SIGNS UNFOUNDED AND CONFOUNDED.
A REPLY TO SØREN LUND

by
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This paper responds to Søren Lund's critique of Roy Harris and integrational linguistics published in this issue. We demonstrate that Lund's characterization of Harris as an armchair linguist and integrational linguistics as merely a development of ideas found in his predecessors is based on ignorance of Harris's writings. We argue that the integrational conception of signs was developed by Harris as a response to his personal linguistic experiences as both a student in post-war England and later as a Romance philologist and dialectologist – experiences which so obviously clashed with the linguistic theories of his days. Furthermore, the paper refutes Lund's views of (i) natural signs as 'found' (not made), (ii) the indeterminacy of the sign, and (iii) integrationism as a form of radical relativism, and attempts to give the reader what the authors think is a more accurate understanding of a Harrisean semiology.

1. On armchairs and other chairs

In his paper, Søren Lund offers 'refutations' of three theses of integrational linguistics. He begins his discussion by characterizing Roy Harris as an 'armchair linguist', and integrational linguistics as an elaboration of the positions of earlier linguists. We shall begin our response to his article with Lund's introductory remarks on Harris and his predecessors, and then attempt to point out why we think his refutations of integrationism are unconvincing. We shall begin

each discussion point by citing Lund in order to make our responses relevant to the article under scrutiny.

Lund (2012: 5) claims that Harris is 'an essential armchair linguist' and adds in a footnote:

> Harris reports (1990:18) from fieldwork he apparently conducted in dialectology, and he cites as evidence an informant's reply. But, curiously, the paper from which this alleged evidence comes from does not appear in the references to the 1990 paper in question. (Lund 2012: 38)

Lund has not done his homework here, for prior to being the first appointee to the Chair of General Linguistics in Oxford, Roy Harris sat briefly in 'the only established Chair of the Romance Languages in Britain, inaugurated in 1909' (http://www.ling-phil.ox.ac.uk/romance-linguistics/). While both chairs may have been armchairs before or after Harris occupied them, they were hardly that while Harris rocked them. Furthermore, between 1966 and 1974 – before he even had a chair to sit upon – Roy Harris published eight papers on Gallo-Romance morphology (Harris 1966), paradigms in vulgar Latin (Harris 1968), Franco-Provençal historical morphology (Harris 1968a), Italian dialectology (Harris 1967 and 1969), medieval French orthography (Harris 1970), French lexicology (Harris 1972) and French phonology (Harris and Love 1974), as well as fifteen reviews of books on Romance linguistics, nine papers on theoretical linguistics and reviews of the same. And those publications followed a decade of earlier publications on medieval French literature. One can find in those early publications abundant evidence that Roy Harris the Romance linguist had a prodigious appetite for the investigation of language in medieval manuscripts and published texts as well as in Alpine villages. The transcriptions and maps in the papers on dialects demonstrate that Harris could hold his own

against any traditional linguist – indeed, enough to be appointed to a professorship at Oxford.

At the same time that Harris was doing linguistics in the spirit of the prevailing view, he was making observations that escaped other contemporary linguists, and was considering their theoretical implications. The fact that Harris refers back to an incident from that period in the 1990 passage Lund mentions demonstrates that Harris continues to return to his own linguistic experience in order to better understand both the original experience and the theoretical positions he developed later. Perhaps more interestingly in light of Lund's claim regarding Harris 'the armchair linguist' is that in the passage mentioned Harris draws on that personal experience to evaluate the existing literature on the topic. The passage is worth quoting in full here:

> In a paper published in 1962 Ivić (p. 34) wrote: 'For decades the classic and most controversial question of dialectology has been: 'Do dialects … actually exist?'. Many years before, Schuchardt – perhaps the most brilliant linguistic scholar of his day – had given an emphatically negative answer to that question. From my own experience of fieldwork in dialectology, the best evidence I can cite in support of Schuchardt's answer was given to me by an old man whom I asked whether the patois of his Alpine village was the same as that of another locality a few miles distant. I here translate his reply, given in (what I at that time called) 'Valdôtain':
>
>> Is it the same? I would not know how to answer you. Even in this village the younger people speak differently from my generation. And in the next valley perhaps they use words we don't use here. But, for all that, everyone understands everyone else well enough. Is that the same?

> By turning my own question back on me, he had made me understand that the mistake lay in the question. What I was asking corresponded to nothing in his own linguistic experience which could provide a determinate answer. When theorists begin to ask unanswerable questions about language (or – which amounts to the same thing – questions which can be answered 'yes' or 'no' as you please) that is the surest indication that in their investigations linguistic myth has taken over from linguistic reality. (Harris 1990a:18)

Lund appears to reject this anecdote on the grounds that Harris does not cite the paper in which the research was published. The incident concerning the villager did not appear in those papers published in the 1960s; is there something wrong with recounting an incident dating back to one's early days as a fieldworker many years later after one has finally understood what it meant? Harris undertook the fieldwork 'in August 1966 […] at twenty-five localities in the Aosta valley' (Harris 1969:133) and his published papers on the language of that region (what he then called the Valdôtain dialect) were written before he had integrated that experience into his theoretical understanding of language. Yet by his own account the encounter with the old man during his fieldwork did lead Harris to subsequent theoretical reflections and a new understanding of language.

Even though the issues discussed in Harris (1990a) did not arise in the papers of 1967 and 1969, there are other early papers – both in linguistics and in literature – in which one can find not only evidence of Roy Harris the competent but orthodox data-collecting linguist, but also, and more importantly, Roy Harris the data-questioning linguist, and Roy Harris the probing critical philosopher of language and linguistics. One can even find some of the original questions that would eventually lead to the development of integrational linguistics. As one example, in a 1967 review of *Latino 'circa romançum' e 'rus-*

tica romana lingua', a collection of 7-9th century texts, Harris comments:

> One is surprised to find that the discussion of scriptae is conducted without any reference to the complex problems of correspondence between written and spoken languages. The subject is touched upon only in the appendix, and there only in connection with phonetics. One would like to have seen in the preface a reminder of the variety of types of relation which may obtain between spoken and written discourse. For it is doubtful whether the beginner can make much of the notion of the development of a scripta at all, unless he is given some idea of the level or levels of abstraction involved. Without such guidance, he tends to be left with the impression that at one time (in some idealized Classical period) Latin was 'written as it was spoken', and that gradually the spoken language changed while the written language remained static, or lagged far behind, until the post-Carolingian period when the new vernaculars were again 'written as they were spoken'. … Thus the fact that there is any linguistic problem as regards how changes in a spoken language may be expected to give rise to changes in a corresponding written language becomes obscured. It is of course true that at present general linguistics offers no satisfactory theoretical framework for the treatment of such problems, but this is no reason for ignoring their existence. Indeed, one might have hoped that precisely in such a field as this traditional philology would have been able to make some distinctive contribution to modern linguistic theory. (Harris 1967a: 29)

When it does not simply make do with the (armchair) linguist's logic and intuitions, linguistic theory has been and remains largely dependent upon written records as its empirical basis, yet even today linguists assume that the oral and the written are related directly and

unproblematically in some manner that is theoretically insignificant. Harris alone pursued the theoretical implications of this problem, and by doing so identified a crucial problem in linguistic methodology. Harris noted these problems precisely because he was not an 'armchair' linguist but one who already in 1967 had been deeply involved in the linguistic study of historical texts for more than a decade.

In a paper published a few years later Harris investigated word criteria in French, and in the process questioned both criteria proposed by earlier linguists and the universality of 'word' as a unit of linguistic analysis. On 'potential pause' as a criterion for identifying words, Harris wrote: 'all it is is a misguided attempt to transfer into the analysis of speech the traditional 'word-divider' of writing, namely the space' (Harris 1972: 121-122). In the conclusion he remarked: 'The 'something' about French which the investigation of word criteria shows up is the fact that French has no unit which corresponds exactly to the 'word' of Latin (or of English)' (Harris 1972: 133). Here we see not 'armchair linguistics' but the probing questions of a linguist intimately acquainted with the facts of spoken and written French (and Latin and English) drawing out the theoretical implications of that problem noted in the paper of 1967.

In one of Harris's last writings on Medieval French, a 1976 review of *L'expression de l'affectivité dans la poésie lyrique française du moyen âge (XIIe-XIIIe s.): étude sémantique et stylistique du réseau lexical 'joie'-'dolor'*, he noted the possibility of alternate interpretations:

> By means of a liberal treatment of other words as mere 'substitutes' for *joie* and *dolor*, it is possible to make the thesis look plausible. But then is the 'réseau lexical joie-dolor' anything more than a construct imposed upon the language of the trouvères by its investigator? (Harris 1976: 94)

It was Harris's knowledge of the language of the troubadours – not a theory, integrationist or otherwise – that allowed him to see that

the linguistic facts of a 'réseau lexical' are in fact a linguist's fiction. Far from a simple dissolution of linguistic facts conceived as eternal objects waiting for the linguist, Harris recognized that this constructive activity was essential not only for understanding the limits of theory but for understanding language *tout court*. And this recognition arose not from the daydreams of an armchair linguist but as an unavoidable conclusion in the face of conflicting interpretations of apparently identical linguistic 'facts' known to Harris and the linguists and literary scholars with whom he found himself in disagreement.

2. Whence the fundamental ideas of integrationism?

Lund next characterizes integrational linguistics as a set of ideas taken from others, something that he claims Harris himself acknowledges:

> According to Harris, the integrational school's fundamental ideas were already present to a certain extent in the thinking of John Rupert Firth, Bronislaw Manlinowski, Kenneth Pike and Edward Sapir (Harris 1998:10). (Lund 2012: 6)

This (apparently innocuous) statement reveals a problematic failure on the part of the author, Søren Lund, to come to grips with integrational linguistics. For Lund, integrational semiology is merely a continuation of what was already clear to other pragmatically-oriented linguistic thinkers before Harris. Lund believes that Harris takes from Saussure the conception of linguistics 'as an essentially lay-oriented discipline' (Lund 2012: 6) as well as an anti-epistemological position, and from Wittgenstein the view 'that (integrational) linguistic theory should be a kind of lay therapy' (Lund 2012: 7). It seems to us, in fact, that Lund envisages (as an alternative to integrational linguistics) a linguistic theory that complements 'segregational'

insights with 'integrational' ones. He even claims that if his radical reading (Lund 2012: 28) is incorrect and 'integrationism should be interpreted in a more charitable way' (Lund 2012: 29), then we are all already integrationists. However, what did Harris *actually* write about his predecessors and whether they qualify as proto-integrationists?

> Although there are significant strands of integrationist think-ing in the linguistics of Sapir, Malinowski, Pike and Firth (Harris 1998a), in none of these cases did this develop into anything more than a cautious modification of the prevailing segregationist programme. It always stopped short of calling in question whether the linguist is in a position to do what segregationists have claimed, on behalf of linguistics, to be able to do; namely, to identify by the application of objective criteria a determinate system of signs that constitutes 'the language' of a given community or a given individual. (Harris 1998: 10)

On the following pages Harris continues by identifying six segrega-tional assumptions coupled with their corresponding integrational counterproposals. But contrary to Lund's assertion, Harris does not identify integrationist ideas *avant la lettre*. For a fuller discussion of Harris's understanding of the relation between the fundamental ideas of integrationism and the ideas of Firth and others, we turn to an earlier paper (Harris 1987) in which the linguistics of Firth, Pike, Sapir and Malinowski are discussed.

In that paper Harris provided a brief note on Sapir's recognition of the social function of language:

> An interesting case is that of Sapir, who acknowledges that the normal function of language is to articulate many and varied patterns of social behaviour. Sapir's example is bor-rowing money. 'If one says to me 'Lend me a dollar,' I may hand over the money without a word or I may give it with an

accompanying 'Here it is' or I may say 'I haven't got it' or 'I'll give it to you tomorrow.' Each of these responses,' says Sapir, 'is structurally equivalent, if one thinks of the larger behavior pattern' (Sapir 1933: 12). Yet although Sapir realized this, he does not seem to have realized its theoretical significance, or at least not bothered to analyse the theoretical implications. The integrationalist insight is glimpsed, but then not followed through. Sapir apparently fails to see that this kind of structural equivalence is not something outside language, but something without which what is said would be outside language – that is to say, would be meaningless. (Harris 1987: 135)

There is a world of difference between having a glimpse of something and pursuing the theoretical implications of that insight. 'Recognizing a truth is not to be equated with realizing its theoretical implications' Harris would later write in regard to Firth (Harris 1990:42). Malinowski similarly falls short of developing an integrationist position in spite of his remark regarding language as a 'mode of action':

It is not surprising that the clearest expression of an integrational perspective on language should have come from one of the leading figures in social anthropology of the interwar period, Malinowski. But Malinowski's most famous dictum, that language is 'a mode of action, rather than a countersign of thought,' when watered down into such statements as 'the context of situation is indispensable for the understanding of the words' (Malinowski 1923: 307), or 'the utterance has no meaning except in the context of situation' (Malinowski loc. cit.) appears to reduce readily to truisms with which nobody would disagree. As interpreted by J. R. Firth, Malinowski's claim emerges in the sadly emasculated guise of recognizing an 'outer' layer of contextualization statements which the descriptive linguist is obligated to undertake in order to 'complete'

the description of a language. It is like giving a final road-test to the already assembled car. Thus viewed, what Malinowski says is drained of its most radical theoretical content. (Harris 1987: 135-136)

Harris then identifies the theoretical failures of Pike and Firth:

> In America, the attempt to integrate linguistics into the general study of communicative behaviour was pursued most systematically by Kenneth Pike, while in England a similar emphasis emerged in the work of Firth, for whom 'the central concept of the whole of semantics…, is the context of situation. In that context are the human participant or participants, what they say, and what is going on' (Firth 1957: 27). In both Pike and Firth, however, one sees a further consequence of the compromise between the segregationalist and integrationalist positions. Although Firth uses the term integration, for him the analysis of that wider integration begins 'when phonetician, grammarian and lexicographer have finished.' In other words, Firth works from utterances 'outwards,' and not from the total context 'inwards.' Like Pike, he seems to have conceived of the nonverbal part of communicative behaviour essentially as language carried on by other means. This is evident even terminologically in the case of Pike, who introduced such units as the 'behavioreme' (a term clearly modelled on phoneme and morpheme). Thus, in both cases, the approach eventually adopted envisaged an extension of the analysis of language systems to embrace a certain range of related social facts, rather than any rethinking of the basic assumptions underlying the postulation of language systems in the first place. (Harris 1987: 136)

In conclusion Harris articulates why integrationism is not a development on the basis of these eminent predecessors, but a rejection

of the main principle upon which their science of linguistics rested, i.e. the autonomy of language as a system of signs:

> These cases are instructive because they enable us to pinpoint a deeper reason why segregationalism has dominated modern linguistics for so long. It is simply this: that even those linguists most sympathetic to an integrationalist approach to language on the theoretical level were in teaching methodologically committed to segregationalist practices. There was a failure, in short, to come to terms with the fact that a thorough-going integrationalism requires us to recognize a principle which may be called the 'non-compartmentalization principle' (Harris 1981: 165). Whatever name we choose to give it, this is the principle that as human beings, whose humanity depends on social interaction, we do not inhabit a communicational space which Nature has already divided for us between language and the nonlinguistic. Or, to put it another way, language is not an autonomous mode of communication and languages are not autonomous systems of signs. (Harris 1987: 136)

Only on one occasion does Harris allude to another linguistic school espousing a 'non-segregational' theory, namely the Italian neolinguistic school inspired by Benedetto Croce (Harris 1996: 7, fn 9). However, this does not mean for Harris that Croce, or any of the Neolinguists, would qualify as proto-integrationists.

Lund's characterizations of Harris and integrationism, as discussed in this section, are intimately related; an understanding of Harris that labels him an 'armchair linguist' is one that also finds nothing new in his linguistics. According to Lund (and other critics), Harris is guilty of recycling ideas that were not only not his own, but which are relics of a theoretical past beyond which modern linguistics has progressed. *Pace* Lund, what we find is that the principles of integrationism arose not from a development of earlier theories – whether

those of Saussure, Sapir, Malinowski, Pike or Firth – nor from the armchair linguist's native speaker intuitions, but from attention to *actual linguistic experience*. This, however, is not tantamount to claiming that integrational linguistics took shape in a historical-discursive vacuum – obviously it did not. In describing his student days Harris himself wrote of the linguistic situation in which he and his fellow students found themselves and how that shaped their theorizing:

> As students of language and languages, we were taught a linguistic orthodoxy which manifestly conflicted with our own linguistic experience. The war was a time when innovations of all kinds abounded in spoken usage and could not be ignored. The conflict between what we were taught and what we could observe for ourselves was blatant and pervasive [...] Wartime English was clearly *not* the English described and exemplified in our school books [...] It was a time when familiar expressions abruptly and inexplicably acquired new and sometime contradictory meanings (*You've had it.*). [...] The upheavals of evacuation had long since taught us that speech went around with people: it was not mysteriously anchored to places. And sound laws were hard to reconcile with the inconsistencies of pronunciation that could be heard within the walls of a single wartime classroom. At no level did one ever feel convinced that the orthodox story actually made explanatory sense of one's own linguistic environment or the linguistic activities in which one was daily engaged. [...] Given the manifest disparity between our first-hand linguistic experience and what we were taught about language in school and at university, we were faced with an awkward choice. Some of us, not unreasonably, decided that the linguistic orthodoxy was antiquated rubbish [...] Others, more cautiously, decided that the experts must have got it right after all [...] and consequently wrote off the perceived disparity between linguistic orthodoxy and linguistic

experience […] Only a very small minority, of which I was one, opted for neither of the aforementioned solutions, but treated the disparity itself as a major source of linguistic interest. […] *Why isn't language as they say it is?* This, in retrospect, seems to me the most obvious, the most basic, the most inevitable question that was bound to emerge from the educational experience of my generation. (Harris 1997:238-239).

As the above passage demonstrates, the principles of integrationism were developed upon the basis of attention to primary experiences of language rather than on the basis of an analysis of linguistic data that is a theory-laden abstraction from the moment it is recorded. It took years of experience, followed by reflection upon that experience, to arrive at the principal tenets of integrationism, but it is from that experience and Harris's reflection upon it that integrationism developed. In a letter to David Bade, Harris himself identified the original situation which eventually led to his realizing that the belief in languages as 'fixed codes' could not yield satisfactory answers to his questions arising from primary linguistic experience:

The first time I began to have doubts about the concept 'a language' was on one occasion years ago during my period as a research student when I had to plough through at some speed a great number of medieval texts, manuscripts and anthologies of various provenances, looking for 'examples'. One evening I found myself reading a poem and realized, to my surprise, that although I could understand it well enough, I had no idea where it originated, what date it was, or what language it was written in, except that – I presumed – it must be some variety of Romance. At the time I explained this experience to myself as being due to the fact that my (imperfect) acquaintance with Old French, Old Provençal, Old Spanish, Catalan and various Italian dialects was sufficient to enable me unconsciously to

construct a kind of hypothetical protolingua that approximated to whatever language the poet had been using. But later I came to realize that this 'explanation;' was even more difficult to understand than the facts it was supposed to 'explain'. (Roy Harris, letter to David Bade, 18 February 2006)

Harris originally interpreted his experience – understanding a text without previously having encountered the language in which it was written – according to the prevailing psycholinguistic theories, attributing his understanding of the text to the unconscious construction of a hypothetical protolingua. Yet that original experience was a matter to which he returned and which he continued to question, developing from that experience – not from his original theoretical explanation of it – his critiques of his predecessors. Eventually Harris concluded that the Saussurean notion that there is no linguistic sign outside the context of its language was wrong. The armchair linguist, like the linguist who takes his ideas from other theorists, never returns to contemplate the original experience but only attends to 'data' that has been prepared according to theoretical dictates. In developing the principles of integrational linguistics Harris discarded existing theories and pursued the theoretical implications of what he would call 'first order linguistic experience'. We shall now turn our attention to the three integrational theses at which Harris eventually arrived and which Lund rejects.

3. Three integrational theses.

3.1. Thesis 1: On the integrational sign.

The second axiom […] seems fairly controversial. It purports that the value of a sign is a property of the act of the 'proficient integrator' rather than of the sign itself. (Lund 2012: 10).

Lund begins the section entitled 'The inadequate character of the integrational sign' (Lund 2012: 9) with a serious misreading. Harris did not claim that the value of a sign is *a property of an act:* he wrote that 'the value of a sign is a function of the integrational proficiency which its identification and interpretation presupposes' (Harris 1993: 321). Of this distinction Harris wrote: 'A proficiency is not an act. If it were, the world would be a quite different place. Proficiency is a potential' (email to the authors, 26 October 2011). In integrationism, a sign is the product of creative and purposive activity and does not preexist that activity as something one finds, takes and interprets. The writer of Lund's paper must mean something by the marks which he makes on the page, or those marks are not signs at all, and the first act that a potential reader of Lund's paper must perform is to identify those marks on paper as having been the product of Lund's (or someone else's) sign making activity, i.e. *to make them signs again as a reader.* Without those two acts of sign-making there is no sign, only ink on paper. In the case of Lund's paper, it is only when two signs have been made, one by the writer and one by the reader, that communication or miscommunication may take place. Perception reveals only a black and white object. It is an act of intelligence based upon past learning that identifies that object as (electronic) paper with (electronic) ink, and the ink as purposely applied in order to communicate; in that act of identification we make the ink-marks-on-paper a sign of a particular kind, namely an academic paper which we may or may not understand and to which we may or may not wish to respond. Further examination allows us to identify (identification being an act of the reader) the various ink marks as signs written in accordance with that social practice called 'Standard English'; what Lund meant by his signs is not so easy to identify and interpret. Our response to Lund may in fact be one more example of the failure of communication due to the *indeterminacy* of linguistic signs.

The issue of making a sign versus finding one is crucial in arguments about construing or understanding natural objects and events

as signs, as well as misunderstanding any and all signs. Harris (1997: 49) noted that when Robinson Crusoe found a footprint it did not matter whether anyone intentionally made a footprint as a sign; all he found was a footprint. Crusoe himself made it into a sign, namely a sign that someone besides Crusoe must be on the island. In similar fashion each of the present authors found something in their email inbox; we have both actively attributed to that something a significance, namely that Søren Lund is trying to communicate with us rather than identifying that email as spam. That attribution and all that has followed from it may be *mistaken attributions of significance*. We begin with Lund's remarks on the latter.

3.1.1. Mistakes

Lund argues against the integrationist understanding of signs by claiming that mistaken attributions of significance prove that the meaning of a sign is independent of anyone's attribution of meaning:

> In addition, I would take exception to Harris's idea that significance is constituted solely by attribution. It is true that we attribute significance to things, but sometimes we do so mistakenly. Given that mistaken attribution is possible, it follows that significance is not constituted by attribution. (Lund 2012:16)

What 'follows' is not *that significance is not constituted by attribution* but precisely that the meaning of a sign is the product of an activity of the one attributing significance, for without that active sign-making there could be no mistaken signs. The activity of the sign maker as interpreter is just as important as is the activity of the one who made the original sign with the intention of communicating something to someone. In integrationist theory, 'signs

are not prerequisites of communication, but its products' (Harris 2005: 110), and 'there is no 'linguistic message itself'. Or rather, the 'linguistic message itself' is an artifact of the theoretical perspective and the analytic methods adopted by linguists' (Harris 1997:267). When Lund misunderstands Harris, what is demonstrated is that Lund has made Harris's language mean something that Harris did not mean, and this is something Lund did, not something which the ink on the paper tried to communicate to him. And if Lund wishes to understand what Harris wrote, no interrogation of the ink on paper in the books by Harris will clear up this mistake since the written sign did not write itself in order to mean something: Harris wrote something and one must ask Harris what he meant by what he wrote, for the problem (we do not say mistake) may be resolved by negotiation involving both persons in communication. The significance of the sign according to integrationist theory is always constituted by both the writer and the reader, the speaker and the hearer; we speak of miscommunication when one of the persons communicating believes that something went wrong. What follows a perception of misunderstanding (or in Lund's terminology 'mistaken attribution') may be restatement, elaboration, discussion, negotiation, appeal to an authority (such as a dictionary), irritation and insult, a parting of ways, fighting, warfare, nuclear meltdown or some other disaster that makes further efforts at communication or other alternatives impossible. The stakes in mistaken communication can be extremely high; hence the insistence within integrationism on the fundamental importance of the lay person's perspective and on taking responsibility for one's language as speaker and hearer, writer and reader.

Lund assumes that signs have an 'inherent' significance: thus, one can attribute significance 'mistakenly', that is, make a sign mean something other than what it means all by itself apart from any person and independent of space and time. On what basis do we judge attribution of significance as 'mistaken' or not? Take the case

of visual experience. Presumably one would have to judge such an experience as either matching reality or not matching reality on the basis of verbal evidence. Suppose a philosopher-turned-neuroscientist invents an illustrative story for his/her students (or readers) involving two hypothetical individuals, A and B, in which B, standing at the window in A's house, tells A: 'There's a *rat* in your garden'. *A* walks to the window himself, looks down into the garden and says: 'No, that's my *hamster*!'. The scientist is likely to make the following claim at this point, namely that B has made a 'mistaken attribution', i.e. 'what B sees' and 'what B says he sees' do not match, and the proof is provided by B's statement concerning the animal in the garden. Yet, as the integrationist would point out, the communicational exchange in this anecdote is an exchange between two anonymous interactants. The anecdote tells us that B is in A's house, but it says nothing about whether A and B are old friends, or mere acquaintances, or perhaps perfect strangers. What is more, B may always refer to rodents as 'rats', but that is a bad habit of his which the scientist's illustrative story is likely to be silent about. What this example illustrates, we believe, is that the whole semiotic discourse of 'mistaken attribution' is the result of an abstraction – here of envisaging communication as an act (performed by an anonymous stand-in) of attaching the wrong linguistic label to a referent. Or let us imagine another (personalized) scenario: for Adrian Pablé to remain seated and pontificate about the 'proper' name of an insect while my wife is running around the house panic-stricken, telling me to 'kill *that bug* on the wall!' is either to fail completely to integrate the present communicational activity or to be deliberately cruel. In both cases described above, an appeal to 'reality' is of little help for the simple reason that what is at issue is not reality, but why someone said what he/she said in a particular situation (to a particular individual). The meaning of what that person said is whatever was meant, not what the dictionary (or encyclopedia) tells us it means. Interpreting what was said 'correctly' is a matter of integration, and

the problem of whether or not what the hearer (reader) construes the speaker (writer) as having meant is 'the same' as what the speaker (writer) intended cannot be resolved by any recourse to the notion of 'code' serving as a guarantor of mutual understanding.

Nor will an appeal to science solve the matter of attributing the right (inherent) names to objects or the proper significance to scientific terms if that was what was at issue, not even if it is the case that the interlocutors agree on the authority of science to interpret the universe and legislate names. Why? Because descriptions, explanations and the terminology in which these matters are discussed are all continuously debated, geographically differentiated and change over time. Lund's semiology fails to take time into account, as it fails to address the multiple issues related to synonymy, i.e. are *mouse, ratón, khul'gan, siçan, Maus, ratolí, topo, sorcio, muis, mus, hiiri, pelė*, etc. synonyms or do they differ in meaning, and in either case what does this mean? Science is in large part a history of contested attributions of significance to natural, indeterminate and nonexistent objects like mouse, human error, missing links, light, life, race, *the* English language, intelligence, quarks, MERGE, cold fusion, the primitive mind, the real number of planets, phlogiston and memes of all sorts. If the history of science is any indication of the nature of the linguistic sign, then we must conclude that if the indeterminacy of a sign renders it incomprehensible, the history of science is nothing but a perpetual history of mistaken attributions and incomprehension.

Whether one calls it attribution of significance or sign-making, a sign misunderstood is precisely a making of the wrong sign from a given material, and the only one in a position to call a mistake a mistake is the person who meant what he or she said or the person who for whatever reason comes to believe that he or she has been mistaken. In order for someone not to be mistaken, the sign need not do anything – how could it? Why would it? The *sign-maker* must make the right sign, and the right sign will be simply that sign

which will lead him/her to do the one thing necessary (to quote a great Danish writer).

But what constitutes the right sign as opposed to the wrong sign? Lund writes of mistakes as though one can identify a mistake simply on the basis of the inherent meaning of the sign without any reference to the persons engaged in communication via signs. A look at the mathematical theory of error reveals the interesting (to the integrationist) identification of measurement error with necessary uncertainty and variation:

> Strictly speaking, we ought, in the expression of our general idea, to use the word 'uncertainty' instead of 'error.' For we cannot at any time assert positively that our estimate or measure, though fallible, is not perfectly correct ; and therefore it may happen that there is no 'error' in the ordinary sense of the word. And, in like manner, when from the general or abstract idea we proceed to concrete numerical evaluations, we ought, instead of 'error,' to say 'uncertain error'; including, among the uncertainties of value, the possible case that the uncertain error may = 0. (Airy 1861:4)

Measurement *presupposes* the correct identification of the objects to be counted, the units of measure, and the validity of the purpose for measurement. With those matters assumed to be correct, the inevitable phenomena of error, variation or uncertainty – 'no measurement can ever be exact' (Zebrowski 1979:22) – can be dealt with mathematically to the degree required by the results desired, for 'the acceptable error in a measurement will depend on the reason the measurement is made' (Zebrowski 1979:22). In contrast to measurement error, no mistake in identification or classification, much less a confusion of purpose, can be identified or rectified by any mathematical treatment because such mistakes can only be identified by reference to purpose and corrected on that basis. The

only way to identify a mistaken attribution of significance in any particular case is through an appeal to the sign-maker, an appeal that Lund refuses to allow.

Rather than pursuing the matter of errors further, we recommend a reading of two other of Lund's fellow Danes – Jens Rasmussen (1990, and other papers) and Erik Hollnagel (especially Hollnagel 1983, Hollnagel and Amalberti 2001, and Hollnagel and Woods 2005) – not as integrationists *sans la lettre*, but as the best means for countering Lund's inadequate understanding of the phenomena of errors and mistakes. We turn instead to his argument regarding 'natural signs' where the issue at stake is whether nature is trying to tell us something or not.

3.1.2 Natural signs

> Signs are sometimes made, but sometimes – as in the case of medical symptoms and other natural signs, *pace* Umberto Eco – they are discovered. Harris is simply wrong when he likens all signs to language, in the sense of having to be constantly remade, on the penalty of dying. On the contrary, we need to distinguish between signs made and signs found. Falling air pressure will still be a sign of a developing storm, even if human beings lose all knowledge of meteorology and no longer possess barometers, etc. However, that is not to the same as saying that such signs are, to use Harris's phrase, simply 'given' to us. Besides, there is a false dichotomy here between what is given and what is made. Signs not yet made still have to be discovered; they are not obvious. (Lund 2012: 16-17)

Lund's understanding of the significance of nature is as old as the Biblical narrative of creation:

> And God said, Let there be lights in the firmament of the heaven to divide the day from the night; and let them be for signs, and for seasons, and for days, and years. (Genesis 1:14)

The Gospel of Matthew provides a different situation, for in this passage every natural sign is attributed to something that 'ye say' or that 'ye can discern' (we will not argue that Jesus was an integrationist *avant la lettre* on the basis of this passage):

> The Pharisees also with the Sadducees came, and tempting desired him that he would shew them a sign from heaven. He answered and said unto them, when it is evening, ye say, it will be fair weather: for the sky is red. And in the morning, it will be foul weather to day: for the sky is red and lowering. O ye hypocrites, ye can discern the face of the sky; but can ye not discern the signs of the times? A wicked and adulterous generation seeketh after a sign; and there shall no sign be given unto it, but the sign of the prophet Jonas. And he left them, and departed. (Matthew 16:1-4)

According to Lund's doctrine of natural signs, red sky in the morning means foul weather, red sky in the evening means fair weather, clouds mean rain, high bodily temperature means illness, and smoke means fire, whether or not the Sadducees say or discern anything and whether or not it actually does rain, etc. Nature speaks loud and clear everywhere and always, independently of all human knowledge and experience, and with no intention of communicating anything to anyone. Lund goes further than Jesus by insisting that the meaning of natural signs is not in any way determined by our living engagement with the world but may be read directly from the sign itself. Lund does not consider *recognition* to be a matter of human activity upon which signhood depends – and for this reason, when Harris writes of 'recognizing' signs, he accuses Harris of contradicting his own semiology:

> When Harris writes: ''I recognize and contextualize it [the tree] in a certain way in relation to certain activities'', isn't that 'recognizing its sign value'? But if that value is a matter of sheer *making*, then we are back at egocentrism: the only thing that the sign is *qua sign*, is what *I* make it to be. And what could that be, other than a pure figment of my imagination, having nothing to do with the tree itself since the tree's spatial property is impertinent to the sign of that particular tree *as a sign*? Why does Harris even mention 'recognizing', when his recognition has nothing to do with his view of the sign… (Lund 2012: 16)

Integrationism acknowledges that it is the task of every human being to interpret objects and events as meaningful or not in our living engagement with the world. Natural objects and events may have no significance for us here and now and in such situations may be ignored; under other conditions we may indeed interpret similar objects and events as relevant and therefore significant. We may make a connection between smoke and fire (for instance) and thus interpret, understand or recognize smoke as signifying fire. What do recognition, interpretation and understanding all entail? Human activity, and in integrationial semiology 'recognizing' a sign is 'making' a sign! It implies that I integrate my present visual experience with a past one, and thus identify something as something I have already encountered in the past (or as belonging to the same type). That identification and classification are creative acts involving memory. Similarly in the case of 'making' signs as opposed to 'discovering/finding' signs: again, 'discovering' a sign is an activity which requires integration, e.g. of experience. Smoke / red spots are *not signs of anything* without the concrete, experiencing subject. No red sky, no cloud, no tree, no barometer has ever tried to tell anyone anything about the coming of dawn or dusk, sunshine or rain. Rather we inform ourselves of our situation by making signs of the world

around us, and we as well as our friends, neighbors, family members and weathermen sometimes try to tell others something about the coming weather based upon our understanding and interpretation of meteorological phenomena. To speak of 'signs not yet made' which 'still have to be discovered', as Lund (2012: 17) does, is to render meaningless the notion of 'human creativity': *either we are sign-makers or we are not* – there is no middle-way.

Lund's semiology of natural signs appears to imply that everything *may* be a sign – an object meaningful in itself apart from all experiencing subjects – but leaves open the possibility that nothing at all *has* to be a sign. If so, the status of any object as a sign or not is unknown prior to the human act of deciding, discovering, finding or recognizing that something is or is not a sign, and this prior to any interpretation of the meaning of the sign once it has been recognized to be a sign (i.e. made into a sign). For Lund, apparently, some natural objects and events may mean something, but there is no need to assume that all must mean something.

If it is accepted that some objects are signs and others are not, we must ask for whom are natural signs signs? And we respond: to *me* – or else it is not a sign for *me*. For what could and why would any particular cloud, grain of sand, finger, hair, insect mean exactly the same thing objectively and always to everyone everywhere and what would be the point of understanding everything as a sign of something? In real life – we are assuming that attention to the facts of experience are relevant to rational inquiry – we only seek to identify natural objects as signs and interpret them according to how we understand their relation to our activities as, say, scientists or living beings. This holds true not only for natural signs, as Lund would have it, but of the data of scientific enquiry in general. What Hollnagel and Woods (2005) wrote of data collection is equally true of signs:

> In the words of Sherlock Holmes, 'it is a capital mistake to theorize before one has data'. Yet it is an even greater mistake to

> believe that data can be collected without some kind of theory
> or concept. Data exist only in relation to a set of concepts or a
> classification scheme and are not just waiting to be picked up
> by a meandering scientist. (Hollnagel and Woods 2005:50)

If it is then admitted that the significance of signs depends upon
the theories, concepts, circumstances, experience and activities of
particular persons, this means for Lund that we have plunged into
'egocentrism'. So in order to avoid solipsism and egocentrism Lund
himself must assume that *all* natural objects and events are signs
independently of an observer, and construct a theory of signs ac-
cordingly, for the status of any object or event qua sign must be an
eternal and inherent property of that object or event and must not
be dependent upon individuals, time and circumstance.

Clouds mean 'rain', and behold, it rains. Voilà! We have correctly
interpreted the inherent meaning of the sign, and that independently
of our experience. If it does not rain, however, we are left with a
dilemma: either clouds are signs or they are not. If they are signs but
it does not rain then either clouds are not signs of rain or the clouds
were lying, for if clouds sometimes mean 'rain' and sometimes mean
'no rain' then we hardly have a natural sign whose meaning can ever
be known for certain. And we must ask: can natural signs lie, and if
so, do they? Perhaps clouds really only mean there is precipitation
in the atmosphere and maybe it will rain but maybe not? Whence
this indeterminacy of the significance of clouds?

Analogously, where Harris's example of the landmark (discussed by
Lund) is concerned:

> There is, indeed, a certain ambiguity in the way Harris pre-
> sents the case. It can be argued that the sign's value *qua* a
> sign depends on the fact (if fact it is) that the tree stands in a
> certain spatial relationship to the house, to the road leading

to it, etc. If the tree in question does not exhibit that factual relationship, then Harris is just mistaken in making it into a sign having that indexical function […] In case Harris is willing to admit that its spatial features are what causes the tree to be of some use or value as a sign, then I think he is wrong in not recognizing the potentialities of things to be signs; […] In fact, though, Harris keeps insisting that the value of the tree as a sign has no basis in the fact that the tree has a certain *spatial relationship*; this value has nothing to do with the thing itself, nor with what anybody thinks it has – pure egocentrism, as far as I can see. […] The potential impact of the sign's value is wholly a matter of the integrational proficiency of the driver, with due reference to the biomechanical and macrosocial parameters that shape the proficiency in question. It is curious, however, that the impact of the sign's spatial relationship to the house does not constitute part of Harris's contextualized 'programme of activities'. The driver, it seems, operates independently of the fact that there is a spatial relationship from the tree to the house. Therefore, he is not constrained by any influences other than the responses to his *own* activities. (Lund 2012: 15-16)

Lund thus insists that the tree functions as an index for Harris (indicating that soon he has to turn in order to get into the right road leading to his house) precisely because it has the *potential* of functioning as such a sign: in other words, Harris 'finds' (or 'discovers') the sign rather than 'making' it. The indexical potential inherent in the tree qua sign has to do with its spatial relationship to Harris's home, according to Lund, i.e. the tree must already be meaningful (its inherent spatial property) in order for Harris to attribute to it that particular indexical value at all. It is certainly true that Harris is unlikely to make the tree which stands just outside his working-place an indexical sign signaling to him that soon he will have to

turn left to get to his house. His choice will be constrained in so far as presumably he will pick a tree that stands 'in some proximity' to the road which leads to his house. The constraints that Harris *qua* sign-maker is subject to are the same as for anybody else, namely biomechanical, circumstantial and macrosocial ones (Harris 1998: 29). To reiterate a point made previously: in order to distinguish between objects that have 'sign potential' and those that don't, we need to be able to *rule out categorically* that certain objects can be 'potential signs' of something (otherwise the phrase 'potential sign' is meaningless). What the integrationist claims here is that only the sign-makers themselves make something into a sign (or not) – the so-called observer-independent 'external world', with its objects and their 'inherent properties', cannot limit human creativity.

3.1.3 When is nature a sign?

Is the air pressure falling or is it rising? Just ask the barometer! Alas, last time we looked, it said nothing about falling or rising, it just sat there doing nothing discernible. Do those clouds mean rain, or only 'Do not bother sun bathing today'? We need only ask the clouds to be more specific in their messages. And does that tree mean 'My house is 8.2377 meters away on the left', or merely 'My house is nearby'? Can it possibly mean either of these things to anyone but me, and that only while I am living there (I have not lived there always)? We need only ask the tree to be more definite in its meaning – right? If only natural signs, historical events, barometers, human speakers and writers would be more direct and to the point in their efforts to communicate! The world of our experience makes it rather difficult to accept Lund's semiology of an inherent and unchanging meaning in the world around us.

   Just as the meaning of natural objects such as clouds when taken to be signs is dependent upon personal circumstances and both

past and future events (i.e. does it rain or not? Was I going to go sun bathing or water the garden?), the meaning of past events can often only be argued (by someone) in the future – and we use the word 'argued' advisably in any discussion of the meaning of history – and that only by someone intent on making the past a sign of something. The remark found on the Internet 'Please *let this be a sign* of a return to rationality in the Republican party. This would have been unthinkable a year ago' is indicative of the fact that we expect human actions to signify something, that from them we can discern human thought, feeling, intentions and possible futures, but that what we can learn from observable actions is knowable only in part – if at all – after the passage of time and further actions by the same (or by different) actors. We attribute meaning to – and argue about the meaning of – human actions based upon subsequent events.

As in the case of a return to rationality in the Republican Party, the meaning of natural signs are often taken to refer to future possibilities, whether hoped for or feared. Ed. R. Meelhuysen of the Bible Research Company has identified 'Twelve signs that we are very near the end of the world':

> I strongly believe that we are very close to the beginning of the time of Tribulation and the sequence of Events of Revelation culminating with the 'end of the world' as we know it! Here are twelve of the most evident signs that are being fulfilled. Most were prophesied by the Bible or other prophetic writers. (http://www.bibleplus.org/12signs.htm accessed 13 July 2011)

If these twelve signs are indeed signs, then is it possible for any science of linguistics, semiology or history to determine their meanings for the future? It is, of course, possible to investigate and argue about the possible meanings of signs such as 'the near saturation of the gospel to every language tribe and people', 'financial collapse

in countries around the world', and 'the growth of knowledge', 'spiritualism', 'moral decline', 'ecumenism', 'governmental regulations', and any of the other signs listed by Mr Meelhuysen, *and this is just what Meelhuysen and his readers and respondents do.* Who is to say whether these people have mistakenly attributed or correctly divined the meaning of these 'natural' signs? In every case of signs of the end of the world, the final, definite and determinate proof of their being signs of the end of the world will entail the end of any possible interpreter, scientific or not. These events certainly are signs because people have made them signs of something, yet the meaning of these signs cannot be simply read off from the events and social situations Meelhuysen mentions – no matter what scientific methodology we might employ, for the existence of such things as 'growth of knowledge' and 'moral decline' are themselves debatable, and that apart from any question of their being signs. Exactly the same considerations apply to landmarks, but Lund disagrees:

> Actually, it could be argued that the integrationist sign presupposes that very sign concept, namely the Saussurean bi-planar sign, which Harris finds inadequate. In the long passage quoted earlier, he speaks of a 'tree' serving as a 'landmark'. In the immediate surroundings, phenomena are often exposed to signification when a spatial-temporal object becomes provided with content. However, the important thing is that no *integrated activities* are involved in calling the phenomenon in question a 'landmark' or 'tree'. In other words, Harris seems to presuppose a signhood which appears not to be integrational in nature because the act or process of making something (e.g. an object) into a sign is not *per se* an integrational act of contextual activities. It is the simple irony of Harris's own example (which he claims is supported by our own experience) that it demonstrates the very existence of a social convention which he wants to refute (Lund 2012: 17)

To call an object 'tree' or to refer to something as 'moral decline', are certainly integrational acts or processes. Moreover, in the example of the tree Lund conflates 'perceiving a tree as a tree' and 'telling somebody else (e.g. your readers) that this is a tree'. *Pace* Saussure, the two integrational processes require different kinds of integrations and orders of knowledge (see Harris 2009:166). The indeterminacy of the sign, the second thesis that Lund rejects, arises directly from the recognition that the sign is necessarily the product of communicational activity rather than something existing external to and independently of the sign-maker.

3.2 Thesis 2. On the indeterminacy of the sign

> First, the integrationist doctrine assumes that *all* linguistic signs are indeterminate. What is the epistemological value of this assumption? In order to make sense, the tenet needs to be expressed in linguistic signs; otherwise it would not be understandable. Furthermore, in accordance with the integrationist claim, these linguistic signs are indeterminate. Take now the proposition 'p': 'All linguistic signs are indeterminate', and apply it to itself. The integrationists must apply signs in order to make sense; but at the same time the signs which they apply are, according to their own claim, characterized by indeterminacy. Consequently, *ex hypothesi*, the propositions of integrationism are themselves indeterminate and thereby incomprehensible. In short: integrationists presuppose what they, in fact, deny. (Lund 2012: 19)

There can be no numerical identity of spoken or written signs for the simple yet profound reason that no two signs – whether utterances or texts – can be made of exactly the same physical materials. That in itself precludes the identity of any two signs, even without

considering the temporal dimension of signs and communication. If sameness or similarity is to be declared, that must be an analytical act of some human being. Saussure understood this and proposed the rather unconvincing notion of 'approximate sameness' in his discussion of the intersubjectivity of mental concepts:

> All the individuals linguistically linked in this manner will establish among themselves a kind of mean; all of them will produce - doubtless not exactly, but approximately – the same signs linked to the same concepts… (Saussure, CLG, § 2: 13)

Linguistic variation is the only experience and the only data available to the linguist, yet from that world of singularities the linguist (e.g. Saussure) produces determinate signs. How? As Harris argues:

> A classic example is Daniel Jones's account of 'the phoneme'. In his *Outline of English Phonetics* (1962) he states as a basic fact: 'No two persons of the same nationality pronounce their own language exactly alike' (§55). He goes on to mention various reasons why this is so, including regional provenance, 'educated' and 'uneducated' speech, and 'individual peculiarities for which is it is difficult or impossible to account'. His introduction of 'the phoneme' is intended to explain how, in spite of these many differences, they all manage to speak 'the same language'.

> In order to do this, Jones sets up two related theoretical manoeuvres. One is to postulate what he calls a system of 'cardinal' sounds, defined by reference to extreme positions of the organs of speech. In ordinary conversation, no one actually uses the system of cardinal sounds, but every sound a speaker utters can be measured, according to Jones, by approximation to one of the cardinals. (He gives them numbers, even while

admitting that there are an infinite number.) For Jones, what makes phonemics ‹science› is this ultimate possibility of physiological measurement.

The other theoretical manoeuvre is to introduce the concept of positional variant, or 'allophone'. Certain allophones, he claims, are regularly substituted for others in certain phonetic environments. (Why this should happen is not explained.)

Between them, the phoneme and its allophones explain how all the different pronunciations we hear are actually pronunciations of sounds of 'the same language', in spite of not sounding alike. Nevertheless, Jones sacrifices consistency when he admits that there are cases where it is difficult to prove whether or not a phonetic distinction is phonemic (e.g. §466). (Roy Harris, email to David Bade, 13 September 2011)

'The view that 'everyone speaks differently but somehow they all manage to communicate'' Harris wrote in that same email, 'sums up succinctly the problem encountered in orthodox linguistics'. For Lund, semantic indeterminacy entails incomprehensibility and therefore 'the propositions of integrationism are themselves indeterminate and thereby incomprehensible' (Lund 2012: 19). Linguistic indeterminacy, however, does not mean that what is said/written by someone cannot but be 'unintelligible' to someone else. Signs are *indeterminate* because there will be as many integrational processes involved as there are hearers/readers. What these people take an utterance/sentence to mean may well be very similar. No integrationist has ever said that what integrationists say/write will not be subject to sign-identification and sign-making on the part of their hearers/readers – quite the contrary.

The indeterminacy of the sign is nowhere more obvious than in scientific debates regarding terminology and nomenclature. In a

recent study of terminology related to libraries and information science, Morales López (2008) provides page after page of definitions for various terms. He discusses dozens of definitions of 'Information science' and 'informatics', devoting forty-five pages to a discussion of the history of the use of these two terms, concluding that there was never any consensus on the meaning of either term. He quotes an authority on the subject who declared in 1971 that both terms referred not to any determinate science but to a combination of different disciplines and that the meanings of the terms 'varied according to the authors consulted' (175). Thirty years later, he declares, there is still no agreement on the meaning of these terms, yet their use continues unabated and everyone seems to think they know what they are talking about!

It is clear from the definitions that Morales López quotes that when each of the authors used the terms 'informatics' or 'information science' they each meant what they each meant and not what the other authors meant. The definitions Morales López discusses are in many ways similar, it is just that each of the information scientists quoted keep arguing about what is different – and therefore wrong – with other definitions. And this is the essence of the integrational understanding of the indeterminacy of the sign: when *I* say/write something *I* mean by *my* words what *I* mean, and it is that meaning that *I* wish the hearer/reader to understand and not someone else's, or a dictionary's, meaning. This is not egocentrism; *I* have something to say and *I* want *you* to understand *me*. *I* make linguistic signs, gestures, *I* use objects in the vicinity, *I* enact something, *I* restate *my* point in other words, *I* define my terms and *I* argue with my interlocutor when *I* believe that he/she has misunderstood what *I* mean. And all of those activities I engage in not because I am enclosed within my solipsistic (or 'egocentric') world but because I am trying to communicate with someone else.

With his insistence upon the inherent and objective meaning of the sign, Lund effectively banishes all persons involved in any

communicative act as irrelevant. He misunderstands and misrepresents the integrationist position when he claims that the thesis that *signs are made* implies 'egocentrism: the only thing that the sign is *qua* sign, is what *I* make it to be' (Lund 2012: 16). Not so, for integrationism insists that the communicational relationships among persons is the only reason for the existence of signs at all.

Lund (2012: 20) stresses the importance of 'unambiguous communication' and 'utterances [which] are completely understood'. However, is there ever a moment in our lives when we have the impression to have understood (or have been understood) 'completely'? We don't think so, but this does not mean that successful communication cannot be attained. Likewise, integrationists do not presume to communicate 'unambiguously' with one another. In fact, Adrian Pablé has had to read some passages in Harris' writings again and again (and still struggles with understanding some of them). By reading passages one finds difficult on various occasions one gets the impression of understanding something 'better' or 'more clearly' – hardly, however, 'completely'. The belief that certain communication situations are examples of 'unambiguous' communication stems from extrapolating seemingly unambiguous words and phrases from the continuum of activities in which uttering these words is embedded.

In the realm of language, does yes always mean 'yes'? Does no always mean 'no'? Obviously they do not, since in the preceding sentences both are used as metalinguistic notations. Since words do not always mean the same thing and, as our own experience with mass media and political discourse notoriously proves minute by minute, they (i.e. we) mean now one thing, now their opposites or nothing at all, it cannot be that the meaning of the sign be associated solely with the sign, excluding both sign-maker and sign-interpreter as well as time and circumstance. Every speaker/reader of any language knows that, but Harris alone pursued the theoretical

implications of that indeterminacy further than any other linguist or philosopher ever had.

The linguistic sign does not have an eternal meaning that forces itself equally upon speaker/writer and hearer/reader, for if it did *I* could never mean anything except what the sign imposes (as Whorf claimed). Nor would misunderstanding and mistaken attribution be possible: misunderstanding is a common phenomenon precisely because the act of interpretation – construing something as a sign and making it mean something – is involved in every act of hearing/reading. The sign itself – could it actually exist 'in itself' – could not care less whether the result is communication or miscommunication. Clouds, words in dictionaries, and all signs considered independently of communicating beings never attempt to communicate anything; weathermen, lexicographers and peasants from Aosta do attempt to communicate, and integrational semiology makes this cloudy issue perfectly clear.

3.3. Thesis 3. On epistemological relativism

> It is evident that this radical contextualist approach can be turned against itself. If it is the case, as Harris states in the proposition, that 'there is no universal dimension of rightness that applies to all statements alike ….' this must, according to Harris's own contextualism *stricto sensu*, mean that he is stating a particular truth which is only valid in the situational context, or which only applies in the context in which it is stated. Is this really Harris's intention because that would boil down to only a trivial statement? On the contrary, what he means is, of course, a generalized truth that not only applies in the situational context. But this goes against Harris's own doctrine in which all truth (even his own just stated) are context bound. And again: 'there are no statements about

the past which must be regarded as true ….' What kind of status does this proposition have? Is it only true relative to the situation? If so, it is uninteresting and idiosyncratic. If not, he has committed a blatant self-contradiction according to his own premises. Here we see Harris, the logician, at his worst. It does not seem that he knows about a Cretan who said that all Cretans were liars: how about the permissive relativist who said that all forms of life are valid – including those which absolutise themselves and condemn relativism? (Lund 2012: 28)

Lund claims that Harris, when talking *qua* integrationist (discussing communication or, as it were, history), states 'generalized truths' which are context-independent. But this is to misinterpret the very idea of sense-making as integrationists understand it. To be sure, an individual may claim that something is an 'absolute truth', and he/she may refuse to discuss it with others: however, this certainly goes against the Socratic principle, dear to Harris (2009b), that everything needs to be subjected to critical scrutiny, including one's own beliefs. Every linguistic statement, in fact, is *sponsored*: for knowledge to exist there needs to be a *knower*, whose knowledge is the result of a context-bound integrational activity. 'What I believe/know to be true' does therefore not have an atemporal status: rather my knowledge/belief is always contextualized and subjected to my integrational proficiency by those who stimulate my thoughts, whether in speech or writing. It is thus always possible for someone to disagree with what integrationists say or write: this, in turn, should not be taken as a warranty for absolute freedom of interpretation, the latter not being a corollary of freedom of speech (Harris 2009b: 115). Hence, neither is 'integration', which constitutes the bulwark of a Harrisean semiology, exempt from the biomechanical, macrosocial and circumstantial constraints on communication (and self-communi-

cation). Harris himself must have subjected the axioms of integrational semiology to critical scrutiny innumerable times (and still does), as any responsible language-maker (and language-master) ought to. This does not 'relativize' Harris's conviction that integrational linguistics constitutes the right (and perhaps only possible?) way of thinking about language and communication. But that conviction is not context-independent: it is *made* every time Harris sits down 'in his armchair' and thinks about integrationism. Harris is not an absolutist – he is an intellectual waiting to be challenged by people asking intelligent questions about integrational semiology, able to prove his theory wrong; hence his question at the end of the *Introduction to Integrational Linguistics*: 'Does the orthodox position in turn provide a basis on which to subject the assumptions of integrationism to critical scrutiny?' (Harris 1998: 150). It seems that those who have tried so far have failed to convince him. It is likely that Lund's critique will not impress him either.

It is important to underline that Harris is not attempting to silence everyone else in linguistics; nor is he desperate to convert unrepenting segregationists into integrationists. In fact, the present authors do not know of any case in which Harris pushed a non-integrationist to embrace integrationism – on the contrary: unlike many other founders of linguistic schools, Harris never was interested in recruiting followers to increase his integrational troops. Harris waits for people to approach him, as the present authors did. Integrationism is not something you embrace because it offers interesting prospective academic careers, or because it allows you to fully devote yourself to data-collection and not bother about theory: one has to make integrationism one's own and take responsibility for that choice.

Charges of relativism and poor logic simply miss the whole point of what Harris is arguing about with his focus upon the 'lay speaker'. Returning to the encounter with the peasant in the Italian Alpine

village that Lund dismissed at the beginning of his article: we suggest that what Harris realized as a consequence of this encounter was that there is no socially shared knowledge (made visible by being communicated) which supposedly transpires when dialectologists explore lay people's perceptions of varieties of language. Harris eventually realized, as he would later elaborate in his book *After Epistemology* (Harris 2009), that knowledge and beliefs never exist in any determinate (underlyingly given) state: being confronted with that question in that particular situation, the rural informant took Harris' question seriously by giving an honest answer: besides the fact that dialectal 'sameness' is not just a matter of geographical variation (but also a generational concern), more importantly, 'sameness' is not an issue as far as understanding (as part of first-order communication) goes. 'Sameness' and 'non-sameness' might be an issue in other communicational situations, such as when the natives of one locality, when amongst themselves, make fun of the speech of people from another locality. It might well be that jocular remarks about 'words they use in the next valley that we don't use here' were part of the old man's linguistic experience, but he may have felt that this was not what the dialectologist interviewing him (i.e. Harris) wanted to hear. At the level of communication, at any rate, the researcher's question did not seem to point to any real concern for the informant. What Harris must have realized at the time, therefore, was that linguistic inquiry *had to be* 'lay-oriented' in the sense that linguistic knowledge (including dialect perception) is not collective, i.e. existing as something extraneous which the individual has access to in virtue of being a member of a particular collectivity. Questions about language are highly personal, i.e. they should be asked – not to a *representative* of a group – but to a *person*, who has his/her own (i.e. unique) linguistic-communicational biography. Moreover, he may also have realized that 'sameness' is not a semantically determinate concept, and therefore unsuitable for the purposes of a scientific metalinguistics.

If Lund wants to charge Harris with clinging to a form of *subjective relativism*, that is fine. What is important to understand, however, is that adopting a Harrisean stance does not lead to a solipsistic view of the individual: it is our responsibility as social beings to communicate with other individuals. Without language there is no hope for the individual to progress, advance his/her knowledge, understand his/her rights and responsibilities – we are social beings, and language is a social ability: we do not possess language in the first place to talk to ourselves, but to talk to our fellow-beings. It is correct that in *The Linguistics of History* (Harris 2004) Harris objects to the idea of linguistic absolutism in the sense that there are no linguistic statements (about the past) which are 'absolutely true'. But as he goes on to say:

> …it does not follow from this that the truth is whatever we declare it to be. Playing off rigid absolutism against please-yourself relativism is the last thing that will help us make sense of history. (Harris 2004: 221)

Precisely because signs are *radically indeterminate* we are called to use language responsibly, which requires us to understand that our experiences are nobody else's experiences (and thus *they are relative*); but it does not follow from this that our experiences, being unique, are absolute (in the sense that they cannot, and should not, be subjected to further reflection, discussion, and possibly modification, rectification, etc.).

4. Concluding remarks

If we examine the three integrational theses in light of an informed view of Harris's chairs and predecessors, we can see both how and why Harris broke with all of his predecessors and arrived at

the three theses that Lund rejects. All three theses are unacceptable for Lund because in Lund's semiology, no human beings ever mean anything – only sound waves, ink and other natural objects do.

The attraction of integrational linguistics for the authors of this article is precisely that it is the only linguistics, the only philosophy of language, the only semiology that we know of that had its origin in and has continued to develop on the basis of attention to the experience of the language-maker. *Signs are made to mean, not found ready-made*; language exists only because people want to communicate with someone. The origin of language is therefore not in the discovery of some objective meaning inherent in some arbitrary thing, but in making signs in order to communicate with others. That *signs are indeterminate* is a necessary consequence of understanding signs as the result of creative communicative activity. Every other linguistic theory that we know of begins with an abstraction and disregards all linguistic experience that has not been previously theoretically constructed. Among the many linguistic experiences that Harris has attended to are orthographic variations in manuscripts, public debates about brand names of toothpaste, public debates about good and bad English, terminological debates in the scientific literature, debates about the meaning of art, courtroom debates, historical debates, philosophical debates, linguistic debates, and the villager who claimed to be able to communicate with the inhabitants of his neighbor village despite all the linguistic variation that characterized his region. In all these cases and debates Harris has shown that *what is signified is what the participants construe as having been signified*. Precisely as a result of that orientation towards the world of our actual experience Harris has developed a linguistics appropriate for a world of facts that are debated because their existence is in fact debatable, a world not at all like a dictionary, nor like one in line with a Lundian semiology in which everything comes already made and

provided with a definition that has nothing whatsoever to do with time and circumstance, and much less with you and me.

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