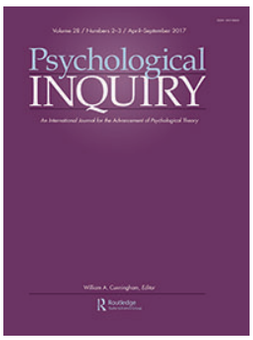

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REPLY

Twelve Questions for the Theory of Affective Pragmatics

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I want to sincerely thank my commentators for their critical analyses of my work. I have learned a lot from their commentaries, both about the virtues and about the flaws of my *theory of affective pragmatics* (TAP). As a result, I am clearer now on what needs to be done to further articulate, clarify, and improve TAP. Thank you! I will discuss some of the overarching critical themes that have emerged in the commentaries, and in the process put some additional flesh around the skeleton of TAP. I organize my reply around a number of central questions that have attracted the attention of commentators, discussed in what I take to be a natural progression.

In What Sense Is TAP a Pragmatic Theory?

Morris (1938, pp. 6-7) defined *pragmatics* as the study of the "relation of signs to interpreters," distinguishing it from *syntax* (the study of the relations among sign vehicles) and *semantics* (the study of the relations between sign vehicles and their semiotic referents). This initial characterization carries the useful suggestion that pragmatics focuses on sign usage in the context of communicative behavior, rather than on signs in relation to other signs or to the world, but it does not go far enough in distinguishing contemporary pragmatic approaches to communication.

As Moore points out in his commentary, two more restrictive notions of pragmatics have been influential in linguistics and animal communication theory alike. On the *Gricean conception* of pragmatics, linguistic meaning depends on communicative intentions. We owe it to Grice (1957) to have made it clear that linguistic meaning does not emerge from a process of mere coding and decoding (the code model), but rather from an inferential process the aim of which is to reconstruct the reflexive intentions with which linguistic utterances are produced (the ostensive-inferential model). A reflexive intention is an intention to produce a response in an interlocutor at least in part by virtue of such intention being recognized, and it is a key requirement for Gricean communication (a.k.a. overt communication).

On what Moore calls the *Carnapian conception* of pragmatics, on the other hand, linguistic meaning depends on the context. There is no implication here that the context serves to infer reflexive intentions (although this option remains open): All that is claimed by this variety of pragmatics is that what a sentence means cannot be established independently of its context of production.

Both varieties of pragmatics can be applied to nonlinguistic items, including emotional expressions, gestures, and other possible bearers of meaning. As Moore correctly diagnoses, TAP is pragmatic in the Carnapian sense only: It aims to capture how the meaning of emotional expressions depends on their context of production, not how the meaning of emotional expressions depends on the reflexive intentions of their producers.

This is why TAP goes to great pains to emphasize that emotional expressions have *natural meaning*, a type of meaning that relies on statistical correlations, rather than *non-natural meaning*, a type of meaning that relies on the reflexive intentions of signalers.

Why does TAP do that? The problem with a Gricean pragmatics-first approach to language evolution is that in many of its formulations it runs into a circularity problem. We are set on a wild goose chase for precursors of the non-naturalness of linguistic meaning in nonlinguistic creatures when it is far from clear that the reflexive intentions presupposed by non-natural meaning are available in a prelinguistic world.

So TAP takes a different path: It starts from an ability that nonlinguistic creatures clearly have, namely, the ability to pick up natural meaning or natural information (I use these two terms interchangeably) and explores how far such ability can take us in spawning precursors of design features of language *other than* non-natural meaning. Moore highlights a theoretical cost involved in this strategy, namely, that the emergence of reflexive intentions is not accounted for.

This is a significant cost, but one I am fully prepared to pay: TAP is programmatically *not* a pragmatic theory in the Gricean sense. My hope is precisely the one Moore voices in the course of his commentary, that TAP can be paired in a cooperative venture for mutual advantage with theories specifically designed to explain how Gricean or overt communication came about.

Moore's own theory is a promising candidate in this regard. Unlike other proponents of a Gricean approach, Moore is clear-eyed on the need to avoid making cognitive demands on the path toward Gricean communication that only language users can satisfy. He is ultimately convinced that Gricean communication can get off the ground *without* the complex cognitive abilities many others—me included—have assumed it requires. Specifically, Moore proposes that there exist forms of "minimally Gricean" communication available to creatures who, despite lacking language, can act with, and attribute to each other, at least some simple reflexive intentions.

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I do hope that Moore's project succeeds. In combination with non-Gricean theories that trace back the origins of linguistic meaning to emotional expressions (e.g., TAP; Bar-On, 2013), Gricean theories can provide us with valuable tools for making progress on the mystery of language evolution. But, contra Moore, I remain convinced that TAP can make a contribution on its own prior to being paired with complementary accounts of how reflexive intentions evolved. This is because Gricean interpretation is not the *only* design feature of language worth finding precursors for, although it may well be an especially significant one.

TAP aims to find precursors for a different design feature of language, namely, the production of illocutionary acts. Elaborating on Austin's previous work, Searle (1969) proposed that such illocutionary acts belong to five and only five categories: asserting what is the case (Assertives), directing others to behave in certain ways (Directives), committing to certain courses of actions (Commissives), expressing what is inside (Expressives), and bringing about states of affairs by saying so (Proclamatives).

The core thesis of my target article is that emotional expressions allow for analogs of the first four types of illocutionary acts, which I labeled Declaratives$_{EE}$[1] (analogs of Assertives), Imperatives$_{EE}$ (analogs of Directives), Commissives$_{EE}$ (analogs of Commissives), and Expressives$_{EE}$ (analogs of Expressives).[2] Crucially, emotional expressions make these Speech Act Analogs possible merely by virtue of the natural information they carry and without involving reflexive intentions and Gricean interpretation at all.

A convincing story will have to be told about how we transitioned from the nonlinguistic communicative moves I characterized to their linguistic counterparts. But if TAP is right on the basics (a topic up for debate!), the broad *storyline* is set. The essential plot is this: Nonlinguistic creatures first developed the ability to represent what is the case, direct others to behave in certain ways, commit to certain courses of actions, and express what is inside without words, but could engage in these communicative moves only with respect to highly restricted propositional contents.

Eventually, positive selection pressures emerged to perform these basic communicative moves with respect to *new* propositional contents—to represent, direct, commit, and express *new things*. Over evolutionary time, and with the emergence of Gricean interpretation, this led to the birth of language, which replaces natural with non-natural meaning and thereby allows communicative moves to be performed with respect to unbound propositional contents.

To sum up, TAP's proposal is that natural meaning and Carnapian pragmatics come first, and non-natural meaning and Gricean pragmatics come later in the order of explanation.

How Does TAP Differ from Other Pragmatic Approaches?

In his zeal to show that I failed to properly cite my sources and that this is somehow a fatal flaw for my theory—"the most glaring reason why his TAP is ill-conceived … is his failure to acknowledge, let alone legitimize, TAP's older siblings" (p. 204 a bad criterion for theory evaluation to begin with)—Fridlund misses the distinction between context-based and intention-based varieties of pragmatics entirely.

Fridlund's (this issue) summary of TAP is as follows: "Language is not only propositional but also pragmatic, and gosh darn, maybe some nonverbal expressions work likewise. The same finger that can indicate the number 1 can also tell someone what to do with himself" (p. 197). Well, not quite. My point is not the generic one that there are unspecified pragmatic dimensions to nonverbal expressions (a trivial point indeed) but that nonverbal expressions work like language in an essential and surprising capacity: They make available a very similar set of communicative moves despite the fact that they rely on a different type of meaning (natural meaning vs. non-natural meaning).

One can certainly remain unpersuaded of this point, but one should at least demonstrate a modicum of good will in trying to understand it. TAP has nothing to do with the fact that the same finger that can indicate the number 1 can also tell someone what to do with himself. These are both examples of *non-natural meaning*, the sort of meaning linguistic and nonlinguistic conventions make possible.

What I am proposing is rather different: What nonverbal expressions naturally mean without relying on *any* conventions is analogous to what speech acts non-naturally mean by relying on conventions. For example, just as a conventional nonverbal expression like a raised middle finger can non-naturally direct someone to get lost, so a nonconventional nonverbal expression like the involuntary baring of one's teeth out of anger can naturally direct someone to get lost.

This may well be wrong, but trivial it is not, because it suggests that the complex network of reflexive intentions underlying Gricean communication is not strictly necessary for making some of the communicative moves that language makes possible. And if such network is not required, this makes it potentially easier to reconstruct the evolutionary path that has led from nonconventional communicative moves to conventional illocutionary acts.

Because Fridlund appears to think that the pragmatic approach is a well-defined and unified approach, he proceeds to scold me for not having sufficiently credited my pragmatist predecessors, as if any two theories that discuss nonverbal behavior and call themselves pragmatist were interestingly alike. He more ominously suggests that my failure to compare and contrast TAP with other pragmatic theories indicates mischievous intent to inflate my own originality. A more charitable

[1] Searle originally used the label *Declarations* to designate what I call *Proclamatives*. As I explained in my target article, I have repurposed this term to designate the category of nonlinguistic Declaratives$_{EE}$, the communicative point of which is to describe what the world is like. The similarity between the terms Declaration and Declaratives$_{EE}$ has understandably led to confusion in some commentators. For example, Fischer argues that she does not see what analogy there could possibly be between my Declaratives$_{EE}$ and Searle's Declarations, the communicative point of which is to bring about states of affairs just by saying so, as we do when we baptize a child, appoint someone as chairman, or fire an employee. The answer is that there is no analogy whatsoever. My Declaratives$_{EE}$ are intended to be analogous with Searle's Assertives, *not* with Searle's Declarations, which I referred to as Proclamatives in my article. It may turn out that my choice of label for nonlinguistic Declaratives$_{EE}$ is unwise because it is unnecessarily confusing. In previous drafts I called them Representatives$_{EE}$, which may ultimately work better.

[2] EE stands for "emotional expression."

interpretation is readily available: Target articles have limited length, and I was busy introducing TAP for the first time while discussing its predecessors in the scientific literature on emotional expressions, rather than within pragmatics more generally.

It is especially puzzling that Fridlund's impassioned crusade on behalf of neglected predecessors would fail to acknowledge my most important predecessor: Fridlund himself. A reader of his commentary who is unfamiliar with my target article would surely conclude that our theories have very little in common. What a mistake that would be! As I tried to emphasize throughout my article, TAP aims to develop Fridlund's important insights on nonverbal displays as social tools while freeing his theory from the shackles of a poorly motivated opposition to the concepts of emotion and emotional expression (more on this in a later section).

Baseless or not, Fridlund charge of not having paid enough homage to some of the theorists who preceded me gives me an opportunity to distinguish my theory from other pragmatist theories. One of the elder siblings I don't give enough credit to is certainly Tim Wharton, whose influential 2009 book *Pragmatics and Nonverbal Communication* I regretfully did not include in my reference list in the copy circulated to commentators (had Fridlund not singled Wharton out as Exhibit A for my failure to honor sources, I would have had a chance to include him at the proof stage).

This being said, there are important differences between Wharton's approach and mine. Wharton is engaged in a project of Gricean pragmatics, I am engaged in a project of Carnapian pragmatics. His primary interest lies in understanding how "the tone of voice we use, the facial expressions and bodily gestures we adopt while we are talking" affect the non-natural meaning of our words. Wharton's pragmatic framework is *relevance theory* (Sperber & Wilson, 1986), a theory of linguistic interpretation that "combines Gricean intention-based pragmatics with aspects of modern research in cognitive science to provide a cognitive-inferential pragmatic framework" (Wharton, 2009, p. 8).

Relevance theory holds that hearers infer the communicative intentions of speakers on the basis of a *principle of relevance*, according to which what speakers non-naturally mean is what maximizes the relevance of what they say. The question of central interest to Wharton is, how do the "natural non-verbal properties of utterances interact with [the] linguistic ones" in determining non-natural meaning?

To answer this question, Wharton explores the natural information carried by nonverbal expressions, and he does so in a way that has various points of contact with my analysis, but he nowhere argues that such natural information allows nonverbal expressions to perform analogs of illocutionary acts, which is the punch line of my article. His focus is on how natural and non-natural information interact in shaping what maximizes relevance and so in determining the inferences hearers make about the communicative intentions of speakers.

José-Miguel Fernández-Dols, whom I repeatedly cite in my target article and from whom I have learned much over the years, is another proponent of a pragmatic approach to facial expressions I allegedly have not credited enough, but the differences between my project and his are major and could not be dealt with properly in my target article due to space constraints. Fernández-Dols engages in a project of Carnapian pragmatics as I do, in the sense that he is interested in understanding how context resolves the ambiguity of what he calls "natural facial expressions."

Fernández-Dols and I agree that the caricatured facial expressions of emotions used in Ekman's snapshot method rarely occur in the wild. The "natural facial expressions" we observe in real-life circumstances manifest a great deal of variability, of a magnitude that goes well beyond what display rules can account for. It follows that we cannot associate to any face an encoded meaning that is preserved in all contexts. Fernández-Dols (2017) describes the view the faces encode fixed meanings as the "classical semantic view of communication," a view commonly referred to in linguistics, as I mentioned earlier, as the "code model" of communication (p. 461).

What Fernández-Dols proposes instead is a pragmatic approach, inspired by the "ostensive-inferential" model of linguistic communication, according to which facial movements are, jointly with their context of production, the evidence on the basis of which recipients draw inferences. This is where the similarities between our theories end. TAP goes on to propose that facial movements have context-dependent natural meanings, and that one of the things they naturally mean in some contexts (but not in others) is that the agent is having a certain emotion. Fernández-Dols's view is instead that natural facial expressions (a) "do not have a specific meaning, *but they hint at potential outcomes of an interaction for either the sender or the receiver*" (emphasis in original) (p. 465), from which he takes to follow that (b) studying facial expressions as if they "were *expressions of basic emotion* does not make sense" (emphasis in original) (pp. 465–466).

Neither (a) nor (b) follow from the context dependence of meaning. Facial expressions can have a specific natural meaning once we have fixed the context, just like sentences can have a specific non-natural meaning once we have fixed the context,[3] and it makes perfect sense to assume that in some circumstances nonverbal displays express basic emotions. Fernández-Dols has inferred from the true claim that natural facial expressions do not express the *same* basic emotion in all contexts according to a fixed code the false claim that natural facial expressions *never* express *any* basic emotion in a given context in light of the evidence they provide in that context.

This is not a good inference. We can acknowledge that there is no sadness encoded in crying, that sad people often express their sadness without crying, and that non-sad people often express their emotions by crying (all points on which Fernández-Dols, Fridlund, and I agree) while holding on to the claim that crying expresses different emotions in different contexts.

Fischer and Sauter offer some examples of the many context-dependent meanings of crying in their commentary. They point out that crying can express "relief" in a context of

[3] A different question is whether the meaning of a sentence determines its truth value. My view is that it does (once we have removed ambiguities and made vague expressions precise), whereas Fernández-Dols, with inspiration from Austin, seems open to the possibility that "[a] spoken statement, even the simplest one, has an undetermined truth value" (p. 465). The discussion of this additional disagreement would take me too far afield.

"distress," sadness upon the "loss of one's dearest friend," "exuberance" because "one's soccer team has finally won," and "being moved" upon observing "the first performance of one's child." So I fundamentally disagree with Fernández-Dols (and Fridlund) that a pragmatic approach justifies the rejection of the idea that natural facial expressions express emotions in some circumstances. What a pragmatic approach warrants is the conclusion that facial expressions do not express anything in a context-independent fashion, either because they have different meanings in different contexts or because they are meaningless in isolation from the context.

I am thankful to Fridlund for bringing Levinson's work to my attention. He was not on my radar when I wrote the article. I have now read the two pieces Fridlund cites, and I find Levinson's work highly stimulating. But there is no reason for TAP to "pay homage" to Levinson's work, because such work does not overlap in any significant way with TAP. Levinson's two cited works focus on conversation analysis and turn taking (Levinson, 2013) and on the idea that human interaction is governed by a core interaction engine, which prominently involves mind-reading, mutual salience, the attribution of Gricean intentions, cooperation, and other features (Levinson, 2006). TAP has nothing to say on either topic, so no umbrage on Levinson's behalf should be taken.

As to Fridlund's suggestion to focus on work in gestural pragmatics by Ray Birdwhistell, Adam Kendon, David McNeill, and Susan Goldin-Meadow, I welcome it, and will do so in the future, hoping to find further points of contact between TAP and other broadly pragmatic theories.

To What Extent Do Emotional Expressions Have a Natural Meaning Separate from Context?

Fischer and Sauter raise this central question in their commentary, pointing out that often an emotional expression in "itself does not provide sufficient natural information for an observer to infer [an] emotion" (p. 192). Note that their worry is not that nonverbal displays do not express *any* emotions, as held by Fernández-Dols and Fridlund, but rather than nonverbal displays express *different* emotions depending on the context. They wonder whether this context dependence dooms from the start the project of associating natural meaning and communicative moves to emotional expressions, given that the bearer of natural meaning is never the emotional expression alone.

My view is that the project remains viable as long as we keep in mind that the units of natural meaning are for TAP "emotional expressions *in a context*." As Smith (1977) argued in his landmark book on animal communication, "without contextual sources of information displays would literally be meaningless, because the information provided by a display can rarely be sufficient to enable a recipient animal to choose an appropriate response" (pp. 19–20). If we add the idea that contextless displays can also be polysemous (multiple meanings) rather than just meaningless (no meaning), the quote accurately captures my view of emotional displays.

So, to address Fisher and Sauter's question, emotional expressions only have a specific natural meaning, and consequently the ability to make communicative moves, in their context of production. Once isolated from such context, they become at best ambiguous with respect to their meaning. Now, there may in principle be exceptions to this rule, constituted by multimodal emotional expressions that have the exact same natural meaning in all contexts. Perhaps a certain combination of facial, vocal, postural, autonomic, and behavioral changes always and only naturally means that a certain emotion is under way, independently of the context in which it is produced.

But the great majority of emotional expressions—and certainly facial, postural, and vocal expressions taken in isolation—do not work that way: Context plays a key role in determining what natural information they carry and consequently what communicative moves they make. This is why in Table 2 of my target article I have explicitly added the parenthetical "(in the right context)" to each emotional expression in the Examples column, to emphasize that the particular move I ascribe to the expression listed only holds in a certain context and may no longer apply once the context changes.

Why Does TAP Think of Emotional Expressions as Necessarily Nonverbal?

Austin (1962) distinguishes between three things we do when we utter a sentence: the locutionary act (the act of uttering it), the illocutionary act (what one does *in* uttering it), and the perlocutionary act (what one does *by* uttering it). TAP similarly distinguishes between the emotional expression (the nonverbal behavior of expressing the emotion), the communicative move (what one does *in* expressing the emotion), and the communicative effects (what one does *by* expressing the emotion).

In his commentary, Van Kleef puts pressure on the idea that emotional expressions must be nonverbal behaviors, contrary to my stipulation. He argues that there is *functional equivalence* between verbal and nonverbal ways of expressing emotions in terms of the communicative moves and communicative effects they make possible. For example, if I utter the sentence "I am angry with you right now" (in the right context) or if I bare my teeth and clench my fists (in the right context), I have thereby expressed my anger and I engage in the very same suite of communicative moves. According to TAP, in expressing my anger I represent your conduct as a slight, I direct you to back off and I commit to aggressive action if the matter of contention is not resolved.

Furthermore, the speech act and the emotional expression are alike in terms of their communicative effects. TAP does not say much about how these communicative effects come about, whereas the Emotions As Social Information (EASI) model developed by Van Kleef is specifically designed to answer that question. EASI's core proposal is that my expression of anger, regardless of whether it is verbal or nonverbal, may lead you to make inferences (e.g., that you have done something wrong), experience affective responses (e.g., become afraid of me), and change your behavior (e.g., you give up on the contested resource).

I agree with all that. So why do I say that emotional expressions must be nonverbal? My stipulation has a specific objective: to emphasize that the *mechanisms* through which speech acts and nonverbal expressions make communicative moves and bring about communicative effects differ in an essential way. In the first case, the speaker voluntarily provides evidence of his or her reflexive intentions, and the hearer makes an

inference about such intentions, reconstructing that the speaker non-naturally meant that he or she is angry.

In the second case, the signaler voluntarily or involuntarily provides evidence of his anger, and the recipient makes an inference that the nonverbal behavior naturally means that the signaler is angry. It follows that, although the communicative moves and the communicative effects are potentially exactly the same, the mechanisms through which they are brought about are relevantly different.

It is helpful in this context to remind ourselves of Marr's (1982) three levels of analysis in the study of information-processing devices:

- *Computational level*: What is the computational task being achieved?
- *Algorithmic level*: How is information processed to accomplish the task?
- *Implementation level*: What are the physical mechanisms underlying the information-processing algorithm?

Van Kleef's point is that, at the computational level, speech acts and nonverbal behaviors expressive of emotions are *functionally equivalent*, in the sense that they "do" the same thing from a communicative point of view. I agree, but I also want to emphasize that expressive speech acts and expressive nonverbal behaviors are different at the *algorithmic* and *implementational* levels. They say/show the same things and they bring about the same effects, but they do so by relying on different types of information processing (natural vs. non-natural information) and they are implemented by different physical mediums (physical implementers of bodily changes vs. physical implementers of utterances).

This being said, I am not wedded to my stipulated terminology, which goes against our habits of language. We ordinarily say that speech acts and nonverbal behaviors both count as emotional expressions. So I can be persuaded to change it. An alternative that van Kleef may find more appealing would be to distinguish between *overt* emotional expressions, which rely on non-natural meaning, and *nonovert* emotional expressions, which rely on natural meaning. What matters to me is that we keep track of both the functional equivalence between the verbal and nonverbal cases and the distinctions at the algorithmic and implementational levels.

Finally, it is definitely the case that what matters "from a practical point of view" are the communicative effects emotional expressions bring about, which is the topic EASI focuses on more closely. The first step in figuring out the communicative effects of emotional expressions, however, must be figuring out what communicative moves they make possible, which is what TAP tries to shed light on. Unless we know *what* emotional expressions communicate, we won't be in a position to systematically explore *how* what they communicate affects the inferences, affective reactions, and behaviors of recipients. To conclude, I consider TAP and EASI as fellow travelers in the project of explaining how emotional expressions communicate, and I look forward to putting their respective strength at the service of projects of common interest.

Are Commissives$_{EE}$ and Imperatives$_{EE}$ Really Possible?

I want to add a couple of final points on communicative moves and their distinction from communicative effects. Fischer and Sauter have voiced some misgivings about the existence of Commissive$_{EE}$ and Imperative$_{EE}$ analogs of speech acts. The problem they highlight with Commissives$_{EE}$ is that there is a great deal of variability when it comes to what one actually does *after* having expressed a certain emotion. Take an anger expression: Depending on the characteristics of the angry person and on the behavior of the interactant, the expression could be followed by "hitting, cursing, slamming the door, or sitting still depend[ing] on the context" (p. 192). Furthermore, as Fischer points out in her commentary, no action needs to follow the emotional expression, in the sense that an anger signal may "serve the function of 'not having to do certain things'" (p. 195). for instance, not having to engage in a physical fight.

This is all true, but it does not undermine the thesis that emotional expressions instantiate communicative moves of the Commissive$_{EE}$ variety. First, what emoters commit to is not any specific action, but an abstract goal that can be achieved through a variety of different concrete actions, depending on the context. The transition from the emotion to the goal is mediated by cognitive regulation, which joins forces with the emotion in the determination of behavior (Scarantino, in press).

Second, we should not think of commitments to action as indissoluble. As I discuss in my target article, commitments to action come in degrees, and they are rarely, if ever, carried out no matter what. For example, anger commits you to the broad goal of aggression through an internal mechanism that potentiates options for aggression and depotentiates options for peaceful resolution. It may turn out that the matter of contention is resolved before you have to do anything. But the fact that the communicative effect of an anger expression may be that you don't have "do certain things" does not show that you have not committed to aggressive action, namely, that you have not engaged in a Commissive$_{EE}$. On the contrary, the very reason why your opponent gives up on the contested resource, sparing you a physical fight, may be that you have manifested a commitment to aggressive action unless the matter of contention is resolved.

Fischer has another concern, namely, that "Commissives in speech act theory set up an obligation to perform a certain action [and] therefore have a normative connotation" (p. 195). For example, if I say, "I promise I will come to your party," I have thereby incurred an obligation to do so. She suggests that this normative dimension is absent in the nonverbal case. My first reaction to this objection is to note two points. First, there appear to be cases in which an emotional expression *does* generate an obligation. For example, if I express guilt over having cheated on you, I am not only expressing a commitment not to cheat on you in the future but also arguably generating an additional moral obligation not to cheat.

Second, not all linguistic Commissives generate obligations of the sort Fischer has noted. For example, if I say, "I will kill you unless you stop bothering my sister," I am expressing a (conditional) commitment to kill you but I am not incurring any obligation to do so if you do not stop bothering my sister. Now, I do not think these remarks fully dispose of the normative objection raised by Fischer, so I commit to thinking about it more in the future (am I thereby obligated to do so?).

Fischer and Sauter's concern about Imperatives$_{EE}$ is of a different sort. They point out that "there can be a huge

discrepancy between the information the signaler wants to get across, (e.g., by showing an angry face), and by the effects the expression has on the perceiver" (p. 192). An angry face may be a demand that the opponent gives up on a contested resource (backed up by a commitment to aggressive action), but this demand could have very different communicative effects on recipients, "ranging from sincere apologies to overt aggression" (p. 192).

This is also true, but not a threat to the claim that your anger expression is a demand to back off. Communicative moves consist of what you are communicating by means of a certain emotional expression. The communicative effects of such emotional expression can be highly variable with respect to the same communicative move. Notably, language works the same way. When I say "pass me the salt," I engage in an Imperative illocutionary act. But the perlocutionary effects of my act could be of many different kinds, ranging from the recipient passing me the salt to the recipient screaming that he or she is not my slave. To reiterate, communicative moves and communicative effects are two different beasts, and we must understand both—and their relation—to make sense of emotional expressions.

What Is the Relation Between the Production and the Perception of Emotional Expressions for TAP?

Let us assume for the sake of argument that emotional expressions must be nonverbal, and further explore what emotional expressions are. Fischer and Sauter rightly urge me to be clearer on the relation between the *production* of emotional expressions and the *perception* of emotional expressions. They note that I seem

> to implicitly define expressions as emotional, if observers can infer an emotion. This is a practical solution to this definitional problem, but it still raises the question of whether EE that are not perceived as emotional, but still were produced while experiencing an emotion, would count as EE. (p. 190)

For example, suppose that an agent produces a certain vocalization out of relief but that such vocalization is not recognized as expressing relief. As it turns out, this is not just a hypothetical scenario. Fischer and Sauter report that there is empirical evidence that relief vocalizations are not reliably recognized cross-culturally.[4] The question is, Would the agent have thereby expressed relief if recipients cannot perceive it?

To properly answer this question requires that I say more about what natural information is. In so doing, I will also address some of Fridlund's concerns, which dovetail with those articulated by Fischer and Sauter. Fridlund asks, "Whose natural information are we talking about?" (p. 199). I mentioned in my target article that natural information is carried by means of statistical correlations. But which statistical correlations matter? If my lab discovers that relief and sighing are statistically correlated, Fridlund asks, but one or more recipients fail to recognize this, has the sighing carried natural information about relief/expressed relief?

These are all good questions, and I hope I can answer them. My theory of natural information, articulated in detail in Scarantino (2015), is inspired by Dretske's (1981) account of natural information. Dretske's foundational insight was the following: "Information is that commodity capable of yielding knowledge, and what information a signal carries is what we can learn from it" (p. 44). On this view, if we can learn that you are relieved from a certain vocal expression X, then X carries natural information about your relief. Carrying natural information boils down to affording *opportunities for learning*.

I emphasize that the notion of learning used in this literature is probabilistic. For example, we can learn that you are relieved from a certain vocalization x just in case the following inequality holds: p(relief|vocalization x & background knowledge) > p (relief | background knowledge). In other words, vocalization x carries natural information about relief when the probability that you are relieved given x and our background knowledge is higher than the probability that you are relieved given just our background knowledge. Another way to put the point is that we can learn about relief from vocalization x just in case events of the relief type and events of the vocalization x type are positively statistically correlated relative to background knowledge.[5]

But who are "we" exactly? It is pretty obvious that different recipients will learn different things from the same signal, given their different states of background knowledge. For example, if I am your husband and I intimately know how you behave when relieved, my background knowledge will allow me to learn from your subtle sighing that you are relieved. But if I am a stranger, I do not share the same extensive background knowledge, and the statistical correlation may not hold for me: Given my limited background knowledge, the probability that you are relieved is not increased by my witnessing your subtle vocalization.

This leads me to distinguish two notions of natural information as it pertains to the expression of emotions "in a context":

> A bodily change x in context C carries natural information about emotion E *to standard recipients in standard circumstances* just in case such recipients in such circumstances would learn about E from the bodily change x upon observing it in context C

> A bodily change x in context C carries natural information about emotion E *to some specific recipient R* just in case R would learn about E from the bodily change x upon observing it in context C

On this view, a relief vocalization carries natural information about relief *to standard recipients in standard circumstances* just in case such recipients would learn that the signaler is relieved from the vocalization. If Fischer and Sauter are right

[4] Fischer and Sauter also report on the case of surprise expressions, which appear to be recognizable cross-culturally through the snapshot method but are often not spontaneously produced when surprise is experienced, and they wonder what TAP makes of this difference between perception and production of surprise expressions. My view is that the recognition of the snapshot of a highly prototypical facial expression of surprise across cultures does not "point to a one-to-one mapping of emotion type and EE" even at the level of perception, contrary to what Fischer and Sauter suggest. All that it shows is that there exists *one* expression of surprise—the one captured by the snapshot—that is recognized cross-culturally. That very expression of surprise captured by the snapshot may rarely be produced spontaneously, and it may also be the case that no visual displays—prototypical or not—are reliably produced when surprise is experienced. I take this to show that there is a many-to-many association between emotion types and types of facial expressions at the level of both production and perception.

[5] To say that X and Y are statistically correlated relative to background knowledge k is simply to say that p(X given Y & k) is higher or lower than p(X given k).

about the empirical data on relief, standard recipients in standard circumstances would *not* be able to perceive/infer the presence of relief from such vocalizations, and so natural information is not carried *to them*.

At the same time, it may well be that there is a specific recipient R relative to which the second condition is satisfied (R could be the signaler's wife, or, as in Fridlund's example, "Scarantino in his lab"). Given R's distinctive background knowledge, R may well be in a position to learn about the presence of relief from the sighing even though standard recipients in standard circumstances would fail to do so.

The upshot is that it is possible for a certain bodily change to express a certain emotion to a certain recipient without doing so to standard recipients in standard circumstances. Conversely, it is possible for a certain bodily change to express a certain emotion to standard recipients in standard circumstances, but not do so relative to a specific recipient who is not standard in some relevant sense or who is not operating in standard circumstances.

Our common language is systematically ambiguous between a "standard recipients in standard circumstances" reading and a "specific recipient" reading of emotion expression ascriptions. For example, when we say that "John's facial movements expressed his anger," we sometimes mean that they did so relative to a specific recipient (e.g., his wife), and we sometimes mean that they did so relative to unspecified standard recipients in standard circumstances.

The implication is that, as far as my theory of emotional expressions is concerned, whether or not a bodily change is *produced* by an emotion is irrelevant to the question of whether the bodily change expresses the emotion: What matters is whether the bodily change carries natural information about the emotion (in one of the two senses specified), which in turn depends on whether the bodily change provides an opportunity for probabilistic learning about the emotion by providing evidence for it.

But what about the expresser, Fischer, Sauter, and Fridlund ask? For example, if the expresser is in fact angry and his anger is what made him produce a certain facial movement and the expresser takes himself to be "mad as hell," does it not follow that the facial movement expresses anger? Not necessarily. According to TAP, a facial movement cannot express an emotion unless it carries natural information about it. The very etymology of the term "expression" is to "press out," which suggests that something is an expression insofar as it is brought "out" for public consumption. In other words, the expresser is expressing anger only if he provides evidence for it, which is very likely—but not strictly assured—if the expresser is in fact mad as hell.

Now back to Fischer and Sauter's question: Can a bodily change produced while experiencing an emotion count as an emotional expression even if none perceives it as such? My answer is: yes, with a caveat. A bodily change performed in solitude, although not perceived by anyone, can still express an emotion. The reason why it can still express it, however, has all to do with the learning opportunities it affords to hypothetical recipients. The basic idea here is that a bodily change x produced while experiencing an emotion E expresses it just in case recipients would learn about E were they exposed to bodily change x.

This is to say that the property of carrying natural information is a *dispositional property*: Things have it or lack it by virtue of what would happen in hypothetical circumstances, just like things are fragile or soluble by virtue of what would happen in hypothetical circumstances (they would break if hit, they would dissolve if put in water, etc.). An analogy that may help is that with color properties, which many philosophers of perception consider to be dispositional properties.

In their view, an object can be yellow even when no one is perceiving it as yellow because the object is stuck, say, in a completely dark room. The reason *why* it is yellow, however, is that, were that object placed in front of standard recipients in standard circumstances, such recipients would have a perceptual experience of yellowness. In the color case, as in the emotional expression case, *actual perception* is dispensable, whereas *potential perception* is essential.

There are of course alternatives to my *informational theory* of emotional expressions. On a *causal theory* of emotional expression, for instance, a bodily change expresses an emotion just in case it is caused by the emotion. This is not the place to explain why I consider causal theories, which privilege the causal relation between the expresser and the emotion, inferior to informational theories, which privilege the evidential relation between the emotion and the recipient. In this section, I hope I achieved a clarification of the relation between production and perception of emotional expressions proposed by TAP.

How Much "Nested" Natural Information Does TAP Think an Emotional Expression Carries?

Fridlund describes as "preposterous" Ekman's (1997) claim that emotional expressions carry seven types of information: information about (a) the antecedent circumstances, (b) the expresser's thoughts, (c) the internal physical state of the expresser, (d) metaphors applicable to the expresser, (e) what the expresser is likely to do next, (f) what the expresser wants the perceiver to do, and (g) what emotion the expresser is experiencing.

For example, it is allegedly preposterous that a "bared-teeth face" carries information about anger, plus information about specific "memories, injunctions, requests, reports of internal physiology" (p. 199). This is because the "specifics" of the context of production may "equally lead me to conclude that the expressor was threatened, traumatized, constipated or otherwise in terrible pain" (Fridlund, this issue, p. 199). Fridlund concludes that "for Scarantino, like Ekman, the bared-teeth face must be an 'emotional expression,' specifically an 'anger face'" (p. 199). If we don't see "the alternatives," it must be because we are wearing basic emotion theory "blinders."

This is a gross misrepresentation of TAP (I won't discuss the extent to which the critique applies to Ekman). As I emphasize in my article, "Students of facial movements have … realized the importance of context in determining what facial movements carry natural information about" (p. 170). I have proceeded to give specific examples of how changes in contexts—environmental context, bodily context, and so on—can change what natural information a given facial change carries.

So my noncaricatured view is that the "bared-teeth face" expresses different emotions in different contexts, and no emotion in some contexts. It *can be* an "anger face," but it *can also*

be a "threatened face," a "traumatized face," a "constipated face," or a "pained face." One looks in vain for the reason why the "anger face" should be singled out as the only one worthy of elimination. Or does Fridlund think that our science of threats, traumas, constipation, and pain is incomparably better than our science of anger?

Fridlund describes himself as an agnostic about emotion, but his methodological stance is best described as *ostrich agnosticism*, because it involves the pretense that our collective failure to come up with a consensual scientific definition of emotions makes us incapable of agreeing on whether what we are observing is or is not an emotion. This would be like saying that because chemists could not agree on a definition of water until the 19th century—when the consensual scientific definition "water is H_2O" finally emerged—there was no agreement among them for centuries on whether water was present or absent. Modern-day agnosticism about emotion is about as warranted as pre-19th-century agnosticism about water.

Fridlund worries that emotion science lacks "inclusion and exclusion criteria for stipulating whether people are having a particular emotion," namely, criteria that state which "specific signs and symptoms must be present to say that the emotion is present" and which "specific signs and symptoms must be present to say that it is absent" (p. 201). The three most influential theories of emotions in affective science these days are probably basic emotion theory, psychological constructionism, and appraisal theory. Fridlund is absolutely right that there is no consensus among these research programs about how emotions should be defined. If you ask Ekman (a basic emotion theorist), Russell (a psychological construtionist), and Scherer (an appraisal theorist) to define emotion in general, or a particular emotion like anger, you are going to get three different answers.

However, if you present Ekman, Russell, and Scherer with a prototypical episode of anger—say, the case of a man who is insulted in a parking lot, starts trembling, manifests decrease in saliva flow and increase in rate of respiration, shows eyebrows lowered and pulled together and a stiff posture, and screams in a high-pitched voice "I will kill you!" while lunging toward the opponent—they will all agree that anger is present, while continuing to disagree on how anger should be defined. This means that their three distinct definitions of anger overlap a great deal with respect to which items they pick out in the world, although disagreements about candidate anger instances are likely to become increasingly significant as we move away from prototypical cases (so there is no consensus on *all* instances of anger).

Fridlund retorts that "there is no evidence that [affective scientists] systematically agree" (p. 202) on what count as instances of emotions, but this is simply not true. They do not agree on what emotions are, but they agree on what count as instances of emotions, just like early chemists disagreed on what water is but agreed on what count as instances of water (in prototypical cases at least). To sum up, the inclusion and exclusion criteria we need to allow emotions within the explanatory toolbox of contemporary affective science are readily available, and they take the following form:

> *Consensual Meta-Criterion of Inclusion*: If an item x satisfies the scientific definition D of anger/fear/shame/guilt/etc. proposed by basic emotion theory & the scientific definition D' of anger/fear/shame/guilt/ proposed by psychological constructionism & the scientific definition D" of anger/fear/shame/guilt/etc. proposed by appraisal theory, then conclude that x is an episode of anger/fear/shame/guilt/etc. (i.e. emotion is present)
>
> *Consensual Meta-Criterion of Exclusion*: If an item x does not satisfy the scientific definition D of anger/fear/shame/guilt/etc. proposed by basic emotion theory & does not satisfy the scientific definition D' of anger/fear/shame/guilt/ proposed by psychological constructionism & does not satisfy the scientific definition D" of anger/fear/shame/guilt/etc. proposed by appraisal theory, then conclude that x is not an episode of anger/fear/shame/guilt/etc. (i.e. emotion is absent)

The reason why these are *meta-criteria* is that they combine the criteria of inclusion and exclusion used by competing theories of emotion, namely the various "signs and symptoms" used by, respectively, basic emotion theorists, psychological constructionists and appraisal theorists to decide whether anger/fear/shame/guilt/etc. is instantiated. These two meta-criteria fall far short of a consensual scientific definition of anger/fear/shame/guilt/etc., but they do allow us to determine *with the force of scientific consensus* whether anger/fear/shame/guilt/etc. is present or absent. The reason is that in a great many – but not all – cases the criteria of inclusion and exclusion used by competing theories of emotions agree on whether or not some x is anger/fear/shame/guilt/etc. (we can label all candidate instances for which scientific consensus is lacking as "borderline cases").

To emphasize this point is not to engage in a "naked rhetorical ploy", as Fridlund suggests, but to earnestly try to bridge a gap that has for far too long divided the Behavioral Ecology View from the majority of scientific accounts of expressions, which take it for granted that, as as you can legitimately describe a face in some contexts as a "threatened face", a "traumatized face", a "constipated face", or a "pained face", you can also legitimately describe it in some contexts as an "anger face".

There is a deeper reason why lack of scientific consensus on how to define anger, fear, shame, guilt, etc. does not support agnosticism, besides the fact that the inclusion and exclusion criteria for such categories overlap in their membership verdicts and so allow for consensus on what count as prototypical instances. The reason is that a consensual scientific definition of anger/fear/shame/guilt/etc. is *not* what we should be looking for, because these folk categories are unlikely to designate natural kinds (Scarantino 2012).

The categories of anger, fear, shame, guilt and so on are too heterogeneous to allow for the extrapolation of scientific discoveries about category members to the whole category. What I recommended in my target paper is the exact opposite of the search for consensus, namely *pluralism* about how to define anger/fear/shame/guilt/etc. Pluralism is idea that *multiple* scientific definitions of anger/fear/shame/guilt/etc. are required to carve out natural sub-categories of these folk categories, just as the distinct definitions of *short-term memory* and *long-term memory* were required to carve out two natural sub-categories of the folk category of memory.

Fridlund's agnosticism about emotions is also powered by the long-standing assumption that affective scientists tend to conceptualize emotions as feelings or states of subjective experience or qualia. Absent this assumption, Fridlund's frequent use of the analogy between emotions and Cartesian "ghosts in

the machine" would not make sense, because these ghostly events are supposed by Ryle (1949) to take place within the stream of consciousness. The assumption also explains why Fridlund reacted to my claim that a dog can communicate pleasure by asking how we could possibly measure "canine qualia".

The trouble is that none of the contemporary dominant theories of emotions in affective science – basic emotion theory, psychological constructionism and appraisal theory – holds that emotions are to be defined as states of consciousness (Scarantino 2016).[6] Fridlund appears wedded to his own very personal "dogma of the ghost in the machine of emotion", namely the dogma that affective scientists conceptualize emotions as states of consciousness or qualia. Well, they don't. If so, the problem of qualia, canine or otherwise, does not support agnosticism about emotions either, because emotions can be understood in a qualia-free fashion, for instance as motivational states with control precedence (Scarantino 2014). As it turns out, agnosticism about emotions lies on very shaky foundations: its supporters should either come up with better arguments for it or abandon it for good.

But something else seems to bother Fridlund with respect to the idea that emotional expressions carry a multiplicity of kinds of information, a prospect he describes as "a feat of signal multiplexing worthy of fiber-optic cable" (p. 199). This remark suggests that he thinks that we are facing something along the lines of a *bandwidth problem*, in the sense that we are squeezing into emotional expressions *more* natural information than they could possibly carry.

There is nothing mysterious about this multiplexing feat if one understands how natural information is transmitted, namely, by delivering learning opportunities relative to background knowledge. Let us focus on card games first. Suppose that, while playing cards with Fridlund, you receive a visual signal that lets you know that he has been left with only one card: an ace. What information does this signal carry to you? The answer is: an unlimited amount. This is because the number of things you can learn from this signal in combination with your background knowledge has no upper limit, even though most of what you can learn will be of no interest whatsoever.

The reason why you can learn innumerable things from a solitary signal is that the signal, in combination with your background knowledge, licenses innumerable inferences about the world. For example, any signal carrying the information that Fridlund has an ace *also* carries information about his having either an ace or a seven (by logic), about his having an ace or a seven or a three (by logic), about his not having a king (by logic), about his not having a queen (by logic), about his having your wife's favorite card (by knowledge of her preferences), about his having won the game (by knowledge of the game's rules), about his being a really good player (by knowledge of his previous four wins), about his having sharp mental faculties (by knowledge of what it takes to win), about Anthony Campbell having framed you (by knowledge of how you were lured into the game by Campbell's false promise of playing an amateur), and innumerable other things, contingent on your background knowledge.

This well-known property of natural information is often referred to as *nestedness*: Within any piece of information, there are nested innumerable other pieces of information. Some of the nested information is underwritten by relations of necessitation, and some is underwritten by probabilistic relations. For example, it is necessary that if Fridlund has an ace, he also has either an ace or a king, but it is only highly probable that if Fridlund completes his fifth win at a game of cards then he is a great player (he might just be a very lucky amateur).

Why is this relevant to emotional expressions? Because their reception in a given context does not simply inform you that the agent is having a certain emotion in that context—in the probabilistic sense of raising the probability of such state of affairs relative to background knowledge. A whole lot more additional information is "nested" within the information that the agent is in a certain emotional state.

This fact can be used to address a worry that some commentators have voiced about the very possibility of engaging in four communicative moves in one fell swoop. When we express an emotion ($\text{Expressive}_{\text{EE}}$), do we always represent a state of affairs ($\text{Declarative}_{\text{EE}}$), make a demand ($\text{Imperative}_{\text{EE}}$), and make a commitment to future action ($\text{Commissive}_{\text{EE}}$)? My answer is affirmative: Anything that carries information about what emotion someone is having ipso facto carries "nested information" about anything that correlates with the emotion, including how the emotion represents the world, what demands characterize the emotion, and what behavioral commitments are associated with the emotion.

As it turns out, there is even "more" nested information than Ekman or I accounted for, enough to give Fridlund a serious case of heartburn. As Bjornsdottir and Rule's commentary shows, emotional expressions correlate with membership to a variety of social groups, and are richer communicative devices than either BET or TAP has acknowledged. But before I move to Bjornsdottir and Rule's commentary, I need to address one of Fridlund's most extravagant charges, namely, that my "treatment of Ekman is superficial and contradictory" (p. 197). It is "superficial" because I do not sufficiently emphasize the *changes* in Ekman's thinking pre- versus post-1990. I have presented the post-1990 version of basic emotion theory because—surprise, surprise—this is the one that holds right now. I had no time, space, or inclination in my target article for discussing *how* Ekman arrived at his current position. Nothing about my analysis hinges on what Ekman thought pre-1990.

In addition, Fridlund suggests that my "treatment of Ekman is … contradictory" (p. 197). The alleged contradiction amounts to this: Whereas I present

> the argument that Ekman's early cross-cultural studies may demonstrate uniformity via common descent, [I] also praise Ekman's

[6] Ekman, Russell and Scherer are three examples of emotion scientists from different research programs who do not consider emotions to necessarily involve states of consciousness. At least since the mid 1990s, Ekman has described subjective experiences as being a distinctive feature of basic emotions but not a 'sine qua non' feature (and Ekman 1992 did not even include subjective experience among the defining characteristics of basic emotions). Russell (2003) has described emotion episodes as commonly having core affect as their parts, where core affect is a neurophysiological state that is consciously accessible as a feeling. But Russell has added that core affect is not necessarily involved in episodes of emotions, and that core affect itself need not be accessed as a feeling (it has to be accessible but not accessed). Scherer (2005) has described subjective experience as just one of five components of emotions (the others being an appraisal component, an autonomic physiology component, an action tendency component, and a motor expression component), allowing emotions to be instantiated whenever three of these five components co-occur, which allows for emotions which do not involve any qualia.

> litany of seven items of information that all facial "emotional expressions" supposedly carry. But if those expressions carry seven kinds of information, how can those early studies possibly prove "universality of basic emotions" if they assessed only one of the seven? That Scarantino can uncritically entertain both positions attests to his superficial treatment of Ekman's thinking. (p. 197)

First, as it will be obvious to any detail-oriented reader of my article, I am emphatically *not* endorsing the view that Ekman's early cross-cultural studies demonstrate uniformity. I am simply presenting Ekman's view while remaining noncommittal on whether it is convincing (as Fridlund himself acknowledges a few pages later, in a confusing reversal). Second, whether expressions carry the seven kinds of information Ekman lists is irrelevant to the universality thesis he proposes and defends. Fridlund is conflating the following two universality theses:

> *Universality Thesis 1*: Snapshots of facial expressions carry across cultures the same information about what emotion the subject is experiencing (what Ekman calls information about "emotion words").

> *Universality Thesis 2*: Snapshots of facial expressions carry across cultures the same information about antecedents, the same information about the person's thoughts, the same information about the internal physical state, the same information about metaphors, the same information about what the expresser is likely to do next, and the same information about what the expresser wants the perceiver to do.

To confirm that snapshots of facial expressions carry the same information about what emotion the subject is experiencing across cultures (Thesis 1), we do not need to investigate whether they carry information in all other domains (Thesis 2). My view is that Ekman has *not* established the truth of Universality Thesis 1. But the reasons why he has failed to do so have nothing to do with the fact that he has not established the truth of Universality Thesis 2, because Thesis 1 differs from Thesis 2.

This being said, I am in agreement with Fridlund that basic emotion theorists are wrong to presuppose that "uniformity implies phylogeny, whereas diversity implies cultural learning" (p. 199). Even if Ekman had successfully established the truth of Universality Thesis 1, it would not follow that the emotions that produced such universal expressions evolved. As Fridlund points out, "Communalities in expression can arise either phyletically *or* via convergence, and cultural variability can owe either to learning *or* to genetic differences among the cultures from migration, drift, founder effects, mutation, and/or epigenetic marking" (p. 199).

Do Emotional Expressions Carry Natural Information About Group Membership?

Bjornsdottir and Rule (this issue) propose a development of TAP in new directions, in ways I find promising and definitely worth pursuing. Their main point can be summarized as follows: Nested within the information that an agent is having a certain emotion, there is also information about the agent's *social group*, which becomes relevant for social categorization especially in the presence of ambiguity. In particular, Bjornsdottir and Rule provide plenty of data that emotional expressions can carry natural information about the agent's race, sex, sexual orientation, political affiliation, and social class.

For example, they report that if a face carries information about a subject being angry, it also carries information about the subject being Black relative to the background knowledge available to White perceivers high in implicit prejudice against Blacks. In other words, detecting anger on a face makes it more likely for people with implicit prejudice that the subject will be categorized as Black, especially when the faces are racially ambiguous.

Nested within the information about what emotions the subject is expressing is also information about sex and sexual orientation. As Bjornsdottir and Rule report, if people think that a certain androgynous face expresses anger, they are more likely to categorize it as male, whereas if they think that it expresses fear or happiness they are more likely to categorize it as female. In addition, when people are asked to nonverbally communicate that they are gay or straight, they enact, respectively, happy or angry expressions (this holds true whether the enacters are straight or gay).

Bjornsdottir and Rule add that expressing happiness and anger can also provide relevant information about political affiliation. For example, people expressing happiness and anger are more easily categorized as, respectively, liberals and conservatives. Finally, facial expressions of emotion carry information about an agent's social class, with happiness signaling wealth and sadness signaling poverty.

Van Kleef has summarized other data along similar lines in his commentary, with focus on personality traits and group membership. He reports that several studies have shown that detecting an anger expression or a happiness expression in a negotiating partner conveys the information that the negotiator is, respectively, tough and ambitious or lenient and easily satisfied. In addition, deviant group members pick up information about their status within the group on the basis of the emotional expressions they witness, with anger informing about rejection and happiness informing about acceptance. The amount of "social information" nested within emotional expressions could probably be further expanded. The bottom line is that emotional expressions are rich "informational hubs": They carry information not only about emotions but also about many other things that correlate with emotions.

Bjornsdottir and Rule raise an important question at the end of their commentary: How does the social information nested within emotional expressions affect the communicative moves we make with them? For example, if my bodily changes do not simply carry information about what emotions I am having but also information about my race, sex, sexual orientation, political affiliation, social class, and personality traits, how does this affect the Declaratives_{EE}, Imperatives_{EE}, Commissives_{EE}, and Expressives_{EE} I produce? Bjornsdottir and Rule invite us to consider an expression of anger by a heterosexual patron in a gay club: What communicative moves does this expression make?

The answer is not obvious, and the authors walk us through some interesting possibilities. It seems clear that the nested social information can affect the message delivered by sharpening it or modifying it in various ways. For example, what is expressed by a frowning face in the context of a gay bar may not just be that the signaler is angry but also that he is feeling threatened. What is declared may not just be that the signaler

at some level blames the patrons but also that he is heterosexual. What is demanded may not just be that the patrons should stop considering him one of them but also that they should not approach him with sexual propositions. What the signaler commits to may not just be generic aggressive action but more specifically aggressive action in response to sexual propositions, perhaps combined with openness to more friendly engagement with patrons who respect his heterosexuality.

Whether these speculations are on the right track remains to be experimentally explored, on the background of a general theory of how the social information nested within emotional expressions affects the communicative moves and the communicative effects they make available.

Does TAP Really Think That All Emotional Expressions Are Signals?

TAP holds that carrying natural information is necessary but not sufficient for something to count as an emotional expression. In addition, an emotional expression must be "designed" for information-mediated influence. In other words, an emotional expression must have the *function* of influencing a recipient by means of information transfer.[7]

Not all things that carry natural information are selected for doing so. For example, a bird nest carries natural information about the presence of young birds to predators, but it does not have the function of carrying such natural information. On the other hand, a vervet monkey alarm call not only carries natural information about predators but also has the function of doing so.

In animal communication theory, information bearers with the function of carrying information for the purposes of influencing have generally been labeled as *signals*, whereas information bearers that happen to carry natural information but do not have the function of doing do are called *cues* or *signs*.

Fischer and Sauter argue that it is not clear that all emotional expressions are signals: Some of them might be cues or signs. The reason is that "some emotional expressions" may have been "selected primarily—or even exclusively—for noncommunicative functions" (p. 190). They point out for instance how fear expressions can serve a practical purpose like increasing visual acuity or how pain expressions can serve a practical purpose like removing oneself from the source of pain.

I do agree that some emotional expressions may have been selected for their practical rather than for their communicative functions, and I have said as much in my target article. The question is, Does it follow from the fact that some emotional expressions were selected for practical functions that they are not signals, in the sense that they do not have the function of influencing recipients through the information they carry? Well, it all depends on which notion of function we adopt.

The notion of function that is germane to the explication of signals is that of *selected function* (a.k.a. historical function, etiological function, backward-looking function), where the selected function of a behavior or a trait consists of those effects that have in the past contributed to the selection of organisms who perform that behavior/have that trait. An alternative is the notion of the *current function* of a behavior or a trait (a.k.a. forward-looking function, dispositional function), which has to do with what the beneficial effects the behavior or trait is currently disposed to produce (see Garson, 2016, for an accessible introduction to the literature on biological functions).

When we call a behavior a signal, we are saying that the behavior has the selected function of influencing by means of information transfer. But how far in the past should we go to establish the selected function of a behavior? Students of functions have drawn a distinction between *ancient history* and *modern history* theories of selected functions (Godfrey-Smith, 1994). According to the ancient history theory, the selected function of a behavior is what it did in the environment of evolutionary adaptiveness that explains why we have it. According to the modern history theory, the selected function of a trait is what it did during modern history that explains why we maintain it.

This distinction becomes critical when we consider the difference between *adaptations* and *exaptations*:

- *Adaptation*: "any feature that promotes fitness and was built by selection for its current role" (Gould & Vrba, 1982, p. 6)
- *Exaptation*: any feature that "evolved for other usages (or for no function at all), and later "coopted" for their current [fitness-promoting] role" (Gould & Vrba, 1982, p. 6)

For example, the heart is an adaptation for pumping blood, because pumping blood is what hearts were selected for, and pumping blood is their current fitness-promoting role. Bird feathers are an exaptation for flying, because they evolved for thermoregulation but were at some stage coopted to enable flying.

If we apply the ancient history notion of selected function to bird feathers, we will conclude that they do not have the selected function of flying, because this is not what they did in the environment of evolutionary adaptiveness. But if we apply the modern history notion, we will conclude that bird feathers have the selected function of flying, because this is the fitness-promoting role they started playing at some point in evolutionary history that explains why they were maintained (and eventually modified in various ways).

Fischer and Sauter are right that not all emotional expressions are *adaptations for communication*, because some (and possibly many) were originally selected for practical rather than for communicative purposes. This is compatible with thinking of them as *exaptations for communication*, provided that they were eventually coopted for their current communicative role. And if they are exaptations for communication, they could still qualify as signals on the modern history notion of selected function. Although they were not originally selected for communicating, they started being selected for communicating in more recent history.

I am convinced that many of the examples offered by Fischer and Sauter fully qualify as exaptations for communication. At the same time, I do not think that they all necessarily are. It is quite possible that at least some emotional expressions never had even a recent history of selection as communicative devices. Furthermore, it is conceivable that at least some

[7] Fridlund states that I starkly dichotomize influence and information, but this is yet another misreading of my target article. I have clearly stated that *Dawkins and Krebs* (not me!) initially dichotomized information and influence, and then I explained why they changed their mind, coming to the conclusion that, to quote my article, "the extraction of natural information on the basis of statistical correlations and the attempt to exercise influence are complementary and equally important aspects of nonverbal communication" (p. 169).

emotional expressions had no history of selection of any kind, whether as practical or as communicative devices.

What follows from this is that TAP's assumption that *all* emotional expressions are signals is probably wrong, just as Fischer and Sauter have suggested. The good news is that, whether or not an emotional expression is a signal has no obvious impact on the communicative moves it makes and on the communicative effects it brings about on recipients. In other words, it does not matter to recipients whether a certain bodily change that informs them that, say, an agent is afraid was selected for communicative purposes. What matters is that it currently informs them that their interlocutor is afraid, a realization that licenses a number of important inferences about the agent's representations, demands, and commitments. So it is my hope that, once we open the door to emotional expressions that are cues rather than signals, the basic taxonomy of communicative moves I have offered remains intact.

Did Darwin Consider Emotional Expressions to be Adaptations, Exaptations, or Vestiges?

My discussion of Darwin's legacy has attracted quite a bit of critical attention from Fridlund. This is understandable because his careful historical work has been instrumental in shedding light on the complexities of Darwin's analysis of expressions and of the broader role it plays in the Darwinian corpus. In my brief treatment, I certainly did not do justice to the richness of Darwin's views on expressions. But I stand behind my assessment that the balance of evidence suggests that a great many emotional expressions are ultimately not vestiges for Darwin, contrary to Fridlund's interpretation.

We have covered adaptations and exaptations in the previous section, so what remains to be defined is the notion of a vestige:

- *Vestige*: any feature that used to promote fitness but no longer plays a fitness-promoting role

For example, the human appendix is a vestige, because it now plays no useful role even though it used to play a useful role in evolutionary ancestors of humans. Note that the notion of a vestige is sometimes used more broadly, to include within it both features that are now functionless and features that now have a new function (exaptations).

Fridlund begins by saying that "contra Scarantino, for Darwin our facial expressions are *not* selected, even 'as component parts of full-fledged adaptive actions' … because they are *not* adaptations" (p. 198). There is a misunderstanding here, because I do not think, nor have I said, that Darwin takes facial expressions to be adaptations, at least in the sense in which I understand this term. For them to be adaptations sensu Gould and Vrba (1982), they would need to have now the same fitness-promoting role they had in the environment of evolutionary adaptation, which is definitely not Darwin's position.[8]

My view is rather that for Darwin a great many facial expressions are exaptations for communication, namely, features that used to have a practical function they have now lost and have been coopted for communication. Fridlund's view is instead that they are "degenerated, fragmented rudiments of movements that were *once* functional, and … are [now] mere vestiges," with the latter understood as movements that make "no current functional sense but may have made sense ancestrally" (p. 198).

I will bracket the question of whether for Darwin facial expressions were selected "as component parts of full-fledged adaptive actions" (p. 198). To arbitrate this aspect of the debate would require a lengthy discussion of Darwin's ideas on the workings of natural selection, with special focus on the role the Lamarckian principle of inheritance of acquired characteristics played in Darwin's understanding of how natural selection works on acquired and random variations. In addition, I would have to say more about how my concept of selected function maps onto the concept of natural selection (I am, e.g., not committed to natural selection being the only mechanism of selection that underlies selected functions).

I want to focus on the question of whether emotional expressions are vestiges or exaptations for Darwin. It seems clear to me that Darwin thought that emotional expressions can take on a new communicative function in current environments. I reported several quotes to this effect in my target article, concerning, for instance, the communication between mothers and infants or the communication of sympathy between adults. This satisfies Gould and Vrba's (1982) definition of an exaptation, which is a feature that is currently fitness promoting but "evolved for other usages (or for no function at all)" (p. 227).

Fridlund remains convinced that Darwin understands emotional expressions as vestiges.[9] Now, it is certainly true that the idea of emotional expressions as vestiges must have been very attractive to Darwin. To respond to Bell's claim that God had given emotional expressions to man so that they could communicate their emotions, Darwin was indisputably tempted by the claim that they are now functionless, because this would have been a clear-cut way to undermine the creationist argument that God had given them to man for their communicative function.

Furthermore, Fridlund is quite correct that Darwin had already used vestiges to bolster his argument for natural selection in the *Origin of Species* (Darwin, 1859). As Fridlund points out, the preservation of traits that have no function played a major role in the *Origin* in rebutting creationist arguments, because God would not create functionless or otherwise imperfect traits. At the same time, there is enough textual evidence to support the view that for Darwin emotional expressions, regardless of their origin, can be of current use in communicating with one another.

Interestingly, Fridlund admits as much, but adds that if emotional expressions had "any current communicative use, it

[8] A possible exception concerns a handful of emotional expressions that Darwin takes to have evolved through sexual selection, but I disregard this case in what follows. I also disregard the case of the emotional expressions that are direct effects of the activation of the nervous system or produced by antithesis. My focus throughout this section is exclusively on those emotional expressions Darwin explains through the principle of serviceable associated habits.

[9] I notice in passing that Fridlund (1991, p. 5) seemed open to the idea that Darwin thought of emotional expression as exaptations. I focus on Fridlund's response to my target article here, without trying to reconcile different stages of Fridlund's thinking.

was entirely incidental" (p. 198). As he continues, "Darwin conceded that our expressions might be useful to others, but that is a happy accident, in the way that burps tell others that we're full, but that's not what burps were, or are, *for*" (p. 198). This is a peculiar rebuttal, because happy accidents are the bread and butter of exaptations. For example, it is a happy accident that the feathers birds used for thermal isolation could also be used for flying. What they were *for*, as Fridlund would put it, is thermal insulation.

But would we not give away Darwin's game by foisting upon him this interpretation? Fridlund seems to think so, as he thunders that I have "missed completely" the subtle logic of Darwin's *Expression*. I beg to differ. The first point to emphasize is that Darwin was familiar with the concept of exaptation, even though he did not have a name for it. One of the many examples of exaptations we find in the *Origin* is that of the "the sutures in the skulls of young mammals" (Burkhardt, 1985, p. 413).

Darwin points out that such sutures are not an "adaptation for aiding parturition," but he immediately adds that they "facilitate, or may be indispensable for this act" (p. 413). What sutures in the skulls of young mammals are for Darwin, then, is exaptations for parturition. The same logic seems to me broadly applicable to Darwin's views on emotional expressions: Although they are not an adaptation for communication, they may facilitate or even be indispensable for communication. In this sense, they differ from vestiges like "phalanges in bat wings, pelves in snakes and whales, congenitally blind salamanders, and the human appendix" (Fridlund, this issue, p. 198), which have not acquired any incidental new function.

As I mention in a footnote, Darwin's primary concern was to argue that emotional expressions have emerged from a process of evolution—by which I do not mean, contrary to what Fridlund suggests, that they are adaptations but rather that they have their origin not in divine intervention but in a natural process of modification through successive generations. Explaining how a natural process of progressive change over time could have led to the emergence of emotional expressions served Darwin's main purpose in *Expression*, namely, to "upset Sir C. Bell's view … that certain muscles have been given to man solely that he may reveal to other men his feelings" (Darwin in a letter to Wallace in 1887, as cited in Fridlund, 1992, p. 119).

It is important to note that this objective remains within reach for Darwin if emotional expressions are exaptations rather than vestiges. This is because if emotional expressions did not emerge for communicative purposes, but rather acquired a communicative function over time through a happy accident, the idea that God created them for man *specifically* for communicative purposes would still be undermined (God is not in the "happy accidents" business).

To conclude, Fridlund deserves much credit for having undermined the once prevalent but mistaken interpretation that Darwin believed that emotional expressions evolved for communicative purposes. At the same time, I submit he has made Darwin more anti-Darwinian than he needs to be, saddling him with the problematic view that not only emotional expressions did not evolve for communicative purposes (something Darwin did believe), but also that they have not come to play an incidental communicative role in current environments via exaptation (something Darwin – probably – did not believe).

Are Emotional Expressions Voluntary, Involuntary, or Both?

Several commentators have discussed the distinction between voluntary and involuntary emotional expressions. In my article, I argued that emotional expressions can be both, but I did not explain in any detail what it means for a behavior to be voluntary or involuntary. Unfortunately, I won't be able to offer any amount of satisfactory detail here, as the topic is too complicated for a quick treatment. The rough idea I continue to work with is that the central feature of voluntary actions is that they "are not fully determined by the immediate stimulus situation but depend on mental representations of intended goals and anticipated effects" (Goschke, 2013, p. 410). Conversely, involuntary actions are fully determined by the immediate stimulus situation.

Things quickly get more complicated when we try to understand more precisely what role intentions (i.e., representations of intended goals) play in triggering and controlling voluntary actions. This is largely because the nature of intentions is also in contention. It is generally agreed upon that intentions are a species of the genus of *intentional states*, which are mental states endowed with the property of being "about" the world. As Searle (1979) points out, "All intentional states consist of a representative content in a psychological mode" (p. 48). Three examples of intentional states with the same representative content but different psychological modes would be my *intention* that I will go to Paris, my *belief* that I will go to Paris, and my *desire* that I will go to Paris. Their shared representative content is *that I go to Paris*, but the three psychological modes are different. We mark this difference by describing their conditions of satisfaction in a different way: Beliefs can be true or false, desires can be satisfied or unsatisfied, and intentions can be fulfilled or unfulfilled.

Part of my problem with accurately assessing the Behavioral Ecology View with respect to the topic of whether bodily displays are voluntary or involuntary (or both) is that I am not clear at all on how Fridlund understands intentions. It is consequently quite possible that I misrepresented his understanding of voluntariness and involuntariness, for which I am sorry. What confuses me to no end are questions of this sort:

> What does Scarantino mean by "intentions" anyway? Does he mean "intention" psychologically, as, for example, a stated promise or commitment (whether made to others or oneself), or philosophically, as the "aboutness," or functionality, of an act (see Dennett, 1987)? The distinction is crucial, because it is strictly in the latter sense, and only in the latter sense, that I ever used the word in [the Behavioral Ecology View]. (p. 200)

Anyone who works on intentionality would understand this to be a false and deeply baffling dichotomy. As Searle (1979) made clear, intentional states are a combination of a psychological mode and a representative content. The psychological mode distinguishes intentional states from one another. Furthermore, the psychological mode allows intentional states

to have their distinctive causal powers, which is something most people who work on intentional states aim to preserve. For example, intentions are commonly understood as having the ability to trigger and guide actions, and this ability is essentially connected to their distinctive psychological mode (Pacherie, 2006). I am not sure about what Fridlund takes intentions "in the psychological sense" to be, but his notion of intention better have psychological reality, lest the causation of intentional actions remains completely unaccounted for.

To sum up, there is no philosophical room for thinking that intentions are intentional states but have no psychological mode, if one understands what an intentional state is. Fridlund sometimes talks as if intentions were the *only* intentional states, but they clearly are not, which requires that intentions are carefully distinguished from other intentional states by their psychological mode.

To make matters more confusing, Fridlund treats *intentionality* (a.k.a. aboutness or the capacity to represent) and *functionality* as if they were one and the same thing, but they are not. In a book Fridlund often cites but may consider reading again more closely, Dennett (1987) tried to offer a function-based theory of intentionality (a teleosemantic theory), according to which a system represents a property just in case it has the function of carrying natural information about it. But teleosemantics is just one of the many available theories of intentionality, and so we should most definitely not equate the two.

Furthermore, it is simply not the case that "intentionality, in the philosophical sense, is often used to refer to the functionality of evolved morphologies or behaviors" (p. 200). Philosophers ascribe *intentionality* to mental states (intentions, beliefs, desires, etc.) rather than to morphologies or behaviors, and they ascribe *functions* to evolved morphologies or behaviors. On teleosemantic theories of intentionality, the functions of intentions, beliefs, and desires are part of the explanation for why they have aboutness, but no one with a good theoretical grasp of either intentions or functions would ever think that "intention is function" (Fridlund, 1994, p. 146).

Finally, when philosophers say that the "bat's wing is 'intended' for flight," what they mean is that it has the function of flying. Contrary to Fridlund's suggestion, they do not mean that the wing is "about" flying (i.e., that it has intentionality), and they sure do not mean that the bat's wing has the "intention" of flying. Intentions, functions and intentionality are three completely different concepts Fridlund unhelpfully mixes together.

What I *think* Fridlund is trying to say about intentions, once we bracket the shambolic detour onto intentionality and functions, is something I agree with, namely, the idea that "neither humans nor nonhumans have to know what they intend" (Fridlund, 1994, p. 61). This is to say that in some cases agents are aware of the goals of their behavior, and in some cases they are not, because the behavior has been programmed into the agent by natural or cultural selection. In the first case, we can figure out what agents intended to do by asking them, whereas in the latter we have to reconstruct it from observing what their behavior is persistently and flexibly pursuing.

My claim that communicative signals can be "designed" for information-mediated influence "voluntarily by means of individual intentions or involuntarily by means of selective pressures like natural or even cultural selection" is an admittedly clumsy way to make the same point (p. 169). Some of the goals of communicative signals are in the minds of agents, captured by their conscious intentions. Other goals of communicative signals are in the world, so to say, in the sense that they are the end states toward which expressive actions are aimed despite not being represented in the agent's head. If this is what Fridlund also thinks, we can agree at least on this one point.

Other commentators had different worries about the way I draw the distinction between voluntary and involuntary emotional expressions. Fischer points out that the presence of audience effects alone does not entail that the emotional expression is voluntary. She is right about that, and I was sloppy: The immediate stimulus situation to which agents involuntarily respond may include the presence or absence of an audience. On the other hand, she acknowledges that we have other types of evidence for the conclusion that that at least "nonhuman primates have some [voluntary] control over the usage of their vocalizations" (p. 196).

Fischer and Sauter wonder whether involuntary emotional expressions communicate the way voluntary emotional expressions do. For instance, they wonder whether voluntarily produced emotional expressions enable different kinds of $\text{Expressive}_{\text{EE}}$ moves in comparison with involuntarily produced ones. I do not see why there would be any difference: Voluntary and involuntary emotional expressions appear equally able to carry natural information about emotions, and, if they do, they allow for the very same communicative moves and effects. I ultimately concur with Van Kleef that "whether … displays are, at the extremes, automatic and involuntary readouts of internal emotional states or deliberate communicative acts in the service of social motives" does not matter, because both voluntary and involuntary bodily changes "provide a window into what is going on in the expresser's mind" (p. 211).

Does the Analogy Between Emotional Expressions and Speech Acts Create Confusion or Clarification?

Some commentators have voiced concerns about the analogy I am drawing between language and emotional expressions, and wondered whether the analogy does more harm than good. For example, Fischer has stated that "the application of concepts derived for human speech … to communication in a broad array of species creates a number of problems that simpler evolutionary conceptions of communication avoid" (p. 194). She highlights some significant disanalogies between human language and nonhuman communication, concerning, for instance, our superior theory of mind abilities, our higher degree of voluntary control over emotional expressions, our unique sensibility to cultural conventions, our sophisticated ability to feign emotions, and so on. In light of these differences and others, Fischer concludes that that the analogy with language "holds great potential for creating confusion" (p. 195).

Fischer and Sauter also question the ultimate value of the language analogy, emphasizing other relevant differences between language and emotional expressions. For example, the acquisition of language mastery differs developmentally from the acquisition of the ability to express emotions. Whereas

humans first learn to understand speech and then learn to produce it, both humans and animals first learn to produce nonverbal vocalizations, and then learn to understand what they mean. In addition, Fischer and Sauter worry that analogizing language and nonverbal communication while emphasizing the unsurpassed expressive power of the former risks relegating all forms of communication other than language—say, dance—to the role of "poor copies."

I think that the "added confusion" and the "poor copy" objection are worth taking seriously, but they are by no means fatal. The analogy with language creates confusion only if we lose track of the very differences Fischer, on one hand, and Fischer and Sauter, on the other, emphasize. But there is no reason why we should. To say that two things are analogous in one important respect is compatible with saying that they are disanalogous in a dozen other important respects.

If we want to understand how language evolved, there is no real alternative to tracing back the origins of the lofty human abilities for communication to earlier animal abilities that are much less lofty. In fact, I take this to be one of the principal strengths of my account as compared to accounts that go Gricean from the get-go, and find themselves quickly running into a circle when it becomes apparent that the abilities required for Gricean communication—first and foremost our sophisticated theory of mind abilities—are not available until language has evolved (but see Moore's commentary).

Fischer recommends the following "bottom-up" strategy as a remedy to TAP's attempt to "fit a fish with a pair of trousers": (a) investigating "the mechanisms generating the behavior," (b) investigating "the function of the behavior," (c) investigating "what … receivers infer from the behavior about past events, and what about future actions" (p. 195). But this is nothing more and nothing less than what TAP recommends. To wit: TAP focuses on the emotions as *mechanisms* that generate emotional expressions. It tries to understand the communicative *functions* of emotional expressions, understood both as the effects that emotional expressions have that explain why we have them (selected functions) and the beneficial effects they currently have (current functions). Finally, TAP's main focus is precisely on what receivers can *infer* from emotional expressions, both about what the world is like and about future behaviors demanded from recipients and committed to by the signaler.

What TAP offers that previous accounts lack is a systematic framework that allows us to organize the inferences recipients make into a finite set of basic categories—expressions, declarations, demands, and commitments—which happen to closely resemble the basic categories of things we "do" with language. This is what ultimately grounds the analogy between speech acts and emotional expressions. We won't understand how language came to be if we don't pay attention to forms of communication that, despite relying on more primitive mental faculties, managed to foreshadow some of the achievements of linguistic communication.

But am I turning nonverbal communication into a "poor copy" of linguistic communication, as Fischer and Sauter suggest? It is a poor copy in one specific and limited sense: Whereas there are propositional restrictions on what can be expressed, represented, demanded, and committed to without language, such restrictions are lifted once language becomes available. We should not deny that language and language alone allows communicators to "make infinite use of finite means" (Abler, 1989), endowing us with never-before-dreamed communicative powers. As Fridlund points out, you just can't "smile a synecdoche or gesticulate a Grignard reaction diagram" (p. 197) without language.

But there are many things that nonverbal expressions do better than language, which shows why nonverbal communication is not just a "poor copy" of linguistic communication. As the example of dancing and other expressive art forms shows, nonverbal communication is one of our means of access to the ineffable, namely, to what resists being put into propositional form. Nonverbal communication is arguably worse than language at *showing* what language can *say*, but there are plenty of things language *cannot say* that nonverbal communication *can show*.

Furthermore, there is a *communicative strength* to emotional expressions that language on its own lacks, which explains in part why emotional expressions do not just fall to the wayside once language becomes available. Two intuitive sources of strength appear relevant. First, it is harder to credibly fake a nonverbal emotional expression than it is to utter a false sentence. Second, emotional expressions are often accompanied by bodily preparations that vouch for the intensity with which certain representations, commitments, and demands are held. In short, emotional expressions can often communicate more *credibly* and more *intensely* than language unaided by emotions does.

I am far from having seen my way through what makes emotional expressions valuable in themselves and in comparison to language. But I remain convinced that emotional expressions provided our ancestors with basic communicative building blocks that were exploited, developed, and built upon on the path to linguistic communication. I am hopeful that this debate will contribute to the ongoing interdisciplinary conversation on the nature of emotional expressions and their role in language evolution, and I want to express my gratitude once again to my esteemed colleagues for their much appreciated commentaries.

References

Abler, W. L. (1989). On the particulate principle of self-diversifying systems. *Journal of Social and Biological Structures*, *12*, 1–13.

Austin, J. L. (1962). *How to do things with words*. Cambridge, MA: Harvard University Press.

Bar-On, D. (2013). Expressive communication and continuity skepticism. *Journal of Philosophy*, *110*(6), 293–330.

Burkhardt, R. W. (1985). Darwin on animal behavior and evolution. In D. Kohn (Ed.), *The Darwinian heritage* (pp. 327–365). Princeton, NJ: Princeton University Press.

Dennett, D. C. (1987). *The intentional stance*. Cambridge, MA: Bradford/MIT Press.

Dretske, F. (1981). *Knowledge and the flow of information*. Oxford, UK: Blackwell.

Ekman, P. (1997). Expression or communication about emotion. In N. L. Segal, G. E. Weisfeld, & C. C. Weisfeld (Eds.), *Uniting biology and psychology: Integrated perspectives on human development* (pp. 315–338). Washington, DC: APA Press.

Fernández-Dols, J. M. (2017). Natural facial expression: A view from psychological constructionism and pragmatics. In J.-M. Fernández-

Dols & J. Russell (Eds.), *The science of facial expression* (pp. 457–475). Oxford, UK: Oxford University Press.
Fridlund, A. J. (1991). Evolution and facial action in reflex, social motive, and paralanguage. *Biological Psychology*, *32*, 3–100.
Fridlund, A. J. (1992). Darwin's anti-Darwinism in *The expression of the emotions in man and animals*. In K. T. Strongman (ed.), *International review of studies on emotion* (Vol. 2, pp. 117–137). New York, NY: Wiley.
Fridlund, A. J. (1994). *Human facial expression: An evolutionary view*. San Diego, CA: Academic Press.
Garson, J. (2016). *A critical overview of biological functions*. Dordrecht, the Netherlands: Springer.
Godfrey-Smith, P. (1994). A modern history theory of functions. *Nous*, *28*, 344–62.
Goschke, T. (2013). Volition in action: Intentions, control dilemmas and the dynamic regulation of intentional control. In W. Prinz, A. Beisert & A. Herwig (Ed.), *Action science: Foundations of an emerging discipline* (pp. 409–434). Cambridge, MA: MIT Press.
Gould, S. J., & Vrba, E. (1982). Exaptation—A missing term in the science of form. *Paleobiology*, *8*, 4–15.
Grice, P. (1957). Meaning. *Philosophical Review*, *66*(3), 377–388.
Levinson, S. C. (2006). On the human "Interaction Engine". In N. J. Enfield & S. C. Levinson (Eds.), *Roots of human sociality: Culture, cognition and interaction* (pp. 39–69). Oxford, UK: Berg.
Levinson, S. C. (2013). Action formation and ascription. In T. Stivers & J. Sidnell (Eds.), *The handbook of conversation analysis* (pp. 103–130). Malden, MA: Wiley-Blackwell. doi:10.1002/9781118325001.ch6
Marr, D. (1982). *Vision*. New York, NY: Freeman.
Morris, C. W. (1938). Foundations of the theory of signs. In O. Neurath. R. Carnap, & C. W. Morris (Eds.), *International encyclopedia of unified science* (Vol. 1, no. 2, pp. 77–137). Chicago, IL: University of Chicago Press.
Pacherie, E. (2006). Towards a dynamic theory of intentions. In S. Pockett, W. P. Banks, & S. Gallagher (Eds.), *Does consciousness cause behavior? An investigation of the nature of volition* (pp. 145–167). Cambridge, MA: MIT Press.
Russell, J. A. (2003). Core affect and the psychological construction of emotion. *Psychological Review*, *110*, 145–172.
Ryle, G. (1949). *The concept of mind*. London, UK: Hutchinson.
Scarantino, A. (2012). How to define emotions scientifically. *Emotion Review*, *4*(4), 358–368.
Scarantino, A. (2014). The motivational theory of emotions. In D. Jacobson and J. D'Arms (Eds.), *Moral psychology and human agency* (pp. 156–185). Cambridge, UK: Cambridge University Press.
Scarantino, A. (2015). Information as a probabilistic difference maker. *Australasian Journal of Philosophy*, *93*(3), 1–25.
Scarantino, A. (in press). Do emotions cause actions, and if so how? *Emotion Review*.
Searle, J. R. (1969). *Speech acts: An essay in the philosophy of language*. Cambridge, UK: Cambridge University Press.
Searle, J. R. (1979). The intentionality of intention and action. *Inquiry*, *22*, 253–80.
Smith, W. J. (1977). *The behavior of communicating: An ethological approach*. Cambridge, MA: Harvard University Press.
Sperber, D., & Wilson, D. (1986). *Relevance: Communication and cognition*. Oxford, UK: Blackwell.
Wharton, T. (2009). *Pragmatics and nonverbal communication*. Cambridge, MA: Cambridge University Press.