EINSTEIN AND CHOMSKY ON SCIENTIFIC METHODOLOGY*

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Two contrasting views of science might be summed up like this:[1]

(1). There exist a certain set of procedures and operations with which the scientist operates on the data to discover a theory to describe the phenomena under investigation. The theory is derived from the data by what might be called inductive processes. If these inductive processes, which consist of explicit procedures and operations to be performed on the data, are followed faithfully, then the investigator will arrive at an empirically motivated theory to describe the phenomena under consideration. The empirical motivation for accepting (or rejecting) a theory stems from the data which give rise to the theory, i. e. the data which played a role in its discovery. In this view, the discovery of a theory and the justification of a theory are a single process; discovery and justification cannot be distinguished.

(2). There does not exist a set of procedures and operations with which the scientist operates on the data to discover a theory to describe the phenomena under investigation. A theory is a product of the human imagination. A theory is a conjecture, which can be formulated in mathematical terms to comprise a deductive system, which is tentatively advanced as a possible description, or explanation, of the phenomena under investigation. The means by which a theory is arrived at are irrelevant in determining its empirical adequacy. The theory derives its total empirical motivation from the comparison of the consequences deduced from the theory with observable experiential phenomena. In this view, the discovery of a theory and the justification of a theory are two very different processes.

Before discussing linguistics as a science, let us examine (1) and (2) in order to clarify their content. These two assumptions about the nature of scientific activity have a long history outside of linguistics.

In 1620 Francis Bacon championed (1) over (2) in his *Novum Organum.* He criticized those working in his time for going too rapidly from the data, i. e. 'the senses and particulars', to the theoretical constructs, i. e. 'the axioms':

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> There are and can exist but two ways of investigating and discovering truth. The one hurries on rapidly from the senses and particulars to the most general axioms, and from them, as principles, and from their supposed indisputable truth, deduces the intermediate axioms. This is the way now in use. The other constructs its axioms from the senses and particulars, by ascending continually and gradually until it finally arrives at the most general axioms, which is the true but unattempted way. (Bacon, *Novum Organum,* Book I, § 19)

Bacon believed he had devised a recipe for research which anyone could use to make scientific discoveries. His method of extracting the theory from the data is essentially an algorithm which takes no specific talent to apply:

> . . .my way of discovering sciences goes far to level men's wits, and leaves but little to individual excellence, because it performs everything by the surest rules and demonstrations. (ibid: §122)

In the 19th century, J. S. Mill championed (1) over (2). He went so far as to present a set of inductive cannons which, he contended, when systematically applied to the data, would yield an empirically motivated theory of those data. F. H. Bradley 1928 presents a devastating criticism of Mill's inductive cannons.

A champion of (2) over (1) was Whewell, a leading opponent of Mill's ideas about induction. Whewell 1840 contended that in any case in which a theory was proposed, i. e. in any 'inductive process', the theory must be considered an invention of the mind and not as something deduced from the data:

> In each inductive process, there is some general idea introduced, which is given, not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a new generality. In order to obtain our inference, we travel beyond the cases we have before us; we consider them as exemplifications of, or derivations from, some ideal case in which the relations are complete and intelligible. We take a standard, and measure the facts by it; and this standard is created by us, not offered by Nature. (Whewell 1840: Vol. II, Section II, §55)

Considerations directly related to ideas (1) and (2) played a major role in the development of modern physics in the period 1900-1950. Albert Einstein championed (2) over (1). Several others, most notably Ernst Mach, championed (1) over (2). When discussing the epistemological and philo-

sophical implications of quantum mechanics and the atomic theory, Einstein contrasted (1) and (2). Let us examine some examples.
In 1923, Einstein indicated that (2) was no less tenable than (1):

> . . .For even if it should appear that the universe of ideas cannot be deduced from experience by logical means, but is, in a sense, a creation of the human mind, without which no science is possible, nevertheless the universe of ideas is just as little independent of the nature of our experiences as clothes are of the human body. (Einstein 1923: 2f)

In his Herbert Spencer Lecture, delivered in London in 1933, Einstein indicates that the general theory of relativity was a crucial example in that it showed the 'erroneousness' of (1) and the correctness of (2):

> The natural philosophers of those days [18th and 19th centuries] were. . .most of them possessed with the idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind but could be deduced from experience by 'abstraction' – that is to say, by logical means. A clear recognition of the erroneousness of this notion really only came with the general theory of relativity,. . .the fictitious character of fundamental principles is perfectly evident from the fact that we can point to two essentially different principles, both of which correspond with experience to a large extent. . . (Einstein 1933: 273)
> If it is true that this axiomatic basis of theoretical physics cannot be extracted from experience, but must be freely invented, can we ever hope to find the right way? Nay, more, has this right way any existence outside our illusions? . . . I answer without hesitation that there is, in my opinion a right way, and that we are capable of finding it. . . (ibid: 274)

Victor H. Lenzen discusses those who, against Einstein, championed (1) over (2): '. . . During the latter half of the nineteenth century many scientists, in particular Ernst Mach, envisaged the goal of physical science as the representation of processes through concepts inductively derived from sensory experience.' (Lenzen: 375)

I. Bernard Cohen, in his fascinationg article 'An Interview with Einstein', indicates how Einstein viewed the position of Mach:

> Einstein asked Mach what his position would be if it proved possible to predict a property of a gas by assuming the existence of atoms – some property that could not be predicted without the assumption

> of atoms and yet one that could be observed. Einstein said he had always believed that the invention of scientific concepts and the building of theories upon them was one of the great creative properties of the human mind. His own view was thus opposed to Mach's, because Mach assumed that the laws of science were only an economical way of describing a large collection of facts. Could Mach accept the hypothesis of atoms under the circumstances Einstein had stated, even if it meant very complicated computations? (Cohen 1955: 75)

After Mach's death, Einstein strengthened his criticism of Mach's system. In 1922, answering a question presented to him by Emile Mayerson, Einstein said that the most Mach could achieve by his assumptions about science, i. e. (1), was a 'catalog':

> . . .Mach's system studied the relationships which exist between the data of experience. For Mach, science is the sum total of these relationships. It is a bad point of view; in short, what Mach created was a catalog and not a system. To the extent that Mach was a good mechanic he was a deplorable philosopher. (Einstein 1922: 101; See Clark 1971: 288 for discussion of the circumstances which led Einstein to take this stand.)

Einstein felt that the discovery of a theory, in fact any discovery, involves a 'leap':

> . . .There comes a point where the mind takes a higher plane of knowledge, but can never prove how it got there. All great discoveries have involved such a leap. (Einstein cited in Clark 1971: 622)

Einstein makes clear that although the discovery, or better, invention, of a theory goes beyond experience, experience remains the sole criterion for judging the utility of the theory:

> . . .I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. . . (Einstein 1933: 274)

Victor F. Lenzen, in 'Einstein's theory of knowledge', offers this philosopher's eye view of Einstein's ideas:2

> Einstein also has expounded general conclusions concerning the nature of knowledge as exemplified by theoretical physics. His views constitute a contribution to the development of the theory of knowledge as a philosophical discipline. As preparation for a preliminary statement of his doctrine I recall the distinction between sensory experience and conceptual description. Empirical knowledge originates in sense-impressions, but its goal is understanding through concepts. The media of scientific knowledge are concepts of properties of things and processes which constitute natural phenomena. A basic problem of theory of knowledge is the relation of concepts to sensory experience; in this question one may distinguish between the role of experience in the origin of concepts and the function of concepts in ordering experience. According to empiricism concepts are abstracted from experience. An observer perceives several white things, for example, and abstracts from these particulars the common quality whiteness. The concept of the property whiteness, a universal, thus is explained as derived from experiences that exemplify the universal. The doctrine of empiricism as applied to natural science is that concepts and natural laws are abstracted from experience. It can be claimed that Newton viewed the principles by which he described gravitational phenomena as derived from experience. Einstein has declared that Newton's '*hypotheses non fingo*' can be interpreted only in the sense that Newton held that the concepts of mass, acceleration, and force, and the laws connecting them, were directly borrowed from experience. Although Einstein acknowledges the stimulus which he owes to the empiricists Hume and Mach, he rejects an empirical account for the origin of concepts.
> According to Einstein, the concepts which arise in thought and in our linguistic expressions logically are free creations of thought which cannot be derived inductively from sensory experiences. Like Plato, Einstein stresses the gap between data of sense and concepts of thought. He contends that there is a gulf, logically unbridgeable, which separates the world of sensory experiences from the realm of concepts and conceptual relations which constitute propositions. The constructive nature of concepts is not easily noticed, Einstein asserts, because we have the habit of combining certain concepts and conceptual relations definitely with certain sensory experiences. In contrast to John Stuart Mill, who held that propositions of mathematics are inductions from experience, Einstein offers the

> series of integers as obviously an invention of the human mind, a self-created tool which simplifies the ordering of sensory experiences. He asserts that there is no way in which the concept could be made to grow, as it were, directly out of sensory experiences. The concept of number belongs to pre-scientific thought, but its constructive character is still easily recognizable. (Lenzen: 359-60)

Recall Francis Bacon's concept of science: '. . .my way of discovering sciences goes far to level men's wits, and leaves but little to individual excellence, because it performs everything by the surest rules and demonstrations.' (Bacon, Book I: §102). Northrop, in 'Einstein's conception of science', indicates that Einstein's position diametrically opposes that of Bacon. Northrop states:

> The way from the empirical data to the postulates of deductively formulated physical science is a frightfully difficult one. Here, rather than anywhere else, the scientist's genius exhibits itself. The way is so difficult that no methods whatever must be barred; no sources of meaning whatever, imaginative, theoretical, of whatever kind, are to be excluded. It appears that nature covers up her basic secrets; she does not wear her heart upon her sleeve. Thus only by the freest play of the imagination, both the intuitive imagination and the non-intuitive, formal, theoretical imagination can the basic concepts and postulates of natural science be discovered. In fact, Einstein writes with respect to the discovery of 'the principles which are to serve as the starting point. . .' of the theoretical physicist's deductive system, that 'there is no method capable of being learnt and systematically applied so that it leads to the goal.' (Northrop: 394)
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> . . .Einstein affirms that neither the formal, logical relation of implication or any probability or other formulation of induction can define the method by which the scientist goes from the empirical data to the basic postulates of scientific theory. The scientist has, by trial and error and the free play of his imagination, to hit upon the basic notions. Moreover, it has been noted that these basic notions receive their verification only through a long chain of deductive proofs of theorems which are correlated with the inductive data.
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> With time and new empirical information the traditional basic postulates have to be rejected and replaced by new ones. (Northrop: 397)

As everyone is no doubt well aware, the most revolutionary aspect of *Syntactic Structures* was that it defined linguistics as a science of type (2) and thus was opposed to the prevailing opinion that linguistics was a science of type (1). Compare these passages, perhaps the most revolutionary in *Syntactic Structures*, with some of the selections from Einstein's works. In these passages, Chomsky offers his unique definition of 'explanation in linguistics':

> In short, we shall never consider the question of how one might have arrived at the grammar whose simplicity is being determined; e. g. how one might have discovered the analysis of the verb phrase presented in 5. 3. Questions of this sort are not relevant to the program of research that we have outlined above. One may arrive at a grammar by intuition, guess-work, all sorts of partial methodological hints, reliance on past experience, etc. It is no doubt possible to give an organized account of many useful procedures of analysis, but it is questionable whether these can be formulated rigorously, exhaustively and simply enough to qualify as a practical and mechanical discovery procedure. At any rate, this problem is not within the scope of our investigations here. Our ultimate aim is to provide an objective, non-intuitive way to evaluate a grammar once presented, and to compare it with other proposed grammars. We are thus interested in describing the form of grammars (equivalently, the nature of linguistic structure) and investigating the empirical consequences of adopting a certain model for linguistic structure, rather than in showing how, in principle, one might have arrived at the grammar of a language. (Chomsky 1957: 56)
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> Our fundamental concern throughout this discussion of linguistic structure is the problem of justification of grammars. A grammar of the language L is essentially a theory of L. Any scientific theory is based on a finite number of observations, and it seeks to relate the observed phenomena and to predict new phenomena by constructing general laws in terms of hypothetical constructs such as (in physics, for example) 'mass' and 'electron'. Similarly, a grammar of English is based on a finite corpus of utterances (observations), and it will contain certain grammatical rules (laws) stated in terms of the particular phonemes, phrases, etc., of English (hypothetical constructs). These rules express structural relations among the sentences of the corpus and the indefinite number of sentences generated by the grammar beyond the corpus (predictions). Our problem is to develop and clarify the criteria for selecting the correct grammar for each language, that is, the correct theory of this language. (Chomsky 1957: 49)

Both Einstein's revolution in physics and Chomsky's revolution in linguistics caused researchers to shift from perspective (1) to perspective (2). By studying the revolution caused by Einstein in physics, a linguist can see the taxonomy-generative grammar revolution in a broader perspective -- in a perspective which includes modern physics.

An excellent place to start reading about Einstein's revolution in physics is Gerald Holton's book, *The Thematic Origins of Scientific Thought: Kepler to Einstein.*

It is not my intention to review the book. I only wish to point out its relevance to linguists concerned with the evolution of methods in linguistics and with the relation of linguistics to other sciences. Suffice it to say that Holton writes clearly. Although he has packed his book with quotations and scholarly references, his style seldom becomes ponderous and is at times even lively. His multitudinous references will facilitate the linguist's digging into primary sources. His translations are fair to both Einstein and Mach. Holton has no axe to grind nor any Procrustean bed into which he feels obliged to tuck the dramatis personae who, years ago, played out their roles in Einstein's revolution.

A linguist might do well to read chapter 8 before anything else. The material early in the book might not seem immediately relevant to a linguist. But in chapter 8 the linguist will certainly enjoy Holton's discussion of Mach and Einstein. Holton's book contains many passages, such as the following, where Einstein discusses Mach's methods:

> I see [Mach's] weakness in this, that he more or less believed science to consist in a mere ordering of empirical material; that is to say, he did not recognize the freely constructive element in formation of concepts. In a way he thought that theories arise through *discoveries* [emphasis in original, RCD] and not through inventions. He even went so far that he regarded 'sensations' not only as material which has to be investigated, but, as it were, as the building blocks of the real world; thereby, he believed, he could overcome the difference between psychology and physics. If he had drawn the full consequences, he would have had to reject not only atomism but also the idea of a physical reality. (Einstein 1948, letter to Besso, cited in Holton: 231-2)

NOTES

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1. I purposely avoid characterizing (1) and (2) by terms which end in *-ism*. Terms ending in *-ism* supposedly characterize systematic epistemologies which are more or less well-defined. The practicing scientist, however, is seldom restricted in the construction of his conceptual world by strict adherence to a well-defined epistemological system. There is little reason to expect, therefore, that a term characterizing an epistemological system will be useful in characterizing the work of any practicing scientist. Einstein makes this point neatly when he says that the scientist 'must appear to the systematic epistemologist as a type of unscrupulous opportunist':

> "The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is – insofar as it is thinkable at all – primitive and muddled. However, no sooner has the epistemologist, who is seeking a clear system, fought his way through to such a system, than he is inclined to interpret the thought-content of science in the sense of his sytem and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. He accepts gratefully the epistemological conceptual analysis; but the external conditions, which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist: he appears as *realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as the free inventions of the human spirit (not logically derivable from what is empirically given); as *positivist* insofar as he considers his concepts and theories justified *only* to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research.
> All of this is spendidly elucidated in Lenzen's and Northrop's essays". (Einstein 1949: 684).

2. I quote passages from Lenzen 1949 and Northrop 1949. Concerning these essays, Einstein says:

> The essays by Lenzen and Northrop both aim to treat my occasional utterances of epistemological content systematically. From those utterances Lenzen constructs a synoptic total picture, in which what is missing in the utterances is carefully and with delicacy of feeling supplied. Everything said therein appears to me convincing and correct. Northrop uses these utterances as a point of departure for a comparative critique of the major epistemological system. I see in this critique a masterpiece of unbiased thinking and concise discussion, which nowhere permits itself to be diverted from the essential. (Einstein 1949: 683).

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