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ON BEING A LINGUISTIC ANTHROPOLOGIST

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Like those who have preceded me in writing Overviews for the *Annual Review of Anthropology,* I became an anthropologist through a series of accidents. It was still true during my undergraduate years at Columbia (1932–36) that anthropology programs existed only at the graduate level, and even then at only a few universities. The central figure was, of course, Franz Boas at Columbia, and anthropology programs at other universities were, to my knowledge, all founded by students of Boas. Anthropology during that period had a very important component in museums and governmental institutions such as the Bureau of American Ethnology, a role which has diminished proportionally during my life. Because of Boas' stature in the field and its chronological priority, the Columbia program during the period that I was an undergraduate there had an indisputable primacy in the country.

As an undergraduate, however, I was completely unaware of all this until the summer of 1935, just before I entered my senior year. During that summer I chanced to read a volume edited by Calverton, *The Making of Man* (5), available in the then popular and easily affordable Modern Library series. My curiosity was aroused, and I noticed in the catalogue that there was a course in general anthropology (actually the only undergraduate course offered). It was taught by Alexander Lesser, himself a student of Boas.

My interests up to that time seemed, at least on the surface, to be quite different. My main field of study had been language which fascinated me from an early age. During my high school years and even a bit earlier I had acquired the habit of studying languages independently by reading grammars and texts.

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At James Madison High School in Brooklyn, I took courses in Latin and German, but I had a great desire to study Classical Greek, which I learned was given at Erasmus High School. An attempt to transfer was foiled by a bureaucratic rule, namely that I lived in an area assigned to James Madison. Had I chanced to live two blocks away I would have attended Erasmus. I still remember that my parents took me to see the Principal of Madison about this problem. He asked me rather pointedly why I wanted to study Greek and I had no real answer, simply that I wanted to.

That very day, seeing that I was heartbroken over the incident, my father took me to a second-hand bookstore and bought me a grammar of Greek. I should mention that I had already made some attempt to learn Greek. I had discovered that a local public library, contrary to any reasonable policy of acquisition that present-day librarians would devise, had somehow obtained several volumes of Jebb's edition of the plays of Sophocles. As I remember, they were the *Ajax* and and the *Oedipus at Colonus*. The Greek text was on one side and the English translation on the other. I tried to analyze the Greek texts with the help of the Abridged Oxford Dictionary of English which we had at home. In this dictionary all English words derived from Greek were given in the Greek alphabet. This allowed me to decipher the alphabet and also provided me with a fair-sized vocabulary.

When I acquired the grammar, it was indeed a revelation. The varying forms of the noun and verb which I had puzzled over now became clear. There were cases, genders, tenses, and other categories that I had never heard of before but some of which I would soon encounter in my study of Latin and German. However, I never did find a dictionary so it was still very hard going.

Later, when I attended Columbia, I took courses in Latin and Greek. In addition, I had begun to teach myself Classical Arabic even in high school and couldn't help noticing how similar it was to the Hebrew I had studied in Talmud Torah. The reason for this similarity, of course, eluded me.

It was the glory of Columbia in those days that there were scholars in such languages as Arabic, Akkadian, and the Slavic languages who were members of Columbia's teaching staff and who listed language courses in the catalog which almost no one ever took. As a result, they were free to devote all their energies to research. I suppose I was a sort of nuisance in that I took these courses, invariably being the only student in the class.

By my junior year I had become aware of comparative linguistics and took a course with Louis Gray, which as far as I can recall was essentially a course in comparative Indo-European. Given these interests, had it not been for the complete absence of academic jobs in such subjects during the depression, I would quite naturally have become a Classical or Semitic scholar, or perhaps a specialist in comparative Indo-European. The Chairman of Classics had indeed informed me that there was no future for a scholar with an interest in the Classics.

The events that changed the course of my life occurred in 1936–37. As I mentioned earlier, my interest in anthropology had been aroused by the reading of Calverton's book, and as a result, I enrolled in Lesser's course in undergraduate anthropology. I noted also that there was a graduate course given by Boas in American Indian languages, and I was given permission to audit it. Actually, I fully participated in the course with perhaps five or six graduate students. At about that time I discovered in the Columbia library the "Handbook of American Indian Languages," edited with an introduction by Franz Boas (4). I read all the grammars in the volumes. They seemed strange and fascinating in their differences from each other and from the Indo-European and Semitic languages that I had studied.

All this occurred during my senior year, while the problem of what to do when I graduated from Columbia loomed on the horizon. I formed a completely unrealistic plan, a fantasy would be the more appropriate term. The previous year, under a grant from the National Youth Administration, I had assisted a professor of medieval history in the translation of the late Christian Latin historian, Orosius. I thought that my background in Semitics and Classics would qualify me to be a medieval historian with special emphasis on the contacts and mutual influences of the Christian West and the Islamic East. However, I did not mention these plans to the medieval historian with whom I had worked.

In the meantime, Lesser in his introductory course in anthropology required a term paper, and he interviewed each student about the choice of a topic. I said that I wished to write a paper about the celebrated medieval Arab traveler Ibn Batutah, who had, in the course of his journeys, crossed the Sahara and visited the African kingdom of Mali. Lesser replied that it was more like a lifework than a term paper, but he approved the topic.

He then went on to ask what I planned to do after graduation from Columbia. I replied that I was going to specialize in Medieval History. Where would I get the money and to what university would I go? I didn't have the faintest notion. He then informed me that there was an institution called the Social Science Research Council which was initiating a new program of graduate fellowships in the social sciences. He suggested that in view of my background, I should apply and ask to go to Northwestern University to study with Melville Herskovits, then the leading Africanist, concentrating on the study of Islam in Africa.

He said I could obtain supporting letters from Boas and Ruth Benedict. The former I knew from the course I was auditing in American Indian languages. Besides, Boas had ascertained that I could read Russian, and I prepared for him a translation of extensive portions of the sections on the Gilyak in a work of Shternberg (18). Boas was interested in the cultural connections of the peoples of the Northwest Coast with those of Northeastern Asia, among whom were the Gilyak.

As for Ruth Benedict, I had never met her, but Lesser brought me into her office and we talked for a brief period. I will always remember her graciousness and her assistance to me on this and later occasions, although my own interests were quite distant from her central concern with psychological anthropology and the then popular area of Personality and Culture.

I applied for the SSRC grant and received it. I was still basically interested in linguistics, but from Boas's course on American Indian languages and from Lesser's class lectures in which anthropology was said to consist of four subfields, one of which was linguistics, it seemed possible to me to pursue my linguistic interests as an anthropologist. I then proceeded to Northwestern to study in a two-member department in which no linguistics was taught. I did, however, become acquainted with Werner Leopold, the great pioneer in the field of child language acquisition.

In those days Yale was the great center for linguistics. As far as I know, it was the only university with a separate linguistics department. The usual thing in those days was for those interested in linguistics to participate in the linguistics clubs that existed at every major university and were the common meeting ground for faculty and students from English and various foreign language departments as well as anthropologists. Even Leonard Bloomfield, the acknowledged leader of linguistics in the United States, in the list of founding members of the Linguistic Society of America published in *Language* in 1925, listed his affiliations as German and Linguistics. He had originally been a member of a German department at Ohio State before coming to Yale.

Because of my interest in linguistics and its virtual absence from Northwestern, Herskovits encouraged my plan to spend my second year of graduate study at Yale. There I took courses from Leslie Spier and Robert Lowie. As far as I can remember, I took only one course in linguistics and am no longer certain whether I was a registered student. It was Bloomfield's course on Comparative Indo-European. I did become acquainted with other linguists at Yale, notably the Hittitologist and Indo-Europeanist Edgar Sturtevant and the Sanskritist Franklin Edgerton.

After the year at Yale, I undertook fieldwork in Northern Nigeria among the Hausa. My topic was the comparison of the Muslim Hausa religion with that of one of the very few communities of still surviving non-Moslem Hausa (the Maguzawa). I learned Hausa and used it in my fieldwork and subsequently wrote a few articles about it. The importance of this experience was not confined to Hausa. It aroused in me an interest in the very controversial problems of the time regarding the classification of African languages in which the genetic position of Hausa played an important role. This led ultimately to my general work on the historical classification of African languages (9).

In my next year of graduate work, under relentless pressure from Herskovits, which I appreciate in retrospect, I finished my dissertation, which was subsequently published in book form under the title *The Influence of Islam on a Sudanese Religion* (7).

After I received my doctorate there were just no academic jobs. I wished to continue with linguistics and especially to work with Edward Sapir, who by that time was at Yale. I therefore applied for and received an SSRC postdoctoral grant that once more took me to Yale. I heard there that Sapir was ill and was living at home in New York. I felt very shy about meeting him and taking up his time under the circumstances. As a result, I never made his acquaintance, but I did audit courses with Bloch, Trager, and Whorf. In spite of my earlier contacts with Bloomfield, this was my first real acquaintance with the sort of structural linguistics then dominant in the United States.

In the absence of any prospect for academic employment, I almost felt relieved when in early 1940 I was drafted into the Army. I was supposed to serve for one year, but in my heart of hearts I knew better. I actually ended up serving for about five years, including a period overseas in North Africa and Italy. When I was mustered out in 1945, there was a pentup demand for new academic appointments because of the virtual freeze in wartime and the great influx of veterans under the GI Bill of Rights.

I soon received a position at the University of Minnesota, where I stayed during academic 1946–47. I then went to Columbia, where I remained until my move to Stanford in 1962, where I spent the rest of my academic career.

From the preceding account it is clear that for me a professional career in anthropology was essentially a way of practicing linguistics at a time when even graduate programs in linguistics were almost nonexistent. Had I been born 40 years later, I would in all probability have chosen to carry out my undergraduate and graduate work in linguistics programs.

In retrospect, however, I believe that the series of accidents that brought me into anthropology was a fortunate one. It was because of the specific anthropological training I had in African studies that my first major project had to do with the historical classification of African languages, a project which also gave me a wide acquaintance with a whole series of language structures that were different from Indo-European, the only family of languages in which I had any comparative training. More broadly, it made me aware of the social and cultural dimensions of language in a far more vivid and direct way than would have been possible in a purely linguistic program. Beyond that, my first acquaintance with Lowie and his interest in the history of anthropology helped focus my rather diffuse interest in the history of ideas into a concentration on the history of anthropology. I consistently gave courses on this topic throughout my periods at Columbia and Stanford. It helped me to put developments in linguistics itself into a wider perspective of the history of the social sciences and to view them both historically and in their relation to other fields. Eventually I gave a course in the history of linguistics also.

From the foregoing account it should also be clear that I did not have any coherent training in linguistics. I became an anthropologist by training, but to a large extent I made myself into a linguist. Paradoxical as it may seem, the absence of an organized program in linguistics turned out, I believe, to be an intellectual advantage. It meant that I had no fixed emotional or intellectual commitment to the structuralist approach which dominated American linguistics during the earlier part of my career.

At Columbia, from the first, I gave courses in such topics as Language and Culture and Linguistic Field Methods. At Stanford I participated actively in linguistics activities. When I arrived it was only a program without an undergraduate major, but it has since developed into a full-fledged department with an undergraduate program. From the beginning I was a member of the program and later a member of both anthropology and linguistics departments until my recent retirement. I owe an enormous debt of intellectual gratitude to my colleagues in linguistics. In spite of this, I have always felt that I was an anthropologist whose specialty was linguistics rather than a linguist who just happened to be an anthropologist.

In what follows I shall try to trace as clearly and succinctly as I can the course of change and development which my approach to language underwent from the period when I first began to have a serious acquaintance with scientific linguistics, namely, my stay at Yale in late 1940 and early 1941.

Naturally enough, my first approach to language was strongly influenced by the dominant American Structural School of the period. Its basic premises and methodology can be found in convenient form in both the articles and the preliminary editorial comments in Joos' reader (14). Above all, it worked to abolish mentalist assumptions, a goal which it shared with the American behavioristic psychology of the period.

The entire intellectual climate in which both of these attitudes flourished was that of the logical positivist approach to the philosophy of science. In fact, Bloomfield himself recognized the kinship between structural linguistics and logical positivism by contributing a monograph on the linguistics aspects of science (3) to the series *International Encyclopedia of Unified Science,* an endeavor planned and carried out by the leading exponents of logical positivism of the period. Further, Bloomfield published a well-known article on the postulates of linguistics (2). The setting up of such systems of postulates was a central activity of adherents of the logical positivist approach. My earliest thinking in regard to language was naturally influenced by the current logical positivism and the great work that laid the foundation of much of this approach, namely the *Principia Mathematica* of Whitehead and Russell (20).

Sometime during my period at Yale in 1937–38, I remember that I was talking to Bloomfield in his office, when he walked over to his bookshelves and handed me a copy of Carnap's *Logische Syntax der Sprache* (6) (at that time it was only available in the German original). He suggested that I read it. I can still remember him saying, "A fellow might get a lot out of reading this but, on the other hand, he might spend a lot of time and effort without it being of any real value to him as a linguist." He evidently felt that at his age it would be inadvisable to attempt it. I did read it and, of course, found it hard going because of an insufficient background in formal logic. Still, what it said about language was to me at that time both intriguing and stimulating in ways that will be discussed below.

It was the dominant school at the time and very "fashionable," and after the manner of the young, I followed the current trends enthusiastically. In 1939–40 when I returned to Northwestern from my fieldwork, Carnap was a visiting professor at the University of Chicago. I heard that he was going to give a public lecture, so I went with a number of other students to the University of Chicago to hear him. For us it was like a pilgrimage.

By that time I had become aware of the importance of the *Principia Mathematica* as a foundation for the logical positivist approach. I therefore began to study it and to master its complex symbolism. I continued this study during intervals of leave from the Army and during my initial years in academic life.

The logical positivists and the logicians of Russell and Whitehead's school were saying that natural languages were at once too complex and too irregular to be studied by exact methods. By this, as logicians, they meant not so much language itself as a subject of scientific study but the use of "ordinary language" in deductive reasoning. Hence it was necessary to devise artificial languages like that of the *Principia,* and moreover such "languages" must be completely formalized. By this they meant that it would be a calculus in which deduction proceeded by well-defined and purely mechanical rules which made no appeal to our treacherous intuitions about meaning.

Only in this way could one avoid the fallacies of reasoning inevitably connected with the use of natural language. Hence the artificial languages devised by the logicians of this period took the form of axiomatic systems. They started with a set of primitive terms and relations and a primitive set of propositions (these latter might fittingly be called postulates). To begin with, these primitive concepts and propositions were purely formal symbols devoid of any semantic interpretation though later they might be provided with a semantic interpretation when they were applied to some empirical subject matter.

The resemblance to American structural linguistics in this regard is particularly striking. While linguists of this school did deal with meaningful elements, the minimal one being called the morpheme, the only claim made regarding it from the semantic point of view was that it *had* a meaning. The meanings themselves however were felt to be too complex and inexact to be dealt with scientifically.

As noted by Lounsbury in his article reviewing linguistics in the first issue of the *Biennial Review* (16, p. 91), "The study of meaning does not have a place within descriptive linguistics, at least in its American variety . . ."

In the axiomatic systems of the logical positivists there was, however, a place for meaning. It would be possible by so-called coordinating definitions to provide the calculus with an interpretation. In fact, there was no reason why a single calculus could not have two differing semantic interpretations or, for that matter, none at all. When a calculus was provided with an interpretation, then one could be assured that if the primitive propositions were true, it was guaranteed that anything deduced from them would be true because of the formal and purely mechanical nature of deduction in the calculus. Still, the introduction of coordinating definitions with their semantic reference to things in the world outside of the purely formal calculus did seem an inherently inexact procedure.

I may add that right from the beginning I was not convinced by the so-called empiricist criterion of meaning which stated in its simplest form (later subject to much revision) that the meaning of a statement was its method of verification. If one could not state how it could be verified, then the statement itself was meaningless.

At any rate, I felt that these were philosophical questions and that my own primary interest was in language. However, I did find that there were two valuable byproducts from the logical positivist approach and particularly from the study of the *Principia Mathematica* that had preceded it historically. In an axiomatic system, each step in reasoning was required to be justified by exact methods of deduction, either from the primitive propositions of the system or from statements which already had been deduced from these by the same exact methods of deduction. Of course, the axiomatic method of geometry based on Euclid had a basic similarity to that of positivistic axiomatics, but my early exposure to Euclid did not have the same effect on me. This was probably because I had encountered it at too early an age for it to affect me strongly, but even more, I think, because the *Principia* and subsequent systems of the logicist school gave explicit rules for deduction, and by not giving the primitive propositions any meaning seemed to avoid the apparent vagueness and arbitrariness of the Euclidean postulates.

In particular, the study of the *Principia* provided me with an intellectual discipline which has ever since stood me in good stead. Further, it forced one in any field to inquire concerning the logical relations of basic concepts to each other. Which ones could be defined in terms of others and which were truly primitive? Even the empiricist criterion of meaning, although it had to be rejected, still had a kernel of useful application. I believe that it is always relevant to ask regarding any statement what are the conceivable facts about the world which would decide its truth or falsity.

Outside of my disagreement with the empiricist criterion of meaning, which even in its various revised forms seemed unsatisfactory in that it did not, to use a favorite term of the positivists, really "explicate" what we mean by meaning, there remained other differences which derived from my anthropological and linguistic background. Natural language did not seem to me to be so irregular and complicated that it could not be described or even used for scientific purposes. Even semantics, which seemed the most intractible of all, not only to positivist logicians but to American linguists of the period, seemed to me to have a sufficient degree of organization, at least in some favorable areas, that it could even be analyzed by means of an interpreted axiomatic system. One such topic was kinship terminology, and one of my earliest papers concerned the axiomatic analysis of kinship terms (8). It was "universalistic" in one respect in that I examined a fairly large number of terminologies so that I could take into account all the categories that were known to occur in actual systems. In regard to this, I was greatly assisted by the pioneer paper of Kroeber (15), in which he discussed all the categories that he knew to occur in the kinship systems of the languages of the world, including such exotic ones as "state of connecting relative," e.g. the difference in some Amerindian systems between daughter of my living brother and daughter of my dead brother. As with axiomatic systems in general, the notion here was to discover the basic and minimal number of terms, relations, and propositions by means of which all of the existing terms in any language could be defined once these primitive notions were, for any language, given interpretations. I believe that this paper, along with several others by different investigators at about this time, all had the basic notion that certain areas of linguistic terminology were systematically organized and were among the precursors of the later ethnosemantics.

I sent a copy of my paper on the axiomatics of kinship to Carnap. He replied and said that he was very pleased that the methods he had used could in fact be applied to portions of natural language and that the logical positivist approach could make some contribution to the social sciences.

I believe that even from the earliest period of my acquaintance with American structural linguistics, I was at least dimly aware of certain aspects of the then dominant theory which I felt were unsatisfactory. I think there were three main sources of this rather vague feeling of dissatisfaction. One was that I was by no means convinced of the efficacy of the procedures employed to provide definitions of the two basic units, the phoneme and the morpheme. This was related to my second major source of disagreement which had to do with the role of meaning and its study, the field of semantics, within linguistics. Meaning seemed to me a central factor in language, given that its very raison d'etre was communication, so that the study of both lexical meaning and the meanings of grammatical categories and relations must be an integral part of linguistics.

However, in the structuralism of the 1940s and 1950s in the United States, phonemes had a semantic function in that they were the minimal units which *distinguished* meanings, so that, for example, the minimal contrast between *bad* and *sad* showed that *b* and *s* were separate phonemes. Similarly, the morphemes were the minimal units which *had* a meaning as in the English word *play-er-s* in which each of the three parts had a meaning and could not be divided further into parts which themselves possessed a meaning.

Although meaning entered into the definition of the two basic units of linguistics, they only entered in a very marginal way. The meanings themselves were, on the usual view, not really part of linguistics. Once the basic units were defined for any language, the grammar would consist of stating the grammatically possible sequences and arrangements of each. In this way the grammar did indeed become something like the purely formal uninterpreted systems of the logicians.

It should be added, however, that theory and practice often differed considerably during this period. After all, in the practical activity of writing grammars of foreign languages, one could hardly present sentences without giving their translation, even though it was felt that on scientific grounds this was not a very exact procedure.

A third important source of uneasiness with the then current theory pertained to the position of historical and comparative linguistics. After all, a large part of the formal training I had in linguistics was in these fields. Clearly every language was a product of historical evolution. Hence one kind of reasonable explanation of many linguistic phenomena was the historical one. Indeed, for the nineteenth century and the early part of the twentieth, to most linguists this seemed to be the only kind of explanation. To me it still made sense to explain linguistic phenomena historically. Yet the American structuralism of the period emphasized the complete separateness of synchronic and diachronic linguistics. Historical considerations must never be allowed to influence synchronic analysis. Moreover, although it seemed on the surface that synchrony and diachrony were coequal branches of linguistics, it was clear that synchrony was at the center of the stage. When linguists talked of linguistic theory in this period, they simply meant the theory of producing synchronic description. There really seemed to be no justification for carrying on historical and comparative studies. They seemed to continue by sheer force of habit and because certain people found them to be fascinating topics.

When I came to Columbia in 1948, I found a very different sort of linguistics in a dominant position. A large proportion of the linguists in the New York area, most notably Andre Martinet and Roman Jakobson at Columbia, were Europeans who followed a very different form of structuralism, that of the Prague school. They formed the Linguistic Circle of New York and published the journal *Word* with which I soon became involved editorially. The main reason they wanted me, as I was well aware, was that I was almost the only available linguist in the area whose native language was English.

I thus came to be acquainted with Prague school linguistics of which I had been only dimly aware up to that time. As I realize now, it influenced my thinking to a greater extent than I knew at the time. My first reaction though was that its methodology was very loose compared to the rigorous procedures to which I had become accustomed in American structuralism.

Among the writings of Prague linguists that I got to know during my initial period at Columbia was the fundamental monograph of Trubetskoy on phonology (19). Here, for the first time I found a linguist looking at a large number of languages and comparing their structures. Although on the surface it did not lead to explicit generalizations, it did show how to compare the phonological systems of different languages in spite of the numerous differences in phonetic detail. This was done by setting up a limited number of binary features, e.g. voiced versus unvoiced. Prague analysis thus went, as it were, beneath the phoneme level. In theory any phoneme in any language could be defined as a "bundle" of simultaneous features, e.g. voiced versus unvoiced, stop versus fricative, nasal versus non-nasal, etc. These feature oppositions were universal not in the sense that all phonological systems possessed them, but that all systems utilized some of a rather limited set of universal feature oppositions. There was therefore a general vocabulary by means of which the systems of different languages could be compared in spite of the differences of phonetic details. Moreover, there was one other aspect of Trubetskoy's analysis the significance of which I did not at that period discern. In regard to vowel systems, Trubetskoy had developed a typology. All systems fell into three classes named on the basis of the shape of the figure produced using the usual diagrams with a back-front dimension and a second dimension of vowel height. However, it was not clear what further conclusions could be derived from such a typological classification.

Trubetskoy's analysis also incorporated one further fundamental Prague notion, namely that the binary oppositions each involved a hierarchy. Of the members of each opposition, one of the members was, in a certain sense, preferred over the other. This preferred member was called the unmarked while the subordinate one was called the marked. The term marked was used because, characteristically, the marked member contained an extra element, the "mark" which made it more complex than the unmarked. For example, in the opposition nasal versus non-nasal, the former was marked by the possession of nasality whereas the unmarked member was characterized simply by its absence.

Moreover, across languages, marked features, when compared to unmarked, possessed other properties than the phonetic characteristics of greater complexity, which were further indications of their subordinate status. For example, it was generally true that they were smaller in lexical and textual frequency, and it was often possible to state that for any language, the possession of the marked member implied the presence of the unmarked member, but not vice versa.

The notion of marking had already been extended to grammatical categories by the 1930s; e.g. the singular was unmarked in relation to the plural. The parallel with phonology was particularly close in regard to the property of marking itself. For example, in English the marked category of the plural has an overt mark, usually *s,* while the singular is expressed merely by its absence. An additional and very basic property of the unmarked category was its ambiguity in that it might, under certain circumstances, stand for the category as a whole rather than as specific member of an opposition. This was called neutralization. A semantic example is the unmarked status of "long" as against short. The term "length" derived from "long" represents the entire category.

A further important step had been taken by Jakobson. He tried to show that there was hierarchy not merely within a single feature opposition, but the features themselves were hierarchically organized (13). Some were more basic than others. Most importantly, there were implicational relationships: the more basic implied the less basic but not vice versa. Jakobson had proceeded on a grand scale. The implicational hierarchy appeared not only in adult normal language, but also in child language acquisition and language loss in aphasia. In child language, learning the more basic was acquired before the less basic. In aphasia, in mirror-image fashion the less basic was lost before the more basic. Thus in all three situations the same implicational relationships held in that the less basic was never found unless the more basic was present. These are exciting ideas. Numerous exceptions and qualifications have been found, but they helped guide research by generating specific hypotheses and they still remain a fundamental insight.

In yet another respect, the Prague approach differed from that of American linguistics. The separation of synchrony and diachrony was not quite as strict. Although the center of interest was synchrony, certain members of the school talked about dynamic synchrony. At any given synchronic stage, there were some characteristics which were on the verge of extinction or were literary reminiscences while others were recent innovations, or even old features that had taken on new life and were in the process of spreading.

As can be seen, this view of language as undergoing, at any stage, dynamic change also suggests that it is not a homogeneous entity. There are differences of levels of style along with social and regional differences within the population which are reflected in linguistic differences. If it should turn out that this variation within the speech community is not random but can be the subject of systematic study, we have one of the foundations for the modern field of sociolinguistics. In fact, one of the prime movers in the development of this area of study was William Labov, a student of the Uriel Weinrich, himself a student of Martinet, a leading member of the Prague school who had taught at Columbia.

However, in spite of my doubts regarding American structuralism and my growing acquaintance with the Prague approach, I was still essentially loyal to the American school during the early 1950s. To me the Prague approach seemed vague and inexact in comparison to that of American structuralism.

It was in 1953, and once more through the intervention of the Social Science Research Council, that I underwent an experience that had far-reaching consequences for my subsequent work. The Council had in the previous year formed a Committee on Linguistics and Psychology of which I was not at that time a member. It held a two-week seminar at Cornell. The results seemed sufficiently encouraging for the Committee to plan a summer seminar in conjunction with the Summer Institute of Linguistics at the University of Indiana in 1953.

The seminar was based on the assumption that three areas—linguistics, psychology, and information theory—had developed to the extent that interaction among specialists in these fields would be fruitful and might perhaps help toward their ultimate integration. The seminar itself consisted of a small number of faculty members and a few graduate students from the three fields.

The first step was to acquire some elementary acquaintance with each other's fields. It fell to me to give the basic initial exposition of linguistics in several two-hour sessions. I described with some pride, though not without inner misgivings, the rigorous method by which a linguist confronted with a corpus, as it was then called, of utterances from a language could discover the basic units of phonology and grammar, the phoneme and the morpheme respectively, as well as the rules concerning permissible use of these elements on both levels.

When I had finished, Cornelius Osgood, one of the psychologist members of the seminar, asked me a question which was to haunt me thenceforth and helped determine the direction of much of my future work. I cannot now recall his exact words, but they were approximately the following. "You have described a very impressive procedure for analyzing any language into its basic units. However, if you could tell me something that was true about all languages, that would be of interest to psychologists."

It was this remark that brought home to me the realization that all of contemporary American linguistics consisted of elaborate but essentially descriptive procedures. One could go on making analyses of language after language. In each case the concrete meaningful elements and the grammatical categories would turn out to be different. The only thing that was universal was the procedure for arriving at these differing results. What one would do with these various grammars after writing them was not clear.

The topic of language universals which was broached in this way turned out to be one of the interests of the seminar. Ultimately, the Committee on Linguistics and Psychology, which had sponsored the summer seminar, was to organize the Dobbs Ferry Conference on Language Universals April 13–15, 1961, the results of which were published as the volume *Universals of Language* (11).

After the summer seminar at Bloomington, I continued my interest in the subject of language universals, but there was one basically disturbing question. Assuming that it was important to discover generalizations which were valid for all languages, would not such statements be few in number and on the whole quite banal? Examples would be that all languages had nouns and verbs (although some linguists denied even that) or that all languages had sound systems and distinguished between phonetic vowels and consonants.

It was at this point that another linguistic topic I had just begun to work on became relevant, that of typology. When linguists mentioned typology during this period, what they usually had in mind was the traditional nineteenth century classification of languages into isolating, agglutinative, and inflective which was in general disrepute. The classification was inexact in that the definitions of the types were never stated with sufficient clarity, and also ethnocentric. It was assumed that the three types formed a sort of evolutionary progression and that the highest stage, inflective, had only been reached by the Indo-European and Semitic groups. However, since the more recent Indo-European languages such as English and French were generally less inflective than the older languages such as Latin, Greek, and Sanskrit, a second basis of classification was introduced to account for this, namely that between synthetic and analytic. The modern Indo-European languages were, in general, analytic in that they "analyzed out" what in languages like Latin would be fused in a single inflection. For example, Latin *puerō,* the dative singular of *puer* "boy," had an inflection that expressed in a single vowel the categories of case (dative), number (singular), and gender (masculine) which were separately expressed in analytic languages.

In the early 1950s, Voegelin in particular sought to revive interest in typology. There were two reasons why I took the topic seriously. One was that in my earlier research on the historical classification of African languages, my chief methodological concern was to distinguish between genetic historical criteria involving simultaneous sound-meaning resemblances, whether in lexical items or those with a grammatical function, from typological criteria like gender and tone which had been widely employed by previous classifiers of African languages. When one talks of gender as a typological characteristic one does not take into account the concrete sound used to indicate gender, like the *t* of the feminine in Semitic, but merely whether sex gender exists in the language however it is expressed. Yet sex gender is found in Chinook, the Tucanoan languages of South America, and in still other areas outside of Africa. Again, to use the existence of tone as a historical criterion is to use sound abstracted from meaning, just as the use of gender involves meaning abstracted from sound. In regard to tone it was clear that tone had developed quite often outside of Africa, e.g. in Southeast Asia and in the many indigenous languages of Mexico, independent of its occurrence in Africa.

The research on African linguistic classification had been carried out mainly in the period 1949–1950. In regard to typology it had two main effects on me. The first was the realization that typology was a broader subject than the traditional nineteenth century typology. This notion was reinforced when I became acquainted with Trubetskoy's phonological typology. Moreover, although typological criteria were irrelevant to historical classification they seemed too important not to have some significance, perhaps of a different sort.

The second reason for my interest in typology was simply that Sapir, whom I admired greatly, had made it a central topic in his seminal book *Language* (17). In his book Sapir had sought to reanalyze the criteria employed in the traditional ninteenth century typology into a number of different and independent dimensions, as well as to rid it of its ethnocentrism. In 1954, the year after the summer seminar at Indiana, I tried to formulate a reanalysis of the traditional typology not unlike that of Sapir but with less reliance on intuition both for the definitions of typological dimensions and for the assignment of languages to particular typological classes (10). In fact, I quantified the typology so that instead of stating that a language was highly synthetic, I described a procedure which might lead to the calculation of a synthetic index of, say .94, whereas a language which was far less synthetic might have an index of synthesis with the value .17.

In the period between 1954 and my residence as a Fellow at the Behavioral Sciences Center in 1958–59, I gradually came to the realization that a systematic treatment of typology in its broader sense might, in fact, answer the question that had troubled me about the meagerness and relative triteness of statements that were simply true of all languages (what I came to call unrestricted universals). This seems something of a paradox. On the surface, typology, by assigning languages to different types, seems to emphasize their differences while the study of universals emphasizes their uniformity.

By now it was clear to me that typology, universals, implicational relations, and marking theory were all intimately related to each other. Let us consider a simple example, that of implosive stops. These are sounds which are produced by lowering the larynx with the glottis closed so that the air in the cavity formed by the glottal chords and some closure above the larynx (e.g. the closing of the lips in forming labial sounds) is rarified compared to the outside air. When the supralaryngeal closure is released, the air temporarily rushes in (hence the term implosive), but the inward direction of the air stream is quickly reversed as the lung air rushes out after the glottal closure is released.

If we examine the languages of the world that have implosives, we note a strong favoring of front implosives over back implosives. The phonetic reason for this still remains obscure. There are four typical positions of the supraglottal closure from back to front: 1. velar (back tongue), 2. palatal (mid tongue), 3. alveolar (tongue tip), and 4. labial. If we call 1 and 2 back implosives and 3 and 4 front implosives, we can construct a typology of two dimensions on each of which there are two values, presence or absence. There are four logically possible types: 1. languages with both back and front implosives, 2. languages with back but not front implosives, 3. languages with front but not back implosives, 4. languages without either front or back implosives.

Of these four types, type 3, with front but not back implosives, is the only one not known to occur. This nonrandom distribution of languages among types can be restated as an implicational universal. The presence of back implosives in any language implies the presence of front implosives, but not vice versa. Moreover, back implosives show the usual characteristics of marked categories.

An unrestricted universal such as that all languages have phonetic vowels can then be considered the logically limiting case involving the simplest possible typology, one with a single dimension and two values. In regard to vowels there are two logically possible types, languages with vowels and languages without vowels. The second type is not found in actual languages.

When I began to realize the significance of implication and marking relations, I experienced what Germans have called the *Aha-Erlebnis*. So this was what Jakobson and other members of the Prague school had been driving at all this time!

In 1958–59 I was a fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford, Calif. It had been planned that a number of members of the Indiana summer seminar would be present. Among these were myself and two of the psychologists, Cornelius Osgood and James J.

Jenkins. We prepared a memorandum on language universals that was submitted to the SSRC as the basis of a conference on language universals that was to be held at Dobbs Ferry in 1961. At the Center I presented a preliminary version of a paper on universals of word and morpheme order which applied the principles just discussed. This paper, along with other papers given at the conference, the original memorandum, and a noteworthy summation by Jakobson, were included in the volume that resulted from the meeting (11).

It is common, I believe, for scholars at some stage of their careers, often an early one, to reach a point after which they do not undergo any fundamental change in their point of view. For good or ill, this was, I think, the case with me from about 1960. The investigation of typological characteristics of language on a broad scale and of the regularities that can be derived from them is obviously beyond the capabilities of a single researcher. During the period 1968–76, a Stanford Project on Language Universals, of which I was codirector with Charles A. Ferguson, was funded by the National Science Foundation. Among its results was a 20-volume series of Working Papers (21) and a 4-volume work, *Universals of Human Language* (12), which consisted largely of papers written by members of the project. Work of this kind continues partly on an individual basis but also in a Center at Cologne which is on a virtually permanent basis.

The main respect in which my approach has continued to develop since the early 1960s is my continually expanding recognition of the importance of diachronic factors in relation to language universals. Although historical events have commonly been viewed as unique and therefore not subject to generalization, in the case of language at least I believe this is not the case.

Not all diachronic generalizations are derivable from typological studies, but it will be helpful, in light of the previous discussion, to use typology as an illustration. Let us consider any typological scheme in which, as is frequently the case, one or more of the logical possible types is not found to occur empirically. Consider then any two of the types which actually occur and, for the sake of the example, let us call them type A and type B. We can ask whether a language of type A ever changes to a language of type B or a language of type B to type A and so on for every pair of existing types.

The study of such typological change in general may be called the field of dynamic typology. In carrying out a dynamic typological study we, as it were, carry the conventional methods of comparative linguistics to a higher plane. We compare historically independent instances of the same change of type. We often find, along with the inevitable differences of detail, a certain uniformity in the stages of the changes which allows us to formulate processual generalizations.

Not all generalizations about change can be formulated by means of typologies. For example, all languages have terms for parts of the human body and all languages have additional meanings which derive from them that are either synchronically obvious as in English 'in back of' or a survival of a body part term in transferred meaning when the source meaning has become obsolete.

The first extension, if it occurs, always seems to be to spatial relations and only later, in some instances, to temporal and more abstract concepts. Once more it is possible to formulate such relations as implications. However, in diachronic universal implications there is an asymmetry not found in the synchronic case. The later implies the earlier and not vice versa. Thus extension to a temporal meaning implies previous extension to a local meaning.

In instances such as the preceding we are not dealing with typological change. All languages have body part terms and all languages have spatial, temporal, and abstract terms which derive historically from such body part terms. Hence no change in type is involved.

My other major concern besides typology and universals has been with historical comparative linguistics in the traditional sense and with the genetic classification of languages. Historical and genetic studies are related to the study of language universals in a number of ways. In the study of diachronic processual universals as described above, every individual case of development that enters into the comparison requires the application of the comparative method in the traditional sense.

A second important link between typology/universals and historical linguistics concerns the problem of language sampling in synchronic studies. When we assert, on the basis of a particular sample of languages, that a certain linguistic property is related to another, it becomes relevant to investigate the extent to which the properties in specific languages are independent, and by this we mean historically independent.

This question is not reducible merely to whether the languages are related. If all the languages of the world had a single origin so that they were all ultimately related, the problem would still remain. The question in each case has to do with the historical independence of particular linguistic traits. Related languages often share typological characteristics which are not part of their common inheritance but have developed independently since their separation (convergence).

For example, the basic word order verb-subject-object is found generally in early Semitic languages and widely in other branches of Afroasiatic, the larger stock to which Semitic is affiliated. Doubtless the occurrences of the opposite, verb final order, subject-object-verb in both ancient Akkadian and modern languages of the southern branch of Ethiopian Semitic, such as Amharic, are historically independent developments in spite of the Semitic affiliations of both.

The first step in the historical comparative method itself is genetic classification because without it we will not know which languages to compare. Here again we are not talking merely about whether all the languages are related. We wish to distinguish valid genetic units at whatever level. Thus the Germanic languages are a valid genetic unit in the sense that they are all more closely related to each other than any of them is to any non-Germanic language. A group consisting of Swedish, Polish, and Albanian is not a valid genetic unit and therefore not the basis for comparative historical study even though all these languages are related, since they are all Indo-European.

The genetic classification of all the world's languages has, of course, not been completed. In this respect linguistics compares unfavorably with biology because in the latter field the task was already completed by Linnaeus in the eighteenth century. Of course major modifications have since been made but in many fundamental respects it has held up. It is likely that the task is more difficult in regard to language, but I believe we are now at the stage at which, if it is feasible, it should be possible to carry it out.

If this major task, which has been relegated to the periphery in contemporary linguistics, could be completed, we would have a tool of immense importance for human cultural and biological history. Even the partial results obtained up to now are obviously valuable. For example, the linguistic comparative work and the reconstruction to a considerable extent of the vocabulary of the Proto-Indo-European speech community connects in important ways with archeology and human genetics.

In describing how I became a linguistic anthropologist, I noted that although it resulted from a series of accidental events, given my interest in language, it seemed a natural decision to enter anthropology. In American anthropology, as contrasted with British social anthropology and the French tradition, a synoptic view of man embracing both physical and cultural aspects prevailed. In the so-called four fields approach, to which most of American anthropology adheres at least in principle, anthropology consists of four subfields: cultural anthropology, linguistics, archeology, and physical anthropology.

It is obvious that this division has no consistent logical basis. Language is a part, indeed a central part of culture; it is simply that the complexity of its study made it a separate field and this separateness has continued and increased over the course of time. Archeology, once more, is essentially a part of cultural anthropology but involves a set of techniques which also have become greatly specialized over the course of time.

If we consider the four conventional subfields of anthropology, it also clear that from the beginning the branches were not of equal importance. Cultural anthropology was and is central, and although there are a few academic departments with strong concentrations in fields other than cultural anthropology, many smaller departments have little or only token representation in the other fields.

There have been two major developments since the period when I entered anthropology which have tended toward the diminution of the role of linguistics within anthropology and of the position of linguistic anthropology in relation to linguistics as a whole.

One has been the continual development of new specialties within cultural anthropology (a glance at the *Annual Review of Anthropology* through the years is enough to illustrate this). Arensberg (1), in his overview in the first issue of the *Annual Review* series, called attention to this, noting that ". . . the progress of anthropology displays a proliferation and sprawl that threatens its advance." Some of the pioneers like Boas and Kroeber worked in several branches of anthropology, which frequently included linguistics. When I was at Columbia, Kroeber taught there in the early 1950s after his retirement from Berkeley. He was sufficiently interested in linguistics to go with me regularly to the meetings of the Linguistic Circle of New York. Linguistics was still not so esoteric that an interested outsider could not understand it. Among the founding members of the Linguistic Society of America we find a number of anthropologists who were not linguistic specialists. During the initial period of my career, I wished to work both in linguistics and cultural anthropology but soon found that this was no longer possible, at least in regard to research.

The development of new specialties has clearly diminished the proportional role of linguistics within anthropology. This has been accompanied in the last two decades by an explosive growth of linguistics as an independent discipline. Most universities, whatever their size, now have separate linguistics departments, often of considerable size.

The result of these developments has been to question with increasing insistency what, if any, distinctive role remains for linguistics within anthropology. On the one hand, it is clear that as long as anthropology claims human culture as its central domain, there are both practical and theoretical reasons for it not to abandon linguistics. On the other hand, one may well ask why a graduate student with a basic interest in language would choose to enter an anthropological rather than a linguistic program.

I would maintain that there is still a role for linguistic anthropology. In attempting to show this, I will outline two basic approaches to language. To avoid misunderstanding it should be made clear that the division is not an absolute one. Many individuals will be involved to some extent in both. Moreover, on the theoretical plane each can and must fructify the other. Nor do I claim the superior importance of one over the other. Again, although a rough correlation with disciplinary lines will be evident, it is far from absolute.

With all these qualifications in mind, we can still characterize two ways of regarding language. One is language as a sociocultural institution and the other as a system of signs that can be the subject of formal analysis. The term formal is not here intended to exclude semantics, for which there is a place in present-day formal approaches. I believe this will be clear in the sequel.

Since the notion of language as a human cultural institution will be immediately clear to anthropologists, nothing further will be said about it for the present. The other way of looking at language, which is attractive in some degree to all linguists, can be illustrated by a simple example.

Let us consider a restricted part of elementary arithmetic and consider in what ways it resembles ordinary spoken language and in what ways it differs. This "language" will contain only statements involving cardinal numbers, both positive and negative, and the operations of addition and subtraction. All the expressions will be equalities. For example, $3 + 4 = 7$ will be a possible sequence in this language. Such a sequence will be similar to a positive declarative sentence in ordinary language. Among other properties it has a "complete sense" and is subject to tests of truth and falsity.

This language will have a finite vocabulary, containing the symbols for 0 to 9 inclusive, $+$, $-$, $=$ and open and closed parentheses. Thus $4 + (2 - 3) = 3$ will be an expression in the language. Such a system, simple though it is, has important resemblances to natural languages beyond the paralellism of expressions of equality to positive declarative sentences already mentioned. For example, all expressions involving sequences of the symbols of the language will be true, false, or syntactically ill-formed, "ungrammatical." If close enough to well-formed expressions, they will be interpretable in the way that we can construe grammatically defective utterances that result from speech lapses. For example, $4 + 2 = 6$ is true, $4 + 2 = 7$ is false, and $3 = 4 + -$ is ungrammatical and uninterpretable while $7 = 5 + (4 - 2$ is ungrammatical but with good will on the part of the receiver of the message can be interpreted by supplying a closed parenthesis after 2.

It is obviously akin to language in having a finite vocabulary; it further resembles natural language in that some items such as the cardinal numbers are purely lexical while others such as the parentheses, which show that the enclosed items belong together and express also the order of operations, are much like inflectional markers or significant word order. They thus express syntactic relations and seem to belong more to the internal mechanism of the language.

Even some of the "concrete vocabulary" requires additional semantic rules in certain contexts. For example, 13 in $13 = 6 + 7$ is interpreted as 10 times 1 plus 3. Natural language also contains such complications. The closest parallel is the numerical system of certain languages like Chinese in which the sequence 3, 10, 2 is to be interpreted as 32. Further, some of the symbols have relational meaning, e.g. $+$ and $-$, in that they connect two other expressions and express a relation between them just as 'under' in 'the book is under the table' describes a relation between two entities. Our language even has ambiguities (homonyms). For example, in $6 - (-6 + 3) = 3$, $-$ is used to express a relational operation in its first occurrence while it is very much like a modifier in its second occurrence in that it qualifies the same basic meaning 'six' to make it negative. From this example we see that it even has marking conventions like natural languages, showing how deep seated such concepts are. By itself 6 (unmarked) is taken as positive. Moreover, as in linguistic marking theory, the unmarked is inherently ambiguous. It stands for either $+6$ or 6 abstracted from either positive or negative value.

Note also that any expression is divisible into subordinate parts which are much like phrases and clauses that are constituents of sentences in natural languages. In $3 + (5 + 7) = 12$, 5 is in closer syntactic relation to 7 than to 3 and $(5 + 7)$ can be considered a sort of constituent within the expression as a whole.

These and other parallels that might be pointed out suggest that "natural languages" are a species within a larger class. The by now somewhat archaic term "sign system" has been replaced in general use by "language," while those spoken by human beings like English and French belong to a subclass called "natural languages." One talks now about "computer languages" as a subset of languages alongside of natural languages. It is clear that this way of looking at natural languages as but one subclass belonging to a more inclusive category of language in a broader sense can be exceedingly fruitful in regard to certain kinds of problems.

There are, of course, important differences between the restricted portion of arithmetic we have been discussing and natural languages. Some of these recur when we compare natural languages to other language systems and some do not. However, there remains an important core of properties that are unique to natural language. Some pertain to its inner structure and some to its relation to its users and the speech setting in general.

For example, arithemetic lacks "indexical signs" such as pronouns and demonstratives whose reference shifts on each individual occasion but have a reasonably constant relation to the participants in the discourse, to the discourse which has preceded it, and the physical setting in which it takes place. Ordinary languages can of course also express general truths or falsehoods that are independent of the momentary setting.

The lack of indexical signs in the language of arithmetic is related to the kind of truth values of its statements which are eternally ("logically") true or false. The true statements of mathematics thus consist entirely of tautologies, but in individual more complex cases, the problem is precisely to show if they are logically true or logically false. A mathematician friend of mine once cited a definition of mathematics as the study of interesting tautologies.

Ordinary languages have further numerous characteristics unrelated to their structure which make them unique and fundamental. They have priority in the history of humanity and of the individual. Spoken language existed long before the first invention even of writing, with which it has peculiar and important relations and *a fortiori* to other sign systems. Every individual learns to speak before learning mathematics or computer languages, and the priority is not merely chronological. We learn the numbers and even some of the most elementary operations such as addition in ordinary language which thus constitutes a bridge and a medium of instruction for other systems.

These are but some of the properties of natural languages which indicate their priority and centrality in nonstructural aspects to other systems with which they share important commonalities of structure.

Anthropology, as the most comprehensive social discipline, will of necessity continue to include the study of natural language as an essential part of its task. This does not exclude either the role of linguistics or language departments in the study of natural languages. It is also compatible with the more formal approaches that are so prominent in present-day linguistics. Indeed, it is precisely the study of these similarities and differences that will put natural language in a broader perspective, while in turn the comprehensive typological and historical study of natural languages, the most complex member of the genus, will help to shed light on the nature of communication systems in general.

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