

Reply to Kensy Cooperrider*

Tim Wharton

Department of English Language and Communication

Kingston University

t.wharton@kingston.ac.uk

Bridge building is not easy. Finding people from neighbouring disciplines who are prepared to consider questions from unfamiliar theoretical perspectives is hard enough; finding people who are prepared to look at them seriously is even harder. In that regard I am hugely grateful to Kensy Cooperrider (KC), not only for reading my book *Pragmatics and Non-Verbal Communication* but also engaging with it in the way he has, and writing the perceptive review to which this is a reply. It's true that there are sections of his review where I think he may be mistaken, and I mention a few of these below. But the following reply is not about setting the record straight. Rather, it is about trying to encourage debate between our two disciplines and initiate a mutually beneficial dialogue.

This reply falls into two parts: the first deals with two specific quibbles that I think are worth mentioning since they are fundamental to the main thesis of the book; the second deals with a couple of global issues that are relevant to the debate.

In Chapter Two of the book, I introduce the notion of 'deliberately and openly showing' facial expressions, a crucial part of my account. KC asks:

What does it mean exactly to *show* someone that you are smiling? (p. 82)

Let me suggest the following. We are all, I think, happy with the notion of a smile being concealed or suppressed. Many facial expressions can be, though there also exist phenomena that betray our internal state – such as sweating and blushing – which can't. Those behaviours aside, I don't think it is an unrealistic step to suggest that those facial expressions that we are capable of concealing or suppressing we are also capable of *not concealing or suppressing*. As I discuss in the book, there is interesting research which suggests that smiling is prompted by the presence of an audience anyway (much as crying among children is prompted by the presence of a carer). My claim, then, is that making no attempt to conceal or suppress a spontaneously-produced behaviour in circumstances where it is obvious to both communicator and audience that she could have taken steps to conceal or suppress it, amounts to deliberately showing it.

Does anything, I hear you ask, ride on that? Well, if we were talking about the facial expressions of vervet monkeys, the answer would probably be no. But in the human case I think it does, and so do many people working in pragmatics. (I expand on the reasons why in my book.) Humans seem to be highly sensitive to behaviours interpreted as 'deliberate and openly shown'. Relevance theory (Sperber and Wilson 1985/1996) is built around a Communicative Principle, which suggests that any behaviour that is 'shown' in this way (i.e. is 'ostensive') come with an expectation

* I am extremely grateful to Kensy Cooperrider for insightful comments on an earlier version of this reply.

that it is worth paying attention to. Together with the Cognitive Principle (itself independently endorsed by cognitive scientists), it forms the cornerstone of all relevance-theoretic analyses of communicative acts. In Sperber and Wilson's words (1997, p. 151):

Because humans have such predictive purchase, they can act on each other's mental lives. They do this by manipulating each other's expectations of relevance.

KC wonders whether those researching gesture should bother with pragmatic concerns such as these. If pragmatics were the study of crop-circles, I'd suggest not. But pragmatics is the study of the interpretation of utterances, with all their linguistic and non-linguistic properties. Researchers into gesture should no more ignore pragmatics than those working in pragmatics should ignore the study of gesture. The fact that most people in pragmatics do ignore gesture is not to their credit.

Moving on, KC raises as a serious issue the whole notion of the 'naturalness' of certain behaviours, another important part of my account. He asks:

Is pointing natural? What about headshakes, or palm-up gestures? (p. 81)

It is true that there are behaviours that are hard to pin down as either non-natural or non-natural, but pointing is not among them. Understanding an act of pointing depends entirely on attending to what the pointer is pointing at. Imagine a person is pointing at a bottle of wine in a shop window. How do you know what they are pointing at? Is it the bottle, the label on the bottle, the liquid in the bottle? Is it the size of the bottle, the price of the bottle, the fact that it is the same wine they drunk at that restaurant the night before? Joint attention is achieved when minds meet – i.e. when the audience infers the intention of the person pointing: it therefore *can't* be natural.

Headshakes seem to me to be different. There is a *coded* element to culture-dependent headshakes that is barely present in acts of pointing (although there is a little culture-variation in what part of the body people point with). Headshakes, according to the distinctions made in my book, are cultural codes: these, then, are also non-natural rather than natural.¹

As far as palm-up gestures are concerned, I have to confess that before writing this reply I had no idea what they were. I can now add a very basic knowledge of palm-up gestures to the list of things I have learned since I read KC's review.

There are more quibbles, but in the interest of sticking to the brief I outlined in my introduction, let me turn to two, more global points. Both of these are in response to the following short quote from near the end of KC's review. He writes:
I remain skeptical that the analytic framework he (TW) adopts — which leans heavily on the folk-theoretic notion of intention — and the method he employs — with exclusive reliance on impression — will get us very far in understanding non-verbal communication. (p. 87)

¹ KC has since pointed out to me Kendon's (2002) work in which: (a) it is shown that headshakes partner with discourse in more subtle ways than I have realized; and (b) it is claimed that from the fact that headshakes emerge early in development, and appear to be cross-culturally widespread, they may present an interesting fusion the natural and the non-natural.

The 'exclusive reliance on impression' is something to which KC refers on several occasions. My use of 'made-up' examples and 'intuition' are presented as shortcomings (albeit with a generosity typical of the review), and I am accused of paying *no* attention to the empirical findings of others. I think this last point is a little unfair, but the question of the reliability of introspection as evidence is a serious one, and one that often comes up in my field, linguistics. It's a serious point, and there is a debate worth having. For the clear implication in KC's comments is that intuitive evidence from introspection is not empirical. But why?

At a conference I recently attended, one delegate told the audience (during a friendly post-talk debate, after I had pointed out that the results she had found did not fit with my own native-speaker intuitions) that this disagreement was an example of precisely why, as researchers, native speaker intuitions are not to be trusted. Reliable conclusions, she concluded, are only to be drawn on the basis of 'hard data'. There was plenty of such data in the talk that followed our exchange, a wealth of facts, presented with all manner of complex and colourful graphics, flying in and out more in the manner of a Pixar feature than an academic presentation. But what interested me was how this hard data had been obtained. You see, it had all been obtained by that old psycholinguistic technique of *asking native-speaker subjects to 'rate' or 'judge' the well-formedness of sentences*.

Why, I wondered at the time, and still wonder, do the intuitions such as these constitute hard data and mine not? And all sciences, including hard sciences such as physics, make significant use of introspection in form thought experiments. The claim made by philosophers of science that such experiments are not empirical because they contain no data (Duhem 1914) has been widely challenged (Koyre 1939, Kuhn 1964). Nersessian (1992) proposes that thought experiments are a form of mental model-based simulation.

Theoretical predictions from relevance theory concerning the normal child's meta-communicative development, which I discuss in the book, and the problems that arise in *verbal* comprehension for those with impaired mind-reading ability, have been borne out by psychological research (Leslie and Happé 1989, Happé 1993, see Wilson 2000 for discussion). Many other aspects of relevance theory have also been experimentally tested (see e.g. Sperber, Cara and Girotto 1995; van der Henst, Sperber and Politzer 2002; van der Henst, Carles and Sperber 2002). Chevallier et al. (2009) is a paper largely informed by relevance-theoretic claims on the interpretation of prosody in people with Asperger's Syndrome.

Of course, there is a sense in which KC is right in the quote above. A 'theory' that makes no predictions, or makes predictions that cannot be empirically tested, is not really a theory. But the book does make concrete claims, and does include empirically testable hypotheses. With colleagues from Kingston University, Middlesex University and University College London, I am currently in the process of setting up a large scale project which tests claims made in the book with particular relation to the interpretation of prosody in typical and atypical speaker/hearers.

All of which brings me on to the other part of KC's quote I would like to comment on: the 'folk-theoretic' (p. 86) notion of intention.

Since the work of Grice and his predecessors (KC implies Grice is tedious – the very thought!), many people working in philosophy and pragmatics take seriously the claim that human linguistic communication (which includes non-verbal behaviours) is massively inferential, and much more than a coding-decoding process. What is it that hearers infer in acts of communication? They infer speakers' intended meanings. What are speakers' intended meanings? They are intentions.² But implicit in the remark that the whole notion of intention is simply a 'folk-theoretic' concept is that they are nebulous, flimsy phenomena not really worthy of addressing. I would be the first to admit we know very little about such things as the neural correlates of intentions but ignoring them does not seem to me to be the answer. We can't see *concepts*, but that doesn't mean we can't develop theories of word meaning. We can't see thoughts at all, but does that mean we shouldn't talk about them?

I stand by my claim that in the study of non-verbal communication the role of intentions takes a very secondary role. In his account of facial expression Alan Fridlund (1994, p. 146) abstracts away from it entirely:

I have circumvented these 'levels of intentionality' issues in the interests of space, and use intentionality in a purely functionalist sense.

Adam Kendon (2004, p. 15) himself writes:

[T]he judgement of an action's intentionality is a matter of how it appears to others and not a matter of some *mysterious process by which the intention or intentions themselves that may guide the action may be known*. (My emphasis, TW)

But the aim of pragmatics is surely to *demystify*. A spoken utterance is typically a composite of linguistic signals, which interact in complex ways to yield a hypothesis about the speaker's meaning. Only by engaging with these 'mysterious processes' can we hope to better understand the intricate, interwoven relationship between the words we say, *how* we say the words we say, the kinesic movements we make when we say the words we say and how all these factors contribute to our meanings.

Notwithstanding all of which, I would like to take this opportunity to thank KC for his hard work both in reading my book and engaging with what I'm sure was an unfamiliar perspective so totally, and also Adam Kendon for permitting me to write this brief reply. To both of them I smile deliberately and openly and nod in their general direction, safe in the knowledge they will successfully infer the intentions behind both behaviours.

References:

² For several interesting responses to the book KC references in his review – Suchman (1987) – see Cohen, Morgan and Pollack 1990; for another, more recent, development on the neural bases of intention see Desmerget and Sirigu 2009).

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