on a daily basis. Sikka and Gross (2023) identify what in retrospect seems like an obvious part of emotional everyday life, largely ignored by affective science: what happens when we sleep and dream. Hoemann and colleagues (2023) point to the potential of multimodal sensing as people navigate their everyday lives.

More radically, yet of great importance, multiple contributors urge affective scientists to consider the needs and constraints of applied contexts in developing the basic science agenda. Ferrer and Gillman (2023) list several ways in which increased ecological validity will facilitate translation of basic science into real-world behavior change interventions. Somewhat paradoxically, inertia and bias in basic affective science itself might sometimes result from failure to think ahead, carefully considering its mapping to the real world and thus its potential real-world applications. Wilson-Mendenhall and Holmes (2023) therefore urge investigators to focus on "use-inspired" basic research, offering guidelines and examples of this process. And Simmons and colleagues (2023) highlight priority areas for basic affective science that will inform investments in public health. These and other contributions in this special issue make a simple but important point: it is now eminently possible for affective science to approach, and have impact in, the real world. We just have to think broadly about what the "real world" comprises, and think inventively about how we can take advantage of modern methods to capture it.

Additional Thoughts on the Future of Applied Affective Science

Beyond enhancing the ecological validity of basic research, a rich applied affective science requires developing strong relationships with people outside our immediate professional bubbles. Enhanced communication among researchers representing different disciplinary traditions (e.g., social, developmental, cognitive, and clinical psychology; human and animal neuroscience; sociology, anthropology, and linguistics) will facilitate integration of knowledge across multiple levels of analysis, as needed for application in the real world. Interdisciplinary conferences, collaborations, and indeed publications are required; we at *Affective Science*, the journal, will continue to prioritize such interdisciplinary work. We also need to engage extensively with those who inhabit the applied domains in which we aim to have impact. This includes public health professionals as well as NIH program officers, community mental health practitioners, and representatives from companies and community-based organizations aimed at creating positive change. We need to disseminate affective science beyond the pages of academic journals, communicating effectively with journalists,

policy-makers, high-school students in outreach programs, and of course the students in all of our classes. This will build public trust, promote a more accurate understanding of affective science (and science in general), and encourage the development of the next generation of young scholars to tackle the challenges outlined herein.

Pulling It All Together: Common Themes, What Is Missing, and How to Prioritize

The future directions addressed in this special issue are extraordinarily diverse, encompassing conceptual, methodological, and applied concerns as well as a wide range of substantive domains. Emotion and affect are themselves complex, multi-faceted phenomena, so this should come as no surprise. It is easy to lose the forest for the trees when asking what the future of affective science should hold, and how we should prioritize our ambitions. In this final section of the introductory article, we highlight common themes across articles in the special issue; note what is missing from these articles, and why that is important to fix; and offer thoughts on setting priorities for the work ahead.

Overarching Themes Across the Special Issue

Three themes have appeared sufficiently often in this article that we can enumerate them, without going into extensive detail. The first is the need for a quantum leap in the ecological validity of empirical affective science. This may mean collecting data in the world beyond the lab, facilitating more realistic situations and behavior within the lab, and/or using stimuli and tasks that are more in tune with the real-life contexts of emotional and affective experience. The second theme is closely related—the importance of acknowledging the deeply social nature of human emotion, in both theory and research (Boiger & Mesquita, 2012; Campos et al., 2018; Saarni, 1999). Given that a huge proportion of affective and emotional experience occurs during social interaction (Parkinson, 1996), and considering the wide-ranging functions of emotion in interpersonal relationships (Niedenthal & Brauer, 2012), the dominance of affective science conducted with lone humans interacting with paper surveys, computer screens, and/or television monitors rather than other people is remarkable. Each of these future directions will mean reducing our reliance on some tried-and-true methods and tolerating a period of messiness while new best practices are developed. We are not starting from scratch: affective science studies have examined dyadic and small group interactions in the laboratory and beyond (e.g., Algoe et al., 2020; Brown et al., 2022; Danvers & Shiota, 2018; Keltner et al., 1998; Sels et al., 2020); and as illustrated by articles in this special issue, increasing ecological validity both is possible and pays off (e.g., Park et al., 2023; Shore et al., 2023). This research is not easy, but models for doing it are available, and it has the added benefit of being quite a lot of fun.

The need for transdisciplinary communication, training, and collaboration is the third common theme. This is illustrated not by any one article within this special issue, but rather by the sheer range of topics the articles address and of the contributors' methodological expertise. From experiences of receiving unwanted gifts (Brady et al., 2023) to interacting with robots (Kappas & Gratch, 2023) and dreaming (Sikka & Gross, 2023) and from psychophysiology (Hoemann et al., 2023; Park et al., 2023) to experience sampling (DiGarolamo et al., in press), animal neuroscience (Michon et al., 2023), and dynamic systems modeling of "big data" from participants' smartphones (Rocklin et al., 2023), the scope of affective science is remarkable. Inevitably, studies examining different measures, populations, contexts, and aspects of affective and emotional experience will tell different stories. If we remain within disciplinary comfort zones, the epic tale of human emotion will remain beyond our reach. In the section on applied affective science, we emphasized the importance of communicating with stakeholders in translational and policy domains. Affective science would be enriched by cross-fertilization with the humanities as well; we are delighted to see that an incoming editor of *Emotion Review* is an expert in the history of emotion in sixteenth-century English literature.

Transdisciplinary sharing of ideas and knowledge can occur while still anchoring affective science in the empirical study of human experience. Input from the biological sciences, in particular, should not serve to define conceptual and theoretical questions about emotion; these questions need to originate within psychology—the study of the mind. Asking whether the subjective experience of fear depends on activation within particular brain regions, such as the amygdala, is a legitimate question in affective science. Asking "what does the amygdala do?" is not. As editors of this journal, we are still wrestling with the question of what does and does not count as affective science. Centering our field on understanding the causes, mechanisms, and effects of emotional and affective experience, while drawing freely from various disciplines in the search for relevant knowledge, seems a promising approach.

What Is Missing, and Why Does It Matter?

In preparing this introductory article, we were also struck by the absence or minimal presence of certain kinds of research. First, only two articles present new neuroscience data—one with nonhuman animals (Michon et al.,

2023) and one with human participants (Zhang et al., 2023). This may in part reflect a perception among neuroscientists that "affective science" is not their field. However, we believe this also reflects a more pervasive and problematic segregation of neuroscience from the rest of behavioral science. This is partly due to a lack of cross-training (which has begun to improve) and partly because the payoff of human affective neuroscience thus far has been lower than many initially hoped. Given that most efforts to link activation of whole brain structures to specific emotions and/or individual aspects of emotional responding have led to disappointment, finer-grained measures of neural activity and connectivity are needed to push forward. While network analyses and other approaches to studying circuits in fMRI data can help to some extent, figuring out how to synthesize the neuroscience literature across human and nonhuman participants has the potential to help as well.

Second, none of the articles in this special issue presented, or even systematically analyzed, data from long-term longitudinal studies. Such studies have enormous value, however; autoregressive analyses (regression models that control for the outcome variable at a prior time point when assessing the statistical effect of some predictor on that outcome at a subsequent time point) offer an imperfect but useful alternative to experimental designs in demonstrating causal direction. As affective science moves increasingly out of the lab and into the real world, longitudinal data—and the appropriate analyses—will be increasingly important. Third, only one article in this special issue (Brady et al., 2023) presented data collected beyond the WEIRD world. This gap is not specific to this special issue or this journal; as noted earlier, the scarcity of cross-cultural research in twenty-first-century affective science is glaring, and must be addressed.

These three gaps in the research covered by special issue articles have something in common—the work involved is highly resource-intensive in terms of time, specialized training, and sheer financial expense. We are well aware that funding for this kind of research has been scarce in recent decades. The question then becomes: how can funding agencies be convinced to invest in highly rigorous, innovative, yet costly programs of basic affective science? This leads to our thoughts on how to prioritize the many recommendations for the future of affective science offered herein.

Where Do We Start? Priorities for the Future of Affective Science

While we are enthusiastic about all of the items on the agenda for the future, one clear priority stands out—the need to reconnect basic and translational/applied affective science. Three articles within this special issue call loudly for researchers to consider the many ways in which rigorous, ecologically valid basic affective science might inform and support endeavors to improve people's lives in the real world. As emphasized by Ferrer and Gillman (2023) and Wilson-Mendenhall and Holmes (2023), consideration of how basic affective science findings might eventually be applied can even spark new, intriguing, theory-building questions for basic science itself. Moreover, as noted by Simmons and colleagues (2023), programs of research with clear translational significance are more competitive for funding by the US National Institutes of Health. This is the case for many other government funding agencies and foundations as well.

As another priority, it is time to invest hard intellectual work in generating new theories. To begin with—what *is* an emotion? In the 140 years since William James (1884) published an article with this question as the title, surprisingly few distinct answers have been proposed. Proponents of these answers have battled fiercely, to the point where many trainees and junior scholars may resist engaging with major theoretical issues at all, for fear of getting caught up in the nastiness. We must get beyond this. No single current theory of emotion sufficiently accounts for the rich data at our disposal. Neither hewing dogmatically to a preferred theory nor ignoring the problem and flying theory-free are acceptable in the long run. Theoretical perspectives both guide and constrain our research questions; new ones will be worth the time, discussion, and intellectual capital required for their development.

Conclusion

If we can offer one central message, distilled from everything above, it is this: it is time to start asking new kinds of questions, and expanding the methods we use to search for answers. Given the plethora of open questions, we need to step back often and ask what we are missing, and how our approaches might be biased. We must solicit and engage with diverse viewpoints and wrestle with new theories, tolerating the discomfort of uncertainty as we find our way forward. No single person or lab could possibly do all of this, but collectively, we can forge a field that will keep future generations of affective scientists busy for many years to come.

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Additional Information

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