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5

Logic Clarifying and Logic Obscuring

Now might I do it pat, now he is praying;
And now I'll do it. And so he goes to heaven.
And so am I revenged. That would be scanned.
A villain kills my father; and for that,
I, his sole son, do this same villain send
To heaven.
Why, this is hire and salary, not revenge!

Hamlet, act III, scene 3

Shakespeare's Prince of Denmark had choices. He considered the likely consequences of his conduct, such as the possible action of killing his uncle while he is praying. He also conscientiously gained further information by experiment: "I'll have these players/ Play something like the murder of my father/ Before mine uncle. I'll observe his looks,/ I'll tent him to the quick; if he but blench,/ I know my course . . ." The modern decision theorists would have approved both those aspects of Hamlet's decision process. But they might be a bit uncomfortable with his nonutilitarian criteria for choice, such as "whether 'tis nobler in the mind," and they might uneasily wonder whether his comment, that "There are more things in heaven and earth, Horatio,/ Then are dreamt of in your philosophy," applies to them as well. Hamlet's concern lest "the native hue of resolution/ Is sicklied o'er with the pale cast of thought," and whether "thinking too precisely on the event" is an excuse for cowardice are outside of the mathematical decision theorists' domain of discourse.

It was also in the sixteenth century, the time of Shakespeare, that scholars in Italy began to go beyond the study of Greek and Arabic mathematics. Hieronimo Cardano (1501–1576) in

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particular analyzed games of chance mathematically, in effect founding the theory of probability, and thus providing what is probably the first mathematical procedure for smart decisions when gambling. Galileo Galilei (1564–1642), famous for other work, further supplemented Cardano's analysis for dice games.[1] These precursors of modern mathematical decision theory focused on decisions of a special kind—when to bet in simple games of chance—far removed from the kinds of complex political and interpersonal choices confronting Hamlet.

Modern mathematical (i.e., statistical) decision theorists, on the other hand, have extended the ideas far beyond those beginnings. They claim for their techniques a wide scope, including even complicated interpersonal and political choices. They claim a comprehensiveness for their procedures that includes all decisions by individuals entailing uncertainty about the consequences. Part of the procedure can be carried out by a suitably programmed computer. Enthusiasts for statistical decision theory, by calling attention to their technology, tend to obscure the fact that it in no way replaces the truly difficult and often decisive aspects in people making choices: How among possible ways to best understand and conceptualize themselves, the world about them, and the impact of their actions on both; how to take into account the seemingly irrational elements when humor, paradox, or a multiplicity of objectives come into play; when to trust impulse to action or habitual passivity and when to delay so as to collect data and calculate, and so on.

Leonard Jimmy Savage, a young mathematician, did not belong to the cluster I have called the cyberneticians. During the years he was attending the conferences he worked on the reformulation of mathematical statistics and developing a systematic procedure for making "wise" decisions. He is a particularly interesting figure among the originators of modern statistical decision theories because he took subjective judgments seriously and paid considerable attention to philosophical critiques of his own method. Gregory Bateson, on the other hand, had come to consider questions of wisdom via biology, cultural anthropology, observation of natural language, and some reading in the Russell-Whitehead *Principia Mathematica*. Aside from having come to these questions through disparate intellectual routes, Bateson and Savage had entirely different personal and social backgrounds as well. They present a study in contrasts. Although not professional philosophers, neither shied away

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from reflecting about pertinent philosophical issues. Good decisions concern us all.

The interdisciplinary conferences presented a setting well suited for manifesting their difference in style and outlook. For instance, the persistent theme of comparing computers and brains had led to characterizing mechanisms in the brain as either "analogical" or "digital," in spite of some ambiguity in the definition of these concepts. After Ralph Gerard had at the seventh conference spoken about the respective roles of the two types of mechanism in the central nervous system, Bateson characteristically interjected, "I am a little disoriented by the opposition between analogical and digital." While the discussion focused primarily on detailed neurophysiological data, Bateson kept insisting on a clarification of the concepts. "It would be a good thing to tidy up our vocabulary," he said. Bateson recalled the historic conflict in Great Britain between the geneticists, especially his father, and the statisticians, especially Karl Pearson, concerning whether variation of biological species formed a continuum or is discontinuous.[2] He also recalled that an analogous argument had taken place at the meeting which Köhler attended (see chapter 10): the continuum of the Gestalt theory vs. the atomism of the Pitts-McCulloch model. But in the discussion following Gerard's paper at the seventh meeting one detects a tug of war between those intrigued with clarifying the concepts and those interested in data and description of experiment. Bateson and Savage were tugging in opposite directions. Walter Pitts finally cleared the air as to the concepts by succinctly pointing out that "digital and analogical sorts of devices have been defined quite independently and are not logical opposites." Although Bateson had pioneered the use of films and photographs in the human sciences, words and concepts continued to be for him the essential mode for representing reality in the social sciences. It was especially because the concepts of cybernetics had arisen in connection with engineering and mathematics that Bateson looked to them as tools for overcoming the vagueness and ambiguity of the conventional language of social science. Thus careful and precise use of language was crucial.

During the discussion Savage (as well as Bigelow, Licklider, and others) apparently became bored and impatient. As to the "battle" concerning analogical and digital, Savage interjected, "The battle is whether the distinction is worth making or not."

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He further demeaned the "concepts" by referring to them as mere "nomenclature." The authentic scientific description was for Savage always operational, mathematical and mechanical, never merely verbal. His outlook was close to the logical positivists. In his own work the role of concepts and words was to describe actual or contemplated operations that are used to define mathematical symbols, but the words or concepts had no particular importance in themselves.[3] He wanted to get on with a description of the technical details.

A similar tug of war reinforcing earlier alignments occurred at the ninth conference following a presentation of Gerard's dealing with the details of excitation and inhibition of neurons, with Bateson attempting to shift the discussion so that it was relevant to social scientists. He interposed, "I have been racking my brains to consider what order of question I can put which will, at least implicitly, focus discussion upon general philosophical and epistemological problems, even though it be verbally a domestic neurophysiological question . . ." and indeed he then produces two such questions.[4] He was bored by the "domestic neurophysiological questions" themselves.

Bateson would also have been bored by the details of the still-infant phage work of Max Delbrück, whereas Savage would have been fascinated. Delbrück was one of those guests at the Macy who, like Erikson, did not return. In Delbrück's case, however, cyberneticians Pitts, von Neumann, McCulloch, and others wanted him to become a regular member, but Delbrück was not interested. I digress to recount the diversity of approaches to genetics at the meetings, analogous to the diverse approaches to making wise choices, and its consequences.

Gregory Bateson had imbibed discussion of issues in genetics practically with his mother's milk. Having learned of de Vries's rediscovery of Gregor Mendel's work in 1900, William Bateson became the chief publicist for it in England, and when his third son was born, in 1904, he named him Gregory in honor of Mendel. William Bateson coined the word "genetics" to encompass the study of heredity and variation of species, and—although some of his ideas were off the mark—as early as 1907 he had anticipated that biochemical factors play a role in linking genotype to somatic changes.[5] These issues, which Gregory had been exposed to and become involved with in his youth, continued to tug at Gregory, and he became actively engaged with them again some time after the cybernetics conferences.

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Gregory Bateson's interest in evolutionary theory lay in exploring the dynamic pattern of relationships among genotype, phenotype, and the environment generally, and in concrete instances, so as to properly understand the phenomena of adaptation and phylogeny.[6]

John von Neumann had an entirely different kind of interest in genetics. He knew Max Delbrück and his work on bacteriophage—primitive self-reproducing organisms conveniently accessible to experimental study—and as early as 1946 thought it would be a challenge to devise a rigorous description of them.[7] Within a few years von Neumann developed a theory of self-reproducing automata, although, with modern molecular biology only in its infancy, he did not explicitly apply it to genetics. At the third meeting (March 1947) von Neumann said that a geneticist should be invited to become a regular participant.[8] Delbrück would be particularly congenial to him.

It was decided that a geneticist should be invited: Aside from Delbrück, Spiegelmann, Sewell Wright, and Dobzansky were also considered.[9] Some thought in terms of a biochemist rather than a geneticist.[10] Bateson, as well as Hutchinson, favored Spiegelmann, although he was more a biochemist, and there was a question whether he could be regarded as a proper geneticist who would satisfy von Neumann.[11] Hutchinson wrote to McCulloch:

> My chief reason for wanting Spiegelmann is that I know he has a very great amount of significant material ready along the general lines of the conference, relating to enzyme systems and cell metabolism. . . . It would be extremely instructive for the group to hear about it. . . .[12]

Bateson and Hutchinson were presumably anticipating an application of cybernetics along the lines described by Quastler at the ninth meeting, which came concretely into its own in what Jacques Monod much later called "microscopic cybernetics."[13]

Max Delbrück, von Neumann's choice, was invited to the fifth meeting and was expected to become a regular member of the group.[14] But the first day of the fifth meeting, orchestrated by Bateson and Mead, was given over to the subject of language (Roman Jakobson, Charles Morris, Dorothy Lee were among the invited guests). The second day was dominated by Wiener speaking about the evolution of order from chaos and giving an analysis of mechanisms serving as Maxwell demons,

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and by Pitts making a formal analogy from molecular collisions to describe the establishment of a pecking order among chickens, a problem in mechanism for social organization. Both analyses assumed initial randomness. The final topic was the use of the concept "I," a topic with which Dorothy Lee had been concerned in language, and questions such as whether the word "I" originates only in a social context. But where did all this connect to Delbrück's concerns?

Max Delbrück, a physicist strongly influenced by Niels Bohr's thinking about "complementarity," had in the 1930s turned to biology with ideas about the physical basis of heredity. By the time he came to the cybernetics conferences, he was deeply enmeshed in experimental work on bacteriophage, optimistic that he and his coworkers were on the right track to solve "the riddle of life." Others were similarly fascinated and optimistic, and after the war Delbrück had brought together a sharply focused research and learning summer community, the "phage group." He "succeeded in creating at Cold Spring Harbor (on Long Island) that spirit of ceaseless questioning, dialogue, and open-armed embrace of a life in science which he had learned from Bohr—but with a down-to-earth American character and a good measure of his own high-minded intolerance of shoddy thinking."[15] The narrow focus on the one specific organism, bacteriophage, was an effort to reduce heredity to specific molecular mechanisms and to identify these and the structure of the molecules involved. Thus heredity would then be reduced to physics.

By contrast, the interdisciplinary cybernetics group was diffuse in its focus, and Delbrück had no interest in it. He was famous for his bluntness. Commenting on the fifth meeting, he later said: "It was vacuous in the extreme and positively inane. Genetics did not, and at that time could not, enter into it at all. Also I was not then, and have not later, been much interested in the areas of information processing in the vertebrate CNS, or in simpler nervous systems, and in computer analogues."[16]

It is noteworthy that the molecular biology pursued by Delbrück and the phage group was on a very different track from that of the cyberneticians. It attracted many physicists who had read Schrödinger's *What is Life?*, while cybernetics did not—with the exception of Förster, who was in any case more an engineer than a physicist. Physicists in the 1940s tended to believe in a "building block" model of the universe, in which the

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smaller the unit, the more fundamental the explanation. The conceptions of Rosenblueth, Wiener, and Bigelow related engineering and high technology to the behavior of organisms on a human scale, but they were not particularly to the taste of physicists because they did not reduce description to laws of physics or to molecular building blocks; in that sense were not reductionist enough. Nor did they make contact with quantum theory. The notion of information, however, purported to extend one respected principle of physics, the Second Law of Thermodynamics, and soon caught the attention of and was used by physicists.[17] The McCulloch-Pitts model as well was not an application of principles familiar to physicists, but rather those of symbolic logic. The topic was not amenable to the normal theoretical tools of physicists at that time. The problem of the mechanism of gene multiplication and heredity was a different matter. Indeed Schrödinger's book (1944), citing Delbrück's work, had proclaimed the possibility that the mechanism for heredity is a fundamental topic in biology that can be reduced to molecules and principles of physics. Not surprisingly, many physicists were attracted to it after the war, the more so if they were sick of science linked to instruments of death.

But Delbrück came and went, and the cybernetics group never had a geneticist or a biochemist as a regular member. Molecular biologists were relatively slow in adapting the ideas of cybernetics to their needs. And Bateson never changed his view that the ideas of cybernetics are of far greater fundamental import than molecular biology.

In spite of his impatience with "domestic neurophysiological questions" Bateson regarded the cybernetics conferences as an intellectual event of the first magnitude, very exciting, and in many of his later writings he generously acknowledged their enormous influence on his own work.[18] But in sharp contrast to Bateson, Savage saw them primarily as pleasant "bull sessions with a very elite group . . . ; I could never take seriously the ostensible purpose of coming there to blend our disciplines or to grapple with each other's problems."[19]

As a further illustration of Savage's and Bateson's different styles, we compare their responses to the important ideas of "information" in communication theory, and the relation of information to entropy, both of which had already been pre-

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sented at the March 1946 meeting of the group. At that time the theory was new, and the classic articles of Claude Shannon and Norbert Wiener appeared in print only in 1948. How did Savage and Bateson respond to the new information theory?

Savage studied the Shannon paper, followed the theorems and proofs. He enjoyed Shannon's work, saw it as posing and neatly solving some consequential intellectual puzzles of a mathematical kind. Wiener had emphasized in particular the concept of "information," or "information-theoretic entropy," which had been introduced by him and independently by Shannon. Savage was not interested in putting this concept on a pedestal; it was solutions to problems that mattered to him, not the idea of "information." He did not even take to the idea, as others would, of using the concept of "information" to obtain the probability distribution when limited data is available.[20] Bateson, whose mathematical training was quite limited, had the opposite reaction. He was stimulated by Wiener's suggestion that in a broad historical context the concepts of information and communication theory were ideas whose time had come, not only applicable to engineering but of significant heuristic value for the biological and social sciences. Bateson noticed that psychiatric thinking had increasingly come to emphasize interpersonal communication, and he set out to relate the new emphasis in engineering to the shifting emphasis in psychiatry.[21] In fact, a qualitative analog to the communication-theoretic concept of "message" came to play a central role in nearly all of Bateson's subsequent work on human interactions and animal behavior.

Much of Bateson's pleasure in science arose from creating a connected understanding of the most diverse kinds of knowledge and experience of the world. He had quite naturally wandered in his own career from zoology to social anthropology, and from there to a description of psychiatry; nor would he stop there. He has said, "I grew up in a world that has the idea that some ideas are non-trivial."[22] "Information" is that kind of an idea. To Bateson, the elucidation of general principles that cut across scientific disciplines is a crucial part of what makes science interesting and gives insight into the nature of things. Savage, in contrast, was ill at ease with such general principles; he limited his scope, as he relied on the solidity of mathematical proofs, axioms, or mechanical processes. Savage's rejection of those general ideas so fascinating to Bateson expresses perhaps

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an innate modesty. Like many American scientists, he tended to be repelled by some far-reaching ideas, largely because of their grandness.[23]

More immediately pertinent to decision theory than "information" is the question of the existence or nonexistence of a hierarchy of values for a single person. It was well known that if several voters are to choose among three candidates A, B, and C, the paradox of the cyclical majority may arise: Even if each voter can list the candidates in his or her order of preference, it can easily turn out after counting the votes that the majority prefers A over B, B over C, and C over A—in other words, a circular preference.[24] But Warren McCulloch had asserted that in experimental aesthetics he had found instances in which preferences are circular even for a single individual, and that experimental psychologists observing animal behavior had found analogous results for rats.[25] Bateson was immediately intrigued. McCulloch had concluded that "for values there can be no common scale," according to which all can be arranged in hierarchical order. "Therefore, as to the good in the biological sense, and the beautiful in the sense in which it can be inferred from mere human preference, we indeed live in a world of many incomparable values, where fate compels us to choose whenever we act. . . ."[26] McCulloch was not defending irrationality, but he constructed a hypothetical pattern of neurons in the brain that could give rise to such nonhierarchical schemes of preference.

Bateson played with this idea and contemplated various possibilities for understanding the empirical result. For example, one might analyze a net preference as in effect due to several entities (subsystems) within one person, each maximizing a particular variable, and that could, like several voters, easily lead to a circular preference.[27] He also sought from McCulloch more information about the empirical data, but didn't get much. For Bateson, careful observation of human behavior was fundamental to the human sciences. Savage's attitude, when constructing his statistical decision theory, was entirely different. His theory required that a decision maker can construct a hierarchical list of preferences. He said logical consistency required it. The wise decision maker, according to Savage, will be willing in every case to construct a "logically consistent" hierarchical list of preferences for the various possible outcomes of

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an action. Thus, as he admits, his theory is intended to be normative rather than empirical.[28]

Born in 1917, Savage spent his boyhood in Detroit. His father, a Jew without much formal education who had changed his name from Ogashevits to Savage, was in commercial real estate. Jimmy Savage has written, "My grandparents were hopeful immigrants from a cruel country, and the elan of the great American melting-pot epoch came down to me in many ways through my father and mother, giving me some disposition to reach for the stars."[29] But the obstacle to the aspiring boy scientist was that his teachers seconded the result of his IQ test, which put him into the class of morons, and strongly recommended against his going to college. He suffered from myopia and severe nystagmus, eye conditions that doubtless contributed to his failure in high school.[30] But once in college his abilities were appraised differently, and he obtained his doctorate in mathematics from the University of Michigan at the age of twenty-three.

As a graduate student Savage had encountered von Neumann's classic 1928 paper on game theory, was intrigued by it, and appreciated its potential applicability to economics. His first published paper was in collaboration with an economist.[31] Savage spent a postdoctoral year at the Institute for Advanced Study, in Princeton, where he became further acquainted with von Neumann and his work. At the end of the year von Neumann offered him a position at the institute as his assistant. Despite his admiration for that extraordinary mathematician Savage turned it down, just as he did a 1946 offer to join von Neumann's computer group.[32] During the Second World War he joined the Statistical Research Group at Columbia University, where among his colleagues were the pioneer of decision theory Abraham Wald, the economist Milton Friedman (with whom Savage later collaborated), and statistician W. Allen Wallis. Savage was working in applied mathematics, and he came to think of mathematical applications to biology as his own line of work. After the war he continued consulting with biologists about application of mathematics, especially statistics, to their various research problems, and obtained a special Rockefeller Fellowship to pursue this at Woods Hole Marine Biology Laboratory and at the University of Chicago. After some years at Chicago Savage shifted direction. As he wrote in a fellowship

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application, he had found that "the work in biology, while constructive has tended to be intellectually rather elementary, so that, though I expect to continue activity in biology, most of my research for the next several years is likely to be in the more academic aspects of statistics and the closely related field of probability."[33] Just as Norbert Wiener's enthusiasm for linking mathematical and biological topics was riding high, Savage's focus had shifted away from biological applications to the foundations of statistics.

His idea was first of all to follow von Neumann and Morgenstern,[34] notwithstanding McCulloch's arguments, and introduce a person's "utility function"—a linearly ordered quantitative measure for the "value" one places on the various possible outcomes of an action. To construct a "behavioral statistics" or "statistical decision theory" also requires a clear definition of the "probability" for each possible outcome. Only then could one compute the expected utility of an action and pick out the preferred one. Here Savage parted company with Abraham Wald, R. A. Fischer, and the leading statisticians of his day, members of the "frequentist" school, who had defined probability in terms of the results of an actual or hypothetical experiment repeated many times.[35] Savage preferred to define probability not as the result of any objective experiment but as a measure of the strength of a person's belief in a particular proposition, as, for example, the bets he would in principle be willing to make that a particular event would occur. The "frequentist," or objectivist, school could not readily assign a number to the likelihood of a unique event, such as the probability that a particular person will be the Democratic nominee in the next U.S. presidential election, because it is a question not suited for repeated experiment.[36] On the other hand a person could give his "personal probability," as Savage liked to call it, about such a one-shot event. Before Savage had come to it, the personal probability concept had been used by Frank Ramsey in England and Bruno de Finetti in Italy, who had ideas similar to Savage's, although the underlying idea of a subjective probability goes back further to Pierre Simon de Laplace (1749–1827) and Jacob Bernoulli (1654–1705).

The task Savage set for himself was in terms of what he saw as the task of statistics, namely, "the formulation of satisfactory rules for action in the face of uncertainty." I. J. Good in England was independently working along similar lines. At the

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time of the conferences Savage was occupied with the exercise in logic and mathematics posed by the idea of reformulating decision theory or "behaviorist statistics" in terms of the two basic notions of utility and personal probability, an exercise in manipulating axioms, definitions, and symbols. As he was trying out axioms and definitions, the rules of the game were that the basic concepts (utility, probability) be related to actual possible operations in the spirit of Bridgeman's operationalism, and that the definitions and postulates were to be complete, consistent, and logically correct. In addition, he appears to have chosen concepts and postulates in a way that he found aesthetically and mathematically pleasing. His book *The Foundations of Statistics*, published just a year after the last of the ten Macy conferences, sums up his main original work of those years.

I met Savage in 1968 at Yale University, where I was a visiting scholar, and interviewed him twice.[37] He was a tall, moderately slim man with a gray beard and sideburns, extremely thick glasses, dressed in the usual professorial attire of slacks and sport coat. A slight nasal quality and expressive rhythmic changes in inflection in his voice characterized his articulate speech. He was a technological optimist who could expound eloquently on the wonders of penicillin, radar, transistors, and space travel, retaining some of the ebullience of the boy-scientist. Lest he take himself too seriously, he also collected spoofs on science: "It is good to laugh at ourselves sometimes."

His office was a large department chairman's office: the rugs on the floor were Navajo, the paintings on the wall were good reproductions of Breughels and an early Italian or two. Watching Savage pick up a letter and place it practically right against his face to read it, I was reminded of his eye condition and wondered whether he at all saw or could appreciate the paintings. When I later inquired about this, he responded, "I do see them and enjoy them in my way. The Breughels with all their interesting detail are a little more frustrating for me than the others." We sat down, he on a couch and I on a comfortable chair, and began to talk. While we were talking, Savage sitting on the couch with his legs crossed, he repeatedly touched the couch and a cushion in a way that suggested considerable tactile sensitivity.

He told me of his fascination with hoaxes, crackpots in science and the ostracized scientist, and more generally controversy in science.

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"I've worked with crackpots. I used to be the specialist on angle-trisectors and the like in Chicago for a time. . . . I always wonder about the line between being a crackpot and being really original but unheard. There is no sure way. Galois, one of the greatest mathematicians that ever lived, was rejected from school because all he had to offer to get in were these crackpot theories which were absolutely correct. . . . I am in correspondence now with a wonderful old Indian gentleman who desperately needs a teaching job. He has finally got his immigration status settled in the United States. His written English is just beautiful. As a youth he formulated some antirelativistic theory of relativity. I just saw his manuscripts, beginning yesterday . . ."

Yet on an earlier occasion, when we had been discussing some successful established scientists, he expressed a very different attitude toward some of them.

"I rarely can take 'big geniuses' seriously. I am always crossing people who think, whether rightly or wrongly, that they have some really great discovery . . . some of those people might take me into their confidence and I begin to see that they consider me as bright as they are, and then I am even more upset than I was when I thought they were arrogant. I am a lot older now, and I see that a lot of these people are just naïve."

Now he candidly volunteered a more personal concern, perhaps not uncommon among successful innovators: "I hold a minority view (in statistics), I hold it quite rigorously and narrowly . . . I have sometimes wondered, how do I know that I am not a crackpot?" In strongly espousing a viewpoint in statistics, he was going well beyond what can be rigorously proved by mathematics or refuted by experiment. He further reflected on the fact that in spite of his minority view, which had grown increasingly radical, he had not been ostracized by the statistics community. On the contrary, the viewpoint he had first introduced in the United States twenty years ago has won many adherents.

Savage spoke of encountering people—both professional statisticians and those in quite different professions—who naturally, habitually think along lines of personal probabilities, utilities, Bayesian statistics. It seems more like a personality trait than the result of intellectual training. Savage described the reassuring experience of encountering someone who is viscerally and intellectually attuned to his own outlook.

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"We are far apart geographically and far apart in our other scientific interests, but when we talk we know that we agree about almost everything. There is a system there that works. It's as though you had been taught in childhood what was supposed to be Albanian, but you never knew an Albanian and you weren't quite sure whether you'd been tricked. But if you got off the dock in Albania you would know. . . . It's like that. I know that it's cogent because it's in such absolute detailed agreement with Schlaifer."

The growth in acceptance of personalistic statistics in the United States was nurtured by Savage's vigorous and patient didactic efforts. In conversation about his work, as in his writings, Savage showed a constant awareness of who the listener or reader was; he did not try to impress the reader, but conscientiously helped him to understand each step, besides trying to persuade him of a point of view. In the preface to his book, which is not a textbook but a presentation of Savage's new formulation of statistics, a mixture of formal mathematics and homey examples, he nevertheless offered the following pedagogical advice:

> If one wants only the gist of it (a long mathematical argument), he may read such material once only; but otherwise he must expect to read it at least once again. Serious reading of mathematics is best done sitting bolt upright on a hard chair at a desk. Pencil and paper are nearly indispensable; for there are always figures to be sketched and steps in the argument to be verified by calculation.[38]

Savage was an able polemicist and writer of English prose, good humored, respectful and courteous toward opposing views, disarmingly candid about the difficulties of his own view, and relentless about the difficulties of alternate viewpoints. Thus section 4.2 of his book is devoted to those criticisms of his viewpoint that he admittedly finds difficult and confusing to answer. Again, in an essay he read to the American Philosophical Association in 1967, thirteen years after the book was published, he said: "We who defend the personalistic view naturally consider some of the most striking difficulties raised in objection to be illusory. I shall talk here not about those difficulties with the personalistic view that I can resolve, at least to my own satisfaction, but about those that particularly puzzle or confuse me. Consequently, I shall be raising many vague questions and making relatively few clear statements."[39] The vague and puz-

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zling aspects of which Savage refers are not in the formal mathematics but in the philosophical foundations of his normative theory. (Since Savage's death one serious difficulty with his personal statistics has arisen from experiments that show that most people's utility functions and personal probabilities depend strongly on the words used in stating facts and alternatives, not only on the facts and alternatives actually presented.)

Savage's book on statistics is dedicated to his father. If any area of scholarship would be meaningful to a businessman, I suppose it would be the problem of how to make sound decisions in the face of uncertainty. Of course it's not the same as coming into the business.

Gregory Bateson, in contrast to Jimmie Savage, grew up in a world where intellectual preoccupations were commonplace—especially the large ideas around the Darwinian theory of variation of species. He was heir to a kind of intellectual aristocracy. The countryside around Cambridge and the world of creatures to which it was hospitable, so different from the industrial urban Detroit environment, provided the favorite pastime for Gregory as a youngster, his brothers, and their friend G. Evelyn Hutchinson. As Hutchinson recalled,

> Every autumn one could find the large fawn-and-chestnut-colored caterpillars of the goat moth crossing the road, looking for sites into which to burrow and become chrysalises. . . . Equally exciting were the bizarre caterpillar of the puss moth, the various species of hawk moth—poplar, line, and eyed—and the great green metallic musk beetle that sometimes was seen sitting on arrowhead leaves growing from backwaters of the river . . . adjacent rough pastures with ditches and ponds yielded newts and sticklebacks; occasionally one saw the kingfisher, the most spectacular of all ordinary British birds, darting along a stream . . . Daubenton's bat used to sleep under the spans of the bridges and hawk gnats up and down over the water at dusk. . . . Much more obscure, and in some ways more fascinating, were the animals that one knew some learned men studied, for the town was full of amateur naturalists as well as having its academic professional zoologists. . . .[40]

Gregory thirty years later brought his boyhood friend Hutchinson to join him at the cybernetics conferences.

Gregory's style as anthropologist was informed by the habits of a trained naturalist, habits and pleasures of observation. His style of intellectual understanding, which he could adapt from one discipline to another, was primarily ecological, long before

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"ecology" had become a popular notion. At a conference in the 1950s, when asked to speak about himself, he talked informally:

> . . . I suppose the thing I hold sacred is something in the nature of patterns, and that which makes me angry is the violation of patterns in some form or other. So I find myself today studying schizophrenic subjects, trying to help them find valuable patterns in their lives; and on the other side of the same picture, I find myself angry at the distortions of pattern that happen to them as children. . . .[41]

But thinking in terms of patterns and ecological stability, as Bateson did, led from concrete observation to reflection on larger systems. Elucidation of general principles, abstract language, was needed to make sense out of nature's diversity. For his own early notions of proto-learning, deutero-learning, and higher order learning,[42] i.e., levels of learning, he sought a still more abstract and widely applicable formulation; he found it at the cybernetics conferences by adopting the language of the Russellian "theory of types" as a metaphor. Similarly, for comprehending his early observation of stability in the Iatmul culture,[43] his own concepts of "symmetrical schismogenesis" and "complementary schismogenesis" seemed too narrow, and Bateson adopted the language of cybernetics pertaining to circuits with positive or negative feedbacks. General and abstract principles helped him to understand how the world worked and especially permitted him to see mind, culture, and biology as a natural unity. He addressed and stayed with the issues that mattered to him, rather than emasculating the questions so that they might be answered neatly in terms of available scientific techniques. Thus he often found himself an intellectual pioneer, confidently and resourcefully surveying the wilderness. Far more directly than for most scientists, science for Bateson was a means for general understanding and wisdom. To Bateson wise action required first of all deep and rich understanding, a going beyond what the academies, with their departmentalization, recognize as learning.

I interviewed Bateson at the Oceanic Institute in 1968, spending most of a day with him.[44] He spoke in complete sentences, highly intelligent and articulate. Yet he seemed to be making an effort to describe his recollections accurately rather than merely indulge reminiscences. . . . His tone of voice was authoritative, implying naturally, quite unassertively "this is

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how it was" or "this is how it is." From time to time a particular memory would cause laughter to well up—a gentle, earthy and voluptuous laughter, that seemed to express delight and appreciation of the human comedy in its many-fold subtleties.

One of the members of Bateson's graduate seminar twenty years earlier evoked in his description qualities I had seen in Bateson in a different setting:

> . . . Bateson taught us to see the symbolism, like nerve and muscle, beneath the skin of ordinary appearances. He noticed everything, as if seeing was a marvelous game. The Iatmul culture of New Guinea, which he analyzed in *Naven*, seemed to fill him with joy because it was so absurdly and touchingly human. He talked about it as if he felt an immense relief that men and women somewhere in the world should have clear-cut and splendidly eccentric customs. He took a special delight in the men's ritual boasting in the ceremonial house, as if he thought it both comical and natural that men should boast.[45]

This humor-filled and appreciative understanding of things human would have to be corroborated by any codification of wisdom, if such codification were to be convincing to Bateson. It is somewhat ironic that he reached for the formal logic of the *Principia Mathematica* of Russell and Whitehead, as mediated to him through the mathematician and student of Russell, Norbert Wiener, at the Macy conferences, as a metalanguage suited for his purposes.

From that first conference in March 1946 when Wiener had discussed Russellian paradox and the computer's oscillatory (yes-no-yes-no-yes-no) response to it, Bateson found considerable rapport with Wiener.[46] They had in common some uncommon preconceptions about science that influenced their scientific styles: both were highly interdisciplinary in their range of curiosity and had a sense that everything is connected to everything else, and that such connections can be manifested on an abstract or philosophical plane. Unlike Savage, neither agreed with the prevailing logical positivistic philosophy of science. Both found highly abstract, cross-disciplinary principles interesting, although they knew, as Wiener put it, "as a rule 'high' order, very abstract and general statements are not amenable to experimental test. They have to be broken down into more specific terms."[47] Wiener and Bateson were both willing to translate exact theorems of communication engineering, physics, and formal logic into relatively loose verbal, formal

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statements—which they would then extend and apply in a heuristic way to other areas of science, although most scientists frowned on such practices. Bateson in particular applied them to the human sciences. Having accepted the legitimacy of the human endeavor to understand the world in a more than piecemeal, "departmentalized" way, they had necessarily to take into the bargain paradoxes, incompleteness, vagueness, and tentativeness. Narrow specialization had been the sine qua non of science for generations; consequently, Wiener's and Bateson's efforts to describe the world and ourselves in a comprehensive, holistic way and yet function as scientists were not taken seriously by colleagues and tended to isolate them.

Their collaboration, which took place through visits and correspondence, suggests that social scientists and mathematicians may be able to work together if they have common assumptions about science. Savage and Bateson ignored each other's work. Had either attempted to impose his own method of thought on the other's work, the other's weaknesses would have been pointed up. At the time of the Macy meetings, when each was busy developing his own constructive ideas, such criticisms could only have hindered them. Thus the mutual lack of interest was fortunate for both. It tends to support the birds-of-a-feather thesis that the only Macy group social scientist with whom Savage collaborated was sociologist Paul Lazarsfeld. Lazarsfeld, the director of the Bureau of Applied Social Research of Columbia University, favored a positivistic philosophy of science as Savage did. He had been a champion of quantitative and statistical sociology, and his institute was a beneficiary of the generous financial support that flowed into mathematical social science in the United States in the late 1940s and early 1950s. Savage was helpful to him over the years in some statistical problems.[48] Like Savage, Lazarsfeld did not find the Macy conferences particularly relevant to his own work. He had been absent from the sixth meeting and was subsequently dropped from the roster.[49]

Bertrand Russell's theory of logical types arose in the early part of the twentieth century as a result of contradictions and paradoxes in the mathematical theory of infinite sets. For example, the concept of "the class of all those classes that are not members of themselves" was self-contradictory; one such contradictory entity within mathematics endangered the self-consistency of all of mathematics, so that Russell came to the

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conclusion that all such paradoxical statements had to be ruled out. He devised a "theory of types," according to which an individual, a class of individuals, a class of classes, etc., form an ascending hierarchy—and to avoid paradox, one must remember that the legitimate members of a set at one level all belong to the level just below. The classic Greek paradox about the barber who shaves all those men in his village, and only those, who do not shave themselves, led to contradictory conclusions as to whether the barber shaves himself. According to Russell the confusion arises because the statement speaks of a class (shaver) that is or is not a member of itself.

A parallel consideration of language leads to a corresponding hierarchy of simple statements, statements about statements (metastatements), statements about metastatements, and so on. To avoid paradox, each statement may be about only elements at the next lower level. The paradoxical assertion "This statement is a lie" is ruled illegitimate because it is a statement about itself rather than about elements at a lower level in the hierarchy. After 1946 Bateson increasingly used the Russell theory as a heuristic device and metaphor for describing human communication.[50] In the years following the Macy conferences, he employed it for a general theory of play and fantasy as well as for the double-bind hypothesis in schizophrenia.

Bateson's originality in using the theory was already evident in his provocative presentation at the ninth Macy conference on humor in human communication.

> One of the rather curious things about *homo sapiens* is laughter, one of the three common convulsive behaviors of people in daily life, the others being grief and orgasm. . . . Of the three types of convulsions, laughter is the one for which there is the clearest ideational content. It is relatively easy to discuss what is a joke, what are the characteristics that make a joke, what is the point of a joke. The sort of analysis that I want to propose assumes that the messages in the first phase of telling the joke are such that while the informational content is, so to speak, on the surface, the other content types in various forms are implicit in the background. When the point of a joke is reached, suddenly this background material is brought into attention and a paradox or something like it is touched off. A circuit of contradictory notions is completed.[51]

Bateson argued that the "paradox" in humor is the same as that which arises from mixing of logical types and presented the hypothesis "That the paradoxes are the prototypic paradigm

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for humor, and that laughter occurs at the moment when a circuit of that kind is completed." Unfortunately, Savage missed that particular conference and both Wiener and von Neumann had dropped out just prior to the ninth conference; their responses would have been interesting. In the ensuing discussion Bateson entered into the spirit of the cyberneticians and suggested that one might construct a cybernetic circuit that would oscillate in response to certain types of contradictions. This idea, a machine with a sense of humor, was immediately taken up by others. Bateson also noted one important role of humor in the relation of people to one another: "It gives the persons an indirect clue to what sort of view of life they share or might share"; and went on to examine the pattern of interaction in such a process.

Although the discussion, especially comments by Walter Pitts, showed Bateson's theory of humor to be only incompletely worked out, he was eager to describe his basic thesis, which goes beyond an analysis of humor:

> I should like to present to you the notion that these paradoxes are the stuff of human communication. As scientists, we try very hard to keep our levels of abstraction straight; for instance, in the conferences we have gotten into very great trouble when the levels of abstraction became tangled and the theory of types showed itself. In ordinary life, as distinct from scientific talk, we continually accept implicit paradoxes. . . . The freedom to talk nonsense, the freedom to entertain illogical alternatives, the freedom to ignore the theory of types, is probably essential for comfortable human relations. . . . I am arguing that there is an important ingredient common to comfortable human relations, humor and psychotherapeutic change, and that this ingredient is the implicit presence and acceptance of the paradoxes. . . . The alternative to the freedoms introduced by paradox is the rigidity of logic.[52]

Bateson later insisted that an awareness of the many levels of reality and a sensitive appreciation of the contradictions and paradoxes they create is crucial to making wise interpersonal, social, and political choices. In this connection he liked the analogy to the task of backing up a truck with several trailers attached, and the difficulties encountered when one turns and the successive trailers move in contrary direction.[53]

Bateson strongly criticized some applications of von Neumann's game theory by visualizing it on a different conceptual level than Game Theory itself.[54] It ignores the quintessentially

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human and leaves no room for learning, in particular, disallowing deutero-learning. Savage's decision theory depends on practically the same "utility" concept as game theory and is open to the same criticism. Further, the premises of game theory propagate changes that Bateson suspected "are in a paranoidal direction and odious" (the latter comment would apply to Game Theory but not to decision theory). Wiener seconded Bateson in using the theory of types as a metaphor to criticize applications of game theory: you can win the battle and lose the war; and you can win the war and lose everything that is dear to you.[55] McCulloch also disagreed with the utility concept of Savage and von Neumann. In a little confrontation at the eighth meeting between him and Savage, McCulloch simply characterized the utility concept as "illusory," because he regarded "value" as multidimensional, "with very little chance of its being simplified to a measure." Savage's decision theory and Bateson's approach to good choices may be viewed as orthogonal to each other: Savage worked out the techniques in rigorous detail on one level, whereas Bateson emphasized the necessity of considering many levels simultaneously; but in Bateson's formulation the rigor and clarity of Savage's model are missing and the techniques for arriving at good decisions are not fully spelled out. Bateson could view the Savage theory from the outside, in the context of a particular kind of decision to be made, and ask whether the utilitarian and quantitative assumptions were suitable for the choice in question. Bateson's disagreement with the use of statistical decision theory as a generally applicable approach is reflected in his later insistence on "the obsolescence of the anti-aesthetic assumption . . . that all phenomena (including the mental) can and shall be studied and *evaluated* in quantitative terms."[56]

Bateson's most explicit discussion of "making choices" is found in a 1968 paper presented to a Wenner-Gren Conference, where he discusses different categories of learning.[57] Learning I serves to correct errors of choice within a set of alternatives, whereas Learning II, which is synonymous with "the acquisition of world views," results in corrective change in the set of alternatives from which a choice is made. The phenomenon of Learning II, according to Bateson, involves changes in which the "stream of action and experience is segmented or punctuated into contexts together with changes in the context

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markers." For example, certain meditations or psychotherapies may result in a corrective of a person's habitual inability to entertain seriously certain choices and thus increase the options available to her or him.[58] This is an example of Learning II. In political decisions as well, everything depends on how the various options are framed. Thus the decisive battle between the Democratic and Republican candidates for office is typically over which of them succeeds in defining what the issues are. "The outcome of the game of politics depends on which of a multitude of possible conflicts gains the dominant position," although the unaware voter usually just chooses sides in a conflict that has been decided on by others.[59]

Savage's prescription for making choices encourages and essentially corresponds to Learning I. It does not concern itself with higher-order learning, even though such learning could turn all evaluations on their heads. Although in his later philosophical reflections Savage sought to identify some problematic issues concerning his theories, he was not even close to coming to terms with Bateson's concerns.[60] Nevertheless it is noteworthy that Savage did wonder how to understand a person's feeling of compulsion to "correct" his or her own circular preferences when confronted with them. He also wondered how to incorporate into theory, or if it is a source of paradox how one is to judge, the worthwhileness of expenditure of a given amount of money to make an "optimum" economic decision.

At the Macy conferences the distinction between a probability space with fixed options and the alternative consideration of the possibility of enlarging the space of possible occurrences had arisen as a source of controversy in another connection, namely, with two different definitions of "information," one favored by the American engineer Claude Shannon, a frequent guest at the meetings, and the other championed by a visitor from the British Isles, Donald MacKay. Both were at the eighth conference, and in the lively debate that followed MacKay's presentation Savage was an active participant.[61] (Bateson did not attend that conference.) Shannon's concept of "information" arose in connection with communication engineering and the problem of specifying the channel capacity required to transmit "information."[62] From the engineering point of view it is irrelevant whether the messages have any meaning whatsoever. Heinz von Förster's criticism applies, that the an-

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thropomorphic notion of "information" is misplaced here; nevertheless, it had become the accepted word in communication engineering. The great merit of Shannon's and also Wiener's equivalent definition of information is that it lent itself to proving a number of rigorous mathematical theorems. MacKay also took into consideration the new information obtained in a scientific experiment, which might not be included in the space of possibilities envisioned prior to the experiment, and required an expansion of that space. But beyond this, he was interested in expanding the concept of information to encompass "meaning." In any case MacKay's concept of information included Shannon's as a special case, which he called the "selective-information-content." His broader definition included as well what he termed "structural-information-content" (reflecting the number of independent features that are to be specified) and "metrical-information-content" (reflecting the number of units of evidence required for the description).[63] MacKay's later philosophical reflections on probability, meaning, mechanism, and information are given a special character by the circumstance of his religious piety and the explicit effort to reconcile it with the Bible and his Calvinistic faith.[64]

Bateson's use of the notion of logical types is an artifact. He used the concepts of formal logic to show that logic is not suitable, yet he was able to get considerable mileage out of that unsuitability. By implication, ordinary or literary language seems more suited to questions of wise choice than does the language of logic, and he might have come to that positive conclusion. He emphasized that wisdom consists of knowing the larger interactive system in which exponential changes can be initiated when choices are made. He was specifically aware of the complexity of the consequences of actions: short term and long term, for an individual as well as for the larger ecological system, cultural as well as psychological and biological, circularly and linearly causal. He had considerable success reconciling detailed scientific research with efforts at comprehensive, holistic understanding of himself and the world, an "ecology of mind"—which in his terms meant encompassing the tradition of Heraclitus and William Blake as well as of Newton and Darwin.

Bateson's view was the traditional one that wisdom resides in a (person's) mind. He was not so concerned with managerial

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decision making, for his was not the world of politics or business. He disliked power over people and manipulation of people, and his focus was on knowledge and understanding. Only when he thought that the powers-that-be were engendering ecological disasters did he seek to influence their awareness of the dangers. For Savage, on the other hand, wisdom resides in a technique, and he provided us with a prescription for good decision, which anyone who knows precisely what he or she wants can use.

In our complex world it is a great relief that learned men are willing to tell us practically how we can make purportedly good decisions. Interpreters of the I Ching have served a similar function in China and astronomer/astrologers did so in earlier centuries in the West. Mathematical decision theory has found resonance in business schools, the business sector of the economy, and in public administration, as well as with social scientists concerned with good design of experiments. It is well suited to bureaucracies because in spite of "personal probability" it can reflect the objectives and judgments of an administrative entity and depersonalize and mechanize the decision process. The wisdom of diffusing responsibility, often desired in organizations and readily achieved by a relatively mechanical decision process, is open to question from the perspective of the larger society. For policy and military decisions the issue has been treated elsewhere in connection with game theory.[65] A facet of mechanizing medical diagnostic decisions (and similarly choices about cures), is that "as the physician makes greater use of the technology of diagnosis, he perceives his patient more and more indirectly through a screen of machines and specialists; he also relinquishes his control over more and more of the diagnostic process. These circumstances tend to estrange him from his patient and from his own judgment."[66]

Bateson spent his life exploring new territory. He asked the questions that seem to matter most, eschewing the limitations imposed by convention and academic institutions. It is not surprising that he increasingly focused not on administration but on teaching, and that in those years—especially the 1960s—when he was outside of the mainstream (even outside of his former "social science tribe"), young people found he had something to say to them. One of them wrote the preface to

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Steps to an Ecology of Mind, and characterized Bateson's work thus:

> I believe that this is a very important book . . . especially for those of my generation born since Hiroshima—who are searching for a better understanding of themselves and their world. . . . 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. . . . This book is a sample of the best thinking I have 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.[67]

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The Cybernetics Group

By: Steve Joshua Heims

Citation:
The Cybernetics Group
By: Steve Joshua Heims
DOI: 10.7551/mitpress/2260.001.0001
ISBN (electronic): 9780262362757
Publisher: The MIT Press
Published: 1991

The open access edition of this book was made possible by generous funding and support from Arcadia – a charitable fund of Lisbet Rausing and Peter Baldwin

The MIT Press

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This book was set in Baskerville by Compset, Inc. and was printed and bound in the United States of America.

Library of Congress Cataloging-in-Publication Data

Heims, Steve J.
The cybernetics group / Steve Joshua Heims.
p. cm.
Includes bibliographical references and index.
ISBN 0-262-08200-4
1. Social sciences—Research—United States. 2. Cybernetics—United States. 3. Science—Social aspects—United States.
4. Josiah Macy, Jr. Foundation. I. Title.
H62.5.U5H45 1991
003'.5—dc20 91-409
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