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Clearing the ground: Foundational questions once again

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1. Introduction

Surveying all of the commentaries on our target article (this issue) reminded us what a diversity of opinions there is in contemporary linguistics. A few of the commentators were sympathetic to our position (*Janney, Brandt,* and *Buck*). The rest were largely critical, and from widely varying perspectives. Our task in replying is made more difficult by the realization that whatever delights some of our commentators will predictably infuriate others.

The strategy we adopted for our reply was to follow the original organization of our target article and pick up comments or criticisms where they intersect our path of argument. The article began with a critique of encodingism, and a good deal of criticism understandably centered on the incoherence argument against foundational encodings (section 3). Even for those who found our case against encodingism credible, our presentation of interactive representation as an alternative conception led to many questions and criticisms (section 4). For those who clear the hurdle posed by the interactive conception of representation, there are additional questions about the interactive treatment of language: why the additional machinery of situation conventions and utterances as operators on those conventions? Why deny the usual distinctions between syntax, semantics, and pragmatics? (section 5). Why was categorial grammar selected as the starting point for understanding the conventional decomposition of utterances as operator forms, instead of an apparently more congenial tradition such as Prague School functionalism? (section 6).

Our critique and proposed revision of model-theoretic semantics seems to have provoked little comment. Maybe those who favor model-theoretic semantics for natural languages (and none of the commentators, even *Robering,* are willing to mount a strong defense) are more concerned about the

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arguments against encodingism and the semantics–pragmatics distinction. Our treatment of a few examples did not elicit much of a response either. *Leinfellner-Rupertsberger's* critique of our account of 'pronouns of laziness' is unfortunately tangled up in her misunderstandings of interactive representation (see section 2). Robering, in fact, cites Kaplan's (1979) treatment of indexicals as a useful extension of model theory, while completely overlooking our discussion of it on pp. 422–424 of the target article. (Robering also cites Bennett, who is not treated in the target article but is discussed in detail in Bickhard 1980a). But then our examples were few and schematic, and the commentators may have rightly sensed that the soundness of the underlying principles of interactivism take precedence over their application to the examples.

The interactivist program is an attempt to ground linguistics in epistemology, psychology, and sociology. The account of interactive knowing is a 'materialist' and functionalist one, ultimately based on a variant of abstract machine theory. We consider linguistics to be a science. The foundations of our approach have come under fire for varying reasons. The positivists think we are not scientific enough (section 7). The adepts of various Continental philosophies, such as hermeneutics and deconstructionism, think we are missing the point about language by trying to do science at all (section 8). Critics from various camps have accused us of an illicit dalliance with behaviorism (section 9). Other critics consider interactivism to be mistakenly aiming, not at an empirical science of language, but rather at an analytic science, treating linguistics like a branch of logic or mathematics (section 10). We will never be able to satisfy all of the contradictory demands of our commentators (what would delight the positivists would disgust the deconstructionists, and vice versa), but we make our best effort to explicate and defend our research program.

An issue that we devoted only one page to in our target article has come back to haunt us. This issue is the limits of formal approaches to language, and the emphasis in our article on interactivist revisions and extensions of categorial grammar and model theory has led many of our critics to suppose that we believe that formalisms like algebraic logic are entirely adequate to account for everything about language. In fact, interactivism leads to the conclusion that important aspects of language use are strongly resistant to any currently available formalism, so we try to give this issue a more adequate treatment in section 11. We conclude in section 12 with some meditations on the continuing importance of foundational questions to the study of language.

First of all, however, we must consider some critics who have so seriously misunderstood our position that they are not really engaging the interactivist position about anything. Readers who are interested in the more important issues raised by the commentators, such as the status of the incoherence argument against encodings, the problem of linguistic idealism, or the limits of formalization, may want to skip this section.

2. Two erroneous interpretations

Leinfellner-Rupertsberger does not seem to have grasped the basic tenets of the interactivist model. The problems begin with the concept of representation. She claims that our "exclusive emphasis on pragmatics leads to a neglect of those essential features of language which distinguish linguistic communication from other forms of representation" (p. 492) and that "examples of representation" include "an utterance used in a social situation (p. 413)". On p. 493 she states that "socially used utterances may become representations with truth values". We do not understand how Leinfellner-Rupertsberger arrived at this interpretation, especially when pp. 412–413 of our article expressly deny that utterances can be representations. Utterances are operators on situation conventions. The *result* of their operation on a situation convention is what can have a truth value.

At an even deeper level, she misrepresents the scope of our argument against foundational encodingism. "They tacitly assume that everything that is true of message encoding is also true of referential encoding – and this is rather doubtful" (p. 493). Our argument against foundational encodings applies to any instance of them, referential or otherwise, and is expressly meant to do so. If there are grounds for doubt here, Leinfellner-Rupertsberger needs to say what they are. "How do we interact socially if utterances and language never encode, that is, on Bickhard and Campbell's view, if there is neither referential encoding, nor, as a result, empirical or text reference, not even for terms (p. 424)?" (p. 494). Errors multiply. First, she assumes without argument that reference must be encoding. Second, she assumes that interactive representations have no way of getting any representational content ("We may, indeed, utter socially embedded utterances, i.e., produce representations without knowing what they represent", on p. 493). Third, the whole point of our discussion on pp. 424–425 is that pronoun reference can be accomplished by context-dependent differentiators and does not require any encoding relations.

She persists in assuming that reference requires encoding: "Instead of the authors' tunnel vision according to which language does not encode at all we get a modified position: ... words, however, must (referentially) encode and be able to refer" (p. 496). Still no reason given. On page 498, we are again said to "exclude reference". Finally, at the end of the commentary, we encounter some recognition of our real position: "We do need cognitive representations, but we also need a – relaxed – notion of referential encodings, since we need reference. *Or we have to theoretically divorce encodings from reference*" (p. 502, our emphasis). Of course, recognizing that reference is possible without encodings – which was our point all along – undercuts nearly all of the argument in Leinfellner-Rupertsberger's commentary.

Probably because of her failure to understand the incoherence argument against foundational encodings, Leinfellner-Rupertsberger has no sense of what interactive representation is supposed to be. Trying to cast interactive representation in terms of words and lexemes, or *langue* and *parole* is a mistake – not just because of the difficulty of making such distinctions compatible with the interactive account of language, but because it ignores the nonlinguistic representation. Stating that interactivism introduces a "pragmatic conception of truth with all its attending problems" (p. 3) is not helpful, when no elaboration of this claim is made, and no counterarguments are presented. Given the emphasis on potential interactions in our framework, one could call our conception of truth "pragmatic" – but it has little to do with Peirce's or Dewey's conception.

Still another fundamental error in the commentary is the assertion that the interactive account of knowledge seeks to replace accounts of mind with accounts of social interaction. "The authors do not say explicitly where their views on interaction come from. But it is useful to know that they are nothing else but a variation of sociology's (e.g., G. Mead's) concept of social and symbolic interaction" (p. 493). Wrong. First of all, this claim conflates an account of the interaction between the knowing system and the environment with an account of social interactions. Second, Mead and his followers never addressed the central question of interactivism: How the organism is in epistemic contact with the world at all. By positing symbols, Mead presupposed an answer to that question. Third, our account of *social* interaction has been influenced by Mead (Bickhard 1988a), but his focus was on the perceptual and symbolic consequences of actions, not on internal indications of potential further interactions (not all of which need be social, by the way). Maybe this confusion between interactivism and symbolic interactionism gave rise to Leinfellner-Rupertsberger's charges of behaviorism (section 9 below).

Further distortions arise in her account of situation conventions, a key concept of our framework. "On p. 418, however, they contradict this (and themselves): Situation conventions are said to be constituted out of webs of interactive potentialities; that is, like all potentialities they lie, in a sense, in the future" (p. 497). What we actually said was:

> "Utterances do not encode structures of possible worlds, but they do operate on organizations of (representations of) possibilities – possible further interactions – and herein lies a potential similarity between Montague grammars and the interactive approach to language. Utterances operate on situation conventions, and situation conventions are constituted out of (relationships among) webs of indicated interactive potentialities – webs of interactive relevancies of the interactive implicit definitions of the persons involved in the situation." (p. 418)

Her construal ignores our references to *representations of* possibilities, and to *relationships among* webs of *indicated* interactive potentialities. Representations and indications of potentialities are in the present, not in the future, and there is nothing in this passage to contradict our claim that utterances operate on situation conventions.

Finally, Leinfellner-Rupertsberger invents something called the pragmatic context, attributes it to interactivism, and artificially divorces it from the linguistic context. "If, contrary to the authors' view, the pragmatic context is not all-important, what may take its place?" (p. 500). "The authors now do what they are not really supposed to do, they replace the pragmatic context with the linguistic context" (p. 500). Well, there is no such thing as a pragmatic context in interactivism. The alleged distinction between pragmatic and linguistic contexts ignores our discussion of "semantic structuring" on pp. 422–423, which clearly introduces the institutional linguistic context as part of the resources of the institution of language. Elsewhere we have discussed the 'linguistic situation convention' (Bickhard 1980a). In fact, the situation convention can be extended, indefinite as to its participants (as in writing), and so on. One of the themes of language development is the ability to communicate to broader and broader, less and less specific, audiences and contexts – as in writing. Leinfellner-Rupertsberger's interpretation bears no relation to what we actually wrote.

Stamenov's commentary also poses considerable difficulties. It is just as laden with errors as Leinfellner-Rupertsberger's, but they are harder to analyze and respond to, for lack of any consistent pattern that we can discern. So we will have to limit ourselves to some examples of mistaken interpretation.

Beginning with basic issues, Stamenov maintains that an 'encoding' could represent something without our clearly knowing what it represents – as in the case of a patient reporting a symptom (p. 534). This has nothing to do with our conception of an encoding. It conflates natural and nonnatural 'meaning'. We never attempted to deny tht there are non-epistemic kinds of encoding, for instance, stretches of DNA coding for proteins. When we speak of foundational encodings, we are speaking of purported *mental* representations of something in the *environment*. In consequence the claim that "B&C are inconsistent in giving up [the] correspondence theory of meaning and truth, but not giving up the idea that something is [an] encoding insofar as is it represents something else and the epistemic agent must know what is represented" (p. 534) does not make a great deal of sense.

Stamenov keeps assuming, without giving any arguments, that representation must be encoded and structural. "Exactly from [the] formal-structural point of view, a high level of correspondence is required, in order to guarantee the effectiveness of language as instrument of social communication and interaction" (p. 536). This bald assertion ignores our entire point about functional relationships not requiring specific structural relationships. Subsequently he declares that "the price paid is in the form of splitting [the] processual aspect of language from the structural (representational) specification of knowledge" (p. 536). Once again he assumes that representation must be structural. Besides, we do not regard this split between linguistic operators and interactive representations as the slightest bit costly. It is a deep source of explanatory power capturing something that couldn't be captured before.

When responding to our treatment of utterances as operations on situation conventions, which makes precisely this split between linguistic operators and interactive representations, Stamenov begins chasing his own tail. He claims that "Operations [on situation conventions] are constituted out of [the] individual's representations" (p. 536), which is clearly not our position. "If syntactic structures are structures and differentiations of operations, if utterances operate on social conventions as constituted in the individual's representations, and if sentences are operative forms (= types of operations?), [then] the outcomes become operations on operations ad infinitum, without the possibility to reach representations themselves. ... Everything becomes not a representation but an operation on a representation ..." (pp. 536–537). The conclusion is a non-sequitur from the antecedents: "if utterances operate on social conventions as constituted in the individual's representations" is part of his own sentence, yet the "conclusion" denies it. Besides, the conclusion is false about the interactive model: He ignores his own statement earlier in his own sentence, and he ignores the entire attempt to model the emergence of representation out of system organization in interactive systems. The vicious cycle of operations on operations is entirely a product of Stamenov's confusion.

One more example: "The important thing to be aware of is that the properties of representations with truth values become associated not with sentences or utterances, but to the supposed fit (or misfit) between them and the social conventions which form the 'contextual outcomes of utterances' (p. 413)" (p. 537). Here is our actual wording:

"From the interactive standpoint, however, utterances are *operations* on situation conventions, which are, in turn, constituted out of individuals' representations. Such operations can *generate* new situation conventions, with new representations, which may have truth values, but neither the utterances themselves, nor the operative forms (sentences), are themselves bearers of truth values. They are *operations on* representations, not representations per se. On the other hand, a possible social, communicative *use* of an utterance might be precisely to construct a representation (with a truth value)." (p. 413)

"The interactive approach, then, removes properties of representations with truth values from sentences or utterances per se and locates them in the *contextual outcomes* of utterances, and it locates issues of potential operative use – of operative power – in sentences. In the interactive approach, the supposedly semantic issues of representations with truth values become part of the supposedly pragmatic issues of outcomes and usages of utterances, while the supposedly pragmatic issues of the social operative use of language becomes part of the supposedly semantic issues of the operative power – the meaning – of sentences. The standard conceptions of semantics and pragmatics, thus, divide up and group the properties of languge in ways that are committed to the encoding approach. They are not theory-neutral ways of defining the subject matter (Bickhard 1980a); they are not mere descriptions." (p. 413).

One would not know from Stamenov's discussion that we were trying to show that the issues involved in the definitions of semantics and pragmatics don't divide up in standard ways within the interactive perspective. He seems to be taking a point that holds within that framework of discussion and raising it to constitute the whole of the model – particularly when he construes truth values of representations solely in terms of their relationships to social conventions.

It is unfortunate that Stamenov's commentary is so clouded by distortions of the interactive approach, because some genuinely interesting issues get obscured by them. On p. 538, he makes some critical comments about computational models of intelligence, citing Penrose (1990). Interactivism provides a thoroughgoing critique of computational models of intelligence, of which only the most basic, the incoherence argument against encodings, figures in our target article. Among the others are the need for timing in real-life representation, and the inability of current computational approaches to provide timing (Bickhard and Richie 1983). For a full presentation of our critique of computational models, we direct the reader to Bickhard (in press b), Bickhard and Terveen (in preparation) and Campbell and Bickhard (in press). Stamenov also discusses cognitive linguistics (pp. 541–542). We agree with the guiding principle of cognitive linguistics that language cannot be treated as autonomous and that underlying cognition places significant constraints on the organization of language. But contrary to Stamenov's contention, cognitive linguistics remains committed to encodingism.

3. Critique of encodingism

At the very heart of the target article is our argument for the incoherence of foundational encodings, with its corollaries, the impossibility of novel encodings, and the inadequacy of factual or nomic correspondences as the basis of representation. As our critique of standard approaches to linguistics, and our press for an interactive alternative to them, are ultimately grounded in the incoherence argument, it is obviously appropriate for our critics to scrutinize the argument closely.

Various responses to the incoherence argument are possible. One could argue that linguists are not committed to encodings in the first place, so the argument is irrelevant (section 3.1). One could argue that encodingism has already been overthrown and that there is no need to turn to interactivism as an alternative (section 3.2). One could defend encodingism against the incoherence argument (section 3.3). One could try to convert the incoherence argument into a more familiar and acceptable argument, like Wittgenstein's argument against a private language (section 3.4).

3.1. Is encodingism real?

If no one really believes in encogingism, refuting it is a waste of time. *Bogusławski* asserts that encoding conceptions of representation are chimeras of our own invention. Bogusławski's counter is silly. Explicit affirmations of encodingism abound in philosophy. There is hardly a clearer or more classic statement of encodingism than Wittgenstein's ([1922]1961) picture theory of meaning. Artistotle's conception of *νοῦς* getting forms impressed on it by the environment, or Descartes' worries about what innate ideas correspond to in the world, are also classic instances of encodingism – and we have considered only philosophers whom Bogusławski cites. In contemporary cognitive science and artificial intelligence, explicit avowals of encodingism are commonplace, for instance in Newell's (1990) influential version of information-processing psychology or in Fodor's (1981, 1987b, 1990) explorations of propositional attitudes, psychosemantics, and nativism. The foundation of model-theoretic semantics is Tarski's ([1936]1956) scheme for rendering the meaning of quantifiers in encoding terms. Moreover, we are not saying that encodingism is declared by all practitioners of standard linguistics. In many cases it is merely presupposed, showing up in the alternatives that are seriously considered or dismissed out of hand, and in the problems that follow from a commitment to encodings but are merely mysterious and persistent from the practitioner's standpoint.

A charge made by *Kasher* is therefore also rather silly: We are not claiming that Chomsky, Fodor, and others are aware of the problems of encodingism, and have resorted to their nativist formulations in order to dodge them. Nativism is an inadequate response to some of the derivative problems of encodingism, notably the impossibility of novel encodings; if Chomsky and Fodor were aware that the problem is a consequence of encodingism, their response to it would probably be very different.

3.2. Is encodingism already dead?

We think, then, that encodingism is real enough to deserve a critique. Though sympathetic to our critique, *Dascal* denies that it is really new (p. 456): Rorty (1979), Wittgenstein (1953), Winograd and Flores (1986) and others have criticized 'representationalism' already and safely dispatched it. We have paid close attention to these and other partial critiques of encodingism (those of Dreyfus, Searle, Harnad, Shanon, Maturana, Piaget, etc.) None of them, to our knowledge, has stated anything like the argument that foundational encodings are logically incoherent (Bickhard 1980a). Indeed, we have pointed out the incompleteness of some of these critiques, such as Wittgenstein's (Bickhard 1987), Piaget's (Bickhard and Campbell 1989), and Searle's (Bickhard and Richie 1983). Nor have these critics proposed anything like interactive representation as an alternative. Partly in consequence, none have fully escaped the traps that encodingism lays.

Like many of today's critics of symbolic approaches to cognition, Dascal thinks that connectionism, which treats representation as parallel and distributed and rejects the standard view of thought as serial symbol processing, is not vulnerable to the encoding critique (p. 456). We disagree. Connectionist networks do not act in the world, and hence are not capable of interactive representation. Whereas symbols in standard approaches to artificial intelligence encode via correspondence relations stipulated in advance by the programmer, connectionist networks form their correspondences over the course of 'training'. But the only sense in which connectionist networks are currently taken to represent anything is that they succeed in corresponding with aspects of the environment. And a representation defined in terms of correspondences – whether stipulated or trained – is an encoding. Meaning still has to be attributed to connectionist networks by their 'keepers', as Newell (1990) puts it. Connectionist encodings are not atomistic in the manner of encodings in standard AI, but they provide no better explication of the nature and origins of representational content than do the encodings in standard models. (For more about connectionist approaches, and the changes that would be required to make interactive representation possible within them, see Bickhard and Terveen, in preparation.)

3.3. The incoherence argument

Whereas *Dascal* thinks the incoherence argument is unnecessary because encodingism is dead, others are still striving to defend encodingism.

Meseguer wants to limit the scope of our critique of encodingism. Specifically (p. 506), he is willing to acknowledge that encodingism is invalid in the 'cognitive world' (the world of abstraction) but maintains that words encode aspects of reality in the 'physical world' (the world of what is perceived) and the 'affective world' (the world of feelings and bodily sensations). Collapsing any serious distinction between language and thought, as encodingists often do, he claims that in the physical and affective worlds there are straightforward relations of designation; for instance, "in the psychic world, nouns, verbs, and declarative sentences ... appear in our mind as having a vague analogy with things, actions and states of things of the physical world" (p. 505). To claim that there are such relations of designation, be they vaguely or sharply defined, is to claim that nouns, verbs, and declarative sentences are foundational encodings. Meseguer gives no arguments of his own; he offers no response to our argument that foundational encodings cannot be coherently defined; indeed, he never says what our argument is.

Meseguer seems to take our argument (p. 507) as applying only to encodings that are defined by one-to-one correspondences. "We can now conclude that the authors' theory fully applies also in the other two worlds [physical and affective], with the exception of [the] domain of proper names: when Bickhard and Campbell say that 'proper names such as John and Mary ... do not encode' (p. 423), they are going too far ... the link between language and reality is biunivocal only in the narrow field of proper names; in the rest of the fields, the relationship between language and reality is not biunivocal, but doubly univocal" (p. 507). It does not come as a surprise that Meseguer would find our denial that proper names are encodings to be especially objectionable; the strongest intuitions of encoding always hold for the good old-fashioned naming relation. But the incoherence argument applies against proper name encodings just as it applies against any other sort of foundational encoding. Moreover, the incoherence argument applies against any attempt to define representation in terms of correspondences, because in all such cases it is necessary to know what is on the other end of the correspondence relationship. It matters not a whit whether the alleged correspondences are isomorphic or homomorphic (for more about proper names, see section 4.3).

By contrast, *Kasher* forthrightly claims that the incoherence argument is invalid. It is important to realize, though, that he makes an error in his formal rendition of the encoding relation; Given an encoding relationship R, an encoded element e from the set E, and an encoding element c from the set C, then $e = R(c)$ is *not* the representational content of c (p. 477); it *provides* the representational content of c. *Kasher* conflates use with mention; he conflates the representational relationship with the stand-in relationship. '...' does not represent the character 'S'; it stands in for it, and thereby represents the same thing that 'S' represents. Stand-in relationships are transitive; representational relationships are not.

Kasher then attempts to defend Fodor's (1975) conception of a 'language of thought' and reject the incoherence argument. "Assume RED is an element in the Language of Thought that represents a causal feature of certain objects (under certain conditions). It is a feature of objects that is thus represented, not a word in any language. Let us assume that our brains embody this representation relation. Where is the intrinsic circle?" (p. 478) The circle lies in the assumption that our brains embody a representational relation. "When semantics rests on a core of causal relations, the foundational representation relations involve no intrinsic circularity, no vicious incoherence, no pressing need to look for an alternative" (p. 478). But the problem is exactly *how* any epistemic relationship could 'rest on' causal relationships. Causal relationships are everywhere. Epistemic relationships are not. Besides, the circularity is blatant in Kasher's own reasoning: Taking a causal relationship as an encoding relationship presupposes that the system knows what is on the other end of the causal relationship (what the correspondence or the causal relationship is with). Yet the 'causally resting' encoding was supposed to provide that knowledge in the first place.

We might add that Fodor himself has expressed doubts about the adequacy of grounding encodings on causal relations: " ... of the semanticity of mental representations we have, as things now stand, no adequate account (1990: 28); "not every situation encodes the information that it contains" and "we haven't got the ghost of Naturalistic theory about" encodings (1987: 87).

Hertzberg declares that the incoherence argument is "rather vague" and that it may not "get at the problem" (p. 462). His example of a foundational encoding is the sentence 'This is red'. Actually, any novel representation – in a new theory of physics, or in the course of child development – would be an even better example, because advocates of encodingism like Fodor (1981) admittedly have no way of accounting for the origin of a novel encoding. All that can be learned in a strict encoding approach is a combination of already present encodings. In any case, "Why should it not be open to [the encoding advocate] to support this claim by saying that all we know, of any given physical object, [is] whether or not it is red, without being aware of any more basic fact about the object" (p. 462). No further knowledge of what is encoded is supposed to be presupposed. Problem: the example presupposes that what is known is a color! Encodings cannot be defined without knowing what is at the other end of the encoding relationship.

Bogusławski acknowledges the regress that results from having to define encodings in terms of the other representations they stand in for. But he thinks he has stopped the regress and arrived at a 'non-circular conceptual analysis' by positing semantic primitives. But positing semantic primitives doesn't help at all. The semantic primitives must be foundational encodings, and how are they to be defined? Moreover, *Bogusławski* is stuck with the impossibility of evolution for his semantic primitives; and he has the additional problem of countering Fodor's (1981) arguments that lexical concepts cannot generally be rendered as combinations of semantic primitives, so they must *all* be primitives, *telephone* and *quark* and *democracy* and the whole nine yards. The only alternative is to give up on identifying anything at the other end of the encodings for them to refer to, and to treat the supposed encoding representations as comprising what is known, in other words, to fall back on idealism.

Idealism is always a danger for linguists. There is always the temptation to expand the undoubted importance of language for human beings into a claim that all knowledge takes linguistic form, or, even worse, to claim that language is all there is. Bogusławski ends up casting his lot with idealism: "Inasmuch as what we pursue is insight and understanding, we can only make a stop at normal linguistic expressions, expressions we begin to use, quite successfully, in the earliest days of our conscious life. We cannot make a stop at the stage of those scientific formulas: these are incomprehensible without the assistance of the former kind of expressions which, for their part, do not require any 'physical, chemical or biological' assistance in order to be understood" (p. 443) "In the beginning was the Word" (p. 445). Presumably we can just do away with psychology and epistemology because "know about ____ that ____" is a semantic primitive (p. 444). We will return to the dangers and temptations of linguistic idealism in section 8.

3.4. The incoherence argument vs. the private language argument

A final response to the incoherence argument is to try to replace it with a different but more familiar argument. *Hertzberg* (p. 463) wants to replace the incoherence argument with an argument against absolute signs (those that have meaning independently or in isolation from a socially constituted system of signs). In other words, he wants to convert it into Wittgenstein's argument against a private language (*Leinfellner-Rupertsberger* (p. 491) simply treats the two arguments as equivalent without further ado). There is an affinity between the incoherence argument and the argument against absolute signs: A differentiator with only one final state and no alternative final states implicitly defines 'the world' and so cannot provide any differentiating information to guide further interactions. Note, however, that the need for more than one possible final state arises at the purely functional level, and is by no means specific to the level of linguistic meaning, notwithstanding assumptions to the contrary that trace as far back as Saussure and probably farther. In any case, having a position in a system of signs does not solve the problem of representational content, so Hertzberg's recasting is not as powerful – it is not only less general, but it loses one of the central points of the incoherence argument.

The various objections that the commentators have raised against the incoherence argument are worth addressing, but none of them has made a dent in it. If foundational encodings are indeed impossible, then we need to consider alternative conceptions of representation.

4. Interactive representation

Although we believe that the incoherence argument is new, many of the criticisms of the encoding conception have been around for a while, and some of them are gaining wide currency in philosophy, cognitive science, and linguistics. By contrast, interactive representation is definitely new, makes use of a number of unfamiliar concepts, and got a very compressed presentation in our target article (for far more detailed presentations, see Bickhard 1980a, b, in press b; Bickhard and Richie 1983).

Some commentators are largely sympathetic to the interactive approach, and we will begin with a brief response to them (section 4.1).

4.1. Some clarifications of interactivism (for those already sympathetic)

Janney, Brandt, and *Buck* are largely sympathetic to the interactive standpoint. We would like to thank them for their careful attention to our proposals, especially in their compressed form, and head off some minor misunderstandings, many of them undoubtedly due to the issues that got insufficient discussion in our target article.

Brandt has a strongly operative conception of language: "any situation is ... already a modal creature" (p. 436) and that "doing ... is re-modalizing, modifying a modal situation" (p. 436). However, his conception of representation is not so clearly interactive, involving "schemes" and "icons" and even "schemes showing temporal processes" (p. 436). Careful – to whom are the temporal processes shown? If there has to be a full-blown active agent, a homunculus, interpreting the schemes, then we have fallen back into encodings (encodings have to be interpreted; interactive representations do not). We are also a bit concerned about Brandt's endorsement of catastrophe theory, an approach to dynamic modeling that makes certain metrical assumptions, but is otherwise indifferent to the underlying ontology of what is being modeled. We believe that attention to psychological and linguistic ontology is crucial for progress in these areas of endeavor (Bickhard and Campbell in prep.).

Buck's conception of knowledge by acquaintance is very much like interactive representation. We believe he is correct in his surmise that interactive representational content is similar to Gibson's (1979) affordances. Gibson, however, does not really discuss how affordances are represented. We will not dwell on this question here because we have previously offered an interactive interpretation of Gibson (Bickhard and Richie 1983). We are somewhat concerned, however, about Buck's view that "communication proceeds in two simultaneous streams: one learned and propositional, the other biologically based, emotional, and based upon phylogenetic adaptation" (p. 451). If the streams are really independent of one another, then the "propositional" stream has to make use of propositions not based on affordances or on knowledge by acquaintance. In other words, the propositional stream has to incorporate foundational encodings, and is therefore vulnerable to our arguments against them.

A related problem arises when Brandt (p. 437) proposes that "in a first approximation, it seems reasonable to postulate two sources of meaning, perception and interaction". Perception should not be divorced from interaction; it is a species of interaction, as Buck has recognized. Again, there is detailed discussion in Bickhard and Richie (1983).

We commend *Janney* for his careful and insightful interpretation of our work. His summary of the interactive approach bears repeating: "The conceptual focus is not on *what* specific knowledge of the world is represented in the mind (and is, according to the encoding metaphor, sent from mind to mind), but on *how* knowledge of the world arises in the mind, how this is functionally related to interactional choices in different situations, and how such choices come to have intersubjective significance to partners" (p. 469).

In fact, Janney has gone so far as to fill in a piece of the interactive framework that we (perhaps carelessly) left out of the target article. He finds it necessary (p. 469) to differentiate between *global assumptions*, which delineate the individual's total realm of interactive possibilities, and *situational knowledge*, the individual's tentatively constructed understanding of the possibilities for interaction in the current situation. (The distinction appears again in his figure 1.) In Bickhard (1980a) this same distinction was developed and treated at length, although the terms used there were *world image* and *situation image* (not as good as Janney's, because images may sound encoded or static).

On p. 472, Janney criticizes us for taking goals for granted, for assuming that goals are given in advance. We shouldn't be doing that, of course. Although we did not undertake it in our target article, we have attempted a naturalistic, nonreductive grounding for functional analysis and goal-directedness (Bickhard 1980b; much amplified in Bickhard in press b). For this reason, too, we are uncomfortable with claims about "the radical subjectivity of thought and perception" (p. 468). We have also responded in the past to Fodor's critique of Gibsonian affordances (Bickhard and Richie 1983).

Finally, we have undertaken a variation and selection account of the construction of new interactive system organization and new interactive representations (Bickhard 1980b, 1988, 1991b). The process that Janney likens to Peircean abduction is not interactive implicit definition or differentiation (these characterize knowing, but not changes in knowledge through learning or development). It is construction by variation and selection: "Partners interpret each other's behavior, and project the potential consequences of their own interactive possibilities in the situation .... The correctness of particular interpretations, and the appropriacy of particular interactional moves, is determined largely by whether these prove helpful to the respective partners in reaching their goals in the situation" (p. 471).

Janney likens interactivism to Varela's epistemology (p. 467, n. 3). We are not up to date on Varela's work, but we are familiar with his mentor Maturana's, and we need to issue a cautionary note about that. Maturana and Varela (1980) claim that the nervous system is closed, that system and environment do not truly interact; 'structural coupling' does not exist from the system's point of view, as it goes about its self-contained activities, but only from the viewpoint of an observer. Thus, although Maturana has correctly identified some of the problems of encodingism, his response is to cut off the unknowable environmental ends of encodings and adopt a form of subjective idealism, rather than make the move to interactive representation. Interactivism gets rid of the observer; knowledge has to be knowledge *for the* *knowing system*, and the ability to be an observer must be explained rather than presupposed (for further discussion, see Bickhard and Terveen in prep.). We do not want to overstate our case, however. A new account of color vision by Thompson, Palacios, and Varela (in press) propounds an 'enactivist' account of perception that seems very close to the interactivist one, and may have broken with subjective idealism.

4.2. Interactive knowing systems

Most of the commentators had major difficulties with our conception of interactive representation, and presented major objections. Let's begin with our root characterization of interactive systems.

We characterize systems using mathematical models of process, specifically automata theory (Bickhard 1980b). We presume that readers, even those unsympathetic to naturalist accounts of knowledge, will pay serious attention to our efforts to describe internal system organizations, instead of reducing our position to an "activation account of cognitive processing" (*Tyler*, p. 546), or demeaning it even further to "the foundational operation of a simple light switch" (p. 547). Tyler also denies the possibility of purely internal goals, without providing any reason for his rejection.

Tyler goes so far as to reject the very distinction between system and environment as "the archaic opposition of inside and outside" (p. 547). We are not quite sure what he means. Does Tyler wish to reject biology as a science – after all, it draws a distinction between organism and environment, and tries to identify where one ends and the other begins? Or is his aim simply to reject the relevance of biology to explaining cognition and language? Moreover, Tyler insinuates that adopting the organism/environment distinction implies that "the activity and form of the inside originates in the activity and form of the outside" (p. 547). Such crude empiricism in no way follows from adopting a biological perspective, and it is definitely not characteristic of interactivism (besides our target article, see Bickhard et al. 1985; Bickhard 1991a).

Dascal (p. 456) thinks that the interactivist critique of encodings is "clearly not as radical as Rorty's, insofar as it does not espouse either the latter's antifoundationalism nor a complete demise of all forms of representations". Indeed, we do not reject representation; we are proposing a different account of it. But it is important to realize that interactivism is thoroughly antifoundationalist, as far as our conceptions of knowledge and rationality are concerned. There is no absolutely certain foundation for knowledge in interactivism (no Cartesian innate ideas, no semantic primitives, no incorrigible sense data, nothing that behaves the way foundational encodings are supposed to); knowledge is always fallible and defeasible; and rationality consists primarily in the progressive elaboration of heuristics for variation and criteria for selection that avoid error. On the other hand, we are not advocates of skepticism or idealism (Bickhard 1991b). We think that interactivism is more radical than throwing up one's hands at the untenability of encodingism and declaring that knowledge is impossible.

Somewhat later in his commentary, Dascal makes a separate argument that "thoroughly functionalist accounts of content run into well-known difficulties, such as those pointed out by Fodor's (1987b) critique of meaning holism and conceptual or functional role theories" (p. 458). A detailed response to this charge would take us far afield. Suffice it to say that, although interactivism is a functionalist approach, it does not fit Fodor's conception of functionalism very well. The issue of meaning holism does not arise for interactivism, because interactive representational content can be defined for very simple systems, like *Paramecium*, that have no 'holistic' net (see Bickhard in press b, for a critical discussion of Fodor on representation in *Paramecium*).

4.3. Emergent representation, realism, and the intentional stance

Schneider is sympathetic with our concern about foundational questions, and endorses our critique of encodingism. However, he jumps the rails in his understanding of our proposed alternative. Our account of interactive representation aims to show how representation can emerge from the non-representational. According to Schneider, "the authors intend to use these terms ['information', 'goal', 'control', and 'representation'] in a non-mental, 'materialistic' way, the exact meaning of which would have to be spelled out along the following lines, 'The system S has received the information that p' means the same as 'Under our current interests (to understand the possibility of representation) it is most useful to describe the observed behavior of S *as if it were* an epistemic agent like we ourselves are, about whom we would be justified to say that he or she has received the information that p'" (p. 524).

Interactivism, however, is not an as-if interpretation of system behavior; we are not engaged in an interpretive language game like Dennett's (1987) intentional stance. Instead, we aim to explicate information, goals, control, and representation in functional, control system terms. We claim that a system that is so organized as to interact competently with an environment *really does* have goals and control structures and representation. If it can modify itself in certain ways, then it *really does* learn. If it has a higher level that can interact with the level that knows the environment, it *really does* have consciousness. Critics might consider all of these claims to be horribly wrong, but we do intend our theory to be a realistic one.

4.4. Interactive differentiation

We have characterized the epistemic contact between interactive representations and the environment in terms of interactive differentiation and interactive implicit definition. Interactive representation differentiates between environments in the sense that arriving in state A has different implications for the flow of control in the system than arriving in state B has. In this regard, we must consider *Kasher's* rejoinder: "Consider a chemical ingredient *C* of a pregnant woman's blood. Assume that the level of *C* in her blood has a crucial effect on the effect on the development of the fetus ... the chemical presence of *C* in the woman's blood is a[n interactive] representation, because it is an appropriate source of selection: When the level of *C* is above a certain threshold, the development of the fetus is normal, but otherwise, all kinds of problems might arise. It is a source of selection that permits a goal-directed (fetal) subsystem to reach its (developmental) goal" (pp. 481–482). Kasher's example hardly qualifies as an instance of interactive representation. For whom (what system) does *C* function as a representation? What are the relevant internal states, and what are the possible courses of actions or strategies that get selected among? Moreover, although knowing is goal directed, and many goal-directed systems can modify themselves, it is an elementary error to treat development as goal directed (Bickhard 1988).

As he goes about elaborating his naive encodingism, *Meseguer* raises a valuable issue about interactive representation. Again collapsing the cognitive and the linguistic, he presumes that proper names, or encoded representations of individual entities, are prior to common nouns, or encoded representations of categories of entities. "Each tree is a physical entity that may be encoded with a proper name: *Tree-A, Tree-B, Tree-C* ... Now, linguistic cognition processes these encodings and creates a grammatical unit, a common noun as a generalization of proper names. ... The physical (and the affective) worlds are sets of individual entities that can only be encoded with proper names" (p. 506). From an interactive standpoint, Meseguer has everything backwards. Interactive representation is inherently categorical: An internal final state implicitly defines and differentiates the *class* of environments that would yield that final state. Representing individuals requires additional differentiations; those differentiations are context-dependent; and we never know for certain when the differentiations are complete (pick out only individuals). From an interactive standpoint, a representation of the tree now in front of me is a good deal more basic than a representation of tree A, tree B, or tree C, which have been picked out as individuals in a context-independent manner (Bickhard 1980a, b; for an appreciation of this same issue in artificial intelligence, see Agre 1988).

4.5. Interactive implicit definition

Interactive representations not only differentiate, they also implicitly define classes of environments. Our recourse to implicit definition has drawn fire from *Robering*. His argument is long and detailed, and we will have to appeal to the history of formal logic in order to respond to it, so the general reader may want to treat this subsection as an excursus.

At first we thought Robering wanted to defend model theory against alleged misinterpretations, one of which was our rendition of implicit definition. But his critique is based on a different conception of implicit definition than ours, and in consequence his critique mostly misses the target. In fact, it ends up supporting our critique of model theory. "There is a model-theoretic notion of implicit definability ... according to which an expression R is implicitly definable from expressions $Q_1, \ldots, Q_m$ (different from R) in theory TH if any two models of TH with the same domain which agree in their assignments of entities to $Q_1, \ldots, Q_m$ also agree in what they assign to R. This notion is of no help in explaining how the basic signs of B are acquired by a learner ..." (p. 514, n. 7). Quite true, but the interactivism isn't trying to explain how some set of basic signs is acquired by a learner. We explicitly argue that basic signs are foundational encodings, they cannot exist, therefore they cannot be learned.

Maybe a historical overview would help. The idea of implicit definition originated in the last century with the realization that appropriate axiomatizations of geometry could be taken as implicitly defining the notions of geometry (Kneale and Kneale 1986). It was adopted by Hilbert as a formalist approach to mathematics in general, in which all of mathematics would be so implicitly defined (Moore 1988). It carries on in formal model theory in more refined notions such as those of categorical or monomorphic axioms (Kneale and Kneale 1986) or of an elementary class of models (Keisler 1977). It is this notion of axioms implicitly defining a class of models (Kneale and Kneale 1986; Quine 1966) that is generalized in the idea of interactive implicit definition.

There is also a deeply related notion of the implicit definition of a term in an axiom system by its position and role within that system, and a proof that the possibility of such an implicit definition implies the possibility of (a somewhat ad hoc) explicit definition (Boolos and Jeffrey 1974; Kneale and Kneale 1986; Quine 1966). The interactive notion of implicit definition is not the definition of a term, but this theorem is nevertheless of relevance in showing that implicit definition is not intrinsically less powerful than explicit definition (Quine 1966) – when explicit definition is possible at all. It is this last point, of course, that is at the core of the issue: such explicit definition is possible only in terms of already available representations – it is an encoding definition – and, therefore, cannot serve any fundamental epistemological functions. Explicit definitions cannot yield new representational content or new knowledge; they can only yield stand-ins for representation and knowledge already available. Implicit definitions can.

Robering claims that "... implicit definability (in the sense explained) coincides with explicit definability" (p. 514, n. 7). The theorems and proofs that

Robering is referring to presuppose other *already interpreted* languages in which the explicit definitions can be given. This class of theorems does not address the basic epistemological issues that we are concerned with any more than does Tarski's (1936/1956) rendering of the semantics of one language in terms of that of another. In fact, the reliance of model theory on already interpreted languages is a problem that we discussed in the target article.

"What B&C seem to envisage is a version of the doctrine of implicit definability according to which the meanings of the basic signs from B determine mutually each other by simultaneously fulfilling a set A of conditions (axioms)" (p. 514, n. 7). What we actually said was: "The outcome defines a set of environmental states, but the definition is completely implicit. In fact, the relationship between the outcome and its environmental set is an interactive version of model theoretic implicit definition – the sense in which a logical system implicitly defines its class of models ..." Robering ignores the entire explication of interactive implicit definition, along with the key phrases "an interactive version of" and "the sense in which a logical system implicitly defines its class of models". And once again, we are not trying to implicitly define "basic signs".

"But this doctrine is incoherent: A (first order) axiom system (e.g., that of group theory) does not implicitly define the basic expressions of the language in which it is formulated (in the example given: does not attach a meaning to the circle 'o', which is intended to denote the group operation) but explicitly defines a second-order relation (in the example: that between a domain G and a binary operation o on G such that G is a group with respect to o)" (p. 514, n. 7) Here, Robering is still focusing on the implicit definition of terms – in this case, the group operator term 'o'. Though the example is unclear, our best interpretation is one in which the group axioms do not define 'o' so much as they define a relation between G and 'o' so that a pair in that relation constitutes a group. The relevant relationship for us is that the group axioms define what it is to be a group, and, therefore, implicitly define the class of groups. So "defines a second-order relation ... between a domain G and a binary operation o on G such that G is a group with respect to o" may be an *explicit* definition of the relation between G and o, but it is an *implicit* definition of the class of groups.

In the interactivist approach, we do not want or need such explicit definitions for our implicit definitions because we are not defining terms or expressions, and what counts as an *interactive version* of an implicit definer is a functional organization, which does not have to be interpreted at all, but rather is simply the organization with which a process proceeds. It is that organization of (potential) process that implicitly defines, and it itself is not an explicit definition at all, of anything at all.

The relevance for our purposes of the term- or expression-focused implicit definition theorems cited by Robering is that, in situations in which explicit definability is possible, implicit definition is just as legitimate as explicit definition. For epistemological purposes, though, explicit definability is encoding, and gets nowhere at a foundational level. There is no already interpreted language, no Fodor-style language of thought or Montague-style intensional logic, in terms of which the encodings can be explicitly defined. That is where the power of implicit definability makes all the difference. Robering has things backwards: The term- and expression-based equivalence between implicit and explicit definition (when explicit definition makes sense) blocks the denigration or dismissal of implicit definition.

In fact, a closer examination of Robering's defense of model theory suggests that he is no happier than we are with the uses of it that we were questioning. "Now, the notion of an intended model is not a technical notion of model theory: it presupposes that the language in question is an interpreted one, that its expressions have been given meanings in advance" (p. 516). Yes, this is true of mathematical model theory. But it is also a concise statement of the inability of model theory to provide semantics in an epistemic sense. Later, Robering seems to want to dismiss the epistemic employment of model theory: "A widespread misunderstanding of model-theoretic semantics which seems to have influenced B&C is summed up in the popular description of, for example, Davidson's version of formal semantics (among others), but the model-theoretic approach is more general and employs not the concept of truth but the relation of being true in a model" (p. 517). To stop committing this "widespread misunderstanding", all of the purveyors of model-theoretic formal semantics for natural languages, from Davidson to Hintikka to Montague to Cresswell to Lycan to Barwise and Perry, would have had to close up shop.

Contrary to Robering's assertion on page 519, we are not urging the abandonment of the model-theoretic paradigm. We have no quarrel with its use for already interpreted languages, as in some areas of mathematics. We are arguing against its epistemic adequacy, and its use in modeling the semantics of natural language when no attention has been given to its epistemic adequacy.

4.6. Interactive representation and social interaction

So far we have responded to claims that the interactive characterization of epistemic agents or knowing systems is inadequate (4.2), that interactivism is not a realistic theory but is merely taking the intentional stance (4.3), and that interactive differentiation (4.4) and implicit definition (4.4) cannot function the way we say they do. All of these objections can be taken as objections to the interactive conception of knowledge, rather than the interactive conception of language as a conventional system of operations on situation conventions. Further difficulties arise, however, if the interactive conception of knowledge and the interactive conception of language are not clearly distinguished. After all, conventional approaches, which identify knowledge as encodings, and language as the recoding and transmission of encoded mental contents, obey the tendency for levels of encoded knowledge to undergo 'transitive collapse' (Bickhard and Campbell 1989) into a single level.

Some commentators (*Yngve, Dascal, Hertzberg, Kirkeby,* and *Leinfellner-Rupertsberger*) have clearly confused our interactivist account of representation with our view that language is a communicative action system grounded in social interaction. Run together, these two projects become something we never intended: "grounding meaning in social interaction" (*Dascal*, p. 456). Interactivism (which Dascal misrenders as interact*ion*ism) is an explication of the nature of representation. There is nothing social about it, and it is not fundamentally concerned with linguistic meaning. The social interactionism in our approach follows from concerns about social interaction and language, and is concerned with the nature of language as a system of operations on situation conventions. Indeed, the sense in which meaning is grounded in social interaction is quite different from the sense in which it is in the later Wittgenstein, Mead and so on (Bickhard 1987).

Similarly, *Kirkeby* (pp. 487) grossly misinterprets our characterization of interactive knowledge: "B&C ... try to escape from reflection by defining representation as a function of communicative webs of events that do not necessarily imply individual consciousness". The passage he is referring to in our target article reads:

> "Thus, something is an *interactive representation* insofar as it implicitly defines/differentiates something about the environment, and knowledge of what is being represented is constituted as relational organizations – webs – of interactive relevancies concerning such implicit definitions/differentiations." (p. 410)

Webs of interactive relevancies are internal to the knowing system; they are not necessarily social, they are not communicative, and they do not consist of events in Kirkeby's sense. (Kirkeby, p. 487, construes events as "complexes of social actions mediated by reflection", as invariably social and as constituted by the participants' interpretations.) Moreover, an interactive representation is a representation for a particular knower, so it is not true that our conception of representation avoids reference to individual epistemic agents.

Hertzberg (p. 464) also conflates interactive representation with social interaction: "they make the concept of control basic to that of interaction ... The meaning of an utterance, I take it they are saying, in brief, is its efficiency in controlling social situations". The concept of control is part of the basic explication of interaction with the environment and of interactive representation; social interaction, norms, and conventions belong to another level of the theory (see, for instance, Bickhard 1980a).

A distinction that was developed at some length in Bickhard (1980a) is useful here: conventional meaning versus situation meaning. *Situation meaning* is created within social interaction; it is the result of an utterance operating on a prior situation convention. *Conventional meaning* is not grounded in single social interactions as such, but rather emerges within institutionalized social convention; it is the operative power of sentences, as forms of potential utterances, to transform situation conventions.

5. Implications for language studies

Interactivism, if we have understood its implications correctly, requires a major reformulation and reorganization of the way we study language. Language cannot be treated as a recoding of encoded mental contents. (Nor can it be conflated with higher-level understandings of cognition and treated as 'an interactive representation of cognition', contrary to *Meseguer's* (p. 507) serious misreading of our position.) Syntax, semantics, and pragmatics can no longer be distinguished as the study of well-formed encoded messages, the correspondences between encoded messages and what they refer to, and the practical or social uses of encoded messages; the social, pragmatic functions of language have to be given paramount consideration. Commentators had serious objections to these implications. We will start with attempts to salvage language as a transmission of encoded messages (5.1), then move the alleged autonomy of syntax from semantics and pragmatics (5.2), then to the alleged autonomy of semantics from pragmatics.

5.1. Language and derivative encodings

A careful reader will note that interactivism rejects foundational encodings, but does not do away with encodings. Derivative encodings can be defined in terms of prior interactive representations, or iteratively in terms of prior derivative encodings. Moreover, derivative encodings can be useful because they change the form of knowledge so as to make processing easier (as digital encoding does with sound waves, or ASCII codes do to alphanumeric characters).

Several commentators (*Sgall, Dascal*) ask why we can't treat most of cognition, and all of language, as trafficking in derivative encodings. Down at the bottom somewhere are foundational interactive representations, but derivative encodings can be defined on top on them, then linguistic processes can operate on the derivative encodings without 'cashing in' their interactive foundations. If that were possible, then linguistics, after genuflecting to the gods of a new epistemology, could carry right on with business as usual. *Dascal* (p. 457) argues that "it may well be that, though encodings are admittedly 'derivative' representations, that they form *as a matter of fact* the vast majority of the representations we use". We don't profess to know what percentage of the representations used by adult human beings are derivative encodings. But we doubt that most of them are. Derivative encodings cannot do the work that encodings are normally expected to do (Bickhard and Richie 1983). The crippling limitation is that encodings only change the form of existing knowledge, and truly novel representations cannot be encodings – foundational or derivative. The widespread creation of new representations – historically, developmentally, in poetry, in word games, in mathematics, and so on — is a prima facie argument that many if not most representations are not derivative (see also Shanon 1988, for another version of this argument).

When Dascal explains how he thinks derivative encodings are used in cognition, further difficulties arise. "There is evidence that much of our reasoning consists of 'manipulating symbols', i.e., in replacing encodings (e.g., words) by other encodings (e.g., their definitions)" (p. 457). Really? Since Dascal takes connectionsim seriously, he ought to consider the questions connectionists raise about the true frequency of serial symbol processing in human thought. Except when following recipes, how much serial symbol processing do we really do? The classical encodingist accounts of thought given by Hobbes and Leibniz, which emphasize 'lexical decomposition' or the unpacking of concepts into their definitions, run into additional difficulties. Fodor (1981) has argued on empirical grounds that unpacking into definitions is hardly routine in human thought; conceptions of language based on classic eliminative definitions and semantic features have been abandoned (Clark 1983); indeed, the entire view of definitions which supports such unpacking is rejected by natural-kinds theorists (Campbell in press; Keil 1989; Putnam 1975; Rand 1967).

So there is a serious question about the true extent of derivative encodings in thought. And we haven't reached language yet. "But if many *mental* representations are in fact derivative encodings, enjoying the required stability of meaning, why couldn't *linguistic* encodings be encodings as well?" (p. 459). Let's suppose that most mental representations are derivative encodings. It still doesn't follow that one individual's mental representations will show a great deal of structural similarity to anybody else's. If derivative encodings are based on functional, interactive representation, and interactive representations vary considerably from one individual to another, then the derivative encodings are going to vary considerably as well. The only way to get the requisite structural similarity is to have foundational encodings, which would be structurally similar because they are defined in terms of what they encode, and they would encode the same pieces of the environment. Once we turn to an interactive account of representation, there is no way for language to function on the basis of words and sentences encoding other mental representations. Consequently, when *Meseguer* declares that "the main problem of poets is how to describe their personal feelings, which for them are proper names, when what language offers them are just common nouns" (p. 507), he, too, is presupposing structural similarity of encoded mental contents as a prerequisite for communication. We would say, rather, that it is hard for poets to construct a situation convention (a shared understanding) and linguistic operators that can accomplish appropriate context-dependent differentiations of feelings in the situation images of members of their audience.

Dascal (p. 458) also expresses concern about the principle that encoding relationships cannot be defined across epistemic boundaries. Specifically, he wonders whether we have arbitrarily defined an epistemic boundary between "linguistic and mental representations" (p. 458), because, without such a boundary, there seems to be no reason that language could not be an encoding of mental contents. The location of epistemic boundaries between domains of knowledge is in part an empirical question (Campbell and Bickhard, in press). The epistemic boundary between mental representation and language can be established on the basis of the considerations we have already given: Derivative encodings cannot be genuinely novel, and derivative encodings do not make possible the degree of structural similarity between different people's representations that would be needed for language to successfully encode and transmit encoded mental contents. Most fundamentally, however, utterances are in the world and mental representations are in the mind – encodings can't cross between them.

Another, perhaps more familiar, way of stating the epistemic boundary issue is that representations can only affect each other in terms of their functional or causal instantiations, not in terms of their extensional content.

In the contemporary literature (e.g., Fodor 1990), this distinction shows up as 'narrow content' versus 'wide content'. Only narrow content is in the brain, but wide content is what in the world the narrow content somehow specifies. Only narrow contents can have causal access to one another, but the folk psychology of beliefs and desires seems to require that representations be able to affect each other in terms of their wide content. The narrow vs. wide content problem does not affect interactivism because the representational content is functionally present inside the system (in the form of implications for further interactions), whereas what is represented in the world is implicitly defined by that functionally present content (Bickhard, in press b).

Derivative encodings are not powerful enough, nor stable enough across individuals, to serve as material for the recoding and transmission that language is thought to accomplish under standard conceptions. The interactive conception of representation, together with our conception of social interaction, have led us to the distinction between situation conventions, the shared understandings that people develop in order to solve the problem of knowing one another's situation images, and the conventionally decomposable operators (utterances) that transform situation conventions in the course of linguistic communication. One consequence of the interactive approach is that language is radically context-dependent, and the evolution of less context-dependent meanings (such as those of most written texts) is what stands in need of explanation. Another consequence is that the standard divisions of syntax, semantics, and pragmatics no longer hold up. There are no more well-formed encodings for syntax to study, only conventionally decomposable operator forms. Utterances as operator forms are bound up with the uses of language; situation conventions, which get modified by those operators, are what is capable of truth or falsity; so the distinction between semantics and pragmatics can no longer be drawn at all.

5.2. *Is syntax autonomous?*

Kasher takes exception to our view that standard conceptions to syntax define it as the study of well-formed encodings. He defends Chomsky's doctrine of the autonomy of syntax and his nativism regarding syntax learning, insisting they are not instances of encodingism. We only mentioned Chomsky in passing in our treatment of encodingism, preferring to focus on Fodor. There are, in fact, important differences between Chomsky and Fodor, between Universal Grammar and the Language of Thought, and we welcome the chance to explicate our views.

According to Kasher, "Chomsky's reasons for introducing the theoretical conception of an innate 'Universal Grammar' ... had nothing to do with any 'foundational level of independent encodings'" (p. 479). Not so. To begin with, Chomsky has endorsed Fodor's brand of nativism for lexical concepts (Piattelli-Palmarini 1980). That already commits Chomsky to encodingism; moreover, despite his chronic rhetorical slipperiness, Chomsky's appeals to Descartes, Leibniz, and Kant are clear enough to indicate an allegiance to encodingism. As Kasher reminds us, Chomsky's arguments for nativism are different from Fodor's, but we also find them a good deal less interesting. Here is the essence of Chomsky's argument as we understand it:

(1) Syntax is autonomous. Its primitive concepts are irreducible to, and distinct from, those of semantics, cognitive science, or anything else.
(2) Therefore, only the most abstract syntactic inputs can be relevant to the learning of syntax. Context, meaning, intrinsic developmental constraints, and so on, are ruled out a priori.
(3) Given his austere construal of the language learning problem, Chomsky concludes that the 'available information' is insufficient to specify the grammar of the language being learned within the space of abstractly possible grammars.

(4) Because of the lack of available information, he concludes that there must be an innate syntax-learning facility or organ. Otherwise the task of learning syntax would be impossible.
(5) The dedicated innate syntax-learning facility constitutes further confirmation that syntax is autonomous.

After ruling out every possible constraint on the understanding of syntax besides the abstract tree structures of his favored notation (Where does Chomsky get those tree structures? How does the child come by them, either?), Chomsky concludes that syntax learning is impossible and therefore requires an innate autonomous faculty. He impoverishes the learning situation by removing all of the relevant constraints from it, then turns around and claims that syntax learning is critically underdetermined – the "argument from the poverty of the stimulus". This argument is circular, and only a little work is needed to expose its circularity. Fodor's argument rests on deeper errors.

Kasher claims in addition that Chomsky does not regard sentences as encodings. Sentences may not function as encodings for 'autonomous' syntactic processes to operate on. Just what those processes are (if any) fluctuates from one version of Chomsky's theory to another, anyhow. But the assumption that autonomous syntax could have any point for the individual (e.g., by forming part of the individual's 'linguistic competence', or knowledge of language) or in the world unavoidably turns those tree structures into encodings. Calling the Language Acquisition Device a schematism doesn't help. A schematism was originally Kant's (1781/1965) device for recoding the sensory manifold into conceptual information so the categories of the understanding could apply.

Our response to Chomsky obliges Kasher to explain why syntax should be regarded as autonomous by any serious investigator. It also apropos for *Bogusławski*, who asserts that sentences are a basic kind of stuff, to wit, "bilateral, perceptual-intelligible entities" (p. 444) and that they are "primary data" (p. 444) The doctrine of the autonomy of syntax is a variant of encodingism, relies on additional circular arguments, and cannot survive an interactivist critique.

5.3. Is semantics autonomous?

The centrality of situation images and situation conventions in interactivism seems to have led *Kirkeby* (p. 488, "B&C's attempt to revise situation semantics as a basis of a quasi-pragmatic theory of language") to conflate our view of language with Barwise and Perry's (1983, 1987) situation semantics. Certainly situation semantics has improved on standard model-theoretic approaches. There are some interesting convergences with interactivism: a concern with the indexical situatedness, or inherent context-dependence of language; a metaphysics of properties and relations instead of entire possible worlds; and a foundational recognition of the need for partial information of a limited portion of the world. But situation semantics remains committed to encodingism. There is no model of representation (hence, encodingism by default), no treatment of mental or social processes, no account of variation and selection (we have one, even though we skimped on it in our target article), no sense of interactive differentiations as contextually open. In consequence, there is no conception of utterances as operative – situation semantics is intended to account for the content of both language and mental states. From an encoding standpoint, language just encodes mental states, which are themselves encoded, so, as we mentioned above (sections 3.4 and 4.6), any principled distinction between language and knowledge is hard to maintain.

Dascal (pp. 458–460) argues that, even in the face of our argument against foundational encodings, semantics should still be regarded as autonomous from pragmatics because meanings can be treated as largely independent of any functional or interactive basis that they might have. Dascal claims to base this argument on the ubiquity of derivative encodings and the high degree of structural convergence between derivative encodings in different individuals (we challenged both of these claims in section 5.1). There is really no way, given an interactive conception of representation, to treat language as an encoding of mental contents.

There may be an entirely different reason for Dascal's sense that meanings are autonomous: "Semantics, to my mind, is the account of ... relatively stable conventions ... once such conventions become 'fossilized' or 'crystallized', the workings of the communication process change substantially, for utterers not only *can* rely on them to in order to 'operate' on their audiences' mental representations: they also *must* do so, for such stable meanings are now givens of the communicative situation ..." (p. 459). Once again we refer to our distinction between conventional and situation meanings (Bickhard 1980a). What Dascal calls fossilization or crystallization is very similar to the institutionalization of conventional meaning via precedent and habituation. We have no quarrel with the idea that conventional meanings become relatively autonomous from situation meanings. Because this distinction presupposes situation conventions and the operative character of language, though, it will not underwrite a split between meaning and truth on one side and use on the other.

Some commentators (*Kasher* and *Leinfellner-Rupertsberger*) have taken issue with our description of conventional approaches to semantics as truth-conditional. They claim that there are alternatives, in particular approaches based on semantic features (e.g., Katz 1972) or semantic networks. But semantic feature approaches are just as committed to encodingism as model-theoretic approaches, and they have the additional difficulty of accounting for truth values. A collection of words or a collection of encodings doesn't yield truth or falsity. Model theory and its derivatives are the best efforts of conventional semantics. Nothing is gained by appealing to substantially weaker approaches.

In further defense of the standard distinction between syntax, semantics, and pragmatics, Kasher claims (p. 481) that, in standard approaches to language, pragmatic forces can still have a constitutive bearing on the meaning of related sentences, and that pragmatic forces can be given a formal treatment. We have dealt with such formal treatments in the past (Bickhard 1980a). A formal treatment of pragmatics as non-autonomous and subsidiary to syntax is very different from a treatment of pragmatic aspects of language as having a constitutive bearing on central, 'autonomous' aspects of language. Moreover, the very distinction between semantic *content* and pragmatic *force* is an encoding-based distinction, not makable in the interactive framework. Speech act theory, for instance, is a theory of actions that can be performed with encoded propositions.

Robering, too, maintains a distinction between force and content, and devotes several pages to its elaboration. He finally arrives (p. 520) at a commitment-slate model. He is correct in assuming that this sort of model is closer to the interactivist conception than are the models that make a rigid distinction between force and content. But even so, in the commitment-slate model it is encoded propositions that get written on the slates. Which means that, like other current formalisms that we criticized in our target article and in Bickhard (1980a), it has not shaken off its commitment to encodingism. *Meseguer*, though he does not explicitly advocate it, is still committed to the standard distinction between semantics and pragmatics. He is willing to acknowledge context-dependent differentiation in specific cases, such as inferring the sex of a deputy in parliament when a masculine noun is used to refer to that deputy in the context of a secret ballot or a roll call vote (pp. 508–509). But he is not willing to acknowledge that linguistic meaning is context-dependent in general, nor that interactive representation is itself context-dependent (section 4.4, above).

5.4. The evolution of communicative action systems

We remain convinced that the conventional tripartite division of language studies must go, and that an interactive, operative research program in linguistics will give pride of place to considerations which are currently (and somewhat inaccurately) called pragmatic. One other feature of the interactive account deserves mention, even though we did not develop it in our target article. Our conception of language includes an evolutionary sequence for communicative action systems (from prelocutions to illocutions to locutions to linguistic acts; extensively developed in Bickhard 1980a). From an evolutionary perspective, there is much to be said for *Buck's* conception of the primacy of 'spontaneous communication", or, in our terms, of the emotive, motivational, and social grounds for the emergence and character of language. The problem is that Buck's conception of 'two streams', one based on affordances, the other on propositions (section 4.1, above), does not explain how one emerged from the other, how one could emerge from the other, or, for that matter, how any propositionally encoded communication could happen at all. The interactive model, which rejects foundational encodings and accepts only encodings that derive from interactive representation, provides a natural path from simple social affordances as content for communication to more general sorts of content. Beginning with simple social affordances, a process of differentiation and elaboration of possible aspects and contents of situation conventions could operate through referential intermediaries (danger, from the air, not from the ground; from a snake, not a lion), instrumental or locational supports (there's water in this direction), and indications of social status or intent ('I'm allied with him against you').

6. Categorial grammar and Prague School functionalism

Once we view utterances as operators on situation conventions, the conventionalized decomposition of these operators replaces conventional conceptions of syntax. Because of our interest in formalizing the operativity of utterances, including the issue of how to define partial operators, we found categorial grammar a congenial starting point for our investigations. But it is not the only one, nor the best one for all purposes.

On the contrary, when we look at their underlying commitments we find that the closest approaches to interactivism are the various functional conceptions of language. In particular, as *Sgall* rightly reminds us, we have been remiss in not pointing out our affinities with the Prague school. The first draft of our target article was written in 1985 specifically for a book on categorial grammars, from which it was later excluded on account of editorial politics. Also, our American linguistic training gave a lot more emphasis to Chomsky and Montague than it did to functionalism. So we welcome the opportunity to identify the Prague school as the existing school of linguistics which comes closest to our approach; we also find considerable affinities with the London functionalist school (e.g., Halliday 1977).

Like Sgall, and unlike Montague (Thomason 1974) or Chomsky (1975), we believe that language is fundamentally a communicative action system (Sgall calls this its "teleonomic attitude", 1.1), and of course we agree about the interactive character of language and linguistic expressions (1.2). We agree wholeheartedly with his rejection of the standard distinction between semantics and pragmatics, emphasizing the inherent involvement of "pragmatic moments" in the determination of meaning (2.1) and the emphasis on "anthropocentric shaping" of the lexicon and of syntax (2.1). "It should be noted the semiotic trichotomy of syntax, semantics, and *pragmatics*, known from Morris and Carnap ... does not directly reflect three different levels in the structure of natural languages" (Sgall et al. 1986: 11–12).

But as Sgall points out in 1.2 and 2.2 the Prague school still relies on encoded propositional contents: "Truth conditional semantics presents the only safe basis for a systematic description of the semantics of natural languages in its interplay with pragmatics" (Sgall et al. 1986: 10). "It is generally accepted in linguistics ... that the outer shape or the surface structure of utterances can be conceived as a many-to-many encoding of their underlying (disambiguated) representations" (1.2). Although Sgall suggests that many-to-one mappings are involved, with various imperfections and asymmetries, he still wants to treat utterances as encodings of primary interactive representations (2.2) and as requiring the specification of "a piece of cognitive content". Such a position is vulnerable to our arguments (section 5.1, above) about the lack of common encoded knowledge for linguistic structures to encode, and to our argument that linguistic meaning is context-dependent in the general case.

Some other points of difference: Even though the categorial grammar approach is more committed to encodings than Prague school functionalism, algebraic logic allows the development of its formalisms in a manner more congenial to an operative conception of language, whereas we don't know of a comparable mathematical formalism that could be used to move the Prague School approach towards greater operativity. We submitted a chapter for a book on categorical grammars because we knew of no other formal approach that could be readily adapted to deal with operativity, partial operations, and decomposition. Also, as we mentioned in section 4.6 above, interactivism draws a strong distinction (Bickhard 1980a) between *conventional meaning* (the institutionalized transformational power of utterances as operators) and *situation meaning* (the change in the situation convention created by an utterance with some conventional meaning). This distinction is central to interactivism, but we are not sure how it fits into the Prague School approach. None of these considerations makes categorial grammar more worthy of consideration than Prague School functionalism; indeed, we hope to see a renewal of interest in the Prague School.

7. Interactivism meets positivism

In a discussion of foundational issues in the study of language, philosophy of science questions are never far below the surface. In the next few sections (7 through 10), we explicate the philosophy of science that underlies the interactivist program, at least by contrast with the positions taken by various commentators, like positivism and phenomenology or hermeneutics. Let's begin with positivism.

We are much indebted to *Yngve* for pointing out that the study of language, since ancient times, has engaged in a circular idealization: It has latched onto certain aspects of the social institution of language, reified them into formal objects, and proclaimed that linguistics studies only those idealized formal objects. The study of language has thus been cut off from its roots in the study of mind and of social interaction (Bickhard 1980a: 197, n. 1). Among other things, the general form of Yngve's critique is apparent in our response to Chomsky's doctrine of the autonomy of syntax (section 5.2, above). So we are sympathetic to Yngve's general program of grounding the study of linguistic communication in the study of social interaction (though, like *Dascal* and *Hertzberg*, he seems to think interactivism is only about social interaction, and not primarily about mental representation).

But we cannot accept his view of what it means to have a science of language. When he declares that "Science works with only the bare minimum of assumptions, building the rest on the solid support of observation and experiment" (p. 550), he is expounding positivism, and in a rather naive form. When he declares that properly scientific linguistics "is built on a foundation of observationally supported general laws instead of on assumptions capturing or explicating intuitions" (p. 552), he ignores the assumptions involved in the observational categories he is using, and in the generalizations he "founds" on them. This is utterly naive positivism – a theory-neutral data language. Theories account for and are tested by data; they do not 'rest upon' them.

In objecting to our argument for an interactive conception of representation, Yngve says that "Not only must [the interactive model of representation] be free from incoherences and objections, it should at least be falsifiable in Popper's sense". Popper (1965) never made such a claim himself. Falsifiability is Popper's criterion of empirical content, not of scientific usefulness. Popper claimed that science has to involve universal statements, which are falsifiable, but he did not thereby exclude other kinds of statements. Existential statements – like positing the existence of the neutrino in particle physics – play a role in genuine science, even though they are not falsifiable. Programmatic claims that include higher-order existential quantifiers, for instance 'There exists a theory satisfying such and such constraints that can explain these phenomena', aren't empirically falsifiable either. They can still be refuted by logical means, and they can be scientifically useful. Similarly, although we don't see why Lewis's (1969) account of convention becomes unscientific just because it is based on game theory, let's suppose it is. It is foolish to conclude that using Lewis's notion in science is therefore illegitimate. The mathematical theory of Riemann manifolds is unscientific by Yngve's criteria, but its application in Einstein's theories would surely have to qualify as scientific. In fact, Popper expressly acknowledged that science cannot proceed without some framework of 'metaphysical research programs' (Popper 1965; Bartley 1990). He did not share in the positivist rejection of metaphysics that Yngve still clings to.

Contemporary philosophy of science, in looking more closely at what scientists do now and have done in the past, has shown repeatedly that positivism is untenable and that it never correctly described scientific practice (e.g., Bartley 1990; Bickhard in press a; Bickhard et al. 1985; Hull 1988; Laudan 1977, 1984; Shapere 1984; Suppe 1977). Not only Yngve, but a number of other commentators who believe the positivist tales about the scientific enterprise, should take note.

Yngve (p. 551) lists a set of unexamined assumptions that we have supposedly drawn straight out of standard approaches to linguistics – that meaning exists, that language is based on cognition, and so on. But we have rejected the standard treatment of meanings, as based on encodingism, and split the standard notion of meaning into two distinct parts: situation conventions and operations on them. We have also introduced a distinction between conventional and situation meanings. We have massively altered the view of language as based on cognition, from the transmission of encoded mental contents, to a sensitivity to interactive representations implicated in conventionalized transformations of situation conventions (Bickhard 1980a). Perhaps Yngve is concerned that we have not presented the entire interactive framework in a single article, but that is hardly possible, and we have dealt with these matters in depth elsewhere. Similarly we have dealt in depth with hierarchies of emergents, interactive epistemic agents, and the like, in other publications (Bickhard 1980b; Bickhard and Richie 1983; Campbell and Bickhard 1986).

Thus, although, we are sympathetic to many of Yngve's concerns about language, we do not believe that there is anything wrong with doing science as it is normally done, that is, without heeding or obeying positivist strictures.

8. Interactivism meets Continental philosophy

The interactivist approach has come under rather different criticisms from a group of commentators allied with various forms of contemporary Continental philosophy: deconstructionism (*Tyler, Kirkeby*), the later Wittgenstein (*Hertzberg, Schneider*), and phenomenology and hermeneutics (*Kirkeby* again). For these commentators, our interest in formal approaches like model theory, however revisionist, is highly suspect, and our adherence to a functionalist program of accounting for the emergence of knowledge, and for the emergence of language as a form of communicative action system, are more suspect still.

As we mentioned in section 6, our target article began its life as a rather narrow response to categorial grammar and possible worlds semantics, not as a survey of the major conceptions of language. Hence the significant contributions of Continental philosophy, especially hermeneutics and the later Wittgenstein, to the study of language got slighted.

In previous work (Bickhard 1980a, 1987; Campbell and Bickhard 1986) we have explored Heidegger, Gadamer, and Wittgenstein, and pointed out strong areas of agreement with Continental conceptions: Language is social, interactive, and meaning is radically context-dependent. Encoding conceptions of knowledge are fatally flawed, as is the view of language as a system for stating facts by transmitting propositions. The use and understanding of language involve an unavoidable historicity. The historical embeddedness of psychological processes, along with the variation and selection process that is always involved in constructing and modifying situation conventions, results in the hermeneutic circle.

There are also strong points of disagreement, however. The two main ones are our commitment to naturalism and our rejection of linguistic idealism. First, we do not accept the linguistic idealism that lurks in much Continental philosophy and, not all that rarely, finds overt expression: "*Nothing exists except through language*" (Winograd and Flores 1986: 68). We believe that linguistic idealism is fueled by the skepticism that comes from realizing that encodingism doesn't work and can't tie language to reality, but not having a conception of what would work in its place. We have offered an alternative conception of representation, and our evolutionary and developmental concerns make it impossible for us to ignore nonlinguistic knowledge (section 8.1, below).

Second, we do not accept any diremption between scientific studies of the natural world, and the humanities into which no science dare intrude. We believe that language can be studied scientifically, and in particular, that it can be founded on a functional, psychological account of knowing and representation. In rejecting encodingism, we may have turned the conception of representation upside down but we have not thrown it away (section 8.2, below).

8.1. Linguistic idealism: Could mind be a text?

We have noted already a worrisome tendency toward linguistic idealism in some commentators. The degree of idealism ranges in severity from a tacit assimilation of all cognitive representation to language (*Leinfellner-Rupertsberger*), to an explicit assimilation (*Hertzberg, Meseguer*), to a full-blown declaration that language is everything (*Bogusławski*).

We have already discussed Bogusławksi's thoroughgoing idealism (section 3.3, above). *Meseguer's* idealism is more restricted in scope. He maintains that words encode, and thereby refer to, things in the physical and affective worlds, like the sun and hunger, but that "the separation between language and reality in the cognitive world is impossible: (abstract) words like *idea, set* point to referents that have no existence outside of my brain and have no resonance in my body ... *deduction* has no referent outside the linguistic system" (p. 506). Meseguer's criterion of existence outside the brain seems to be an empiricist one: If I can't sense it or feel it, it must not be real. Does the word *atom*, then, have no referents outside the linguistic system? How about *time*? If time is purely 'linguistically determined', as Meseguer proclaims, why should we worry about the role of time in our models of knowing systems interacting with the environment? (see section 11, below). As for ideas and sets and deduction, we have previously developed an interactivist account of abstract knowledge (Bickhard 1980b, 1988; Bickhard and Campbell 1989; Campbell and Bickhard 1986) which we left out of our target article. An interactive knowing system, by itself, is capable of interacting with, and consequently knowing, only the external environment. It cannot know itself, even though it has properties that might be useful to know. A second-level knowing system that interacts with the first, however, is capable of interactive knowledge about properties of first-level knowing. Indeed, an unbounded hierarchy of knowing levels can be defined, each of which knows properties of the next lower level. This hierarchy of knowing levels can be used to define developmental stages and to explicate Piaget's conception of reflective abstraction. More to the point here, it can be used to explicate knowledge of logic and mathematics and knowledge about the mind. Ideas and sets and deduction are quite real from the interactivist standpoint – they belong to lower levels of the knowing system rather than to the external environement. But because under interactivism knowledge cannot be reduced to language, they do indeed exist outside the linguistic system.

A more ambiguous manifestation of linguistic idealism is *Kirkeby's* claim that the mind ought to be regarded as a text, and that it is a matter for serious discussion whether context should also be so regarded. We find the emphasis on text problematic even from a narrowly linguistic standpoint. *Leinfellner-Rupertsberger* (p. 497) suggests that text, and not utterance, should be taken as the unit of analysis in linguistics. The problem with this proposal is that language has to satisfy many constraints, and many sources of constraint. One such constraint is language development, and language development does not and cannot originate at a full text level. When children begin speaking, they produce holophrases or one-word utterances. (Whole utterances, in turn, are preceded by prelinguistic forms of communication.) Textual complexities have to develop out of children's accomplishments and skills with single utterances. Text is to be explained, not assumed or taken as a foundation.

Kirkeby's (p. 483) claim that mind is a text ("we experience the content of our mind as a text, as a body of signs that we must interpret") poses problems of far greater severity. Is the process of knowing a text? We cannot see how – but then perhaps Kirkeby wants to do away with the knowing subject. Well, is to be known to be a text? Our objection is that any conception of mind has to deal with nonlinguistic or prelinguistic knowledge. Many other organisms (goldfish, dogs, apes) have nonlinguistic knowledge. So do human beings; in fact all of us went through the first year to year and a half of our lives without any linguistic skills whatsoever. Just as a theory of mind has to account for the emergence of representation out of the nonrepresentational, so must a theory of language account for the evolution and the development of language out of the nonlinguistic.

Indeed, to characterize the human mind as a text is to define it as an instance of one of its most complex and highly evolved products. It makes as much sense to us to define the human mind as a scientific theory, or as a musical composition. Moreover, to characterize mind as a text, without further explicating how texts come to mean anything, amounts to treating mind as "a body of signs that we must interpret" – in other words, as a bunch of encodings with nothing to encode. Besides, if the mind has to be interpreted, what is going to interpret *it*? There is no way that such a characterization could escape the incoherence argument (an argument which Kirkeby never responds to). In the end, then, we don't subscribe to Kirkeby's paradox of dual textuality because we deny its premise – that mind is a text.

Kirkeby could certainly be taken to be advocating some form of linguistic idealism. It all depends on how Derrida's conception of *la différance* (which surfaces in "one cannot formalize a difference of a difference" (p. 488)) is to be interpreted. If *différance* can ultimately be modeled naturalistically, then Kirkeby is not espousing idealism. If *différance* is not naturalistically modelable, then he is committed either to dualism (mind as text is a different kind of stuff from anything in the natural world) or idealism (language, or text, is all there is). Derrida and his followers are faced with these choices – we just don't know which they would choose.

8.2. Can there be a science of language?

Our attempt to build a science of language on the basis of epistemology, psychology, and sociology is objectionable to most advocates of Continental philosophies, and has consequently come under fire.

For instance, *Hertzberg* seizes on our use of the concept of control in explicating the workings of a knowing system, and jumps to the conclusion that "The meaning of an utterance ..., in brief, is its efficiency in controlling social situations" (p. 464). This ignores our development of the concept of a situation convention (a type of shared understanding) and of utterances as operators on, or means of changing, situation conventions.

Hertzberg objects to the interactive approach on the basis of determinism.

He claims that deterministic approaches cannot explain errors in the use of language, or creative language use. Ignoring his conflation of basic knowing interaction with social interaction, we note that the interactive model explains error in terms of falsified expectations of what strategy to use when a given state is attained, so there is no basis for the claim that we have no way to deal with "erroneous linguistic reactions" (p. 464). Then he claims that there is no way for nonconventional utterances to make sense on a deterministic account. We believe that the variation and selection construction process is adequate to account for nonconventional utterances and for the ability of the participants in a conversation to understand how they modify situation conventions. Hertzberg further equates the interactive approach with claiming that our use of language is behavioristically "under the control of specifiable features of the situation" (p. 464) – interactivism only claims joint determination, and there's lots going on in the minds of speakers and hearers. To go further into Hertzberg's concerns about creativity or spontaneity in language use would require us to develop our variation and selection constructivism, which we only mentioned on p. 428 of our target article (but see section 11, below).

Tyler takes us to task for not rejecting the concept of representation altogether, instead of seeking to replace encodings with interactive representation. His critique, to the extent we can understand it, keeps treating attributes of encodings as though they were basic properties of representations. Representation in "psychology's narratives of perception, memory, and thought" (p. 545) is supposed to center on substitutions of appearances and recurrent presence. Tyler doesn't bother to explicate these notions. But from his statements on p. 464, we gather that recurrence (as required for memory) requires the existence of stable mental objects or substances, and that therefore recurrence is unobtainable in a process framework. Here Tyler simply assumes, like any good encodingist, that representation consists of stable substances. From an interactivist standpoint, what needs to be explained is not change but stability; nothing in the mind can remain the same without a process to maintain its stability (Bickhard 1992).

Deconstructionism gleefully exhibits many of the epistemological dead-ends of encodingism, as we noted in our response to *Kirkeby* (8.1, above). Because the deconstructionists do not have an alternative conception of representation, they are stuck with the skepticism and even nihilism that ensue from the final collapse of encodingism. Deconstructionism needs its chains of encoded signifiers so it can keep pointing to the impossibility of establishing their correspondence to reality.

Nonetheless, there are plenty of interesting arguments in deconstructionism, as Kirkeby has shown. Tyler, however, is not interested in presenting arguments. He resorts to the worst devices of deconstructionism: Clichés like calling everything, including scientific theories, a narrative; deliberately perverse interpretations, like his reference to Aristotle's account of induction as an "ironic narrative" (p. 547), unaccompanied by any reason to take them seriously; and sneering dismissals of concepts that we have carefully explicated (like control structure) as mere 'word magic'. Such dismissals relieve Tyler of the responsibility of dealing with the substance of our position on language – or of anybody's position on anything.

Commentators allied with hermeneutics, deconstructionism, and the later Wittgenstein (who expressly eschewed scientific explanation in his treatment of mind and language) are accustomed to viewing 'scientific' approaches to knowledge and language as positivistic. What has really happened is that they have handed science over to the positivists without a fight. They shouldn't have been so hasty. Positivism does not offer an adequate account of physics, much less of psychology, as any sort of reading of post-positivist philosophy of science would show (section 7, above). In any case, we find it amusing to be accused of positivism, when Yngve, a real positivist, accuses us of being mired in metaphysics!

9. Are we behaviorists?

Partiularly odious to our ears are the frequent accusations of behaviorism, made openly by *Kasher, Leinfellner-Rupertsberger*, and *Tyler*, and implicitly by *Hertzberg*. *Kirkeby* moderates the charge: He considers us merely neobehavioristic. Comparisons with real behaviorist theories, care for historical accuracy, and even elementary reasoning about the implications of interactivism, are conspicuously absent from these assertions. *Tyler* goes so far as to lump together frameworks as disparate as stimulus–response associationism, interactivism and connectionism (p. 546). Since neither of us has ever had any sympathy for behaviorism, we find such charges puzzling, to say the least, but we have tried our best to understand why they are being made.

Kasher, for instance, appears to seize on a single detail of our model – on webs of interactive relevancies, which he turns into webs of associations (p. 482). But under interactivism, no information comes into the mind from the environment; an interacitve representation that indicates possible further interactions is part of an internal flow of control and is not a stimulus–response association in any sense; behaviorism was never concerned with interaction between system and environment; behaviorism never had a model of representation; behaviorism banned any serious investigation into psychological ontology; and the behaviorist language of *S*'s, *R*'s, and hyphens has insufficient computational power to account for human thought and behavior (see Bickhard 1980a, b; Campbell and Bickhard, 1986). The radical differences between interactivism and behaviorism ought to be obvious.

Leinfellner-Rupertsberger is more persistent. After incorrectly assimilating our position to symbolic interactionism (section 2, above), she claims that both "lend themselves to (methodologically) behaviorist approaches. The attraction of the interaction approach lies for the authors ... in the fact that one may dispense with 'the mind' – 'the mind' may be inferred from actions, reactions and responses" (p. 493). On p. 497, she declares that our characterization of situation conventions is inconsistent with our alleged behaviorist strictures: "a frame cannot exist 'empirically'". On p. 501 she assimilates our conception of communication to W. John Smith's account of communication in gulls, and again concludes that we are methodological behaviorists. "In the same way as classical behaviorism the authors consider the mental as a black box; the analysis of the mind (irrespective of whether it encodes or does not encode) has to be replaced by the analysis of social interaction" (p. 501). We find this treatment of our position incomprehensible. If we really considered the mind to be a black box, why would we devote so much attention to what is happening inside it? What is the point of our discussions of system organization? The functional organizations and organizational principles at the heart of the interactive model are not directly observable, so they could hardly comply with the taboos of methodological behaviorism. If it is correct, our critique of encodings, and of the acquisition of encodings from the world by transduction or induction, completely demolishes empiricism and makes methodological behaviorism untenable.

Kirkeby asserts (p. 486) that "B&C apparently want to escape from certain types of logical problems related to the assumption that linguistic and cognitive competencies are founded on reflection. In doing this, however, they fall far back of the more basic advances of modern systems science, into a kind of neo-behaviorism". Since being concerned about the nature of mental representation is prima facie contrary to any species of behaviorism, and Kirkeby is clearly aware that we are attempting to explicate representation, we do not understand why he would call us neobehavioristic. That aside, what are the advances in modern systems science that we have altogether missed? We regret to say that we are unfamiliar with the work of Luhmann, but we learn in a footnote that he "attempts to integrate reflection into cybernetics via the epistemologically sound concept of 'autopoiesis'. Approaches similar to Luhmann's are developed by Piaget in his 'genetic epistemology' and, more recently, by Maturana and Varela" (p. 486, n. 2). Our debt to Piaget is massive; our differences with him are considerable; and we have explored these matters in depth in other publications (notably Bickhard and Campbell 1989; Campbell and Bickhard 1986). We have commented on Maturana's conception of autopoiesis and his subjective idealism in section 4.1 above.

Kirkeby's invocation of Luhmann is puzzling to us in another way. Luhmann is commended for incorporating levels of reflection into his theory. We have had some trouble following Kirkeby's discussion of reflection. But it is clear that is not Piaget's reflective abstraction (under any interpretation that we know), nor is it reflection in the interactivist sense. For us, reflection means consciousness, reflexive knowing, one knowing level interacting with, and thereby knowing, the next lower level, just as the lowest level knows the environment (see our response to *Meseguer* in 8.1 above). For Kirkeby, reflection seems to be *recursive interpretation*, that is, interpretation of the results of prior interpretations. "One simultaneously reflects on one's own personal interpretation of a historically and socializationally relative vocabulary, *and* on the external collective interpretation of this interpretation as realized when one acts" (p. 486). On p. 487 Kirkeby refers to "the interpretational (and most reflexive) web between [speaker] and [hearer]". From an interactivist standpoint, reflection is not the same as recursion; a reflexive interpretation would be an interpretation about other interpretations. There's no point in saying that "linguistic and cognitive competencies are founded on reflection" (p. 486) if reflection is meant in our sense, because that would rule out prereflective knowing. In any case, we have devoted an entire book to reflection in our sense and to levels of knowing (Campbell and Bickhard 1986). We haven't entirely overlooked recursive interpretation either (see Bickhard 1980a; also section 11 below on the variation and selection process of situation meaning construction and on the regress of assumptions behind the situation convention).

In *Kasher's* case, we suspect that Chomsky's rhetorical trick of accusing all non-Chomskyans of behaviorism is continuing to corrupt discourse about language. In *Leinfellner-Rupertsberger's* critique, confusing interactivism with symbolic interactionism may have been partly responsible. For commentators like *Kirkeby* and *Tyler*, who are miles away from Chomsky, the Continental tendency to give science away to the positivists, then to deny in the direst terms that there can be any science of human beings, may be what is at work.

10. Are we mathematicians?

As we have seen, advocates of phenomenology and other Continental philosophies usually react to our project of founding linguistics on psychology as though it were a positivist mission, perhaps even a behaviorist one, based in any case on a narrow vision of empirical science. Kirkeby, however, has taken our remarks about potentially useful mathematical formalisms as indicating that we are attempting an "analytic science of mind, communication, society, and history" (p. 484), where *analytic* is to be opposed to *empirical à la* Kant. He then hastens to deny that any such analytic science is possible. This imputation is seriously mistaken. For starters, we do not accept Kant's (1781/1965) conception of analyticity, because his definition of analytic truth requires encoded propositions whose terms can be lexically decomposed or 'unpacked' into simpler concepts; otherwise it makes no sense to say that the predicate is contained in the subject. Nor do we consider the demarcation between science and non-science to be all that important an epistemological issue (section 7). All science has both empirical and conceptual aspects.

If we take 'analytic science' to mean logic and mathematics, then presumably what Kirkeby has in mind is something like Montague's proclamation that the semantics of natural language is a branch of mathematics (Thomason 1974). It should be abundantly clear that interactivism, despite its recourse to mathematical modeling, is not attempting an analytic science of anything. Interactivism is a framework for psychology, and different theories within that framework are meant to be empirically testable. Those who, like Frege, take the analytic-empirical distinction seriously want to isolate linguistics from psychology; from their standpoint, our approach to language is thoroughly infected with 'psychologism'. In fact, we have argued elsewhere that formal logical systems are inherently deficient as descriptions of the way people think, and we have criticized Piaget, among others, for trying to use systems of logic to model human thought (Campbell and Bickhard 1986).

Surveying all of these ill-grounded and manifestly contradictory accusations of behaviorism, positivism, logicism, antiquated metaphysics, and so forth, we get the sinking feeling that what unites the adepts of particular sects in linguistics is primarily fear of the same *bête noire*. Anyone who comes from the outside and proposes a different approach to language gets assimilated to the universal threat, to whatever could have the most pernicious influence. For the advocates of the later Wittgenstein, hermeneutics, phenomenology, and deconstructionism, anyone who disagrees must be a reductive materialist, a mechanist, a determinist, a positivist – or a logician run amok. For Chomskyans, anyone who disagrees must be behaviorist. For positivists, anyone who disagrees must be intoxicated with metaphysical obscurantism. Such epithets get slapped on any different approach, regardless of its specifics or the merits of its case. Unless linguists undertake a serious study of rival conceptions to their own, there will be little progress in the study of language.

11. The limits of formalization

An issue that we did not give enough attention in our paper is the limits of formalization. We criticize model-theoretic semantics for making untenable assumptions, then propose that certain other mathematical formalisms, such as algebraic logic, have more desirable properties for formalizing the more conventionalized or institutionalized operative aspects of language (or as *Dascal* says, what has become fossilized or crystallized). But there are still temporal and creative aspects of language use that appear to defy formalization, and it is perfectly reasonable to ask how they could ever be formalized. We gave this question a very brief discussion on pp. 428–429 of our target article –

too brief. Enlarging on this issue in the target article might have saved our commentators a good deal of confusion.

First we will consider two distorted renditions of this question. *Leinfellner-Rupertsberger* declares that "The notion that logic may not be able at all to accommodate their approach does not really occur to Bickhard and Campbell. In the same 'logical' spirit, they are convinced that the problem of the origin of encodings is a logical one (p. 404), and that language as the system of all utterances has a formal character (p. 416)". What we actually said was:

"As a system of operators, language has a definite formal character, and this is what most approaches to language have attempted to capture, albeit from the inadequate and incoherent encoding perspective". (p. 416)

She ignores the qualifier "As a system of operators", then claims that we retract the above statement "in favor of a view of language as 'intrinsically temporal and creative' (p. 428)". Moreover, the connection between identifying a logical problem with encodings and promoting the virtues of 'logical' treatments of language escapes us (see also section 10). The full quote is:

"The convergence between interactivism and hermeneutics is even stronger when it is recognized that an utterance cannot *encode* an organization of operators any more than it can encode a proposition. Utterances must be apperceptively *interpreted* for the operations they select – potentially in an open, multiply constrained, non-algorithmic, problem solving manner: the hermeneutic circle. Timeless descriptive formalisms, then, even of suitably modified algebraic logic operators on relational structures of implicitly defined realms of interactive possibilities, can only be approximations. The realities intrinsically, not just contingently, involve the iterative and progressive variations and selections of mental process (Bickhard 1988). Ultimately, language is intrinsically temporal and creative."

She ignores, among other things, the sentence about timeless descriptive formalisms. This is a serious misunderstanding.

Stamenov starts out better: "B&C try to establish some compromise between the way that natural language functions and the ways that different formalized descriptions of its structure function, i.e., they try to equate the structure of the formalized description with the structure of the object described (language)" (p. 533). This is true in a sense. On p. 428, we point out that available formalized descriptions (even those using particularly appropriate formalisms like algebraic logic) can at best be approximations to the creative aspects of language that come about through variation and selection processes. We don't claim to know how far formalization can go. Formalization should be able to capture the crystallized, well habituated aspects of language, but it will have its own intrinsic limitations – unless we can find a formalism that captures variation and selection as such (for our initial investigations in this area, see Bickhard and Campbell, in preparation).

Later, though, Stamenov jumps the rails, referring to "the 'intrinsic creativity' (p. 428) of 'the formal interactive approach to language' (p. 429)". But, as we just said, we are hardly attributing creativity to any of the available formalizations of language. The first quotation in Stamenov's sentence comes from a discussion of the limitations of any formal approach to the interactive model of language, including algebraic logic. The second comes from a completely different discussion – the conclusion of the article. There is no connection between these word strings in our text of the sort implied by Stamenov's juxtaposition of two out-of-context quotations.

Much of the confusion in *Dascal's* discussion of language as an encoding of mental contents (pp. 458–459) can be traced to one of the issues underlying the limits to formalization. In focusing on the closest relevant formalism for capturing the interactive character of language, we largely neglected the underlying process of (roughly hermeneutic) variation and selection construction of apperceptive understandings (Bickhard 1980a). Including the construction of new system organizations by variation and selection would have made a long paper even longer (only a hint appears, on p. 428). (For a detailed discussion of variation and selection as the minimal information epistemic relation, see Bickhard 1992.)

Kirkeby is also concerned about the ability of any formal treatment to deal with intuition and creativity. He identifies the interactivist account of language with model theory – if not Montague grammar, at least a modified version like Barwise and Perry's situation semantics (see section 5.3 above). In doing so, he also overlooks our (admittedly brief) discussion of the limits of formalization. In examining Montague grammar and other systems of model-theoretic possible worlds semantics, our aim was to outline the best possible formal approximation to the operative character of language. Again, we never claimed that model theory, or algebraic logic, or any other mathematical formalism that we know of would ultimately suffice as an account of language. Current formalisms cannot deal with the construction of meanings, especially situation meanings, by variation and selection. Were the variation and selection process to be formalized – probably in a manner new and strange to contemporary linguistics – the approximation would be greatly improved, but even then it might not capture all linguistic phenomena.

Not only are contemporary mathematical formalisms unsuited to capturing the creative aspects of language, but Kirkeby (p. 489) in his discussion of Buber's philosophy of communication, ends up demanding a lot more from the interactive approach than could be gleaned from a single rather narrow article about linguistics. To deal with the issues Kirkeby raises in this brief discussion, we would have to introduce interactive accounts of emotions, moods, attitudes, the self, consciousness, and existential reflections or meanings (see, for instance, Bickhard 1980b, 1989; Campbell and Bickhard 1986). The epistemological and ontological regress of assumptions behind the situation convention has been extensively discussed by Bickhard (1980a). A full, integrated account of the areas we mentioned above is part of our general project for psychology (Bickhard and Campbell, in preparation b) and takes us far beyond the scope of a discussion of linguistic pragmatics. Perhaps, however, Kirkeby believes that we cannot provide a full accounting of human experience and reality because some of the presuppositions of interactivism prevent us from doing it. This is a perfectly legitimate sort of argument, but to the extent we can reconstruct Kirkeby's objections (most notably, his claim that interactivism is trying to be an analytic science, section 10), we believe they fall wide of the mark.

12. Conclusion: Foundational inquiry in linguistics

While reading the commentaries and writing our reply, we were frequently reminded that linguists, like most people, don't like to have their foundations questioned. Formalists, like *Kasher* and *Robering* traditionally don't care for foundational questions or programmatic alternatives, preferring formal analyses conducted according to familiar ground rules. The formalists need to realize that conceptual arguments have a legitimate place in any science; there are always going to be issues that can never be settled empirically. There is no way to determine whether a particular research program is ultimately adequate to its subject matter, whether more of the same kind of theorizing will be able to explain what needs to be explained, without conceptual arguments.

Those who are more humanistically inclined are more tolerant of foundational inquiry, but they are not pleased with any suggestions that linguistics ought to end its splendid isolation. Proposals that make the study of language dependent on anything else, that tie linguistics to epistemology, psychology, and sociology, smack of positivism, reductionism, and other sins of science. But as *Brandt* points out, "The actual situation, at least in modal semiotics, but also in linguistics, a discipline marked by the analytic failures of its immanent, anti-psychological, anti-sociological methodologies, and now threatened philosophically by cognitivistic reductionism, seems to admit and justify foundational questionings" (p. 437).

In any case, it should be clear by now that in recommending a major reformulation of linguistics, we are not proposing a Cartesian program of establishing new axioms, then deductively exfoliating the substance of linguistics out of those axioms. Nor are we proposing to expurgate the entire apparatus of traditional linguistics in favor of psychological and sociological laws that are based, in turn, on a theory-neutral data language. We haven't discarded the entire catalog of linguistic phenomena; we haven't stopped talking about demonstratives, or pronouns, or subjects and predicates. We have already suggested some major changes, not least of them doing away with the usual categories of semantics and pragmatics. And the interactivist research program will no doubt end up reinterpreting or replacing more of the traditional categories as it progresses. As far as we know, that's how science progresses in general; if we find erroneous commitments inherent in a conception (e.g., semantics as currently defined), then it's time to replace it or reinterpret it.

Dascal takes us to task for making claims about "the true nature or the essence of representation, language, and cognition" (p. 456). Of essentialism, we stand guilty as charged. A major assumption of the interactivist inquiry into language (Bickhard 1980a, 1987) is that language, like most other things, does have a set of fundamental properties – namely, being a certain kind of communicative action system. Language it is not just a "Wittgensteinian plurality of language games with no common denominator" (p. 457). Our essentialist claims cannot be ruled out a priori, but they are subject to refutation by conceptual argument.

Notwithstanding the manifold and diverse critiques of our foundational arguments, it is these arguments, and not our rather limited attempts at application, on which our commentators focused. And that suggests that there is a basic core of agreement on which to found our situation convention in this discussion. There are basic questions about the nature of language that must be dealt with and that are not about to go away. The prevailing misconceptions about language have to be exposed and cleared away before much progress will be possible.

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