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A Meta-Analysis of the Facial Feedback Literature: Effects of Facial Feedback on Emotional Experience Are Small and Variable

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The facial feedback hypothesis suggests that an individual's experience of emotion is influenced by feedback from their facial movements. To evaluate the cumulative evidence for this hypothesis, we conducted a meta-analysis on 286 effect sizes derived from 138 studies that manipulated facial feedback and collected emotion self-reports. Using random effects meta-regression with robust variance estimates, we found that the overall effect of facial feedback was significant but small. Results also indicated that feedback effects are stronger in some circumstances than others. We examined 12 potential moderators, and 3 were associated with differences in effect sizes: (a) Type of emotional outcome: Facial feedback influenced emotional experience (e.g., reported amusement) and, to a greater degree, affective judgments of a stimulus (e.g., the objective funniness of a cartoon). Three publication bias detection methods did not reveal evidence of publication bias in studies examining the effects of facial feedback on emotional experience, but all 3 methods revealed evidence of publication bias in studies examining affective judgments. (b) Presence of emotional stimuli: Facial feedback effects on emotional experience were larger in the absence of emotionally evocative stimuli (e.g., cartoons). (c) Type of stimuli: When participants were presented with emotionally evocative stimuli, facial feedback effects were larger in the presence of some types of stimuli (e.g., emotional sentences) than others (e.g., pictures). The available evidence supports the facial feedback hypothesis' central claim that facial feedback influences emotional experience, although these effects tend to be small and heterogeneous.

Public Significance Statement
This meta-analysis suggests that posed emotional facial expressions influence self-reported emotional experience. However, the size of these effects varies and tends to be small.

Keywords: emotion, facial feedback hypothesis, meta-analysis, replication

> "Sometimes your joy is the source of your smile, but sometimes your smile can be the source of your joy." —Thích Nhất Hạnh

Buddhist monk Thích Nhất Hạnh's deep spiritual reflection on human nature has led him to an idea deeply rooted both in our lay and scientific theories of emotion: feedback from our facial movements can influence our experience of emotion. In society, people often articulate this idea through sayings such as "grin and bear it," "fake it 'til you make it," and "smile your way to happiness" (Ansfield, 2007; Kraft & Pressman, 2012; Lyubomirsky, 2008). In psychology, we simply refer to this idea as the *facial feedback hypothesis*.

The facial feedback hypothesis suggests that facial movements provide sensorimotor feedback that (a) contributes to the sensation of an emotion (Ekman, 1979; Izard, 1971; Tomkins, 1962, 1981), (b) primes emotion-related concepts, facilitating emotion reports (Berkowitz, 1990; Bower, 1981), or (c) serves as a cue that individuals use to make sense of ongoing emotional feelings (Allport, 1922, 1924; Laird & Bresler, 1992; Laird & Crosby, 1974). Unfortunately, more than a century's worth of research has not yet clarified whether facial feedback effects are reliable. For example, researchers have produced a variety of theoretical disagreements about when facial feedback effects should emerge, but it remains unclear which, if any, of these theories are correct. Furthermore, 17 labs recently found that even the most seminal demonstration of facial feedback effects is not clearly replicable (Wagenmakers et al., 2016). Amid this uncertainty, we provide a narrative review of research on the facial feedback hypothesis and a meta-analysis of all available experimental evidence.

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Through narrative review, our goal is to provide a more full historical account of the facial feedback hypothesis, although one that is certainly not exhaustive. Through meta-analysis, our goal is to assess the reliability of these facial feedback effects, including the potential extent and impact of publication bias, and weigh-in on theoretical disagreements in the facial feedback hypothesis literature. Last, in our discussion, we will consider how the facial feedback hypothesis broadly fits—or does not fit—into basic, appraisal, and constructionist theories of emotion.

The term "facial feedback" is often used to denote the effects of facial movements on any outcome of interest, such as emotion perception (Neal & Chartrand, 2011) or implicit racial bias (Ito, Chiao, Devine, Lorig, & Cacioppo, 2006). However, the term "facial feedback hypothesis" is usually reserved to refer to the effects of facial feedback on emotional experience. This review will focus almost exclusively on the facial feedback hypothesis. Consequently, for our purposes we will use the following definition of "facial feedback" throughout this review: the effects of facial movements[1] prototypically associated with the expression of emotion on emotional experience.

The Origins of Research on the Facial Feedback Hypothesis

Research related to the facial feedback hypothesis was catalyzed by the writings of William James (1884, 1890, 1894) and Carl Lange (1885), who both proposed that our conscious experience of emotion is built from sensed changes in our bodily states.[2] However, although these theorists provided the theoretical foundation that the facial feedback hypothesis would later be built upon, neither emphasized the role of the face. For Lange, the face was irrelevant, as he contended that emotional experience was produced solely by sensed changes in the autonomic nervous system. James, on the other hand, allowed for the possibility that facial feedback could play some role in the experience of emotion. However, acknowledging the parallels between his and Lange's theories, James' (1890, 1894) later writings tended to emphasize the importance of the autonomic nervous system. Indeed, James contended that any emotional experience elicited solely by voluntary muscular movements "is apt to be rather 'hollow'" (James, 1884, p. 192). Given that James and Lange focused primarily on sensed changes in the autonomic nervous system, it is perhaps surprising that researchers would eventually narrow their focus to the role of facial feedback. To speculate why, it helps to consider the historical debates that surrounded the James-Lange theories.

James and Lange's theories proved to be one of the most controversial sets of ideas in early psychological research on emotion, meeting strong opposition from the likes of Wundt (1886), Worcester (1893), Irons (1894), Sherrington (1900), and Cannon (1915). One enduring concern, perhaps first raised by Worcester (1893), was that autonomic nervous system activity was too undifferentiated to distinguish among various discrete emotional experiences, such as anger or sadness. Indeed, Cannon (1915, 1927) noted that (a) different emotional states evoked similar changes in the autonomic nervous system and (b) nonemotional states shared similar autonomic nervous system patterns as emotional states. Consequently, Lange's sole emphasis on autonomic nervous system activity could not explain how people experienced discrete emotions. James' theory, on the other hand, suggested that differentiation was determined not only by the autonomic nervous system but also by skeletomuscular activity. Although James never specified what these patterns of activity were, Angell (1916) suggested that emotion differentiation may be determined by facial feedback. Several years later, Allport (1922, 1924) elaborated upon this idea in one of the first formal theories of facial feedback. According to Allport, autonomic activity created undifferentiated feelings of positivity and negativity that were subsequently differentiated into discrete emotional categories based on patterns of facial feedback. Surprisingly, Allport's key prediction that facial feedback guides the categorization of underlying positivity/negativity seems to have never been experimentally tested. Nevertheless, Allport's theory highlights the historical link between the James–Lange theory of emotion and what would later be known as the facial feedback hypothesis.

Fifty-five years after Allport published his theory of facial feedback, the term facial feedback *hypothesis* first appeared in print (Izard, 1977). However, by this point, researchers interested in these effects had already spent more than half a century producing theoretical disagreements about these facial feedback effects (Allport, 1922, 1924; Bull, 1945, 1946; Gellhorn, 1958, 1964; Tomkins, 1962). Consequently, the idea quickly splintered into various facial feedback *hypotheses* (Adelmann & Zajonc, 1989; McIntosh, 1996; Tourangeau & Ellsworth, 1979). Next, we review the four most prominent theoretical disagreements in the facial feedback hypothesis literature, each of which will be addressed in some form by our meta-analysis.

Modulation Versus Initiation of Emotional Experience

One of the most active debates surrounding the James–Lange theories was whether bodily activity—autonomic for Lange, autonomic and skeletomuscular for James—initiated emotional experiences or only modified ongoing experiences of emotion. James and Lange believed that bodily activity could do both. For example, Lange stated that "emotions may be induced by a variety of causes which are utterly independent of disturbances of the mind" and that they may also "be suppressed and modified by pure physical means" (Lange, 1922, p. 66). Similarly, James stated, "If our theory be true . . . any voluntary arousal of the so-called [bodily] manifestations of a special emotion ought to give us the emotion itself" and, in a more well-known quote, "Refuse to express a passion, and it dies" (James, 1884, p. 197). Skeptics, however, were especially critical of the proposed initiation function. In fact, many of the most well-known critics of the James-Lange view conceded that it was possible for bodily states to modulate, but not initiate, emotional experiences (Cannon, 1927; Irons, 1894; Sherrington, 1900; Worcester, 1893). For example,

[1] Some researchers have opted to define facial feedback in terms of "facial expressions" instead of "facial movements." However, others have argued that this terminology is inappropriate because an individual's face can resemble an emotional expression even when they are not experiencing that emotion (e.g., polite smiles; Zajonc, 1985).

[2] What we now refer to as the James–Lange theory of emotion was historically often called the "James–Lange–Sergi" theory of emotion. The Italian anthropologist Giuseppe Sergi (1894) proposed similar ideas as James and Lange but his contributions have become less recognized, perhaps because his work has never been translated to English. Alexander Sutherland (1898) also independently formulated a similar theory.

Cannon believed that the perception of an emotional stimulus caused the thalamus to discharge a signal that independently produced the experience of emotion and an accompanying set of bodily responses. However, Cannon (1927) acknowledged that these bodily responses might generate "faint" feedback signals, although he added that they likely played "a minor role in the affective complex" (p. 114). Consequently, the modulation versus initiation distinction represents an important disagreement in the James–Lange Cannon–Bard emotion debates.

Given the historical role of initiation versus modulation debates in James and Lange's more general bodily feedback theories, it is not surprising that similar disagreements emerged when researchers began developing theories of facial feedback. As noted above, Allport (1922, 1924) believed that facial feedback could only modulate emotional experience. According to his view, facial feedback guided the categorization of feelings of positivity and negativity, but it could not initiate emotional experiences in the absence of these underlying feelings. Gellhorn (1958, 1964) believed that the hypothalamus was the primary driver of emotional experience but that facial feedback could modulate ongoing hypothalamic activity. Although Gellhorn suggested that it was possible for proprioceptive feedback from the entire body to initiate emotional experiences, he doubted whether facial feedback could initiate emotional experiences on its own.

Although most early facial feedback theories stressed a modulating function, researchers later proposed that facial feedback could also initiate emotional experiences. For example, Ekman (1979) posited that each discrete emotion is activated by a biologically innate affect program that produces a set of bodily responses that later merge in consciousness to form emotional experience. Although these affect programs were believed to be typically activated by stimuli in the environment, Ekman and colleagues suggested that simply producing a facial configuration associated with an emotion could activate its affect program, thereby initiating the corresponding emotional experience (Levenson, Ekman, & Friesen, 1990). Similar predictions are made by some network and grounded cognition theories of emotion (Berkowitz, 1990; Bower, 1981; Niedenthal, Barsalou, Winkielman, Krauth-Gruber, & Ric, 2005), although they typically posit the existence of association-based emotion networks instead of biologically hardwired affect programs.

It is worth noting that the distinction between modulation and initiation implies that emotional experiences are episodes with clear-cut beginnings and endings. When emotional experience is conceptualized as a process that is constantly in flux (e.g., Russell, 2003; Wundt, 1886), the terms *modulation* and *initiation* are less applicable (Ellsworth, 1994). Under this alternative conceptualization, the initiation versus modulation distinction can instead be described as the effects of facial feedback on emotional experience in the presence [modulation] versus absence [initiation] of external emotional stimuli. To stay consistent with the language traditionally used in the facial feedback hypothesis literature, we will use the terms *modulation* and *initiation* when we examine this distinction as a potential moderator in our meta-analysis. However, we will later discuss these effects in the contexts of theories that conceptualize emotional experience as a continuous stream.

Discrete Versus Dimensional Levels of Emotional Experience

There is ongoing debate in the affective sciences regarding whether emotions are best conceptualized as discrete categories, such as happiness, anger, and sadness (Ekman, 1999; Izard, 2007; Tomkins, 1962), or as phenomena that are reducible to more primitive dimensions, such as valence (i.e., degree of positivity vs. negativity) and arousal (Russell, 1980) or positive and negative activation (Watson, Clark, & Tellegen, 1988). A similar discrete versus dimensional distinction exists in the facial feedback hypothesis literature. As previously noted, facial feedback theories were initially developed to explain the role of facial feedback in the experience of discrete emotions (Allport, 1922, 1924; Angell, 1916). For example, Tomkins (1962) and Izard (1971) proposed that affect programs created emotional experience primarily through various sources of facial feedback.[3] On the other hand, Zajonc later proposed that facial feedback could also influence feelings of valence (Zajonc, 1985; Zajonc, Murphy, & Inglehart, 1989). According to Zajonc's vascular theory of emotion—which was a modernization of an earlier theory proposed by Israel Waynbaum (1907)—subjective feelings of valence are caused by general and regional brain temperatures. Facial movements, Zajonc suggested, regulated air flow through the nasal and cavernous sinuses, which subsequently produced changes in brain temperature and emotional experience. By this account, scowling might make people experience more negative affect but not necessarily anger.

Debates regarding the effects of facial feedback on discrete versus dimensional levels of emotional experience remain unresolved. Reviews have typically agreed that facial feedback can influence dimensional reports of emotion. Interestingly, however, the effects of facial feedback on discrete emotions have been described as nonexistent (Winton, 1986), preliminary (Adelmann & Zajonc, 1989), mixed (McIntosh, 1996), and controversial (Soussignan, 2004). Later, we will weigh-in on this issue via moderator analyses.

Awareness of Facial Feedback Manipulation

Another prominent debate in the facial feedback literature concerns the role of participants' awareness of the purpose of facial feedback manipulations. For early facial feedback researchers, this raised the possibility that facial feedback effects are driven by demand characteristics (Buck, 1980). To address the role of awareness, Strack, Martin, and Stepper (1988) introduced the first incidental facial feedback manipulation: the pen-in-mouth procedure. In two studies ostensibly about psychomotor coordination, participants held a pen in their mouth in a manner that either forced them to smile (pen held in teeth) or prevented them from doing so (pen held by lips). While maintaining these poses, participants viewed humorous cartoons and reported how amused they felt. Consistent with the facial feedback hypothesis, Strack and colleagues reported

[3] Throughout the evolution of their theories, Tomkins and Izard were inconsistent in which sources of facial feedback they discussed. For example, Tomkins (1962) initially emphasized the role of facial movements, but later revised his theory to emphasize feedback from blood flow, temperature, and skin on the face (Tomkins, 1981). Izard (1971) mainly focused on the role of afferent muscular signals from the face, but also contended that efferent signals to the facial musculature could contribute to facial feedback effects.

that participants who posed smiles reported feeling more amused by cartoons than those who were prevented from smiling.

In addition to reducing the role of demand characteristics, Strack and colleagues suggested that their findings indicated that facial feedback effects occurred outside of awareness, an issue that theorists disagreed about. For example, some researchers suggested that such effects were driven by physiological mechanisms that occur outside of people's awareness (Gellhorn, 1958, 1964; Zajonc, 1985). Others contended that they are driven by consciously-accessible proprioception or self-perception mechanisms (Izard, 1977; Laird, 1974; Laird & Bresler, 1992; Tomkins, 1962). For example, Laird suggested that emotional experience is built from a self-perception process (e.g., people might conclude that they are happy because they perceive themselves to be smiling). Because Strack and colleagues created a manipulation that limited the degree to which participants were aware that their facial configurations resembled an emotional expression, they concluded that their results were inconsistent with these latter set of theories.

More recently, there is uncertainty regarding the reliability of Strack and colleagues' pen-in-mouth effect. Seventeen labs conducted preregistered replications of one of Strack et al.'s (1988) two studies, and none of the replications found that the pen smiling manipulation made people feel significantly more amused while viewing cartoons (Wagenmakers et al., 2016; but see Noah, Schul, & Mayo, 2018; Strack, 2016). This failure-to-replicate has revived the debate about the role of participant awareness, although no one has yet considered the cumulative evidence for incidental facial feedback manipulations. Importantly, several other researchers have tested facial feedback effects using incidental facial feedback manipulations, some using the pen-in-mouth manipulation (e.g., Soussignan, 2002) and others creating new incidental manipulations. For example, R. J. Larsen, Kasimatis, and Frey (1992) incidentally manipulated frowning behavior by attaching golf tees to participants' brow regions and asking them to touch the tees together (by pulling the brows downward). By comparing studies that used such incidental facial feedback manipulations with studies that used manipulations more susceptible to demand characteristics, the cumulative evidence for the role of participant awareness can be evaluated.

Effects on Affective Judgments

The central tenet of the facial feedback hypothesis is that facial feedback influences emotional experience. However, many researchers in the facial feedback literature have expanded upon the original focus of the facial feedback hypothesis by suggesting that facial feedback also influences other emotional responses, including those we will call *affective judgments*. We use this term to refer broadly to judgments about the emotional characteristics of some stimulus. For example, a question about the objective funniness of a cartoon can be considered an affective judgment because it is a question about the stimulus, not about how an individual felt when they encountered that stimulus.

Researchers in the facial feedback literature have disagreed about whether facial feedback can influence affective judgments. For example, Strack and colleagues (1988) had participants report both their affective judgments about the cartoons (i.e., how objectively funny they thought the cartoons were) and their emotional experience (i.e., how amused they were by the cartoons). They found evidence that facial feedback influenced emotional experience, but little evidence that it influenced affective judgments. However, others have contended that the effects of facial feedback on emotional experience can subsequently influence affective judgments (Dzokoto, Wallace, Peters, & Bentsi-Enchill, 2014; Ohira & Kurono, 1993). For example, Ohira and Kurono (1993) reported that frowning participants judged a target person to be more negative and that smiling participants judged them to be more positive. Others have suggested that facial feedback can influence affective judgments because these cognitive process are partially grounded in the automatic reactivation of related somatosensory and motor systems (e.g., facial movements; Davis, Winkielman, & Coulson, 2015). In our meta-analysis, we will examine the effects of facial feedback both on emotional experience and affective judgments and assess whether these different outcomes moderate facial feedback effects.

Current Meta-Analysis

The last meta-analysis on the facial feedback hypothesis was performed 30 years ago and revealed a medium effect size ($r = .34$) among 16 studies that included 532 participants (Matsumoto, 1987). Two more recent meta-analyses have included facial feedback effects but either did not address the effects of facial feedback separately from other types of behavioral manipulations (e.g., changing breathing rate; Lench, Flores, & Bench, 2011) or included a very small group of studies ($s = 8$; Westermann, Spies, Stahl, & Hesse, 1996). Given (a) the large number of studies that have been published since the last meta-analysis specifically reviewing the facial feedback hypothesis, (b) recent controversies over the reliability of some facial feedback effects (Wagenmakers et al., 2016), (c) laypersons' belief in the facial feedback hypothesis (e.g., "smile your way to happiness"; Lyubomirsky, 2008), and (d) unresolved theoretical disagreements, we believe that an up-to-date meta-analysis is in order.

Methodological Moderators of Interest

In addition to coding for moderators that addressed the aforementioned theoretical disagreements (i.e., modulation vs. initiation; discrete vs. dimensional; role of awareness; effects on affective judgment vs. experience), we examined potential methodological moderators of facial feedback effects. All moderator coding was completed by three coders (the lead author and two trained research assistants) who discussed and resolved discrepancies throughout the coding process. Coding criteria for each moderator are available in Table 1.

Facial feedback manipulation procedure. Facial feedback has been manipulated in a variety of ways, including tasks that incidentally produce facial postures (e.g., Strack et al., 1988), experimenter-instructed facial posing (e.g., Tourangeau & Ellsworth, 1979), expression suppression (e.g., Gross, 1998), expression exaggeration (e.g., Demaree et al., 2006) and Botox treatments[4] (e.g., Davis, Senghas, Brandt, & Ochsner, 2010). Methodological differences are a common source of variation in effect sizes, and Izard (1990a) speculated that some facial feedback methodologies may produce larger effect sizes than others. We examined this possibility by including manipulation procedure in our moderator analyses.

[4] Studies that examine the effects of Botox on emotional experience are often quasi-experimental in that they compare people who did versus did not opt to receive Botox injections. For ease of communication, we will refer to both experimental and quasi-experimental approaches as *manipulations*.

Table 1
Moderator Coding Criteria

Moderator (bolded) and level	Criteria
Modulation versus Initiation	
Modulation	Emotional stimuli were presented.
Initiation	Either no stimuli were presented *or* nonevocative stimuli/tasks were presented (e.g., neutral images & filler tasks).
Discrete versus Dimensional emotion measure	
Discrete	Measures of discrete emotions (such as anger or happiness) were collected.
Discrete emotion	
Anger	Classified according to the discrete emotions identified in Ekman and Cordaro's (2011) basic emotion theory.[a] Some studies measured emotions that were similar, but not included in Ekman and Cordaro's classification. We categorized these cases into their most similar discrete emotion.
Disgust	
Fear	
Happiness	
Sadness	
Surprise	
Dimensional	Bipolar or separate measures of positive or negative affect were reported.
Dimensional emotion	
Positivity	If the facial feedback manipulation was positive in nature (e.g., smiling) *or* the facial feedback manipulation was neither positive nor negative (e.g., suppression) but the stimuli were positive.
Negativity	If the facial feedback manipulation was negative in nature (e.g., frowning) *or* the facial feedback manipulation was neither positive nor negative (e.g., suppression) but the stimuli were negative.
Awareness of facial feedback manipulation	
Aware	For ease of comparison, only study designs that used a control group comparison were included: Exaggeration-control, Posing-control, Suppression-control. Botox-control was excluded from both levels of this moderator because of uncertainty regarding the degree to which participants recognize the impact of botulinum toxins on facial movements.
Unaware	For ease of comparison, only study designs that used a control group comparison were included: Incidental-control. Botox-control was excluded from both levels of this moderator because of uncertainty regarding the degree to which participants recognize the impact of botulinum toxins on facial movements.
Awareness of video recording	
Yes	Participants were told they were going to be recorded *or* the methodology stated that a video camera was placed within participant view.
No	Methodology stated that participants were unaware of video recording, that the video camera was hidden, *or* that there was no camera.
Emotional experience versus Affective judgments	
Emotional experience	Participants reported their emotional experience (e.g., "How amused did the photo make you feel?").
Affective judgments	Participants reported their affective reaction to the stimulus (e.g., "How funny is the photo?").
Facial feedback manipulation	
Botox–Control	All procedures were coded in a manner that captures both the procedure used in the experimental group and the procedure used in the comparison group.
Exaggeration–Control	
Posing–Control	
Incidental–Control	
Suppression–Control	
Posing–Posing	
Posing–Suppression	
Incidental–Incidental	
Incidental–Suppression	
Suppression–Exaggeration	
Between versus Within-subjects design	
Between-subjects	Effect size estimates from between-subject comparisons.
Within-subjects	Effect size estimates from within-subject comparisons.
Type of stimuli	
Audio	
Film	
Imagined scenarios	
Pictures	
Sentences	
Social context	
Stories	
Gender (Proportion of women)	Calculated using each study's reported gender composition for their entire sample. If studies excluded participants and reported the gender composition of their remaining sample, we used these updated values.

Table 1 (*continued*)

Moderator (bolded) and level	Criteria
Timing of measurement	
During manipulation	Methodology stated participants engaged in the manipulation while providing self-reports *or* participants were instructed to engage in the manipulation throughout the experiment.
After manipulation	Methodology stated participants did not engage in the manipulation while giving self-reports, there was a break between the manipulation and self-reports, *or* participants were instructed to engage in the manipulation at a specific moment in the experiment.
Publication year	
Publication status	
Unpublished	Dissertations, unpublished data, and in-prep manuscripts.
Published	Peer-reviewed articles.

[a] Ekman and Cordaro (2011) included contempt in their list of basic emotions, but no facial feedback studies have investigated contempt.

Effect sizes in this meta-analysis represent the magnitude of the difference between two groups. Therefore, codes for the moderator had to convey the procedure used in both groups. Consequently, we created a moderator variable that captures both the procedure used in the experimental group and the procedure used in the comparison group (for a similar approach, see Webb, Miles, & Sheeran, 2012). For example, if a study compared the effects of posing a smile to the effects of suppressing a smile, it was coded as *posing–suppression*. If the study compared the effects of posing a smile to posing a frown, it was coded as *posing–posing*.

Between versus within-subject designs. An early criticism of the facial feedback literature was that it focused almost exclusively on within-subject designs. Buck (1980) noted that all studies that found evidence for the facial feedback hypothesis to that point had employed within-subject designs, which he suggested raised concerns about demand characteristics. Since then, researchers have used more between-subjects than within-subject designs. To assess whether between- and within-subject designs yield different effect sizes, we investigated the experimental design of an effect-size estimate as a potential methodological moderator.

Type of stimuli. Facial feedback experiments that include emotionally evocative stimuli have used a variety of stimuli, including emotional sounds (e.g., Vieillard, Harm, & Bigand, 2015), images (e.g., Strack et al., 1988), films (e.g., Soussignan, 2002), imagined scenarios (e.g., McCanne & Anderson, 1987), sentences (e.g., Lewis, 2012), stories (e.g., Paredes, Stavraki, Briñol, & Petty, 2013), and emotional social contexts (e.g., Butler et al., 2003). We examined whether stimulus type is a significant moderator of facial feedback effects.

Gender. There are many well-documented gender effects in the emotion literature. For example, researchers have reported gender differences in emotion regulation (Gross & John, 2003; McRae, Ochsner, Mauss, Gabrieli, & Gross, 2008; Nolen-Hoeksema & Aldao, 2011), emotional expressivity (Kring, Smith, & Neale, 1994), and smiling behavior (LaFrance, Hecht, & Paluck, 2003). Some researchers have suggested that there may also be gender differences in bodily feedback effects, like facial feedback. For example, Pennebaker and Roberts (1992) suggested that men rely more on bodily cues than women when making inferences about what emotions they are experiencing. If so, women should show smaller facial feedback effects than men. To examine whether there are gender differences in facial feedback effects, we examined the proportion of women in the sample as a moderator. If women exhibit weaker facial feedback effects, we should find that studies with higher proportions of women have smaller effect sizes.

Awareness of video recording. In a commentary on Wagenmakers et al.'s (2016) failure-to-replicate, Strack (2016) suggested that one reason the results of the original experiment may not have replicated is that cameras were directed at participants in the replication studies. Strack reasoned that awareness of video recording may induce a subjective self-focus that disrupts the flow of experience and suppresses emotional responses. More recently, Noah and colleagues (2018) tested this possibility by manipulating participants' awareness of video recording. They found marginal evidence of the effects of video camera presence. To examine the cumulative evidence for this claim, we coded whether participants were aware versus unaware of video recording.

Timing of measurement. Studies differ in whether the dependent variable is measured during or after the facial feedback manipulation. For example, Reisenzein and Studtmann (2007) had participants maintain a facial configuration until they had completed a measure of emotional experience. In contrast, Duncan and Laird (1980) had participants complete a measure of emotional experience after completing the posing procedure. Research indicates that emotions can be fleeting (Verduyn, Delaveau, Rotgé, Fossati, & Van Mechelen, 2015), so we reasoned that facial feedback effects may be stronger when the dependent measure is assessed during the facial feedback manipulation. To test this hypothesis, we investigated timing of measurement as a moderator.

Additional Moderators of Interest

Publication year. The *decline effect* refers to the observation that effect sizes sometimes get smaller over time (Lehrer, 2010). It is unknown which mechanism produces this phenomenon, but Schooler (2011) suggested that it may be driven by statistical self-correction or publication bias. Yet another possibility is that researchers focus on more nuanced and conceptually weaker effect sizes over time. To test whether there is a decline effect in the facial feedback literature, we tested publication year as a moderator.

Publication status. Publication bias is a well-documented phenomenon in science (Rothstein, Sutton, & Borenstein, 2006). Publication bias poses a risk to meta-analyses if the unpublished literature differs systematically from the published literature. If published studies have larger effect sizes and are more likely to have significant findings than studies that are not published, then a meta-analysis of only the published studies will yield inflated effect size estimates. Fortunately, we were able to gather several

unpublished records for this meta-analysis (reviewed later in Selection of Studies). This moderator was included to test whether published studies had larger effects than the unpublished studies we obtained.

Method

All materials for this meta-analysis are available on the Open Science Framework (https://osf.io/v8kxb/), including (a) preregistered analysis plan, (b) detailed outline of search strategy, (c) list of all screened articles and other reports (e.g., dissertations, unpublished articles) with explanations of exclusions, (d) quotes and rationale behind all moderator and effect size coding decisions, (e) materials and instructions for an open-source plot extraction tool used to extract relevant statistics (e.g., means) that were not reported but were displayed in figures (Rohatgi, 2011), and (f) R code to replicate all analyses. After public discussion of a preprint of this paper and feedback from peer-reviewers, some minor modifications were made to the preregistration plan. Materials detailing these modifications are also available on the Open Science Framework.

Scope

For the purposes of this meta-analysis, we focused only on dependent variables that matched the facial feedback manipulation. For example, if a researcher manipulated whether participants smiled and collected measures of both happiness and sadness, we focused only on the happiness ratings. Although the effects of facial feedback manipulations on nontarget emotions would be theoretically interesting to debates about whether facial poses have emotion-specific effects (e.g., whether posing sadness can produce sadness, but not other discrete negative emotions), this question fell beyond our scope.[5]

Selection of Studies

Our literature search strategy was developed in consultation with an experienced librarian at the University of Tennessee. Additional searches performed after reviewer feedback are denoted with asterisks. Figure 1 is a PRISMA flowchart that outlines the overall process for selecting studies for inclusion in the meta-analysis (Moher, Liberati, Tetzlaff, Altman, & The PRISMA Group, 2009). To gather reports, we searched the following for articles published before 2017:

- PsycINFO: SU.EXACT.EXPLODE ("Feedback") AND SU.EXACT ("Facial Expressions")
- PsycINFO: expressive suppression AND "emotion regulation"
- *PsycINFO: ("embodiment" OR "sensorimotor simulation") AND ("emotion" OR "cognition") AND "face"
- PubMed: feedback[All Fields] AND "facial expressions" [All Fields] OR "facial feedback" OR "facial feedback hypothesis"
- *PubMed: ("embodiment" OR "sensorimotor simulation") AND ("emotion" OR "cognition") AND "face"
- Web of Science: ("feedback" AND "facial expression*" AND emotion) OR ("facial feedback" AND emotion) OR "facial feedback hypothesis"
- *Web of Science: ("embodiment" OR "sensorimotor simulation") AND ("emotion" OR "cognition") AND "face"
- References of 17 reviews on the facial feedback hypothesis (Adelmann & Zajonc, 1989; Buck, 1980; Gerrards-Hesse, Spies, & Hesse, 1994; Izard, 1990b; Laird, 1984; Lench et al., 2011; Martin, 1990; Matsumoto, 1987; McIntosh, 1996; Price & Harmon-Jones, 2015; Price, Peterson, & Harmon-Jones, 2012; Soussignan, 2004; Strack, 2016; Wagenmakers et al., 2016; Webb et al., 2012; Westermann et al., 1996; Whissell, 1985)

To capture the unpublished literature, we conducted the following searches:

- ProQuest Dissertations and Theses Global: "facial feedback hypothesis" AND "emotion"
- *ProQuest Dissertations and Theses Global: ("embodiment" OR "sensorimotor simulation") AND ("emotion" OR "cognition") AND "face")
- Calls for unpublished data: SPSP Open Forum, ResearchGate, Facebook Psychological Methods Discussion Group
- Direct requests for unpublished data from 81 facial feedback researchers identified through our screening process.

After removing duplicate records, there were 1,595 records to screen. The lead author screened the titles and abstracts of these records for studies that manipulated facial movements and measured emotional experience or affective judgments. If there was any doubt about an article's eligibility, it was retained for further review. During this screening, 1,158 full-text reports were excluded, leaving 437 reports to assess for eligibility.

To assess full-text reports for eligibility, the lead author used the following criteria:

1. Facial movements were manipulated. To provide a clear assessment of the facial feedback hypothesis, studies that simultaneously manipulated facial movements and other body postures were excluded.

2. Measures of emotional experience or affective judgments were collected. Studies that measured pain were excluded because previous facial feedback and emotion researchers have argued that pain is not a clearly emotional outcome (e.g., Lumley et al., 2011; McIntosh, 1996).

3. Data from nonclinical samples were reported. If a study examining a clinical sample also included data from a nonclinical sample, only the data from the nonclinical sample was included.

4. Information necessary to compute effect sizes was included (reviewed in Variable Coding).

5. Article was in English.

[5] We did not examine the effects of facial movements on nontarget emotions because it would have further increased the degree to which the effect sizes in the meta-analysis are dissimilar and complicated the analyses. Furthermore, we felt that it was more important to first focus on the simpler question of whether facial feedback influences target emotions before examining whether it can also influence nontarget emotions.

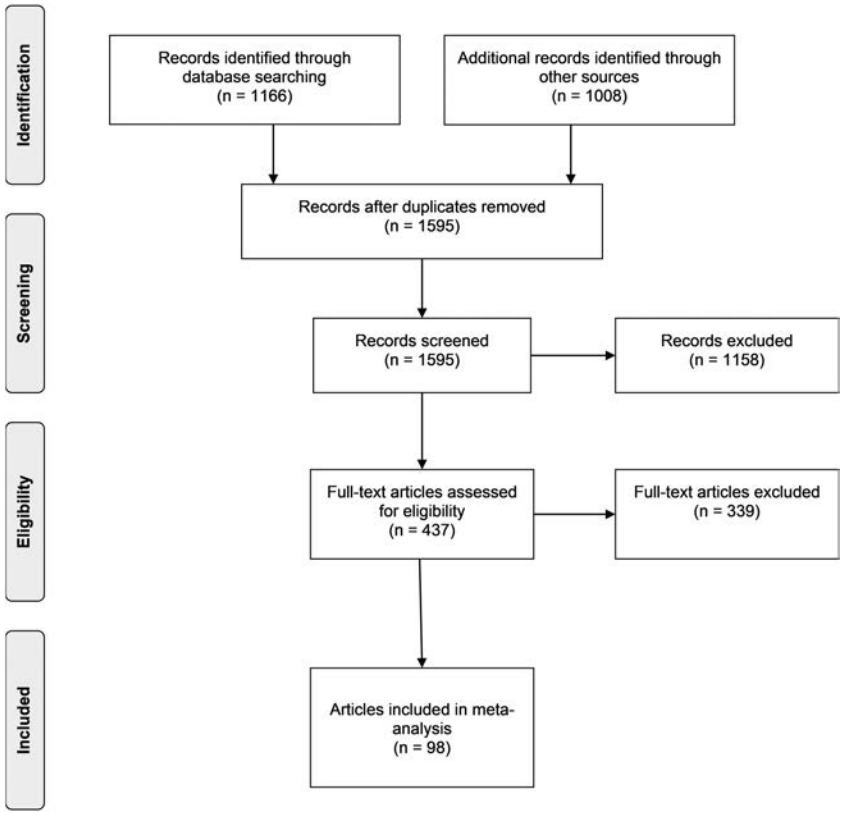


Figure 1. PRISMA-style flowchart showing selection of studies for meta-analysis on facial feedback literature. See the online article for the color version of this figure.

6. Article was a primary study whose relevant results were not reported in a previous record.

Based on these criteria, 98 reports were included that contained a total of 138 studies. From these 138 studies, 286 effect sizes were extracted.

Variable Coding

Moderator coding was completed by three coders (the lead author and two trained research assistants) who discussed and resolved discrepancies throughout the coding process (see Table 1 for coding criteria). The lead author extracted all information related to effect size (sample sizes, means and standard deviation, *t* values, *F* values, or *p* values). If relevant statistics were not included in the report, but informative graphs were included, we used an open-source program to extract data from the graphs (Rohatgi, 2011). If a report did not include additional information or graphs but did indicate whether there was or was not a significant facial feedback effect, we assumed conservative *p* values of .05 or .50, respectively, in our effect size calculations. If the sample size for each condition was not reported in a study with between-subjects comparisons, we estimated sample size by dividing the total sample by the number of conditions.

Meta-Analytic Approach

Effect size index. We used Cohen's standardized *d* as our effect size index, which represents the difference between two group means divided by their pooled standard deviation (Cohen, 1988). Effect sizes were calculated in R 3.4.0 (R Core Team, 2017) using formulas provided by Borenstein (2009). Effect sizes were calculated so that positive values always indicated an effect consistent with the facial feedback hypothesis. For example, the facial feedback hypothesis predicts that facilitating facial expressions leads to increased emotional intensity, whereas suppressing facial expressions leads to decreased emotional intensity. Therefore, increased emotional intensity in a facilitative condition (e.g., Flack, 2006) and decreased emotional intensity in a suppression condition (e.g., Gross & Levenson, 1997) both represent predicted facial feedback effects and were coded in the positive direction.

For within-subject designs, the correlation between the pre- and post- measures is necessary for calculating Cohen's *d*. Unfortunately, this correlation is rarely reported, so it is recommended that meta-analysts assume a correlation and perform a sensitivity analysis on the assumed value (Borenstein, 2009). We preregistered a default correlation of .50 but performed additional analyses to determine the impact of the assumed correlation on the overall effect size estimate (testing $r = .10, .30, .50, .70, 90$). This did not affect the inferences made from the overall effect size, so we only report analyses that used the default $r = .50$ value. All effect sizes are reported in Table 2.

Meta-analysis with robust variance estimates. Fifty-three percent of studies provided multiple effect sizes of interest. For example, Flack, Laird, and Cavallaro (1999b) examined the impact of angry, sad, fearful, and happy facial expression on emotional

Table 2
Characteristics and Effect Size Information of Study Samples Included in the Meta-Analysis (K = 286)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Andréasson & Dimberg (2008)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Film	51.79	During	Published	112	−.22
Andréasson (2010) Study 3	Judgment	—	—	—	NA	No	Incidental–Suppress	Within	Film	—	During	Unpublished	48	−.05
Andréasson (2010) Study 3	Judgment	—	—	—	NA	No	Incidental–Suppress	Within	Film	—	During	Unpublished	48	−.35
Andréasson (2010) Study 4	Judgment	—	—	—	NA	No	Exp.pose–Exp.pose	Within	Film	51.14	During	Unpublished	44	.49
Andréasson (2010) Study 4	Judgment	—	—	—	NA	No	Exp.pose–Exp.pose	Within	Film	51.14	During	Unpublished	44	.31
Baumeister, Papa, and Foroni (2016)	Judgment	—	—	—	NA	No	Botox–Control	Within	Sentences	100	During	Published	10	1.26
Baumeister et al. (2016)	Judgment	—	—	—	NA	No	Botox–Control	Within	Sentences	100	During	Published	10	.63
Bodenhausen, Kramer, and Süsser (1994)	Experience	Initiation	—	—	Aware	No	Exp.pose–Control	Between	NA	72.55	—	Published	51	.55
Bush, Barr, McHugo, and Lanzetta (1989)	Experience	Modulation	Discrete	Happiness	Aware	No	Suppress–Control	Between	Film	46.58	After	Published	69	.16
Butler et al. (2003) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Context	100	After	Published	24	−.1
Butler et al. (2003) Study 2	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Context	100	After	Published	42	−.83
Butler, Wilhelm, and Gross (2006)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	100	After	Published	69	−.03
Cai, Lou, Long, and Yuan (2016)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Within	Pictures	51.47	After	Published	68	−.08
Ceschi & Scherer (2003)	Judgment	—	—	—	Aware	No	Suppress–Control	Between	Context	51.56	—	Published	64	.74
Clapp (2012)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Suppress–Control	Between	Film	56.8	During	Unpublished	99	.69
Clapp (2012)	Experience	Modulation	Discrete	Sadness	Aware	Yes	Suppress–Control	Between	Film	56.8	During	Unpublished	93	.08
Clapp (2012)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Film	56.8	During	Unpublished	93	.17
Clapp (2012)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Between	Film	56.8	During	Unpublished	99	.27

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Davey, Sired, Jones, Meeten, and Dash (2013) Study 1	Experience	Initiation	Discrete	Anger	Unaware	No	Incidental–Control	Between	NA	76.67	During	Published	28	.41
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Anger	Unaware	No	Incidental–Control	Within	NA	76.67	During	Published	14	.62
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Fear	Unaware	No	Incidental–Control	Between	NA	76.67	During	Published	28	.52
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Fear	Unaware	No	Incidental–Control	Within	NA	76.67	During	Published	14	.13
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Disgust	Unaware	No	Incidental–Control	Between	NA	76.67	During	Published	28	.69
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Disgust	Unaware	No	Incidental–Control	Within	NA	76.67	During	Published	14	.42
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Sadness	Unaware	No	Incidental–Control	Between	NA	76.67	During	Published	28	.35
Davey et al. (2013) Study 1	Experience	Initiation	Discrete	Sadness	Unaware	No	Incidental–Control	Within	NA	76.67	During	Published	14	.14
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Anger	Unaware	No	Incidental–Control	Between	NA	80.65	During	Published	29	.73
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Anger	Unaware	No	Incidental–Control	Within	NA	80.65	During	Published	15	.63
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Fear	Unaware	No	Incidental–Control	Between	NA	80.65	During	Published	29	.4
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Fear	Unaware	No	Incidental–Control	Within	NA	80.65	During	Published	15	0
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Disgust	Unaware	No	Incidental–Control	Between	NA	80.65	During	Published	29	.08
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Disgust	Unaware	No	Incidental–Control	Within	NA	80.65	During	Published	15	−.25
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Sadness	Unaware	No	Incidental–Control	Between	NA	80.65	During	Published	29	.03

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Davey et al. (2013) Study 2	Experience	Initiation	Discrete	Sadness	Unaware	No	Incidental–Control	Within	NA	80.65	During	Published	15	−.06
Davis (2008) Study 1	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Between	Film	64.29	After	Unpublished	28	.99
Davis (2008) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Film	64.29	After	Unpublished	28	.87
Davis (2008) Study 2	Experience	Modulation	Dimensional	Positivity	NA	Yes	Exp.pose–Suppress	Between	Film	52.17	After	Unpublished	31	.26
Davis (2008) Study 2	Experience	Modulation	Dimensional	Negativity	NA	Yes	Exp.pose–Suppress	Between	Film	52.17	After	Unpublished	30	−.19
Davis, Senghas, and Ochsner (2009)	Experience	Modulation	Dimensional	Positivity	Aware	No	Suppress–Control	Between	Film	63.43	After	Published	69	.07
Davis et al. (2009)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	63.43	After	Published	69	.51
Davis, Senghas, Brandt, and Ochsner (2010)	Experience	Modulation	Dimensional	Negativity	NA	No	Botox–Control	Between	Film	100	During	Published	68	.1
Davis et al. (2010)	Experience	Modulation	Dimensional	Positivity	NA	No	Botox–Control	Between	Film	100	During	Published	68	.05
Davis et al. (2010)	Experience	Modulation	Dimensional	Positivity	NA	No	Botox–Control	Between	Film	100	During	Published	68	−.15
Davis, Winkielman, and Coulson (2015)	Judgment	—	—	—	NA	No	NA	Within	NA	55.56	—	Published	18	−.16
Demaree, Robinson, Everhart, and Schmeichel (2004)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Exaggerate–Control	Between	Film	49.51	After	Published	53	.62
Demaree et al. (2004)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Exaggerate–Control	Between	Film	49.51	After	Published	50	.16
Demaree et al. (2006)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Exaggerate–Control	Between	Film	52.17	After	Published	32	−.64
Demaree et al. (2006)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Film	52.17	After	Published	35	.06
Demaree et al. (2006)	Experience	Modulation	Dimensional	Negativity	NA	Yes	suppress-exaggerate	Between	Film	52.17	After	Published	37	−.38
Dillon, Ritchey, Johnson, and LaBar (2007)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Within	Pictures	50	After	Published	36	.11

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Dimberg & Söderkvist (2011) Study 1	Experience	Initiation	Dimensional	—	NA	No	Incidental–Incidental	Within	NA	50	During	Published	48	.51
Dimberg & Söderkvist (2011) Study 2	Experience	Modulation	Dimensional	Positivity	NA	No	Incidental–Incidental	Within	Pictures	50	After	Published	96	.1
Dimberg & Söderkvist (2011) Study 2	Experience	Modulation	Dimensional	Negativity	NA	No	Incidental–Incidental	Within	Pictures	50	After	Published	96	.32
Dimberg & Söderkvist (2011) Study 3	Experience	Initiation	Dimensional	—	NA	No	Incidental–Incidental	Within	NA	50.82	During	Published	61	.06
Dimberg & Söderkvist (2011) Study 3	Experience	Modulation	Dimensional	Positivity	NA	No	Incidental–Incidental	Within	Pictures	50.82	During	Published	61	.31
Dimberg & Söderkvist (2011) Study 3	Experience	Modulation	Dimensional	Negativity	NA	No	Incidental–Incidental	Within	Pictures	50.82	During	Published	61	.34
Duncan & Laird (1977)	Experience	Initiation	Discrete	Anger	Aware	Yes	Exp.pose–Control	Within	NA	57.5	After	Published	31	.44
Duncan & Laird (1977)	Experience	Initiation	Discrete	Happiness	Aware	Yes	Exp.pose–Control	Within	NA	57.5	After	Published	31	.38
Duncan & Laird (1977)	Experience	Initiation	Discrete	Happiness	Aware	Yes	Exp.pose–Control	Within	NA	57.5	After	Published	31	.51
Duncan & Laird (1980)	Experience	Initiation	Dimensional	Negativity	Aware	No	Exp.pose–Control	Within	NA	—	After	Published	60	.59
Duncan & Laird (1980)	Experience	Initiation	Dimensional	Positivity	Aware	No	Exp.pose–Control	Within	NA	—	After	Published	60	.44
Dzokoto, Wallace, Peters, and Bentsi-Enchill (2014)	Judgment	—	—	—	Unaware	No	Incidental–Control	Between	Pictures	56.65	During	Published	70	1.02
Dzokoto et al. (2014)	Judgment	—	—	—	Unaware	No	Incidental–Control	Between	Pictures	56.65	During	Published	59	.07
Dzokoto et al. (2014)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	56.65	During	Published	35	1.07
Dzokoto et al. (2014)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	56.65	During	Published	51	.2

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Flack, Laird, & Cavallaro (1999b) Study 1	Experience	Initiation	Discrete	Anger	NA	Yes	Exp.pose–Exp.pose	Within	NA	73.33	After	Published	60	1.2
Flack, Laird, & Cavallaro (1999b) Study 1	Experience	Initiation	Discrete	Disgust	NA	Yes	Exp.pose–Exp.pose	Within	NA	73.33	After	Published	60	.7
Flack, Laird, & Cavallaro (1999b) Study 1	Experience	Initiation	Discrete	Fear	NA	Yes	Exp.pose–Exp.pose	Within	NA	73.33	After	Published	60	.31
Flack, Laird, & Cavallaro (1999b) Study 1	Experience	Initiation	Discrete	Happiness	NA	Yes	Exp.pose–Exp.pose	Within	NA	73.33	After	Published	60	.86
Flack, Laird, & Cavallaro (1999b) Study 1	Experience	Initiation	Discrete	Sadness	NA	Yes	Exp.pose–Exp.pose	Within	NA	73.33	After	Published	60	1.31
Flack, Laird, & Cavallaro (1999b) Study 2	Experience	Initiation	Discrete	Anger	NA	Yes	Exp.pose–Exp.pose	Within	NA	0	After	Published	29	.39
Flack, Laird, & Cavallaro (1999b) Study 2	Experience	Initiation	Discrete	Disgust	NA	Yes	Exp.pose–Exp.pose	Within	NA	0	After	Published	29	.23
Flack, Laird, & Cavallaro (1999b) Study 2	Experience	Initiation	Discrete	Fear	NA	Yes	Exp.pose–Exp.pose	Within	NA	0	After	Published	29	−.16
Flack, Laird, & Cavallaro (1999b) Study 2	Experience	Initiation	Discrete	Happiness	NA	Yes	Exp.pose–Exp.pose	Within	NA	0	After	Published	29	−.49
Flack, Laird, & Cavallaro (1999b) Study 2	Experience	Initiation	Discrete	Sadness	NA	Yes	Exp.pose–Exp.pose	Within	NA	0	After	Published	29	.25
Flack, Laird, & Cavallaro (1999a)	Experience	Initiation	Discrete	Anger	NA	Yes	Exp.pose–Exp.pose	Within	NA	33.33	After	Published	54	1.41
Flack, Laird, & Cavallaro (1999b)	Experience	Initiation	Discrete	Fear	NA	Yes	Exp.pose–Exp.pose	Within	NA	33.33	After	Published	54	.29

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Flack, Laird, & Cavallaro (1999b)	Experience	Initiation	Discrete	Happiness	NA	Yes	Exp.pose–Exp.pose	Within	NA	33.33	After	Published	54	1.18
Flack, Laird, & Cavallaro (1999b)	Experience	Initiation	Discrete	Sadness	NA	Yes	Exp.pose–Exp.pose	Within	NA	33.33	After	Published	54	1.21
Flack (2006)	Experience	Initiation	Discrete	Anger	NA	Yes	Exp.pose–Exp.pose	Within	NA	61.54	After	Published	51	.72
Flack (2006)	Experience	Initiation	Discrete	Fear	NA	Yes	Exp.pose–Exp.pose	Within	NA	61.54	After	Published	51	.35
Flack (2006)	Experience	Initiation	Discrete	Happiness	NA	Yes	Exp.pose–Exp.pose	Within	NA	61.54	After	Published	51	.59
Flack (2006)	Experience	Initiation	Discrete	Sadness	NA	Yes	Exp.pose–Exp.pose	Within	NA	61.54	After	Published	51	.68
Gan, Yang, Chen, and Yang (2015)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Within	Pictures	100	After	Published	34	−.11
Goldin, McRae, Ramel, and Gross (2008)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Within	Film	100	After	Published	17	.8
Gross & Levenson (1993)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Between	Film	49.41	After	Published	85	.04
Gross & Levenson (1997)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Suppress–Control	Between	Film	100	After	Published	180	.37
Gross & Levenson (1997)	Experience	Modulation	Discrete	Sadness	Aware	Yes	Suppress–Control	Between	Film	100	After	Published	180	.16
Gross (1993)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Suppress–Control	Between	Film	100	After	Unpublished	180	.37
Gross (1993)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Suppress–Control	Between	Film	100	After	Unpublished	180	.09
Gross (1993)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Between	Film	100	After	Unpublished	180	.2
Gross (1993)	Experience	Modulation	Discrete	Sadness	Aware	Yes	Suppress–Control	Between	Film	100	After	Unpublished	180	.16
Gross (1993)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Between	Film	100	After	Unpublished	180	−.23
Gross (1998)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Between	Film	100	After	Published	80	.18
Harris (2001)	Experience	Modulation	Discrete	—	Aware	Yes	Suppress–Control	Between	Context	58.33	After	Published	36	.07
Hawk, Fischer, and Van Kleef (2012)	Experience	Modulation	Discrete	Disgust	Aware	No	Suppress–Control	Between	Audio	85.5	After	Published	41	.85
Helt & Fein (2016)	Experience	Modulation	Dimensional	Positivity	Unaware	NA	Incidental–Control	Within	Film	16.28	—	Published	43	.42
Hendricks & Buchanan (2016)	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Within	Pictures	56.96	After	Published	79	−.08
Hendricks (2013)	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Within	Pictures	56.96	After	Unpublished	79	.02
Henry et al. (2007)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Exaggerate–Control	Within	Film	53.33	—	Published	30	−.49
Henry et al. (2007)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Film	53.33	—	Published	30	.25
Henry, Green, et al. (2009)$_a$	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Exaggerate–Control	Within	Film	66.67	—	Published	26	−.05

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Henry, Green, et al. (2009)[a]	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Film	66.67	—	Published	26	.53
Henry, Rendell, et al. (2009)[b]	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Exaggerate–Control	Within	Film	65	—	Published	20	−.05
Henry, Rendell, et al. (2009)[b]	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Film	65	—	Published	20	.48
Hess, Kappas, McHugo, Lanzetta, and Kleck (1992)	Experience	Initiation	Discrete	Anger	Aware	No	Exaggerate–Control	Within	NA	100	After	Published	28	−.28
Hess et al. (1992)	Experience	Initiation	Discrete	Happiness	Aware	No	Exaggerate–Control	Within	NA	100	After	Published	28	.14
Hess et al. (1992)	Experience	Initiation	Discrete	Happiness	Aware	No	Exaggerate–Control	Within	NA	100	After	Published	28	−.26
Hess et al. (1992)	Experience	Initiation	Discrete	Sadness	Aware	No	Exaggerate–Control	Within	NA	100	After	Published	28	−.16
Hofmann, Heering, Sawyer, and Asnaani (2009)	Experience	Modulation	Discrete	Fear	Aware	Yes	Suppress–Control	Between	Context	—	—	Published	134	−.03
Ito, Chiao, Devine, Lorig, and Cacioppo (2006)	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	—	After	Published	40	−.39
Ito et al. (2006)	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Between	NA	—	After	Published	33	−.25
Kalokerinos, Greenaway, and Denson (2015) Study 1	Experience	Modulation	Discrete	Happiness	Aware	No	Suppress–Control	Between	Film	50	After	Published	133.67[b]	−.06
Kalokerinos et al. (2015) Study 1	Experience	Modulation	Discrete	Sadness	Aware	No	Suppress–Control	Between	Film	50	After	Published	133.67[b]	−.02
Kalokerinos et al. (2015) Study 2	Experience	Modulation	Discrete	Happiness	Aware	No	Suppress–Control	Between	Film	43	After	Published	295	1.32
Kalokerinos et al. (2015) Study 2	Experience	Modulation	Discrete	Sadness	Aware	No	Suppress–Control	Between	Film	43	After	Published	295	.2
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	Aware	No	Exaggerate–Control	Between	Context	50.41	After	Published	41	.09

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	Aware	No	Exaggerate–Control	Between	Context	50.41	After	Published	41	−.39
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	Aware	No	Suppress–Control	Between	Context	50.41	After	Published	41	.8
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	Aware	No	Suppress–Control	Between	Context	50.41	After	Published	41	−.34
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	NA	No	Suppress–Exaggerate	Between	Context	50.41	After	Published	41	.98
Kao et al. (2017)	Experience	Modulation	Discrete	Anger	NA	No	Suppress–Exaggerate	Between	Context	50.41	After	Published	41	−.67
Kircher et al. (2013)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Exp.pose–Control	Within	Pictures	53.13	After	Published	27	1.89
Kircher et al. (2013)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Exp.pose–Control	Within	Pictures	53.13	After	Published	27	1.14
Korb, Grandjean, Samson, Delplanque, and Scherer (2012)	Experience	Modulation	Discrete	Happiness	Aware	No	Suppress–Control	Within	Pictures	100	—	Published	22	.21
Labott & Teleha (1996)	Experience	Modulation	Dimensional	Negativity	NA	No	Suppress–Exaggerate	Between	Film	100	After	Published	19	.04
Labott & Teleha (1996)	Experience	Modulation	Dimensional	Negativity	NA	No	Suppress–Exaggerate	Between	Film	100	After	Published	16	.91
Laird (1974) Study 1	Experience	Modulation	Discrete	Anger	NA	No	Exp.pose–Exp.pose	Within	Pictures	—	—	Published	38	.46
Laird (1974) Study 1	Experience	Modulation	Discrete	Happiness	NA	No	Exp.pose–Exp.pose	Within	Pictures	—	—	Published	38	.44
Laird (1974) Study 1	Experience	Modulation	Discrete	Happiness	NA	No	Exp.pose–Exp.pose	Within	Pictures	—	—	Published	38	.39
Laird (1974) Study 2	Judgment	—	—	—	NA	No	Exp.pose–Exp.pose	Within	Pictures	—	—	Published	26	.55
Laird (1974) Study 2	Experience	—	Discrete	Happiness	NA	No	Exp.pose–Exp.pose	Within	Pictures	—	—	Published	26	.13
Laird and Crosby (1974) Study 1	Experience	Modulation	Dimensional	Positivity	NA	No	Exp.pose–Exp.pose	Within	Pictures	50	—	Published	26	−.13
Laird and Crosby (1974) Study 2	Experience	Modulation	Dimensional	Positivity	NA	No	Exp.pose–Exp.pose	Within	Pictures	50	—	Published	26	.35
Lalot, Delplanque, and Sander (2014)	Judgment	—	—	—	Aware	Yes	Suppress–Control	Within	Film	66.67	After	Published	45	−.17
R. J. Larsen, Kasimatis, and Frey (1992)	Experience	Modulation	Discrete	Sadness	NA	No	Incidental–Suppress	Within	Pictures	30	During	Published	27	.43

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Lee (2011)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Exaggerate–Control	Within	Film	54.17	After	Unpublished	52	.48
Lee (2011)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Exaggerate–Control	Within	Film	54.17	After	Unpublished	44	.17
Lee (2011)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Within	Film	54.17	After	Unpublished	52	−.27
Lee (2011)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Within	Film	54.17	After	Unpublished	44	−.26
Lewis & Bowler (2009)	Experience	Modulation	Dimensional	Negativity	NA	No	Botox–Control	Between	NA	100	During	Published	25	1.35
Lewis (2012)	Judgment	—	—	—	NA	No	Exp.pose–Exp.pose	Within	Sentences	100	During	Published	24	.71
Lewis (2012)	Judgment	—	—	—	NA	No	Exp.pose–Suppress	Within	Sentences	100	During	Published	24	.56
Ma (2011)	Experience	Modulation	Discrete	Fear	Aware	Yes	Suppress–Control	Between	Film	23.44	—	Unpublished	42.67[b]	−.21
Ma (2011)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Between	Film	23.44	—	Unpublished	42.67[b]	−.21
Ma (2011)	Experience	Modulation	Discrete	Sadness	Aware	Yes	Suppress–Control	Between	Film	23.44	—	Unpublished	42.67[b]	−.21
Ma (2011)	Experience	Modulation	Discrete	Happiness	Aware	Yes	Suppress–Control	Between	Film	23.44	—	Unpublished	42.67[b]	−.21
Maldonado, DiLillo, and Hoffman (2015)	Experience	Modulation	Discrete	Anger	Aware	NA	Suppress–Control	Between	Stories	58.47	After	Unpublished	157.33[b]	.12
Marmolejo-Ramos & Dunn (2013) Study 1	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	78.85	—	Published	100	−.07
Marmolejo-Ramos & Dunn (2013) Study 2	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	75.47	—	Published	106	−.07
Marmolejo-Ramos & Dunn (2013) Study 3	Experience	Modulation	Dimensional	Positivity	NA	No	Incidental–Suppress	Within	Pictures	73.08	—	Published	104	−.07
Marmolejo-Ramos & Dunn (2013) Study 4	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	63	—	Published	100	−.07
Marmolejo-Ramos & Dunn (2013) Study 5	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	71.21	—	Published	66	.27
Marmolejo-Ramos & Dunn (2013) Study 6	Experience	Initiation	Dimensional	Positivity	Unaware	No	Incidental–Control	Within	NA	61.19	—	Published	67	.38
Martijn, Tenbült, Merckelbach, Dreezens, and de Vries (2002)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	86.79	After	Published	33	−.24
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Exaggerate–Control	Within	Context	100	After	Published	30	−2.16

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Exaggerate–Control	Within	Imagined scenarios	100	After	Published	30	−2.07
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Imagined scenarios	100	After	Published	30	4.73
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Within	Imagined scenarios	100	After	Published	30	1.67
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Suppress–Exaggerate	Within	Imagined scenarios	100	After	Published	30	2.48
McCanne & Anderson (1987)	Experience	Modulation	Dimensional	Negativity	NA	Yes	Suppress–Exaggerate	Within	Imagined scenarios	100	After	Published	30	−.25
McCaul, Holmes, and Solomon (1982)	Experience	Initiation	Discrete	Fear	Aware	Yes	Exp.pose–Control	Within	NA	55.56	After	Published	27	.25
McIntosh, Zajonc, Vig, and Emerick (1997)	Experience	Initiation	Dimensional	Negativity	NA	No	Incidental–Incidental	Within	NA	50	After	Published	26	.54
Meeten et al. (2015)	Judgment	—	—	—	NA	No	Exp.pose–Exp.pose	Within	Pictures	76.06	After	Published	71	.49
Miyamoto (2006) Study 1	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	24.69	During	Unpublished	40	.17
Miyamoto (2006) Study 1	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	24.69	During	Unpublished	40	.53
Miyamoto (2006) Study 2	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	60	During	Unpublished	77	.49
Moore & Zoellner (2012)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	—	After	Published	23.33[b]	−.87
Kappas (1989)	Judgment	—	—	—	Aware	No	Exaggerate–Control	Within	Film	43.75	—	Unpublished	32	.08
Kappas (1989)	Judgment	—	—	—	Aware	No	Exaggerate–Control	Within	Film	43.75	—	Unpublished	32	.26
Kappas (1989)	Judgment	—	—	—	Aware	No	Suppress–Control	Within	Film	43.75	—	Unpublished	32	.27
Kappas (1989)	Judgment	—	—	—	Aware	No	Suppress–Control	Within	Film	43.75	—	Unpublished	32	.1
Kappas (1989)	Experience	Modulation	Discrete	Disgust	Aware	No	Exaggerate–Control	Within	Film	43.75	—	Unpublished	32	.17
Kappas (1989)	Experience	Modulation	Discrete	Happiness	Aware	No	Exaggerate–Control	Within	Film	43.75	—	Unpublished	32	.52
Kappas (1989)	Experience	Initiation	Discrete	Disgust	NA	No	Exp.pose–Exp.pose	Within	NA	43.75	—	Unpublished	32	.62
Kappas (1989)	Experience	Initiation	Discrete	Happiness	NA	No	Exp.pose–Exp.pose	Within	NA	43.75	—	Unpublished	32	.74
Kappas (1989)	Experience	Modulation	Discrete	Disgust	Aware	No	Suppress–Control	Within	Film	43.75	—	Unpublished	32	.18
Kappas (1989)	Experience	Modulation	Discrete	Happiness	Aware	No	Suppress–Control	Within	Film	43.75	—	Unpublished	32	.42
Ohira & Kurono (1993) Study 1	Judgment	—	—	—	Aware	No	Exaggerate–Control	Between	Context	100	After	Published	20	1.23

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Ohira & Kurono (1993) Study 1	Judgment	—	—	—	Aware	No	Suppress–Control	Between	Context	100	After	Published	20	.31
Ohira & Kurono (1993) Study 2	Judgment	—	—	—	Aware	No	Exaggerate–Control	Between	Context	100	After	Published	20	1.61
Ohira & Kurono (1993) Study 2	Judgment	—	—	—	Aware	No	Suppress–Control	Between	Context	100	After	Published	20	−1.38
Paredes, Stavraki, Briñol, and Petty (2013)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Stories	—	—	Published	31	.85
Paul, Simon, Kniesche, Kathmann, and Endrass (2013)	Judgment	—	—	—	Aware	No	Suppress–Control	Within	Pictures	50	—	Published	20	.91
Pedder et al. (2016)	Experience	Modulation	Dimensional	Positivity	Aware	NA	Suppress–Control	Within	Pictures	64.29	After	Published	68	.7
Pedder et al. (2016)	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Within	Pictures	64.29	After	Published	68	.22
Phillips, Henry, Hosie, and Milne (2008)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Film	54.7	After	Published	32	.18
Phillips et al. (2008)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	film	54.7	After	Published	32	.08
Reisenzein & Studtmann (2007) Study 1	Experience	Initiation	Discrete	Surprise	Aware	No	Exp.pose–Control	Between	NA	61.25	During	Published	53	.18
Reisenzein & Studtmann (2007) Study 1	Experience	Initiation	Discrete	Surprise	Aware	No	Exp.pose–Control	Between	NA	61.25	During	Published	55	.34
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	Aware	No	Exp.pose–Control	Between	Pictures	61.25	During	Published	55	−.08
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	Aware	No	Exp.pose–Control	Between	Pictures	61.25	During	Published	55	.3

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	NA	No	Exp.pose–Suppress	Between	Pictures	61.25	During	Published	53	−.12
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	NA	No	Exp.pose–Suppress	Between	Pictures	61.25	During	Published	53	.22
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	Aware	No	Suppress–Control	Between	Pictures	61.25	During	Published	52	−.04
Reisenzein & Studtmann (2007) Study 1	Experience	Modulation	Discrete	Surprise	Aware	No	Suppress–Control	Between	Pictures	61.25	During	Published	52	−.09
Reisenzein & Studtmann (2007) Study 3	Experience	Modulation	Discrete	Surprise	Aware	No	Exp.pose–Control	Between	Pictures	50	After	Published	40	−.74
Richards, Butler, & Gross (2003)	Experience	Modulation	Dimensional	Positivity	Aware	No	Suppress–Control	Between	Context	50	After	Published	59	.19
Richards et al. (2003)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Context	50	After	Published	59	−.12
Richards & Gross (1999) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	58	−.1
Richards & Gross (1999) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	58	.25
Richards & Gross (1999) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	58	.36
Richards & Gross (1999) Study 2	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	85	.13
Richards & Gross (1999) Study 2	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	85	.24
Richards & Gross (1999) Study 2	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Between	Pictures	100	After	Published	85	.06
Richards & Gross (2000) Study 1	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	55	After	Published	53	−.12

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Richards & Gross (2000) Study 2	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Pictures	100	After	Published	61	.39
Richards & Gross (2006)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Between	Film	65	After	Published	131	.34
Roberts, Levenson, & Gross (2008)	Experience	Modulation	Discrete	Disgust	Aware	No	Suppress–Control	Between	Film	60	After	Published	160	.07
Rohrmann, Hopp, Schienle, & Hodapp (2009)	Experience	Modulation	Dimensional	Negativity	Aware	NA	Exaggerate–Control	Within	Film	50.98	After	Published	102	−.04
Robinson & Demaree (2009)	Experience	Modulation	Discrete	Sadness	Aware	NA	Exaggerate–Control	Within	Film	50.98	After	Published	102	.03
Robinson & Demaree (2009)	Experience	Modulation	Dimensional	Negativity	Aware	NA	Suppress–Control	Within	Film	50.98	After	Published	102	0
Robinson & Demaree (2009)	Experience	Modulation	Discrete	Sadness	Aware	NA	Suppress–Control	Within	Film	50.98	After	Published	102	0
Roemer (2014)	Experience	Modulation	Dimensional	Positivity	Unaware	Yes	Incidental–Control	Between	Film	81.82	After	Unpublished	44	.58
Roemer (2014)	Experience	Modulation	Dimensional	Positivity	Unaware	Yes	Incidental–Control	Between	Film	81.82	After	Unpublished	44	.29
Rohrmann, Hopp, Schienle, & Hodapp (2009)	Experience	Modulation	Discrete	Disgust	Aware	NA	Suppress–Control	Between	Film	0	After	Published	36	.16
Rohrmann, Hopp, Schienle, & Hodapp (2009)	Experience	Modulation	Discrete	Disgust	Aware	NA	Suppress–Control	Between	Film	0	After	Published	36	.13
Rummer, Schweppe, Schlegelmilch, & Grice (2014)	Judgment	—	—	—	NA	No	Incidental–Incidental	Between	Pictures	—	During	Published	74	.57
Rummer, Schweppe, Schlegelmilch, & Grice (2014)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	—	During	Published	74	.46
Schmeichel, Vohs, & Baumeister (2003)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Between	Film	59.46	After	Published	37	−.23

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Schmeichel, Volokhov, & Demaree (2008)	Experience	Modulation	Dimensional	Positivity	Aware	NA	Suppress–Control	Between	Film	62	During	Published	50	.1
Söderkvist & Dimberg (unpublished)	Experience	Modulation	Dimensional	—	NA	No	Incidental–Incidental	Within	Pictures	50	During	Unpublished	32	.36
Söderkvist, Ohlén, & Dimberg (2018) Study 1[a]	Experience	Modulation	Dimensional	—	NA	No	Incidental–Incidental	Within	Pictures	50	During	Unpublished	32	.34
Söderkvist et al. (2018) Study 2[a]	Experience	Modulation	Dimensional	—	NA	No	Incidental–Incidental	Within	Pictures	50	During	Unpublished	64	.17
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	incidental-suppress	Between	Film	100	After	Published	33	−.17
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	incidental-suppress	Between	Film	100	After	Published	33	.48
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	33	.47
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	33	.44
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	32	.53
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	32	1.1
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	32	1.11
Soussignan (2002)	Experience	Modulation	Dimensional	Positivity	NA	Yes	Incidental–Suppress	Between	Film	100	After	Published	32	.94
Stel, van den Heuvel, & Smeets (2008) Study 2	Experience	Initiation	Dimensional	Positivity	NA	No	NA	Between	NA	—	After	Published	18.67[b]	1.11
Stel et al. (2008) Study 3	Judgment	—	—	—	Unaware	No	Incidental–Control	Between	Pictures	—	During	Published	24	1
Strack, Martin, & Stepper (1988) Study 1	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	—	During	Published	76.67[b]	.43
Strack et al. (1988) Study 2	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	45.78	During	Published	83	−.15
Strack et al. (1988) Study 2	Experience	Modulation	Discrete	Happiness	NA	No	Incidental–Suppress	Between	Pictures	45.78	During	Published	41.5	.55

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Strack et al. (1988) Study 2	Experience	Modulation	Discrete	Happiness	NA	No	Incidental–Suppress	Between	Pictures	45.78	During	Published	41.5	−.51
Tamir, Robinson, Clore, Martin, and Whitaker (2004)	Experience	Modulation	Dimensional	Positivity	NA	No	Exp.pose–Exp.pose	Between	Pictures	—	After	Published	72	−.16
Tourangeau & Ellsworth (1979)	Experience	Initiation	Discrete	Fear	Aware	Yes	Exp.pose–Control	Between	NA	—	After	Published	20.5[b]	.3
Tourangeau & Ellsworth (1979)	Experience	Initiation	Discrete	Sadness	Aware	Yes	Exp.pose–Control	Between	NA	—	After	Published	20.5[b]	.3
Tourangeau & Ellsworth (1979)	Experience	Modulation	Discrete	Fear	Aware	Yes	Exp.pose–Control	Between	Film	—	After	Published	20.5[b]	.3
Tourangeau & Ellsworth (1979)	Experience	Modulation	Discrete	Sadness	Aware	Yes	Exp.pose–Control	Between	Film	—	After	Published	20.5[b]	.3
Trent (2010)	Judgment	—	—	—	Unaware	No	Incidental–Control	Between	Pictures	74.07	After	Unpublished	107.33[b]	−.22
Trent (2010)	Judgment	—	—	—	NA	No	Incidental–Suppress	Between	Pictures	74.07	After	Unpublished	107.33[b]	−.22
Trent (2010)	Experience	Modulation	Dimensional	Positivity	Unaware	No	Incidental–Control	Between	Pictures	74.07	After	Unpublished	107.33[b]	−.06
Trent (2010)	Experience	Modulation	Dimensional	Positivity	NA	No	Incidental–Suppress	Between	Pictures	74.07	After	Unpublished	107.33[b]	−.06
Vieillard, Harm, and Bigand (2015)	Experience	Modulation	Dimensional	Positivity	Aware	No	Exaggerate–Control	Within	Audio	59.02	After	Published	31	.25
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Negativity	Aware	No	Exaggerate–Control	Within	Audio	59.02	After	Published	31	.66
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Positivity	Aware	No	Exaggerate–Control	Within	Audio	59.02	After	Published	30	.21
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Negativity	Aware	No	Exaggerate–Control	Within	Audio	59.02	After	Published	30	.14
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Positivity	Aware	No	Suppress–Control	Within	Audio	59.02	After	Published	31	−.05
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Within	Audio	59.02	After	Published	31	−.5
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Positivity	Aware	No	Suppress–Control	Within	Audio	59.02	After	Published	30	.07
Vieillard et al. (2015)	Experience	Modulation	Dimensional	Negativity	Aware	No	Suppress–Control	Within	Audio	59.02	After	Published	30	−.12
Wagenmakers et al. (2016) Albohn site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	55.21	During	Published	139	.09
Wagenmakers et al. (2016) Allard site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	74.85	During	Published	125	.09

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Wagenmakers et al. (2016) Benning site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	58.04	During	Published	115	−.01
Wagenmakers et al. (2016) Bulnes site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	83.33	During	Published	101	.09
Wagenmakers et al. (2016) Capaldi site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	69.33	During	Published	117	−.07
Wagenmakers et al. (2016) Chasten site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	70.37	During	Published	94	−.04
Wagenmakers et al. (2016) Holmes site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	63.98	During	Published	99	.15
Wagenmakers et al. (2016) Koch site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	66.38	During	Published	100	−.14
Wagenmakers et al. (2016) Korb site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	37.93	During	Published	101	.01
Wagenmakers et al. (2016) Lynott site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	38.61	During	Published	126	.23
Wagenmakers et al. (2016) Oosterwijk site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	30.2	During	Published	110	−.17
Wagenmakers et al. (2016) Ozdogru site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	35.03	During	Published	87	−.3
Wagenmakers et al. (2016) Pacheco-Unguetti site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	24.32	During	Published	120	−.08
Wagenmakers et al. (2016) Talarico site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	23.27	During	Published	112	.02
Wagenmakers et al. (2016) Wagenmakers site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	37.02	During	Published	130	.13
Wagenmakers et al. (2016) Wayand site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	18	During	Published	110	−.14

(*table continues*)

Table 2 (*continued*)

Study	Experience or judgment	Modulation or initiation	Discrete or dimensional	Emotion	Awareness of manipulation	Awareness of recording	Manipulation	Design	Stimuli	% of women	Measurement timing	Publication status	*N*	*d*
Wagenmakers et al. (2016) Zeelenberg site	Experience	Modulation	Discrete	Happiness	NA	Yes	Incidental–Suppress	Between	Pictures	22.76	During	Published	108	.25
Wittmer (1985)	Experience	Modulation	Discrete	Fear	Aware	Yes	Exaggerate–Control	Within	Film	0	—	Unpublished	30	−.36
Wittmer (1985)	Experience	Modulation	Discrete	Fear	Aware	Yes	Suppress–Control	Within	Film	0	—	Unpublished	30	−.21
Yartz (2003)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	28	−.05
Yartz (2003)	Experience	Modulation	Discrete	Disgust	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	30	−.18
Yartz (2003)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	28	−.08
Yartz (2003)	Experience	Modulation	Dimensional	Negativity	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	30	−.09
Yartz (2003)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	28	.04
Yartz (2003)	Experience	Modulation	Dimensional	Positivity	Aware	Yes	Suppress–Control	Within	Pictures	41.38	—	Unpublished	30	.5
Zajonc, Murphy, and Inglehart (1989) Study 3	Judgment	—	—	—	NA	No	Incidental–Incidental	Within	Audio	—	After	Published	37	1.27
Zajonc et al. (1989) Study 4	Experience	Initiation	Dimensional	—	NA	NA	Incidental–Incidental	Within	NA	0	After	Published	26	.47
Zajonc et al. (1989) Study 4	Experience	Initiation	Dimensional	—	NA	NA	Incidental–Incidental	Within	NA	0	After	Published	26	.31
Zariffa, Hitzig, and Popovic (2014)	Experience	Initiation	Dimensional	Positivity	Aware	No	Exp.pose–Control	Within	NA	50	After	Published	24	−.57
Zariffa et al. (2014)	Experience	Initiation	Dimensional	Positivity	Aware	No	Exp.pose–Control	Within	NA	50	After	Published	24	−.14
Zhu, Cai, Sun, and Yang-yang (2015)	Experience	Initiation	Discrete	Disgust	NA	Yes	Exp.pose–Exp.pose	Between	NA	74.55	After	Published	55	1.74

Note. A more detailed data file is available on the Open Science Framework. *N* = total sample size for two-group comparison; *d* = Cohen's standardized difference.
[a] Results were unpublished at time of meta-analysis but are now published in Söderkvist et al. (2018). [b] Estimated sample size.

experience. When a study provides multiple effect size estimates, it is best to record all effect sizes to be comprehensive. However, one drawback of this approach is that it violates the statistical assumption that effect sizes are independent. There are several ways to deal with dependency in meta-analysis. The simplest approach is to aggregate effect sizes drawn from the same study (Borenstein, Hedges, Higgins, & Rothstein, 2009; Rosenthal & Rubin, 1986). Although this removes dependency, it results in a loss of information regarding comparisons among multiple levels of a moderator in a single study. A second approach is to use multivariate meta-regression (Raudenbush, Becker, & Kalaian, 1988). However, this approach requires knowledge of the underlying covariation structure among effect sizes, which is almost always unknown. A third approach is to use meta-analysis with robust variance estimates (RVE; Hedges, Tipton, & Johnson, 2010). Similar to its application in general linear models, RVE can be used in meta-analysis to adjust for dependencies among effect sizes. This approach does not result in the loss of any information, does not require knowledge of the underlying correlation structure, and can accommodate multiple sources of dependencies. We use this RVE approach to estimate our overall effect size, conduct moderator analyses, and perform most of our publication bias analyses.[6]

Meta-analysis with RVE weighting scheme. When averaging the results of multiple studies, meta-analyses typically give more weight to effect sizes with higher precision (i.e., smaller variance) via a procedure termed inverse-variance weighting. Meta-analysis with robust variance estimates uses similar weighting schemes that provide adjustments for the types of dependency among effect sizes. If dependency primarily arises from studies providing multiple effect sizes for the same outcome of interest, the correlated effects weighting scheme is recommended. On the other hand, if dependency primarily arises from authors reporting multiple studies, the hierarchical effects weighting scheme is recommended (Hedges et al., 2010). In practice, both types of dependencies often exist in a meta-analysis, and it is recommended to choose weighting based on the predominant type of dependency (Tanner-Smith & Tipton, 2014). Twenty-one percent of the reports in the present meta-analysis included multiple studies, and 53% of the reports included studies that provided multiple effect sizes for the outcome of interest. Therefore, we used the correlated effects weighting scheme.

When calculating weights, meta-analysis with RVE requires an estimate of the within-study effect-size correlation (i.e., the average correlation among the dependent effect sizes). The default assumed value is $r = .80$. We preregistered this as the default value to inform our conclusions but performed additional sensitivity analyses to determine the impact of this assumed value on our overall effect estimate (testing $r = 0, .20, .40, .60, .80, 1.00$). This did not affect inferences about effect sizes, so we only report analyses that used the default value of $r = .80$.

Testing overall effects and moderators. To test the overall effect size, we fit an intercept-only random-effects meta-regression model with RVE using the R package, robumeta (Fisher & Tipton, 2015). The intercept of this model can be interpreted as the precision-weighted overall effect size, adjusted for correlated-effect dependencies. We used the same approach to calculate overall effect sizes for each level of each moderator. For cases where a level of a moderator had too few observations for the RVE approach, we calculated overall effect sizes using random-effects meta-regression models (these exceptions are noted in Table 3).

We also used the RVE approach to perform separate hypothesis tests for the effects of each moderator.[7] Continuous moderators were entered into a meta-regression equation without transformation, except publication year, which was centered at 2017 to ease interpretation of the regression intercept. Categorical moderators with two levels (i.e., type of experience) were dummy coded and entered into meta-regression equations. The significance test corresponding to the regression coefficient for the predictor variable in these models can be interpreted as a test of whether the variable is a significant moderator.

Examining categorical moderators with more than two levels required an additional step. Like the former process, they were first dummy coded and entered into meta-regression equations. However, the regression coefficients only test whether there is a difference between a single level of a moderator and a single comparison level. To perform an omnibus test of moderators with more than two levels, we followed the recommendations of Tanner-Smith, Tipton, and Polanin (2016) and conducted Approximate Hotelling-Zhang with small sample correction tests using the clubSandwhich R package (Pustejovsky, 2017). This test produces an F value that indicates whether there is a difference among all levels of the moderator. We forewarn the reader that the Approximate Hotelling-Zhang produces atypical degrees of freedom, and refer the curious reader to Tanner-Smith et al. (2016) for a more detailed explanation.

Notably, moderator analyses typically need a large amount of observations to achieve high power (Hedges & Pigott, 2004), and the power to detect moderators is reduced by higher levels of heterogeneity and robust variance estimation procedures. Consequently, null effects in our tests of moderation should be cautiously interpreted.

Outlier detection. Methods for identifying outliers for meta-regression models with RVE are not yet available, so we identified outliers in a random-effects intercept-only meta-regression model using the base R function influence.measures. After fitting an intercept-only meta-regression model, this function calculates a variety of influential outlier diagnostics (such as covariance ratios, Cook's distances, and diagonal elements of the hat matrix), and identifies cases that are influential on any one of the diagnostic criteria.

Examining publication bias. Many methods for testing the extent and impact of publication bias in meta-analysis have been developed. Unfortunately, most of these methods were developed and tested under the assumption that the effect sizes are independent, which is typically unrealistic in meta-analyses of the psy-

[6] Although we believe that meta-analysis with RVE was the best approach for our data analysis, we also calculated the overall effect size using the Borenstein et al. (2009) aggregation method for correcting for dependencies, three-level meta-analysis (Van den Noortgate, Lopez-Lopez, Marin-Martinez, & Sanchez-Meca, 2015), and a random-effects meta-analysis without corrections for dependencies. We obtained results that were nearly identical to those generated by the RVE approach. Therefore, we only report the results of the RVE approach.

[7] In our preregistration plan, we also noted that we would re-examine important theoretical moderators with any significant methodological moderators we find included as covariates. These analyses did not affect our conclusions, so we do not report them here.

Table 3
Moderator Analyses

Moderator (bolded) and level	*s*	*k*	*d*	β_1	*F*	95% CI	*p*
Modulation versus Initiation of emotion	117	246	—	.19	—	[−.37, −.01]	.04
Modulation	93	179	.13	—	—	[.07, .18]	.00006
Initiation	28	67	.32	—	—	[.15, .49]	.0005
Discrete versus Dimensional emotion measure	117	246	—	.05	—	[−.07, .18]	.42
Discrete	57	130	.19	—	—	[.09, .29]	.0003
Discrete emotion	56	129	—	—	.77	—	.60
Anger	11	18	.53	—	—	[.19, .87]	.006
Disgust	14	23	.29	—	—	[.03, .56]	.03
Fear	11	15	.13	—	—	[−.05, .3]	.13
Happiness	36	44	.23	—	—	[.08, .37]	.004
Sadness	16	20	.30	—	—	[.06, .55]	.02
Surprise	2	9	−.31	—	—	[−5.57, 4.95]	.59
Dimensional	64	116	.14	—	—	[.06, .21]	.0005
Dimensional emotion	59	109	—	.04	—	[−.12, .19]	.64
Positivity	36	57	.18	—	—	[.07, .28]	.002
Negativity	37	52	.12	—	—	[.01, .22]	.03
Awareness of facial feedback manipulation	81	176	—	.004	—	[−.19, .19]	.97
Aware	67	145	.15	—	—	[.06, .24]	.001
Unaware	14	31	.13	—	—	[−.05, .31]	.15
Awareness of video recording	127	265	—	−.06	—	[−.20, .07]	.36
Yes	54	116	.17	—	—	[.06, .28]	.003
No	73	149	.23	—	—	[.15, .32]	.0000007
Emotional experience versus Affective judgments	138	286		.24	—	[.04, .44]	.02
Emotional experience	118	247	.17	—	—	[.11, .23]	.0000004
Affective judgments	24	39	.38	—	—	[.19, .57]	.0004
Facial feedback manipulation	136	284	—	—	1.62[b]	—	.20
Botox–Control	3	6	.71	—	—	[−1.07, 2.49]	.23
Exaggeration–Control	15	29	−.04	—	—	[−.41, .33]	.82
Posing–Control	9	20	.30	—	—	[−.16, .76]	.17
Incidental–Control	14	31	.13	—	—	[−.05, .31]	.15
Suppression-Control	57	96	.15	—	—	[.04, .25]	.006
Posing–Posing	14	33	.51	—	—	[.26, .76]	.0007
Posing–Suppression	3	5	.26	—	—	[−.55, 1.08]	.30
Incidental–Incidental	10	14	.43	—	—	[.22, .63]	.001
Incidental–Suppression	30	43	.07	—	—	[−.02, .16]	.11
Suppression–Exaggeration	4	7	.34	—	—	[−.68, 1.36]	.36
Between versus Within-subjects design	138	286	—	.09	—	[−.03, .21]	.14
Between	80	150	.16	—	—	[.08, .24]	.0001
Within	60	136	.25	—	—	[.16, .34]	.000001
Stimuli	112	217	—	—	92.83	—	.003
Audio	3	10	.72	—	—	[−.82, 2.27]	.18
Film	42	94	.13	—	—	[.03, .22]	.009
Imagined scenarios[a]	1	5	1.28	—	—	[−.98, 3.53]	.27
Pictures	53	84	.16	—	—	[.08, .23]	.0002
Sentences[a]	2	4	.70	—	—	[.43, .96]	.0000003
Social context	10	18	−.14	—	—	[−.74, .46]	.61
Stories[a]	2	2	.41	—	—	[−.29, 1.10]	.25
Proportion of women (0–100)	122	261	—	.17	—	[−.09, .42]	.21
Timing of measurement	113	237	—	−.03	—	[−.17, .11]	.65
During manipulation	42	81	.18	—	—	[.09, .26]	.0001
After manipulation	71	156	.22	—	—	[.12, .33]	.00008
Publication year	135	283	—	−.01	—	[−.01, .001]	.06
Publication status	138	286	—	−.05	—	[−.18, .08]	.45
Unpublished	20	57	.15	—	—	[.04, .26]	.01
Published	118	229	.21	—	—	[.14, .28]	.00000003

Note. *k* = number of effect size estimates; *s* = number of studies; *d* = Cohen's standardized difference; β_1 coefficients are from separate meta-regressions with RVE where a continuous moderator was entered in the model as a predictor or a categorical moderator with two levels was dummy-coded and entered into the model as a predictor; *F* values are from Approximate Hotelling-Zhang with small sample correction omnibus tests of the effects of moderators with more than two levels; 95% CI corresponds to the β_1 coefficient for moderators or *d* values for individual levels of moderators; *p* corresponds to the β_1 coefficient or *F* value for moderators, or *t* value for individuals levels of a moderator. The number of effect size estimates and studies often do not add up as expected because some studies provided multiple effect size estimates and/or did not provide data for a level of a moderator.

[a] For cases with too few observations for the RVE approach, we calculated their mean effect size using a traditional random-effects meta-regression model. [b] *F* test is comparing all types of methodologies. *F* test that compares only studies featuring a true control condition yielded the following results, $F(4, 10.4) = .62$, $p = .66$.

chology literature. Below, we outline the two approaches we used to examine publication bias with dependent effect sizes.

Publication bias analyses on aggregated dependent effect sizes. The most common way to assess publication bias with dependent structures is to aggregate the dependent effect sizes and perform standard publication bias tests on the aggregated estimates. To aggregate dependent effect sizes, we used the R package MAd (Del Re & Hoyt, 2010). Using the Borenstein et al. (2009) aggregation method, this function calculates aggregated effect size and effect size variance estimates by taking into account a prespecified correlation among the clusters of dependent effect sizes (set, by default, at $r = .50$[8]).

We then used these aggregated estimates to examine the funnel plot distribution of effect sizes and perform three tests of publication bias: trim-and-fill (Duval & Tweedie, 2000), weight-function modeling (Vevea & Hedges, 1995), and PET-PEESE (Stanley & Doucouliagos, 2014).

PET-PEESE with robust variance estimates. Although researchers typically aggregate dependent effect sizes before examining publication bias, it is worth noting that the PET-PEESE approach can be conducted using RVE. Because PET-PEESE is essentially a meta-regression equation with standard error or variance as a predictor, robust variance estimates can easily be implemented when fitting the meta-regression model. Compared with the aggregation method, the benefit of this approach is that it does not require us to assume a correlation among the clusters of dependent effect sizes. However, a drawback is that the statistical properties of this approach are currently unknown.

Publication bias sensitivity analyses. Heterogeneity, which represents how much variation is observed beyond what would be expected from sampling error alone, can pose problems for many tests of publication bias (Stanley, 2017; Sterne et al., 2011; Terrin, Schmid, Lau, & Olkin, 2003). Therefore, we performed preplanned sensitivity analyses on our publication bias tests by splitting our dataset by significant moderators.

In instances where we did not uncover any evidence of publication bias, we conducted additional preplanned sensitivity analyses by rerunning the analyses: (a) excluding suppression studies, (b) excluding Wagenmakers et al. (2016), and (c) excluding Wagenmakers et al. (2016) and all unpublished data. The purpose of these sensitivity analyses was to ensure that publication bias was not masked by subsets of studies that we might expect to skew the distribution of effect sizes. For example, the emotion regulation literature suggests that suppression is a relatively ineffective way of managing emotional experience (e.g., Gross, 1998). Therefore, it is feasible that publication bias could be masked by the inclusion of relatively small effect sizes from suppression studies. By this same logic, we reasoned that the replication and unpublished studies could have similar effects on our publication bias analyses. These sensitivity analyses never affected our conclusions, but we report them to convey the robustness of the publication bias results.

Results

Overall analyses included 98 articles, 138 studies, and 286 effect sizes (see Table 2). Notably, 20% of these effect sizes came from unpublished sources.

Overall Effect

Using meta-regression with RVE, the overall size of the effect of facial feedback on self-reported affective experience was $d = 0.20$, 95% CI [0.14, 0.26], $t(137) = 6.42$, $p = .000000001$. This indicates that, overall, facial feedback manipulations have a small effect on emotional experience and affective judgments.

Outlier Detection

To examine whether there were any influential outliers, we used the base R function influence.measures. This method detected eight influential outliers,[9] two of which were in the negative direction. Removing the eight outliers did not affect our overall effect size estimate (adjusted $d = 0.19$, 95% CI [0.13, 0.25], $t(137) = 6.31$, $p = .000000004$) or any of the overall publication bias results we report below. Therefore, all effect size estimates were retained in all further analyses.

Moderator Analyses

There was a large amount of heterogeneity in the effect sizes ($T^2 = 0.11$, $I^2 = 75.41$). Such heterogeneity suggests that there may be meaningful differences among studies that can be further explored through moderator analyses. Table 3 contains effect size estimates for each level of each moderator and the accompanying moderator analyses.

Modulation versus initiation of emotional experience. Researchers have long debated whether facial feedback can only modulate emotional experiences produced by emotional stimuli, versus initiate emotional experiences in otherwise nonemotional situations (for reviews see Adelmann & Zajonc, 1989; McIntosh, 1996; Soussignan, 2004). Our results suggested that effect sizes are larger in the absence of emotional stimuli ($d = 0.32$, 95% CI [0.15, 0.49], $p = .0005$) than in the presence of emotional stimuli ($d = 0.13$, 95% CI [0.07, 0.18], $p = .00006$), $\beta_1 = 0.19$, 95% CI [−0.37, −0.01], $p = .04$, suggesting that facial movements have larger initiating than modulating effects.

Discrete versus dimensional levels of emotional experience. Facial feedback researchers have assessed the impact of facial feedback on emotional experience using both discrete emotion measures (Whissell, 1985) and dimensional measures of positivity/negativity (Winton, 1986). Our results uncovered no significant evidence of differences in the magnitude of the effects of facial movements on specific emotions ($d = 0.19$, 95% CI [0.09, 0.29], $p = .0003$) versus general positivity/negativity ($d = 0.14$, 95% CI [0.06, 0.21], $p = .0005$), $\beta_1 = 0.05$, 95% CI [−0.07, 0.18], $p = .42$.

For studies in which discrete emotions were measured, we further assessed whether different emotions yielded different effect

[8] When the correlation among clusters of dependent effect sizes is unknown, it is recommended that meta-analysts assume a correlation and perform additional sensitivity analyses on this assumed value (Borenstein, 2009). In line with this recommendation, we assumed a default correlation of $r = .50$ and performed sensitivity analyses to determine impact of the assumed correlation on our tests of publication bias (testing $r = .10, .30, .50, .70, 90$). We indicate in the manuscript the one instance where this affected our conclusions.

[9] Single influential outliers were detected in Flack, Laird, and Cavallaro (1999a), Kalokerinos et al. (2015), and Kircher et al. (2013). Five influential outliers were detected in McCanne and Anderson (1987).

sizes. We found no evidence that specific discrete emotion was a moderator of facial feedback effects, $F(5, 6.42) = 0.77$, $p = .60$.[10] As shown in Table 3, an examination of the effect sizes for each specific emotion suggested that facial movements had small-to-medium effects on self-reports of happiness ($d = 0.23$, 95% CI [0.08, 0.37], $p = .004$), sadness ($d = 0.30$, 95% CI [0.06, 0.55], $p = .02$), anger ($d = 0.53$, 95% CI [0.19, 0.87], $p = .006$), and disgust ($d = 0.29$, 95% CI [0.03, 0.56], $p = .03$). The effect sizes for fear ($d = 0.13$, 95% CI [−0.05, 0.30], $p = .13$) and surprise ($d = -0.31$, 95% CI [−5.57, 4.95], $p = .59$) did not statistically differ from zero; however, these estimates are based on relatively few effect sizes ($k_{fear} = 15$; $k_{surprise} = 9$).

The effect sizes were small for both positivity ($d = 0.18$, 95% CI [0.07, 0.28], $p = .002$) and negativity ($d = 0.12$, 95% CI [0.01, 0.22], $p = .03$), and the magnitude of these effects did not differ, $\beta_1 = 0.04$, 95% CI [−0.12, 0.19], $p = .64$.

Awareness of facial feedback manipulation. A prominent debate in the facial feedback literature concerns the role of participants' awareness of their posed movements and the emotional concepts typically associated with these movements (Strack et al., 1988). We found no evidence of differences in the magnitude of effects in studies that used procedures that limited participants' awareness of the purpose of the manipulation ($d = 0.13$, 95% CI [−0.05, 0.31], $p = .15$) versus studies that used procedures that did not limit participants' awareness ($d = 0.15$, 95% CI [0.06, 0.24], $p = .001$), $\beta_1 = 0.004$, 95% CI [−0.19, 0.19], $p = .97$.

Awareness of video recording. In reply to Wagenmakers and colleagues' (2016) failed replication attempt, Strack (2016) suggested that one reason the results of the original experiment may not have replicated is that there was a camera directed at participants in the replication study. Across all studies included in our review, there was very little evidence that this methodological difference is associated with different facial feedback effects, $\beta_1 = -0.06$, 95% CI [−0.20, 0.07], $p = .36$. Facial feedback effects were small both when participants were aware ($d = 0.17$, 95% CI [0.06, 0.28], $p = .003$) and unaware of video recording ($d = 0.23$, 95% CI [0.15, 0.32], $p = .0000007$).

Effects on affective judgments versus experience. Although the facial feedback hypothesis is primarily concerned with the effects of facial feedback on emotional experience, many researchers have extended this phenomenon to examine the effects of facial feedback on affective judgments. Subgroup analyses suggested that facial movements have a significant effect on both emotional experience ($d = 0.17$, 95% CI [0.11, 0.23], $p = .0000004$) and affective judgments ($d = 0.38$, 95% CI [0.19, 0.57], $p = .0004$; see Table 3), and a moderator analysis suggested that the facial feedback effects were larger for affective judgments than emotional experience, $\beta_1 = 0.24$, 95% CI [0.04, 0.44], $p = .02$.

Facial feedback manipulation procedure. Determining whether some facial feedback manipulations have stronger effects than others is complicated by the fact that studies vary in the types of comparison groups included in experiments. For example, some studies include comparison groups that receive no facial movement manipulation (e.g., Stel, van den Heuvel, & Smeets, 2008), whereas others include comparison groups that did receive a facial feedback manipulation (R. J. Larsen et al., 1992).

To provide the cleanest test of whether there are differences in effect sizes among facial feedback manipulations, we limited our analyses to studies featuring a comparison group that received no facial feedback manipulation.[11] Effect sizes varied from $d = -0.04$ (exaggeration–control) to $d = 0.71$ (Botox–control), but most manipulation procedures produced small effect sizes (posing–control, $d = 0.30$, 95% CI [−0.16, 0.76], $p = .17$; incidental–control, $d = 0.13$, 95% CI [−0.05, 0.31], $p = .15$; suppression–control, $d = 0.15$, 95% CI [0.04, 0.25], $p = .006$). Nevertheless, we did not find evidence that manipulation procedure was a significant moderator of facial feedback effects, $F(4, 10.41) = 0.62$, $p = .66$ (see Table 3), although small numbers of effects and resulting low power also limit inferences from these results.

Between versus within-subjects design. There were early concerns that facial feedback effects may not emerge in between-subjects comparisons (Buck, 1980). Our results indicated that facial feedback effects emerged both in studies using between-subjects ($d = 0.16$, 95% CI [0.08, 0.24], $p = .0001$) and within-subject designs ($d = 0.25$, 95% CI [0.16, 0.34], $p = .000001$). Although within-subject designs tended to be associated with slightly larger effect sizes, the difference was not significant, $\beta_1 = 0.09$, 95% CI [−0.03, 0.21], $p = .14$.

Type of stimuli. Facial feedback experiments that include the presentation of emotional stimuli have used a variety of different stimuli. We found that there were differences in the magnitude of facial feedback effects based on the type of stimulus used, $F(6, 2.77) = 92.83$, $p = .003$ (see Table 3). Most stimuli produced effect sizes that were small in magnitude (pictures, $d = 0.16$, 95% CI [0.08, 0.23], $p = .0002$; films, $d = 0.13$, 95% CI [0.03, 0.22], $p = .009$; stories, $d = 0.41$, 95% CI [−0.29, 1.10], $p = .25$; social contexts, $d = -0.14$, 95% CI [−0.74, 0.46], $p = .61$), but emotional audio ($d = 0.72$, 95% CI [−0.82, 2.27], $p = .18$) and imagined scenarios produced very large effect sizes ($d = 1.28$, 95% CI [−0.98, 3.53], $p = .27$).

Gender. Given gender differences in other emotion effects (Gross & John, 2003; Kring et al., 1994; LaFrance et al., 2003; McRae et al., 2008; Nolen-Hoeksema & Aldao, 2011) and proposed gender differences in embodied effects (Pennebaker & Roberts, 1992), we tested whether the proportion of women in a sample was related to the magnitude of facial feedback effects. Contrary to the proposition that proprioceptive signals may influence women's emotional experience less so than men's, our results indicated that larger proportions of women tended to have *larger* effect sizes, but that the association was not significant, $\beta_1 = 0.17$, 95% CI [−0.09, 0.42], $p = .21$.

Timing of measurement. There are inconsistencies regarding whether experimenters collect self-reports during ($d = 0.18$, 95% CI [0.09, 0.26], $p = .0001$) or after the facial feedback manipulation ($d = 0.22$, 95% CI [0.12, 0.33], $p = .00008$). Results provided no evidence that this methodological difference influences the

[10] We remind the reader that this F value is based on an Approximate Hotelling-Zhang test with small sample correction. Even though this analysis has 129 effect sizes, the degrees of freedom are low because some of the levels of this moderator had a small number of effect sizes in it. See Tanner-Smith et al. (2016) for more information on degrees of freedom.

[11] Although we believe that comparing cases in which the experiment had a control group that received no facial feedback manipulation provides the clearest test of whether procedure is a significant moderator, we also reran the analyses including studies that did not include a control group. Similar to results reported above, effect sizes tended to be small and type of manipulation was not a significant moderator, $F(9, 14.49) = 1.62$, $p = .20$ (see Table 3).

magnitude of facial feedback effects, $\beta_1 = -0.03$, 95% CI [−0.17, 0.11], $p = .65$.

Publication year. Our results provided marginal evidence that effect sizes in the facial feedback literature tend to become smaller over time of publication (i.e., that effect sizes increase with distance from 2017), $\beta_1 = -0.006$, 95% CI [−0.01, 0.001], $p = .06$. When controlling for publication year, the overall effect of facial feedback is smaller, but still significant, $d = 0.15$, 95% CI [0.06, 0.23], $t(133) = 3423$, $p = .0008$. However, exploratory follow-up analyses suggest that the relationship between publication year and observed effect sizes may be driven by the 17 studies included in Wagenmakers et al.'s (2016) registered replication. When removing these studies, the relationship between publication year and observed effect sizes is smaller, $\beta_1 = -0.002$, 95% CI [−0.008, 0.003], $p = .45$.

Publication status. A common concern in any meta-analysis is that effect sizes in the published literature are larger than those in the unpublished literature. Twenty-six percent of effect size estimates in this meta-analysis came from unpublished sources, but the magnitude of effect sizes was not significantly smaller for unpublished studies ($d = 0.15$, 95% CI [0.04, 0.26], $p = .01$) than it was for published studies ($d = 0.21$, 95% CI [0.14, 0.28], $p = .00000003$), $\beta_1 = -0.05$, 95% CI [−0.18, 0.08], $p = .45$. This analysis cannot rule out the possibility that there is a large unpublished literature that is not represented in the meta-analysis, but it does not support the proposition that uncovering a file-drawer would change the reported overall effect size.

Publication bias. Even though publication status was not a significant moderator of facial feedback effects, we used two methods to assess potential publication bias more directly.

Publication bias analyses with aggregated dependent effect sizes. First, we used aggregated dependent effect sizes to examine the funnel plot distribution of effect sizes and perform three statistical tests of publication bias: trim-and-fill, PET-PEESE, and weight-function modeling.

To visually assess the possibility of publication bias, we first used the aggregated estimates to create a funnel plot of the effect size estimates and standard errors. In the absence of publication bias, this pattern should resemble a funnel, where effect size estimates with smaller standard errors cluster around the mean effect size, and effect size estimates with larger standard errors fan out in both directions. A typical pattern suggestive of publication bias is asymmetry in the bottom of the distribution. As can be seen in Figure 2, there was no pattern in the overall funnel plot of the aggregated effect sizes that was clearly suggestive of publication bias.

To further assess the possibility of publication bias in our overall sample, we conducted three statistical tests of publication bias. First, we used Duval and Tweedie's (2000) trim-and-fill technique. This method trims the values of extreme observations that lead to asymmetry in the funnel plot distribution and imputes values to even out the distribution. This technique was not able to impute any missing studies in our data (i.e., did not detect any publication bias). Second, we created PET-PEESE models (Stanley & Doucouliagos, 2014). PET-PEESE models estimate publication bias by calculating the relationship between effect size and variability and controlling for this relationship in a meta-regression model. Both the PET and PEESE models failed to uncover significant evidence of publication bias, PET $\beta_1 = 0.63$, $p = .16$; PEESE $\beta_1 = 1.59$, $p = .13$.[12] Last, we used Vevea and Hedges' (1995) weight-function modeling. This method creates a meta-analytic model that is adjusted for publication bias and compares its fit to an unadjusted model. If an increase in fit is observed, publication bias is a concern. Results indicated that the model adjusted for publication bias did not increase model fit, which provides no evidence of publication bias, $\chi^2(1) = 0.14$, $p = .71$.

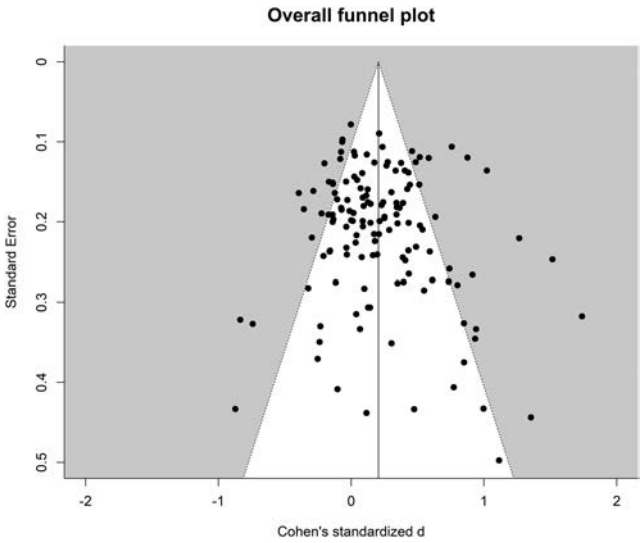


Figure 2. Overall funnel plot for studies examining the impact of facial expressions on emotional experience and affective judgments.

Publication bias analyses with robust variance estimates. Our second approach for examining publication bias was to reexamine PET-PEESE with RVE to adjust for dependency instead of aggregating over dependent effect sizes. Compared with the aggregation method, the benefit of this approach is that it does not require us to assume a correlation among the clusters of dependent effect sizes. Contrary to the results produced by the aggregation method, the results of both the PET and PEESE models with robust variance estimates uncovered significant evidence of publication bias, PETrve $\beta_1 = 1.11$, $p = .02$; PEESErve $\beta_1 = 2.32$, $p = .01$. Furthermore, after controlling for this significant bias, the estimate of the overall effect size did not significantly differ from zero, PETrve $d = -0.03$, $p = .73$; PEESErve $d = 0.08$, $p = .09$.

Summary. Different approaches for assessing publication bias in the facial feedback literature led to different conclusions. When we aggregated the dependent effect sizes, we consistently found no significant evidence of publication bias. However, when we conducted PET-PEESE analyses with RVE, we did find evidence of publication bias. Future research will shed light on which approach is superior. In the meantime, we cannot reject the possibility of publication bias in the overall facial feedback literature.

Publication bias sensitivity analyses. As noted above, there is a large degree of heterogeneity in the overall size of facial feedback effects, $T^2 = 0.11$, $I^2 = 75.6$. This heterogeneity can pose problems for many tests of publication bias (Stanley, 2017;

[12] We preregistered $r = .50$ as our assumed correlation among effect sizes in the aggregation of dependent effect sizes. When we performed sensitivity analyses on this assumed correlation, we did find evidence of publication bias in our PET-PEESE models when $r = .90$.

Sterne et al., 2011; Terrin et al., 2003), and suggests that it may be more fruitful to examine publication bias on individual levels of significant moderators. We found three significant moderators in our meta-analysis: (a) type of affective reaction (emotional experience or affective judgments), (b) whether facial feedback initiates or modulates emotional experience, and (c) the type of stimuli used in the experiment. In line with our preregistration plan, we reran all publication bias analyses on individual levels of these significant moderators. We found no evidence of publication bias when we split our analyses by the initiation versus modulation or stimulus type moderator but did find evidence of publication bias when we split our analyses by type of affective reaction.

Publication bias in studies examining affective judgments. Publication bias sensitivity analyses revealed evidence of publication bias in studies that examined the effects of facial feedback on affective judgments. As shown in the left panel of Figure 3, the funnel plot is largely asymmetrical. The trim-and-fill method imputed five missing observations but suggested that the adjusted overall effect was still significant (adjusted $d = 0.25$, 95% CI [0.06, 0.44], $p = .01$). The PET and PEESE models both suggested that publication bias was present (PET $\beta_1 = 2.65$, $p = .03$; PEESE $\beta_1 = 5.05$, $p = .048$; PETrve $\beta_1 = 2.28$, $p = .01$; PEESErve $\beta_1 = 3.41$, $p = .04$) and that the bias-corrected overall effect is *not* significant (PET $d = -0.22$, $p = .36$; PEESE $d = 0.08$, $p = .52$; PETrve $d = -0.17$, $p = .49$; PEESErve $d = 0.16$, $p = .28$). The weight-function model also provided marginal evidence that publication bias was a concern, $\chi^2(1) = 3.17$, $p = .07$, and suggested that the bias-corrected overall effect is not significant (adjusted $d = 0.18$, $p = .18$). This suggest that, when controlling for publication bias, the cumulative evidence does not support the notion that facial feedback influences affective judgments.

Publication bias in studies examining emotional experience. When we examined the effects of facial feedback on emotional experience, we consistently found no evidence of publication bias. As shown in the right panel of Figure 3, the funnel plot of effect sizes appeared symmetrical. Furthermore, the trim-and-fill method imputed no missing studies, PET-PEESE estimates of publication bias were not significant (PET $\beta_1 = 0.14$, $p = .77$; PEESE $\beta_1 = 0.46$, $p = .69$; PETrve $\beta_1 = 0.70$, $p = .20$; PEESErve $\beta_1 = 1.75$, $p = .13$), and weight-function modeling found that the meta-analytic model that is adjusted for publication bias did not provide better fit than a nonadjusted model, $\chi^2(1) = 1.14$, $p = .29$. Because we did not find evidence of publication bias in studies that examined the effects of facial feedback on emotional experience, we performed additional preplanned sensitivity analyses. More specifically, we reran the publication bias tests (a) excluding suppression studies, (b) excluding Wagenmakers et al. (2016), and (c) excluding Wagenmakers et al. (2016) and all unpublished data. None of these sensitivity analyses suggested the presence of publication bias in studies that examined the effects of facial feedback on emotional experience. This suggests that the cumulative evidence supports the assertion that facial feedback influences emotional experience, the central tenet of the facial feedback hypothesis.

Discussion

Lay people and scientists alike have long wondered whether feedback from our facial movements can influence our experience of emotion. The combined results from nearly 300 effect sizes generated from 138 studies suggest that facial feedback can indeed influence emotional experience, although these effects tend to be small and heterogenous. Importantly, based on the results of a variety of publication bias analyses, the effects of facial feedback on emotional experience (but not affective judgments) do not appear to be driven by publication bias

Addressing Disagreements in the Facial Feedback Hypothesis Literature

The results of this meta-analysis support the general claim that facial feedback influences emotional experience. However, facial

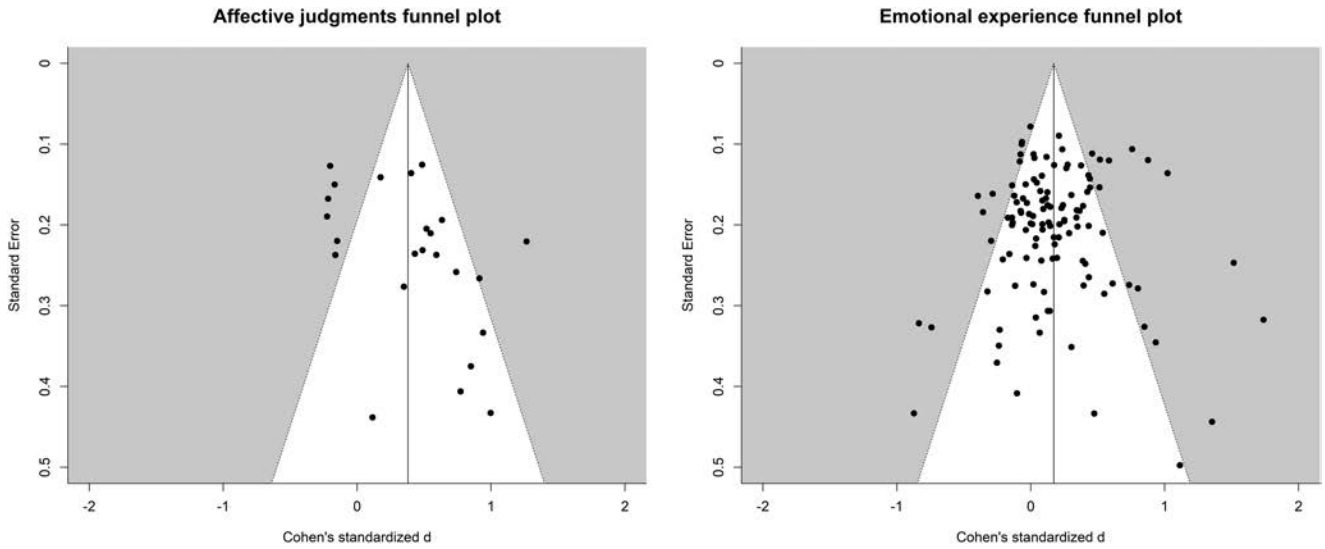


Figure 3. Funnel plots for studies examining the effect of facial feedback on emotional experience and the effect of facial feedback on affective judgments.

feedback theorists have typically disagreed not about whether these effects exist, but rather the specific contexts in which one can expect to observe these effects. Next, we consider the implications of our results for the major theoretical disagreements in the facial feedback literature.

Facial feedback can initiate and modulate emotional experience. When James and Lange proposed that bodily perturbations both initiated and modulated emotional experiences over 100 years ago, they were met with a great deal of incredulity. Although many critics conceded that bodily changes could perhaps modulate emotional experiences, they often rejected the notion that these bodily states were sufficient in creating experiences of emotion (Cannon, 1927; Irons, 1894; Sherrington, 1900; Worcester, 1893). Lange speculated that these initiating effects could actually be demonstrated quite easily. Because Lange believed that emotional experience was built entirely upon sensed changes in the autonomic nervous system, he suggested that any substance that influenced this system (e.g., alcohol) had the potential to initiate an emotional experience, even in otherwise nonemotional situations. James agreed that initiation effects were theoretically possible. However, he did not agree that producing such effects would be easy, contending that it would require a coordinated set of responses across the entire body. Despite their disagreements, one thing that James, Lange, and their critics would have likely agreed upon is the prediction that *facial feedback*, by itself, could not initiate emotional experience. Consequently, our finding that facial feedback can both modulate as well as initiate emotional experiences is quite remarkable.

Although surprising from a historical perspective, most theories in the facial feedback literature are consistent with the observation that facial feedback can initiate emotional experiences (Berkowitz, 1990; Ekman, 1979; Izard, 1977; Laird, 1974; Laird & Bresler, 1992; Tomkins, 1962). For example, Ekman (1979) suggests that each discrete emotion is activated by a biologically innate affect program that produces a set of bodily responses that merge in consciousness to form emotional experience. Although these affect programs are often activated by external stimuli, Levenson and colleagues suggested that they can also be activated by facial movements (Levenson et al., 1990). Nevertheless, although many facial feedback theories are consistent with the observed initiation effects, theorists have typically speculated that such effects would be difficult to obtain. For example, Tomkins (1981) suggested that facial movements can only initiate emotional responses if they match the intensity, duration, and configuration of naturally occurring emotional expressions. These exact specifications are rarely adhered to in experiments on the facial feedback hypothesis (Matsumoto, 1987; Soussignan, 2002). Consequently, our results suggest that initiating facial feedback effects may be easier to obtain than researchers have previously believed.

Although consistent with most facial feedback theories, the observed initiating facial feedback effect is inconsistent with Allport's pioneering theory of facial feedback. Allport (1922, 1924) believed that the autonomic nervous system created undifferentiated feelings of positivity and negativity that were differentiated into discrete emotional categories based on patterns of facial feedback. According to this view, facial feedback cannot initiate emotional experiences in the absence of ongoing feelings of positivity and negativity. Assuming that participants in facial feedback experiments are not incidentally experiencing strong feelings of positivity or negativity, the observed initiating facial feedback effects are inconsistent with Allport's theory.

In addition to contending that facial feedback cannot initiate emotional experiences, Allport suggested that facial feedback could only influence discrete, but not dimensional, levels of emotion. Next, we review results that disconfirm this prediction.

Facial feedback can influence discrete and dimensional reports of emotion. Facial feedback theorists like Allport have tended to emphasize the effects of facial feedback on discrete emotions (Berkowitz, 1990; Izard, 1977; Tomkins, 1962), although later work raised the possibility that facial feedback may also influence dimensional reports of emotion (Zajonc, 1985; Zajonc et al., 1989). Given facial feedback theorists' interest in discrete emotions, it is notable that previous reviews have described these effects as nonexistent (Winton, 1986), preliminary (Adelmann & Zajonc, 1989), mixed (McIntosh, 1996), and controversial (Soussignan, 2004). Our results suggest that facial feedback can influence both discrete and dimensional reports of emotion,[13] and we uncovered little evidence that facial feedback effects are larger for one than the other.

To date, facial feedback theorists have not typically considered whether facial feedback effects might be larger for some discrete emotions than others. However, in a recent narrative review of a theoretical model of surprise, Reisenzein, Horstmann, and Schützwohl (2017) noted that there was mixed evidence for the effects of facial feedback on the experience of surprise. Furthermore, they suggested that if these facial feedback effects do exist, they "cannot play a prominent role for the experience of surprise" (p. 16). Our results indicated that facial feedback effects do not significantly differ based on the type of discrete emotion measured. However, consistent with Reisenzein et al.'s (2017) assertions, we failed to observe significant facial feedback effects in the subset of studies examining surprise. In fact, the overall effect for these studies were in the opposite direction predicted by the facial feedback hypothesis. In addition, we did not observe a significant facial feedback effect in studies that examined feelings of fear. Although these results may suggest that facial feedback does not influence the experience of all discrete emotions, we currently caution against this conclusion; type of emotion was not a significant moderator in this meta-analysis, and there is still only a handful of studies that have examined fear and surprise facial feedback effects.

The role of awareness. Strack et al.'s (1988) pen-in-mouth paper is the most well-known demonstration of the facial feedback hypothesis not just because of the elegance of their manipulation, but also because the work is cited as evidence that the effects of facial feedback on emotional experience are not driven by demand characteristics. In addition to addressing this major methodological concern, their work is often considered to have provided evidence that facial feedback effects can occur outside of people's awareness. However, a large failure-to-replicate has created uncertainty regarding the reliability of the pen-in-mouth effect (Wagenmakers et al., 2016; but see Noah et al., 2018; Strack, 2016). Although a failure-to-replicate 2% of the experimental evidence for the facial

[13] We found evidence that facial feedback influences both discrete and dimensional levels of emotion. However, it is possible that facial feedback only directly influences one of these levels of emotion and these effects indirectly influence reports of the other level. For example, perhaps smiling makes people feel more happy but not more positive, but people report higher levels of positivity because they are experiencing higher levels of happiness. Nevertheless, such a speculation seems difficult to experimentally confirm.

feedback hypothesis does not invalidate the overall claim that facial movements influence emotional experience, it has revived concerns that these effects are driven by demand characteristics and reopened the discussion about the mechanism that underlies these effects.

The cumulative evidence suggests that studies that use procedures that limit participants' awareness of the purpose facial feedback manipulation produce similar effect sizes as studies that do not. Notably, these analyses included all types of incidental facial movement manipulations (i.e., were not limited to the pen-in-mouth manipulation). These results suggest that the effects of facial feedback on emotional experience are not necessarily driven by demand characteristics, although this does not preclude the possibility that they sometimes are (e.g., when experimenters do not effectively mask the purpose of their experiment). These results would seem to be inconsistent with theories that predict that such effects are mediated by self-perception mechanisms (Laird, 1984; Laird & Bresler, 1992). However, Laird later argued that the self-perception process did not necessarily require awareness of the facial movements (e.g., moving the corners of one's mouth into a smile) or the purpose of these movements (e.g., to smile; Laird & Bresler, 1992). Consequently, although our results fail to confirm that awareness of the purpose of the facial feedback experiment is necessary for facial feedback effects to emerge, it does not necessarily disconfirm Laird's self-perception theory of emotion.

Do facial movements influence affective judgments? The central tenet of the facial feedback hypothesis is that facial feedback influences emotional experience. However, many researchers in the facial feedback literature have expanded upon this original scope by suggesting that facial feedback can also influence affective judgments (Davis et al., 2015; Dzokoto et al., 2014; Ohira & Kurono, 1993), a term we have used to broadly refer to judgments about the emotional characteristics of a stimulus. Results initially indicated that facial feedback does influence affective judgments and that facial feedback effects are larger for affective judgments than emotional experience. However, we subsequently uncovered consistent evidence of publication bias in this subset of studies. Depending on the method for generating bias-corrected overall effect size estimates, the adjusted overall effect size was either close to zero or in the opposite direction. Regardless, the bias-corrected overall effect size estimates did not significantly differ from zero.

Although the current balance of evidence does not support the assertion that facial feedback influences affective judgments, we strongly caution against prematurely abandoning research on these effects. Researchers who have examined the effects of emotional states on subsequent judgments have often emphasized that such effects do not emerge in all contexts (Clore, Schiller, & Shaked, 2018; Schwarz & Clore, 2007). For example, Schwarz and Clore (2007) suggest that feelings only influence judgments when they seem relevant to the task at hand. Based on this view, facial feedback will only influence affective judgments when the elicited emotional experiences are perceived to be relevant to the target being evaluated. Interestingly, these context-dependent effects are also predicted by Laird's self-perception theory of emotion (Laird, 1984; Laird & Bresler, 1992), but this prediction has gotten little attention in the facial feedback literature.

Although emotional experience and affective judgments are considered distinct in the emotion literature, a clear operational distinction between the two remains elusive. Consider theoretical debates about whether people can experience simultaneously mixed emotions of happiness and sadness (J. T. Larsen, McGraw, & Cacioppo, 2001; Russell & Carroll, 1999). Russell (2017) has pointed out that all researchers who have ostensibly observed mixed emotions (for a meta-analytic review, see Berrios, Totterdell, & Kellett, 2015) might have inadvertently measured affective judgments rather than emotional experience (see also J. T. Larsen, 2017). In the facial feedback literature, Strack et al. (1988) measured affective judgments by asking participants "How funny do you think these cartoons are?" This dependent measure can be considered an affective judgment because it is a question about the stimuli, not felt experience. However, it is plausible that many participants interpreted it as a question about their experience of amusement. Future research can more clearly assess the relationship between facial movements and affective judgments by using measures that more clearly isolate affective judgments from emotional experience (e.g., Hunter, Schellenberg, & Schimmack, 2010; Itkes, Kimchi, Haj-Ali, Shapiro, & Kron, 2017). In any event, our observation that facial feedback effects can occur in otherwise nonemotional situations suggests that effects of facial feedback on emotional experience need not be mediated by affective judgments. In summary, the current balance of evidence does not support the assertion that facial feedback influences affective judgments, but we caution against abandoning this line of research.

Implications for Other Emotion Theories

The primary goal of this meta-analysis was to address disagreements among emotion theorists who have made explicit predictions about the impact of facial feedback on emotional experience. However, most of these theories fall into two categories: (a) basic emotion theories, which postulate the existence of a finite set of biological affect programs that elicit coordinated sets of emotion-specific responses (Allport, 1922; Ekman, 1979; Izard, 1971; Tomkins, 1962), or (b) network theories of emotion, which postulate association-based cognitive organizations of emotion concepts (Berkowitz, 1990; Bower, 1981). Interestingly, facial feedback effects are less frequently discussed in the context of contemporary appraisal and constructionist theories of emotion despite the fact that these effects are not generally inconsistent with these theories. Next, we will briefly consider our results in the context of appraisal and constructionist emotion theories, focusing on broad implications as opposed to nuanced distinctions among theories within each tradition.

Appraisal theories of emotion. A fundamental assumption of appraisal theories of emotion is that automatic or controlled cognitive appraisals are the antecedents of emotional reactions (Moors, Ellsworth, Scherer, & Frijda, 2013; Roseman & Smith, 2001). According to these views, cognitive appraisals produce a set of action tendencies, physiological responses, and motor behaviors, all of which contribute to the experience of emotion. To the degree that the effects of appraisals on emotional experience are mediated by motor behaviors (Scherer, 2009), appraisal theories would expect facial feedback to influence emotional experience. However, given that appraisal theories argue that cognitive appraisals are the antecedents of emotional reactions, these theories have more difficulty reconciling their views with the obser-

vation that facial movements can initiate emotional experiences in the absence of emotional stimuli.

From one perspective, facial feedback effects might simply represent exceptions to a rule that do not characterize typical emotional experiences (Ellsworth & Scherer, 2003; Roseman & Smith, 2001). On the other hand, Berkowitz and Harmon-Jones (2004) have argued that "a truly comprehensive theory of affective states should attempt to deal with relatively unusual occurrences as well as the more common ones" (p. 125). To that end, appraisal theorists Smith and Kirby (2004) have suggested that facial feedback can initiate an emotional experience if it activates the emotion's corresponding appraisal pattern via associative processing. Two of our findings suggest otherwise. First, it only makes sense to suggest that facial feedback has initiated an emotional reaction if no emotional stimulus is present. In these scenarios, there is not much to engage the appraisal process. Second, our results thus far have failed to provide evidence that facial feedback influences affective judgments, which are conceptually distinct but similar to cognitive appraisals. Nevertheless, a more direct test of this assertion would ultimately be more informative.

Psychological constructionist theories of emotion. Modern psychological constructionist theories of emotion postulate that the experience of discrete emotions represent the outcome of a mental categorization process (Barrett, Wilson-Mendenhall, & Barsalou, 2014; Lindquist, 2013; Russell, 2014). Central to these models is the concept of *core affect*, which represents "the most elementary consciously accessible affective feelings" that people can experience (Russell & Barrett, 1999, p. 806). Core affect is thought to vary along a bipolar valence dimension and a unipolar activation or arousal dimension ranging from states of low to high arousal. According to these models, core affect is ever-present (at least when we are awake or dreaming) but emotions only occur occasionally. Specifically, people experience what we typically refer to "emotions" when they categorize their core affect into a discrete emotional category (e.g., anger, fear) based on physiological states, conceptual knowledge about emotions, and situational cues. For example, the discrete emotion that people will experience in a high-arousal unpleasant state will depend on whether the situational cues more closely resemble their prototype of fear, anger, or some other emotion.

Constructionist theories of emotion are often contrasted with the basic emotion theoretical tradition, which views emotional experience as a byproduct of a coordinated set of responses elicited by the activation of biologically hardwired affect programs. Although the facial feedback hypothesis has traditionally been most closely associated with basic emotion theories, modern psychological constructionist theories of emotion provide a framework for exploring two different facial feedback effects: (a) the effects of facial feedback on core affect, and (b) the effects of facial feedback on the mental categorization of core affect.

Core affect has been described as a "neurophysiological barometer of the individual's relation to an environment at a given point in time" (Barrett, 2006, p. 31; Barrett & Bliss-Moreau, 2009; Duncan & Barrett, 2007). Researchers have tended to focus on the effects of interoceptive feedback on core affect (MacCormack & Lindquist, 2017, 2019), but have also noted that proprioceptive feedback can influence core affect (Barrett & Bliss-Moreau, 2009; Lindquist, 2013). Our observation that facial feedback influences dimensional reports of emotion suggests that facial feedback may be one of type of proprioceptive feedback that contributes to core affect.

From a constructionist perspective, a second possibility is that facial feedback can influence whether and how core affect is categorized into discrete emotions. For instance, people who are in unpleasant but otherwise ambiguous situations may be more likely to categorize their unpleasant core affect as anger if they have been induced to scowl as opposed to frown. This idea echoes Allport's (1922, 1924) contention that facial feedback guides the categorization of underlying valanced feelings. However, whereas Allport suggested that the patterns of facial movements that guide the categorization process are biologically innate, psychological constructionist theories would argue that these effects are driven by learned associations between patterns of facial movements and emotional concepts. In other words, constructionist theories of emotion would predict that the effects of smiling on the categorization of positive affect as happiness, for example, may be mediated by the extent to which an individual believes smiling is a symptom of happiness. It is worth noting that even though Allport proposed that facial feedback can influence emotion categorization nearly a century ago, this hypothesis remains untested (McIntosh, 1996).

Other Potential Sources of Heterogeneity

In addition to examining moderators that provided insight into theoretical disagreements in the emotion literature, this meta-analysis examined several other methodological moderators proposed by previous facial feedback researchers, including whether effect sizes came from between- or within-subject comparisons (Buck, 1980), the procedure used to manipulate facial poses (Izard, 1990a), gender (Pennebaker & Roberts, 1992), and whether participants were aware of video recording (Strack, 2016). In addition, we tested methodological moderators we thought might influence facial feedback effects, such as the timing of self-reported affective experience. We did not uncover significant evidence that these factors were associated with differences in the magnitude of facial feedback effects. We did, however, find evidence that facial feedback effects were larger in the presence of some types of stimuli (e.g., emotional sentences) than others (e.g., pictures; see Table 3). Nevertheless, there are large amounts of heterogeneity within different stimulus types, suggesting that even within a group of studies using similar types of stimuli (e.g., pictures), other methodological choices (e.g., different pictures; different presentation modes) may affect the magnitude of facial feedback effects.

Although we examined moderators that figured prominently in the facial feedback literature, given the large degree of heterogeneity in facial feedback effects, we believe that there are potential moderators that we did not evaluate. For example, Laird and colleagues argued that individual differences in the degree to which individuals attend to their bodily cues—including but not limited to proprioceptive cues from the face—is a key moderator of facial feedback effects (Laird & Bresler, 1992; Laird & Crosby, 1974; Laird & Lacasse, 2014). Unfortunately, we were not able to assess this moderator because we cannot assess how different experimental procedures influenced the degree to which participants attended to their bodily cues. Furthermore, Laird and colleagues' own work sheds little light on this question because they

often used circular reasoning, classifying only participants who demonstrated larger facial feedback effects as individuals who attend more to bodily cues.

Future research can investigate the role of individual differences in proprioceptive awareness using both self-reports and behavioral measures. For example, Mehling and colleagues (2012) have developed a self-report measure that assesses individual differences in the degree to which people believe they attend to interoceptive cues and use these cues to make sense of their emotions. Scales like these could potentially be adapted for research on proprioceptive awareness. In addition, researchers can use behavioral measures of proprioceptive awareness of facial expressions (e.g., Leplow, Schlüter, & Ferstl, 1992). Furthermore, it is likely that methods for measuring proprioceptive awareness of other bodily regions can be adapted to the study of facial feedback (for a review, see Hillier, Immink, & Thewlis, 2015).

Exclusion criteria may be an important source of heterogeneity in facial feedback research because researchers use varying sets of exclusion criteria to sometimes exclude large proportions of participants. Approximately half of the studies in our review did not report any exclusion criteria, and those that did used a variety of criteria. For example, researchers sometimes excluded participants who were aware of the purpose of the experiment (e.g., Baumeister, Papa, & Foroni, 2016; Duncan & Laird, 1977; Laird, 1974), failed an attention-check (e.g., Kalokerinos, Greenaway, & Denson, 2015), experienced equipment errors (e.g., Pedder et al., 2016), produced unreadable or missing data (e.g., Dzokoto et al., 2014; Zajonc et al., 1989), or were outliers (e.g., Korb, Grandjean, Samson, Delplanque, & Scherer, 2012; Marmolejo-Ramos & Dunn, 2013; Zhu, Cai, Sun, & Yang-yang, 2015). Exclusion criteria choice might be especially important in the facial feedback literature given the large proportions of participants that are sometimes excluded. For example, Soussignan (2002) excluded approximately 30% of participants because they did not contract the correct facial muscles. Wagenmakers et al. (2016) used a combination of several exclusion criteria and, on average, excluded 25% of their participants. These various exclusion criteria have the potential to both deflate effect sizes (e.g., excluding participants who exhibit demand characteristics would presumably lower the effect size) and inflate effect sizes (e.g., excluding participants who failed to smile would presumably increase the effect size), which further contributes to heterogeneity in the facial feedback literature.

Limitations of the Meta-Analytic Approach

This meta-analysis provides the most comprehensive integrative review of the facial feedback hypothesis to date. However, it would be a mistake to interpret the comprehensive nature of this work as providing authoritative conclusions about facial feedback effects. Although meta-analysis is a valuable tool, it possesses a variety of limitations. Next, we will discuss some of the most pressing limitations of this meta-analytic work.

Meta-analytic conclusions can be compromised by the presence of questionable research practices (QRPs). To date, meta-analysts have been primarily interested in the effects of publication bias, and researchers have subsequently developed several tests of the extent and impact of this bias. However, methods for detecting publication bias are imperfect. Publication bias detection methods have suboptimal statistical properties in a variety of scenarios (Carter, Schönbrodt, Hilgard, & Gervais, 2017; Macaskill, Walter, & Irwig, 2001; Stanley, 2017) and were developed and tested under the assumption that the underlying effect sizes are independent. More than half (53%) of our studies provided multiple effect sizes, and different approaches for dealing with such dependencies led to slightly different conclusions regarding publication bias in the overall facial feedback literature. Fortunately, more clear patterns emerged in our sensitivity analyses, where all approaches produced evidence of publication bias in studies examining affective judgments and a lack of evidence of publication bias in studies examining emotional experience. Nevertheless, we believe future research should continue to develop and validate methods for detecting publication bias and evaluate the effectiveness of these approaches when dependent data structures exist.

Other QRPs, such as optimal stopping, *p*-hacking, and infrequent cases of outright fraud, also threaten the validity of meta-analytic conclusions. John, Loewenstein, and Prelec (2012) found that a high proportion of psychology researchers admitted to performing these practices, including deciding whether to exclude data after looking at the impact of doing so on the results (43%), deciding whether to continue data collection after looking to see whether the results were significant (58%), and stopping data collection early once significant results have been found (23%). These practices inflate meta-analytic estimates, which can create misleading conclusions (Bierman, Spottiswoode, & Bijl, 2016; Head, Holman, Lanfear, Kahn, & Jennions, 2015). Although some newer methods for detecting bias—such as *p*-curve (Simonsohn, Nelson, & Simmons, 2014) and the incredibility index (Schimmack, 2012)—may help identify the existence of other QRPs, these methods also currently assume that effect sizes are independent. Therefore, it is currently unclear to what degree QRPs may have inflated the effect sizes we observed in this meta-analysis.

Last, despite the large size of the facial feedback literature, it is likely that many of our moderator analyses lack adequate statistical power. Moderator analyses typically need a large amount of observations to achieve high power (Hedges & Pigott, 2004), and the power to detect moderators is reduced by higher levels of heterogeneity and robust variance estimation procedures. Given the high level of heterogeneity in our meta-analysis, it is quite possible that future researchers can devise more powerful tests of moderation by manipulating a moderating factor in an experiment. Consequently, null effects in our tests of moderation should be cautiously interpreted, and future research should continue to consider the impact of these potential moderators.

Conclusion

When Thích Nhất Hạnh stated that "sometimes your smile can be the source of your joy," he may not have been aware that what he apparently took to be a settled fact had a long, contentious history in psychological science. Indeed, it has been more than 30 years since the "facial feedback hypothes*is*" fragmented into a variety of "facial feedback hypothes*es*" (Adelmann & Zajonc, 1989; McIntosh, 1996; Tourangeau & Ellsworth, 1979). In retrospect, such fragmentation helped clarify unresolved theoretical disagreements and facilitated more nuanced discussions about when and why facial feedback effects emerge. Subsequent primary research studies have gone only some way toward resolving these

disagreements, in part because of discrepant findings (e.g., Strack et al., 1988; Wagenmakers et al., 2016). We believe our meta-analysis has resolved many of these theoretical disagreements. Based on a review of more than 100 years of research, 138 studies, and 286 effect sizes, our understanding of the effects of facial feedback on emotional experience is becoming more clear. The cumulative evidence, to date, suggests that facial feedback does indeed influence emotional experience. Facial feedback appears to influence undifferentiated feelings of positivity, negativity, and a variety of discrete emotions (e.g., happiness, anger, disgust). However, so far the evidence does not suggest that facial feedback influences all emotions (e.g., fear and surprise). Interestingly, it appears that facial feedback effects are largest in otherwise non-emotional situations, which not only indicates that facial feedback is sufficient for the experience of emotions but also suggests that this may be the most powerful context to examine these effects.

The nature of scientific inference prevents us from concluding that "your smile can be the source of your joy" with anywhere near the confidence that Thích Nhất Hạnh could. Besides, Thích Nhất Hạnh's concept of *joy* is probably a rare commodity in most psychology laboratories. Nonetheless, a half century's worth of experimental findings does provide considerable evidence that smiles, frowns, scowls, and other facial movements can affect emotional experience in a variety of scenarios. At the same time, our meta-analysis indicates that the effects are quite small and appear to vary for reasons that our meta-analysis did not shed light on. Having demonstrated that facial feedback effects can occur, we hope that future research sheds further light on why they do.

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