

*The Origins of Meaning*, by JAMES R. HURFORD, OXFORD, OXFORD UNIVERSITY PRESS 2007, xiii + 388 pp.

The origins of language are currently the focus of some of the most interesting lines of research in linguistics and anthropology. The individual who has undoubtedly had most influence in this area – through his own research, as the editor of this series from OUP, and as the organizer of regular conferences (evolang) – is Hurford, a professor of linguistics at Edinburgh University. In 2007, he published the first of two volumes which set out to study *Language in the Light of Evolution*. This first volume focuses on the origins of the semantics and pragmatics of language, as part of an undertaking to consider the origins of the four aspects of language which have given rise to the four sub-disciplines of linguistics: semantics, pragmatics, phonology and syntax. As he points out [p. xi], this is an innovative approach, as other authors have tended to address only one of these aspects, or at most two. This attempt to cover the entire field systematically should be applauded, as it makes it far more difficult to skirt around problems, and it commits the author from the outset to a very demanding endeavour. However, I believe that there is also a risk inherent in this structure and this relates to my main criticism of Hurford: that the intrinsic link between semantics and syntax in language – or, to put it another way, the fact that each linguistic meaning is attended to in itself before combining with other meanings – may condition the view of prelinguistic semantics too heavily.

But let us follow the book step by step. Hurford argues that quite complex thought arises before language. When studying the origin of semantics he therefore takes animal thought as his starting point. I believe that this is both a brave and a sensible decision. As he argues, “the first hurdle to overcome is to grant that what is private is nevertheless real and can be intruded upon by science” [p. 51]. Although we have in principle been living in an era of cognitivism for many years, the criticisms of Wittgenstein and the behaviourists of efforts to explore the private realm continue to be influential, and this gives rise to a reluctant scepticism towards any attempt to study the animal mind. Animals don’t talk, and current neurophysiological knowledge is not such as to be able to explain how the mass of cells which make up the brain could give rise to awareness. Given this state of affairs, it is often felt that it is best simply to ignore this issue and to adopt the ‘intentional stance’ towards animals. This strategy, applied equally to machines and to animals, is certainly risk-free, but this is precisely because it avoids saying anything of note: this would be my answer to anyone who finds Hurford’s decision and his consistent Darwinism too hazardous. Although, under the auspices of cognitive ethology, there have been misleadingly anthropomorphic interpretations which might leave us yearning for behaviourist self-restraint, this should not be a reason for giving up altogether, but rather for exercising greater cau-

tion. I therefore strongly agree with Hurford when he states that, “While the use of coherent language is the strongest indicator of awareness, there can be awareness without language,” [p. 33] and I also endorse his decision to investigate animal consciousness.

Hurford’s strategy is to align our mental states with our behaviour and with the data which we can collect with regard to our brain at that moment in time. This triad functions as a point of comparison with an analogous triad for animals. Indeed, he argues that we are able to study the brains and behaviour of animals more closely than those of humans, and that we therefore have the opportunity of making accurate inferences regarding animal mental states [p. 51]. But Hurford does not forget that the difficulties are huge, and comments, “Presumably cats, who sometimes chase wind-blown leaves, have a different conception (of biological motion), one that is less adaptive, because they don’t eat leaves. But maybe cats are just playing” [p. 53, note 20]. I suspect that when cats encounter their prey, they have a biochemical state which prepares them for the fight, and it might be worth carrying out a comparative study of the biochemical states of cats chasing a mouse and cats chasing leaves to solve this problem. But there are bigger obstacles than this.

Under the heading ‘Object permanence and displaced reference’ (section 3 of Chapter 2) Hurford, focusing on the two Piagetian tasks of visible and invisible displacement, recalls the subtle difference which Watson *et al.* identified between the success of dogs and of children in the task of invisible displacement, and he stresses the affinity between the issue of object permanence and that of episodic memory. While the researchers who study these two types of displacement, whether in children or in animals, do not tend to make much reference to episodic memory, there is clearly some convergence between the capacity to remember a specific event and the capacity to remember the location of a hidden object. In his refusal to be blinkered by traditional distinctions between categories, then, Hurford is offering a valuable insight; indeed, this whole section is excellent. However, both here and in Chapter 3, when he discusses episodic (or evocative) memory, the discussion would benefit if we asked whether animal memory achievements imply an ability to evoke absent objects or, on the contrary, if the notion of ‘expectation plus recognition’ is sufficient to explain these achievements.

In Chapter 3 we learn that jays, for example, remember not only where and when they have hidden food, but also whether this food is more or less perishable. Likewise, a chimpanzee is capable of recalling hiding places for over twelve hours. Hurford has some doubts about the degree of ‘uncertainty-monitoring’ or metacognitive ability involved (an issue already discussed in Chapter 2.2). By contrast, and this is what interests me, he states categorically that these animal feats cannot be based on mere recognition memory. However, let us see what Hurford means by ‘recognition’: “[The chimpanzee] was not shown sets of alternatives, from which she was supposed to choose. Thus, an

important criterion, recall rather than recognition, was met.” [p. 72]. Here, we see a recognition which is not complemented by the expectations which give rise to the search (the animal must be shown a set of alternatives). I would argue that this passive ‘recognition memory’ should be contrasted with a concept of recognition guided by the ‘teacher mechanism’ constituted by expectations, both innate and learnt.

One aspect given great attention within this chapter is the question of whether or not animal memory is restricted to a single domain: that of searching for food. Does the animal only remember those features which it can relate to this domain? While Hurford is very careful when outlining how this could be tested, in my opinion he disregards the crucial point in the issue of episodic memory. As a result, he treats as nothing more than a question of degree the difference between animal memory skills and ‘mental time travel’. According to Hurford, the disagreement between Clayton, the researcher who demonstrated these skills, and Suddendorf, whose work has focused on ‘mental time travel’, is terminological rather than substantive [p. 78]. However, I believe not only that there is indeed a very significant difference, but also that this is of great relevance to the question of the origins of language. I would also take issue with the following statement: “If an animal perceiving an event were capable of episodic memory, of bringing this event to mind at a later time (a big ‘if’), its brain would be in some similar state when recalling that event” [p. 96]. Here, Hurford appears not to recognize (or at least misses the opportunity to explain) the great complexity involved in ‘bringing a past event to mind’. If we are to note the similarity between the mental state of perceiving an event and that of reliving it in one’s memory, we should also recognize the difference. The past episode must be marked as past, and the memory of that episode must occur at the same time as the individual is perceiving the current surroundings. The key question is which brains are capable of this simultaneous, dual awareness. At this point I should observe that, in my opinion, the book –the whole book– would have benefited from a more detailed consideration of the issue of child development. Consider, for example, the lateness with which children master displaced speech, or how, in the universal human activity of symbolic play, in order to evoke the horse which it has seen, the child uses its muscles to reproduce the motor pattern of horseriding. Had Hurford taken this into account, perhaps he would have hesitated more before attributing to animals the ability to evoke absent objects.

And now let us move on to another issue. As we would have imagined given Hurford’s research record, the issue in animal thought to which he dedicates most space is that of “proto-propositions”. He starts his book by arguing, and I agree, that animals possess concepts (including their ‘affordances’ or ‘assimilation’ schemes, pp. 60–62, and perhaps also their proximity to essentialism, p. 86) even if these concepts do not necessarily fully coincide with our

own. The question which immediately arises is how these concepts operate within animal perception.

Hurford seeks to discover in perception a germinal, prelinguistic form of syntactic compositionality. Notwithstanding, he refuses to take the easy option. Aware that the isomorphism between perception and language invoked by defenders of *mentalese* is an assumption which is not supported by any empirical data, Hurford wisely rejects the idea that the ‘agent/action’ dichotomy, so common in the syntax of the majority of human languages, figures in prelinguistic thought. Instead, he agrees with Bickerton’s argument that, “language deceives us here. Instead of holistic descriptions, ‘bird-flying’, ‘cow-chewing’, and so on, we are obliged to separate an actor or topic from an action, an event, or a state – ‘The bird flew’, ‘The cows chew’, and so on” [p. 34]. He also goes on to agree with Carstairs-McCarthy [p. 156], who has realized that the traditional distinction between truth and reference is nothing more than a reflection of linguistic syntax. (Sentences are what can aspire to be true, while it is names which make, or fail to make references; however, if we concentrate on knowledge separated from language, then there is no difference between what is expressed by a sentence and that which is expressed by a name.) In summary, Hurford rejects all the traditional ways of translating syntax into prelinguistic perception. Despite this, he continues to search for the origin of the propositional scheme in perception. To do this he summarizes his work from 2003, and has recourse to the notion of two separate neural pathways (the ‘what stream’ and the ‘where stream’) distinguished by Milner and Goodale. Visual perception arises from combining the ‘what’ and the ‘where’; that is, the information about the features which make it possible to categorize an object, and that which “simply identifies the location of an object in the space around the animal” [pp. 99-100].

Before moving on to question the assumed equivalence between these two elements and prelinguistic syntactic compositionality, I would like to stress the importance of the ‘where’ information. Indeed, I would argue that it is precisely in ‘distal localization’ that the great achievement of animal perception lies. Sensory data are always inside the animal’s skin, so to speak. Despite this, animals with brains perceive these data as distal stimuli, as being located at a particular distance. This is not the case with plant tropisms, or even with insect vision. (It is true that insects move towards the relevant stimulus as the sensory ganglion activates the appropriate motor ganglion. However, there is no need to assume that insects are supplied with any distal information by their compound eyes, and indeed this information is not necessary to them. A lot of distant flowers are equal to a few flowers close by. The balance between cost and benefit is identical.) By contrast, animals with brains manage to calculate the distance at which the stimulus is located and, given that distance is always relative to a centre of reference, an animal’s awareness of itself surely arises jointly with the perception of distance. For

this distance to be detected there must be some degree of awareness of the body, even if at the beginning this is only as the centre of reference. It is possible for the animal to perceive external objects because it has some kind of awareness of what is not external – that is, an awareness of itself – and, by the same token, the animal has an awareness of itself because it perceives what is exterior to it. In conclusion, I could not agree more with Hurford when he argues that the ‘distal (egocentric) location’ of the object is not a property of the object, and is far more than just one of many features. It should be noted that Hurford does not base this argument solely on the differentiation between cerebral areas for ‘what’ and ‘where’. He also notes that “the majority of philosophers have concluded that the existence of an object is not one of its properties”, and invokes the “etymological connection observed in many languages between existence and spatial location, as with English ‘There is’” [p. 103].

But let us now turn our attention to Hurford’s own endeavour. He proposes that prelinguistic syntax is to be found in the perceptual integration of ‘where’ and ‘what’. On this basis, all visual perception would involve a predication, however much this may differ from the traditional notion of what a predication is. I would explain Hurford’s reasons for elaborating this proposition as follows. He is searching for the origins of syntax and, as the antecedents of syntax cannot, of course, lie in yet more linguistic syntax, Hurford therefore concludes that we should not search for those antecedents in language. As a result, he decides to look for prelinguistic syntax in animal thought or perception. I would reject both (negative and positive) conclusions here. In response to the first, while it is clear that the search for the origins of syntax must take us beyond the boundaries of syntactic language, this does not necessarily mean that we must look outside of all language, and it may well be sufficient to focus on non-syntactic communicative protolanguage.

But let us move on to the second point, which is the one which is of real interest here: that prelinguistic syntax is to be found in animal perception. It is my opinion that syntactic compositionality is very different from any perceptive (subpersonal) compositionality. Syntax implies a composition where elements must be attended to separately. By contrast, in perception it is extremely doubtful whether we can talk of elements being attended to separately. Hurford, because he has referred to ‘what’ and ‘where’ (and not to ‘agent / action’), is able to present evidence in favour of the separability of these two types of information. There are occasions when an as yet indeterminate stimulus is perceived; that is, one which has not yet been categorized by the cerebral area concerned with ‘the what’. On these occasions, however, there is one very clearly determined element of the perception; the location of the as yet indeterminate object has been detected clearly. If this were not the case, the subject would not be able to immediately direct its gaze straight at the object as in fact it always does in such cases. We have, then, a ‘where’

without a 'what'. Is this sufficient to sustain Hurford's proposal? In my opinion, this would only be sufficient if we accept that this first perception, of a 'where' without a 'what', remains identical within the following perception, that is, within the perception in which the object is not only located but also categorized. In this case we would have a 'where', attended to separately, and forming part of a perception. We would then have a perceptive compositionality where the elements are attended to separately. However, the key question is that of whether we accept that a perception which has already been superseded can nevertheless be maintained. It seems to me that there is evidence against this or, to put it another way, there is evidence in favour of a constant and never-ending updating of the perception. Of course, the information obtained previously will continue to be used. This happens, for example, when a general focus is followed by a focus on a reduced domain. The perception of the smaller area is accompanied by clearly defined expectations with regard to what will be found if perception is extended in each direction. However, the old perception as such no longer exists, and the same thing would occur in the case we are interested in. In the second perception, the 'what' and the 'where' are both present but neither of them receives separate attention.

Let us explore this whole issue further. If I reject Hurford's proposal regarding the origins of syntax, this is above all because he looks for this origin in thought and not in communication. Of course, once linguistic communication begins, thoughts will increasingly come to reflect the influence of language: in the words of Vygotsky, superior processes, which are originated interpersonally, will become intrapersonal. However, I would argue that both the initial impetus and the whole original structure of the syntactic process came exclusively from pragmatics.

To consider this issue more closely, let us look at Hurford's discussion of the philosophical dichotomy between analytical and synthetic sentences, and the psychological dichotomy between semantic memory (or memory of stable associations) and memory of singular episodes [pp. 85-86]. Traditional philosophy did not in principle seek, through the concept of analytical sentences, to do anything more than delimit knowledge of immutable essences. However, Hurford links the two dichotomies; the correspondence which he proposes is that analytical sentences correspond to semantic memory, while synthetic sentences (or to be more precise, the 'new content' which appears in them) correspond to the memory of singular episodes. But he immediately admits that "this parallelism is imperfect". On the one hand, he recognizes that the semantic memory of one's own house could in no way be expressed in an analytical sentence. And on the other hand, when he wishes to explain the novelty of the predicate of synthetic sentences, he falls back on interpersonality, arguing that in synthetic sentences, "we tell people about events that we know happened, but that they don't know about".

In my view, the failure of this parallelism between dichotomies reveals the wider failure of any attempt to locate the basis of linguistic syntax in the thought – in the beliefs – of any one individual. (Note: I have already expressed my criticisms of the attempt to locate prelinguistic syntax in the perception or composition consisting of ‘what’ and ‘where’. What I am rejecting here is the idea – signalled but not defended by Hurford – that ‘the new’ in synthetic sentences – or, to put it another way, in sentences which are suited to communication – can be explained through a link with ‘memory of singular episodes’.) In short, if animal thought about the world cannot form the basis of syntax, this is not, in my opinion, because the concepts of animals are inadequate. Far from believing that “all this may sound like dangerous anthropomorphism, thrusting man-made logical categories onto poor dumb beasts” [p. 99], I agree with Hurford as to the proximity which exists in this regard between human and non-human primates. The key to this incapacity to generate syntax lies, instead, in the fact that animal thought does not have access to other minds. Only by detecting the thoughts of others – and identifying these as false, insufficient or out of date – do we have the possibility of a truly synthetic sentence and of a predicate which the speaker considers new: not for the speaker himself, which would be impossible, but for the listener. Thus, my main criticism would be that Hurford has incorrectly syntacticised prelinguistic or precommunicative meaning. But we do not know what he will say about the origins of syntax in the second volume.

The second part of the book is dedicated to pragmatics, and addresses a number of interesting issues. However, I will focus solely on the question with which Hurford appears to be most closely concerned: “From the viewpoint of Darwinian theory, which emphasizes selection of traits benefiting individuals, it is a *prima facie* puzzle why any creature should help another” [p. 254]. Should we accept the controversial ‘group selection’ theory, defended by Sober and Wilson, or should we follow Dessalles in arguing that relevant speech offers significant individual advantages? Can kin selection or reciprocal altruism explain cooperation in general?

Hurford’s account of this question is detailed and up to date. But I would like to focus mainly on what appears to be Hurford’s preferred theory, and the one which, at least to my mind, is particularly appealing. According to this, trust (on which Knight places such emphasis: cf. p. 204) would have arisen from a process of self-domestication, occurring in parallel to the extension of infancy in humans far beyond that of other primates (a co-occurrence which is probably not the result of mere chance; see in particular p. 219). It could also be explained in terms of the link between affectivity and imitation (the imitation upon which culture is based): “the empathic bonding with the adult who is being imitated” [p. 203]. And I would also argue that there may be a connection, however loose, with the ‘Veridicality Assumption’ which Harris, 2001, has studied in children. Furthermore, in my opinion trust in lin-

guistic communication is made possible because linguistic information is received in a manner very different from the way in which we receive perceptual information. In other words, I would complement Knight's idea with that of Jerison (1988). It is precisely because linguistic information remains 'in quarantine' after it has been received that it can be attended to without excessive risk. This risk to the recipient, I should point out, would arise not only due to the possible 'dishonesty of the signal', but also as a result of false beliefs that the speaker might hold.

In conclusion, anyone who is interested not just in the origins of language but also in human beings or in evolution should read this book. Although the term 'interdisciplinary' appears only rarely (the single instance I spotted was on p. xii), here interdisciplinarity presents a real and exacting challenge. The approach Hurford has taken means that he will ultimately be judged by specialists in different areas, and this is certainly a stiff test to have to pass. However, he should at least be sure of one reward in the future: that when the science of the origins of language takes shape, this book will be considered a key milestone.

*Teresa Bejarano*

*Departamento de Filosofía y Lógica y Filosofía de la Ciencia*

*Universidad de Sevilla*

*C/ Camilo José Cela s/n, 41018 Sevilla*

*E-mail: tebefer@us.es*

#### REFERENCES

- HARRIS, P. L. (2001), "The Veridicality Assumption", *Mind & Language*, pp. 247-262.  
 JERISON, H. (1988), "Evolutionary neurology and the origin of language as a cognitive adaptation". In M. E. Landsberg (ed.), *The genesis of language*, Berlin, Mouton, de Gruyter, pp. 3-9.

*Einstein. Eine Biographie*, de JÜRGEN NEFFE, REINBECK, ROWOHLT, 2005; 496 pp. [Traducción al inglés: *Einstein. A Biography*, de JÜRGEN NEFFE, CAMBRIDGE, Polity Press, 2008, 461 pp.]

Pocas cosas nuevas se pueden decir sobre la vida mítica de Albert Einstein a estas alturas y, sin embargo, Jürgen Neffe lo consigue. Su novedad estriba en haber coordinado todo un conjunto de archivos que hasta hace poco estaban dispersos, aunque en su mayor parte fueran conocidos. De todos modos también se incorporan nuevos testimonios, como el del médico de cabecera y amigo de Einstein, János Plesch, que le permiten reconstruir zonas oscuras que hasta ahora habían quedado deliberadamente muy autocensuradas