

Commentaries

Signals may have predictive value, but only perceivers predict. Scarantino argues that ‘information talk’ is useful in animal communication, in the particular sense that signals have *predictive value*. We agree with his approach of understanding information as environmental regularity (similar to Scott-Phillips and Kirby’s *indication function*). Importantly, however, that regularity exists independently of perceivers. A conceptual firewall is therefore needed between information as correlations in the world versus observer representations of those correlations. Furthermore, information-as-correlation exists only via the recurrence of events over time, and is not instantiated in any one occurrence. Signals thus have predictive value only to observers that have been previously exposed to the regularity involved – whether developmentally or through phylogeny. When signals are said to contain or transmit information, the firewall is critically breached – environmental regularity becomes conflated with observer knowledge of that regularity. Information talk is therefore harmful – in its current form – because it blurs a critical distinction that it should instead highlight and codify. For Scarantino, true communication occurs when signallers are specialised to influence others specifically through information transmission. Yet, that transmission is metaphorical, relying on notions of information being contained and conveyed. When a signal occurs, perceivers may respond *as if* information transmission has occurred, but such cannot be literally true. The metaphor ‘works’ only because the perceiver already has experience of relationships between signals and events – the correlation does not reside in the signal itself. While Scarantino’s solution is to invoke a metaphorical characterisation, scientific explanation requires that we remain firmly in the material realm. To be useful, information talk should help keep us in that world, rather than separating us from it.

Michael J. Owren and Drew Rendall

Defending an effects-based definition of communication. Scarantino argues that influence-based definitions (IBDs) of animal communication are neither scientifically fruitful nor extensionally adequate. This commentary replies to both criticisms.

The first criticism is that IBDs exclude what is central to communication, namely information transfer. Two points should be made in response. First, the adoption of an IBD does not imply the rejection of

information (Carazo & Font, 2010). The point made by IBDs is only that communication is, at bottom, a matter of effects. Information transfer is a consequence of this. IBDs are not claims that we should remove all talk of communication-as-information-transfer; only that we should be clear that such talk is coherent only if the criteria for an IBD are satisfied. Second, IBDs have informed several theoretical issues in animal signalling and in linguistic pragmatics, and as such are fruitful in their own right. (I briefly discuss these applications in my own contribution to this volume.) The reason it is fruitful is that it captures the functional interdependence of signal and response which lies at the heart of communication (Scott-Phillips *et al.*, 2012).

Scarantino discusses three types of interaction that, he argues, are extensionally problematic for IBDs. As Scarantino notes, IBDs typically include what he calls a *selected response requirement* (SRR) to address such cases. Scarantino shows that this requirement only excludes *happenings* – yet IBDs are typically expressed in terms of behaviours, and hence the SRR fails to achieve its purpose. Scarantino is correct in all of this. However, IBDs are easily saved if they are expressed in terms of happenings rather than behaviours. When IBDs are expressed this way, both coercion and reciprocity fall outside them. (I will not enter into the details of this argument here, for reasons of space.) At least for me, the idea that signals cause happenings has always been the spirit of IBDs; this is why I prefer to talk of *effects* rather than *influence*. However, Scarantino is right to draw attention to the lack of precision in present formulations of IBDs, and the distinction between behaviours and happenings is a useful one. But his comments do not fatally undermine either the utility or the extensional adequacy of IBDs.

Carazo, P. & Font, E. (2010). Putting information back into biological communication. *Journal of Evolutionary Biology* **23**, 661–669.

Scott-Phillips, T. C., Blythe, R., Gardner, A. & West, S. A. (2012). How do communication systems emerge? *Proceedings of the Royal Society of London B*, **279**, 1943–1949.

Thomas C. Scott-Phillips

Can animals know things on Scarantino's account of information? Can the reason animal A chooses to mate with animal B be that signal 's' informs A about B's certain healthful properties if the signal 's' can happen without those properties being possessed by the animal B? On his view this should be possible, but it seems false.

Fred Adams and Steve Beighley

Response

Influence and information are both necessary to define communication

The take-home message of my paper is that any suitable definition of animal communication must include both influence and information in order to be scientifically fruitful and extensionally adequate. In his commentary, **Scott-Phillips** defended influence-based definitions from the charge of inadequacy. He pointed out that endorsing an influence-based definition of animal communication does not entail rejecting information, and that influence-based definitions have led to fruitful discoveries in animal communication and linguistic pragmatics.

I agree, which is why I proposed to preserve the insights of influence-based accounts in a new hybrid definition of animal communication. My charge was addressed to those influence theorists who believe that animal communication should be defined *exclusively* in terms of influence. This group includes Scott-Phillips (2008, p. 388), according to whom “if it is to be discussed at all information should be seen only as an emergent property of communication and certainly not as a defining quality”.

As testified by this volume, many influence theorists still consider the thesis that signals carry information superfluous for definitional purposes, if not problematic in its own right. I replied that definitions of animal communication which fail to treat information as a “defining quality” miss the essential difference between communication and other forms of influence (they are not fruitful) and give the wrong verdict about what is and what is not a signal (they are not extensionally adequate).

In particular, some popular information-free influence-based definitions wrongly count coercion and reciprocation as signals, and wrongly exclude deception from the domain of signals. Scott-Phillips does not address the counterexample of deception, but argues that coercion and reciprocation would not count as signals if we expressed influence-based definitions in terms of *effects* rather than *behaviours*.

I argued that in cases of coercion and reciprocation two types of effects must be distinguished: effects that are happenings (e.g. being pushed, being groomed) and effects that are behaviours (e.g. giving up on a contested resource, grooming back). Scott-Phillips’ proposal is not developed in any detail owing to space limitations, but I understand it roughly as follows. Signals should be re-defined as acts or structures that produce *effects* (rather than mere behaviours) in other organisms, that evolved because of such effects and that are effective because the effects evolved to be affected by the act or structure.

This shift from behaviours to effects, however, would not solve the extensional problem faced by the influence account. It would still be the case that the *behavioural effects* of being pushed by a stronger competitor or being groomed by a conspecific are as likely to have been selected as the pushing and grooming behaviours of the coercer and of the one who initiates reciprocation. If so, coercion and reciprocation continue to satisfy the definition of a signal: they are acts producing effects in recipients (happenings and behaviours), they evolved because of such effects and they are effective because at least some of such effects evolved to be affected by the coercive or reciprocating act.¹

My commentators have also challenged my analysis of information. **Owren and Rendall** have defended the view that signals do not carry predictive information *in the literal sense*, from which it follows that information talk must be *metaphorical*. Since metaphorical claims lack straightforward truth conditions, information talk stands in the way of the scientific understanding of animal communication. I will now explain why my claim that signals carry predictive information is not metaphorical at all.

Owren and Rendall infer that signals do not carry predictive information in the literal sense from the recipient-relativity of predictive value. For instance, whereas a ringing bell has predictive value with respect to a foot shock relative to a fear-conditioned rat, it does not have such predictive value relative to a rat just placed into the conditioning box.

Owren and Rendall interpret this as indicating that predictive information is not *in* the signals themselves. If it were, they assume, any recipient would be able to pick it up. This supposedly distinguishes signals from sentences, in which information is encoded. We should first notice that in order for a sentence to have any informative value for a recipient, such recipient must be familiar with the language in which it is written/spoken. This is to say that the informative value of both linguistic and non-linguistic signals amounts to a dispositional property, namely the property of allowing a suitable recipient to learn something from the signal in suitable circumstances.²

¹ If anything, the shift from behaviours to effect would block a possible line of defence on the part of the influence theorist. Such a theorist may argue that coercion and reciprocation are not signals because the definition of a signal requires that what is affected by a signal is a behaviour, and the effects of coercion and reciprocation are happenings rather than behaviours. Replacing behaviours with effects in the definition of animal communication would effectively block this line of reply. It is a reply worth rejecting anyway, because when considering the proposal that signals are adaptations for influencing another, all evolutionarily relevant aspects of the influence an act or structure has must be taken into account, whether they consist of happenings or behaviours.

² The way we learn from sentences is different from the way we learn from non-linguistic signals in ways I cannot explore in this reply.

This being said, there is an important difference between linguistic and non-linguistic signals. Whereas linguistic signals carry the same encoded information to all recipients familiar with the language, what predictive information a signal carries changes depending on what the recipient already knows. This is a central property of predictive information, but not one that supports the conclusion that signals carry predictive information only in a metaphorical sense.

Rather, it supports the conclusion that signals *literally speaking* carry predictive information in a recipient-dependent fashion. On this view, the information relation is a *three-place relation* between signals, states of affairs and the states of background knowledge of recipients. For example, a ringing bell carries predictive information about a foot shock relative to the background knowledge of a fear-conditioned rat, but it does not carry such information relative to the background knowledge of a rat just introduced into the conditioning box.

When fully spelled out, the predictive information relation takes the following form: X carries information about Y relative to background knowledge k when $P(Y \text{ given } X \ \& \ k) \neq P(Y \text{ given } k)$. This inequality provides a non-metaphorical definition of information, clearly spelling out the conditions under which X carries predictive information about Y relative to k . If $P(Y \text{ given } X \ \& \ k)$ is larger (smaller) than $P(Y \text{ given } k)$, we will say that the signal and what the signal is about are positively (negatively) correlated.

A corollary of this analysis is that, contra Owren and Rendall, no “conceptual firewall” is needed between “correlations in the world” and “observer representations of those correlations”. For purposes of transmission of predictive information, the only correlations that matter are those defined relative to the background knowledge of observers, namely those defined relative to the conditional probabilities $P(Y \text{ given } X \ \& \ k)$ and $P(Y \text{ given } k)$.

A final challenge to my analysis comes from **Adams and Beighley**, who wonder whether “animals can know things on [my] account of information”, and whether “the reason animal A chooses to mate with animal B [can] be that signal ‘ s ’ informs A about B ’s . . . healthful properties . . . if the signal ‘ s ’ can happen without those properties being possessed by the animal B .” My reply is that what animals come to know from receiving predictive information is simply that the probabilities of various states of affairs have changed. This generally falls short of knowing what states of affairs have occurred, so the reception of predictive information does not generate knowledge in the traditional sense.

For example, a signal s may carry predictive information to animal A about animal B being healthy because s – e.g. a courtship call – raises significantly the probability of B being healthy. But receiving this sort of information is *not*

coming to know that B is healthy, and it is compatible with B *not* being healthy. Adams and Beighley find this objectionable, but I consider it an asset of my theory. Following Dretske (1981), Adams and Beighley assume that a single theory of information can reduce knowledge to information and explain the behaviour of creatures in natural environments. My view is that two distinct theories are required to fulfil these two tasks.

This is because what explains why A chooses to mate with B is not that A received knowledge-producing information that B is healthy (real world courtship calls do not conclusively settle such matters) but that he received probabilistic confirmation that B is healthy. So, while I concede that knowledge is not gained from the reception of predictive information, I hold that is the reception of predictive information that explains the behaviour of animals in natural environments.

Dretske, F. (1981). *Knowledge and the Flow of Information*. Cambridge, MA: MIT Press.

Andrea Scarantino

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