STEPS TO AN ECOLOGY OF MIND

COLLECTED ESSAYS IN ANTHROPOLOGY, PSYCHIATRY, EVOLUTION, AND EPISTEMOLOGY

Gregory Bateson

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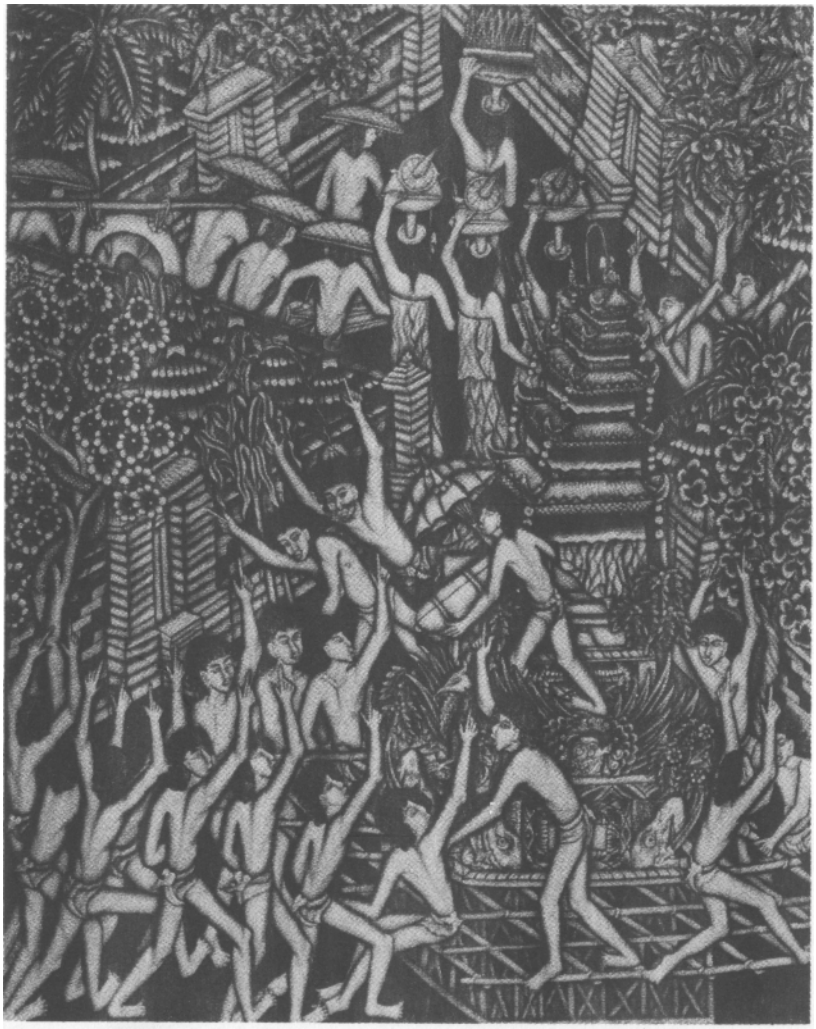

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1.1 1971 PREFACE

I have been one of Gregory Bateson's students for three years and I was able to help him select the essays which are here brought together for the first time in one volume. I believe that this is a very important book, not only for those who are professionally concerned with the behavioral sciences, biology, and philosophy, but also and especially for those of my generation — the generation born since Hiroshima—who are searching for a better understanding of themselves and their world.

The central idea in this book is that we create the world that we perceive, not because there is no reality outside our heads (the Indochinese war is wrong, we *are* destroying our ecosystem and therefore ourselves, whether we believe it or not), but because we select and edit the reality we see to conform to our beliefs about what sort of world we live in. The man who believes that the re-sources of the world are infinite, for example, or that if something is good for you then the more of it the better, will not be able to see his errors, because he will not look for evidence of them.

For a man to change his basic, perception-determining beliefs — what Bateson calls his epistemological premises—he must first be-come aware that reality is not necessarily as he believes it to be. This is not an easy or comfortable thing to learn, and most men in history have probably been able to avoid thinking about it. And I am not convinced that the unexamined life is never worth leading. But sometimes the dissonance between reality and false beliefs reaches a point when it becomes impossible to avoid the awareness that the world no longer makes sense. Only then is it possible for the mind to consider radically different ideas and perceptions.

Specifically, it is clear that our cultural mind has come to such a point. But there is danger as well as possibility in our situation. There is no guarantee that the new ideas will be an improvement over the old. Nor can we hope that the change will be smooth.

Already there are psychic casualties of the culture change. The psychedelics are a powerful educational tool. They are the surest way to learn the arbitrariness of our ordinary perception. Many of us have had to use them to find out how little we knew. Too many of us have become lost in the labyrinth, have decided that if reality doesn't mean what we thought it did then there is no meaning in it at all. I know that place. I have been lost there myself. As far as I know, there are only two ways out.

One is religious conversion. (I tried Taoism. Others are choosing various versions of Hinduism, Buddhism, and even Christianity. And such times always produce a host of self-proclaimed messiahs. Also, a few of those who study radical ideologies do so for religious rather than political reasons.) This solution may satisfy some, although there is always the danger of satanism. But I think that those who choose ready-made systems of belief lose the chance to do some truly creative thinking, and perhaps nothing less will save us.

This second way out—thinking things through and taking as little as possible on faith— is the more difficult. Intellectual activity — from science to poetry—has a bad reputation in my generation. The blame falls on our so-called educational system, which seems designed to prevent its victims from learning to think, while telling

them that thinking is what you do when you study a textbook. Also, to learn to think, you must have a teacher who can think. The low level of what passes for thinking among most of the American academic community can perhaps only be appreciated by contrast with a man like Gregory Bateson, but it's bad enough to cause many of our best minds to give up looking for better.

But the essence of all our problems is bad thinking, and the' only medicine for that is better thinking. This book is a sample of the best thinking I've found. I commend it to you, my brothers and sisters of the new culture, in the hope that it will help us on our journey.

—Mark Engel Honolulu, Hawaii April 16, 1971

1.2 1987 PREFACE

Gregory Bateson was fond of quoting Heracleitus: "Into the same river no man can step twice," particularly in his later work, in which he was trying to define the nature of the interface between the realm of mind and physical reality, and to discuss the way in which mental process establishes landmarks or thresholds, meanings and definitions in the world of flux. But a book is like a river, not in the simple sense of water flowing by, but because the intellectual context, like the reader, changes steadily. Whether one is reading it for the first time or returning after a lapse of years, Steps to an Ecology of Mind is today not the same book as it was when first published some fifteen years ago, and for most readers its impact should be greater. We have changed and the broad intellectual climate has changed. It would not be fair to say that this is the more important publication, but it is certainly more accessible. The increased accessibility of Gregory's thought today has come about largely because of the steady influence of these essays and other writers drawing on them in the interval, and because, after recognizing the unity of this collection, Gregory himself was able to write at a more general level.

The work of Gregory Bateson has been widely read during this intervening period. Ever year now I hear of two or three conferences focused on some aspect of his thought, sometimes within a single discipline, sometimes across a wider range, and his name crops up more and more often. Even more significantly, many of the ideas that were most important to him have become familiar notions that we feel at home with. He was one of a group of thinkers working toward an understanding of communications, of the importance of self-regulating systems, and the causal role of ideas, messages, differences. This has made him a central figure in the growing appreciation of the importance of looking at events and messages in context and looking at systems holistically, whether we are concerned with the health of the human body/mind or the biosphere. The importance of epistemology is more and more widely understood. At the same time, much of this familiarity is illusory. Strange or unsettling ideas are dealt with as the oyster deals with the bit of grit, packaged in soothing ways, smoothed over. The risk for a reader of Gregory Bateson in 1972 was that he or she would too readily say, "This doesn't make sense. It's too obscure for me." The risk today is the premature claim of understanding, the premature application.

I have had two surprising experiences going back over these articles: The first was the discovery of how many of the ideas that seemed important in his later work were already here, although few will have grasped them completely on first encounter. The second is how much more still awaits discovery in these articles for one who has become accustomed to Gregory's thought. Working with Gregory and writing about him, wrestling together with new ideas. as they came along, I am probably as much at home here as any of his students and colleagues, and yet the rereading remains a discovery. Most of the pieces in this volume are tight, intense, abstract arguments, that Gregory and others labored to "unpack" over the intervening years; and still there are surprises hidden within them that become visible as the reader comes to move freely in the text.

Frequently , during his career, as his Introduction indicates, Gregory felt as if he were speaking and writing in a foreign language. People did not simply agree or disagree with him; they were bewildered or intoxicated. Mark Engels, in his 1971 Preface, recognized the analogy between the "mind expanding" experiences of drugs and religious conversion and the kinds of intellectual change that could be achieved by a pervasive reshaping of patterns of thought. In retrospect it strikes me that intoxication and conversion were common responses even to these abstract and difficult pieces—responses in which a fraction of the argument was carried on a tide of intuitive affirmation. Today, however, it is becoming increasingly possible to come to grips with Gregory's thinking, to select, affirm, contest, question. Throughout his life, he treasured the relationships in which he found opportunities for intellectual grappling that went beyond admiration adulation; critical reading is essential.

This new edition, then, invites readers into an encounter with the work of Gregory Bateson that was only available to a few when the collection first appeared. My advice to readers would be to hang on to the challenge as well as the affirmation. We have not as a civilization achieved those epistemological shifts that may some day enable nuclear disarmament, ecological responsibility, and new approaches to both education and healing that will value and enhance the complexity of persons in their familial and social setting. In these and in Gregory's later books (Mind and Nature: A Necessary Unity, Dutton 1979, and, jointly with me, Angels Fear: Toward an Epistemology of the Sacred, Macmillan, 1987) the intellectual tools are offered. Today they will come more readily to hand, be easier to balance and handle in a disciplined manner than they were in the early 1970s, be more accessible to practice and skill. But still there remains the challenge of using the tools in such a way that they be-come a part of the user. And still the tasks for which these tools have been shaped largely remain to be done, more urgent today than ever.

—Mary Catherine Bateson Cambridge, Mass. August 1987

1.3 FOREWORD

Some men seem able to go on working steadily with little success and no reassurance from outside. I am not one of these. I have needed to know that somebody else believed that my work had promise and direction, and I have often been surprised that others had faith in me when I had very little in myself. I have, at times, even tried to shrug off the responsibility which their continued faith imposed on me by thinking, "But they don't really know what I am doing. How can they know when I myself do not?"

My first anthropological field work among the Baining of New Britain was a failure, and I had a period of partial failu60re in research with dolphins. Neither of these failures has ever been held against me.

I therefore have to thank many people and institutions for backing me, at times when I did not consider myself a good bet.

First, I have to thank the Council of Fellows of St. John's College, Cambridge, who elected me to a Fellowship immediately after my failure among the Baining.

Next, in chronological order, I owe a deep debt to Margaret Mead, who was my wife and very close co-worker in Bali and New Guinea, and who since then has continued as a friend and professional colleague.

In 1942, at a Macy Foundation conference, I met Warren McCulloch and Julian Bigelow, who were then talking excitedly about "feedback." The writing of Naven had brought me to the very edge of what later became cybernetics, but I lacked the concept of negative feedback. When I returned from overseas after the war, I went to Frank Fremont-Smith of the Macy Foundation to ask for a conference on this then-mysterious matter. Frank said that he had just arranged such a conference with McCulloch as chair-man. It thus happened that I was privileged to be a member of the famous Macy Conferences on Cybernetics. My debt to Warren McCulloch, Norbert Wiener, John von Neumann, Evelyn Hutchinson, and other members of these conferences is evident in everything that I have written since World War II.

In my first attempts to synthesize cybernetic ideas with anthropological data, I had the benefit of a Guggenheim Fellowship.

In the period of my entry into the psychiatric field, it was Jurgen Ruesch, with whom I worked in the Langley Porter Clinic, who initiated me into many of the curious features of the psychiatric world.

From 1949 to 1962, I had the title of "Ethnologist" in the Veterans Administration Hospital at Palo Alto, where I was given singular freedom to study whatever I thought interesting. I was protected from outside demands and given this freedom by the director of the hospital, Dr. John J. Prusmack.

In this period, Bernard Siegel suggested that the Stanford University Press republish my book, Naven, which had fallen flat on its face when first published in 1936; and I was lucky enough to get film footage of a sequence of play between otters in the Fleishhacker Zoo which seemed to me of such theoretical interest as to justify a small research program.

I owe my first research grant in the psychiatric field to the late Chester Barnard of the Rockefeller Foundation, who had kept a copy of Naven for some years by his

bedside. This was a grant to study "the role of the Paradoxes of Abstraction in Communication."

Under this grant, Jay Haley, John Weakland, and Bill Fry joined me to form a small research team within the V.A. Hospital.

But again there was failure. Our grant was for only two years, Chester Barnard had retired, and in the opinion of the Foundation staff we did not have enough results to justify renewal. The grant ran out, but my team loyally stayed with me without pay. The work went on, and, a few days after the end of the grant, while I was writing a desperate letter to Norbert Wiener for his advice about where to get the next grant, the double bind hypothesis fell into place.

Finally Frank Fremont-Smith and the Macy Foundation saved us.

After that there were grants from the Foundations Fund for Psychiatry and from the National Institute of Mental Health.

Gradually it appeared that for the next advances in the study of logical typing in communication I should work with animal material, and I started to work with octopus. My wife, Lois, worked with me, and for over a year we kept a dozen octopuses in our living room. This preliminary work was promising but needed to be repeated and extended under better conditions. For this no grants were available.

At this point, John Lilly came forward and invited me to be the director of his dolphin laboratory in the Virgin Islands. I worked there for about a year and became interested in the problems of cetacean communication, but I think I am not cut out to administer a laboratory dubiously funded in a place where the logistics are intolerably difficult.

It was while I was struggling with these problems that I received a Career Development Award under the National Institute of Mental Health. These awards were administered by Bert Boothe, and I owe much to his continued faith and interest.

In 1963, Taylor Pryor of the Oceanic Foundation in Hawaii invited me to work in his Oceanic Institute on cetacean and other problems of animal and human communication. It is here that I have written more than half of the present book, including the whole of Part V.

While in Hawaii, I have also been working recently with the Culture Learning Institute of the East-West Center in the University of Hawaii, and owe some theoretical insights regarding Learning III to discussions held in that Institute.

My debt to the Wenner-Gren Foundation is evident from the fact that the book contains no less than four position papers written for Wenner-Gren conferences. I wish also to thank personally Mrs. Lita Osmundsen, the Director of Research of that Foundation.

Many also have labored along the road to help me. Most of these cannot be mentioned here, but I must particularly thank Dr. Vern Carroll, who prepared the bibliography, and my secretary, Judith Van Slooten, who labored with accuracy through long hours in preparing this book for press.

Finally there is the debt that every man of science owes to the giants of the past. It is no mean comfort, at times when the next idea cannot be found and the whole enterprise seems futile, to remember that greater men have wrestled with the same problems. My personal inspiration has owed much to the men who over the last 200 years have kept alive the idea of unity between mind and body: Lamarck, the founder of evolutionary theory, miserable, old, and blind, and damned by Cuvier,

who believed in Special Creation; William Blake, the poet and painter, who saw "through his eyes, not with them," and knew more about what it is to be human than any other man; Samuel Butler, the ablest contemporary critic of Darwinian evolution and the first analyst of a schizophrenogenic family; R. G. Collingwood, the first man to recognize—and to analyze in crystalline prose—the nature of context; and William Bateson, my father, who was certainly ready in 1894 to receive the cybernetic ideas.

Selection and Arrangement of Items

The book contains almost everything that I have written, with the exception of items too long to be included, such as books and extensive analyses of data; and items too trivial or ephemeral, such as book reviews and controversial notes. A complete personal bibliography is appended.

Broadly, I have been concerned with four sorts of subject matter: anthropology, psychiatry, biological evolution and genetics, and the new epistemology which comes out of systems theory and ecology. Essays on these subjects make up Parts II, III, IV, and V of the book, and the order of these parts corresponds to the chronological order of four overlapping periods in my life in which these subjects have been central to my thinking. Within each part, the essays are in chronological order.

I recognize that readers are likely to attend most carefully to those parts of the book dealing with their particular subjects. I have therefore not edited out some repetition. The psychiatrist interested in alcoholism will encounter in "The Cybernetics of `Self' " ideas which appear again in more philosophic dress in "Form, Substance, and Difference."

Oceanic Institute, Hawaii Apra 16, 1971

1.4 INTRODUCTION

1.4.1 The Science of Mind and Order*

The title of this book of collected essays and lectures is intended precisely to define the contents. The essays, spread over thirty-five years, combine to propose a new way of thinking about ideas and about those aggregates of ideas which I call "minds." This way of thinking I call the "ecology of mind," or the ecology of ideas. It is a science which does not yet exist as an organized body of theory or knowledge.

But the definition of an "idea" which the essays combine to pro-pose is much wider and more formal than is conventional. The es-says must speak for themselves, but here at the beginning let me state my belief that such matters as the bilateral symmetry of an animal, the patterned arrangement of leaves in a plant, the escalation of an armaments race, the processes of courtship, the nature of play, the grammar of a sentence, the mystery of biological evolution, and the contemporary crises in man's relationship to him environment, can only be understood in terms of such an ecology of ideas as I propose.

The questions which the book raises are ecological: How do ideas interact? Is there some sort of natural selection which determines the survival of some ideas and the extinction or death of others? What sort of economics limits the multiplicity of ideas in a given region of mind? What are the necessary conditions for stability (or survival) of such a system or subsystem?

Some of these questions are touched upon in the essays, but the main thrust of the book is to clear the way so that such questions can be meaningfully asked.

It was only in late 1969 that I became fully conscious of what I had been doing. With the writing of the Korzybski Lecture, "Form, Substance, and Difference," I found that in my work with primitive peoples, schizophrenia, biological symmetry, and in my discontent with the conventional theories of evolution and learning, I had identified a widely scattered set of bench marks or points of reference from which a new scientific territory could be defined. These bench marks I have called "steps" in the title of the book.

In the nature of the case, an explorer can never know what he is exploring until it has been explored. He carries no Baedeker in his pocket, no guidebook which will tell him which churches he should visit or at which hotels he should stay. He has only the ambiguous folklore of others who have passed that way. No doubt deeper levels of the mind guide the scientist or the artist toward experiences and thoughts which are relevant to those problems which are somehow his, and this guidance seems to operate long before the scientist has any conscious knowledge of his goals. But how this happens we do not know.

I have often been impatient with colleagues who seemed unable to discern the difference between the trivial and the profound. But when students have asked me to

* This essay, written in 1971, has not been published else-where.

define that difference, I have been struck dumb. I have said vaguely that any study which throws light upon the nature of "order" or "pattern" in the universe is surely nontrivial.

But this answer only begs the question.

I used to teach an informal course for psychiatric residents in the Veterans Administration Hospital at Palo Alto, trying to get them to think some of the thoughts that are in these essays. They would attend dutifully and even with intense interest to what I was saying, but every year the question would arise after three or four sessions of the class: "What is this course all about?"

I tried various answers to this question. Once I drew up a sort of catechism and offered it to the class as a sampling of the questions which I hoped they would be able to discuss after completing the course. The questions ranged from "What is a sacrament?' to "What is entropy?" and "What is play?"

As a didactic maneuver, my cathechism was a failure: it silenced the class. But one question in it was useful:

> A certain mother habitually rewards her small son with ice cream after he eats his spinach. What additional information would you need to be able to predict whether the child will: a. Come to love or hate spinach, b. Love or hate ice cream, or c. Love or hate Mother?

We devoted one or two sessions of the class to exploring the many ramifications of this question, and it became clear to me that all the needed additional information concerned the context of the mother's and son's behavior. In fact, the phenomenon of context and the closely related phenomenon of "meaning" defined a division between the "hard" sciences and the sort of science which I was trying to build.

Gradually I discovered that what made it difficult to tell the class what the course was about was the fact that my way of thinking was different from theirs. A clue to this difference came from one of the students. It was the first session of the class and I had talked about the cultural differences between England and America—a matter which should always be touched on when an Englishman must teach Americans about cultural anthropology. At the end of the session, one resident came up. He glanced over his shoulder to be sure that the others were all leaving, and then said rather hesitantly, "I want to ask a question." "Yes." "It's—do you want us to learn what you are telling us?" I hesitated a moment, but he rushed on with, "Or is it all a sort of example, an illustration of something else?" "Yes, indeed!"

But an example of what?

And then there was, almost every year, a vague complaint which usually came to me as a rumor. It was alleged that "Bateson knows something which he does not tell you," or "There's something be-hind what Bateson says, but he never says what it is."

Evidently I was not answering the question, "An example of what?"

In desperation, I constructed a diagram to describe what I conceive to be the task of the scientist. By use of this diagram, it became clear that a difference between my habits of thought and those of my students sprang from the fact that they were trained to think and argue inductively from data to hypotheses but never to test hypotheses against knowledge derived by deduction from the fundamentals of science or philosophy.

The diagram had three columns. On the left, I listed various sorts of uninterpreted data, such as a film record of human or animal behavior, a description of an experiment, a description or photograph of a beetle's leg, or a recorded human utterance. I stressed the fact that "data" are not events or objects but always records or descriptions or memories of events or objects. Always there is a transformation or recoding of the raw event which intervenes between the scientist and his object. The weight of an object is measured against the weight of some other object or registered on a me-ter. The human voice is transformed into variable magnetizations of tape. Moreover, always and inevitably, there is a selection of data because the total universe, past and present, is not subject to observation from any given observer's position.

In a strict sense, therefore, no data are truly "raw," and every record has been somehow subjected to editing and transformation either by man or by his instruments.

But still the data are the most reliable source of information, and from them the scientist must start. They provide his first inspiration and to them he must later return.

In the middle column, I listed a number of imperfectly defined explanatory notions which are commonly used in the behavioral sciences—"ego," "anxiety," "instinct," "purpose," "mind," "self," "fixed action pattern," "intelligence," "stupidity," "maturity," and the like. For the sake of politeness, I call these "heuristic" concepts; but, in truth, most of them are so loosely derived and so mutually irrelevant that they mix together to make a sort of conceptual fog which does much to delay the progress of science.

In the right-hand column, I listed what I call "fundamentals." These are of two kinds: propositions and systems of propositions which are truistical, and propositions or "laws" which are generally true. Among the truistical propositions I included the "Eternal Verities" of mathematics where truth is tautologically limited to the do-mains within which man-made sets of axioms and definitions obtain: "If numbers are appropriately defined and if the operation of addition is appropriately defined; then 5 + 7 = 12." Among propositions which I would describe as scientifically or generally and empirically true, I would list the conservation "laws" for mass and energy, the Second Law of Thermodynamics, and so on. But the line between tautological truths and empirical generalizations is not sharply definable, and, among my "fundamentals," there are many propositions whose truth no sensible man can doubt but which can-not easily be classified as either empirical or tautological. The "laws" of probability cannot be stated so as to be understood and not be believed, but it is not easy to decide whether they are empirical or tautological; and this is also true of Shannon's theorems in Information Theory.

With the aid of such a diagram, much can be said about the whole scientific endeavor and about the position and direction of any particular piece of inquiry within it. "Explanation" is the mapping of data onto fundamentals, but the ultimate goal of science is the increase of fundamental knowledge.

Many investigators, especially in the behavioral sciences, seem to believe that scientific advance is predominantly inductive and should be inductive. In terms of the diagram, they believe that progress is made by study of the "raw" data, leading to new heuristic concepts. The heuristic concepts are then to be regarded as "working hypotheses" and tested against more data. Gradually, it is hoped, the heuristic

concepts will be corrected and improved until at last they are worthy of a place in the list of fundamentals. About fifty years of work in which thousands of clever men have had their share have, in fact, produced a rich crop of several hundred heuristic concepts, but, alas, scarcely a single principle worthy of a place in the list of fundamentals.

It is all too clear that the vast majority of the concepts of contemporary psychology, psychiatry, anthropology, sociology, and economics are totally detached from the network of scientific fundamentals.

Moliere, long ago, depicted an oral doctoral examination in which the learned doctors ask the candidate to state the "cause and reason" why opium puts people to sleep. The candidate triumphantly answers in dog Latin, "Because there is in it a dormitive principle (virtus dormitiva)."

Characteristically, the scientist confronts a complex interactive system—in this case, an interaction between man and opium. He observes a change in the system — the man falls asleep. The scientist then explains the change by giving a name to a fictitious "cause," located in one or other component of the interacting system. Either the opium contains a reified dormitive principle, or the man contains a reified need for sleep, an adormitosis, which is "expressed" in his response to opium.

And, characteristically, all such hypotheses are "dormitive" in the sense that they put to sleep the "critical faculty" (another reified fictitious cause) within the scientist himself.

The state of mind or habit of thought which goes from data to dormitive hypothesis and back to data is self-reinforcing. There is, among all scientists, a high value set upon prediction, and, indeed, to be able to predict phenomena is a fine thing. But prediction is a rather poor test of an hypothesis, and this is especially true of "dormitive hypotheses." If we assert that opium contains a dormitive principle, we can then devote a lifetime of research to studying the characteristics of this principle. Is it heat-stable? In which fraction of a distillate is it located? What is its molecular formula? And so on. Many of these questions will be answerable in the laboratory and will lead on to derivative hypotheses no less "dormitive" than that from which we started.

In fact, the multiplication of dormitive hypotheses is a symptom of excessive preference for induction, and this preference must al-ways lead to something like the present state of the behavioral sciences— a mass of quasi-theoretical speculation unconnected with any core of fundamental knowledge.

In contrast, I try to teach students— and this collection of essays is very much concerned with trying to communicate this thesis—that in scientific research you start from two beginnings, each of which has its own kind of authority: the observations cannot be denied, and the fundamentals must be fitted. You must achieve a sort of pincers maneuver.

If you are surveying a piece of land, or mapping the stars, you have two bodies of knowledge, neither of which can be ignored. There are your own empirical measurements on the one hand and there is Euclidean geometry on the other. If these two cannot be made to fit together, then either the data are wrong or you have argued wrongly from them or you have made a major discovery leading to a revision of the whole of geometry.

The would-be behavioral scientist who knows nothing of the basic structure of science and nothing of the 3000 years of careful philosophic and humanistic thought

about man — who cannot define either entropy or a sacrament —had better hold his peace rather than add to the existing jungle of half-baked hypotheses.

But the gulf between the heuristic and the fundamental is not solely due to empiricism and the inductive habit, nor even to the seductions of quick application and the faulty educational system which makes professional scientists out of men who care little for the fundamental structure of science. It is due also to the circumstance that a very large part of the fundamental structure of nineteenth-century science was inappropriate or irrelevant to the problems and phenomena which confronted the biologist and behavioral scientist.

For at least 200 years, say from the time of Newton to the late nineteenth century, the dominant preoccupation of science was with those chains of cause and effect which could be referred to forces and impacts. The mathematics available to Newton was preponderantly quantitative, and this fact, combined with the central focus upon forces and impacts, led men to measure with remarkable accuracy quantities of distance, time, matter, and energy.

As the measurements of the surveyor must jibe with Euclidean geometry, so scientific thought had to jibe with the great conservative laws. The description of any event examined by a physicist or chemist was to be founded upon budgets of mass and energy, and this rule gave a particular kind of rigor to the whole of thought in the hard sciences.

The early pioneers of behavioral science not unnaturally began their survey of behavior by desiring a similar rigorous base to guide their speculations. Length and mass were concepts which they could hardly use in describing behavior (whatever that might be), but energy seemed more handy. It was tempting to relate "energy" to already existing metaphors such as "strength" of emotions or character or "vigor." Or to think of "energy" as somehow the opposite of "fatigue" or "apathy." Metabolism obeys an energy budget (within the strict meaning of "energy"), and energy expended in behavior must surely be included in this budget; therefore it seemed sensible to think of energy as a determinant of behavior.

It would have been more fruitful to think of lack of energy as preventive of behavior, since in the end a starving man will cease to be-have. But even this will not do: an amoeba, deprived of food, be-comes for a time more active. Its energy expenditure is an inverse function of energy input.

The nineteeth-century scientists (notably Freud) who tried to establish a bridge between behavioral data and the fundamentals of physical and chemical science were, surely, correct in insisting upon the need for such a bridge but, I believe, wrong in choosing "energy" as the foundation for that bridge.

If mass and length are inappropriate for the describing of behavior, then energy is unlikely to be more appropriate. After all, energy is Mass x Velocity2, and no behavioral scientist really insists that "psychic energy" is of these dimensions.

It is necessary, therefore, to look again among the fundamentals for an appropriate set of ideas against which we can test our heuristic hypotheses.

But some will argue that the time is not yet ripe; that surely the fundamentals of science were all arrived at by inductive reasoning from experience, so we should continue with induction until we get a fundamental answer.

I believe that it is simply not true that the fundamentals of science began in induction from experience, and I suggest that in the search for a bridgehead among the fundamentals we should go back to the very beginnings of scientific and

philosophic thought; certainly to a period before science, philosophy, and religion had be-come separate activities separately pursued by professionals in separate disciplines.

Consider, for example, the central origin myth of Judaeo-Christian peoples. What are the fundamental philosophic and scientific problems with which this myth is concerned?

> In the beginning God created the heaven and the earth. And the earth was without form, and void; and darkness was upon the face of the deep. And the Spirit of God moved upon the face of the waters.
>
> And God said, Let there be light: and there was light. And God saw the light, that it was good: and God divided the light from the darkness. And God called the light Day, and the darkness he called Night. And the evening and the morning were the first day.
>
> And God said, Let there be a firmament in the midst of the waters, and let it divide the waters from the waters. And God made the firmament, and divided the waters which were under the firmament from the waters which were above the firmament: and it was so. And God called the firmament Heaven. And the evening and the morning were the second day.
>
> And God said, Let the waters under the heaven be gathered together unto one place, and let the dry land appear: and it was so. And God called the dry land Earth; and the gathering together of the waters called he Seas: and God saw that it was good.
>
> Authorized version

Out of these first ten verses of thunderous prose, we can draw some of the premises or fundamentals of ancient Chaldean thought and it is strange, almost eerie, to note how many of the fundamentals and problems of modern science are foreshadowed in the ancient document.

(1) The problem of the origin and nature of matter is summarily dismissed.

(2) The passage deals at length with the problem of the origin of order.

(3) A separation is thus generated between the two sorts of problem. It is possible that this separation of problems was an error, but—error or not—the separation is maintained in the fundamentals of modern science. The conservative laws for matter and energy are still separate from the laws of order, negative entropy, and information.

(4) Orde is seen as a matter of sorting and dividing. But the essential notion in all sorting is that some difference shall cause some other difference at a later time. If we are sorting black balls from white balls, or large balls from small balls, a difference among the balls is to be followed by a difference in their location—balls of one class to one sack and balls of another class to another. For such an operation, we need something like a sieve, a threshold, or, par excellence, a sense organ. It is

understandable, therefore, that a perceiving Entity should have been invoked to perform this function of creating an otherwise improbable order.

(5) Closely linked with the sorting and dividing is the mystery of classification, to be followed later by the extraordinary human achievement of naming.

It is not at all clear that the various components of this myth are all products of inductive reasoning from experience. And the mat-ter becomes still more puzzling when this origin myth is compared with others which embody different fundamental premises.

Among the Iatmul of New Guinea, the central origin myth, like the Genesis story, deals with the question of how dry land was separated from water. They say that in the beginning the crocodile Kavwokmali paddled with his front legs and with his hind legs; and his paddling kept the mud suspended in the water. The great culture hero, Kevembuangga, came with his spear and killed Kavwokmali. After that the mud settled and dry land was formed. Kevembuangga then stamped with his foot on the dry land, i.e., he proudly demonstrated "that it was good."

Here there is a stronger case for deriving the myth from experience combined with inductive reasoning. After all, mud does re-main in suspension if randomly stirred and does settle when the stir-ring ceases. Moreover, the Iatmul people live in the vast swamps of the Sepik River valley where the separation of land from water is imperfect. It is understandable that they might be interested in the differentiation of land from water.

In any case, the Iatmul have arrived at a theory of order which is almost a precise converse of that of the book of Genesis. In Iatmul thought, sorting will occur if randomization is prevented. In Genesis, an agent is invoked to do the sorting and dividing.

But both cultures alike assume a fundamental division between the problems of material creation and the problems of order and differentiation.

Returning now to the question of whether the fundamentals of science and/or philosophy were, at the primitive level, arrived at by inductive reasoning from empirical data, we find that the answer is not simple. It is difficult to see how the dichotomy between substance and form could be arrived at by inductive argument. No man, after all, has ever seen or experienced formless and unsorted matter; just as no man has ever seen or experienced a "random" event. If, therefore, the notion of a universe "without form and void" was arrived at by induction, it was by a monstrous—and perhaps erroneous — jump of extrapolation.

And even so, it is not clear that the starting point from which the primitive philosophers took off was observation. It is at least equally likely that dichotomy between form and substance was an unconscious deduction from the subject-predicate relation in the structure of primitive language. This, however, is a matter beyond the reach of useful speculation.

Be that as it may, the central—but usually not explicit — subject matter of the lectures which I used to give to psychiatric residents and of these essays is the bridge between behavioral data and the "fundamentals" of science and philosophy; and my critical comments above about the metaphoric use of "energy" in the behavioral sciences add up to a rather simple accusation of many of my colleagues, that they have tried to build the bridge to the wrong half of the ancient dichotomy between form and substance. The conservative laws for energy and matter concern substance

rather than form. But mental process, ideas, communication, organization, differentiation, pattern, and so on, are matters of form rather than substance.

Within the body of fundamentals, that half which deals with form has been dramatically enriched in the last thirty years by the discoveries of cybernetics and systems theory. This book is concerned with building a bridge between the facts of life and behavior and what we know today of the nature of pattern and order.