6 Noam Chomsky and generative grammar

Any linguist today measures his intellectual position by reference to that of Noam Chomsky. Chomsky is commonly said to have brought about a 'revolution' in linguistics, and the political metaphor is apt. Just as books published in the Soviet Union on the most abstract academic topics once had to begin with a ritual obeisance to the guiding genius of Stalin, so nowadays even scholars researching aspects of language which have very little connection with Chomsky's work often feel obliged to claim publicly that their writings exemplify the Chomskyan paradigm of linguistic thought; and those scholars who acknowledge no such obligation are seen (and see themselves) as 'anti-Chomskyans' as much as proponents of their own views. Not just received beliefs about language but the whole atmosphere of linguistics as a discipline has changed as the movement initiated by Chomsky has triumphed. We must now turn to consider the nature of this 'revolution'.

Avram Noam Chomsky was born in Philadelphia in 1928, in a family of politically radical Jews of Russian descent. Chomsky's father was a Hebrew scholar of some standing, and Chomsky tells us that the childhood experience of helping to correct proofs of one his father's books on Hebrew was one hint which suggested to him that linguistics might suit his intellectual bent. As a student at the University of Pennsylvania Chomsky turned to linguistics partly through sharing the radical political views of Zellig Harris, who taught there; Chomsky's other subjects were mathematics and philosophy. In the early 1950s Chomsky pursued his research work as a Junior Fellow in Philosophy at Harvard, where Roman Jakobson was teaching; in 1955 Chomsky was given a teaching post at the neighbouring Massachusetts Institute of Technology, and he has remained there ever since.

The fact that he came to scholarly maturity within Jakobson's

sphere of influence is one of the keys to Chomsky's thought. Jakobson, the reader will remember, was centrally concerned with the question of phonological universals: he believed that the different phonological structures found in the languages of the world were merely superficial variations on a common underlying system (a belief which conflicted both with the principled relativism of the Descriptivist school and, one might have supposed, with what an uncommitted observer would conclude from the weight of the *prima facie* evidence). Although Jakobson himself wrote mainly about phonological universals, he believed that the approach was applicable also to other levels of linguistic structure; he got his students the Aginskys to contribute an article on 'The importance of language universals' (dealing mainly with anthropological aspects of language) to an early volume of *Word*, the journal of European linguists exiled to America by the Second World War (B. Aginsky and E. Aginsky 1948). The essence of Chomsky's approach to language is the claim that there are linguistic universals in the domain of syntax; and Chomsky develops the hypothesis of syntactic universals into a theory of considerably more richness and depth than Jakobson's theory of phonological universals.

For Saussure, it will be recalled, syntax was not even part of *langue*, the structure of a given language: putting words together into sentences was something that individual speakers did on particular occasions, not something that a language does once for all – there is an endless variety of sentences possible in any language, even though the range of Saussurean 'signs' (roughly, words) available in any language is limited. Although later writers had not explicitly agreed with Saussure that syntax was a matter of *parole*, the fact remained that they had not on the whole succeeded in finding ways to incorporate syntactic analysis into the scientific study of language. Before he could show that the syntactic structures of different languages were similar, therefore, Chomsky had to show how it was possible to define the syntax of any given language.

Chomsky approached this question in a way that came very naturally to a mathematician, although it was much less natural to anyone whose education had been in the humanities (which is why earlier linguists had not seen the point clearly). To the mathematician it is a cliché that a class of entities may be *well-defined* while nevertheless having infinitely many members.

Think, for instance, of a circle on a sheet of graph-paper, with its centre at the origin (the point where x-axis and y-axis cross) and its radius equal to five of whatever units the graph-paper is marked off in – centimetres, say. (We are considering a geometer's 'ideal' circle, rather than a physical circle drawn by a pencil which makes a line having breadth.) Now we may treat the circle as a class of geometrical points – a subset of all the infinitely many points on the graph-paper. For instance, the point '$x = -5$, $y = 0$' belongs to the circle (it is the left-hand of the two points at which the circle crosses the x-axis), but the point '$x = 4$, $y = 4$' is not (it lies outside the circle to the upper right). Not only does the graph-paper as a whole contain an infinity of points, but the circle alone (and indeed any line or curve having extension in one or more dimensions) has infinitely many points. (Most of the points belonging to the circle will have co-ordinates which are not 'round numbers' like 4 or –5.) Although the set of points which we identify as a circle is infinitely numerous, it is perfectly well defined: it is defined by the equation '$x^2 + y^2 = 5^2$'. Of the infinitely many possible choices of x and y, the infinite subset for which this equation holds constitute the circle; all other choices correspond to points either inside or outside the circle.

Furthermore, not only can this particular circle be defined; we can just as rigorously define the *class of all possible circles* on the graph-paper, which is an infinitely numerous set of infinitely numerous sets of points. (If the reader finds the mathematics slipping out of his grasp here, I apologize; I am trying to keep things simple, but I realize that many people have a blind spot in this area. Normal service will be resumed in the next paragraph.) The class of all possible circles is defined by the equation '$(x-a)^2 + (y-b)^2 = c^2$': for any given a, b, c, the set of points corresponding to all choices of x and y which fit the equation will be a circle, and every circle corresponds to some choice for a, b, and c. (The choice of a and b determines the centre, and c determines the radius – in the case of the circle first described, a and b were both 0 and c was 5.) Thus the class of all possible circles is again a well-defined though infinitely numerous class.

An example of a *non*-well-defined class of linear figures would be the class of all beautiful figures. Some figures (probably figures whose equations would be highly complex)

will be recognizably beautiful or at least attractive, others will be recognizably unattractive, and many (probably including most of the simple figures such as straight lines and circles) will be neither one nor the other. No doubt there will be an infinity of attractive figures, but is seems inconceivable that we could ever rigorously demarcate the membership of that class as we have demarcated the class of circles. The problem is not that attractiveness is a gradient (i.e. more-or-less) property whereas circularity is a sharp yes-or-no question – if *that* were the only difficulty, there are mathematical techniques which would solve it. Rather, the problem is that humans are constantly discovering (or perhaps 'creating' or 'inventing' would be better terms) categories of beauty that no one had previously recognized – we have to learn to see beauty, it is not a category given to mankind in advance; so that the notion of a fixed distinction between beautiful and unbeautiful entities (whether line drawings on a graph-paper or any other sort of thing) just does not apply. Any particular beautiful figure will be definable by a (probably highly complex) equation, but the class of all beautiful figures cannot be defined. It is significant that, in exemplifying the notion 'ill-defined class', I resorted to beauty, which is an aspect of humans' conscious reactions to things rather than a property that inheres in things independently of human-kind (as does the property of circularity). It seems to be exclusively Man, with his creative, unpredictable intelligence, who gives rise to ill-defined classes.

As a circle may be treated as a particular subset of the class of all possible points in a plane, so Chomsky proposed in his first book, *Syntactic Structures* (1957), that we should treat a language, from the syntactic point of view, as a particular subset of the class of all possible sequences of the items in its dictionary. (–5, O) belonged to the circle we discussed while (4, 4) fell outside it; similarly, the sequence *The cat is on the mat* belongs to English while the sequence **Mat the on is cat the* falls outside it. In Chomsky's terms, the former of these sequences is 'grammatical', or 'well-formed', the latter 'ungrammatical' or 'ill-formed'; the asterisk is used to mark ungrammaticality. (Note that these terms are used in a purely descriptive rather than evaluative sense. *I ain't never done nothing* is grammatical in a certain fairly widely spoken dialect of English, although not in the dialect in which this book is written; the fact that the

former dialect is deprecated by our society does not make it any less worthy of study from the scientist's point of view. Since Chomsky is interested in discovering what kinds of language are 'natural' to humans, he might even think the former dialect *more* worthy of study than standard written English, since it has been less regimented by the artificial rules of purists.)

The class of grammatical sentences in any language will surely be infinitely large: after all, from any two declarative sentences in English one can construct a third by interposing the word *and*, and in principle there is no end to the applicability of sentence-forming devices of this kind. But at the same time Chomsky takes it for granted that the class of all grammatical sentences in a language will be well-defined. This is clearly not the truism that Chomsky takes it to be; grammaticality depends on human mental activity rather than being physically 'there' in the sound-sequence, and it might well be that grammaticality was a property akin rather to beauty than to circularity. However, the notion of grammaticality as a well-defined property has proved very fruitful, and I shall say here simply that although Chomsky did not give explicit arguments in favour of his assumption I believe that it has justified itself in practice. Chomsky's exposition of how in principle the syntax of a language can be brought within the purview of scientific linguistic description is a great positive contribution to the discipline.[1]

To call a class 'well-defined' does not imply that someone has already produced an explicit statement of the properties necessary and sufficient for membership in the class: it only means that in principle there is such a statement waiting to be discovered. The next problem for Chomsky was to find some formal means of generating the class of grammatical morpheme-sequences in a language, as the equation '$x^2 + y^2 = 25$' generates the set of points we call a circle. (This use of the term 'generate', normal in mathematics, was borrowed by Chomsky into linguistics, and his approach to syntax is accordingly known as 'generative grammar'.) At this point Chomsky looked to the work of his first teacher, Zellig Harris.

Harris (like his Descriptivist contemporaries, although Harris went rather further than most of them – see particularly Harris 1951) approached syntactic analysis by classifying morphemes

into groups which resembled one another in their distribution with respect to other morphemes. Thus *cat, dog, boy, tail* and many other morphemes can each occur in the frame *The ___ is on the mat*; provided that we do not find a lot of other frames which differentiate between these morphemes, we will regard them as members of a single 'form-class'. Since this form-class is approximately that traditionally called the class of Nouns, we may as well symbolize the class by the letter *N*. It is important to realize, however, that Harris, like Fries (p. 65), by no means took it for granted that the traditional 'parts of speech' would appear in his analysis. Traditional grammatical terminology (which we inherit as the outcome of a several-centuries-long intellectual development culminating in the work of the Alexandrian Dionysius Thrax, *ca.* 100 BC) is based partly on logical analysis of the meanings of words, and partly on formal properties of the grammar of Classical Greek. Although pure distributional analysis applied to Modern English produces results which show considerably similarities with the traditional parts of speech (as well it might, English and Greek both being Indo-European languages) the results are only similar, by no means identical. When distributional analysis is applied to a non-Indo-European language, the classes obtained are often quite unlike those of our traditional grammatical theory (as Boas had stressed at the outset of the Descriptivist tradition); see Honey (1956) for a good example.

Having established that *cat, dog, boy, tail*, etc. belong to one class *N*, and that by parity of reasoning *good, bad, gigantic*, and so on belong to a single class, say *A*, we then find that sequences such as *good cat* or *gigantic dog* occur in the same frames that permit words such as *cat* and *dog* on their own – the two-word phrases are equally appropriate as replacements for the blank in *The ___ is on the mat*, for instance. We record this fact in an equation, '$A\ N = N$'. This is an example of an 'endocentric' construction, which as a whole has the same distributional privileges as one of its parts. We find also 'exocentric' constructions, which behave differently from any of their individual constituents. Thus, we can symbolize the class including *the, a, some*, etc. (each of which can fill the blank in *___ man is here*) as *R*; then we find that *R N* (e.g. *the cat, some bad boy*) behaves neither like *R* nor like *N*, but like a further class, the class of proper names – say *P*. (For instance, *the cat* or

some bad boy can fill the blank —— *is here*, and so can *John* or *Mary*, but neither *the* or *some*, nor again *cat* or *bad boy*, can appear in that slot in isolation.) Thus we have $R\ N = P$. In some cases it will even be convenient to symbolize a class of morpheme-sequences which substitute for one another although they never substitute for any individual morpheme. Thus, sequences such as *who snore -s, which whistle -s*, etc. (which might be symbolized $W\ V$ *-s* with W standing for relative pronouns and V for intransitive verbs) can replace one another in *The dog —— is on the mat*, so we might recognize such sequences as exemplifying a category of their own by writing $W\ V$ *-s* $= L$ even though no single morpheme can act as an 'L'. We may then show that *the dog who whistles* is syntactically equivalent to *some gigantic boy* or to *John* by writing $P\ L = P$; and this is better than writing $P\ W\ V$ *-s* $= P$ directly, since the blank in *The dog —— is on the mat* can be filled also by phrases not of the type $W\ V$ *-s*. For instance, *The dog with a gigantic tail is on the mat* is grammatical, so (given that *with* exemplifies a class E) we may write $E\ P = L$ as well as $W\ V$ *-s* $= L$.

The final step, taken explicitly by Chomsky, is to add a symbol S to stand for the class of complete sentences (so that we can write e.g. $P\ V$ *-s* $= S$, since *John snores* or *The boy whistles* are grammatical sentences). Chomsky prefers to turn the equations round and to replace the 'equals' sign by an arrow, so that equivalences such as those we have discussed would be recorded by Chomsky as follows:

Figure 3

$$S \rightarrow P\ V\ \text{-}s$$

$$P \rightarrow \left\{\begin{array}{ll} P & L \\ R & N \\ John & \\ Mary & \end{array}\right\}$$

$$L \rightarrow \left\{\begin{array}{lll} W & V & \text{-}s \\ E & P & \end{array}\right\}$$

$$N \rightarrow A\ N,\ cat,\ dog,\ boy,\ tail,\ \ldots$$

$$R \rightarrow the,\ a,\ some,\ \ldots$$

$$A \rightarrow good,\ bad,\ gigantic,\ \ldots$$

(A comprehensive grammar would of course need many more rules, e.g. to specify the membership of classes *V* and *E* and to introduce a large number of further form-classes and of further syntactic constructions not discussed above.) The point of replacing 'equals' sign by arrow is that it encourages us to think of the formulae as rules for constructing sentences.[2] We can produce a sentence by beginning with the symbol *S*, for 'sentence', and rewriting it as instructed by the arrows, making a choice at random whenever choices are provided by braces and commas, until we have replaced all capital-letter symbols with morphemes of the language under analysis. The language generated by such a system is the class of all sequences which can be reached from the symbol *S* by following the rules and making some particular choice whenever a choice is offered – just as the circle generated by the equation $x^2 + y^2 = 25$ is the class of all points defined by choices of x and y that satisfy the equation.

Although the geometrical equation involves only half a dozen symbols, there is an infinity of points which satisfy it. Similarly, although a grammar of the kind sketched in Figure 3 will be only finitely complex, it will generate an infinity of morpheme-sequences. A rule such as $P \rightarrow P\ L$, for instance, can be applied any number of times to its own output: e.g. *P* can be rewritten as *PL* which in turn is rewritten as *PLL*, and so on, thus allowing for complex constructions such as *the dog with a gigantic tail who snores*. The initial symbol *S* will itself appear on the right-hand side of a number of rules, in order to allow for sentences such as *John knows that the dog snores* (such a construction might be symbolized as '$S \rightarrow P\ C$ *that* S', with *C* for the class of verbs that take part in such constructions), and clearly such a rule can again apply to its own output, thus permitting, for example, *John knows that the gigantic boy thinks that the dog snores*. Thus a finite (if complex) grammar of this type generates an infinitely large, though well-defined, language (class of sentences).

So far we have said nothing about universals. Chomsky's next point is his most original. He observes that the algebraic notation system which he has borrowed from Harris (and which is more or less similar to the schemes used by those others of the Descriptivist school who attempted to come to grips with syntax) embodies a strong empirical claim about the syntactic

properties of human languages. The class of all possible grammars of the Harris/Chomsky type can itself be treated as a well-defined (though infinitely numerous) class: we may define it as containing any finite set of rules each of which is of the form '$A \rightarrow \varphi$', where A is some single symbol and φ is some sequence of symbols, morphemes, or both. (In Figure 3, groups of rules of this form were collapsed together by using braces and commas to indicate alternatives, but this makes no difference of principle: a rule such as '$L \rightarrow \{W\ V\ \text{-}s,\ E\ P\}$' is equivalent to the pair of rules '$L \rightarrow W\ V\ \text{-}s$' and '$L \rightarrow E\ P$', each of which is of the form '$A \rightarrow \varphi$'.) A set of rules which conforms to the definition just given is technically known as a 'context-free phrase-structure grammar'; since this term is rather cumbersome, I prefer to say 'constituency grammar'. Now, Chomsky (1959) has demonstrated mathematically that there exist well-defined classes of morpheme-sequences which cannot be generated by any constituency grammar, no matter how complex (just as there are linear figures which cannot be generated by any equation drawn from the class of equations defined by the formula '$(x-a)^2 + (y-b)^2 = c^2$'): the class of 'constituency languages' is a well-defined subset of the class of all possible languages, as the class of circles is a well-defined subset of the class of all possible linear figures in a plane. In other words, to assume that constituency grammar is the appropriate tool for describing the syntax of human languages is to assume that human languages all belong syntactically to a certain limited class – which is to say that there exist syntactic universals of human language. Chomsky felt (although this is highly contentious) that the Descriptivists did tacitly make this assumption about the appropriateness of constituency grammar (see Postal 1964, written under Chomsky's aegis), so that the Descriptivists' practice implied the existence of universals even though they claimed overtly to believe in unlimited linguistic diversity.

In order to make this alleged syntactic universal more comprehensible, we can express it pictorially. A constituency grammar associates, with each of the sentences in the language it generates, a 'constituency structure' or hierarchical tree-structure. For instance, the grammar of Figure 3 would associate the structure of Figure 4 with the sentence *The dog with a gigantic tail whistles*:

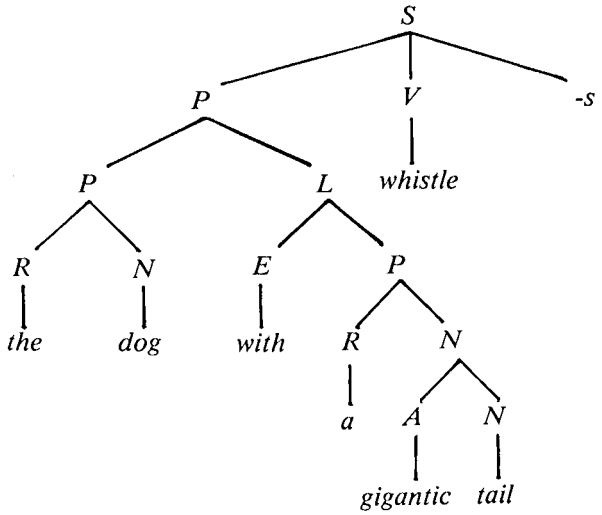

Figure 4

That sentence corresponds to the ordered sequence of 'leaves' of the tree in Figure 4, while the relationship between the rules of Figure 3 and the branching structure of Figure 4 should be clear. (Linguists conventionally draw their trees with the 'root', labelled *S* for 'sentence', at the top and the 'leaves', labelled with morphemes of the language under analysis, at the bottom; clearly linguists are even weaker than Hilary Putnam at nature study!) Constituency grammar could indeed be defined intuitively as the kind of grammatical notation appropriate for languages in which the criteria of grammaticality have to do with class-membership and hierarchical structure.

The fact that grammaticality in human languages has to do with the part-of-speech affiliation of words and with the way in which words are grouped hierarchically into phrases and clauses of various categories is of course by no means novel. Schoolboys were taught to parse their sentences by means of diagrams roughly equivalent to Figure 4 for centuries before Chomsky: the elements labelled *P* were traditionally called 'nominal phrases' (except when only one word long), that labelled *L* was traditionally a 'prepositional phrase', and so forth.[3] But Chomsky is himself arguing that constituency grammar corresponds to a tacitly familiar view of syntax: the novelty lies in the realization that, logically speaking, languages need not be

of the constituency type – it is perfectly easy to define classes of morpheme-sequences to which our traditional grammatical notions would be inapplicable.[4]

Since Chomsky's aim is to establish the existence of syntactic universals, and since he has shown that some fairly standard views of grammar imply that human languages belong to a rather restricted class (in other words, imply the existence of strong syntactic universals), he might well have stopped at this point. In fact he developed his exposition in *Syntactic Structures*, along lines which considerably undermine the force of what had gone before. According to Chomsky, the belief that constituency grammar is adequate to generate human languages, though widely held in an inexplicit form, is actually wrong. He gives examples (which I shall not discuss in detail) of constructions found in English that cannot, he claims, be handled by means of constituency rules.

If this is so, one obvious conclusion might be that the class of languages usable by humans is not, after all, well defined. Chomsky was the first to imagine that there might be an empirical, falsifiable scientific theory of syntactic 'naturalness' (that is, a theory which defines a class of languages to which all extant human languages belong but which is smaller than the class of all conceivable languages); perhaps this enterprise is misconceived, as a Descriptivist who believed in unlimited linguistic diversity would suppose. Admitting that one can dream up classes of morpheme-sequences which look highly 'unnatural' as human languages, perhaps nevertheless the property of 'naturalness' for languages is akin to the property of beauty rather than circularity for linear figures.

This is not Chomsky's conclusion. Rather, Chomsky argues that the constituency theory of syntactic universals should be replaced with a modified theory, which he sketches out in *Syntactic Structures* and which he and many of his followers have developed at length since. The essence of the new theory of syntactic naturalness is that it expands the canonical notation system for grammars by allowing the constituency rules to be supplemented with a series of so-called 'transformational rules'. A transformational rule, briefly, is a rule which operates on the hierarchical structure assigned to a morpheme-sequence by a constituency grammar, and alters it into a new hierarchical structure in a way which modifies the string of morphemes

acting as leaves of the tree. For instance, rather than producing a question such as *Whom did John meet last night*? by means of constituency rules different from the rules needed for a statement such as *John met Mary last night*, a transformational grammar can use just a single set of constituency rules to produce morpheme-strings exclusively in the declarative form; such strings will include *John met whom last night*, which is ungrammatical (unless given a special intonation to mark it as a request to repeat an imperfectly heard statement), but a transformation (or, in fact, a series of transformations) triggered off by the presence of the word *whom* will operate on this to give the correct form of the question.

Thus hierarchical structure still has the special role in Chomsky's new theory of syntax that it had in constituency grammar, however in the new theory a sentence will have not just one but a series of hierarchical structures. (As the theory has developed, the freedom to include transformational rules in grammars has been exploited to the point where all sentences in a language, including declaratives, are represented as having undergone numerous transformations in their 'derivational history'.) A morpheme-sequence belongs to the language generated by a transformational grammar if some tree produced by the constituency 'base' of the grammar, *after having been modified successively by such of the transformational rules as are applicable*, emerges as a tree containing the sequence in question as its leaves. That final tree is called the 'surface structure' of the sentence; the original tree as it emerged from the constituency base, before it was 'transformed', is the 'deep structure' of the sentence.

The 'transformational rule' aspect of Chomsky's work is much less persuasive than the material discussed earlier, For one thing, it is not clear of transformational grammar as it is of constituency grammar that it defines a class of languages smaller than the class of all logically-conceivable languages, i.e. that it makes a testable claim about syntactic universals – it appears possible that there may be a transformational grammar for *any* conceivable class of morpheme-sequences (Wall 1971). It may be possible to defend Chomsky's theory against that objection (Sampson 1973; *Form* [see p. 251, n.1], pp. 112–14); but the other problem is that Chomsky's arguments for the inadequacy of constituency grammar seem very shaky (*Form*, pp. 205–6), and,

in the clearest cases of failure of constituency notation (such as co-ordinate constructions), transformational rules do not seem to help much either (Dik 1968). The theory of transformational rules strikes me, for one, as something of an unfortunate excrescence on the body of Chomsky's linguistic thought. The fact that it is this aspect of Chomsky's work that has attracted probably more attention than any other element, to the point where Chomsky's whole approach to language is often referred to as 'transformational linguistics', seems merely to exemplify the difficulty people sometimes have in distinguishing between the essential and the superficial in a novel body of doctrine.

Be that as it may, the fact is that since the early 1960s a company of scholars, by now very numerous indeed, have been engaged in developing Chomsky's modified theory of syntactic universals. A typical article in any of the many academic journals which are now largely devoted to Chomskyan linguistics will propose some novel candidate as a possible linguistic universal, or will cite evidence from some language to show that an earlier hypothesis about a possible universal must be rejected, or will argue that a deeper analysis of the syntax of the language in question shows it not after all to be a counter-example to the proposed universal, and so forth. In many cases the hypothetical universals have to do with aspects of syntax originally discussed by Chomsky. Typical examples of the kind of topics debated would be the following: What kinds of modifications to trees do and do not occur as transformations in human languages? To what extent do the constituency rules, as well as the transformations, differ from one language to another? (Some argue that there is a fixed constituency 'base' shared by all languages, with syntactic differences being referred exclusively to differences in the 'transformational component'; Emmon Bach (1971) has argued that even 'transformational components' differ only by including different choices from a universally fixed, finite menu of allowable transformations.) What principles determine the order of application of transformations? (It is widely agreed that the sequence of transformations in a language applies to complex tree-structures 'cyclically': that is, the rules are all applied in sequence to the smallest subordinate clauses – i.e. subtrees dominated by an *S* node – then re-applied in sequence to the next most inclusive clauses, and so on until they have been applied to the sentence as a whole; but writers disagree

about whether any special transformations may apply before or after the cyclical application of the main sequence, and about what, if anything, determines the order of rules in that main sequence.) In other cases syntactic universals are proposed which have very little relationship with the matters treated by Chomsky. A survey of the range of universal hypotheses which have been put forward in the two decades since the publication of Chomsky's first book would be far beyond the scope of the present work.

One noteworthy characteristic of this search for universals is that hypotheses are standardly presented in the form of proposals to modify the canonical notation system for linguistic description, or to modify the interpretation of the previously accepted canonical notation. Consider e.g. Chomsky's discussion (1968, pp. 40 ff.) of the so-called 'A-over-A convention'. Briefly, this notion was proposed to account for the fact that, while it is normally possible to form a question from a statement in English by replacing one of the nominal phrases by an interrogative pronoun and moving it to the beginning of the sentence (making certain changes also to the verb and its auxiliaries) – thus sentence (1) below gives (2), if we choose to turn *the boy* into an interrogative – exceptionally, it is *not* possible from (3) to form a question (4).

1 The book interested the boy.
2 Whom did the book interest?
3 He read the book that interested the boy.
4 Whom did he read the book that interested?

Parallel facts can be observed in various other languages. The problem has to do with the fact that the phrase *the boy* in (3), which is the phrase to which the question-formation rules would have to apply in order to give (4), is a nominal phrase which is part of a larger nominal phrase (namely, *the book that interested the boy*), whereas *the boy* in (1) is not included in a larger nominal phrase. Chomsky's suggestion was that, universally, when constituents of the same syntactic type are nested one inside the other a transformation may apply only to the largest: thus, in (3), *the book that interested the boy* may be questioned to give *What did he read?*, but *the boy* alone may not be questioned in (3). In fact the situation has turned out to be rather more complicated than this, but that is beside the point

here. What matters is this: Chomsky does not formulate his proposed universal as a prediction that, when we have adequate syntactic descriptions of the world's various languages, it will turn out that in each case rubrics have had to be appended to the transformational rules noting that they apply only to the largest of nested constituents of a given type. Rather, Chomsky argues that (if his hypothesis is correct) we should agree now to interpret the formulae in which transformational rules are expressed in such a way that they are understood automatically as applying only to the largest constituent in such cases, without this needing to be stated explicitly in the published grammars of individual languages.

Comparable theoretical discussions occur in connection with the conventional notation for abbreviating groups of constituency rules (see Chomsky 1965, pp. 42–5). Thus, it is usual to abbreviate a pair of alternative rules of the form '$A \rightarrow B\ C$, $A \rightarrow D\ E\ F$' by means of braces and/or commas: '$A \rightarrow \{B\ C$, $D\ E\ F\}$'; and rule-pairs such as '$A \rightarrow B\ C$, $A \rightarrow B\ C\ D$' are commonly abbreviated with brackets: '$A \rightarrow B\ \ C\ \ (D)$'. Chomskyan linguists do not discuss whether or not the languages of the world contain syntactic phenomena to which the conventions of abbreviating by means of braces or brackets respectively can usefully be applied; instead, they argue about whether or not the canonical notation system should permit braces and/or brackets.

Historically speaking it is understandable that Chomskyans have come to feel that their theory of universals must be embodied in their notation system. Chomsky began by showing that an accepted notation system (Harris's) presupposed a tacit theory of universals; so, once the theory was made explicit and was modified in certain respects, the natural response seemed to be to make corresponding modifications to the notation. However, from a wider point of view this proceeding is neither particularly natural nor at all desirable. To see its unnaturalness, consider an analogy. It is a universal of geology that all valleys belong to one of two types: flat-bottomed, 'U-shaped' (in cross-section) valleys formed by glacial action, and 'V-shaped' valleys eroded by water. If geologists acted like Chomskyan linguists, they would instruct map-makers to use just two different symbols to represent valley-types, instead of the current system of contour-lines which can indicate far more than

two different cross-sectional configurations. Of course geologists do nothing of the sort, and there is no reason why they should: the fact that contour-lines can potentially indicate a wide range of valley-types in maps of particular territories does nothing to hinder the theoretical geologist in observing that only two out of this range actually occur in any territory, or in explaining why this is so.[5]

The reason why the equation of universal theory with notation-system is undesirable is that it tends to inhibit the process of testing and improving the theory. Just suppose that the accepted theory of geology is wrong and that there is in fact a third category of valleys, formed by some previously unsuspected process, which are W-shaped in cross-section, with a low hump in the valley floor. As things are, there is a good chance that geologists might discover this refutation of received theory about valley-formation by noticing that certain maps contain configurations which fit neither the U nor V categories. If, however, they had instructed cartographers to limit themselves to two notations for these two categories, then the theoretical geologists might never discover the inadequacy of their theory. The surveyors out in the field would do their best to fit the W-shaped valleys into the approved notation: they might map them as a pair of V-shaped valleys, and the theoreticians' own instructions would be to blame for the fact that the maps contained no information from which it could be discovered that the humps between these pairs of parallel valleys were less steep than the outer sides, unlike normal V-shaped valleys in which both sides rise at the same angle. If the rightness of a theory is felt to be no longer open to question in practice, then there might be some practical convenience in a description-system which allows for no more possibilities than those recognized by the theory (a map which indicated V-shaped valleys and U-shaped valleys by two discrete symbols might be less cluttered). While the theory is still being worked out and is open to challenge, though, it is desirable for the notation-system to be as flexible as possible so that counter-examples can be recognized and described for what they are.

Of course all systems of description make some assumptions about the things described. Even the cartographers' contour notation is not perfectly flexible – it does not allow for the

representation of hypothetical valleys whose sides overhang massively so that the valley floor is wider than the airspace at the top. There are obvious engineering reasons why such valleys are impossible, so this limitation in standard map-notation does no harm. Linguistics is in a different case: the search for the limits of syntactic diversity is a new enterprise, many of the worlds's languages have not been researched in this connection, and there is much disagreement about the interpretation of the evidence already examined. If the search is to succeed, our reaction to inflexibilities in the standard descriptive notation should be to encourage field-workers to be ready to change the notation without ceremony whenever it seems convenient to do so, and we should certainly not strive to confine ourselves to a descriptive technique even more formally regimented than the one we inherited.

The ill effects which I suggested might follow from adoption by geologists of the 'theory equals notation' principle are certainly very noticeable in Chomskyan linguistics. Since the Chomskyan 'revolution' it has become usual for a training in linguistics to focus heavily from the start on mastery of the approved grammatical notation system, and this has become highly elaborate as the theory of linguistic universals has evolved. Such a training obviously encourages the student to see examples, in the languages he examines, of the features he is taught to describe, and to overlook features for which no descriptive means are provided. In other words, it trains him to see confirming instances of the theory of universals and to ignore counter-evidence.

This drawback in Chomskyan linguistics has often been aggravated by an intolerant attitude, on the part of members of this school, towards purely descriptive work. One might suppose that a group concerned to discover universal features of language would be delighted about the existence of other linguists who aim to describe various individual languages for their own sake, and that the 'universalists' would warmly encourage such people to continue their work: such a division of labour means that instead of having to do their own donkey-work out in the field, the 'universalists' get much of the data they need handed them on a plate. But the Chomskyans have not always seen the matter in this light; members of this school have on occasion gone so far as to claim explicitly that

purely descriptive lingusitic work simply has no right to exist (see, for example, Schreiber 1974). By contrast with the situation in America before the rise to prominence of the Chomskyan school, during much of the 1960s and 1970s field-work on exotic languages has tended to become a dying art – with obvious adverse consequences for the search for universals. That search, in its relationship to purely descriptive linguistics, may be compared to the work of the theoreticians *vis-à-vis* that of the experimentalists in subjects like physics or chemistry. People who pursue those subjects are well aware that progress in them comes only from a healthy symbiosis between scholars of both categories.

There is an additional reason for the 'theory equals notation' principle, which may excuse the Chomskyans for adopting that principle although it does not reduce the harm caused by it. This has to do with the implications Chomsky believes to flow from the existence of linguistic universals, and we shall now examine those implications before explaining how they connect with the 'theory equals notation' principle.

The reason why Chomsky believes it is important to study universals of human language is the reason why Chomsky's work has attracted great attention in recent years from scholars working in disciplines such as philosophy and psychology, and has made linguistics a subject of much wider public interest than it had ever been before. Chomsky argues that the explanation for the fact that all languages of the world are cut to a common pattern (assuming that they are) is that the inherited structure of Man's mind forces him to use languages of that particular type. Chomsky's Descriptivist predecessors were empiricists, who believed that men are capable of learning as much as they do because the human mind is a thing of great flexibility, capable of accommodating to and finding pattern in the most diverse experiences which may impinge on it. Chomsky, conversely, is a rationalist in the tradition of Plato and Descartes, who believes that the mind is a thing of highly complex fixed structure which largely determines the form of human mental activity: what we can learn depends less on the stimuli that happen to impinge on us than on whether those stimuli are of the appropriate form to trigger off our pre-existing mental potentialities. For an empiricist, there is no general reason to expect any one kind of language to be more natural

than another. Chomsky, on the other hand, sees the child's acquisition of language as the filling-in of relatively trivial details in a pre-ordained structural plan; if one tried to teach a child a language not conforming to that plan, Chomsky suggests, then, no matter how 'simple' the language might otherwise be, the child would be innately incapable of mastering it. It is true that hypothetical non-hierarchically-structured 'languages' invariably seem so artificial that one cannot imagine how they could possibly be used as communication-systems in real life, but to make this point does nothing to remove the force of Chomsky's argument; it merely re-states the question which Chomsky claims to answer. We know that non-hierarchical languages are unnatural for humans, and we want to know why: Chomsky claims that the reason is because we are born with minds geared to hierarchical languages.

I have discussed and criticized these general philosophical aspects of Chomsky's work fairly exhaustively elsewhere (*Form of Language, Liberty and Language, Making Sense*); of Chomsky's various statements of his position the most accessible to the general reader are perhaps (1972a) and (1976). Language is for Chomsky only one source of evidence (though it is a particularly clear case) in favour of rationalism as a general view of human nature. (Incidentally, Chomsky's rationalist approach to language very clearly shows the influence of Roman Jakobson, and runs directly counter to the assumptions of all Chomsky's American predecessors without, I believe, a single exception.)

I shall argue at the end of this chapter that Chomsky is correct in holding that there exist certain logically unnecessary (i.e. contingent) universals of linguistic structure, and he may well be right to claim that this is evidence for a rationalist account of mind. But it should be said also that the existence of linguistic universals is, for Chomsky and his followers, not so much a finding which has emerged from their research despite their expectations, but rather a guiding assumption which determines the nature of the hypotheses they propose in order to account for data. The Chomskyans are always eager to suggest an explanation in 'universalist' terms for data which might well have some 'non-universalist' explanation if one were willing to look for it. When such explanations are false they can, of course, be refuted by counter-evidence from other languages,

but to find and to publish such counter-evidence takes time. For this reason (and for other reasons to be discussed later), at any given time the Chomskyan school tends to believe in a much richer system of universalist hypotheses than are really warranted.

I shall give one example of this 'rush to universals', which happens to concern phonology rather than syntax but has the advantage of being an especially clear (although not unusual) case. Paul Kiparsky (1971) notes a difference between Biblical Hebrew and Modern Israeli Hebrew. In Biblical Hebrew, the stops [p t k b d g] all alternated with fricative counterparts [f θ x v ð ɣ]; of the latter, only [f x v] survive in modern Hebrew, and Kiparsky proposes a rather subtle universal principle of sound-change to account for that fact. One might of course attack Kiparsky for basing a hypothesis about linguistic universals on a single phenomenon in one language, but in the context of his article this is not wholly unreasonable (he suggests a tenuous similarity with certain phenomena in other languages). The point I want to make here is that there is another explanation, in terms specific to Hebrew rather than in terms of linguistic universals, which Kiparsky does not even consider. For some two millennia between the extinction of Biblical Hebrew and the rise of the modern Zionist movement, Hebrew was a dead language learned by Jews as Englishmen learn Latin. We pronounce Latin not with the alien sounds that Romans no doubt used, but with sounds drawn from our own native tongue. For many centuries past the native language of most of the Ashkenazic (East European) Jews from whom the Zionists drew their membership was German, and German happens to contain the sounds [f x v] but not [θ ð ɣ]. All this is quite well-known, but it is characteristic of the Chomskyan approach to linguistics to ignore the possibility of explaining data by reference to particular concrete external facts in favour of postulating universal, abstract linguistic theories.

Let us return to the principle that the theory of linguistic universals is to be encapsulated in an approved set of notational conventions for describing individual languages. Given Chomsky's rationalist explanation for the existence of linguistic universals, the point of this principle is that it enables us to distinguish clearly between the aspects of linguistic structure which a child 'knows before it starts' and the information which

it has to learn through exposure to the speech of its parents and others. The general theory, which prescribes the notation and the proper interpretation of the notation, corresponds to the inherited linguistic faculty; the grammar of an individual language will contain only elements that the individual has to learn. The A-over-A principle for applying transformations is universal, therefore innate, therefore the child does not need to learn it and thus the grammar of English, for example, should not state it explicitly. The convention about use of brackets (say) is appropriately included in the canonical notation scheme if children are pre-programmed to abstract out of experience the particular type of patterning which brackets represent. If children are pre-programmed in that way, then a syntactic structure part of which is describable by a pair of rules such as '$A \rightarrow B\ C,\ A \rightarrow B\ C\ D$' will be simpler for a child to master than an otherwise similar structure which instead contains, say, '$A \rightarrow B\ C,\ A \rightarrow E\ F\ G$'. Use of brackets will reflect that relative simplicity by allowing the former pair of rules to be shortened to '$A \rightarrow B\ C\ (D)$' while the latter pair cannot be written so compactly. Thus, once the correct theory of linguistic universals has been discovered and has been embodied in a corresponding notation system, the relative 'naturalness' for humans of a given hypothetical or real language should correlate directly with the length of the shortest possible description of that language allowed by the canonical notation. (Cf. Sampson 1976 and Hurford 1977 for discussion.) This constitutes a motive for the 'theory equals notation' principle which has no analogue in the geological case, although, as already said, that motive does nothing to mitigate the harmful effects of the principle.

Many scholars do linguistic research without necessarily being greatly interested in the general philosophies of human nature presupposed respectively by Chomsky and by his empiricist predecessors. Perhaps the most salient and pervasive difference between Chomskyan linguistics and the linguistics of the Descriptivist school is an issue, separate from the matters already discussed (although related to them), of research method. According to Chomsky, the appropriate source of data for the linguistic analysis of a language is the introspective judgement of speakers of the language. (For references to various statements of this point of view by Chomsky and his

followers, see, for example, Botha 1968, p. 70; Labov 1971; Derwing 1973, pp. 40–2; my *Form*, p. 202.) When a Descriptivist said that some sequence of words was a sentence of English and should therefore be handled by a grammar of English, he meant, roughly speaking, 'I believe that I have encountered cases of this kind uttered by speakers of English, and if anyone disputes this I am prepared to look for documentary evidence to back up my claim.' When a Chomskyan says that some sequence is grammatical in English, on the other hand, he means, roughly speaking, 'This sentence feels right to me as a speaker of the language; and the possibility of disagreement does not really arise, because my introspections are authoritative at least for my own "dialect" [i.e. idiolect] of English, which is what I am describing.' To use data from introspection rather than fieldwork takes much of the effort out of linguistic research, and at the same time reduces the chances of having one's analysis proved wrong (at least by one's own standards); for both these reasons, Chomsky's methodology has attracted many linguists who care relatively little about his claims concerning inherited mental structure.

The saving of effort is greatest if one uses one's own introspections about one's native language. It is much less noticeable if one works on an 'exotic' language, because the effort of training a member of another culture to recognize his grammatical introspections and to report on them coherently is comparable with the effort of fieldwork in the old style, in which one was supposed to 'accept everything the native speaker says in his language and nothing he says about it'. Therefore the Chomskyan school has tended to concentrate on English and a few closely related European languages at the cost of spending much less time than the Descriptivists on exotic languages. Again, it is obvious that this policy would considerably reduce the chances of successfully developing a theory of linguistic *universals*, even if introspection were acceptable as a basis for the analysis of any particular language.

It is perhaps understandable that Chomsky believes introspection to be an acceptable source of evidence: this is a corollary of his rationalism. The essence of philosophical rationalism is the idea that knowledge is in us from the beginning, and 'learning' means merely learning to recognize and articulate what was in our minds already – observation of

the outside world is more or less irrelevant. (Chomsky is quite explicit about the relationship between his approach to linguistics and the philosophical rationalism of Plato and Descartes; see, for example, Chomsky 1966; 1976, pp. 6–8.) But although it may be understandable that Chomsky makes the mistake he does, one cannot take very seriously the extension of philosophical rationalism to the question of linguistic methodology. Even extreme rationalist philosophers recognize that one knows many factual matters only from experience – Descartes would not have suggested that I had inborn knowledge of what colour dress my wife would wear today, for instance. Clearly any speaker knows a fair number of truths about his language – even an empiricist would be surprised if he did not, considering the opportunities he has had to observe it. But if we ask in a spirit of honest enquiry whether speakers have access to an interior source of authoritative truths about either their personal idiolects or the larger languages spoken by their community, then by all the tests one can think of the answer is a clear 'no'; in the case of syntax, speakers' knowledge in the 'know-that' sense comes nowhere near to matching their 'know-how'. Speakers are often straightforwardly, and startlingly, wrong in their sincere convictions about even the most elementary facts of their own languages. (As mentioned earlier, this has been established most convincingly by William Labov, e.g. 1971, 1975; cf. Snow and Meijer 1977.) And the syntactic introspections of linguists themselves are likely to be the least reliable of all, since (unlike the average speaker of a language) the linguist has a vested interest in the correctness of particular syntactic judgements. A linguist half-sees that it would be convenient for him if some particular, fairly unusual sequence of words were grammatical, perhaps because it enables him to make some part of his grammar of English especially elegant, or because it constitutes a counter-example to some well-entrenched theory of universals and thus leads to fame for him as the David who overturns the theory; he mulls the word-sequence over in his mind for a while and pretty soon, lo and behold! he perceives (quite sincerely) a clear intuitive conviction that the string is indeed grammatical (in 'his dialect'). This sort of thing occurs over and over again in the linguistics of the Chomskyan school, and obviously the results of such 'research' are valueless. Thus, ironically, while Chomsky showed

how syntactic analysis could be a scientific discipline by propounding the notion of grammaticality in a language as a property whose extension is well-defined though infinite, by advocating introspective methodology he simultaneously ensured that syntactic analysis ceased to *be* scientific in practice. Fortunately the solution to this problem is simple, if linguists can be persuaded to adopt it: they should stop writing grammars to generate the strings they feel to be grammatical and instead base their grammars on what they observe to be uttered in speech and/or writing. (Some Chomskyans have suggested that there are reasons of principle why 'objective' grammars of this kind cannot be produced, but those suggestions are naïve: cf. *Form*, ch. 4.)

It is important to realize that, for Chomsky, introspection is not just a supplementary source of linguistic data but actually has an authority which is denied to observation; where the two conflict, according to Chomsky it is introspection which should determine the nature of the linguist's grammar. Even a Descriptivist uses introspection as a 'short cut', rather than attempting to document every single remark he makes about a language with which he is familiar; but if any particular remark is challenged, the Descriptivist will look for objective evidence to back up his claim (rather than waste time discussing the strength of his introspective feeling), and this is all we demand of an empirical science. For Chomsky, an appeal to objective evidence in such a situation would be inappropriate. We have seen Chomsky using the terms 'competence' and 'performance' to distinguish a language as a system from individual exemplifications of the system; but he uses these terms also in another way. (Equivocation on 'competence' and 'performance' is a major source of problems in Chomsky's thought, and it is unfortunate that such confused concepts have been taken up as widely as they have; cf. Fodor and Garrett 1966; Moravcsik 1969.) There are many cases where a grammar will generate some 'sentence' that nobody would actually utter, e.g. because it is too long to be used in practice; in such cases. Chomsky argues that the sentence is 'in our competence' – it is grammatical in the sense that we allegedly 'feel' it to be grammatical – even though it is not observed in our 'performance'. That is, 'competence' is here the class of strings corresponding to the 'ideal' language, in an almost Platonic sense, while

'performance' is the class of strings occurring in the imperfect language actually spoken in this sublunary world.

In many cases Chomsky is correct in saying that there will be a discrepancy between the predictions of a linguist's grammar, taken in isolation, and observed speech; however, these discrepancies argue not for the use of introspective data, but for the principle that (since our various beliefs and theories affect one another's predictions) they should not be considered in isolation from one another, which is a standard principle of empirical science (*Form*, p. 66). Thus, it is an empirically confirmed fact that the duration of human beings' attention-span is limited, and this leads to predictions about maximum length of utterable sentences which will often override the linguist's prediction that some long sequence conforms to the grammatical patterns found in observed shorter sentences and is therefore itself utterable. In other cases (cf. page 180) there is no empirical justification for the discrepancies between the 'ideal language' generated by Chomsky's grammar and the real, observable language, i.e. the Chomskyan grammar is simply wrong.

Chomsky's error about method is in fact precisely the same as the behaviourist fallacy discussed in Chapter 3, except that Chomsky commits the fallacy in reverse. The 'bad behaviourists' reasoned that, because it was forbidden to the scientist to use introspective evidence, therefore there was nothing to introspect. Chomsky holds (rightly, although his rationalism perhaps leads him to lay special emphasis on the point) that we have complex minds with a life of their own to which introspection gives us access, and he infers that it is acceptable to use introspection as evidence in scientific theorizing. Each of these arguments is as bad as the other. The objection to introspective evidence in science is not that there are no such things as introspections, but rather that introspection, while just as fallible as observation, cannot be constructively criticized as reports of observations can. Where a dispute between rival theorists turns on conflicting introspections, there is no method of resolving the dispute short of a shouting-match; the virtue of the scientific method is that, in those intellectual areas to which it is applicable (which include the study of syntax), it gives mankind a means of rising above shouting-matches.

'Shouting-matches' in the literal sense are happily rare even

among Chomskyan linguists, but what is noticeable in that school is that a smallish group of scholars who have succeeded in attracting public attention (whether by force of personality, known intimacy with the founder of the school, or in other ways) are invested with an exaggerated aura of authority, so that their lightest speculations are taken as significant contributions to scholarship while the work of others is largely ignored. (This phenomenon is discussed, for example, by Anttila, 1975; Householder 1978, p. 170; Newman 1978, p. 927.) When correspondence with observation is systematically ruled out as a criterion for choice between theories, it is inevitable that it will be replaced by the criterion of relative charisma of the respective theorists – will be replaced, in fact, by a resurrection of the medieval system of arguments from authority.

One practical difficulty for anyone who believes, with Chomsky, that the data for a grammar should be drawn from introspection is to decide just what categories of fact about his native language a speaker is supposed to be able to introspect. All Chomskyans agree that one can 'intuit' the grammatical status of particular strings of words, but most go much further than this. Chomsky, for instance, has never given syntactic evidence (as Harris did) for the range of form-classes which appear in his grammars: he simply intuits that the terms we inherit from the Alexandrians (Noun, Verb, etc.) are the correct ones.[6] Some writers seem to suggest that we can introspect the 'surface-structure' trees associated with our sentences but not their 'deep structures' (of course, the ordinary speaker untrained in linguistics needs careful prompting to help him articulate his syntactic introspections, but this is not taken as refuting the idea that he 'knew' the facts all along – cf. Langendoen 1969, ch. 2; linguists' classrooms differ from courts of law in having no rule against leading questions). Understandably, the question is very rarely discussed explicitly.[7] I believe that one reason for Chomskyan impatience with purely descriptive work is that the logical conclusion of Chomsky's views on method would be that speakers can ultimately introspect everything about the grammar of their language, so that description of an individual language consists merely of rehashing 'what every speaker knows' and only the theory of linguistic universals involves genuine addition to the total of human knowledge. (However, one Chomskyan

has even suggested that we have authoritative intuitions about linguistic universals – Bach 1974, pp. 165–6; and indeed this might seem to follow from Chomsky's doctrine of linguistic universals as corresponding to innate knowledge of language.)

One special consequence of Chomsky's introspectionist methodology has to do with semantics.

As we saw in Chapter 3, Bloomfield quite rightly felt that the semantic structure of a language was not open, at least in practice, to scientific investigation. Syntax is about the membership of word-sequences in a language, and we can check this objectively by listening to the sequences speakers utter. Semantics is about the chains of inference which allow us to pass from one set of beliefs or hypotheses to others. Here the only observables are the end-points of the chains: a belief will often be induced in a man's mind by his observation of the outside world (which we can observe at the same time), and conversely a man will often reason his way to a conclusion which causes him to act in some observable way. But individual 'inputs' and 'outputs' are commonly linked by such long chains of reasoning that there is no practical possibility of reconstructing the intermediate steps on the basis of objective data about the end-points – each particular intermediate step is wholly unobservable (we cannot observe a man inferring *Mary's neighbour is male* from *Mary's neighbour is a bachelor*).[8]

One point not understood by Bloomfield was that the problem is more than just a practical difficulty about the indirectness of 'input/output' relationships. Philosophers such as Karl Popper (1945), Willard Quine (1951), Ludwig Wittgenstein (1953), Russell Hanson (1958) and Jonathan Cohen (1962) have shown us that, *even if individual inferences could be observed*, the semantic structure of a language still could not be treated scientifically because it is not fixed. An Englishman constructs sentences according to syntactic rules which remain (to a close approximation) constant over time and as between speakers; but in choosing how to move inferentially from one sentence to another we regularly make up and continually modify the rules as we go along. The question 'Does *Mary's neighbour is a bachelor* entail *Mary's neighbour is male*?' is more like the question 'Is this figure beautiful?' than like the question 'Is this figure circular?' – the class of valid inferences in any real language (as opposed to artifical 'languages'

constructed by logicians) is not a well-defined class, it is constantly modified unpredictably by men's creative intelligence. Therefore the semantic structure of a language can be discussed only in the anecdotal, non-predictive fashion proper to arts subjects, rather than analysed scientifically – not just because the data are unavailable but because, if objective evidence were available, it would immediately refute *any* analysis that might be proposed.

This point has never been grasped by Chomskyan linguists, even though they cannot plead, as a defender of Bloomfield can, that the philosophical point was made after their time. One reason for this is that Chomsky himself (together with many of his followers) subscribes to what has been called the fallacy of 'scientism' (Hayek 1955) – he imagines that any subject which can be discussed at all can be treated by the scientific method (cf. Mehta 1971, p. 212).[9] But Chomsky's introspectionist methodology is also a contributory factor in his misunderstanding of the nature of semantics. When a native speaker introspects about the syntax of his language, he produces more-or-less sketchy, vague approximations to the truth; Chomsky argues that there is a complex, precise, fully articulated syntactic structure to which these hints approximate, and he is right, although we have no reason to suppose that the speaker tacitly 'knows' that structure. If one asks a speaker to introspect about the meanings of his words he again produces sketchy, vague, rough statements, and it is natural that Chomskyans again imagine there to be a precise, complete statement waiting to be articulated; but in the semantic case there is not. A linguist with a well-trained introspective faculty can of course set about *creating* a 'scientific theory' which purports to describe the semantics of his language, and many Chomskyan linguists from J. J. Katz and J. A. Fodor (1963) onwards have done so. But the writers just quoted, and many other members of the Chomskyan school (including its founder), have failed to take the first step of realizing that the aim of semantic description is to state the relationships of inference which hold between sentences. They have supposed instead that the aim is to *translate* sentences into an artificial language which is somehow semantically more transparent than the ordinary languages people actually speak, and they 'intuit' that simple words in everyday languages correspond to complexes of

'components' or 'semantic markers' in this 'conceptual language'. This approach seems so fundamentally misguided that it is difficult to find any virtues at all in the theories produced in accordance with it; these theories cannot be *disproved*, because they make no testable claims – they are just empty. To my mind there is no aspect of the Chomskyans' treatment of semantics, including the long debates about the so-called 'generative v. interpretative semantics' controversy, in which the positions adopted by the scholars in question are clear enough to merit examination in a book of this kind. (I criticize the Chomskyan approach to semantics at length in my *Making Sense*.)

Given what I have said so far about the general characteristics of the Chomskyan school, the reader may be surprised to hear that it has gained such a complete ascendancy, and especially to hear that it has been accepted as authoritative by the many scholars who are more interested in description of particular languages than in the search for universals. (Many even of these people feel obliged nowadays to apologize for their imperfect Chomskyanization, like practitioners of art for art's sake behind the Iron Curtain; cf. Hagège 1976, pp. 10 ff.) Here again the answer lies largely in the contrast between rationalist and empiricist methodology. Empiricism tells us to regard our opinions as fallible, and continually to seek counter-evidence to them; rationalism tells us that we are born with true knowledge already in us. This difference of approach operates at all levels: not just in the analysis of English syntax, say, but equally in debates about the theoretical and methodological foundations of the discipline. In general, empiricist philosophy encourages one always to think 'I may be wrong, and the other man may well be right'; rationalism encourages one to think 'I know the truth, so the only point in talking to the other man is in order to show him the light.' When scholars of these contrasting frames of mind encounter one another, it is clear which man is likely to win the debate.[10]

It is no accident that many linguists of the Chomskyan school have enthusiastically embraced Thomas Kuhn's doctrine of the history of science as a series of 'Gestalt switches' or 'conversion experiences', in each of which no reasoned grounds can be assigned for the adoption of the new intellectual 'paradigm' and the old 'paradigm' has disappeared ultimately only because its

remaining adherents died out (Percival 1976). Kuhn's claim resembles the claim that social change has often occurred through political revolution. The constitutionalist's reply is, 'Yes, since people are not political saints that has often happened, but such changes have been for the worse as often as for the better; how much greater genuine progress would have occurred if reformers had always worked within the legal framework of a liberal constitution' (the latter being the political equivalent of an agreed method for selecting between rival theories on their merits by reference to interpersonally-sharable considerations). The thoroughgoing rationalist, however, is obliged to prefer revolution to constitutional reform (in science and in politics): if the correctness of a theory, or the desirability of a form of society, is knowable by the pure light of reason rather than by practical experiment, then no means of peaceful persuasion are available when an opponent obstinately persists in claiming to see things differently. Naturally, those Chomskyan linguists who follow Kuhn, like political revolutionaries, lay much more stress on the notion that it is legitimate for them to come to power through an irrational Kuhnian 'paradigm-shift' than on the corollary that an irrational paradigm-shift which unseated them would have to be accepted as equally legitimate.

Another consequence of the contrast between rationalist and empiricist intellectual styles is a tendency for Chomskyan linguists to abandon the principle that science is cumulative. An empiricist scholar takes it for granted that, although his predecessors in any given field may well have been wrong in many ways, he is able to progress as far as he can only because of the work they have already done. We advance in knowledge by criticizing and replacing elements of the framework of ideas we inherit from previous generations, and a person who was taught nothing by his elders and was thus forced to work out his structure of ideas completely from scratch would never get beyond the cave-man stage. The rationalist does not see matters that way; he thinks of the individual as 'inheriting' true knowledge in the genetic sense, the main problem being to draw out into the open knowledge which is already there inside one – the thought of previous generations is redundant insofar as it is correct, and merely misleading where it is wrong. Accordingly, we find that the leading scholars of the Chomskyan school display unusual reluctance to acknowledge any virtue in studying

the works of predecessors (or, for that matter, contemporaries) of other schools – an attitude which differentiates the Chomskyans as a group from all other schools of linguistics. (See the references to Householder 1978, and Newman 1978, already cited above.) Since humans do not in fact have innate knowledge about linguistic theory, the consequence of this is that much research by members of the Chomskyan school, even when it is not vitiated by reliance on fallacious introspective judgements, consists of time-wasting rediscovery of facts or principles that had long been common knowledge outside the Chomskyan camp. (It should be said in fairness that this tendency is less noticeable in Chomsky's own work than in that of many of his associates.)

I shall quote just one example of this: Morris Halle's 'Prolegomena to a theory of word formation' (Halle 1973). Halle's article is on the theory of morphology (i.e. the organization of morphemes into words, as contrasted with syntax which strictly speaking deals with the arrangement of complete words in sentences);[11] and Halle begins by claiming that the subject 'has been studied only to a very limited extent'. Now Halle is no half-trained neophyte; he is the Chairman of Chomsky's department at MIT, and was elected President of the Linguistic Society of America (the highest honour which the American linguistic community can bestow on one of its number) for the year after his article appeared. But the fact is that there is a vast published literature on morphology (written by scholars who do not belong to the Chomskyan school), although Halle ignores it completely. In a critique of Halle's article, Leonhard Lipka (1975) concludes:

> has Halle brought up any problems which have not already been treated, or proposed any solution for such problems which [has] not been offered elsewhere? It seems that the answer . . . is no.[12]

When I say that rationalism encourages scholars to ignore the work of their predecessors, I mean only that the general frame of mind induced by rationalist assumptions promotes this attitude – certainly I do not suggest that, if René Descartes were alive today, he would explicitly argue that it is desirable for linguists of the Chomskyan school to cut themselves off from others' researches. Chomskyan linguists may object that my account of them is unfair because they know better than to

confuse rationalism as a specific thesis about the nature of mind with rationalism as a very general attitude to scholarship. To this there are two answers. To my mind, the methodological matters discussed in the last paragraphs are much more closely related to Descartes's rationalism than are points (such as the 'A-over-A principle') which Chomsky explicitly claims to be known independently of experience as Cartesian 'innate ideas'. But in any case, if the Chomskyans were to defend themselves as I have just suggested, they would need to give their own explanation of why they are so unusually turned in on themselves intellectually, because about that fact there can scarcely be any dispute.

Whether because the rationalist frame of mind induces in scholars an impatience with the usual disciplines of academic publication, or merely because new technology has made the development possible, one further salient phenomenon associated with the rise of the Chomskyan school has been the spread of what is sometimes called 'underground' or 'samizdat' publication in linguistics, whereby individuals who have failed to get their work accepted (or failed to get it published quickly enough) by standard scholarly journals arrange for the material to be distributed, in mimeographed or photocopied form, through various more or less informal channels. Scholars have always sent their colleagues copies of forthcoming articles for comment, of course, but previously such informal dissemination of ideas did not 'count'; it was merely a preparation for the fully public advancement of knowledge by way of properly printed journal-articles and books. Within the Chomskyan school of linguistics, however, 'underground publication' counts for a great deal, and much has been felt to hang on whether or not one was on the mailing-list of the scholars whose reputation stands highest (cf. McCawley 1976, p. 2). There have been cases of linguists who are accepted as having established a secure scholarly reputation almost entirely on the basis of articles distributed in this informal fashion.[13] The problem about this style of scholarship is that 'underground' work does not normally attempt to meet the standards expected by responsible academic publishing houses or editors of reputable journals; it is regarded as quite permissible in a 'Working Paper' or 'Report of Research in Progress' to omit the hard grind of checking details of data, verifying the references, dealing exhaustively with

recalcitrant counter-examples, and the like. As Hagège points out (1976, p. 35), when the ideas of these scholars are successful they take the credit, but when their work turns out to be thoroughly bad they shrug if off as never having been meant very seriously anyway.

For all the reasons discussed in the preceding paragraphs, members of the Chomskyan school (together with onlookers who take them at their own self-evaluation) usually have an exaggerated impression of how much this school has actually discovered about language. A not untypical view is expressed by Paul Postal (1972, pp. 161–2), who says (referring to Otto Jespersen's monumental seven-volume *Modern English Grammar*, published over the forty-year period 1909–49): 'of course we [Chomskyans] have probably uncovered since the early sixties [i.e. in less than a decade – Postal's paper was originally given as a talk in 1969] more new facts than could be put in a dozen works like Jespersen's biggest'. If Postal is referring purely to the physical quantity of documents circulated by members of his school, then certainly he is right. It is so much easier to do research in the Chomskyan style that far more has been done by Chomskyans than by other schools in a comparable length of time. But the overwhelming majority of the 'facts' Postal has in mind simply are not facts at all.

In many (perhaps most) cases they concern sentences which are claimed to be 'ungrammatical' where what is really meant is that the writer has not succeeded in thinking of a situation in which the sentence would make sense. In his first book, *Syntactic Structures* (1957), Chomsky was careful to draw the distinction between ungrammatical word-sequences and sentences which are nonsensical but syntactically well formed – his famous example of the latter category was *Colourless green ideas sleep furiously*. To call a word-sequence ungrammatical is to say that it simply does not conform to the structural norms of the language, which seems to be a yes-or-no matter; to say that a sequence is nonsensical is to say that it *does* conform to those norms, but that one cannot see any use for that individual example – which is a comment about one's own powers of imagination rather than about the language. (Not surprisingly, the implied challenge was soon taken up in the *Colourless green ideas* case: Harman 1974, p. 1.) But Chomsky soon reversed himself on this issue in practice (*Form*, pp. 80 ff.), and few if

any of his followers have ever taken much account of the ungrammaticality/nonsensicality distinction (probably because, although the distinction is of great importance methodologically, our introspective faculty seems rather insensitive to it).

In many other cases, Chomskyans' new 'facts' are genuinely beliefs about the syntactic rather than semantic status of a sentence, but the beliefs are based purely on introspection and are as likely to be false as true. When the 'facts' are statements about linguistic universals rather than about an individual language, in most cases they are hypotheses which were once put forward tentatively but have long since been abandoned even by their author (the informal style of publication common among the Chomskyans makes it difficult to discover which proposals have been retracted). And even 'facts' about linguistic universals which have stood the test of criticism of the kind practised by the Chomskyan community usually turn out never to have been tested against observational evidence, so that they can hardly be regarded as facts in any ordinary sense.

It is true that there are a number of linguists working today who regard themselves as belonging to the Chomskyan rather than to any other school, but who either base their linguistic analyses on documentary evidence, or, if they do not go as far as that (since nowadays it does not do to give people a chance to call one an empiricist), at least use intuitions exclusively about categories of fact which in principle could be checked against observation and which seem very likely correct. (No one denies that we have many true intutions about our native language; the empiricist insists only that such intuitions must not be treated as authoritative.) But the more 'respectable' (by empiricist standards) these scholars are, the less specifically Chomskyan their work is – particularly since those who are most empiricist in their method tend to make relatively few claims about universals. The best of these scholars are to all intents and purposes continuing the Bloomfieldian, Descriptivist tradition without acknowledging the fact; and there would probably be many more like them, if Descriptivist linguistics had not been given such a bad name.

Clearly, the ascendancy of the Chomskyan school has been a very unfortunate development for the discipline of linguistics. It has occupied many men's attention and has produced a very large corpus of doctrine, and people naturally feel that this work

cannot, surely, all have been in vain; but people no doubt felt similarly about astrology or alchemy when these were flourishing activities, yet we know they were wrong. Is there nothing, then, to be saved from the wreck?

In fact I believe there is; but it has very little to do with the mass of activity by a plethora of scholars over the last twenty years, because it was already stated, about as adequately as it ever has been since, in Chomsky's first book. What I have in mind is the special role of hierarchical structure in the syntax of all human languages. The significant point in Chomsky's *Syntactic Structures* is not the claim that human languages are generated by transformational grammars, which is very possibly a vacuous claim and is in any case not very well confirmed even if empirical; what matters is the statement that, at least to a very close approximation, all human languages can be generated by constituency grammars, and that there is no reason in logic why that should have to be so. It is mathematically proven that many 'languages', in the sense of well-defined classes of morpheme-sequences, cannot be generated by constituency grammars; and I would be prepared to argue that constituency notation is not just very nearly but completely adequate to generate any human language (*Form*, pp. 205–6). If that is so, then Chomsky is right to claim that human languages are all 'cut to a common pattern'; and he *may* be right to infer from this that our species inherits complex, non-plastic psychological machinery which largely determines the structure of our mental life.[14]

The hypothesis that all human languages have constituency grammars can be tested against purely observational evidence, by attempting to construct such grammars to generate the ranges of utterances which we hear or read produced by speakers of different languages in their unstudied moments. As I say, confirmation of the hypothesis might justify Chomsky in adopting a rationalist rather than empiricist theory of the human mind. But nothing about such a discovery could conceivably justify us in abandoning empiricism as a *scientific methodology*; to confuse empiricism as a theory with empiricism as a method is a naïve fallacy.

And certainly nothing in Chomsky's argument for rationalist theory justifies the way in which, for a decade or more, the energies not just of a few enthusiasts but of almost an entire

discipline have been diverted away from the task of recording and describing the various facets of the diverse languages of the world, each in its own terms, towards that of fitting every language into a single, sterile formal framework, which often distorts those aspects of a language to which it is at all relevant, and which encourages the practitioner to overlook completely the many aspects of language with which it is not concerned. This has been simply a wrong track taken by linguistics. Happily, in the late 1970s there are many signs that the discipline is returning to a more healthy, pluralist mood. 'Some welcome breezes are blowing now across the formal desolation', to quote one scholar who never succumbed to the orthodoxy (Bolinger 1977, p. 519).