SOME PROBLEMS WITH RESEARCH IN CHOMSKYAN LINGUISTICS AS APPLIED TO ELT

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Abstract

After discussing some of the most important reasons for conducting ELT research, the argument is made that the conflict between the "hard" sciences" and the humanities which has been a polarizing influence in ELT as it discourages many potential researchers from undertaking research when they think they must have mathematical models and use often opaque terminology. The argument is made that much in the "scientific" paradigm as it has been applied to language research is faulty. This claim is illustrated by looking at several issues raised by the Chomskyan claims to being scientific researchers. Several pitfalls or problems are identified that may warn a future researcher about the dangers of trying to be too much like a physics or chemistry researcher in what is actually a humanities-based discipline.

Key words: Chomsky, research paradigms, research methodologies, art/science divide

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> Truth and falsehood, except in the case of beliefs about our own minds, depend upon the relations of mental occurrences to outside things, and thus take us beyond the analysis of mental occurrences as they are in themselves. (Bertrand Russell, *The Analysis of the Mind*)

Depending on whether one is a researcher, a teacher or a student learning a particular language in a classroom, the study of language often has different though complementary goals. We assume that most students study a language in order to be able to read, write or speak it. Teachers may study the language they teach in order to organize information about it so that students can understand how the language works. Some researchers may focus on aspects

of these two types of learning by developing a better understanding of what the teachers and students are doing. While these goals seem different, there is a synergy between them.

Researchers may also try to develop new or better ways of understanding the language that may be used to teach teachers about language more efficiently and so improve the teacher's knowledge of language that then may be communicated to that teacher's students more effectively and so improve the learning process. Such research is pragmatic insofar as it is results oriented in terms of whether the learning of a language is improved as a result of knowledge produced by the research.

The models described above recognize the relationships between the three main protagonists in the process of studying language and how these relationships work in the real world. This model works in a messy way, by trial and error; it is not based on an obligation of the participants in the process to see language in terms of natural scientific methods or paradigms.

For other researchers, teachers and students, such models seem inefficient, illogical or "unscientific". They often see the participants' roles in more mechanistic terms: the brain, the language and the ways languages are taught should be technical matters which one can be trained to investigate, teach and learn. Such an approach is powerful for those who want to have their lives ordered in a particular way which is free from the doubts, uncertainties and mistakes we actually are prone to make as human beings. For researchers, teachers and students trained in the "hard" sciences, this sense of order is the only one that makes sense: technological developments in physics, chemistry, etc. offer the prospect that such knowledge about language is also possible if one follows the methodologies of those sciences.

The differences between these two types of approaches to research, teaching and learning are important. When we see the world as a technical mechanism which must be explained only in terms of facts and truths which can be established through the "hard" scientific paradigm of research, we place the researcher at the top of the chain of knowledge: the researcher discovers the truth and then communicates it to the teacher who uses that truth to teach the student. This top-down power relationship has led to the elevation of the scientist to a preeminent position in our societies. While this position may be deserved by the scientists who investigate problems in physics etc, who produce technical knowledge that demonstrably improves our lives, it has not so far been deserved by those researchers who have chosen a similar model for researching language.

There may be social and psychological forces at work in those who are drawn to such mechanical research in ELT. When one claims to be a scientist who follows scientific research paradigms, one may be seen to be more powerful as a result in the eyes of those who also believe that this model is the only way to proceed. Such a model allows one to obtain funding for research more easily as findings are meant to be facts that can be acted on to produce predictable results and certainties.

While the domain of the first model discussed above is the classroom understood in a humanistic way, the second model's domain is the brain understood in neuro-psychological terms. The tensions between the advocates of these two research paradigms have marked ELT for several decades. The one sees students and teachers as human beings in action, while the other tends to see the mind as a computer and the person as a biological entity that houses that computer.

Much damage has been done to the teaching community by the conflicts between these two groups of people who do not seem to recognize each others' work. An early case in point is the way followers of Noam Chomsky tried to bring his ideas into second language acquisition research by claiming the field as their own. This led to a certain dogmatic tone in the debates that ensued. This tone may be heard in Kevin R. Gregg who insisted that SLA research lacked any theory of acquisition (15) and so lacked a "research program" (35) except the one he advocated which was based on Chomsky's theory of "grammatical competence":

> I am saying that in comparison with an attempt to construct a theory of acquisition in the domain of grammar, any attempt to construct a theory of acquisition in the domain of pragmatics or communications is going to be handicapped by the lack of a well-articulated formal characterization of the domain. (Gregg: 24)

This claim that a theory of grammar will account for linguistic competence, but not for the acquisition of that competence, comes out of Gregg's reading of Chomsky's transformational-generative grammar (35). Unfortunately, Gregg's attempt to clear the field seems to establish his own identity at the expense of over-emphasising his concept's differences to the ideas of others by stressing that his approach is based on sound scientific procedures and principles. Gregg's espousal of a transformational-generative approach does little to dispel one's impression that it is thin on application or translation into the learning situation; in a word, it seems irrelevant to what is actually occurring in the teaching- learning process. A theory such as Gregg's remains closed to practicalities when it postpones and shifts other research practices to a yet-to-be enunciated domain.

The main attraction of Chomsky's theory for many linguists in the 1970s was that it promised sound scientific thinking and procedures. This may be

illustrated by John Lyon's introduction to Chomsky's work in his *Chomsky* (1970) which was aimed at people who were not trained linguists. In the first ten pages, Lyons uses the word "scientific" several times to describe Chomsky and to distinguish his work from the work of other linguists who, by implication, are made to look "unscientific".

The main problems for Gregg and others who sought to establish a scientific domain for research on language were twofold: One problem was that the research paradigm espoused by those who tried to follow Chomsky's theory did not produce actual findings and the findings offered did not conform to the laws of science as those laws were defined by those researchers. While those researchers claimed to be doing "hard" science", they in fact were not always doing so. The theory they attempted to build on was not completely articulated in a finished and testable form.

For other researchers who were interested in humanistic approaches to research, there was no middle ground on which the two groups could meaningfully debate their ideas. This is because one group had a definition of truth that the other group could not recognize. For Chomsky and Gregg, truth was something to be established through narrowly defined rationalistic "scientific" procedures; for others, truth was more pragmatic and defined in terms of humanistic values which saw teaching as an art based on experience, not a science which only sought validity through the internal logic of a series of untested propositions made from the viewpoint of an observer whose qualification was that he was a native speaker of the language being rationalized.

The point of what follows in this article is that when we look at a theory in linguistics, we need to evaluate how it sets about establishing data and how it interprets that data in a consistent way. When theorists claim, as the Chomskyans claim, that their theory is logically consistent, they are setting up parameters by which they judge or defend the validity of what they do. We should then test whether or not they adhere to their logic. If they do not, then we must seriously doubt their claims to being a science in the technical, hard sense.

What is at issue here is the division between various approaches to language and the learning of language which have divided the field of linguistics into hardened opposing camps which see linguistics as a natural science or as an art. While this division remains a major force in ELT, it will limit the progress of our understanding of how language works. All too often, in my experience, we as teachers are led to accept dicta as true because we accept it as authoritative or because we admit that we are overwhelmed by its intricacies. These are not good reasons for subscribing to a particular point of view.

In order to de-mystify the image of Chomskyan science, a closer look at some of its assumptions and arguments follows. It is hoped that the reader may at least see that when views are expressed in ELT, they need to be questioned before being accepted and that assertions of truth do not amount to proof of truth.

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Instead of writing an argumentative critique that offers itself as an integrated discourse on the limitations of Noam Chomsky's ideas in teaching English, I will offer a number of aphorisms. In this way, I hope to be as flexible as possible in responding to the often-mutating claims of this theory. To be fair to Chomsky, he has said that he is not interested in applying his ideas to language teaching. But that does not mean that his ideas are left in tact if we see logical inconsistencies in them. While recognizing that there are several versions of Chomsky's theories – universal grammar, transformational grammar, generative grammar, the minimalist program, etc. – I have not been able to follow any fundamental changes in the applications of those theories to English teaching. Nor have I seen any move away from the claim that his linguistics is "scientific" in a rationalist sense and is based on a tradition that stems from Rene Descartes. This makes me think that while there is not a single coherently unified articulation of the Chomskyan position on language, its claim to be a focused scientific form of research remains.

To offer an historical critique of the ways the theories have been modified or integrated would necessitate a lot more time and space than this essay provides. Instead, by looking at several of the assumptions apparently still held by Chomskyans, we can see how the errors that Bacon (1994) identifies are still prevalent in this contribution to linguistic theory which seems to hold pride of place to this day, at least in the United States. There is no point in trying to correct these faulty assumptions and practices as their existence seems to invalidate the theoretical claims made by the Chomskyans, a least as far as I can see. As Karl Popper (1963) has argued, once one modifies a theory, it becomes a new theory; one cannot keep the bits of an old theory in such a modified theory while claiming that the general theory still remains valid and while claiming to follow strict "hard" scientific procedures. The best we can do is point out the errors that are made when we adopt his theories as the model for our study of the psychology of language, or psycholinguistics as it has commonly been referred to up to recently.

1

The claim that there is a hard-wiring in our brain that determines how we process, learn and therefore use language is presented as an assumption or a given. The argument is made out of the necessity for this assumption if we are to generalize the different languages into an Ur-language or an I-language or patterning of the brain that makes all languages describable in terms of a single theory. When we propose a universal rule for how a language must work for all minds and at all times, we are vulnerable to Karl Popper's (1963) one thousand and first swan that is black instead of white. It takes only one instance or example of language not working in the claimed way for all languages to disprove the claim to universality. Such a universal rule would have to be demonstrable in practice throughout history and in all cultures.

To argue that it must exist in order for such a finding to be made is only a hypothesis, not evidence that we are correct. Such an argument out of necessity is a-historical and circular (tautological). It substitutes a curious logic for evidence by refusing to argue by example that such a discourse across all languages actually happens in the real world. As Steiner (1975) has argued, while such a discourse may be desirable, albeit in a utopian way, the evidence of how cultures often refuse to understand each other, together with the many problems recognized in translation studies, would actually be evidence that such a discourse is not demonstrably what happens now or has happened in the past.

2

It seems to be premature if not illogical to accept the universalist's claim that Chomskyan grammar establishes the "fundamental, deep-seated" characteristics of the brain in a way that allows us to set out once and for all "the general ground rules of human cognitive processes". This may be the aim of the universalist, but that does not mean that this aim has been achieved. To jump to the conclusion that all the literature purporting or accepting this as having already been achieved is conclusive evidence that it has so been achieved is more parsimoniously evidence for the continued presence of Francis Bacon's idols in our discourses (1994). That the Chomskyans have been trying to make this case for several decades should alert us to the limitations in such behaviour. Popularity or volume of writing is not evidence that we are right. The only evidence necessitated by the claims for a universal grammar would be to provide proof that these linguistic universals

exist in the real world of our language use. And it is precisely this evidence that has yet to be provided.

3

The categories used by the Chomskyans are not universal, but culturally relativistic insofar as they are derived explicitly from classical Greek language, and particularly from Aristotle's use of grammatical categories to structure his science. Bacon (1994) has criticized this bias. This assumption is not objective because it assumes that the Greeks got it right when they made this assumption. The Western-derived terms of mainstream linguistics are all too often unquestioningly accepted as universals: allophones, consonants, phonemes, nasal stops, etc. These divisions are at best of use in describing language in (Greek-defined) terms of word-based semantic logic so as to satisfy our need to divide it into manageable small units. There is no evidence that these technical terms are all that there is to language. Behind this assumption was a view of the world that reinforced the Greek prejudice or belief that their language was objectively logical and therefore had to be patterned on their mind which was also believed to be purely logical. To defend this assumption of Greek grammatical categories, which are in themselves surface structures that were enunciated in Aristotle's philosophy, by arguing that as all languages have the same deep structures, we would have to safely assume that the structures identified by the Greeks are the same as the structures of all languages.

But such an argument is faulty for at least two reasons: it uses its hypothesis as evidence for the truth of that hypothesis, as it does in the logic considered in aphorisms 1 and 2 above, and it does not demonstrate how the Greeks who followed Aristotle were right when they made their assumption that they were accurately describing their minds by spelling out the grammatical categories produced by that mind. While Aristotle's achievements and influence in rhetoric, metaphysics or ethics may be impressive, that does not mean that his categories have been proven to be universals.

The fact that other Greek philosophers (the Pre-Socratics) saw the mind in different ways to Aristotle should at least cause us to pause for reflection. Aristotle is not the dominant theorist for the Greeks. His hypotheses are just some of many that were considered at the time. To assume that Aristotle exhaustively describes the Greek mind, let alone the human mind in general or universal terms, has not been demonstrated to be true. Therefore it seems to be a hasty assumption at best to say that his categories are givens that need not be

questioned. And at worst, it could be a lazy assumption that ignores the evidence of what most of the other Greek philosophers thought on the subject.

To make this assumption without taking into account competing claims that our language shapes the world and our views of that world, and may in turn be shaped by our experience in that world, rather than being a cognitive structure that exists independently as some kind of given in human nature is to leave out too much by ignoring what does not fit the assumption or hypothesis. While accepting, charitably, that such a narrow definition may be necessary in the initial stages of hypothesis-generation, we may reasonably expect that after all the intervening decades from the time the hypothesis was initially made by Chomsky there would be an attempt to justify the basic assumptions or at least to question them in the light of subsequent findings. But as the concrete findings are so thin on the ground, such questioning is still being postponed if not forgotten. The possibility remains of testing the hypothesis of a deep-structure in the mind or brain by starting with a grammar derived from other cultures, such as that of Panini in ancient India, or from Chinese language, for example, to see whether the same result has not been countenanced by the Chomskyans.

When we consider that the preponderance of examples used by the Chomskyans are drawn from only one language, English; this narrowness should alert us to the limitations that they themselves feel in their proceedings. While they argue that if it is true in English it must be true for all languages, again this is circular thinking that uses the hypothesis as concrete evidence. And that is just bad science. Here they seem to be making the same assumption made by Aristotle that their own language is evidence enough for general rules or structures to be identified and proven as deep structures. But what the modern Chomskyans leave themselves open to is the same charge that has been made against Aristotle and his Greek followers: they are at home only in their own linguistic logic's perceived clarity of organization which is easily accessible to study. Such an assumption runs the risk of being labeled as cultural or linguistic chauvinism. By taking the logic of one's own language and filtering all other languages through that logic seems closer to linguistic imperialism and reified ideology than objective science.

4

Partly as a result of using only Greek grammar in Latin terms as categories – and here we should include the Chomskyan promise of explaining the semantic and phonological structures – Chomskyans have only managed to claim that all

languages use nouns or verbs. But is this really a new idea or finding? And even if it is accepted as new out of ignorance of what grammarians have claimed in the past who have not been operating with universalist beliefs or hypotheses, does this finding tell us much about how or why the brain is hard-wired in all of us to generate this "noun" class of word as an isolate? While it may be true that all languages have words that function as names of "things" or "objects", there is no need to assume that these objects or things so named are perceived in the same way or organized in the brain in the same way in all languages.

Such evidence of a universal pattern of identity needs to be provided before we should accept that even at this basic level of grammar there is an actual universal category of the noun. We should also expect evidence that nouns are sub-divided the same way in all languages – for example as nouns adjectival, concrete nouns, common nouns, abstract nouns, etc. While not having to argue that such evidence is impossible to provide, it has not been provided yet. Here, Halliday's (1994) grammar which looks at the noun as the organizing force in a larger group of words which form a single semantic unit has been ignored by the Chomskyans.

Once we find a language that does not exactly correspond to the way we organize nouns in English, we would have to argue either that the other language is an anomaly (and such an argument would not be possible while maintaining the universalist claim), or that such sub-divisions are only surface structures, not deep ones. But to argue for this would require evidence that such structures are only on the surface. We cannot import the classification or category of surface-structures as a gladbag for all that does not fit our hypothesis and then claim that such anomalies actually support our hypothesis of a base which determines the superstructure model) when in fact they intuitively and experientially seem to contradict our hypothesis.

The modification of our hypothesis or the exclusion of evidence that does not fit that hypothesis is not a logical or adequate way of demonstrating the validity of that hypothesis. In fact, it seems more like special pleading. The idea of surface structure is itself a hypothesis, not evidence that supports the original hypothesis. The division between surface and deep structure is not a given or established truth. Such a way of dividing things would have to be demonstrated as present in all languages, and as existing independently of specific ideological positions based on other philosophies. What exactly these philosophies may be is left up to the reader or teacher to conjecture, as they are not discussed at all by the Chomskyans. Rather, these philosophies seem to be accepted as universals in their own right because the Chomskyans do not feel it is necessary to argue them. This seems to be the case with Chomsky's own defense of Descarte's mind-body duality. But to import any one or two philosophies silently into the assumptions

behind the formulation of hypotheses may arguably be seen as naïve or even dishonest. Such a division may be common in several philosophers, but the existence of many other philosophers who reject such a fundamental division as already established in clear and unambiguous terms provides at least some grounds for questioning whether such a division is universal.

To dismiss such an objection is in itself an instance of surface variations of thought and not reflective of linguistic deep structures, which is not in itself a logical thing to do without providing evidence to support such a claim. To assert that such a division exists or must exist in order for us to classify things in the way we do is not evidence based on facts or argument, but an assertion that fails to recognize the difference between a hypothesis and proof of that hypothesis' validity. Here we seem to be in Bacon's Theatre with its Idols, not in the world of "hard" scientific research.

5

If there are in fact two different *structures* – deep and surface – the relationship between them has not been clearly explained. That there is meant to be such a relationship is conveyed by labeling both as structures. This idea of a structure, whether it is found or produced by the mind, in the brain, or only in the words themselves, carries the implication, at least to this writer, that there is always a "structure" to what is being studied or elucidated, and so a theory of organization is introduced which suggests that there is some principle at work that organizes the mind or language, and this principle has undertones of grammar or biological composition. The use of the word "structure" to describe the surface and the deep forms also conveys the impression that the two structures are similar: they are both structures. But which comes first, the deep or the surface structure? Do they influence each other in a symbiotic mutual cause-and-effect way, or does one produce the other? If they are different, then how is this difference affected? And if they are related, then is it possible to accurately discriminate between them given the presumably lengthy process in which the surface structure develops? These are questions that are not directly addressed by the Chomskyans.

6

By claiming without evidence that deep structure "may be" non-verbal or not part of consciousness, we are still left with having to say exactly what this structure is. If it is a part of our mind, then what part? Without being clear about this, how can we claim that deep structure is the enabler of

our skill in doing things with language? When we hypothesize that deep structure cannot be seen or heard as it is not part of our consciousness, we cannot claim, as Chomskyans often do, that this invisibility or inaudibility is evidence that such a level exists: a reason for us not being able to offer evidence is not evidence that our claim is correct.

If such a separation of the two structures or levels (deep and surface) is as clear cut as the Chomskyans claim, then they would need to provide evidence for the completeness of this distance. Instead, by using the terms deep and surface, their terminology seems to cling to the idea that the two are parts of the same thing, much as deep water and surface water are still just water. If the surface structure somehow reflects or sounds the deep structure, then we need to see or hear how this is actually caused or created. Without such an explanation, we are in danger of accepting some form of the hidden god argument: we know God exists but don't know who or what he is except that we see his actions shaping our lives. The failure of these theorists to answer such questions is one reason for the lagging influence their ideas have in the ELT classroom; the applicability of their grammar to practical issues of teaching has so far been lacking.

7

Even if we accept that deep structure is the only real force that shapes our surface structure or language, do we also have to accept the existence of "rules" by which this shaping happens? Rules are man-made and in language usually imply more than a rule of nature. The idea that there are physical rules in nature that determine our linguistic actions or thoughts is too simplistic. As human beings living in social environments, we learn to change these rules or adapt to them just as often as we follow them either deliberately or unintentionally.

Often, when we change the rules, we are also changing the world or nature. If this happens in other domains, then why shouldn't it happen with deep structure? The only rules offered by the Chomskyans are not rules for developing or changing language, but only rules for how to use language. Their often tortuous hypotheses about the transformational procedures we must follow are counter-intuitive and counter-experiential; they also violate Ockam's razor that the most parsimonious explanation is usually closer to the truth than is a more convoluted or complicated explanation.

The idea that "rewriting" is a rule seems unfortunate as it imports our surface practice of learning language into a generalized rule that we say must exist in

the deep structure. How we do things and why we do them, or how we use rules and how these rules are made in the deep structure, are not necessarily the same thing. To suggest that the difference between the surface and the deep structures is a result of a transformational process also seems too narrow. That a complex sentence can be explained only as a result of this transformational process is to ignore the possibility that this complexity is also produced by the words themselves in the rest of the discourse or paragraph. For a Chomskyan, the role of our understanding of the rhetorical or grammatical effectiveness of the sentences in shaping or altering such sentences apparently is not affected by the deep structure, so that we are left with the additional problem of having to explain the relationship between the deep structure, which is not clearly defined in psychological or physiological terms, and other aspects of our mental processes (as well as wider sociocultural processes) that could also shape our choices or intentions in using language.

We are also left with the problem of explaining if and how deep structure relates to this surface action of us changing what we write or speak as we write or speak: if I alter what I have written either while I am writing or in afterthought (as part of an editorial process) then is this too an affect of deep structure? If these changes are produced by deep structure only, then what can be said for more conscious forces in language use? Am I not running the risk of putting everything together in a deterministic way which ignores other aspects of my psychology or experience with words?

If these surface structures are more than linguistic, more than just isolated grammatical, semantic or phonological phenomena, then can I still claim that deep structure is the generative or transformational cause or explanation of my actual language? What seems to be missing is an explanation of the relationship between deep structure and these other, often extra-linguistic, considerations that at least intuitively and experientially seem to be operating on or in my language. Without a clear explanation of this relationship, we seem to be thinking in terms of epiphenomena that somehow exist silently in our methodology or philosophy.

8

When we explain differences in surface structure as mistakes or errors, we are importing value judgments prematurely into our logic. To suggest that this is not important implies that there is such a thing as a value free correct form of discourse and language use that somehow exists in a pristine or ideal state. But how did this perfection come about? And why should we accept that this

ideal state remains unchanged in deep structure, free from any environmental influence? Slips or "mistakes" can be just as natural or transformational as correct or perfect language. Such changes become accepted as natural and acceptable in our discourse with each other, not necessarily corrected by a force that emanates from the deep structure. But if deep structure is innate somewhere in our mind or brain, and if as an innate given it does not change, then can we be justified to see the relationship between deep and surface structure as static when all the evidence suggests that surface structure is always in a state of flux if not chaos?

9

If we must postulate the existence of an innate faculty or structure, why must we insist on saying that it is only grammatical in a narrow rule-driven sense? Surely, our universal predilection for metaphorical and sweet sounding or emotive words and our apparently inherent addiction to playing with words in puns or by other "artistic" means are just as justifiably seen as innate as universal human characteristics. To not recognize the possibility of these other kinds of language seems more than necessarily arbitrary in the interest of developing a working hypothesis.

Without an explanation of how these other kinds of language are either deep structures or surface structures, we are leaving out more of language than we are including in our explanation or definitions of language. Having said this, we should still ask the question: why should there be any innate level or structure to language? The impression created by Chomskyans is that innateness is self-defining; it is whatever is in deep structure. The definition and the structure seem indistinguishable: it is innate so it can't be defined in surface terms, and it is valid as the force of language because it is innate. Even if Aristotelian logic is accepted as innate, why should it be given precedence over other innate qualities such as love or fear or aesthetic concerns? And shouldn't other innate factors influence each other as well as our "language acquisition device"?

10

Even if we accept the existence of grammar and semantics the major components of deep structure, we cannot categorically affirm that they are the only ones. When Chomsky calls these components "certain formal kinds of operations on strings", he seems to raise the possibility that the process is a musical art of how to produce the effects. But such a possibility seems to be closed by the claim

that such operations are arbitrary: they just are. If their existence is arbitrary, then why can't their operation be just as arbitrary? This raises the possibility that such rules are only systematizable arbitrarily. By trying to image or represent these arbitrary things, don't we change them? And if we do change them by representing them, then how can we ever verify their original innate qualities, shapes, roles or existence? The theses of the Chomskyans become untestable and therefore they are not as "scientific" as they are claimed to be.

Here is where I think the Chomskyans' theory starts to behave in a chaotic way. Is there a force of chaos that operates to change our theories and thoughts even as we try to enunciate them or make them clear? If we sequence these arbitrary rules in arbitrary ways in our art or in the ways we create (generate) stories or discourses with them or from them, and see these arbitrary ways as socially produced or validated, then we are left with arbitrary forms and actions that produce meaning chaotically, not "formally" in grammatical terms.

11

Even if we accept that we generate or transform language out of deep structure, it does not follow that this is the same thing as how we learn language or change it. To simply transfer the findings or hypotheses from the one to the other seems sloppy thinking that confuses apparent or real similarities between the two acts as evidence that the two acts are the same.

12

Can we insist on the innateness of semantic rules in deep structure before we have clearly described the grammatical rules? Unless we are prepared to change our conception of these two aspects of linguistics by changing the logic of Greek linguistics, then to assert that semantic rules are innate seems at least premature. But by beginning with grammar and then promising to move on to semantics, the Chomskyans seem to accept the Aristotelean Greek logic of grammar determining semantics.

That they have as yet been unable to enunciate fully the semantic stage of their enterprise could be taken as an indication that the Greek sequencing of the two aspects of linguistics is faulty or incomplete. If such a logical sequence is inoperable in practice, then we may have to conclude that at least for the present that system does not produce results in the ways it expects.

Instead of trying to differentiate between a word's or sentence's grammar and its meaning, wouldn't it be more realistic or parsimonious to see the two as

inextricably bound together? To say that we choose words on a grammatical basis rather than or more than on a meaning basis seems too artificial. Do we really ignore our intended meaning when we are selecting (or generating) sentences? By artificially differentiating between grammar and semantics we may be making more problems for ourselves when and if we ever come to explaining how the surface structures exist either independently of meaning or intention, or how the meaning changes the deep structure grammar rules of production. When we chose a particular sentence form or structure, aren't we responding to the sentences we have already produced in our discourse leading up to that sentence, and the sentences we anticipate getting to subsequently in that discourse? Insofar as our discourse and our artistic aims shape these selections, then to maintain the model of deep structure, we would have to include these aspects of language in deep structure. But then, if this structure is innate, we will have to also accept art and discourse as innate and play important roles in the generation of performance-level language.

13

If a finite set of rules exist as deep structure which shapes (correct) language – and this seems to be the implication – then we are faced with two needs. The first is exhaustively to describe or define those rules as producing the surface grammar of all languages. The second is to demonstrate that the grammars of all languages are finite, or even static. That the first of these tasks has hardly been attempted and the second seems to require yet another assumption – that we have described (or that it is possible to describe) all the grammar rules of English and every other language – makes the likelihood of such evidence being supplied remote if not impossible.

To take such invisible evidence on faith as existing but unarticulated means that we accept the condition that we can never verify or disprove the hypothesis. The fact that we are constantly changing or developing our understanding of the boundaries of grammar to include supra-sentential utterances or wider discourse patterns, and our fuzzy view of the more-or-less grammatical qualities of spoken language or creative or idiomatic language makes the finite definition of grammar as one innate set of rules at least problematical. If the deep structure's rules are innate, finite, or fixed, and if we include rules for these other language uses in that deep structure, then we would have to explain why deep structure produces or generates both types of language or, failing that, we would have to change our definition of correct language to include the other types of language. At this point, the

deep structure hypothesis becomes much more deterministic and limiting. One way out of this is to posit more than one deep structure, but that would complicate the model to the power of n. If it seems unworkable in its narrowly defined grammatical form, then what would it appear like in this expanded form? The danger of having a theory which is so complicated is that such a theory becomes impracticable and unimplementable in the real world of classroom teaching.

14

As language seems constantly and endlessly to expand and vary in a process of decay and renewal, we may be right to think that there are processes for such generation and transformation without having to take on faith that this process must be finite or innate. Instead of accepting the model of a deterministic or linear system which seeks to predict or explain behaviour by filtering out most behaviour that does not fit the system, we need to recognize that there are other ways of approaching the problems posed to and by the Chomskyan models. That the Chomskyan system is powerful and impressive, and has had vast resources to foster its development, but has not delivered much in terms of the roles of semantics and phonology as levels of deep structure should make us question the effectiveness of such an approach in language teaching.

Whenever we as teachers feel imposed upon to accept an idea because that idea has had a lot of research funding or is presented as "hard" science and therefore has some kind of face validity, we are absolving ourselves from our first line of defense as researchers which, somewhat ironically, was proposed by Descartes, the main philosophical foundation of Chomskyan theory, as well as by Bacon: Doubt everything.

We should always remember that it is unethical to accept some idea as truth because we do not understand it, and then teach based on that idea.

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The Chomskyans exhibit many of the habits criticized by Bacon (1994) as preventing the advancement of scientific knowledge. By holding on to ways of thinking that fail to deliver and that over-complicate our studies, we generate vague notions and a lot of inconsistent data. These notions and data may be persuasive to those of us who are attracted to narrow or seemingly logical or neat universal systems and who want to have the fiction of a pre-existing

fixed order to language, but they offer little to others who are interested in the messiness and disorderliness of words as they see and hear those words in practice in their own lives.

It is to this other realm of language as chaos that we should now turn.

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