Part I

BACKGROUND

1 Introduction

1.1 Purpose of the study

This book is primarily a contribution to a relatively new branch of linguistic inquiry, 'foundational analysis'. The term and much of the inspiration for this book come from Stuart (1969), the leading study in this area to date. Like Stuart in that more compact statement, I am mainly interested in seeking answers to such fundamental questions as the following:

(1) What *metatheoretical and methodological assumptions* underly the currently dominant paradigm of linguistic investigation, namely transformational-generative grammar (henceforth TGG)? To what extent are these assumptions tenable and viable?

(2) What is *the status of a linguistic description* (or, as the notion is most commonly construed, of a transformational-generative grammar)? What kind of reality does such a description represent, and to what extent are its claims to a specific ontological status justified?

(3) In sum, to what extent is linguistics, as defined primarily by the paradigm of the generative grammarian, concerned with *establishing knowledge* about some aspect of the real world? Where does this knowledge end and supposition and speculation begin? In other words, to what extent is linguistics as understood today to be taken seriously as a scientific discipline?

Such questions are fundamental to linguistics, as they would be to any field which claimed allegiance (as linguistics does) to the aims and methods of empirical science. Nevertheless, these questions have had surprisingly little attention. One reason for the oversight may be that, until recently, no one really cared much, as the answers to such questions mattered only to a small group of specialists (linguists) whose influence on the world outside their own circumscribed domain of interests was negligible. Today, however, chiefly through the influence of Noam Chomsky, this picture has changed. The influence of current work in linguistic theory is now felt in other disciplines, especially in psychology (beginning with the Chomsky and Miller papers in Luce, *et al.*, 1963) and in philosophy (as indicated by the appearance of such books as Hook, 1969). Moreover, the number of scholars now directly involved in the development and dissemination of linguistic information has grown substantially during the past decade. As a consequence of this rapid growth of influence, linguistics suddenly finds itself far more familiar and popular than before, with news of some of its basic controversies even reaching the popular press (as in the *Time* article of 16 February 1968 on 'The Scholarly Dispute Over the Meaning of Linguistics', the Sklar interview and commentary on 'Chomsky's Revolution in Linguistics', which appeared in volume 207 of *The Nation* that same year, and Mehta's perceptive interview with Chomsky, Hockett and Jakobson in 8 May 1971 issue of *The New Yorker*.) Increasingly, it is coming to matter more and more whether or not linguistics is indeed intent upon the establishment and dissemination of knowledge about some aspect of the real world or whether it is merely playing at science, surrounding itself with much form but little substance.

Considering the substantial investment in time, effort, people and money devoted to research in linguistics today (and especially under the banner of transformationalism), there is another reason for serious exploration of the questions which concern us here. For the linguist, like his fellow researchers in other fields, has to carry out his work in a world beset by serious problems whose resolution may involve the survival of the human species – everything from overpopulation, to environmental pollution, to the inequitable distribution of wealth and resources to the problems of the arms race and war itself. It is all the more obvious that the linguist will have to compete for the dwindling research dollar with agencies and individuals involved in projects of great social, economic and political import. Unless he can demonstrate that the goals he pursues are worthwhile and his methods adequate to the task and likely to bear fruit in real understanding of the nature of human learning or human cognition (for example), he won't get the money.

To illustrate: for the past several years linguistics departments all over North America have been turning out what can be described as a generation of professional grammar writers. This has been primarily because the dominant paradigm (that of TGG) sets great store by the construction of such grammars, in the enunciation and explication of the general principles underlying their construction and in the implications of such work for other disciplines (notably psychology). Yet the empirical status of these grammars has not been analyzed (cf. question 2 above), nor has the basic soundness of the underlying principles (question 1). A central thesis of this book is that the empirical status of such grammars is sufficiently obscure and the viability of the basic assumptions sufficiently in doubt to warrant a fundamental shift in the attitude of linguists both to the nature of the work they do or ought to be doing and of the results they achieve or ought to be achieving. It is characteristic of foundations that they support the superstructure, and linguistics is no exception. It will be a chief contention of this book that current work in linguistics along transformational-generative lines is yielding little in the way of substantive accomplishments of any empirical significance, while offering much in the way of unsupported (and, given present methods of research, unsupportable) speculative assertion, with the larger portion of current metatheoretical discussion being devoted to marginal or irrelevant 'smoke screen' issues which hide deep conceptual and methodological difficulties.[1]

1.2 Rationale for the study

The reader will gather that this book is an attempt to develop a unified and reasonably thorough critique of the theoretical foundations of TGG. Of necessity, therefore, it takes on more of a philosophical flavor (focused primarily at the level of methodology) than a strongly data-oriented one. The problem of language acquisition in children has been a serious study for centuries, but I allude here only briefly to small and isolated parts of the vast (though relatively uncontrolled) literature, making no attempt to survey the whole. That work still begs to be done, of course, but my purpose here is different. My point of departure is not the laboratory, but linguistic theory. I justify my title by arguing (particularly in chapter 3) that all linguistic theorizing is, at base, theorizing about the nature of language acquisition, just as Whitney said a century ago (see epigraph). In the final analysis, therefore, I am concerned here with developing and testing what might be called alterna-

[1] I would contend, for instance, that it makes little sense to argue at great length whether or not a level of 'autonomous phonemics' fits naturally into a generative grammar so long as the notion of a generative grammar itself, construed as a model of 'linguistic competence', remains as confused and ill-defined as it is at present. (See 6.1 and chapter 8 below for detailed treatments of these particular issues.)

tive *linguistic theories of language acquisition*, and with attendant questions related to their essential tenability (Are they the right *kind* of theories? Do they make sense?) and viability (Are they appropriate heuristic devices for increasing our knowledge of the phenomenon of language acquisition? Can they be generalized to account for other kinds of learning?).

My primary audience, then, is my fellow linguists. Non-linguists, as a consequence, may have difficulty coming to grips with the decidedly in-group orientation adopted here. I have tried to avoid using too much linguistic jargon and its formalisms in the hope of making this book accessible to as large and heterogeneous an audience as possible, but the problem goes far deeper than that. For one thing, the outsider (and many an insider, for that matter) may fail to see the point of my excursions into areas of seemingly dubious relevance, while others may think that I appear at times to be belaboring the obvious.

Indeed, I find myself torn between two conflicing interpretations of what it is that I am doing here. On the one hand, I worry that an embarrassingly large portion of what I say is so obvious that it borders on the trite, or even trivial. At other times, however (and particularly when a new book in linguistics or language acquisition or a new issue of a major linguistic journal appears), I find myself in a different frame of mind: I begin to wonder whether the whole field of linguistics has not lost its senses. Yet one continues to read the same kind of thing so often (and in larger and more expensive books all the time – some of which are major incursions into other fields of study[1]) that one gets the definite impression that these statements are intended to be taken seriously and even thought by many to be solid advances in our knowledge of the nature of language and the process of language acquisition, despite the seemingly obvious and fundamental considerations which by now strongly indicate to me the opposite. Even when it comes to the issues at present most heatedly debated within linguistics, it is often the case that the basic questions involved in these discussions are ones which other disciplines have debated to the point of exhaustion and have resolved by reaching a set of generally accepted conclusions.[2] Yet these conclusions,

[1] Cf. the recent outpouring of new books on psycholinguistics and language acquisition with a strong transformational-generative orientation (e.g., Brown, 1970; Deese, 1970; Hayes, 1970; McNeill, 1970a; Slobin, 1971c; and Dale, 1972).

[2] To give three illustrations which I shall refer to again later, consider the old notion in biology (and psychology) that 'ontogeny recapitulates phylogeny' (cf. 4.5.2), as well as the truly ancient 'nature vs. nurture' and 'induction vs. deduction' controversies (cf. 3.5 and 7.1 respectively).

sound as they appear to be, remain unknown to, or have been forgotten by, or have failed to make an impression on linguists, long isolated from other branches of science. It is necessary to inject (or reinject) some of these fundamentals into current linguistic meta-discussion, if only to spare linguists having to go over again ground which has already been well surveyed for us by others. True enough, what is 'obvious' or 'generally accepted' by researchers in other fields may not be appropriate or right for linguists (or even for the other disciplines), but neither should such matters be neglected or ignored.

My dilemma is very much the one described by Koestler (1967, p. 4):

> If one attacks the dominant school in [some field]...one is up against two opposite types of criticism. The first is the natural reaction of the defenders of orthodoxy, who believe that they are in the right and that you are in the wrong – which is only fair and to be expected. The second category of critics belongs to the opposite camp. They argue that, since the pillars of the citadel are already cracked and revealing themselves as hollow, one ought to ignore them and dispense with polemics. Or, to put it more bluntly, why flog a dead horse?

I suspect that the number of scholars in the second of these two categories with respect to Chomsky's views is larger than the average transformational-generative grammarian might like to think (especially if he feeds his mind mainly on unpublished papers circulated privately among a closed circle of like-minded colleagues and friends). There are some people (and not all of them ill-informed or irrational), who find little or nothing which is compelling enough in Chomsky's work to be worth the trouble of criticizing. Yet here I am producing a book which, from that point of view, is simply an overindulgent exercise in 'overkill'. So why do I bother?

Having been trained as a linguist, I naturally feel a close affinity, if not identification, with linguistics over and above any which could possibly be felt for any other discipline, at least for the time being. Consequently, I feel concerned about the sort of image which linguistics presents to the world. To enlightened and severe critics this image is very tarnished: to them, linguistics isn't even worth discussing. But, oddly enough, the impression one gets from within the field is that this image is not the one which *most* outsiders see. To the contrary, the general feeling among linguists seems to be that scholars in other fields have more to learn from linguistics than vice versa and, moreover, that most of these outsiders are becoming more and more aware of this.[1] But in selling one field to another one does have certain inherent advantages, does he not? For one thing, most outsiders are too busy with their own work to have time to analyze the claims which linguists make; if these ambassadors from linguistics talk easily and confidently about their subject, they are likely to be given the benefit of the doubt, particularly if there is a good deal of obvious enthusiasm thrown in for good measure. (After all, a whole discipline cannot be in error, can it?) The self-confidence and apparent unanimity of transformational-generative grammarians has thus proven successful even to the point of persuading the occasional well-known psychologist to hop onto the Chomsky bandwagon, often with little awareness of what it is he is selling.[2] It is to be expected that enthusiasts are going to advocate their own point of view, and I agree with P. Harris that this can be tolerated up to a point (1970, p. 4). But without guidelines to help us determine when that point is reached, there is danger of advocating ideas which are merely very popular, though still open to considerable doubt, as if they were well-established truths. As Seuren says:

> There is no denying that transformational grammar is, broadly speaking, well-established. That is precisely the reason why it must be taught. But it is not well enough established to teach it as a corpus of doctrine in the same way as natural sciences are taught. The pedagogical difficulty in teaching generative grammar is mainly that we want our students to become aware of the debatable and tentative nature of most, if not all, grammatical descriptions given in transformational terms (1969, p. 188).

If this is true in the training of linguists, then it ought all the more to be kept in mind in our dealings with other disciplines. There is an important professional responsibility here, namely, that we must be careful to

[1] So McNeill, in his contribution to Mussen (1970), shuns a literature survey of the sort his predecessor undertook (McCarthy, 1954) on the grounds that 'recent developments in linguistics pose important issues *for psychology*, and examination of them takes priority within the limits of space over comprehensive citation' (p. 1062; italics added).

[2] Cf. Deese, 1970, for example: 'The most significant function [TGG] has had in the past decade...has been to provide our ideas about language with a firm theoretical foundation...Generative theory gives us the means of characterizing those things that are really essential to language in a precise and detailed way...One of the consequences of the development of generative theory has been to show that learning theory, when it is carefully and strictly interpreted,...cannot apply to the way in which people acquire, use, and understand ordinary human languages. Those who have tried to apply learning theory to the problem of how people use languages do not really understand what language really is' (pp. 2–3).

offer people not in a position to verify our claims for themselves a clear differentiation between the speculative assertion and the well-established fact; otherwise, we risk doing our discipline a disservice. One reason, therefore, for an extended critique of Chomsky's views is the very fact that these views have been so widely disseminated and popularly accepted, and that Chomsky's reputation and influence extend across the borders of linguistics itself.

Chomsky's reputation and influence are such that he is accepted as a spokesman for the entire field of linguistics. It is all the more important that workers in other fields be made aware that Chomsky does not, by any means, speak for *all* of us and that, if the truth were known, all is by no means as well within linguistics as they might be led to believe. By this time a subtle, yet quite perceptible, undercurrent of unease is felt within linguistics, even among generative grammarians, though few are aware of the full extent of the difficulties which confront them. To be sure, there is talk of 'real problems with the theory', but usually in the context of 'hope for developments and modifications in Chomsky's theorizing that will eventually take care of the limitations that seem apparent now' (G. Faust, 1970, p. 47). Another reason for this book, therefore, is to convey to the public at large that many linguists are worried about a great many things; to illustrate some of the particular problems which cause this concern; and to develop the thesis that far more radical measures than mere 'developments and modifications in Chomsky's theorizing' are going to have to be adopted in order to bring about any satisfactory solution.

1.3 Some problems of strategy

1.3.1 On Kuhn's notion of the paradigm. Although there are indications that a thoroughgoing critique of the sort I propose is likely to get more attention and acceptance within linguistics today than it could have even five years ago, it would still be a serious mistake to underestimate the difficulties. For the frame of reference under attack here has no doubt achieved the status of a full-fledged 'paradigm' in Kuhn's sense. That is to say, TGG is today the most widely accepted body of theory in linguistics, is taught almost universally as part of initiation into the field, and serves thereafter 'to define the legitimate problems and methods of research' for most of its professional practitioners (cf. Kuhn, 1962, p. 10). Moreover, linguists on the whole seem quite convinced that it is altogether proper (and even advisable) that one particular frame of reference should predominate in this way in their discipline. Quite possibly it was Kuhn who inculcated this particular attitude. He argues persuasively that without a governing paradigm, any science remains 'immature' in the sense that data-collection at that stage is essentially random, often yielding confusion (pp. 15–18). Hempel makes the same point that 'tentative hypotheses are needed to give direction to a scientific investigation' (1966, p. 13). And, indeed, it does seem that if one is ever to have any hope of finding anything, he must have in advance some idea just what he is looking for, lest he remain buried forever in a morass of heterogeneous 'facts'. It is the function of the paradigm to give us this idea.

Having adopted a particular paradigm, then (such as the paradigm of TGG in linguistics), there are advantages in sticking with it, through thick and thin, at least until all its logical possibilities have been fully explored. Kuhn outlines some of these advantages:

> Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them (1962, p. 163).[1]

The paradigm-oriented scientist, in short, need not necessarily be involved in the solution of important or significant problems (from the standpoint of value to society at large), but only with what Kuhn calls those particular 'puzzles' which the dominating paradigm defines as legitimate and to which solutions are more or less assured (p. 37). In this way a whole universe of potential distractions is conveniently avoided, a great concentration of effort permitted on a small range of specific problems, and continued progress guaranteed towards their solution. As Kuhn puts it, 'By focusing attention upon a small range of

[1] Cf. Jungk, citing Franck: 'It is a custom in science – and perhaps a principle – to select from the infinite reservoir of unsolved problems only those simple ones the solution of which seems possible in terms of available knowledge and skills' (1958, p. 34).

relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable' (p. 24).

Yet there is another side to this picture. There are distinct disadvantages, as well as advantages, in the adoption of a particular strict paradigmatic orientation in any field. Kuhn hints at this when he remarks that what he calls 'normal' (or paradigm-oriented) science

> seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies (1962, p. 24).

In other words, whereas pre- or non-paradigmatic research adopts, of necessity, an essentially 'fox' orientation, normal science involves instead a distinctly 'hedgehog' approach, and each has its weak points; in particular, while the former is disoriented, the latter is 'loaded'.[1] The fact that beliefs (or paradigms) can stand in the way of crucial discoveries and distinctions (cf. Toulmin, 1963, p. 95) and even distort reality to suit one's own ends or expectations is so familiar that it has been given a name: it is the phenomenon of 'mental set'.[2] We all have an insidious natural tendency to see only what we want or expect to see, and this can seriously interfere with the objectivity with which we draw many of our conclusions.[3] It is one thing to *think* one has solved a problem (i.e., to get that exhilarating 'Eureka feeling' of which Koestler speaks (1964, pp. 107ff)), but quite another to have *actually* solved it: 'The trouble with "Eureka"', Caws observes, 'is that the temptation to shout it is a very poor index of success in the enterprise at hand' (1969, p. 1375). Thus errors of honest miscalculation or premature conviction, being more subtle in their effects than prejudice, ignorance or outright

[1] The fox knows many things; the hedgehog knows one big thing. For a clear and concise comparison of these two orientations and the effects each can have on research, see M. Maddocks' two book reviews under the caption 'Could Things Be Worse?' in the 9 November 1970 Canadian issue of *Time* magazine, p. 88.

[2] Hyman (1964, pp. 33–5) and Kuhn (1962, pp. 62–4) both provide excellent illustrations involving the use of playing cards to demonstrate the effects of this familiar, but important, limitation in the capacity of the human being as an observer.

[3] As Kuhn puts it, 'In science, as in the playing card experiment [cf. n. 2 above], novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation' (p. 64).