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This article represents the Presidential Address delivered at the Golden Anniversary Meeting of the LSA in New York, 29 December 1974, and attempts to provide a personal answer to the question as to how one goes about doing linguistics. Although the author recommends certain moves—e.g., diligent search for illuminating leading ideas, attention to formal aspects of the description, care and sophistication in dealing with data—he is at pains to point out there is no hope of ever finding a procedure for discovering insightful solutions to linguistic problems. This puts the linguist into the same boat with practitioners of every other science, and suggests that progress in linguistics (as in every other science) is the result of a combination of good ideas, powerful formulations, and careful attention to facts.*

In preparing my address to the Society on the occasion of its Golden Anniversary, I first thought that I would talk about the history of the Society, about its past. In the light of the many scheduled retrospective talks, however, I abandoned this plan. My thoughts next turned to the future, and I considered a talk about where our science and our Society are headed in the coming years. As I tried to write this paper, it quickly became obvious that the role of analyst and prognosticator does not fit me: like the biblical Saul, I clearly do not belong among prophets. Thus, by a process of elimination I had to choose as my topic something that is neither in the past nor in the future. It occurred to me at this point that it might be appropriate for me to discuss how one goes about doing linguistics, especially since I have spent (or, some would say, misspent) most of my adult life doing just that—and since, moreover, there are, to the best of my knowledge, no books or shorter works that treat this question at all seriously.

In a book that I strongly recommend to anyone interested in the philosophy and history of science, *Thematic origins of scientific thought*, G. Holton (1974) observes that every science has two aspects. On the one hand, there is the public aspect, the one that presents science as 'a body of clarified, codified, refined concepts that have passed through a process of scrutiny and have become part of a discipline that can be taught, no longer showing more than SOME traces of the individual struggle by which it had originally been achieved' (p. 15). On the other hand, there is the private aspect, the actual way of going about doing science: the false moves, the intuitive jumps to conclusions, the joy at seeing order in big masses of data, and the despair of realizing that one's favorite idea is untenable and that many months of work will have to be junked. Linguists rarely discuss this aspect of their lives; in their writings they concentrate almost exclusively on the public aspect of science. This reticence on the part of the working scientist concerning the true nature of his activities has not gone unnoticed. Thus Holton

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quotes the following remark by H. D. Smyth: 'The research man may often think and work like an artist, but he has to talk like a bookkeeper, in terms of facts, figures and logical sequences of thought.' The question of how one goes about doing linguistics clearly is a question about the private aspect of science, not about bookkeeping but about art, and it is this question that I wish to address here.

The first and most obvious observation in responding to the question of how to do linguistics is that there is no single procedure that one can learn once and for all. Doing linguistics is, in this respect, like playing chess or climbing mountains: one learns a few elementary principles, one discovers how these principles are put to use by examining instances where they have been employed successfully by others, and one practices applying the principles oneself. And as experience is gained, one becomes more successful at doing linguistics; i.e., one finds answers to specific questions of detail that have previously been beyond reach. But one can never be sure that the moves that have proven successful once will be equally successful in solving the next problem.

In fact, the situation of the working linguist is somewhat worse than that of the chess player or mountain climber, for unlike games won or peaks scaled, an answer to a scientific question is not entered in the record books once and for all. It is common that certain answers, which seemed exceptionally persuasive when originally proposed, appear less so as time goes on, and that large portions of one's work are subsequently shown to be wrong, incomplete, or otherwise inadequate. To accept this when it happens to one's own work, and to go on working in spite of the disappointment one feels, is one of the hardest and also one of the most important lessons to be mastered by anyone who has chosen science as a career. This lesson is particularly difficult to master for young scholars, much of whose education since grade school has been aimed at getting the right answer—so that, for some, getting the right answer is not only the most important thing, it is the only thing. It is hardly surprising that, when faced with a situation where every answer is sooner or later to be shown wrong to some extent, people with the background that many of us have when we enter graduate school find it difficult to react appropriately. It seems to me that students receive much less help from their teachers than perhaps they might, precisely because their teachers, like most professionals, dwell in their writings and lectures on the public aspect of science to the almost total exclusion of its private aspect. I do not know whether the awareness of the fact that even the most established scholars are vulnerable—that work which today is judged of Nobel Prize calibre may tomorrow elicit no more than a yawn—will allow individuals to be more creative; I am reasonably sure, however, that many workers (and not only beginners in the field) will find this information comforting when they compare—as all of us constantly do—their own efforts with those of the recognized leaders of the field.

Another topic that working scientists address quite rarely, and then only with numerous reservations, apologies etc., is the ultimate concern of their science. There exist, of course, books, monographs, and articles with titles such as 'What is life?', 'On the nature of matter', or 'Language' (tout court); but these are the exception rather than the rule. The standard scientific publication deals with matters of detail, such as trajectories of pions in a cloud chamber, the structure of certain protein molecules, double nominatives in Chinese, or the sound pattern of English. This is, of course, as it should be, for the answer to a question like 'What is language?' cannot properly be given in a few words or even in a few pages, but requires a thick volume or perhaps even a library of books if it is to be meaningfully answered; and the articles and monographs that many of us spend much of our time writing and reading are no doubt intended as piecemeal answers to the question that is central to our field.

And yet there seems to be a role for the short, one-sentence or one-paragraph answer to the central question. It is in this highly compressed form that certain guiding areas are expressed which inform our entire outlook, help us to find our way through the thicket of data and theory that is our normal stock-in-trade, and account for the fact that different scholars see different regularities in one and the same body of data. In his book, Holton documents in fascinating detail the role that such guiding ideas—THEMATA, he calls them—have played in the evolution of science, particularly in that of modern physics, from Copernicus to the present. He characterizes themata as 'the fundamental presuppositions, notions, terms, methodological judgments and decisions ... which are themselves neither directly evolved from, nor resolvable into, objective observations, on the one hand, or logical, mathematical, and other formal ratiocination, on the other hand' (57). For this reason, themata, 'unlike the physical theories in which they find embodiment ... are not proved or disproved' (62); they are 'unverifiable, unfalsifiable, and yet not quite arbitrary hypotheses' which influence scientific research every bit as much as the empirical evidence and the formal reasonings upon which public science rests its case.

A major THEME of linguistics has been the concept of language as 'a systematic means for communicating ideas or feelings by the use of conventionalized sounds having understood meanings' (Webster's Third). It is this theme which, no doubt, accounts for the vogue that 'information theory' had in linguistics some twenty to twenty-five years ago. On the other hand, it is this theme which must be credited with the fruitful interest in questions of meaning which has been a persistent characteristic of syntactic research, and which has resulted in many notable advances in our understanding of semantics.

Though by far the most popular theme, this essentially functionalist conception of language as a means for the communication of cognitive information has not monopolized the imagination of students of language. One of the competing themata which I personally have found particularly intriguing is the non-functionalist conception of language as playful activity, as a kind of a game. In the words of the late 18th-century German poet and mystic, Novalis: 'das rechte Gespraech ist ein blosses Wortspiel ... Wenn man den Leuten nur begreiflich machen koennte, dass es mit der Sprache wie mit den mathematischen Formeln sei—Sie machen eine Welt fuer sich aus—Sie spielen nur mit sich selbst, druecken nichts als ihre wunderbare Natur aus, und eben darum sind sie so ausdrucksvoll—eben darum spiegelt sich in ihnen das seltsame Verhaeltnisspiel der Dinge ... So ist es auch mit der Sprache' (*Sprachwissenschaftlicher Monolog*). (For the no doubt vanishingly small minority of linguists whose comprehension of German is not up to the demands of this quaint bit of 18th-century prose, I translate: 'real talk is mere play with words ... If one could make people only understand that language is much like mathematical formulas—These make up a world all of their own—They play only with themselves, express nothing but their own miraculous nature, and it is precisely because of this that they are so expressive, precisely because of this that the curious interplay of things is mirrored in them ... The same is true also of language.')

The theme of language as play suggests inquiries into non-cognitive uses of language such as that found in riddles, jingles, or tongue twisters—and beyond this into the poetic and ritual function of language, as well as into parallels between language and ritual, language and music, and language and dance. It also provides an explanation for the obvious fact that so much in language is non-optimal for purposes of communicating cognitive information.

Since language is not, in its essence, a means for transmitting such information—though no one denies that we constantly use language for this very purpose—then it is hardly surprising to find in languages much ambiguity and redundancy, as well as other properties that are obviously undesirable in a good communication code. In sum, the theme of language as a game opens up perspectives that are by no means unattractive, so that others might wish to explore them further.

Distinct from themata are abstract organizing principles and theoretical constructs in science. These constructs and principles, exemplified by the phoneme in phonology and the transformation in syntax, must, like constructs and principles in other theoretical sciences, be accepted or rejected on the basis of whether or not they are adequate to the task at hand: namely, the description of linguistic regularities and the widest possible generalizations. A large part of the linguistic literature is devoted directly or indirectly to the discussion of the appropriateness of such theoretical notions.

Occasionally it is possible to show that a particular theoretical notion is incapable in principle of doing the job that needs doing. Much more common, however, are the cases where a proposed notion is rejected on the grounds that its use is non-optimal, and this non-optimality can be overcome by employing a different notion. An example of such a replacement of one descriptive concept by a more suitable one is provided by the discussion that took place in the late 1950's concerning the status of the phoneme. One of the central conditions which it was then thought that phonemes should meet was the so-called bi-uniqueness condition; i.e., it was assumed that phonemes were entities of the sort that, to a given string of phonemes, there corresponded a unique sequence of phones (or sounds)—and conversely, to a given sequence of phones, there corresponded a unique string of phonemes. In a paper that I read before the 1957 meeting of this society, I challenged the bi-uniqueness condition. I pointed out that an inevitable consequence of this condition was that, whenever we encountered a language with a phonetic feature that functioned distinctively for some phonemes and not for others, we would have to state certain regularities twice. The specific case I analysed was that of contextually determined voicing in Russian obstruents. I observed that the alternation between distinctively paired obstruents such as [p–b, t–d, k–g] had to be stated as a morphophonemic regularity, whereas the alternation between the non-distinctively paired obstruents [c–ʒ, č–ǯ, x–γ], which takes place under precisely the same conditions, had to be stated as an allomorphic regularity, separate and distinct from the former. My point then was that this redundancy in the description is an artifact directly traceable to the bi-uniqueness condition, and hence an important argument against bi-unique phonemes as suitable tools for phonological descriptions.

In the ensuing discussion, there were proponents as well as opponents of the bi-unique phoneme, and the issue is to some extent being debated to this day. What appears to me especially relevant to the point I wish to make here is not that there was, or continues to be, disagreement on this issue, but rather that no one, to the best of my knowledge, has taken the view that the issue is vacuous. Quite the contrary, everyone appears to agree that the question of whether bi-unique phonemes are appropriate in linguistics (or put differently, whether bi-unique phonemes exist) is a matter of supreme importance to the discipline. Observe that, from a narrowly empiricist point of view, this is a rather curious situation, for the discussion does not concern matters of fact—in the discussion, no one has questioned the fact of how contextually determined voicing operates in Russian, or most other facts that have been brought up subsequently: the disagreement is purely about a matter of theory, about how certain facts should be characterized by linguists.

This purely theoretical aspect of scientific work is often misunderstood, especially among linguists. I recall a conversation I had many years ago with one of the most distinguished members of our profession, concerning the proper formulation of Grimm's and Verner's Laws. Toward the end of that conversation he remarked that he failed to understand why anyone would expend much energy on finding the most elegant formulation of some phonological regularity, especially in a case like Grimm's and Verner's laws where all the relevant examples have been known for a long time. All this concern with rules struck him much like the concern of schoolchildren with jingles designed to help memorize difficult points of grammar or spelling (such as the English 'Write *i* before *e* except after *c*.') 'Since I know how to spell every word in the dictionary, why should I bother with the jingle?' he asked. His implication was that rules were basically more or less arbitrary mnemonic crutches for the linguist who did not have a good memory; the linguist with a good memory (and my interlocutor was blessed with an absolutely phenomenal memory), as well as the speaker with native command of a language, surely knows all the words of his language, and hence has no need for rules. In response, I pointed out that, ever since Verner's discovery of the Law now bearing his name, it has been accepted that this law came into force later than Grimm's Law. Since there is neither documentary evidence nor dialectal evidence for this chronology, the case for the chronology has always rested on the sole basis of descriptive simplicity. Specifically, if Grimm's Law is assumed to have applied first, converting certain IE stops to continuants, then the fact that Verner's Law affected the reflexes both of the IE stops and of the IE continuant /s/ can be stated quite simply; i.e., Verner's Law applies to continuants regardless of their provenience. If, on the other hand, we do not make the assumption that Grimm's Law converted certain IE stops to continuants before Verner's Law came into effect, we can no longer say that Verner's Law applied to continuants, but will be forced to assert that it applied to continuants as well as to stops, provided that these satisfied the conditions for Grimm's Law. Since descriptive simplicity is the basis for the chronology in this, as well as in many other cases, there is very good reason for linguists to be no less concerned with the precise formulation of each and every rule than with the facts themselves. This conclusion should by no means be surprising, once linguistics is viewed in the perspective of other sciences where the attainment of greater descriptive simplicity has explicitly been a primary concern—at least since it was pointed out, in behalf of the Copernican theory, that epicycles could be eliminated from an account of the movement of heavenly bodies, once it was assumed that they revolved around the sun rather than the earth.

It is another characteristic of the more developed sciences, shared by linguistics, that knowledge is advanced not by a more or less fortuitous accumulation of new facts, but rather by the gathering of data specifically designed to answer questions raised by a theory. It is for this reason that physicists know a great deal about such esoteric matters as the weight of various sub-atomic particles, but know very little about such easily ascertainable things as the weights of pebbles. In this respect science is different from mountain climbing: while mountain climbers scale a peak simply because it exists, physicists normally will not tackle a question just because it can be formulated, but will require some theoretical motivation before committing time and resources to its solution. And though the situation in linguistics differs in many respects from that in physics, I have little doubt that linguistics is already at the point where one can no longer be satisfied with simply gathering facts: it is already clear, it seems to me, that not all linguistic facts are equal, but that some are more equal than others—i.e., have considerable greater theoretical interest than others.

This immediately raises the question of how one can judge the theoretical significance of a given fact or body of facts. The answer is that this can be done by showing that the facts in question can be accounted for as consequences of laws organized in a well-articulated theory. Well-articulated theories do not arise spontaneously, but require much hard work and thought. Unfortunately, within linguistics it has not been generally recognized how important such formal, theoretical work is; instead, there is a feeling that too much concern for theoretical detail is a waste of time. I have little doubt that many here agree with the sentiments expressed in the following comment on the section in *The sound pattern of English* (Chomsky & Halle 1968) where the formal apparatus of phonology was developed: 'In the fascination with all the variables, abbreviatory devices, and simplicity measures, the linguist's fundamental concern with the sound of speech is completely submerged by a sterile game with completely ad hoc features' (Wang 1973:83). But the attitude that formal, theoretical work is bound to be both ad-hoc and sterile is, I am convinced, fundamentally mistaken, and I should like to discuss a recent example from my own experience which illustrates this.

As readers of *The sound pattern of English* will no doubt recall, among the devices of phonology which Chomsky and I introduced were superscripts and subscripts. In the *SPE* notation, the symbol A_n^m represents a sequence of not more than *m* and not less than *n* consecutive A's, where A stands for an arbitrary phonological unit. Since the publication of *SPE*, it has turned out that no particular use has been found for the superscript/subscript notation—with one extremely important exception, namely that of the subscript in the notation C_0, which stands for a string of zero or more consonants. This fact, in addition to certain technical criticisms of the *SPE* notation advanced by C. D. Johnson—as well as revisions in the notation proposed by Johnson, I. Howard, and S. Anderson—have led J.-R. Vergnaud, Alan Prince, and me to undertake a fundamental review of one part of the formalism for phonological rules. In our study, we propose that the superscript/subscript notation be entirely abandoned, and be replaced by (among others) a special abbreviatory variable Q that is subject to the following conditions:

(a) Q represents a consecutive sequence of entities from which specified types of segments may be excluded by being mentioned in a special negative condition (which in turn is further constrained in ways that cannot be discussed here).

(b) When a rule containing the variable Q is applied to a particular string, the sequence represented by Q is interpreted as ranging over the longest possible string.

It should be readily seen that $Q \neq [-\text{syllabic}]$ represents C_0 in the superscript/subscript notation.

Given this notational device, we now inquire how we might state the stress rules in a language such as Huasteco—where, according to Larsen & Pike (1949:268), 'stress falls on the last long vowel, or if there is no long vowel in the word, on the first vowel.' It is readily seen that the first half of the regularity just described must be represented as

$$[+\text{syl}] \rightarrow [+\text{stress}] \,/\, \underline{\quad\quad}\, Q\,\#\# \qquad\qquad Q \neq \begin{bmatrix} +\text{syl} \\ +\text{long} \end{bmatrix}$$

Our next move, therefore, must be to ask what needs to be added to the rule in order to account for the treatment of words without long vowels. The answer to this question is—surprisingly—'nothing', for the rule as formulated above will assign stress to the vowel that precedes the longest sequence not containing a long vowel, and in a word that has no long vowels this obviously means the first vowel. In other words, if the abbreviatory variable Q is used to capture the assignment of stress in Huasteco words that contain long vowels, then the initial stress in words without long vowels is an automatic consequence which requires no further comment: it is no more surprising—given the Q notation—than is the fact that we find stress on the fifth syllable in a word where that contains the last long vowel, and on the twelfth syllable in a word where that contains the last long vowel. It is important to emphasize that there is no general connection between stress on the last long vowel of a word and initial stress elsewhere. It is only a theory of phonology that includes Q-variables that implies that there is a connection between the two cases; and since this connection happens to be attested in natural languages, this is by no means a trivial result. Moreover, there is a bit more to the story.

Paul Kiparsky, who was the first to point out the theoretical relevance of this type of rule, has drawn our attention to the stress facts of Eastern Cheremis, which are essentially identical with those of Huasteco. He also points out that, in dialects of the related Komi language, the stress falls 'on the syllable containing the first heavy vowel of the word'; but when the word has no heavy vowel, the stress 'is on the last syllable'. In other words, the Komi rule must be the exact mirror image of the Huasteco (or Cheremis) rule:

$$[+\text{syl}] \rightarrow [+\text{stress}] \,/\, \#\#\, Q \_\_\_ \qquad\qquad Q \neq \begin{bmatrix} +\text{syl} \\ +\text{long} \end{bmatrix}$$

including here the treatment of words without heavy vowels.

It might be useful at this point to recall that we embarked on this discussion of Q variables, and the stress facts of Komi, Huasteco, and Eastern Cheremis, in order to provide an actual example where detailed concern for the formal machinery of phonology has led to significant insights into the relationship between superficially disparate facts. There is, of course, no reason to exaggerate the significance of the example just discussed; but it does document at least one instance where concern for variables, abbreviatory devices etc., has been anything but a sterile game; rather, it has paid off in terms of a deeper grasp of the significance of certain empirical facts.

It would be comforting, no doubt, if one could now go on and assert that detailed attention to formal, theoretical issues invariably leads to new and valuable insights into the nature of language. Unfortunately things are not so simple: there is no royal road to knowledge in linguistics any more than in any branch of science. The most arduous work on formal problems—and, for that matter, on any other aspect of language—may turn out to have no interesting consequences and hence to have been misguided, sterile. Failures of this sort are unavoidable, and prove only that the complex characteristics of language are well beyond the reach of most guesses. To illustrate failures of this sort, I need not look far: my own career—alas—contains numerous instances that adequately bring out this point. From this rich store of sad experiences I shall cite just one example. In the 1950's I spent considerable time and energy on attempts to apply concepts of information theory to phonology. In retrospect, these efforts appear to me to have come to naught. For instance, my elaborate computations of the information content in bits of the different phonemes of Russian (Cherry, Halle & Jakobson 1953) have been, as far as I know, of absolutely no use to anyone working on problems in linguistics. And today the same negative conclusion appears to me to be warranted about all my other efforts to make use of information theory in linguistics. This conclusion, however, could not have been anticipated: the attempt had to be made, and the failure of the attempt explicitly recognized, before this dead end could justifiably be labeled as such. The failure is, therefore, a perfectly honorable one. Unfortunately, this does not make up either for the time wasted or for the disappointment of seeing something for which I had entertained considerable hope turn out to be a will-of-the-wisp. But complaints are really out of place here: like any serious professional, the linguist must be prepared to lose as well as to win.

Since I have dwelt here at such length on the importance of theoretical work in linguistics, it may not be out of place to note explicitly that nothing I have said is to be taken as minimizing the importance of careful and extensive descriptive work. Since linguistics is an empirical science, the only justification for developing a theoretical framework is that the framework mirrors relationships among the facts of natural languages. In this connection I should like to note that the question of what is a fact in a given language is by no means as straightforward as it might appear at first sight, and involves much more than mere care in observation. To illustrate what I have in mind, I should like to discuss some data from Hopi which I have learned about from a recent unpublished paper by LaVerne Jeanne, a native speaker of Hopi now working toward her doctorate in linguistics in our department. As illustrated by the quantity alternations in the reduplicated plurals cited below, it would appear to be a fact of Hopi that, in the second syllable of a word, long vowels are shortened if the vowel in the first syllable is long:

(1) *nó:va* 'food', pl. *nó:nova*
mó:la 'mule', pl. *mó:mola*
tó:ci 'shoe', pl. *tó:toci*
ʔá:ya 'rattle', pl. *ʔá:ʔaya*
só:hi 'star', pl. *só:soh˙*
sí:va 'metal', pl. *sí:siva*

The fact just cited explains why the second vowel in the reduplicated plurals is short.

Consider now the items in 2, where pairs of non-future and future forms of verbs are listed:

(2) a. *pána, pánani* 'act on'
sówa, sówani 'eat'
tíki, tíkini 'cut'
b. *tó:ka, tókni* 'sleep'
mó:ki, mókni 'die'
wí:ki, wíkni 'take along'

It is obvious from 2a that, to form the future, the suffix [ni] must be added. However, as illustrated in 2b, if the stem vowel is long, the vowel in the second syllable of the future form is deleted, and in addition the stem vowel is shortened. In other words, the examples in 2b provide evidence for the following two additional facts of Hopi: (A) In the second syllable of a word, short vowels are deleted if the vowel in the first syllable is long and if the short vowel in question is not word-final. (The last condition in the statement is needed in order to account for the non-deletion of short vowels in the last syllable of the non-future forms.) (B) Long vowels are shortened before two consonants.

The three facts just established can be written as the following three rules:

(3) a. $[+\text{syl}] \rightarrow [-\text{long}] \;/\; \#\text{CV:C}\,\underline{\quad}$
b. $\begin{bmatrix}+\text{syl}\\-\text{long}\end{bmatrix} \rightarrow \emptyset \;/\; \#\text{CV:C}\,\underline{\quad}\,\text{C}_0\text{V}$
c. $[+\text{syl}] \rightarrow [-\text{long}] \;/\; \underline{\quad}\,\text{CC}$

Since 3a–c have many things in common, the question naturally arises as to how this similarity is to be formally captured. This is by no means a trivial problem, as anyone who has read Kiparsky's important paper '"Elsewhere" in phonology' (1973) will readily appreciate. The case under discussion appears especially intriguing, since we have here one pair of rules that are identical on the left-hand side, namely 3a and 3c, and a second pair of rules, namely 3a and 3b, that are partially identical on the right-hand side. This immediately leads to inquiries into the relative ordering of the rules, and perhaps beyond this to inquiries as to how these facts bear on proposals that ordering relationships among rules can be established by certain universal principles. It turns out, however, that all such questions are quite irrelevant, for they are based on the assumption that the facts of Hopi are as stated above, and this assumption is not correct: the facts of Hopi are somewhat different from what they appear to be at first sight.

Suppose that long vowels in Hopi are underlyingly sequences of short vowels. We would then have to rewrite rule 3b as 4:

(4) $[+\text{syl}] \rightarrow \emptyset \,/\, \#\text{CVVC}\_\_\_\, \text{C}_0\text{V}$

and we would, of course, change the representation of the words correspondingly. As soon as this is done, it becomes obvious that there is no place for rule 3a in a grammar of Hopi, for rule 4 does everything that needs doing. Observe, however, that rule 4 does not state the same fact as rule 3b; it states rather that, in a word containing at least four vowels, the third vowel is deleted if no consonant intervenes between the first and the second vowel, and this is quite a different fact from that stated above. In particular, it now appears that the crucial thing that ties short vowel deletion and vowel shortening together as a single phenomenon is the requirement that the short vowel to be deleted cannot be word-final, but rather must be followed by another syllabic.

Since different facts may support vastly different theoretical inferences, it is of the utmost importance for the linguist to make sure that the facts at his disposal are the true facts; and as I have just tried to illustrate, this can be a far from trivial task. Indeed, the task involves the same sort of creative considerations as are involved in the adoption of theoretical constructs.

It is time to bring this lecture to a close so as to allow you to turn to other concerns. Being a schoolmaster to the very marrow of my bones, I cannot resist the impulse to conclude by telling you in a few words what I have been telling you in many more words during the rest of this talk. Fortunately this summary can be very brief, for what I have been trying to say about the way one goes about doing linguistics and about its pitfalls is quite similar to what Sir Francis Bacon wrote long ago about the way one goes about doing science:

> Those who have handled sciences have been either men of experiment or men of dogmas. The men of experiment are like the ant; they only collect and use: the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes a middle course, it gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy: for it neither relies solely or chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiment and lay it up in the memory whole, as it finds it; but lays it up in the understanding altered and digested. Therefore from a closer and purer league between these two faculties, the experimental and the rational (such as has never yet been made) much may be hoped.

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