

Commentaries

I welcome Rendall and Owren's (RO) criticism of the encoding–decoding model (EDM) of animal communication, but I reject their inference that if information is not encoded in a signal, it is not carried by such signal.

According to EDM, a signaller encodes information by transforming it into an arbitrarily selected equivalent suitable for transmission, and a recipient decodes the signal transmitted by applying the code in reverse to recover the original piece of information.

But senders and receivers of animal signals do not have access to any shared system of rules for transforming pieces of information back and forth into arbitrarily selected equivalents. As RO have argued elsewhere, in competitive contexts a cooperative convergence on a shared code cannot be posited, and the ability to communicate by means of a shared code requires a network of coordinated intentions beyond the cognitive reach of non-human animals.

RO suggest that some endorse a weaker understanding of encoding and decoding, according to which signals can be said to encode information anytime “they stand in predictive relation to some event or state of the world”.

This is the notion of information I discussed in my paper, where I argued that signal X carries *predictive information* about state of affairs Y just in case $P(Y \text{ given } X) \neq P(Y)$. When this is the case, X and Y are said to be statistically correlated.¹ A ‘predictable relationship’ between X and Y is instantiated when their correlation is strong enough to allow X to reliably predict Y (or not- Y).

The encoding and decoding metaphors are positively misleading with respect to predictive information. Saying that alarm calls encode information about predators is like saying that smoke encodes information about fire. This phrasing suggests that some process of rule-based transformation takes place between fire and smoke. We know instead that a straightforward causal relation underwrites the correlation between them.

The same holds for the correlation between predators and alarm calls, except that fire causes smoke directly, whereas predators cause alarm calls by means of signallers. I conclude with RO that the encoding and decoding metaphors are detrimental. But it does not follow that animal signals do not contain or carry information.

RO argue that information “does not reside in the signals themselves but rather in the relationship between them and the events they co-occur with”. These remarks are motivated by an important realisation, but they go too far.

¹ They are positively correlated when $P(Y \text{ given } X) > P(Y)$, and negatively correlated when $P(Y \text{ given } X) < P(Y)$.

The realisation is that what information a signal carries is contingent upon what a recipient can predict from it, which is in turn contingent upon what the recipient already knows. RO correctly point out that a light predicts shock for a fear-conditioned rat, but not for a rat just introduced into the conditioning box. Similarly, a snake alarm call predicts snakes for adult vervet monkeys, but not for infants.

Since different recipients make different predictions (or no predictions) from the same signal, RO conclude that the information is not in the signals themselves. My view is instead that information resides neither in the recipient independently of the signal nor in the signal independently of the recipient. It is in the signal itself by virtue of what a potential recipient can predict upon receiving it. This makes the capacity to carry predictive information fundamentally *relational*: it is a capacity that a signal expresses when paired with the right recipient.

The predictive information relation should then be interpreted as a three-place relation: signal X carries information about Y relative to background knowledge k by virtue of a capacity of X to allow a potential recipient in background knowledge k to predict Y or not- Y from the signal.²

Encodable information (non-natural meaning) and predictive information (natural meaning) differ in several respects (Grice, 1957; Scarantino & Piccinini, 2010). The one I wish to emphasise in conclusion is that whereas it is up to senders what encoded information recipients receive, and all recipients decode the same information from the same signal, recipients can pick up predictive information that senders never intended to provide, and the same signal carries different predictive information to different recipients.

Scarantino, A. & Piccinini, G. (2010). Information without truth. *Metaphilosophy*, **41**, 313–330.

Andrea Scarantino

There is much to agree with in Rendall and Owren's (RO) chapter. Their emphasis on influence clearly accords with my view that communication is, by definition, a matter of effects rather than information (Scott-Phillips, 2008; Scott-Phillips & Kirby, Chapter 18 of this volume). Moreover, I agree with their demands that animal signalling theory should be oriented around functional benefits. However, I do not see why the reorganisation they call for is based only around the benefits

² This entails that the probability functions $P(Y \text{ given } X)$ and $P(Y)$ determining whether or not X carries information about Y must reflect *all* the background knowledge relevant to the context of inquiry (e.g. inquiry into the behaviour of a fear-conditioned rat, or of an adult vervet monkey).

that signalling has for signallers. (RO do acknowledge that signals can benefit receivers, but they do not see it as a requirement.)

In the conclusion, RO appeal to a return to the basic principle of natural selection that adaptations are shaped by the benefits they provide to their bearers. Yet communication is the emergent consequence of the interaction of two adaptations, one for signalling and one for receiving (Scott-Phillips *et al.*, 2012). So while RO are correct that we should not assume that “selection on receiver resistance trumps selection on signaller influence”, neither should we do the opposite. As a result, any reorganisation of animal signalling theory should be based around the benefits of influence to both signallers (the influencers) and receivers (the influenced).

I note that RO pick up on the same issue in their commentary on my co-authored chapter in this volume. I expand on these issues in my response.

Scott-Phillips, T. C. (2008). Defining biological communication. *Journal of Evolutionary Biology* **21**(2), 387–395.

Scott-Phillips, T. C., Blythe, R. A., 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

There is little to disagree with in this chapter, except the vehemence with which Rendall and Owren deride informational accounts. Perhaps we are mellow because the excesses of the cognitive revolution did not railroad work on birds. True, it is “difficult to overstate the influence of the vervet alarm-call research”, but it seems that Rendall and Owren have done just that.

Rendall and Owren seem to be advocating the very approach we advocate, yet we find informational constructs interesting and useful, while they find them unrealistic and misleading. Given that we are so in tune with their main message, we suggest that on the latter points we agree to disagree. After all, wasn't it Smith (1977), the whipping boy of anti-information approaches, whose career stressed the importance of context?

Andrew Horn and Peter McGregor

Response

Informational constructs in animal communication theory. Are we ready to cut the apron-strings? We appreciate commentaries by **Horn and McGregor**, **Scarantino** and **Scott-Phillips**. We are delighted they express general agreement with the thrust of our critique, namely that many language-based

metaphors and related constructs arising from the cognitive revolution (e.g. signal encoding and decoding), are generally not apt characterisations of animal signalling phenomena, and that other common constructs, such as the concept of information, deserve additional scrutiny. That consensus is encouraging and suggests progress can be made on updating and clarifying the core theoretical constructs that undergird and motivate future research in the field. Of course, complete consensus may take time and be difficult to achieve. Commentators expressed general agreement with us, but they did not endorse everything in our critique and highlighted some specific points of divergence. We wholly respect these differences and have no desire to discount or dismiss them with further arguments. However, in the interests of continuing the discussion and motivating additional opportunities for it, we attempt some response.

Signallers versus receivers. **Scott-Phillips** agrees that animal communication is not about *information* but about *influence*. However, he is concerned that our characterisation places too much emphasis on signallers and not enough on receivers. He emphasises that animal signalling theory “should be based around the benefits of influence to both signallers (the influencers) and receivers (the influenced)”. We wholly agree that receivers (perceivers) are as important as signallers. Indeed, we (like others, e.g. Owings & Morton, 1988) have emphasised the inherent bidirectionality of communication, “...signallers and perceivers play important and potentially distinct roles in an iterated, reciprocal process of influence and resistance”. However, we do certainly acknowledge that our arguments involve foregrounding the role of signallers in as much as it is evolutionarily axiomatic that signals must ultimately be beneficial to them, or else they would not signal. We feel obliged to reiterate this basic point because it is sometimes overlooked in theorising that champions the principle of signal honesty. We agree receivers should be (and are) inherently skeptical, but we do not accept that they can simply ignore anything but honest signals, thereby forcing signallers to be honest. That would be a very one-sided view of signalling indeed, and one that is, as argued elsewhere, committed to an informational and cooperative view of communication we feel is inappropriate. It is also contradicted by diverse evidence that perceivers are not free to discount signals but instead, owing to a variety of mechanistic and functional factors, can be quite susceptible to signal influence. At bottom, signalling must benefit signallers but need not benefit receivers, and being clear about that is the basis for any special emphasis we place on signallers. Otherwise, we are in full agreement with Scott-Phillips that signallers and perceivers are equally important foci in animal communication theory. It is a further question whether or not signalling necessarily involves perceiver adaptation, which is taken up in commentary accompanying Scott-Phillips’ chapter.

What does information add? Scarantino agrees that signals do not encode information and that talk of signal encoding and decoding is misleading. However, he feels we go too far in rejecting informational constructs entirely. He maintains that signals that do not *encode information* can nevertheless be said to *carry information* in so far as they bear a predictive relationship to events or actions. Thus, the vervet monkey's leopard alarm can be said to carry information about that predator because it reliably occurs in the presence of a leopard. This basic association defines an indexical relationship in C. S. Peirce's classic typology of signs, wherein an index (distinct from either icon or symbol) is a sign that denotes by virtue of its spatio-temporal association with the thing denoted. Using Scarantino's example, smoke can be a signal: it can index (or point to) fire because it is predictably associated with fire. We agree. Smoke indicates the presence of a fire, and vervet alarm calls indicate the presence of a predator. Of course, an important corollary then is that vervet alarms, like smoke, are not at all like language. In language, signs (words) denote specifically *not* by spatio-temporal associations with their designate, but via symbolic relationships to them that are established by convention. As Scarantino, we, and others in the volume emphasise, the latter requires a shared code and "coordinated intentions beyond the cognitive reach of non-human animals". So, we agree entirely that vervet alarm calls, and many other animal signals, qualify as indexes that point to, or indicate, things in the world but do not symbolise them. They are, therefore, quite unlike language.

What we do not fully understand is what is added by re-describing well-defined indexical relationships as 'carrying information'. Specifically, what is gained by parsing the relationship between vervet alarms and leopards as "vervet alarms *carry information* about leopards"? We understand the sense of information Scarantino intends, but do not see the benefit provided by an informational characterisation of the straightforward indexical relationship. Although harmless enough on its own, this kind of informational re-description can also actually be detrimental in encouraging further linguistic and informational characterisations (e.g. signal encoding and decoding, intentionality, representation). In fact, Scarantino seems to agree that such characterisations are unfounded, misleading and draw attention away from more fundamental ethological and evolutionary questions.

To wit, it is more than 30 years since the original description of language-like, symbolic signalling in vervet alarm calls. What we now know is that, lacking the fundamental intentional and representational properties of words, these alarm signals are, in fact, not language-like or symbolic. Revealingly, we do not yet know much about how the alarm-call system does work. For example, we do not know what the selected, adaptive function of the calls is. Is it to warn kin? Is it to

signal to predators that they have been detected? Similarly, we do not know what the proximate mechanisms are that motivate production of alarm calls, nor what the mechanisms are that determine listener responses. These are common and basic features of animal communication systems that should be central in ethological research but that seem to have been cast aside.

We suggest that these details are missing for the alarm call system of vervets (and for many other cases) because investigators have been preoccupied with trying to characterise signalling systems in linguistic or informational terms. As argued, these characterisations are fundamentally out-of-step with basic tenets of ethology and evolutionary biology, and, in the end, have proven misleading. But there is an even greater potential risk associated with informational characterisations: namely that, because of the natural familiarity and intuitive appeal of linguistic and informational constructs, applying them to animal signalling can create an unjustified impression of having explained the core phenomena involved. In reality, and as just noted, the resulting explanations are not substantial. They leave fundamental holes in our understanding of animal signalling, as illustrated by the much-studied and celebrated phenomenon of vervet alarm calling.

Is the case against linguistic and informational constructs overstated? **Horn and McGregor** see general agreement between our respective approaches, but also wonder if the excesses of the cognitive revolution are being overstated. Specifically, while the now familiar constructs on linguistics and information encoding may have infected research on primate communication, they have not had such an influence on bird research. Thus, while acknowledging our concerns, Horn and McGregor are reluctant to abandon informational constructs and still find them 'interesting and useful'. We accept their suggestion that, on this, we might simply 'agree to disagree'. But, prompted to respond, we have to wonder whether their reluctance to abandon informational constructs is not, in fact, evidence of exactly the kind of pervasive influence we propose them to have had?

Drew Rendall and Michael Owren