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**Discussion**

# On some contested suppositions of generative linguistics about the scientific study of language

A response to Dresher and Hornstein's *On some supposed contributions of artificial intelligence to the scientific study of language**

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## 1. Introduction

A recent issue of *Cognition* (December, 1976) contains a paper by Elan Dresher and Norbert Hornstein entitled "On some supposed contributions of artificial intelligence to the scientific study of language". In that paper they discuss the work of a number of researchers, concentrating on papers by Marvin Minsky, Roger Schank, Eric Wanner, Ron Kaplan, and myself. As might be predicted from their title, they conclude that:

> There exists no reason to believe that the type of AI research into language discussed here could lead to explanatory theories of language. This is because first, workers in AI have misconstrued what the goals of an explanatory theory of language should be, and second because there is no reason to believe the development of programs which could understand language in some domain could contribute to the development of an explanatory theory of language. ...Not only has work in AI not *yet* made any contribution to a scientific theory of language, there is no reason to believe that the type of research that we have been discussing will *ever* lead to such theories, for it is aimed in a different direction altogether. (p. 377) [emphasis as quoted]

The editors of *Cognition* invited those whose work was criticized to respond in print. This paper is an attempt to explore some of the basic issues which were raised. On first reading the paper, I had several reactions:

Dresher and Hornstein express a number of specific criticisms of current artificial intelligence research. I find myself in agreement with many of the

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comments which deal with technical details, including some concerning details of my own previous work. However, they make a number of other technical comments with which I do not agree. These lead me to believe that they have not had any experience with the concepts and problems of computing, and this has led to a variety of misinterpretations of the work they criticize.

They adopt unquestioningly and dogmatically a paradigm for the study of language which has been developed and articulately expounded by Noam Chomsky. The real point of their paper is that AI researchers are not working within this paradigm.

They argue their point in a style which is an unintentional caricature of how an established scientific paradigm argues against a prospective competitor. Either they are not familiar with the work of philosophers of science such as Thomas Kuhn* who view science as a succession of paradigms, or they disagree with it so profoundly that they do not even consider the possibility that their methodological assumptions are social conventions rather than eternal truths.

They conclude with an impassioned plea for a recognition of the complexities of human language. I wholeheartedly agree with their point, but it is not a conclusion based on the rest of the paper. It does not address the same issues, or even the same researchers.

There is not sufficient space to explore all of these issues, and it seems most profitable to concentrate on the deeper significance of the paper. I will first build up a framework in which to view the paradigm differences between work in artificial intelligence and the authors' stated views of what a "scientific theory of language" can encompass. The more specific reactions listed above will be discussed within that context. I will argue that the currently dominant school of Chomskian** linguistics is following an extremely narrow and isolated byway of exploration. The limitations result

*It has become overly fashionable for anyone whose work is not generally accepted in a scientific field to claim that this is because he or she is engaging in a "scientific revolution" and that all objections to the work are due to a defense by the old established paradigm. However, even at the risk of guilt by association, I feel that Kuhn's observations apply so well to linguistics (even more so than to the hard sciences for which he originally made his case) that it is of value to point them out, and at least raise legitimate questions about the set of values and methodological assumptions which are taken for granted in current work.

**I am aware that the views expressed by Dresher and Hornstein are not identical to those of Chomsky. In many ways, they abstract and emphasize methodological issues which Chomsky is very careful to hedge in his own writings. However, I feel that their conception is quite close to that of many other linguists, psychologists, and philosophers who have studied Chomsky's writings. It has been noted only half facetiously that Freud would not have been a Freudian. In the same sense, there exists an influential Chomskian dogma, whether or not Chomsky himself would agree to its style, or to all of the conclusions which have been drawn from it.

not from the structure of language, but from a commitment to a specific arbitrary set of meta-linguistic beliefs.

## 2. On the "scientific" study of language

The strongest first impression of Dresher and Hornstein's paper is that it is a prescriptive formulation of how language *should* be studied. They place their major focus on statements like [emphasis added]:

> In this paper, we will show that, contrary to these claims, current work in AI does not in any way address the central questions that *any scientific inquiry* into language *ought* to address. (p. 322)
>
> We have just seen that for a theory of language to be of scientific interest, it *must address itself to*... (p. 329)
>
> If Winograd's question is to be of linguistic, or more generally, of scientific interest, an answer to it *must address itself* to the principles of UG. (p. 333)
>
> The attainment of Schank's professed goal of creating "a theory of human natural language understanding" ... *is impossible if* it is not carried on in the context of a study of the principles of UG... (p. 337)
>
> ... the requirements of a scientific theory of language *can only be met by*... (p. 355)

It is clear that they are not concerned with debating specific aspects of the analysis proposed in the papers they criticize. They are not arguing that a specific theory or set of theories is wrong, but that the entire enterprise is misguided in its very foundations. They return again and again to the criticism that the approach is not "scientific" and does not provide "explanatory" theories.

As Kuhn (1962) and others have pointed out, arguments about what is "scientific" and what is "explanatory" are characteristic of debates between alternative paradigms.

> But paradigms differ in more than the substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific", others that were previously non-existent or trivial may, with a new paradigm become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. – Kuhn (1962), p. 103.

In this light, Dresher and Hornstein's attack can be viewed as a clear statement of the ways in which the work in artificial intelligence does not operate under the set of accepted standards of the "normal science" of language as currently practiced by the followers of Chomsky. I would agree with almost all of Dresher and Hornstein's pronouncements on how work in artificial intelligence strays from "the scientific study of language" if they were translated according to the following rules:

1. The prefix "mis-", and related words such as "wrong" are replaced by the word "different".
2. The word "scientific" is replaced by "Chomskian".

This translation would lead to statements such as:

> ...workers in AI have misconstrued [*differently construed*] what the goals of an explanatory theory of language should be. (p. 377)
>
> ...current AI research into language is headed in a wrong [*different*] direction, and it is this research that is unlikely to contribute to a scientific [*Chomskian*] theory of language. (p. 322)

It would be both premature and self-inflating to proclaim that a scientific revolution is under way in which the Chomskian paradigm will be overthrown by the new "computational paradigm" which includes the work currently being done in artificial intelligence. The issues are far from settled, and only a relative handful of people are working within the new paradigm. We may or may not succeed at redefining the science of language, and cannot reasonably claim to have already done so. However, it is increasingly clear that we can identify a coherent and absorbing body of problems and techniques which have the potential to become the central focus for a science of language.

I do not believe that it is possible to provide logically compelling arguments that one or another paradigm is right*, and it is inevitable that most of the people currently working within the Chomskian paradigm will continue within it. This paper is an attempt to provoke the thinking of those who are not committed to either paradigm, and who therefore can act as observers of the rules within which each side plays the game. The following

*"When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. ...the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. As in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community." – Kuhn, 1962, p. 94.

sections are an attempt to describe and contrast the two approaches. In doing this I will quote extensively from the Dresher and Hornstein paper, and from Chomsky's work (primarily *Aspects of a Theory of Syntax* (1965) and *Reflections on Language* (1975)) as well, since he is the most articulate and respected exponent of the current linguistic orthodoxy.

In this comparison, I cannot pretend to be a disinterested observer, but have tried to accurately represent both sets of assumptions, without portraying either of them as logically necessary or objectively verifiable. The evaluation of which is "better" must inevitably be relative to the beliefs and values of the reader.

## 3. The Chomskian paradigm and the notion of "universal grammar"

Dresher and Hornstein's arguments center around the role that "universal grammar" (UG) must play in the study of language. Their definition of that term is rather vague:

> Note that we are using "universal grammar" in a rather special sense. We do not mean to imply that all languages have the same grammar; nor does the term necessarily cover all those features that all languages might happen to have in common. Rather, we are referring to that set of principles according to which all grammars of human languages must be constructed. (p. 323)

At first glance, this makes sense – it labels as "universal" those principles which are necessary for all grammars. However, the term "universal grammar" is indeed being used in a "rather special sense" which hides implicit assumptions about a number of crucial issues. The assumptions are better discernible in Chomsky's more carefully crafted definition:

> Let us define "universal grammar" (UG) as the system of principles, conditions and rules that are elements or properties of all human languages not merely by accident but by necessity – of course, I mean biological, not logical necessity. (Chomsky, 1975, p. 29).

Taken out of context, this definition appears to justify statements such as "a theory of human natural language understanding is impossible if it is not carried on in the context of a study of the principles of UG". By including the entire "system of principles, conditions, and rules that are elements or properties of all human languages" it is hard to imagine how any generalization about languages would not be a part of UG. In its original context, however, this definition is the first step in a neat piece of intellectual legerdemain which gives the illusion that the detailed methodology of Chomskian linguistics must follow logically from any attempt to understand universal principles of language. The major steps are:

**Step 1: Equating "grammar" with "principles, conditions, and rules"**

In any science, it is necessary to define terms with precise meaning. In doing so, there is no stricture against using words whose informal meaning does not correspond to the definition. The fact that the Holy Roman Empire was neither holy, nor Roman, nor an empire does not prevent the combination from being a useful label.

"Universal Grammar" as Chomsky defines it is not "grammar", according to the common useage of that word. The reader who agrees with the vague but reasonable notion that any theory of language must deal with the "principles, conditions, and rules that are elements or properties of all human languages" discovers that in the argumentation that follows, it is taken for granted that he or she has agreed that some kind of "grammar" is the central focus of language study. By then using "grammar" in its more usual senses within the same arguments, Chomsky is able to let a number of methodological assumptions slip by unnoticed, as we will see below.

**Step 2: Isolating "grammar" from the study of linguistic processes**

The next step is to remove from the purview of "universal grammar" all study of the processes and mechanisms which underlie language use. The distinction between "competence" and "performance" is introduced through quite sensible statements about the need to look at language through idealized abstractions rather than trying to deal with irrelevant details of language behavior:

> Linguistic theory is concerned primarily with an ideal speaker–listener, ... who knows the language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors... in applying his knowledge of the language in actual performance. (Chomsky, 1965, p. 3).
>
> We thus make a fundamental distinction between *competence* (the speaker–hearer's knowledge of his language) and *performance* (the actual use of language in concrete situations). ...In actual fact, it [performance] obviously could not directly reflect competence. A record of natural speech will show numerous false starts, deviations from rules, changes of plan in mid-course, and so on. (Chomsky, 1965, p. 4).

The desire for simplification through idealization is quite reasonable, akin to the physicist's desire to study the mechanics of ideal frictionless objects before dealing with the details of a pebble rolling down a riverbed. In this formulation, "performance" covers the details of how the language user behaves in a particular instance, while "competence" deals with those more

universal properties which apply to all instances. But much of the Chomskian paradigm is based on a shift of the scope of these terms, in which all aspects of language having to do with process of any kind get relegated to the status of "performance", with the corresponding assumption that they are not of interest for the theory of UG. Dresher and Hornstein say, for example:

> The scope of a theory of grammar is basically limited to these kinds of concerns, i.e., to the tacit knowledge that a speaker has of the structure of his language – his linguistic *competence*. However, a theory of grammar does not exhaust the subject matter of research into language. In particular, a study of competence abstracts away from the whole question of linguistic *performance*, which deals with problems of how language is processed in real time, why speakers say what they say, how language is used in various social groups, how it is used in communication, etc. (p. 328) [emphasis as quoted]

Universal grammar, which was initially defined as "the system of principles, conditions, and rules that are elements or properties of all human languages" is implicitly redefined as excluding the study of language comprehension and production along with all aspects of language as a means of communication. In the Chomskian paradigm for the scientific study of language, there is an assumption that valid generalizations can be made about the set of sentences judged grammatical by a native speaker, but that it is not possible to form scientific theories of the mechanisms by which people actually use language.

> The study of the development of cognitive structures ("acceptance of rules", in the first sense) poses problems to be solved, but not, it seems, impenetrable mysteries. The study of the capacity to use these structures and the exercise of this capacity, however, still seems to elude our understanding. (Chomsky, 1975, p. 77)

The implied belief that processing is an "impenetrable mystery" cannot be falsified with examples. It is a paradigm-defining assumption about the range of phenomena it is considered acceptable to study. Every science must make such assumptions, in order to provide a limited enough perspective in which to focus scientific effort. But the choice is a matter of faith, not logic. The work which falls in what I have called the "computational paradigm" has as its main focus the study of the capacity to use cognitive structures. In fact, the renouncement of Chomsky's assumption is the central unifying force in the body of work criticized by Dresher and Hornstein, and can be taken as a useful test of whether someone is working within a "computational" paradigm.

**Step 3: Reifying the "grammar" of the language user**

The next step in shifting the meaning of "grammar" is to use it in referring to individual objects, instead of as an abstraction of "the system of principles, ..."

> The theory of language is simply that part of human psychology that is concerned with one particular "mental organ", human language. Stimulated by appropriate and continuing experience, the language faculty *creates a grammar* that generates sentences with formal and semantic properties. (Chomsky, 1975, p. 36) [emphasis added]

A grammar is something which is created by the language faculty of an individual language user. Again, this sentence is a careful blend of technical terms with suggestive commonsense meanings. One of the major confusions about "generative grammar" throughout its history has been due to the dissonance between the apparent meaning of the verb "generate" and the technical meaning it has been assigned. In its straightforward interpretation, the quoted statement above implies that:

In the mind of each person who learns a language there is a mental structure called a "grammar" which was constructed by the "language faculty"

This grammar is used in generating sentences of the language

This interpretation is a clearly understandable, if inaccurate, concept of grammar. It corresponds to people's common sense notions of how a body of rules is learned and applied. There is a job to be done (generating sentences) and a set of rules (a grammar) which tell you how to go about doing it. The rules can be codified, transmitted, and have the kind of existence which justifies talking about "the set of rules" as if it were a thing.

However, "generate" does not mean "produce", as Chomsky has found it necessary to point out again and again.

> To avoid what has been a continuing misunderstanding, it is perhaps worthwhile to reiterate that a generative grammar is not a model for a speaker or a hearer. It attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of a language by a speaker–hearer. When we speak of a grammar as generating a sentence with a certain structural description, we mean simply that the grammar assigns this structural description to the sentence. When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed in some practical or efficient way to construct such a derivation. (Chomsky, 1965, p. 9)

As with "grammar", "generate" is only a word, and he who uses it is free to make arbitrary definitions. There is a clear mathematical relationship

between a formal grammar and the language it "generates". Chomsky's exposition of this concept was a major impetus in creating a whole field of mathematics dealing with formal languages. It puts grammar on the level of a mathematical abstraction, dealing not with the use of language, but with the non-psychological notion of "derivation".

The problem with this redefinition is that it pulls the rug out from under the naive notion of a grammar as a set of rules for "doing something". In the naive model, the set of rules "exists" in the mind of the person who uses them, just as a set of rules for driving "exist" in a law book. Although Chomsky insists that the grammar is not a set of rules used by a speaker in generating utterances, this independent existence of the grammar is allowed to stand unquestioned. As a result, it is assumed that "the grammar of the language user" is a legitimate object of study. The linguist is seen not as "inventing a set of formal rules whose application leads to the set of sentences of the language" but as "discovering the grammar of an idealized speaker of English".

This leads to the error of believing that the form of the grammar reflects facts about the properties of the language user, rather than properties of the linguistic system used in writing the grammar. This would be a reasonable claim for a set of rules which attempted to reflect the actual processes of language use, but is misplaced in the abstract notion of "generative" used by Chomsky. As a parallel, it is clear that we can write systems of differential equations, using Newtonian physics, which describe the motions of the planets. These equations can be viewed as a kind of "grammar" which formally "generates" the orbits we observe. However it would be an obvious mistake to say that the planet possesses a grammar which corresponds to our equations, or that in refining our mathematical formalism we are somehow studying universal properties of planets.

**Step 4: Identifying "grammar" with a formal syntactic system**

Up to this point, "grammar" is being used in a quite general sense which includes the set of rules and regularities which apply to a language. Even leaving out all concern with the actual processes of language use, as was done in step 2, we might still expect it to deal with a variety of issues having to do with the structure of language as it relates to meaning. However, both in linguistic tradition and in the mathematics of formal languages the word "grammar" is used much more precisely. A grammar is a formal system of rules for characterizing a set of possible arrangements of symbols. The two concepts of grammar are quite distinct, and it is a significant leap to believe

that theories of grammar (in the limited formal sense) form a major part of the theory of Universal Grammar (in the sense defined above).

> My own, quite tentative belief is that there is an autonomous system of formal grammar, determined in principle by the language faculty and its component UG. This formal grammar generates abstract structures that are associated with "logical forms" by further principles of grammar. (Chomsky, 1975, p. 43).

Chomsky quite explicitly hedges in making the leap from "grammar" to "formal grammar". But despite his protestations about feeling tentative, a whole school of linguistics has been based on the "autonomy of syntax" thesis. The substance of Chomskian linguistics is the study of syntax – of grammar in its most restricted sense. The body of work, of articles, books, and lectures, deals first and foremost with the detailed rules concerning the arrangement of words into "grammatical" sentences.

My objection is not to the idea that someone would want to study syntax, or even (although I would argue with their scientific taste) that they want to study it as though it had a formal autonomy. The problem is that through the inconsistent use of the word "grammar", this rather specialized concern and methodology has been elevated to the position of being the only "scientific" study of language.

**Step 5: Equating "explanation" with "simplicity of mechanism"**

The final step in narrowing the scope of linguistic science comes in establishing the sorts of explanation which will be valued in the study of formal grammars. In his early work, Chomsky developed several basic mathematical results concerning the correspondence between the "power" of a formal rule-based mechanism, and the classes of "languages" (sets of strings of symbols) which it could be used to describe. Although there has been a good deal of disagreement about the relevance of this theory to the study of human language, the impressive mathematical results based on restricted formal languages have left their psychological mark on the field. Chomskian linguistics has been dominated by a style of research in which the major emphasis is on finding the "simplest" set of formal mechanisms which can generate the grammatical sentences of a natural language.

There is no agreed upon notion of simplicity, and only a vaguely formulated notion of the "evaluation metric" which can be applied to grammars. Much of the debate within the paradigm consists of variation after variation on the formal mechanism, with each change justified by arguments that it allows the language to be generated by a simpler or more regular set of rules and meta-rules. Often, the notion of looking for the simplest mechanism is confused with the notion of finding restrictions on the possible

classes of grammars. It is assumed that constraints on the possible grammars which could generate human languages must correspond to facts about the mechanisms which people bring to bear on learning a language. It is assumed that such constraints "explain" rather than simply describe the properties of language.

As with the definitions above, Chomsky's statement is much more careful than the conclusions which have been drawn by most Chomskians. He talks about "explanatory adequacy" of a theory as the degree to which it accounts for the universal properties of human languages, as reflected in their learnability. He says:

> We are very far from being able to present a system of formal and substantive linguistic universals that will be sufficiently rich and detailed to account for the facts of language learning. To advance linguistic theory in the direction of explanatory adequacy, we can attempt to refine the evaluation measure for grammars or to tighten the formal constraints on grammars so that it becomes more difficult to find highly valued hypotheses compatible with primary linguistic data... the latter, in general, being the more promising. (Chomsky, 1965, p. 46).

The discovery of formal constraints on classes of grammars is indeed one possible approach to finding explanations for the properties of human language. The fact that so far it has been largely unsuccessful is not sufficient proof that it is not promising. But in incorporating Chomsky's modest methodological suggestion into the theory as a whole, it has often become the only acceptable kind of explanation:

> Minsky presents a totally unconstrained system capable of doing anything at all. Within such a scheme explanation is totally impossible (p. 357).
>
> It is a commonplace of research into language that unconstrained transformational power enables one to do anything. If one can do anything, explanation vanishes. (p. 357)

There is no simple answer to the question "What is explanation?" Indeed, there are whole bodies of philosophy dealing with this problem. The computational paradigm, as described below, has a very different approach to explanation, which is not based on the notions of formal generative power. As with the other issues we have discussed, there can be no measure of what is "explanatory" without appeal to the assumptions of the paradigm.

**Step 6: Justifying the methodology by appeal to problems of "learning"**

Having reduced the scope of "the scientific study of language" to the study of constraints on classes of grammars (where a grammar is "a system of rules that in some explicit and well defined way assigns structural descriptions to

sentences" (Chomsky, 1965, p. 8)) there is a need to provide arguments as to why this limited study should be of interest. This need has provoked a line of argument which can be summarized as:

Our basic goal is to understand the nature of human cognition.

All people learn their native languages without formal training or difficulty.

Formal grammars describing the syntax of these different languages share certain regularities. Some of these regularities can be captured by putting formal constraints on the form of grammars which generate the grammatical sentences of languages.

The fact that these are properties of all languages must mean that they reflect universal properties of the human capacity to learn language.

Therefore by studying the properties of classes of formal grammars we can determine those facts about languages which make them "learnable", and therefore reflect universal facts about human cognition.

It is important to recognize that much of Chomsky's motivation in pursuing this line of argument was his opposition to the behaviorist school of psychology, and his belief that the structure of language provided a powerful argument for the existence of innate specialized cognitive capacities.

> There is nothing essentially mysterious about the concept of an abstract cognitive structure, created by an innate faculty of the mind, represented in some still-unknown way in the brain, and entering into a system of capacities and dispositions to act and interpret. On the contrary, a formulation along these lines, embodying the conceptual competence-performance distinction seems a prerequisite for a serious investigation of behavior. Human action can be understood only on the assumption that first-order capacities and families of dispositions to behave involve the use of cognitive structures that express systems of (unconscious) knowledge, belief, expectation, evaluation, judgment and the like. (Chomsky, 1975, pp. 23–24).

In arguing against strict empiricism, he found it necessary to demonstrate that there were formal methodologies which could be used to reveal the nature of mental constructs. He is joined in this view by almost everyone who works in artificial intelligence. In fact, much of the work criticized in the Dresher and Hornstein paper is an attempt to better understand the "cognitive structures that express systems of (unconscious) knowledge, belief, expectation, evaluation, judgment and the like".

The problem is that in much of the Chomskian literature, the problem of how syntax is learned has been taken not as a demonstration of the feasibility of developing a mentalistic science, but as a definition of the study of language.

> The central problem of a theory of language is to explain how people learn their native language... The question – How does someone learn a language? – reduces to a new question – How does someone construct a grammar? (p. 323)

This is a *reductio ad absurdum* of Chomsky's argument. Indeed, language learning is an important problem, but it is hardly "the central problem" and it certainly does not "reduce" to the problem of constructing a grammar. If "How does someone learn a language?" were the central problem, then the entire Chomskian methodology would be largely irrelevant, since it deals in only the most peripheral way with empirical questions of language acquisition.

Again, my objection is not to the fact that some people are interested in studying how languages are learned, or that they believe they will get useful insights by looking at formal properties of grammars. It is to the blindness engendered by the insistence that this enterprise constitutes the whole of "the scientific study of language". The following section is an attempt to provide a new angle from which to view the nature and effect of their assumptions.

## 4. Language as biology – a meta-scientific metaphor

In explaining why current linguistics does not attempt to deal directly with the question "How is language organized to convey meaning?", Dresher and Hornstein draw an analogy to the study of biology:

> ...biologists rarely attempt to tackle head-on the problem, "What is life?", but generally break it up into smaller and more modest problems, such as "What is the structure of the cell". As a means of getting to the intractable – "How is language organized to convey meaning" – current linguistic theories ask, "What are the principles of UG?" (p. 333)

I believe that this comparison can usefully be extended as a way of clarifying the meta-scientific issues raised by the Chomskian theorists. There is a more than superficial correspondence between the "study of living things" and the "study of language" and our experience with biology can serve as one model for what a "scientific" study of language might be.

I will show the similarities through parodies of statements which have been made about the science of language, reformulating them as statements about the science of biology. At first glance, some of them may seem overly stated, or merely clever. However the exercise is being done with very serious intent. As mentioned above, it is not possible to debate the assumptions of competing paradigms in traditional formal deductive terms, since there is not a sufficient set of shared premises. What is needed are tools which allow us

to extend the domain of our thinking, and metaphor is one of the most accessible of these tools. In some sense, this metaphor is the main "argument" of my paper.

**Principle 1: The centrality of universal anatomy**

First let us look at Chomsky's definition of universal grammar, reformulated as a "definition" of "universal anatomy". As mentioned above with respect to "grammar", this definition can be made independently of any normal usage of the word "anatomy":

> Let us define "universal anatomy" (UA) as the system of principles, conditions, and rules that are elements or properties of all living things not merely by accident but by necessity – of course, I mean physical, not logical necessity.

This definition cannot be shown wrong, but it seems misleading in two ways. First, the use of the word "anatomy" strongly biases the question of which "elements or properties" are to be considered. Second, the emphasis on "all living things" seems to imply no interest in principles, conditions, or rules which are applicable to only some, but not all*. If taken seriously, this would exclude almost the entire study of biology, limiting its domain to those properties shared by bacteria, sea urchins, and people.

There are possible motivations for this kind of strong reductionism. There are indeed general principles of cellular biology, and these form a "basis" for all of the higher properties of living things. However, there is a tremendous difference between forming a basis and forming an explanation. DNA research is one of the most exciting and productive areas of biology today, but there is more to life than DNA. There are whole fields of science (anatomy, physiology, embryology, ecology) which deal with elements or properties of living things at a level which cannot be reduced to a discussion of the genetic mechanics**.

It is hard to imagine what biology would have been like over the past hundred years if it had been dominated by a dogma that only the study of "universal anatomy" was appropriately "scientific".

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*Any linguist reading this definition will also note the ambiguity inherent in the use of the quantifier "all". A phrase such as "the principles that are elements of all human languages" can mean those principles which are applicable to every language, or all those principles which are applicable to any language. On the assumption that the ambiguity was not a conscious attempt to confound, Chomsky's later statements make it clear that it must be interpreted in the former way – only those principles which apply to every language are included in UG.

**Haraway (1976) describes the rise of an "organismic" paradigm for the study of biology, and the ways in which it rejects the reductionistic approach of ardent DNA researchers such as Watson and Crick. There are a number of fascinating parallels between the biological controversies she describes, and the current debates in linguistics.

**Principle 2: Concentrating on ontogeny**

Paraphrasing Dresher and Hornstein:

> The central problem of a theory of living things is to explain how an organism grows from a single cell to its full form and function... The question – how does an organism develop? – reduces to a new question – How does the form of an organism get constructed? ... the relevant principles cannot be specific to any one organism, but must be equally applicable to the construction of the form of all organisms. It is in this sense that a theory of living things will involve the study of universal anatomy (UA).

In this form it becomes apparent how a true and important observation (about the importance of using the process of development as a key to understanding) is being twisted into a strange methodological axiom. Morphogenesis is one of the most important open problems in biology, and has been a source of important questions and observations. But it is not "the central problem", and it does not "reduce" to a simpler problem involving how the forms develop. The developing biochemical processes within the organism play a tremendous role in creating the evolving sequence of forms, and in many ways can be viewed as more primary*.

**Principle 3: There is an abstract formalization of structure**

Even if we limit our interests to the study of anatomy, there is still an open question as to what kinds of theories can explain it. It is in the notion of "explanatory theory" that Chomskian linguistics seems to have strayed the farthest from other areas of science. Formal constraints are viewed as explanatory, while considerations of process are considered extraneous. There would be a clear analog in biology (paraphrasing Chomsky):

> My own, quite tentative belief is that there is an autonomous system of formal anatomy, determined in principle by the nature of living things and its component UA. This formal anatomy generates abstract structures that are associated with "physiological forms" by further principles of anatomy.

In fact, such notions of formal anatomy could be applied to studies which have actually been done in biology. Just as linguists can point to phenomena such as "structure-dependence" and the "coordinate structure constraint", biologists have noted generalizations such as the fact that organisms with

*For a discussion along parallel lines in linguistics, see Halliday (1975), *Learning How to Mean.* He discusses the ways in which the development of communicative functions serves as a primary element in the development of syntactic competence.

spiral shapes are in the form of equi-angular (logarithmic) spirals, or that (as stated in Bateson's Rule):

> When an asymmetrical lateral appendage (e.g. a right hand) is reduplicated, the resulting reduplicated limb will be bilaterally symmetrical, consisting of two parts each a mirror image of the other and so placed that a plane of symmetry could be imagined between them. (Described in G. Bateson, 1972, p. 380).

Such generalizations seem to apply to a wide variety of different living things, and over a broad range of cases. As such they must reflect principles of "universal anatomy". One can imagine the development of "generative anatomy" in which mathematical rules dealing with shapes are applied to "generate" possible forms for animals. It is even possible that some general characteristics of the mathematical formalism could correspond to universal properties of biological form. For example, there are limited kinds of symmetry found in living organisms, and it should be possible to set up the derivation of forms in such a way that other symmetries would not be generated*.

Generative linguistics is based (however tentatively) on the belief that generalizations which hold over human languages can be best explained by building formal theories of "competence" which do not attempt to deal with the processes of language use or language acquisition, but instead seek an abstract "neutral characterization" of the constraints on possible languages.

There is no valid argument that this approach is wrong, or that an abstract "generative anatomy" would be wrong. It can only be argued that it appears inappropriate, given the range of things which we expect it to explain. Biologists would, however, have grounds for objection if it were decreed that only theories of this sort are to be called "explanatory". In fact, the use of this word seems perverse. It seems that even partially sketched theories which deal with the actual biological phenomena and processes "explain" far more than an extensive and successfully fit mathematical abstraction of the resulting forms.

### *A recapitulation of the metaphor*

If biology had followed the lines of current linguistics, the resulting dogma would be:

*There is no study of "generative anatomy", but observations of generalizations like those above have served as the basis for looking into the interactions between process and structure. Work such as D'Arcy Thompson's *On Growth and Form* (first published in 1917) looks for explanations of these regularities in terms of the way animals grow and live, and the effects of the physical processes involved. It is interesting to note that within biology, D'Arcy Thompson was criticized for being overly mathematical, and not resting his work on an "explanation" of the phenomena. Compared to current generative linguistics, however, his work is not at all abstract, with its extensive attention to physical processes and analogies with non-biological physical systems.

The central problem of biology is how organisms grow to be as they are

This growth process is too difficult to study directly, but we can get insight into it by studying those properties which hold for the structure of all organisms (universal anatomy)

The only scientific theories of biology are those which constrain the class of possible forms for organisms.

These theories are best stated as rigorous structures of rules which generate (in a formalizable geometric sense) possible forms.

There is no simple falsehood in this view, but in the context of what we know about biology, it appears myopic and farfetched. Only one biologist in a hundred conducts work which fits this framework at all, and there are whole libraries of work which would not be considered "scientific" if it were taken seriously. I believe that the situation in linguistics is less obvious, but not all that different.

## 5. The computational paradigm

In the light of the preceding sections, it should be clear that I do not view my work or that of the others discussed by Dresher and Hornstein as a better way of finding answers to the questions posed by Chomskian linguistics. The difference is one of paradigms, not methods. It would be misleading to imply that there is a well-defined, coherent paradigm which unites the work which they criticize. There is no single spokesperson who fills the role that Chomsky has in linguistics, and no catechism to be found in the writings. Those readers interested in fleshing out the sketchy picture provided here will have to glean it from research monographs in the area*. In the following paragraphs I can claim only to provide my own interpretation. It is certain that the other researchers criticized in the Dresher and Hornstein paper would not agree with me totally, and in fact it is likely that they would voice substantial objection to many points.

*My own views are expanded in Winograd (1976), and will be developed in a forthcoming book. Schank's current views (which have evolved substantially since the work cited) are best presented in Schank et al. (1975) and Schank and Abelson (in press). Kaplan has discussed his recent work extensively in Kaplan (1977). Other important works in the area are by Charniak and Wilks (1976), Norman and Rumelhart (1975), and in collections of papers edited by Reddy (1975), Bobrow and Collins (1975), and Schank and Nash-Webber (1975). The *Journal of the Association for Computational Linguistics* has published work in this area over the past few years. The journal *Cognitive Science* began publication in January 1977, and is dominated by adherents to the paradigm described here. A number of linguists, including Chafe, Fillmore, G. Lakoff and Morgan have rejected many of the Chomskian assumptions, and are looking at language in a style which is quite compatible with the computational paradigm. For more discussion of the connections, see Winograd (1976).

*A. The basic paradigm*

The computational paradigm for the study of language is based on a set of assumptions about the nature of language and the methods by which it can be understood. Informally stated, those on which there is broad agreement include:

The essential properties of language reflect the cognitive structure of the human language user, including properties of memory structure, processing strategies and limitations.

The primary focus of study is on the processes which underlie the production and understanding of utterances in a linguistic and pragmatic context. The structure of the observable linguistic forms is important, but serves primarily as a clue to the structure of the processes and of the cognitive structures of the language user.

Context is of primary importance, and is best formulated in terms of the cognitive structures of speaker and hearer, rather than in terms of the linguistic text or facts about the situation in which an utterance is produced.

It is possible to study scientifically the processes involved in cognition, and in particular of language use. Some parts of these processes are specialized for language, while other parts may be common to other cognitive processes.

*B. The centrality of process.*

The most important unifying feature of the computational paradigm is the belief that the processes of language use should form the focus of study. We share with Chomsky the belief that it is possible to scientifically study mental objects (in our case, the processes; in his, the grammar) which are not directly observable through textual or experimental observations. But we explicitly reject the Chomskian view that processes are inaccessible to scientific study and that formal properties of grammars are the only basis for linguistic science. The major object of study is the cognitive processes of the language user. This shapes the research in several ways:

*The use of the computer as a metaphor:*

The name "computational" is not applied to this paradigm because computers are used in carrying out the research. One could imagine the concepts being developed without any direct use of computers, and a large percentage of the current applications of computers to the study of language do not fall within this paradigm at all. What is central is the metaphor provided by viewing human cognitive capacity as a kind of "physical symbol

system"*, and drawing parallels between it and those physical symbol systems we are learning to construct out of electronic components. The parallels are not at the level of the physical components, but at the level of the abstract organization of processes and symbol structures. Like any metaphor, the computer metaphor has its limitations**, and provides only a direction of thought, rather than a concrete body of theory. In some sense, the entire paradigm can be described as the search for those insights which can be developed from this metaphor.

*Attention to properties of whole systems:*

Work within the Chomskian paradigm has generally been based on isolating one specific component of language, such as syntax or formal semantic features. In answer to Schank's criticisms, Dresher and Hornstein correctly point out that this is a methodological, not a theoretical stance. No linguist has held the "absurd position" that there is no interaction between the components. However they then proceed to say "...it is not obvious *a priori* which phenomena of language are to be assigned to the syntactic component and which to the semantic, or some third, component" (p. 18). There is a strong basic belief that the best methodology for the study of language is to reduce the language facility to a set of largely independent "components", and assign different phenomena to each of them. This is in direct contrast to a system-centered approach which sees the phenomena as emerging from the interactions within a system of components. Much of the work in the computational paradigm has taken this more systemic viewpoint, emphasizing the mechanisms of interaction between components and concentrating on "process structures" – those aspects of logical and temporal organization which cut across component boundaries. In some cases this has led to investigations into the ways in which the processes of language use are related to a larger range of cognitive processes, such as those involved in planning and visual scene analysis.

*Viewing learning in a secondary role:*

Although questions of language learning are relevant, they appear in a different perspective. The fundamental question is "What mental structures and processes make it possible for a person to use a language?" One key part of using a language is learning it, and no full theory of language can ignore issues of learning. But the place is secondary rather than primary. It may be

*See Newell and Simon (1976) for a definition of this term and its implications for the study of computation and cognition.

**Weizenbaum (1976) argues at length that this metaphor has disastrous consequences for humanity if taken as a view of the "whole person". I am in full agreement with his basic point, but I find that many of his specific arguments about linguistics are based on misunderstandings akin to those of Dresher and Hornstein.

impossible to totally understand physiology without knowing embryology, but there is a good deal which can be said about the functioning of fully formed structures independently of their origins.

### *C. The importance of representation*

Dresher and Hornstein correctly observe the importance of representation to the computational paradigm:

> There are several revealing respects in which Schank's work resembles that of Minsky and Winograd. Foremost among these is an emphasis on representation over explanation. (p. 376)

The fact that they consider representation and explanation to be competitive rather than complementary reflects one of the fundamental gaps in their understanding of computation. They begin with an intuition drawn from traditional mathematics, which is one of the major threads in the fabric of the Chomskian paradigm – the view that logical equivalence is of primary interest in forming theories. From this standpoint, two mechanisms can be considered formally different only if they lead to different sets of possible results, independent of the computational processes by which they arrive at them. This approach has been of great use in developing the theory of formal languages, but is very misleading when dealing with actual computation processes. There is a fundamental theorem of computer science which can be loosely paraphrased as:

> If there are no limitations on the amount of memory or processing time, then any machine with a certain minimal set of mechanisms can perform exactly the same set of computations as any other machine, no matter how complex, which includes that same minimal set.

But this theorem is of interest only when we are dealing with the abstract case in which "there are no limitations on the amount of memory or processing time". As soon as we try to apply computation theory to real systems (whether natural or constructed) we must deal with the fact that every such system is limited in both time and memory. Two systems which are "equivalent" in the formal sense can have entirely different properties if they operate with resource limitations. Furthermore, these differences can be related in systematic and scientific ways to the "representations" used in the different systems*.

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*Like the word "understanding" discussed below, the word "representation" carries with it some dangers. The set of structures within a computational system does not need to "represent" any reality which exists outside of it. Maturana (1970) has pointed out the problems in taking the notion of "representation" as anything but a metaphor in describing a cognitive system.

As an example, we can look at two different representations of arithmetic. Imagine two people called "the calculator" and "the logician". The calculator knows the usual simple facts and rules about addition, multiplication and so on. The logician knows a formal axiomatization of arithmetic, and a set of procedures for making deductions in a formal logical system. If we ask the question "Is (A + B) + C always the same as A + (B + C)?", the logician will immediately answer, while the calculator will only be able to decide after great thought (if at all) that it is a consequence of the rules for carrying out addition. On the other hand, if we ask "Is 52 times 84 equal to 4368?" the calculator will answer immediately while the logician will spend hours going through a proof with thousands of steps. If we only care whether these two people would ever disagree in their answers, the difference in representation is irrelevant, but if we are interested in how they can *use* arithmetic, the difference in representation is crucial. The effects of representation are just as concrete and formalizable as the logical equivalence, but in a different domain – the domain of process.

This is of course an oversimplified example, but it points out the importance of looking at differences in the *accessibility* of information and the *inference procedures* which operate on it. The intellectual substance of artificial intelligence (and of much of computer science) lies in the study of the properties of different representations, and of different process structures which arise from operations using these representations*. It is through an understanding of the deep properties of representations that we hope to find useful "explanations" of cognitive processes such as language use.

The nature of the representations underlying human language use is an area of open and active debate and research. Much of the work quoted by Dresher and Hornstein deals with this issue. There are some researchers who emphasize the "procedural" aspects – the structure of the computations – and others who are more concerned with the "declarative" aspects – the nature of the representations stored in memory. There are some who believe that the representations for syntax and meaning are quite different, and others who assume that they are essentially similar. However, there is broad agreement that it is an issue of central importance to understand the properties of these representations, and develop a better understanding of how they take part in computational processes.

Minsky's (1975) frame paper has to be understood in this light. Dresher and Hornstein point out (again correctly, from my standpoint) that his

*Knuth (1968, 1969, 1973) provides a compendium of the representations commonly used in conventional programming. The papers in Bobrow and Collins (1975) debate many detailed issues about representations used in artificial intelligence research. Bobrow and Winograd (1977) develop some of the issues of accessibility and inference in a representation language.

"frame theory" is not a theory in the usual sense of the word. Their expressed confusion about the relationship between rules, patterns and processes in Minsky's statements is a fair reaction to the lack of specificity in his formulation. However, the paper was of importance because it influenced researchers to look at a class of representations with different computational properties from those which many of them had been studying. It did not postulate a theory, but laid out a direction of exploration*. It is inappropriate to conclude that studies of representation must "presuppose the types of explanatory theories which it is the aim of scientific research to discover" (p. 63). Quite the reverse, any explanatory theories will have to deal with the observed facts about representation and computation.

*D. The relevance of programming to theory*

One of the areas of greatest confusion in understanding the significance of work done in artificial intelligence lies in the relationship between computer programs and computational theories. Many people (including many AI researchers) have the impression that somehow "the program is the theory". This leads to endless argumentation in which the critic says "That theory can't be right because this detail isn't sufficiently justified, that detail doesn't correspond to the facts of human language understanding, etc." while the defender says "I don't see any competing theories which even try to account, however badly, for the things this program attempts". What is clearly at stake is the nature of appropriate "theory", and on this issue as with the ones discussed above, there is wide variation within the research community. The views expressed here are my own, but I believe they would be acceptable to a large fraction of those who work within a computational paradigm.

First, a program is not a theory, even if it is totally correct as a model. If I have a complete blueprint for a complex mechanical device, it is not a "theory" of how that device works. But it would be foolish not to see a blueprint as a valuable part of an "explanation" of that device. Similarly, a program which completely duplicated the processes of human language use would still not be a theory. But any program which is built can be viewed as a hypothesized partial blueprint and can be a step towards understanding.

*The importance of imprecisely specified concepts is a feature of the development of all sciences. In discussing the importance of the concepts of "resonance" and "field" in the work of the biologist Paul Weiss, Haraway (1976, p. 153) notes: "The term *resonance* did not imply a specific mechanism any more than the term *field* implied that its basis was understood. Rather, the principle, first described in 1923, suggested the nature of the relationship so as to stimulate research founded on fruitful analogies"

There is a good deal to be learned in devising such hypotheses. As Dresher and Hornstein point out:

> Thus, Winograd believes that 'the best way to experiment with complex models of language is to write a computer program which can actually understand language within some domain'. (p. 333–334)

Apart from questioning the wisdom of using the phrase "actually understand"* I still believe this to be true. It is based on the belief that the important properties of language will be explained through the way different aspects of language are embodied in processes and the ways these processes interact in language use. Through studying the structure and behavior of computer programs which carry out analogous processes, we will develop a better understanding of this interaction.

Much of the work in AI is based on a methodological assumption that it is most profitable at this stage of the science to develop a body of alternative blueprints – to explore the possibilities before focusing on closely honed explanation. This has the same status as the Chomskian assumption that syntax should be thoroughly studied before turning to problems of meaning. It can only be validated by demonstrating the results eventually achieved by the work of those who believe it. It is one of the major areas in which Dresher and Hornstein find most AI research unacceptable.

The analogy with biology is once again applicable. There is an important level of analysis at which a living organism is seen as a complex system of biochemical interactions. The usefulness of this approach depends on understanding bio-chemistry in its own right – knowing what kinds of processes can take place, what substances result, what conditions are necessary. The biochemist who experiments with the properties of synthesized substances is operating in a style which is close to that of the AI researcher who experiments with the properties of synthesized programs. There is no guarantee that the substances which are created or the processes which happen in the test tube correspond to the actual substances and mechanisms in a living

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*The authors echo the concerns of Dreyfus (1972) and Weizenbaum (1976) about the careless use of the word "understand".

> In AI, language-understanding systems are systems which can carry out certain limited tasks involving language in some way: e.g. answer questions about baseball or engage in limited dialogue about a particular small world of blocks. Why these systems are graced with the epithet "language-understanding' rather than, say, "language receiving and responding" has never been adequately explained (p. 331)

Aside from its patronizing tone, this remark does point to an important issue. Our use of the word "understand" in human interactions implies a kind of empathetic process which is outside the realm not only of artificial intelligence, but of linguistics as a whole. Using "understand" to characterize a situation of instrumental communication is in a way impoverishing its meaning. Perhaps "comprehend" would be a better term for those aspects of understanding which linguists attempt to study.

organism, but the understanding which is gained through experimentation is invaluable in building models and performing experiments on living systems themselves.

In this context, the criteria on choosing what is to go into a computer program are quite different than they would be if the program were to be taken naively as a theory. Dresher and Hornstein criticize the AI approach:

> We cite this example [a mechanism for conjunction] as being characteristic of Winograd's overall approach, which is to *arbitrarily stipulate* what are in reality matters that can only be decided by empirical research, and which can only be explained on the basis of theoretical work (p. 350) [emphasis in original]

It would be equally valid to criticize a biochemist for "arbitrarily stipulating" the mixture of chemicals in an experiment because the properties of those chemicals can only be decided by empirical research and explained on the basis of theory*.

Dresher and Hornstein argue that the desire to build working computer systems is antithetical to the development of linguistic theory. "If one approaches the task with a 'practical desire', the question of universal principles need hardly ever arise ... on the contrary, it leads one away from a consideration of these issues." (p. 16). It is indeed valid for them to question the relative priorities of motivation in carrying out research. A researcher for a pharmaceutical company can spend years trying to synthesize an effective but not previously patented variant on a known drug without ever adding to our understanding of biochemistry. A person can write a "usable language understanding system" for limited purposes without dealing with any of the scientifically important issues. But this is a specific choice, not an inevitable consequence of combining practical and theoretical goals. Many important insights into human biochemistry have come out of research whose goals included practical pharmacology – the desire to synthesize a "usable drug". An AI researcher can choose to ask questions about universal principles and to use the practical goals as a framework providing rough boundaries for the phenomena to be studied.

*It is perhaps too early to compare the state of artificial intelligence to that of modern biochemistry. In some ways, it is more akin to that of medieval alchemy. We are at the stage of pouring together different combinations of substances and seeing what happens, not yet having developed satisfactory theories. This analogy was proposed by Dreyfus (1965) as a condemnation of artificial intelligence, but its aptness need not imply his negative evaluation. Some work can be criticized on the grounds of being enslaved to (and making too many claims about) the goal of creating gold (intelligence) from base materials (computers). But nevertheless, it was the practical experience and curiosity of the alchemists which provided the wealth of data from which a scientific theory of chemistry could be developed.

*E. The relevance of theory to programming*

The fact that Dresher and Hornstein accept the claim that effective programs can be written without dealing with "universal principles" is another clue to their lack of experience with programs and computation. They state that:

> Thus, one could start with fairly primitive components (a small number of syntactic patterns, a small lexicon, etc.) which could be improved indefinitely (by adding *more* syntactic patterns, *more* lexical items) according to practical constraints such as time, money and computer space. (p. 330)

They cannot be faulted for being trapped by this fallacy, since it has infected computer science in various guises throughout its history. However it has proved time and time again to be wrong. Computer programs which try to deal with complex problem areas simply bog down if they are not built with a structure whose complexity and form mirror the properties of the domain in which they work. The failures of the early "learning" programs based on simple perceptrons, and the limited success of the "theorem provers" built over the past ten years are testimony to the importance of this principle.

*F. The organicist/reductionist debate*

The conclusion of Dresher and Hornstein's paper is a valid (if somewhat histrionic) statement of one side of a debate within artificial intelligence. If taken literally, the image they present is false. There are few if any in the "AI community" who believe that:

> ...the fundamental theoretical problems concerning the organization of human cognitive abilities have been solved, and all that remains is to develop improved techniques for the storage and manipulation of vast quantities of information... the principles have been discovered, the information is easily accessible, the techniques are almost perfected... (p. 396)

However, if read in the bombastic spirit with which it was written, it paints a caricature of a widely held view that success in artificial intelligence will come from finding a few underlying basic principles (analogous to the laws of physics) and simply applying them to complex situations (boundary conditions). Herbert Simon has been a strong exponent of this view:

> A man viewed as a behaving system is quite simple. The apparent complexity of his behavior over time is largely a reflection of the complexity of the environment in which he finds himself ... I myself believe that the hypothesis holds even for the whole man, ... generalizations about human thinking ... are emerging from the experimental evidence. They are simple things, just as our hypotheses led us to expect. Moreover though the picture will continue to be enlarged and clarified, we should not expect it to become essentially more complex; (Simon, 1969, p. 24–25 and 52–63).

There are two different threads of reductionism which have been applied to artificial intelligence and the study of language. The simple form, expressed by Simon, views the entire range of observed behavior as being produced by a simple mechanism. A more sophisticated form underlies Chomskian linguistics and some work in AI, including that of Kaplan and Wanner which Hornstein and Dresher criticize. This form is based on assuming that there is a division of the complexities into well-defined components, or faculties, and that these can be studied independently without putting a major emphasis on their interactions.

I personally am at the opposite end of the spectrum – one which might be labelled "organicist", in opposition to this "reductionist" position*. Organicism is the view that "The organism in its totality is as essential to an explanation of its elements as its elements are to an explanation of the organism" (Haraway, 1976, p. 34). It emphasizes the interactions and complexities of the whole, instead of reducing explanation to the finding of simple rules from which all of the properties can be derived. I agree with the position stated by Dresher and Hornstein:

> The fundamental problems of a scientific theory of language have not been solved, and if research into language has shown anything it has demonstrated that – as is the case with explanation in other domains – an explanation of the human language faculty will involve the elaboration of unanticipated theories of tremendous complexity. (p. 396).

My statements in previous sections about the importance of viewing cognitive capacities as systems of complex interactions, and about the need for the complexity of programs to reflect the complexities of their domain are statements of one school of thought within the procedural paradigm, while Simon's statements (and some of those made by Minsky and Schank) reflect another.

Work in artificial intelligence will provide new grounds for continuing an age old and vital epistemological debate, but the arguments will not be along the lines drawn by Hornstein and Dresher. They make the mistake (as does Weizenbaum (1976) in his much more extensive treatment of the same issue) of trying to equate the reductionist views expressed by Simon with the computational paradigm as a whole. Things are, fortunately, more complex than that.

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*Haraway (1976) discusses the corresponding debate in biology at length. It is interesting to note that among all of the metaphors which she describes as having guided thinking within the different paradigms, computers are the only one which has been extensively used by both the reductionists and the organicists.

*G. The role of syntax*

Dresher and Hornstein quote a number of passages in which different researchers express disagreement with the degree to which current linguistic science emphasizes syntax. They state (correctly under the translation rules described in section 2):

> Like Winograd, his [Minsky's] emphasis on language as a device for conveying meaning (an emphasis which is inherent in the task of communicating with machines) leads him to misconstrue [*differently construe*] the aims of syntactic research (p. 335)

Indeed, a basic view of language as a means of communication leads one to "differently construe" the aims of syntactic research*. The emphasis is on "explaining" syntactic phenomenon in terms of the processes which go on in understanding and production. However, there is a wide spectrum of attitudes towards the role which syntax should play. The work of Wanner and Kaplan deals entirely with syntax, while the statements from Schank express his view that syntax is of very little value in understanding language. The authors point out (correctly from my point of view) the weaknesses in Schank's arguments about the irrelevance of syntax, but fall into an equally fallacious view of its centrality. They say:

> If ... syntax plays no major role in conveying meaning, we would expect that the sentence *Tall man the hit small round ball a* should convey about the same meaning as the sentence *The tall man hit a small round ball.* But of course the first of the sentences conveys no meaning at all. (p. 365).

It is not clear what they mean by "conveys no meaning at all", but they must be using the words "convey" and "meaning" in a rather special sense. A young child, a novice second language learner, and a telegrapher all manage to convey a good deal of meaning, while rarely producing a sequence of words which an adult native speaker would be willing to class as "grammatical". Even a list of words can convey meaning: A person who runs up to us on the road and gasps out "... skid ... crash ... ambulance..." has conveyed a good deal. The study of how this happens cannot be excised by fiat from the "scientific study of language". Syntax is important, but it is only one of many levels of structure which are vital to conveying meaning.

This is not the appropriate place to lay out the debates on details of how syntax and other aspects of language can be related within a framework that emphasizes the processes. What is important is to recognize that for the great

*It is not clear why Dresher and Hornstein seem to feel that an emphasis on the communicative function of language is somehow more inherent in the task of communicating with machines than it is in the act of communicating with other people.

majority of people working within a computational paradigm, the question is considered empirical, to be resolved by experimenting with possible models for those processes.

## 6. Conclusions

Much of this paper has been built around analogies between linguistics and biology. The most exciting aspect of these analogies is that the parallel current debates in the two areas reflect a change in world view which is one of the major intellectual events of our century. This change includes most of the natural and social sciences in its scope, and could have profound effects on our entire view of human existence and society.

> "It is possible to maintain that branches of physics, mathematics, linguistics, psychology, and anthropology have all experienced revolutionary and related changes in dominant philosophical perspective. The primary element of the revolution seems to have been an effort to deal with systems and their transformations in time; that is, to take both structure and history seriously without reducing wholes to least common denominators. Organization and process become the key concerns rather than last ditch incantations. (Haraway, 1976, p. 17).

In a way, Chomsky took linguistics the first step along this path. He introduced the notion of "transformation" as a fundamental keystone of the structure of language. But in the twists and turns of the theory described in Section 3, he distorted this insight. Process became a piece of the formal mechanism, rather than the focus of study. The computational paradigm grew out of a desire to look directly at the cognitive processes of people using language. Dresher and Hornstein are right to be critical of many of the details of its analysis, and to ask how much has really been accomplished. The current work is suggestive and enticing, but not authoritative or logically compelling. There is no clear set of "problems" which have been solved in a way which would prove its correctness, or disprove the statements they make in defense of the Chomskian paradigm.

> But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is,

> have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith. (Kuhn, 1962, pp. 157–158.)

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