Biological Foundations of Language

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with appendices by

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Preface

The study of language is pertinent to many fields of inquiry. It is relevant to psychology, anthropology, philosophy, and medicine. It encroaches upon the humanities, as well as upon the social and natural sciences. We may pursue investigations that concentrate on what man has done with or to specific languages; or we may regard language as a natural phenomenon—an aspect of his biological nature, to be studied in the same manner as, for instance, his anatomy. Which of these approaches is to be chosen is entirely a matter of personal curiosity. This book is concerned with the biological aspects of language.

My intention was to write a theoretical treatise, not a textbook or a survey. I have made no attempt at exhaustive coverage of any of the many fields touched upon. For instance, in the field of physiology much outstanding work has been done on voice and speech mechanisms and on auditory perception. This material is clearly relevant to a general biology of language and certainly ought to be included in any course on this topic. I have omitted this and other similar material because it would not have added much to the *main line* of the argument, because it is readily available to the English-speaking reader, and because the technical detail is difficult to follow if one does not possess prior knowledge of the subject. On the other hand, I did add some detailed discussions of modern biological experiments and theory on the assumption that the student of language is today more likely to come with a background in the social sciences than in biology, and he would, therefore, be unwilling to accept some of the claims made in this book unless they were presented together with the substrate from which they originated.

During the last fifty years, many excellent synopses of language studies have appeared; but biology has been badly neglected in this literature, but for two exceptions. One is B. Naunyn's monograph, *Die organischen Wurzeln der Lautsprache des Menschen* (Bergman, Muenchen, 1925) and the other is C. L. Meader and J. H. Muyskens, *Handbook of Biolinguistics* (Weller, Toledo, 1950). The former is badly out of date, and the latter was written *ad usum scholarum*, and in this capacity it is still of interest; but it does not aim at a distinct theoretical

position. Nevertheless, Marx's historical survey in Appendix B makes it clear that there has been an implicit assumption since ancient times that language is somehow dependent upon man's nature. It is only in the last generation or two that behaviorists have openly challenged this position, although even here some ultimate relation to biologically given capacities has never been denied.

This book attempts to reinstate the concept of the biological basis of language capacities and to make the specific assumptions so explicit that they may be subjected to empirical tests. In many instances I have not been able to do more than to formulate questions and to show that they are not spurious. There is no research as yet that provides answers to them. But I hope that I have been able to show what type of investigations might lead to new insights and thus, perhaps, give new directions to old inquiries. A particularly promising approach seems to be the systematic evaluation of patients with various deficits, especially the deaf and the mentally retarded. Modern advances in technology and methodology in behavior research are likely to lead to new knowledge about language function, and thus the patients whose misfortune serves as source material for new studies may, hopefully, eventually profit from the new advances in our understanding of language.

This book must be understood as a discussion rather than a presentation of the biological foundations of language. The exact foundations are still largely unknown. On the other hand, I have considered this book to be the right place to evaluate critically some of the most common claims relating to the biological nature of language. In those instances where I found myself to be in disagreement with widely held opinions, the argument may have taken on a predominantly iconoclastic character, as, for instance, in Chapter six; in other cases the topic seemed to me important enough to warrant a detailed discussion although the data do not lead to new ideas on the nature or origin of language, as, for example, the discussion of peripheral anatomy in Chapter two. However, both the negative and the positive contributions uniformly led me to quite a specific point of view, which I have attempted to summarize in Chapter nine, and which may, some day in the future, become the foundation to a new theory on language.

Ideas do not grow in vacuo. Throughout my fifteen years of residence in the Cambridge area, I have greatly profited from courses taken and given, from conversations, and from general interaction with colleagues and students. I wish to mention particularly Georg v. Békésy, Roger Brown, Jerome Bruner, Noam Chomsky, George Gardner, George Miller, and Peter Wolff. All of them have discussed various aspects of this book with me, and most have read and commented upon several

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Contents

CHAPTER ONE The conceptual framework

I. Thesis: Biological Considerations are Necessary for an Understanding of Behavior 1

II. Form and Function in Ontogeny 4
(1) Mutual influence in the development of nervous and other tissue 4
(*a*) Trophic relationships 4
(*b*) Mechanical relationships 6
(2) The embryology of behavior 8

III. Behavioral Specificity and the Problem of Plasticity 10
(1) The problem 10
(2) Central regulatory mechanisms of motor coordination 12
(3) Developmental history of the central regulatory mechanism 15

IV. Genetic Foundations of Behavior 21

V. Relationship between Form and Behavior 23

VI. Conclusion 27

CHAPTER TWO Morphological correlates

I. Introduction 33

II. Periphery 34
(1) Face, lips and mouth 34
(2) Topographical anatomy of oral cavity, pharynx and hypopharynx 39
(3) Intrinsic anatomy of the larynx 44
(4) Relationship between peripheral anatomy and speech sounds 50

III. Central Nervous System 52
(1) Functional significance of form in the central nervous system 52
(2) The cortex 54
(*a*) Histological maps 54
(*b*) Behavioral maps 56
(*c*) Summary: language and cortex 61
(3) Subcortical structures 62
(4) Lateralization 66

(5) Relative size of the brain 67
IV. Conclusion 71

CHAPTER THREE Some physiological correlates 75

I. Aim of Physiological Discussions in this Monograph 75

II. Respiration 76
(1) Respiratory adaptations in general 76
(2) Respiratory adaptations to speech 77
(3) Other motor changes indirectly related to respiratory adaptations 85

III. Speech Production 89
(1) Discrete articulatory events 89
(2) Rate of articulatory events 90
(3) Ordering of articulatory events 93

IV. Problems Arising from Rate and Ordering 98

V. The Problem of the Organizing Principle: Rhythm 107
(1) The rhythmic nature of articulation 109
(*a*) Delayed feedback 109
(*b*) Signal switching between right and left ear 112
(*c*) Rate of interruptions 113
(*d*) Rate of syllable production 115
(*e*) Psychological correlates 116
(*f*) Neurological correlates: EEG 116
(*g*) Neurological correlates: pacing of speech during thalamic stimulation 117
(2) Final comments on speech rhythmicity (cultural, individual, and biological variations) 118

VI. Summary 119

CHAPTER FOUR Language in the context of growth and maturation 125

1. Characteristics of Maturation of Behavior 125

II. Emergence of Speech and Language 127
(1) Regularity of onset 127
(2) Relation of the environment to the age of onset 135
(3) The role of utility in the onset of speech 139
(4) The importance of practice for the onset of speech 140
(5) "Wolf Children" 141

III. Age Limitations to Language Acquisition 142
(1) Age and recovery from traumatic aphasia 142
(2) Age of lateralization of speech function in the brain 150

(3) Hemispherectomy (effect of removal of an entire hemisphere) 152

IIIa. Preliminary Summary 153
(4) Arrest of language development in the retarded 154
(5) The effect of sudden deafness on language at various ages 155

IV. Concomitants of Physical Maturation 158
(1) Structural changes in the brain 162
(2) Changes in chemical composition of the brain 164
(3) Electrophysiological changes 166
(4) Summary 168

V. Growth Characteristics of the Human Brain and their Possible Relationship to Language Acquisition 170

VI. Further Comments on the "Critical Period" for Language Acquisition 175

VII. Summary and Conclusion 178

CHAPTER FIVE Neurological aspects of speech and language 188

I. Clinical Symptoms of Speech and Language Disorders 188
(1) General characteristics of the patient with aphasia 188
(2) Receptive disorders 191
(3) Expressive disorders 191
(*a*) Subfluency 191
(*b*) Superfluency 192
(*c*) Semantic disturbances 192
(*d*) Difficulty in word finding 193
(*e*) Paraphasic disturbances 193
(*f*) Fixation on phrases 194
(4) Disorders of manner of production 194
(*a*) Errors of order 195
(*b*) Dysarthria 195
(*c*) Discoordinations 197
(5) Other language-related disorders 198

II. The Underlying Pathology 199
(1) Localized lesions 199
(*a*) Cerebro-vascular accidents 199
(*b*) Tumor 202
(*c*) Abscess 204
(*d*) Trauma 204
(*e*) Other focal lesions 204
(2) Diffuse lesions 205

III. Clinical Syndromes 205

IV. Theoretical Interpretations 206

(1) Interference vs. "loss" 206
(2) The problem of neurological correlates 208
(*a*) Connections and associations 209
(*b*) Perceptions 211
(*c*) Storage 213
(*d*) Processing 215
(3) Relevance to language 217
(4) Time, the most significant dimension in language physiology 218
(*a*) Aphasic symptoms as temporal disorders 218

V. Postscript on Innate Mechanisms for Perception and Production 220

VI. Conclusion 222

CHAPTER SIX Language in light of evolution and genetics 227

I. Limitations on Inferences from Animal Comparison 227
(1) Continuity theory *A:* Straight line evolution of language with only quantitative changes 228
(2) Continuity theory *B:* Straight line evolution of complexity by stepwise accretion (with missing links) 230
(3) Justification for a discontinuity theory of language evolution 234
(*a*) The search for true antecedents 234
(*b*) Phylogenetic change 235
(*c*) The sharing of traits 237

II. Are Biological Theories of Language Development Compatible with Concepts of Genetics? 239
(1) Genes and ontogenetic development 239
(2) Relative growth 241
(3) Transformations of form and function 246

III. Evidence for Inheritance of Language Potential 248
(1) Family histories 249
(2) Twin studies 252
(3) Miscellaneous evidence 253
(4) The Darlington-Brosnahan hypothesis 254

IV. Limitations on Reconstruction and "dating" of the History of Language 255
(1) Arguments based on the history of the brain and skull 255
(*a*) Comparison with brains of contemporary animals 255
(*b*) Relevance of the history of the skull 257
(2) Arguments based on other skeletal features 261
(3) Racial diversification and the emergence of language 261
(4) Cultural status as evidence for language 263

V. Summary 264

CHAPTER SEVEN Primitive stages in language development 271

I. The Problem 271

II. Prelanguage Development 276

III. Evolvement of Language in the Healthy Child 279
(1) Phonology 279
(2) Primitive one-word utterances 280
(3) Theoretical considerations 284
(*a*) Understanding-speaking 284
(*b*) How mature speakers understand sentences 286
(4) Structural characteristics of children's primitive sentences 292
(5) General comments on the genesis of phrase-structure, nested dependencies, and recursiveness 294
(6) General comments on the genesis of transformations 296
(7) The development of some specific syntactic mechanisms 302
(*a*) Questions and negations 302
(*b*) Inflectional endings 303

IV. Further Elucidation of Language Acquisition through the Study of Defective Children 304
(1) Language acquisition in the absence of speech production 305
(2) Language development in mongoloid children 309
(3) Language acquisition in the congenitally deaf 320

V. Summary and Conclusion 324

CHAPTER EIGHT Language and cognition 329

I. The Problem 329

II. Toward a Biological Conception of Semantics 331
(1) Words as labels for categorization process 332
(2) Differentiation of categories 334
(3) Interrelating of categories (transformations) 335
(4) Preliminary conclusions 336

III. The Empirical Study of Naming: The Language of Experience 337
(1) Description of referents 337
(2) Referent spaces 338
(3) Names mapped into referent spaces 339
(4) Contextual determinants in common naming 343

IV. Naming and Cognitive Processes 346
(1) General strategies 346
(2) Acuity of discrimination 348
(3) Memory and recognition 350
(4) Concept formation 355

(5) Cognition in deaf children 357
(6) Recent experiments in psycholinguistics 363

V. Postscript to So-Called Language Relativity 363

VI. Summary 365

CHAPTER NINE Toward a biological theory of language development (general summary) 371

I. Five General Premises 371

II. A Concise Statement of the Theory 374

III. Explanatory Power of the Theory 379

IV. Biological Foundations of History and Distribution of Natural Languages 380
(1) Theoretical Foundations 380
(*a*) Source, inhibitors, and determinants of change 381
(*b*) Variance in capacity for latent and realized structure 383
(*c*) Tolerance for variance: the mechanism of all changes 383
(2) Direction and Rate of Historical Changes 387
(3) Distribution 389
(4) A Note on Adaptive Value 390

V. Innate Mechanisms 393

APPENDIX A Noam Chomsky: The formal nature of language 397

APPENDIX B Otto Marx: The history of the biological basis of language 443

CREDITS 470

INDEX 471

CHAPTER One

The conceptual framework

I. THESIS: BIOLOGICAL CONSIDERATIONS ARE NECESSARY FOR AN UNDERSTANDING OF BEHAVIOR

Ever since man first mused about his own nature, it has been the gift of language that has surprised him most. If we search through the most primitive and the most ancient evidence of intellectual activity, through myths, magic, or religions, we will find one question that is repeated over and over: from what source comes the power of speech? Answers offered are either of a mystical or rational nature. The first type does not concern us here; the second, which is still prevalent today, is based on the principles of "discovery and rational utilization of inarticulate sounds." Explanations of this type propose that someone discovered certain advantages arising from accidental or instinctive vocalizations, and that one small discovery after another was incorporated into a communication system adopted by an ever-increasing range of individuals. The verbal behavior that came into existence in this manner proved to be so advantageous to those who adopted it in the struggle for survival, that it affected survival rates and natural selection, resulting in a strain that was endowed with "enlarged intellectual capacities," enabling even small children to learn the complicated natural languages as we know them today. A major objective of this monograph is to take issue with this type of formulation and to show that reason, discovery, and intelligence are concepts that are as irrelevant for an explanation of the existence of language as for the existence of bird songs or the dance of the bees.

The explanatory principles which I consider to be potentially fruitful are of a biological nature. This immediately raises a cardinal problem. What does biology have to add to the explanation of behavior such as

language, which appears to be acquired by trial and error? Is not learning a psychological rather than a biological phenomenon? Has it not been proved that learning can be explained by a few basic principles which operate in all vertebrates and many invertebrates? Biology, it would seem, deals with the difference between species, whereas psychology (at least the theory of learning) is said to deal with what is "common to all behavior, and all organisms." A biological investigation into language must seem the more paradoxical as it is so widely assumed that languages consist of arbitrary, cultural conventions. Wittgenstein and his followers speak of the *word game,* thus likening languages to the arbitrary set of rules encountered in parlor games and sports. It is acceptable usage to speak of the psychology of bridge or poker, but a treatise on the biological foundation of contract bridge would not seem to be an interesting topic.

The rules of natural languages do bear some superficial resemblance to the rules of a game, but I hope to make it obvious in the following chapters that there are major and fundamental differences between rules of languages and rules of games. The former are biologically determined; the latter are arbitrary. Even if this were not so, we can still reasonably speak in terms of a biology of game-playing. For instance, a zoologist might ask, "What are the homologues of man's *need for play* in animal behavior? What are the specific capacities necessary to enable an animal to play a game of chance, to learn probabilities, to engage in activities that seem to lead to no other reward than being busy or whiling away time? Would a classification of gamelike behavior in animals lead to an interesting taxonomy?" The latter is clearly an ethological problem and thus belongs squarely within the realm of biology.

A biological inquiry into language asks, "Why can only man learn to speak a natural language?" This question is fundamentally different from asking, "In what respect is learning to speak similar to conditioning or operant learning as studied by animal psychologists?" The former question requires an investigation into the specific nature of the species *Homo sapiens;* the latter requires a programmatic disregard of species differences. The former will turn to anatomy, physiology, and developmental studies for an answer (all of which are biological disciplines), whereas the latter will endeavor to discover analogies between stimuli, responses, rewards, and the temporal and spatial relationships between them.

Psychologists and other behavioral scientists have often set up equivalences between natural languages and experimental paradigms. Learning the meaning of a word is said to be like learning to press a bar

which will sound the buzzer which represents "food is soon to come"; learning grammar is similar to learning that event *A* is followed by event *B,* which in turn is followed by event *C*. Since these are all accomplishments which many animals can be trained to acquire, some psychologists have asked whether these animals have not in fact learned the essential principles underlying human language. This is a question that must not be answered on the grounds of intuition or common sense, because our understanding of the mechanisms of language is still poor. We do not know intuitively what language is like objectively nor how we manage to communicate with one another. The only way we may judge whether the experimental paradigms have any relevance to natural language at all is to conduct investigations into the nature, structure, and history of natural languages, and then to see whether the empirically determined principles underlying language are, indeed, represented in the experimental paradigms. From this consideration it follows that a biological investigation of language must not only study the organism that speaks but must also investigate the behavior itself—language—much the way the zoologist who studies the badger must study its physique together with its habits in order to give a complete picture of that animal. It is for this reason that some of the material presented in this monograph concerns biological aspects of man and some the biological aspects of language.

The book's fundamental thesis is that behavior, in general, is an integral part of an animal's constitution. Behavior is seen to be an integral part of the organic whole; it is related to structure and function, one being the expression of the other. To put the same thought into its negative form: I do not believe that an animal is like a tool that can be put to just any arbitrary use by a manipulator; I do not believe that anatomy and physiology are comparable to the physical nature of the tool, whereas behavior is like the use to which the tool is put. (The "manipulator" here is either the experimentalist or the vicissitudes of an environment.) Instead, I believe there is evidence that behavior has the same history and the same origin as form and physiological processes; in fact, the division between physiological function and behavioral function is an artifact of our mode of looking at animals, and these functions shade into each other and are, thus, objectively indistinguishable.

This thesis is an anathema in certain circles of behaviorism because it would lead to the conclusion that behavior must always be investigated in terms of specific species, and this proposition runs counter to the belief of many psychologists. On the other hand, if such a thesis *can* be defended, it would at once strengthen the aim of this book: to

discover biological principles that explain why a single species displays behavior that is unique in the animal kingdom. To substantiate the thesis, we must inquire to what extent the central nervous system, the peripheral and skeletal structures, and the animal's behavior are interdependent phenomena.

II. FORM AND FUNCTION IN ONTOGENY

There is evidence (1) that the tissues of the brain and the rest of the body constitute an organic, interdependent unit; and (2) that organisms are not programmed for their behavior by an ex-machina force, but instead they develop a program ontogenetically together with nervous and nonnervous tissues.

(1) *Mutual Influence in the Development of Nervous and Other Tissue*

Let us first consider the developmental relationship between nervous and other tissue. Our discussion can be divided into (a) metabolic or trophic relationships and (b) nonmetabolic, particularly mechanical relationships.

(*a*) *Trophic Relationships.* Nervous tissue stands in an intimate relationship to other tissue anatomically contiguous to it. This is shown most clearly by the essential role played by nerves in the process of regeneration. There are a number of studies available which indicate that regeneration of an entire limb in lower vertebrates (fish, lizards, urodele, salamander, larval anuran, and postmetamorphic frog) and probably also in invertebrates is dependent on the presence of nerves in the amputated stump (Singer, 1959, Gutmann, 1964).

In a series of experiments by Schotté and Butler (1944), Singer (1947), and their students, and Nicholas (1949), it has been shown that an amputated limb will not regenerate unless an intact nerve is either present in the remaining stump from the beginning or is transplanted into the cut surface by autograft (however, see Thornton and Steen, 1962). Morphogenesis, that is, an orderly sequence of tissue differentiation and development of the lost appendage, will not ordinarily take place in the absence of living nervous tissue during the very first stage of regeneration. If all nerves are removed from this stump during the earliest period, mitotic activity is dramatically slowed down; eventually some small amount of connective tissue, cartilage, and muscle may form in a disorderly nonfunctional fashion, giving a shriveled and

shapeless appearance to the stump. If viable nervous tissue is not present from the start of the amputation but brought into the so-called blastema shortly afterward, regeneration takes place, but the regenerate limb is poorly developed. The nerve need not be present throughout the entire period of regeneration; once the limb has begun to grow and tissues are sufficiently differentiated, the nerve may be removed without impairment of the morphogenetic potency acquired by these tissues during their earliest stage of formation. Singer (1947) has shown that it does not matter for regeneration what type of nerve, whether motor, sensory, or autonomic, is present in the blastema. It is merely the amount of nervous tissue present that controls the regenerative possibilities. Apparently a product of nerve-cell metabolism induces morphogenesis in the blastema.

These studies leave many questions about the biochemistry of embryology and growth unanswered; yet they do give us a glimpse of the complete interdependence and the natural integration of different tissues in the animal body. This impression is further strengthened if we consider some of the other trophic relationships that nerves have to peripheral tissue (for instance, the well-known fact of denervation atrophy). If the axon of a motor neuron is cut, the portion distal of the cut will die promptly, presumably because of its separation from its source of supply of vital substances (Gerard, 1950). But this is not the extent of the degenerative changes following the section of a motor nerve. The muscle innervated by the nerve will also undergo dystrophic changes with an extremely characteristic histological appearance. The loss of muscle substance is not due to a "functional" disturbance, such as the inhibition of nerve impulse transmission (Hamburger & Levi-Montalcini, 1950) nor due to disuse of the muscle; the muscle cannot be saved from atrophy by passive exercise. Indeed, the metabolic interdependence of nonnervous, peripheral tissue and nervous tissue is proven by the fact that the nerves themselves must have anatomic continuity with muscles for proper metabolic function. Severance of nerve from muscle will induce retrograde changes in the body of the neuron (the soma), known as chromatolysis, which is a sign of dysfunction.

Perhaps the most striking evidence for the subtle but definite interdependence of peripheral structures and the central nervous system is provided by the stunted growth resulting from large cerebro-hemispheric and specifically parietal lobe lesions in the neonate human. This phenomenon was described by Macdonald Critchley (1955) and has been generally known to occur in connection with a condition called infantile hemiplegia. The stunting of the body side contralateral to the brain lesion occurs both in congenital and in acquired infantile hemi-