FINDING MIND, FORM, ORGANISM, AND PERSON IN A REDUCTIONIST AGE: THE CHALLENGE OF GREGORY BATESON AND C. H. WADDINGTON TO BIOLOGICAL AND ANTHROPOLOGICAL ORTHODOXY, 1924–1980

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CHAPTER EIGHT

NOT THING BUT PROCESS, 1945–1980

I am also beginning to see that the formal problems of schizophrenia are much closer than I had thought to the problem of natural selection and the problems of choice of strategy….

—Gregory Bateson to C. H. Waddington (1958)¹

Not thing but process I now see is sacred.
“That with which” (says Bateson’s ghost) “thou shalt not tinker.”
—Noel G. Charlton (2008)²

Like Waddington, Gregory Bateson often sensed that he was cutting against the philosophical grain. While Waddington opposed a growing neo-Darwinian consensus increasingly harnessed to “beanbag” genetic determinism, Bateson contrasted with two distinct trends. As a descendant of the 1920s organicist tradition—he made frequent references to Whitehead throughout his career, as we will see—Bateson also wanted to avoid anything resembling reductionism, atomism, or even “individualism” as it was expressed by social scientists in the 1960s and ’70s. Yet, as we learned earlier, many anthropologists studied culture as a distinct feature of the human landscape irreducible to biology. In order to escape the race-based biological determinism of the past,

¹ GB to CHW, 30 June 1958, UEL-CHW, MS 3059.8.

anthropologists emphatically declared “culture makes us, not biology.” Bateson disagreed. Biology, too, was part of the process of human existence. Discovering the “pattern that connects” these features of the world to each other, however, would be both difficult and of overwhelming importance to those that purported to study humans. Anthropologists, sociologists, psychologists and other social scientists should proceed with caution.

As in the prior discussion about Waddington, this chapter details Bateson’s attempts to carve a middle way in the study of *Homo sapiens* between genetic reductionism on the one hand and implicitly abiological “culturism,” on the other. Bateson’s reception on this count was somewhat different than Waddington’s. Though neither Bateson nor Waddington persuaded most evolutionary biologists—the group both hoped to impact—regarding the validity of their organismic approach, Bateson at least attracted groups of followers in California as well as isolated thinkers elsewhere in the US and Europe. Many of these were interested in alternative lifestyles and/or the New Age movement. Perhaps unsurprisingly, this fact about his followership muted the impact of Bateson’s contributions to the study of evolution in human society.

Bateson had long been interested in structuralist or systems approaches, as evidenced by his ethnography *Naven*. But during World War II and for years afterward, he and Mead began enunciated their ideas employing a kind of discourse more familiar to engineers and computer scientists. That language was cybernetics. For Bateson, cybernetics reaffirmed the Whitehead-inspired organicism he and Waddington pursued in the 1920s and ’30s.
8.1 Under the Influence of Cybernetics: Gregory Bateson’s Second Iteration

Catherine Beatrice Durham Bateson, mother of Gregory and wife of William, died at the beginning of the War. Gregory did not have any more institutional security at Cambridge (or anywhere else) than Waddington. But the Bateson financial legacy was not small; Gregory could afford to be idle after leaving the OSS in 1945. He taught anthropology courses at the New School and at Harvard over 1946 and ’47, residing with Mead and their young daughter and occasionally with the Franks when not in Boston.3 But the relationship with Mead became strained; she thought he was not working hard enough, that he spent too much time toying around with ideas, mainly surrounding cybernetics, and not enough time doing the harder job of putting his ideas in print. Bateson moved out within a year of returning to the States, and the two were divorced by 1950.4

At Harvard, he met a young engineer, John Weakland, whom Bateson believed could help him apply a greater degree of mathematical rigor to his musings about cybernetics in biology and anthropology. Weakland proved instrumental to Bateson’s work on schizophrenia in the 1950s. And Alfred Kroeber, a visiting professor in the Peabody Museum, who became Mayr’s token anthropologist at the 1959 Darwin Centenary, occupied the office next to Bateson. When Bateson was not rehired at Harvard, Kroeber made a call to a colleague in California, Jurgen Ruesch at the Langley

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1 So fluid was their living situation in the post-war years, that friends like the Waddingtons sent Christmas cards to “Mr. and Mrs. [sic] Gregory Bateson” at the American Museum of Natural History rather than to any residence (UCSC-GB MS 98; Box 36, Folder #1447).

4 Lipset, Gregory Bateson, pp. 174–75.
Porter Neuropsychiatric Clinic in San Francisco. Though Bateson was completely untrained in psychology, Ruesch agreed to hire him as a lecturer in medical anthropology. His specialization, as of 1947, would be defined as “psychiatric communication.” In that new area, Bateson would apply all of his observational skills along with the new terminology and methodology of cybernetics.

Cybernetics emerged as a distinct approach to mathematical and engineering problems from the Second World War work of Norbert Wiener and Julian Bigelow on self-correcting anti-aircraft targeting systems. Historians pinpoint the May 1942 Macy Foundation’s conference on Cerebral Inhibition as the moment when cybernetics became a distinct approach to scientific, engineering, and, eventually, social problems. Physiologist Arturo Rosenbluth delivered the most stimulating presentation at the meeting focusing on circular causality. Though the rest of the meeting largely dealt with “hypnosis,” the joint Rosenbluth, Wiener, and Bigelow paper created considerable stir. At lunch following the paper, the anthropologists present could think of nothing else. Later published in Philosophy of Science as “Behavior, Purpose, and Teleology,” this

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6 Lipset, Gregory Bateson, p. 178.


9 Lipset, Gregory Bateson, p 179. Mead later exclaimed that the 1942 meeting was “so exciting I didn’t notice I had broken one of my teeth until the conference was over” (Margaret Mead, “Cybernetics of Cybernetics,” in Purposive Systems: Proceedings of the First Annual Symposium of the American Society for Cybernetics, ed. Heinz Von Foerster, et al. (New York: Spartan Books, 1968), p. 1).
cross-disciplinary paper effectively launched cybernetics as a research program of its own in 1943.¹⁰

Though largely of interest to mathematicians, engineers, and new “scientists” in the field of computing, Mead, Bateson, and their mutual friend psychologist Larry Frank, played an integral role in the growth and direction of cybernetics in its earliest years, with Frank serving as the link to Macy Foundation funding.¹¹ After the War, the founding group of like-minded thinkers—Frank, Bateson, Mead, Rosenblueth, Wiener, and Bigelow—continued to meet with an expanded “core” of enthusiasts including: mathematicians John von Neumann and Walter Pitts, neurobiologists Warren McCulloch and W. Ross Ashby, and Viennese engineer Heinz von Foerster. Though he was not in a settled academic position in the States, Bateson provided the impetus for the well-known series of Macy-funded cybernetics meetings that commenced in March 1946 in New York City’s Beekman Hotel.¹² The commitment to broad social applications of cybernetics that he shared with Mead and Frank colored the spirit of the first few proceedings. This interest in the anthropological side of cybernetics was especially pronounced in some of the major works to come out of the 1940s meetings: Frank, et.


¹² Bateson, upon returning to the US, telephoned Frank Fremont-Smith, another “Macy-man,” about restarting the conferences. Fremont-Smith mentioned that McCulloch had also been pressing for the resumption of formal meetings. McCulloch had co-authored a crucial paper for the origins of twentieth century cybernetics with Warren Pitts (“A Logical Calculus of the Ideas Immanent in Nervous Activity,” Bulletin of Mathematical Biology 5, no. 4 (1943): 115-33) and hoped to be able to discuss it with the core group of intellectuals interested in cybernetics. Stuart A. Umpleby, “A History of the Cybernetics Movement in the United States” (Paper presented at the Proceedings of The WOSC 13th International Congress of Cybernetics and Systems), 2005.)
al.'s “Teleological Mechanisms,” Shannon and Weaver’s *Mathematical Theory of Communication*, and especially Wiener’s *Cybernetics*.\(^{13}\)

Much like Waddington’s experience with the TBC, Bateson found a degree of personal and intellectual interaction with this community that he had not experienced since his undergraduate days at Cambridge. Aside from working alongside Mead, at the first meeting in the spring of 1946, Bateson reconnected with old Cambridge friend and Biological Tea Club member, now Yale ecologist, G. Evelyn Hutchinson.\(^{14}\) Perhaps unsurprisingly, enjoying “[m]embership” with von Neumann, Wiener, and McCulloch, he later classified among “the great events in my life.”\(^{15}\) So profound was the impact of the Macy cybernetics collective on Bateson that he actively participated in the New York meetings into the 1950s despite working in San Francisco.

Problems emerged even in this decidedly transdisciplinary collective, however. With Heinz von Foerster at the helm after 1948, the group began to divide into three more specialized camps.\(^{16}\) Interested in Allen Turing’s universal computing device, John von Neumann turned one facet of the Macy group toward the development of artificial

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\(^{14}\) Lipset, *Gregory Bateson*, pp. 103–4; Bateson recorded a few of his impressions about these generally congenial conversations in his 1949 “Notebook 10[A]” UCSC-GB MS 98, Box 69.

\(^{15}\) Bateson as quoted by Brockman, “Introduction,” in *About Bateson*, p. 10.

\(^{16}\) With the agreement of the rest of the group, von Foerster formalized the Macy conferences as “Cybernetics: Circular Causal and Feedback Mechanisms in Biological and Social Systems” just after the publication of Wiener’s *Cybernetics*. Umpleby, “History of the Cybernetics Movement.”
intelligence based on his game theory. Bateson found this development discouraging.\textsuperscript{17} With the encouragement of Wiener, von Foerster turned another subset toward toward the networking involved in electrical systems. As a legacy of this move, the Institute of Electrical and Electronics Engineers currently includes a “Systems, Man, and Cybernetics Society,” despite the fact that Wiener himself began to resist the social science approach to cybernetics as early as 1949:

Drs. Gregory Bateson and Margaret Mead have urged me, in view of the present age of confusion, to devote a large part of my energies to the discussion of this side of cybernetics…. Much as I sympathize with their sense of urgency of the situation…. I can neither share their feeling that this field has the first claim on my attention, nor their hopefulness that sufficient progress can be registered in this direction….\textsuperscript{18}

The remaining cybernetics subgroup included McCulloch, Mead, and Bateson, among others.\textsuperscript{19} Another sign of the deteriorating coherence of the Macy collective came in 1951, when core members von Neumann and Wiener dropped out of the meetings entirely. Though the numbers of “members” and “guests” steadily increased over the course of the 1950s, including the addition of cybernetics proselytizer W. Ross Ashby, the character of the original postwar core dissipated into disciplinary specialties.\textsuperscript{20} The new

\textsuperscript{17} Though he continued to count von Neumann among his friends, Bateson spoke out in his adopted ethologist role, strenuously objecting to the oversimplifications involved in game theory as applied to biology, let alone human behavior. Gregory Bateson, “Bali: the Value System of a Steady State,” in \textit{Steps}, pp. 107–27, esp. 121–24. (This article was originally published in the \textit{Festschrift} for Radcliffe-Brown in 1949.)


\textsuperscript{19} Umpleby, “History of the Cybernetics Movement.”

blood that launched cybernetics in its second major phase included a collection of lawyers and a CIA agent among the usual cadre of scientists.\textsuperscript{21}

Nevertheless, Bateson wove cybernetics deep into his worldview by the 1950s. When he revised \textit{Naven} in 1958, he veritably scolded himself for not being more of a cybernetic thinker in the ’30s—this despite the fact that he was already thinking in terms of feedback loops and schismogenesis at that time:

If Sheldon’s typology had been available to me in 1935, I would have used it in preference to that of Kretschmer, but I would still have been wrong. As I see it today, these typologies, whether in cultural anthropology or in psychiatry, are at best heuristic fallacies, \textit{cul de sac}, whose only usefulness is to demonstrate the need for a fresh start.\textsuperscript{22}

In Bateson’s mind, the real problem in the social sciences was an epistemological one, rather than one that could be resolved merely with better empirical content or a different kind of “typology.” In the first edition of \textit{Naven}, Bateson had rejected ruthlessly any hint of teleology or purpose. Purposiveness was, of course, the fly in the ointment of Malinowski’s functionalism that disgusted a younger Bateson. How could a society or culture be “for” anything like the “direct satisfaction of human needs,” “moulding and training of human beings,” or “the maintenance of the status quo,” etc.?\textsuperscript{21} So, to avoid “function,” Bateson (and Mead) made limited use of behavioral “typologies,” intrinsic properties held by the cultural group. Inculcated in the cybernetic world with its much

\textsuperscript{21} The group that eventually founded the still-extant American Society for Cybernetics (ASC) included Paul Henshaw of the Atomic Energy Commission, Carl Hammer, a UNIVAC computer scientist, Jack Ford, who worked for the CIA, Douglas Knight of IBM, and attorneys Walter Munster and Bill Moore. Once the ASC had formalized, the received a grant from the National Science Foundation to establish the \textit{Journal of Cybernetics}. Umpleby, “History of the Cybernetics Movement.”

\textsuperscript{22} Bateson, \textit{Naven} [1958], p. 285.

\textsuperscript{21} Bateson, \textit{Naven}, [1936], p. 28.
more robust notion of feedback, mid-1950s Bateson could still reject the Malinowskians’ sloppy holism with its indiscriminate use of function in favor of a crisper notion of looping causation, information, and teleology from the inside, rather than dredging up personality typologies. “[I]mmament” rather than “transcendent” purposiveness, argued Bateson from his cybernetic point of view, could avoid typologies altogether. It was a Whiteheadian point; a nod toward the lesson of “misplaced concreteness” in scientific terminology and reinforcement of the idea that time was significant in development, including both cultural and biological development. He would have gleaned such ideas from Waddington, Needham, and Woodger in the 1930s.

Convinced by the Macy conferences that the scope of cybernetic explanations would prove to be more or less without limit, Bateson began his work in San Francisco in the spirit of a cybernetic “ethnologist,” actually taking that title in 1949 when he moved from Ruesch’s Langley Porter Clinic to the Veterans Administration Hospital at Palo Alto. At the VA Hospital, and with a new wife and child to keep his spirits high at home, he felt no ties, and no indebtedness, to any particular discipline. He read and

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25 Recall also the discussion of these issues in Chapter Three.

26 Bateson, “Foreword, 1971” to Steps, p. xx. The working relationship between Bateson and Ruesch had proved to be a tumultuous one. Already in 1947, Bateson feared Ruesch was trying to undermine his work. In a protracted, hastily scrawled draft of a letter that may not have been delivered, Bateson admitted that he found it “very difficult, almost impossible” to trust people, but he remained adamant that this distrust this did not, at least in this particular case, have anything to do with money or notoriety—both of which had been points of tension between them. In fact, Bateson asserted, all of Ruesch’s perceived attempts to “discredit [his] work” would ultimately prove futile: “If, as I do, you regard intellectual endeavor as a form of suicide—then the other questions [about reputation] do not arise. But that’s right—you do not have intellectual integrity in that sense. Just an ordinary Joe—well, well, well” (Nov. 1947, Notebook 2, UCSC-GB MS 98, Box 69).
freely quoted both biologists and psychologists in his work on human psychology—Waddington’s *Strategy of the Genes*, especially, appeared in the bibliographies of more than a few of his articles as well as in his 1958 edition of *Naven*.27 He did not abandon anthropology entirely, as Mead implied.28 Rather, unfettered, Bateson fastened on to problems that seemed to transcend boundaries erected not only around disciplines but also around species. He read philosophy again, especially Fichte, Whitehead and Russell, as well as old Bateson family favorites like William Blake and Samuel Butler. Though at a psychiatric hospital studying mentally ill patients, especially schizophrenics, Bateson pursued theories of communication and learning in a general sense. He fully intended to put paid to the promises of cybernetics as a technique applicable “horizontally” (*i.e.*, across broad categories ranging from species to traditional academic disciplines).29 His

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27 See, for instance, Bateson’s “Minimal Requirements for a Theory of Schizophrenia,” in *Steps*, pp. 244–70.

28 Mead accused Bateson of abandoning “conventional anthropological fieldwork after the war” (*Blackberry Winter*, p. 219). But this is only the case if fieldwork requires in principle the anthropologist to be working with a foreign ethnic group. I would argue that mental patients were, indeed, a population able to be assessed from an ethnographer’s point of view, as Bateson himself argued. However, against my suggestion that Bateson continued anthropology, albeit not in a foreign land, stands Waddington’s description of Bateson as “the most original sociologist this country [the UK] has produced...” (my emphasis, CHW to Brumwell, 1 June 1945, UEL-CHW MS 3024.5). Waddington’s statement fits with my own if we recall that, while in the US all of anthropology had allied under the strong arm of Franz Boas, in the UK social anthropology remained (and continues to remain) distinct—with closer ties to sociology and psychology than biology, archaeology, history, etc.

Another clue that Bateson saw himself as doing social anthropology under a different guise appears in his Notebook 11 (undated, but probably late 1950). Here he wrote:

> It is natural to draw and analogy between anthropology and psychology on this matter or to suggest that the need for a comparative approach in anthropology is comparable to the need for therapy for another human being (the therapist) different from the self, against whom as a background, the peculiarity of the self can be seen.

This paragraph followed an entry he titled “The Self-Observer” with references toward self-diagnosis of dreams for ethnographers in line with the suggestions of W. H. R. Rivers. UCSC-GB MS 98, Box 70.

29 Recall Medawar’s definition of “horizontal” science, above.
early 1950s theory of “deutero-learning” became one of Bateson’s chief horizontal applications.

Deutero-learning, or learning to learn, did not originate with Bateson any more than cybernetics. If anything, he imbibed this notion of meta-learning in conversations with Larry Frank even before WWII. However, over the late 1940s, Bateson added deutero-learning to the psychological work of another Macy conference participant, father of social psychology Kurt Lewin, and stirred these concepts together with cybernetic tools of analysis. At Langley-Porter, Bateson, together with Jurgen Ruesch, crafted an elegant theory of dual-level communication. Bateson applied this model of communication first to a theory of play. Then, beginning in 1952, Bateson and his new colleagues at the Palo Alto VA Hospital began developing a cybernetic model of schizophrenia. In 1954, Bateson successfully applied to the Rockefeller Foundation for a grant to assemble a multi-disciplinary team to study schizophrenia as a mental disease arising from errors in inter-personal communication between parent and child. His collaboration with Jay Haley and John Weakland on the one hand and Don D. Jackson

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In his application for the Guggenheim Memorial Foundation grant 1945/6, “Contributions to Psycho-cultural Theory,” Bateson placed Lewin among his major influences, including: Ruth Benedict, A. R. Radcliffe-Brown, Erik Erikson, Larry Frank, and Margaret Mead. LC-SPE/MM Box O5, Folder 6.


on the other, attacking the seemingly insuperable problems presented by schizophrenic patients, led to the kind of high-profile professional recognition for Bateson that he simultaneously cherished and loathed.\(^{34}\) As he perhaps feared in the 1950s, “Toward a Theory of Schizophrenia” initiated a semi-public process of translation of his cybernetic approach to psychology that paradoxically led toward the marginalization of the global evolutionary theories he propounded in the 1960s and ’70s.\(^{35}\)

Like his investigation of play behavior entitled “A Theory of Play and Fantasy,” his article “Toward a Theory of Schizophrenia” began with the (by now so antiquated it seemed novel) philosophy of Russell and Whitehead introduced in Principia

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\(^{34}\) Bateson shared the 1961 Frieda Fromm-Reichmann Award from the American Academy of Psychoanalysis with Don Jackson for their work on schizophrenia (Lipset, Gregory Bateson, pp. 231–2). But, as we will see in more detail below, this resulted neither in eponymy for a Bateson theory or technique nor recognition for his contributions as a systems-thinking evolutionist—the field in which he wished to receive recognition. His fame came in small packages from family therapy, psychiatry, communications theory, and, much later, the West-coast New Age community.

\(^{35}\) Joel Cullin, “Double Bind: Much More Than Just a Step ‘Toward a Theory of Schizophrenia’,” Australian & New Zealand Journal of Family Therapy 27, no. 3 (2006): 135–42. There is a fairly pronounced contrast between Bateson’s approach to schizophrenia and the work of others in that field in the 1950s. Though it is not fitting to pronounce this a contrast between holistic and reductionistic thinking, the Bateson, Haley, Jackson, and Weakland Palo Alto group focused on disruptions in networks of relationships. At the same time many psychiatrists were attempting to simulate schizophrenic episodes in themselves and mitigate them in their patients by taking psychedelic drugs, especially psilocybin and LSD-25.

Originally extracted from rye ergot by Swiss chemist Albert Hofmann in 1938, his twenty-fifth lysergic acid diethylamide derivative became in the early Cold War a manufactured compound being researched by the CIA as a weapon of mind control in their MK-Ultra project. LSD-25 began being prescribed as an antidote to every mental disease including schizophrenia and alcoholism. Harold Abramson, a CIA agent known to Gregory Bateson, introduced him to the psychedelic in the late 1950s. Bateson offered it to acquaintances like beat-poet Allen Ginsberg. Ginsberg’s subsequent history with the drug is well-known. Bateson, however, did not receive the same “inspiration” from the drug that others claimed to have. While other doctors extolled the use of LSD on themselves and others, Bateson continued working on formal theories of organic-psychological structure. See, Leigh A. Henderson, and William J. Glass, LSD: Still With Us After All These Years (San Francisco, CA: Jossey-Bass Inc, 1994); J. R. Neill, “More Than Medical Significance: LSD and American Psychiatry 1953 to 1966,” Journal of Psychoactive Drugs 19, no. 1 (1987): 39–45; Jay Stevens, Storming Heaven: LSD and the American Dream (New York: Grove Press, 1998); and Martin A. Lee, and Bruce Shlain, Acid Dreams: The Complete Social History of LSD: the CIA, the Sixties, and Beyond, Revised ed. (New York: Grove Press, 1992), p. 58.
Schizophrenia, surmised Bateson, emerged from a pathological spiral triggered when a mother communicating with her child breached the formal—deeply psychologically, perhaps even biologically, wired—pattern of communication related to the Theory of Logical Types (TLT) introduced in the *Principia Mathematica*. According to the TLT, a class (e.g., of objects) cannot be a member of itself; nor can a member be the class. Though Bateson had asserted that sometimes in human communication, usually in “play,” the TLT could be violated, in pathological communication leading to schizophrenia, the child learns (from its mother) to mis-reference signals, specifically “signals of that class whose members assign Logical Types to other signals.” A truly schizophrenic individual must, according to Bateson *et. al.*, “live in a universe where the sequences of events are such that his unconventional communicational habits [i.e., those that violate the TLT] will be in some sense appropriate.” Because these dysfunctional signals are repeated so frequently in a household setting, a particularly unsatisfactory and permanent kind of psychological twisting occurs, which creates “inner conflicts of Logical Typing” and “unresolvable sequences of experiences.” Bateson and his co-authors deemed this the “double bind.”

To forge a double bind, five conditions need to be met. Multiple individuals must be involved, one of which is clearly the “victim” of others; and the negative, victimizing experience must be repeated. But aside from these fairly obvious factors, Bateson

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introduced three additional ones directly influenced by his work in cybernetics. A primary injunction is given to the child, either (a) “Do not do so and so, or I will punish you” or (b) “If you do not do so and so, I will punish you.” This first-order injunction is followed by a more abstract, second-level command that conflicts with the first and is often expressed non-verbally. If this second-level command is a verbal one, it is often delivered as “Do not see this as punishment,” “Do not see me as the punisher,” “Do not submit to my prohibitions,” etc. Thirdly, another class of threat is offered: a prohibition against leaving the relationship, often extended as a “love” bond. In other words, a complex contradiction is set up by the stronger party with the weaker unable to cut through the contradiction or leave the situation.

Though vastly more sophisticated, and with a seemingly unrelated set of data (e.g., mental patients rather than South Pacific island aborigines) the double bind thesis differed little from Bateson’s schismogenesis theory in Naven. But Bateson attributed the double bind analysis to an exchange with Norbert Wiener in 1954. Just writing to Wiener made it possible for Bateson “to think those thoughts on that day,” he claimed. The act of translating deuto-learning in the context of schizophrenia in such a way that it would fit into Wiener’s more thoroughly systematized discourse enabled Bateson to see that the mental illness was not the absence or possession of a single input, biological or otherwise. Rather the schizophrenic faltered in their interpretation of “triads of signals” constituting the “nature of metaphor”:

a. Those signals which are parts of inner physiological disturbance which happen to be externally perceptible—blushes, tears, and the like.
b. Dramatized simulations of (a), e.g. in play, etc.

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38 GB to Norbert Wiener, 23 Nov 1959, UCSC-GB MS 98, Box 37, Folder 1496.
c. Signals which indicate whether a given signal is an (a) or a (b). The message 'this is play' is thus a signal of the third type.\(^9\) Schizophrenics, according to Bateson, lack “c”—“this is play”—the signal that completes the contextualization of either of the other two possible signals. The loss of this psychological mechanism for judging meta-signals arises due to the repeated violation of logical consistency between the first- and second-order injunctions, often because the schizophrenic’s “intense and vitally important” relationship (Bateson usually blamed the mother) offered negative injunctions at both levels.\(^40\) Bateson claimed that it was because he was addressing Wiener rather than one of his newer psychiatry colleagues that he made this connection explicit.

\(^9\) GB to Norbert Wiener, April 1954 (photocopy), UCSC-GB MS 98, Box 37, Folder 1496. In his application for the Rockefeller Foundation grant that actually funded the Palo Alto group’s research, Bateson repeated the cybernetic structure of schizophrenia he had written to Wiener, including even the example of the mother-son interaction reported in “Group Dynamics of Schizophrenia” (see following note):

Wherever such multiplicity of levels occurs in communication systems, paradox will be generated if both message and metamessage contain negatives. It follows that paradox should be looked for in those combinations of learning and deuterol-learning where both the learning and the learning to learn are, in some sense, negative. These conditions would obtain when an organism is subjected to both (a) learning situation in which punishment to follow some act (or failure to act) and (b) the organism is punished for acting upon his expectations of punishment. In human terms, this is the experience of the individual who is punished for cringing, or of the child of a perverse mother (in Rosen’s sense) who is penalized by an apparent withdrawal of (non-existent) love when he exhibits those behaviors which are the appropriate response to her lack of love. Imaginative introspection shows such experience to be exceedingly painful [April 1954, Notebook 21B, UCSC-GB MS 98, Box 71].

\(^40\) G. Bateson, “The Group Dynamics of Schizophrenia,” in *Steps*, pp. 228–43. The tragic illustrations from their clinical data included the following:

A young man who had fairly well recovered from an acute schizophrenic episode was visited in the hospital by his mother. He was glad to see her and impulsively put his arm around her shoulders, whereupon she stiffened. He withdrew his arm and she asked, "Don’t you love me any more?" He then blushed, and she said, "Dear, you must not be so easily embarrassed and afraid of your feelings." The patient was able to stay with her only a few minutes more and following her departure he assaulted an aide and was put in the tubs…. The impossible dilemma thus becomes: "If I am to keep my tie to mother, I must not show her that I love her, but if I do not show her that I love her, then I will lose her" [“Toward a Theory of Schizophrenia,” in *Steps*, p. 217–18].
Though generous, his attribution of these ideas to Wiener could be only partially true. Bateson’s interests in multi-level patterns originated at least two decades earlier. These foundational interests began to resurface even in the midst of the successful psychiatric work. Over the course of the 1950s, as the Palo Alto VA Hospital group developed more sophisticated methods of observation, Haley and Jackson concomitantly grew more interested in family therapeutics, seeking to resolve the problems they witnessed in families with schizophrenic members. Bateson, however, remained aloof. As he later admitted, “While I have [personally] cared for several schizophrenic patients, I have never been intellectually interested in them. The same is true of my work with native peoples in New Guinea and Bali. Always my intellectual focus has been on general principles which were illustrated or exemplified in the data.”\footnote{Contrast Bateson’s self-assessment in 1962 with the later judgment of colleague Jay Haley: “Gregory used to bust his ass for patients, in terms of making himself available. He would see them all the time…. He spent hours with them, walking around…. Gregory really extended himself. He’d stay up all night with alcoholics, to get them through…. He felt that being human with people was good for them” (Lipset, \textit{Gregory Bateson}, p 215).} By the end of the decade, “the data” inspired Bateson to begin making conjectures radical both in their breadth and in their similarity to the organic philosophy he had embraced in the 1920s and ’30s.

Initially, he presented his speculations about the anthropological and even biological significance of his double bind explanation to groups of professional therapists. Problems with communication appeared immediately, however. In his 1959 Lasker Memorial Lecture delivered to the Institute for Psychosomatic and Psychiatric Research and Training at Michael Reese Hospital in Chicago, Bateson broached a panoply of topics unfamiliar to his audience. These included: (a) Samuel Butler’s nineteenth century
critique of Darwinian evolution in *Erewhon*, (b) the ontological distinctions between the mechanistic Newtonian worldview and what Bateson called the context-dependent Berkeleyanism implicit in anthropological linguistics, (c) a brief discourse on the definition of randomness and probability, and (d) a historical review of the scientific problems with Lamarck’s (and Darwin’s) theory about the inheritance of acquired characteristics. Yet these dense issues provided only the backdrop for the centerpiece of his lecture—an application of Waddington’s work on genetic assimilation to psychology.

Bateson drew from Waddington’s experiments on the morphology of *Drosophila*. Aside from his use of *Limnea* and the practical livestock breeding experiments with which he was associated as director of Edinburgh’s Animal Genetics Unit, Waddington had conducted most of his experiments on the development of morphology after WWII by genetically manipulating the diminutive fly and photographing the results under a scanning electron microscope. In his 1959 lecture, Bateson cited two of Waddington’s *Drosophila*-related publications, “Genetic Assimilation of an Acquired Character” and “The Integration of Gene-Controlled Processes and Its Bearing on Evolution.” In these experiments, Waddington successfully triggered a mutation in the *bithorax* gene of the

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42 Bateson, “Minimal Requirements for a Theory of Schizophrenia,” in *Steps*, pp. 244–70.

43 This fact is somewhat ironic given Waddington’s complaint to Needham years earlier: “I am really quite anxious to get back to organiser work. I have been enjoying working with Drosophila, but very little is going to come out of it, at least of the kind I wanted. The beast is just too difficult to do any real morphogenetic work on” (CHW to Needham, 11 Dec. 1938, CUL-JN M.94). Some example’s of Waddington’s *Drosophila* work at this time—demonstrating just how difficult it was to do the work he wanted on “the beast”—include, C. H. Waddington, “The Interactions of Some Morphogenetic Genes in *Drosophila melanogaster*,” *Journal of Genetics* 51, no. 2 (1953): 243–58; and C. H. Waddington, and Elizabeth M. Deuchar, “Studies on the Mechanism of Meristic Segmentation: I. The Dimensions of Somites,” *Journal of Embryology and Experimental Morphology* 1, no. 4 (1953): 349–56.

developing fly using ethylated ether. When subjected to the ether, the balancing organs (halteres) on the fly’s third segment grew into functional wings. As he suggested in *Organisers and Genes*, the four-winged mutation could become canalized after being artificially selected for several generations; even without the ether, some selected *Drosophila* would continue to manifest four wings. Even more surprising, these results occasionally occurred in specimens that did not possess the specific *bithorax* gene—the complex of genomic factors produced the mutation, rather than a specific gene.\(^5\)

Bateson viewed the genetic assimilation project as one specific type of a more abstract—perhaps even biologically universal—feature about “learning.” If learning could be defined according to cybernetic principles, then perhaps we could regard any organism as a unified multi-level system with a built-in capacity to learn. On the upper (somatic) level, change would happen through trial and error. Though ensuring flexibility to meet environmental demands, this process would entail a lot of waste. At a lower level, “habits” might form following somatic change in a similarly stochastic process of genetic recombination. In the economics of evolution, “habituation” by making a trait the genetic default would be more efficient than the stochastic, trial-and-error process at the somatic level. Efficiency requires its own sacrifices, however; to habituate, to make deterministic by making an adaptation genetic, the organism must

\(^5\) In his experiments on *crossveinless* flies, Waddington interpreted this not simply as evidence for multiple genomic inputs into a phenotype, but as support for arguing against the connection of what occurred in the genotype under environmental stress with the adaptive need of the individual organism (Waddington, “Genetic Assimilation”). Neither veins in *Drosophila* wings nor even an extra set of wings seemed to confer any direct benefit to the fly. His anti-functional argument put Waddington’s interpretation of his genetic assimilation work (and by extension Bateson) in conflict with the interpretation of Ernst Mayr. Mayr incorrectly read Waddington’s genetic assimilation as supporting the “Darwinization” of formerly neo-Lamarckian territory (See, Amundson, *Changing Role of the Embryo*, pp. 210–11).
sacrifice a great deal of its flexibility to respond to ever-varying environmental challenges.

Crucially, Waddington based his explanation of the process of genetic assimilation on the notion that what genes or groups of genes confer are tendencies, thresholds, potentialities and the like—not individual traits. Bateson, likewise, reminded his audience that schizophrenia was not a “genetic disease,” at least in the traditional sense. No genes operating by themselves gave anyone mental illness. Thus psychiatrists would not be justified in assuming the best way to identify, let alone treat, schizophrenia was as a biologically programmed disease discoverable through “genealogies upon which we discriminate some individuals who have been committed to hospitals, and others who have not.” The genetic component of schizophrenia could only be due to “constellations of genes” that, because development was multi-level process requiring a host of internal and external inputs, “altere[d] patterns and potentialities in the learning process.” These patterns effectuated “overt” schizophrenia only when the developing schizophrenic was “confronted by appropriate forms of environmental stress.”

With Waddington’s pinball-machine-like epigenetic landscape as the operating metaphor, schizophrenia became a potential “canal” for the unfortunate child subjected to the inter-level communicational/relational contradictions inherent in the double bind. If knocked out of his/her normal developmental channel by family stressors, the child’s phenotype would unroll as overt schizophrenia.

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47 In his 1958 “Notebook 28” (UCSC-GB MS 98, Box 71), Bateson attempted to formulate his theory of schizophrenia in formal Russell-Whitehead logic. These initial propositions were quite simple and formulaic, but they quickly became more complex until reaching the dense treatise presented in Chicago in 1959. He titled the entry “Genotype for Schizophrenia.” Though he initially considered the
Disenchantment with the lack of philosophical and even biological rigor of psychiatry became a central theme of Bateson’s last year or two at the Palo Alto VA Hospital. When an opportunity to work with psychoanalyst John Rosen on the East Coast arose in 1958, Bateson wrote Waddington:

[A] research project is being set up for some months for me to study with movies the psychiatric techniques of one Dr. Rosen. … How do you tell a machine to program itself? … If we could get together in Philadelphia for a few months, I think we could push things along quite fast. I am also beginning to see that the formal problems of schizophrenia are much closer than I had thought to the problem of natural selection and the problems of choice of strategy, and the ‘assignment problem’, etc.\(^8\)

But Waddington felt he was at the top of his career and could not pull himself away from Edinburgh. He tried, in fact, to get Bateson to join him in Scotland. But neither man was both willing and able to make such drastic moves. Waddington instead attempted to lure Bateson to Mead’s residence in New York City, where he would be

\(\text{influences of “Rosen, F[remont]-Smith, Army, Cato, Bartless,” he settled on “Wad [underlined several times]” as the major source. Much of the entry was written in explicitly Waddingtonian terms:}
\)
\(\text{I. That both genetic make-up and environmental experience play a part in the aetiology of schizophrenic symptoms. \(S = f(G\text{[enetics]}, E\text{[nvironment]}).\)}
\(\text{Either } a, G \text{ or } E \text{ are multiplicatively related, and } S \text{ therefore approaches zero as either } G \text{ or } E \text{ is reduced.}
\)
\(\text{…or } b. E \text{ is itself a function of } G. \text{ This would mean if you have a high } G, \text{ you will seek out or create those experiences which are the appropriate environmental components of schizophrenic aetiology…. Or } C. \text{ Both } a. \text{ and } b \text{ may be true. The assumptions are not necessarily mutually exclusive and it is indeed probable that [ends abruptly]}
\)
\(\text{… } C. \text{ In the case in which the relationship between } E \text{ and } G \text{ is multiplication, we might say that } G \text{ is a measure of innate sensitivity and this factor can be made definable by suggesting that it be an analogue of gain in electronic systems. In the schizophrenic phenotype, such a factor would be superseded by the ratio Response/Stimulus for certain sorts of Stimulus or Response; this ratio being larger in the more potentially schizophrenic.}
\)
\(\text{d. Another possibility must also be considered: that } G \text{ is of the nature of a threshold or tolerance. In this case, the relationship between both } E \text{ and } G \text{ in our equation will be subtractive. } S \text{ will only appear in the phenotype when the expression } (E-G) \text{ is has positive value, i.e. when } E \text{ is greater than } G.
\)

\(\text{\(8\) GB to CHW, 30 June 1958, UEL-CHW MS 3059.8.}\)
staying between a research project at the Rockefeller Institute and a visit to China. ⁴⁹ No record of a New York meeting exists, however, and the Rosen videotaping project never materialized.

Bateson sought other opportunities on the East Coast. Bemoaning his “lack of sparring partner,” he wrote to J. Robert Oppenheimer in 1959, hoping for a three-year term at the Princeton Institute for Advanced Study. Oppenheimer seemed interested in Bateson’s work, but did not regard Princeton as any more collaborative or any more able to offer the kind of philosophical dialogue Bateson sought than the Palo Alto VA Hospital group. Bateson thanked Oppenheimer, but remained concerned to pursue his work in an interdisciplinary environment. ⁵⁰ This kind of rigorous interaction seemed especially necessary as he embarked on an even more ambitious project than his work on schizophrenia. His plan for the 1960s was to complete a project his father had initiated in the 1890s. But in Gregory’s formulation, this would be even more ambitious: a general theory of communication and behavior that transgressed the gulf between humans and other animals.

8.2 “Any Difference Which Makes a Difference,” 1960–70

Six years after he applied to the Rockefeller for his grant to study schizophrenia in Palo Alto with Haley, Weakland, and Jackson, Bateson applied to the National

⁴⁹ CHW to GB, 19 July 1958, UEL-CHW MS 3059.8.

⁵⁰ Lipset, Gregory Bateson, p. 232.
Science Foundation to study non-verbal interactions between octopi. At one of their meetings, he requested permission from his colleagues to set up octopus tanks in the morgue of the hospital. They were appropriately bewildered. What light could the study of marine invertebrates shine on human mental disease? But Bateson confidently alleged that the multi-level patterns of behavior and communication so distorted in schizophrenics were universal patterns. “Octopus was deliberately chosen,” wrote Bateson in an unpublished manuscript, “as both complex and maximally different from the human species.... It was hoped that from the study of such an animal...it might be possible to add to those generalizations which can be regarded as universally true....”

After a time, the tanks and their eight-legged occupants made their way to Bateson’s own home outside of Palo Alto. But this development served to exacerbate the feelings of distance between him and the other members of the Palo Alto group; he spent an inordinate amount of time observing the creatures and simply trying to keep them from perishing in the artificial environment.

Aside from the octopus work, Bateson gave his colleagues other reasons to suspect his commitment to schizophrenia research. In a 1960 psychiatry lecture, Gregory encouraged social scientists to attend to the theories of William Bateson, work that was virtually ignored when introduced to biologists seventy years earlier:

My father was a geneticist, and he used to say “It’s all vibrations,” and to illustrate this he would point out that the striping of the common zebra is

51 J. Z. Young introduced Bateson to the study of octopus as an interesting communicating contrast class to humans at a late Macy Foundation cybernetics conference. Lipset, Gregory Bateson, p. 233.

52 Lipset, Gregory Bateson, p. 233.

53 Lipset, Gregory Bateson, p. 233–34.
an octave higher than that of Grevy’s zebra…. [H]e was trying to say that it is all a matter of the sort of modifications which could be expected among systems whose determinants are not a matter of physics in the crude sense, but a matter of messages and modulated systems of messages.\textsuperscript{54}

And Gregory began citing the work of D’Arcy Thompson on transformations in the coordinated body structures of vertebrates, another archaic and likely irrelevant reference in the opinions of many psychiatrists.\textsuperscript{55} Clearly by 1960, Bateson was attempting to revitalize the study of merism—patternedness in organisms—that had been at the center of his investigations with his father that resulted in the 1926 partridge paper.

Now, however, the vocabulary felt more precise; Gregory believed that by including the cybernetic worldview and Waddington’s genetic assimilation concept, he was on the verge of restarting William Bateson’s crucial investigations that had been abandoned in light of two factors intrinsic to the Modern Synthesis: (1) the idealizing assumptions necessary for mathematical population genetics to explain evolution (recall Mayr’s “beanbag genetics” accusation made only a year earlier) and, (2) the strong anti-Lamarckian streak in the Modern Synthesis that sought to discount nearly all environmental influences on inheritance.\textsuperscript{56} Bateson wrestled with both heads of this


\textsuperscript{56} William Bateson had no truck with neo-Lamarckianism, as popular accounts such as Koestler’s \textit{Case of the Midwife Toad} would adequately show (see above, this chapter). However, population geneticists such as R. A. Fisher and evolutionary biologists like Julian Huxley and Ernst Mayr had relegated William to “anti-Darwinian” status so many years earlier that it is unsurprising that his even less conventional ideas were entirely misunderstood or ignored (Peterson, \textit{Balanoglossus to Materials}, pp. 294–98). In reexamining questions that had plagued his father years before, Gregory Bateson entered territory almost completely abandoned by biologists.
hydra simultaneously in the paper “The Role of Somatic Change in Evolution,” published in the main professional journal of neo-Darwinism, *Evolution*. In this paper, Bateson purported to account for the specific bio-economics of a revised evolutionary theory that was more inclusive than the Modern Synthesis. The problem with the orthodox Synthetic version of evolution, according to Bateson, was that it relied too heavily upon “mutation or genotypic reshuffling.” In an interwoven, multi-level system like an organism or population, this method would “inevitably use up the available somatic flexibility.” In other words, the more “efficient” a population of organisms became—by reallocating the source of adaptation from the somatic/phenotypic level to the germ/genotypic level—the less able it would be to adjust to new demands. If the Bateson-qua-cybernetician critique of orthodox evolutionary biology was correct, short term evolutionary fitness obtained solely through the stochastic process of DNA mutation could only lead to long-term brittleness of the whole system. Under no circumstances would such a simplistic, genome-centric system lead to the kind of meta-level complexity attained by, for instance, language-and-tool-using humans.

Instead, Bateson posited a more sophisticated, organismic version of evolution, which he explained in the following way. Animals could be roughly divided into two categories, based on the degree to which they held internal variables constant in relation to a related set of external conditions. “Adjusters” vary with conditions; “regulators” hold to a constant. For example, in the case of thermal regulation, “adjusters” like insects

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58 Again Bateson cited Waddington here, both his genetic assimilation paper and *Strategy of the Genes* (1957).
are deeply affected by the temperature of the air while “regulators” like mammals maintain a dynamic equilibrium. Bateson pointed out that, in terms of body plan and the organization of the intricate organic networks, this placed the systems of thermal regulation very close to the points of environmental contact in the regulators; adjusters “cope” with environmental shifts by involving “deeper loops of the total network.”

Humans pushed the regulator category one step further as “extraregulators” capable of altering their environments directly. And in the sense that regulators and extraregulators constitute the greatest proportion of organisms usually considered “higher” on a scale of evolutionary complexity, Bateson surmised that, however “romantic” it appeared, “centrifugal control”—the ability to respond to environmental stressors ever further away from the central processes of living—must be one of the favored trends in evolution.59

But why should this evolutionary trend favoring some invisible centrifugal force pushing control toward the somatic and beyond be the case? Wasn’t somatic change less biologically efficient? Why did evolution seem to be measurable in terms of levels of Logical Types, like Whitehead and Russell postulated for mathematical systems? What

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59 G. Bateson, “Role of Somatic Change,” pp. 537–8. Bateson’s “romantic” hypothesis might appear too far out of the mainstream for serious consideration in an era when many biologists were attempting to explain evolution without reference to final causes. Yet this was a perfectly ordinary claim in the 1950s–60s. In his review of Waddington’s Principles of Embryology (1956), Julian Huxley protested that Waddington did not devote enough attention to teleology: [T]hough Waddington has done so much to link epigenetics with genetics, he has not proceeded to make the further link with evolution. This in my view can only be done by recognizing the pseudo-teleological, or as I would prefer to say, the telic character of all evolutionary processes…. [T]hanks to the operation of natural selection, such processes can only be adequately specified in terms of final states or ends as well as of origins or beginnings, of biological utility or function as well as of underlying mechanism or mode of operation. Thus, though analysis of epigenetic process in terms of physico-chemical factors is essential, it will never be sufficient, and knowledge of origin needs to be supplemented by knowledge of final fate…. [J. Huxley, Nature 177, no. 4514 (5 May 1956): 807–8].
did the organismic deployment of multi-level regulation have to do with the “vibrations” William Bateson believed explained the living world in the 1890s? And how might all of these systems and patterns connect in such a way that they would make a difference to humans? Gregory pondered these issues through the 1960s, even as he moved from studying schizophrenics and octopi in California to observing dolphins in the Virgin Islands.

Though the octopus observation had not produced anything substantial, Bateson was very excited about the potential pay-off of dolphin work. As he told the director of the Communication Research Institute on St. Thomas where he would be working, John C. Lilly, he believed dolphins were “a little nearer to the evolution of language than human beings” and, assuming this, “language still carries for them more of the function of bridging misunderstanding than it does for us.” Yet Bateson had almost no idea how to work with dolphins. The leap from ethnologist and communications theorist to administrator of an animal training facility on a relatively destitute Caribbean Island proved to be enormous. After just over a year the facility closed for lack of funds.

A year later, Bateson and his new family—social worker and animal lover Lois Cammack married Bateson in 1961—traveled to another dolphin facility in Hawaii. The Sea Life Park in Waimanalo, Hawaii, contained a research division, the Oceanic Institute, which began operation as a training and performance facility in 1963. Marine biologists Taylor and Karen Pryor provided the Batesons with an amiable facility from which to conduct their research for the better part of seven years. But little changed in

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60 GB to John C. Lilly, 9 Sept. 1963, UCSC-GB MS 98, Box 20, Folder 898.

Bateson’s approach from his years at the Palo Alto VA Hospital. Though he did produce some articles specifically dealing with cetacean communication, Bateson continued to be preoccupied with abstract issues—almost to the point of obliviousness to his surroundings. Occasionally, the incongruity between Bateson’s cerebral ruminations and the practicalities of training wild animals led to humorous encounters like the following:

Karen [Pryor]. (carrying a bucket of fish) Good morning, Gregory.

Gregory. (He is a huge elderly man [Bateson was 60 at the time] in old trousers, a faded shirt, and the world’s oldest sneakers. He stoops, squints and smiles with an air of surprise at running into a friend) Good morning, Karen.

K. (sets down bucket)

G. You know, I’ve been thinking….

K. (attentive and silent).

G. If you had been born with two left hands on your left arm, would both of them be left hands? Or would one of them be a right hand?

K. (after a pause for consideration of a brand-new puzzle) I don’t know.

G. Hmm…. (nods, smiles and ambles on).62

The answer to Gregory’s bizarre query appeared in William Bateson’s tome *Materials for the Study of Variation*, published in 1894.63 As the elder Bateson reported, a second hand emerging on a left arm would mostly likely replicate a right hand—a mirror image of the normal hand on that arm. Already by 1964, Gregory had moved in his private thoughts from vague references to William’s “vibratory hypothesis” and complaints about the oversimplifications in the Modern Synthesis to a detailed “Typology of

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63 W. Bateson, *Materials for the Study of Variation*. 
Genetics” that he feared would upset many biologists. This attack on twentieth century population genetics featured a “confrontation” between the version of evolution espoused by Samuel Butler and that of Charles Darwin. As Bateson began to realize, the problems with orthodox evolutionary theory were not limited to the biology itself but also involved the worldview the biology was predicated upon. This worldview, which Bateson sometimes called “Newtonian” or “Galilean,” other times “reductivist,” came complete with its own historical narrative that began with founding father, Darwin.

These musings began to take the form published as “A Re-Examination of [William] Bateson’s Rule” in 1968. Out of “filial piety,” Gregory attempted to publish it in his father’s venerable Journal of Genetics, which was run at the time by William’s now aged successor at the John Innes Institute and champion of “beanbag genetics,” J.B.S. Haldane. In his paper on this set of unusual phenomena, Gregory Bateson offered a cybernetic account of some bizarre whole-limb mutations in beetles and frogs. William Bateson had originally published these abberant cases in his Materials for the Study of Variation in 1894. Gregory hoped not so much to redeem his father’s reputation—the younger Bateson did not even acknowledge the low regard that neo-Darwinians such as Ernst Mayr had of William’s work—as to demonstrate the universal applicability of cybernetics to difficult problems in biology. For Gregory, the treatment of information unlocked mysteries in biology and linked together the life, social, and physical sciences.

64 Notebook 31 (Summer 1964), UCSC-GB MS 98, Box 71.

65 After an extended delay, Waddington encouraged Bateson to pull the article and send it to another journal. “JBS took the J. Genet. with him to India, and turned it over to one of Helen’s boy friends…. If you ever want this paper to see the light of day (which it should) I would withdraw it from them…” (CHW to GB, 13 Oct. 1971, UCSC-GB MS 98, Box 36, Folder 1447). The Journal of Genetics did eventually publish the article, but not until after Bateson’s 1971 collection of essays Steps to an Ecology of Mind had been published.
Gregory’s cybernetic solution centered on his definition of information as “any difference which makes a difference in some later event.”66 In his reexamination of William Bateson’s abberant beetles, superfluously petaled flowers, and mutant frogs, Gregory stressed that when an extra appendage erupts out of an existing appendage, that it always resembles the appendage on the other side of the body: a reduplicated left arm looks precisely like the normal right arm.67 Gregory read this “rule” as an example of a lack of information regulation. Typically, a limb would be imbued with information determining its position within the organismal complex along three axes—proximal-distal, ventral-dorsal, and anterior-posterior. Counterintuitively, the information describing each axis was not inherited separately (as might be expected in the standard neo-Darwinian model) but as a whole set. Because limb orientation seemed to be an interconnected system within the larger system of the organism, the limb mutations evident in William’s beetles suggested that information restricting the growth of an additional was lost, not gained. The resulting extra limb developed whole cloth as a mirror of the normal limb it developed alongside—according to the full complement of dimensional information. For Gregory, this developmental pattern indicated that the developing beetle limb lost regulating information saying, in effect, don’t build a complete limb here. When the extra limb was constructed, it followed a fundamental developmental pattern in the organism—a “rule”—that “every reduction in symmetry


67 In more complex organisms, frogs, for instance, the reduplicated limb is also reduced in size.
(from radial to bilateral or from bilateral to asymmetrical) requires additional information.”

Gregory induced, just as William Bateson had almost a full century earlier, that these teratological cases were mutant exceptions that proved an important, though often unseen, rule about normal creatures. As we saw earlier in this dissertation, the elder Bateson regarded important evolutionary change as taking place discontinuously—by leaps, some called them. The creation of whole additional limbs as mirror images of normal existing limbs was strong support for the notion that evolution was not gradual or constant, according to William. Gregory did not make a claim quite this strong. After reexamining his father’s rule, the younger Bateson made a point about the connection between development and evolution that fit well with Waddington’s contemporary work:

We know little about how the pathways of evolutionary change are influenced by such morphogenetic and physiological redundancies. But certainly such internal redundancies must impose nonrandom characters upon the phenomena of variation.69

Evolution, in other words, would not be the smooth “random walk” defended by the Modern Synthesis. Only certain paths would be available for the evolving organism. Though he did not reference Waddington in the article, “Bateson’s rule” seemed to mesh quite nicely with the epigenetic landscape.70


70 Gregory Bateson, and William by extension, had uncovered a developmental process that would remain mysterious until the twenty-first century. See, Carroll, Endless Forms, pp. 140–65.
At roughly the same time that another major interest of Gregory’s returned to the fore—the application of abstract concepts to practical social problems. These two threads, one biological, the other anthropological and, eventually ecological, dominated the rest of Bateson’s life. Twenty years earlier, the Macy Foundation cybernetic conferences provided Bateson with the clarity of thought necessary to address problems in human communication. Over the next decade, these cybernetic concepts gave him the tools to decode the relational dynamics of schizophrenia. But the dolphin work in the Virgin Islands and Hawaii had placed Bateson in a kind of intellectual void, away from the evolutionary theorists—the “sparring partners”—he wanted. In 1967, Bateson traveled to several major European cities to discuss “consciousness vs. nature” in an attempt to reclaim the kind of camaraderie he had lost in the years since the meetings with Mead, Wiener, McCulloch, and the others in his cybernetic network.\(^1\) Newly invigorated, upon his return he began to assemble ideas for a transdisciplinary conference. He drafted a theoretical position piece, called which “Effects of Conscious Purpose on Human Adaptation.” It functioned as the call-for-participants to a 1968 conference funded by the Wenner-Gren Foundation. The book *Our Own Metaphor*, an expressive account of the conference proceedings by Bateson’s daughter, Mary Catherine, came out of this conference.

For Bateson, the results of the 1968 through ’70 Wenner-Gren conferences were decidedly mixed. On the one hand, he again found an energetic, incipient core of individuals with a similar attraction to anti-reductionism in biology. On the other, this group largely concurred with him on the wrong sorts of issues. Rather than helping him

\(^1\) Notebook 39 (1967), UCSC-GB MS 98, Box 72.
draft a third way between the natural sciences and the humanities—an organic structuralism of communication and thought—this group tended toward post-structural cultural theory in literature. The praise that Bateson received in increasing quantities through the late 1960s and into the 1970s came from individuals like Tony Wilden who initially proposed to write a book on Lacan and deconstructivism making use of Bateson’s ideas. Bateson initially agreed with Wilden’s use of his theories. Consequently, the eventual book, System and Structure, contained a great deal of material that Bateson personally shaped. But it was published as a work in communication studies rather than in biology or philosophy. By the early 1970s, Bateson had not been marginalized by competing interests within a specialized field of research as had Waddington. Rather Bateson had been misinterpreted by his supporters in a new area. The community that accepted him was on the humanities side of a “Two Cultures” chasm. Bateson had been trying to bridge this chasm by various means since 1926. But the side of the chasm from which he wanted to begin the building project, the natural sciences, barely knew of his existence.

72 Contrast, for instance, the presenters list at the 1977 Wenner-Gren conference on: “Cultural Frames and Reflections: Ritual, Drama, and Spectacle” with the earlier conferences populated by mathematicians, psychologists, and computer scientists). Participants in 1977 included: Barbara Babcock (English), Paul Bouissac (French), Roberto Da Matta (anthropology), Natalie Zemon Davis (history), Bruce Kapferer (anthropology), Hilda Kuper (anthropology), John MacAlmon (social thought), Barbara Myerhoff (anthropology), Harold Rosenberg (fine arts), Wole Soyinka (literature), Beverly Stoelje (intercultural studies), and Victor Turner (social thought). UCSC-GB MS 98, Box 36, Folder 1483.

73 This initial exchange between Bateson and Wilden took place from October 8–21, 1969. Wilden eventually came to Hawaii so Bateson could help Wilden grasp his ideas. UCSC-GB MS 98, Box 37, Folder 1497.

Batesonianism was intended to be a universal evolutionary theory at least as true to the biological data as Darwinism. But half a century after William Bateson ascended to the heights of the Anglo-American biological community with his forceful defense of Mendelism, few recalled “Bateson’s Rule,” even after Gregory Bateson made a re-examination of it. The inner circle of formative Modern Synthesis evolutionary biologists—Mayr, Dobzhansky, Stebbins, Simpson, and the others—made no mention of this article or the analysis of somatic change that appeared in *Evolution*. Following the triumphs in molecular biology, from Watson and Crick to Jacob and Monod, discussions about whole systems seemed increasingly irrelevant to genetics. By the 1970s, even Mayr’s attack on “beanbag genetics” seemed mere shibboleth. Batesonianism, such as it was, remained an antiquated proposition in Victorian biology rather than an important amendment to, as Waddington called it, post-neo-Darwinism.\footnote{Waddington, “The Evolutionary Process,” in *Evolution of an Evolutionist*, p. 205 (originally published in Population Biology and Evolution (1968)). Waddington also called his “paradigm,” simply—and perhaps more controversially—“post-Darwinism.” C. H. Waddington, “Paradigm for an Evolutionary Process,” in *Towards a Theoretical Biology—2. Sketches*, ed. C. H. Waddington, I.U.B.S. Symposia (Chicago: Aldine Publishing Company, 1969).}

Yet in the human sciences, where post-modernisms of all stripes were beginning to gain momentum by the late-1960s, the Batesonian quest to understand patterns and the meta-patterns that connect patterns made an impression. Double bind caught on, even if the study of merism in morphology did not. In this way, Bateson became something of an icon.
8.3 Popular Sequestration: Batesonianism Misunderstood

Two strange things happened on Bateson’s way toward becoming canonized by post-modernism, however. First of all, in the social sciences community, where for a half century Bateson believed he had been introducing a kind of hard-nosed philosophical and biological rigor based on the works of Whitehead, Russell, and Waddington, among others, he had instead become something of a Zen mystic. We will investigate the impact of this turnabout in the final chapter.

Bateson regarded a second development as more deeply troubling. Even while he promoted the understanding of a multi-level organicism connecting genome to soma to environment in evolutionary change, the applied social sciences began to abandon both unsophisticated forms of Skinnerian behaviorism and the notion of a super-organic culture. This trend would seem to be exactly in line with Bateson’s own predilections. However, social scientists abandoned these other perspectives in favor of a reification of the individual. Though not genetic or deterministic, Bateson nonetheless read the highlighting of the individual over all other levels of evolutionary change as “reductionistic.” Individualism of this sort undermined the very systems approach that Bateson had been advocating since the 1940s. Strangely enough, this creep toward a widespread popular acceptance of an unreflective, philosophically and scientifically naïve, and potentially reductionist individualism confronted Bateson in the oddest place imaginable: on a sweltering gymnasium floor in front of rolling tape recorders.
8.3.1 Carl Rogers and Psychological Individualism

By the 1970s, many regarded Carl Rogers (1902–1987) as the most influential living American psychologist. His client-centered approach to psychotherapy spread throughout the Anglo-American counseling world. At the day of his death, February 4, 1987, the Nobel Committee had just nominated Rogers to receive its Peace Prize. In his over two hundred publications, he stressed an anti-authoritarian relationship between client (versus “patient”) and therapist (versus “doctor”). Occasionally, Rogers would use his own stature to create a forum for public conversations with other significant figures in what he called the “helping professions” of theology, psychiatry, and the like. In the past, these dialogues had included luminaries like Paul Tillich and B. F. Skinner; given the reputation of both Rogers and his guests at these conversations, they were often recorded and broadcast.

In his dialogue with Bateson in May 1975, Rogers opened the program with a prepared ten-minute speech, stressing his interest in what was at that time a new area for him, education. Applying his anti-authoritarian approach, Rogers strongly critiqued American education for its “jug-and-mug” philosophy: “The instructor is the jug and pours knowledge into the passive receptacle...which is the student.” Instead he advocated “person-centered” education wherein “the direction is self-chosen, the learning is self-


initiated, and the whole person with feelings and passions, as well as intellect, is invested in the process.” Bateson followed with a ten-minute address of his own in which he emphasized the overriding importance of contextual communication with his memorable quip: “I can teach the comparative anatomy of the beetles in a way which will make little Hitlers out of you all, or I can teach the comparative anatomy of beetles in a way which will make you all into, what shall we say, dancers or artists… even perhaps democratic citizens.” This pair of monologues laid the foundation for an exchange which proved to be profoundly disappointing for both participants.

The terminological and stylistic distinctions between Rogers and Bateson, and there were many, ultimately resided in a privately defined concept of organism. Superficially, both agreed: organism meant a biological entity. At a root level, however, Rogers used words like organism and organic as synonyms with his “whole person” idea:

To me, the person who offers the most hope in our crazy world today, which could be wiping itself out, is the individual who is most fully aware—most fully aware of what is going on within himself: physiologically, feeling-wise, his thoughts; also aware of the external world that is impinging on him.

Rogers, that is, wanted a kind of internal alignment between thoughts and feelings in the closed box of the individual; the environment “impinged,” but could only be dealt with in a reactive sort of way by adjusting affectations, emotions, etc. inside the atomic person. Bateson, by contrast, meant by organism not an emoting person per se, but, as Woodger speculated long ago, an open box of interlocking, hierarchical set of systems organizing

79 Kirschenbaum and Henderson, Dialogues, pp. 180–1.
80 Kirschenbaum and Henderson, Dialogues, p. 182.
81 Kirschenbaum and Henderson, Dialogues, p. 188.
smaller systems and operating within still larger systems. Batesonianism meant a
Whiteheadian ecological view of the organism. From the Batesonian perspective of
organism, “feelings” mean something much less fuzzy, if seemingly more abstract, than
in Rogers’ concept:

If, as participants in that dance [of context-understanding], you suddenly
find you have put your foot through the floor, this is where feelings are.
This is where, if you offer respect and respect isn’t the thing that was
asked for: ‘Ugh.’ If you offer amusement and amusement isn’t it, if you
are serious and you feel I am mocking you: ‘Ugh.’ Always at this point,
the moment you are not at ease in the context, then you get unpleasant
feelings. And the change towards ease within the context is a very large
part of pleasantness of feelings. 82

As Bateson fondly told his students at the University of California, Santa Cruz, where he
began teaching in 1972, the difference in pattern made the difference in feelings and
associated meanings. Conflict between setting and content created ‘ugh’ feelings.
Without the cybernetically integrated and nested sets of communication structures—
some verbal, some biological, some purely physio-chemical—the individual, if it could
exist at all, could have no feelings to make whole through Rogers’ psychotherapy.

Rogers and Bateson certainly felt tense after the exchange. Some of the thousand-
plus member audience crammed into the Marin College gymnasium to hear the
exchange could sense the frustration crackling in the air. 83 With the audience crowding
around, the hot buzzing of klieg lights overhead, and the discomfort caused by his
inadequate meal beforehand, Bateson had been feeling peevish. Combined with the
dialogue itself, Bateson felt the night was, in a word, “dreadful.” He wrote to Rogers a

82 Kirschenbaum and Henderson, Dialogues, p. 188.

83 Kenneth Cisna, and Rob Anderson, “A Failed Dialogue? Revisiting the 1975 Meeting of
week later to restate his position. He felt Rogers and the larger psychological community had misunderstood him; this misunderstanding was

rooted and re-rooted (meta-rooted) in an unacceptable 17th century model—a materialism wedded to a supernaturalism—a worship of the ‘captain’ who is the captain of somebody’s ‘soul,’ which in turn (intermittently) inhabits somebody’s mechanical ‘body,’ to make, by putting the pieces together, a sacred cow called a person. Or, even more sacred, a ‘whole person.’

Carl, you keep saying that the whole persons matter. No, no, no. In that gathering of fifteen hundred persons, only the ideas mattered, and the ideas included such propositions as ‘it is a hot night’, ‘only the ideas matter’, ‘only the persons matter’, and so on, because the monstrous thing is that in a world where only the ideas matter, contrary ideas can exist and even incorrect ideas. And the incorrect ideas will commonly kill the people, leading them to kill each other, or to poison the water which they drink. But it is an idea that this killing (or life-giving) matters. Do not say that this is “only” an idea. What greater thing could it be? It is, after all, in another epistemology, a part of ‘god’, or “God”. Now, forsooth, you want a theory for humanistic psychology. Up to this point I have not been writing about psychology but about epistemology—the science of how there can be any psychology. And there can be psychology because in the discourse of organisms there can be error on the subject of epistemology. The study of these errors is correctly called ‘psychology’. God rest the arrogant souls of those who will engage in it.

Rogers concurred that the May night had been “dreadful.” But he insisted that if Bateson had willingly shared his feelings about the dreadfulness, then Bateson would have approached Rogers as a “whole person.” As it was, they never “met,” they only “shadow box[ed].”

Upon receiving that conciliatory but not acquiescent letter, Bateson must have felt that he still had not been heard properly, that the message and meta-message had not

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84 GB to Carl Rogers and Rollo May, 11 June 1975, UCSC-GB MS 98 Box 21, Folder 923.

85 Richard Farson, the director of the Humanistic Psychology Institute of San Francisco and the moderator of the dialogue, sent to Bateson a copy of Rogers’ reply agreeing on the “dreadful[ness]” of the exchange. Bateson may have lost (or may never have received) the original from Rogers. Farson to GB, 15 July 1975, UCSC-GB MS 98 Box 21, Folder 923.
aligned, and he scribbled a six-point note. The meta-message of this note said, in effect,

“I am trying to straighten out your sloppy thinking”:

1. There are many sorts of ‘ideas’…. the problem is how to establish some sort of classification of these sorts.

2. As I understand it, ‘feelings’ are members of a class within (1) above….

3. You call ‘feelings’ also ‘personal meanings’ and that is a helpful limitation, a step toward definition….

4. But we get into difficulties with both ‘meanings’ and ‘personal.’

4a. ‘Meanings’ must surely always have their being between at least two ideas, so that we can say that ‘A means B.’

4b. ‘Personal,’ we know, means something different for you from what it means for me. For me, ‘person’ is that ‘nexus in a floating web of ideas which exist within my skin and outside it’ [quoting Rogers’ previous letter]. For you, I guess that ‘person’ is all contained within. (In parenthesis, I strongly recommend my version of ‘person’ for use if and when we face the very difficult formal problems of aesthetics. And don’t forget that other use of ‘person’ as mask—the view of me which I show to others on the outside.)

5. This matter of ‘location’ of ideas, feelings, and whatnot is very awkward for both you and me. Where is the ‘pain’ of my big toe? Or where is the (very terrible) pain of a phantom limb? I think we can agree to have no localization of anything (ideas, feelings, meanings, etc.) within the person or self (in both your and my usage of person). The world of ‘person’ is non-spatial….

6. So now we come to the relationship implicitly mentioned in your phrase ‘personal meanings’ [abrupt stop]86

Even without the completion of this letter, and even though the correspondence seems to fall apart after this last salvo, little doubt remains regarding the debate regarding Bateson’s position versus the celebrated variety of psychology espoused by Rogers.

Though Bateson scorned the false contextlessness of behaviorism, he likewise

disapproved of Rogers’ muddy individualism. Despite the fact that both behaviorism and individualism predominated in psychology at that time, Bateson insisted that neither approach took ecology seriously. By ecology, he called up that old organicist theme: ontologically at least, everything is connected; divisions between phenomena being analyzed were regulative, not constitutive. The organic world was constructed of interlocking systems, rather than isolable entities.

In a sense, Bateson had been making the same point for his entire career. From the first edition of Naven onward, he stressed complex levels of interaction that, when they ran in opposite directions from one another, instigated conflict. If it in fact occurred, this conflict might be “mitigated” by convoluted transexual tribal ceremonies (naven) or erupt as mental illness (schizophrenia). But whatever the outcome, the conflict occurred not to a discrete thing but within a concrescence, as Waddington would have said—an epistemologically, but not ontologically, distinct biological arrangement.

8.4 Conclusion

During their dialogue, Bateson offhandedly dubbed Rogers one of the “action people.” He admitted that he never quite understood the action people: “they have a good time and they improve the world, I guess,” Bateson mused. “And, you know,” he said with a shrug, “that’s fine.” He, however, preferred to be thought of as a “theorist”:

[ Rogers] starts, you see, in the first two minutes, by saying there’s good and evil in the world and he knows which is which…. I believe there is good and evil in the world. As to which they are, that’s difficult.87

87 Kirschenbaum and Henderson, Dialogues, p. 182.
The action people acted too quickly and too decisively without truly understanding their position, Bateson implied. Over the course of the evening encounter with Rogers, and in the few years before his death in 1980, he found it harder to conceal his scorn for those that, without untangling their message from their context, without understanding the apparently universal patternedness in the world, and without questioning the concreteness of their concepts, proceeded to “fix” people, organizations, definitions, and economies. By the look of things in the 1970s, however, theorists of Bateson’s ilk would not inherit the earth.

Waddington, of course, had clashed with action people as well. By the mid-1970s, Francis Crick was one of those. So was Peter Medawar. Biology, like the social sciences, seemed to be full of action people. As geneticist Raymond Pearl reminded J. H. Woodger decades earlier,

Most working biologists, at any rate in America, do not like to think and look with a very fishy eye on anything which savors of philosophy. Just now with us anything or anybody not overtly worshipping with adulation at the shrine of genes and ‘crossing over’ is considered low and wholly lacking in intelligence. But the history of science indicates that it has always been so, hence I suppose there is little that can be done about it.\(^{88}\)

Though Pearl no doubt overstated his case, his point was well taken by Waddington and Bateson as well as Woodger: in the pragmatic, increasingly Americanized biology scene of the mid-twentieth century, productivity spoke louder than precision. Productivity occurred most easily when one inculcated oneself in a recognized scientific area where

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\(^{88}\) Raymond Pearl to Woodger, 1 Aug. 1929 (emphasis in original), UCL-JHW C1/2, Folder “P”.

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problems might be easily defined. Neither Bateson nor Waddington particularly cared for disciplinary and subdisciplinary boundaries, instead taking great pleasure in heterodoxy. But the price they paid in not being part of, as Waddington styled it, the “Conventional Wisdom of the Dominant Group,” was exclusion from the main stream of their science and from the histories of their scientific fields.

Perhaps they were incorrigibly idealistic. Or perhaps their training at Cambridge in the 1920s left an indelible mark on their lives. Or perhaps it was their experiences during the Second World War that conveyed a sense of purpose greater than the pursuit of fiduciary gain or scientific notoriety. Or perhaps it was the influence of their marriages—to Margaret Mead, a world-famous cultural anthropologist, and Justin Bianco-White, a respected artist and architect. Or perhaps they believed (in retrospect we might say mistakenly) they had attained an appropriate degree of notoriety already. Or perhaps it was the inspiration of that earlier circle of thinkers from the Woodgers and the Needhams to Wiener and the other cybernetics cadre—or at least the memory of those inspiring groups from decades past. Whatever it was that drove them forward in the face of ambivalence, if not resistance, Bateson and Waddington doggedly pursued their Whitehead-inspired organicist theory of evolution from genome to phenotype to ecosystem. By the late-1960s and early-’70s it may even have appeared as if the organicist theories, if they could not subsume the other approaches, would at least survive to carve a path between the Scylla of reductionist molecular biology and the Modern Synthesis and

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89 Thomas Kuhn would have defined this as being part of “normal” science, when scientists are solving relatively small puzzles. Bateson and Waddington were operating within a different worldview and on more troubling, larger issues (Kuhn may have said “paradigm,” though this point can be debated).

the Charybdis of Rogers’ self-actualization psychology and “culturist” anthropology. There were a number of indicators that this might come to fruition. At Waddington’s University of Edinburgh, for instance, a “human ecology” program began in the 1970s; and under Waddington’s direction, “epigenetics” had been a viable subfield of biology since the 1960s. And both Waddington and Bateson organized a number of conferences attracting rigorous thinkers and promoters of a “middle-way” theory.

But when sociobiology appeared as a popular theory in the mid-1970s, near the end of both Bateson’s and Waddingon’s lives, it seemed that it might short-circuit the search for this third way that the old friends had advocated through the twentieth century.

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91 Though it fell out of fashion in the 1980s, systems biology is now a major theme in the University of Edinburgh’s overall research program. In 2009, the university opened its Centre for Systems Biology in the £7.2 million C. H. Waddington Building, University of Edinburgh, “Pioneering Research Centre Opens,” http://www.ed.ac.uk/news/all-news/waddington-150909 (accessed 9 December, 2009).
CHAPTER NINE

NATURE-MAKES-NURTURE: ETHOLOGY AND SOCIOBIOLOGY

COMPLETE THE MODERN SYNTHESIS, 1948–1980

Taxonomy and ecology...have been reshaped entirely during the past forty years by integration into neo-Darwinist evolutionary theory...in which each phenomenon is weighed for its adaptive significance and then related to the basic principles of population genetics. It may not be too much to say that sociology and the other social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis.

—E. O. Wilson (1975)\(^1\)

If we think of our existence not as that of a little god outside, but as that of a ganglion within, we have the infinite behind us. It gives us our only but our adequate significance.... If our imagination is strong enough to accept the vision of ourselves as parts inseverable from the rest, and to extend our final interest beyond the boundary of our skins, it justifies the sacrifice even of our lives for ends outside of ourselves.

—Oliver Wendell Holmes (1918)\(^2\)

It might be helpful if a science historian would record these changes in the study of animal behavior while they are still in progress.... My opinion is that the speed of acceptance of sociobiology shows the need for some such theory, shows that the need is evident in our whole climate of opinion—both popular and scientific—and shows that the controversies of the last two years cannot be understood in terms of the progress of science alone.

—S. J. Washburn (1978)\(^3\)

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A century after Darwin published the *Descent of Man*, scholars were again wrangling over the issue of human exceptionalism. By the 1970s, both scholarly and popular realms identified this debate by the moniker “sociobiology”; advocates and opponents alike billed sociobiology as another extension of the neo-Darwinian Synthesis. As was the case in the first two strands of the Modern Synthesis—the mathematical compatibility phase of the 1910s–30s and the consensus building phase of the 1930s–50s—the transition to this new phase was not smooth. Emotional outbursts and *ad hominem* attacks co-mingled with nuanced, substantive arguments over methodology and the philosophical appropriateness of applying scientific findings to human beings. Alliances were formed and relationships broken. To a much greater degree than the first two episodes, sociobiology resonated with already simmering semi-public scientific debates regarding the role of evolutionary biology over a broad range of issues as diverse as: the relative authority of other academic disciplines, the philosophical understanding of human nature, the relative malleability of intelligence, and proposals for the scientific planning of society.¹ Thus by the 1980s, the term sociobiology had become lodged in the Anglo-American popular scientific lexicon, encouraged by sales of E. O. Wilson’s massive *Sociobiology* and Richard Dawkins’ *The Selfish Gene*.⁵

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Resistance to the concept of sociobiology, however poorly some popular opponents understood the concept, coincided with reignited conflicts between conservative American Protestant groups and teachers of evolutionary theory in public schools that had largely lain dormant since the Great Depression. Therefore, and despite the fact that very little of this social resistance was new when it reappeared in the mid-1970s, the noise surrounding the (re)emergence of sociobiology following Wilson’s publication tended to drown out reflective historical comparisons.

Had popular reports investigated the intellectual history of the sociobiology concept, they might have noted that these debates had erupted several times over the nineteenth and twentieth centuries. Prior to the publication of Darwin’s *Descent*, Herbert Spencer and Pyotr Chaadaev wrote extensively and contentiously on the confluence of biology and human ethics. And in a once-celebrated study, *The Nature-Nurture Controversy*, author Nicholas Pastore meticulously examined what he identified as “social Darwinism” in the writings of two dozen prominent life and social scientists from the first half of the twentieth century—from Francis Galton and William Bateson to H. J. Muller and J. B. S. Haldane. He concluded that deeply-ingrained social, religious, and political values influenced their support for either “hereditary” or “environmentalist” theories of human behavior much more strongly than any scientific data. Anticipating, and to some extent surpassing, the work of sociologists of science in

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the 1970s and '80s, such as Shapin and Schaffer,8 Pastore carefully denoted that the influence was not direct, or “one-to-one,” but a diffuse and circular interaction between implicit social and explicit scientific values.9

Beginning contemporaneously with Darwin, resistance to the social applications of biological principles has centered around three concerns. First, skeptics have worried that scholars who wished to “explain” socio-cultural features with reference to merely biological/bio-chemical stimuli were motivated by desires to enforce a ruling class status quo conflating, in the words of the opponents of this so-called “naturalistic fallacy,” what is for what ought to be.10 If Pastore’s findings were correct, these fears may be somewhat justified: “hereditarians” were often born into, or had worked themselves into, positions of relative social influence and power. Secondly, opponents have questioned the appropriateness of any particular model of biology for the reduction of human values to biological instincts. Would, for example, biological models predicated on “superorganic” insect societies like bees and ants be more appropriate than appeals to the influence of

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9 In contrast to his contemporaries Robert Merton and Karl Mannheim, who wished to preserve science (or at least physics) as a model way of knowing, Pastore revealed that socio-economic and political “allegiances…had a marked effect upon the formulation of a hypothesis and the method of is verification, the conclusions drawn from an investigation, and the statement of implications of these conclusions for society…. The nature-nurture controversy, qua controversy, has been sustained by the conflicting social philosophies of the scientists.” Nicholas Pastore, The Nature-Nurture Controversy (New York: King’s Crown Press & Columbia University Press, 1949), pp. 177, 181.

10 For an example of this type of skepticism regarding sociobiological science, see Friedrich Engels, Dialectics of Nature, trans. Clemens Palme Dutt, with a preface by J. B. S. Haldane (New York: International Publishers, 1940).
molecular genes? Thirdly, opponents from the social sciences argued that the old, deeply-held intuition that humans are not *quite* like other animals—the human exceptionalism we explored in earlier chapters—seemed to be strongly supported by global ethnographic studies from the Victorian period onward.

Perhaps it was the confluence of all three of these factors that led to the vitriolic denunciations of Wilson, a Harvard entomologist turned sociobiologist. These first erupted on the pages of the *New York Review of Books* in 1975, and then again in February 1978 at the American Association for the Advancement of Science (AAAS) symposium on sociobiology. As in nineteenth century reactions to Spencer and Social Darwinism (and perhaps counterintuitively if one presumes, given the historical record from the *Scopes* trial to the *Kitzmiller v. Dover, PA School Board* Intelligent Design trial, that resistance to evolutionary biology receives its energy from conservative opponents), the group of individuals that attacked Wilson’s *Sociobiology* stood firmly to the sociopolitical left. Some historians of the 1970s debate even suggest that Wilson’s *Sociobiology* was intentionally placed in the limelight by the Marxist opposition to sociobiology that congregated in the Sociobiology Study Group (SSG) and the allied Science for the People (SftP) organization in order to alert the public to the ways science was being co-opted by

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11. This debate has been central even to the recent discussions of coauthors Bert Höffldobler, and E. O. Wilson (*The Superorganism: The Beauty, Elegance, and Strangeness of Insect Societies* (New York: W.W. Norton, 2008)); and Bert Höffldobler, “The Superorganism: How Did We Come to Understand What it is?” (Paper presented at the History of Science Society, Phoenix, AZ, November 20, 2009).

the ruling conservative politico-economic class.13 Publications following the late-1970s sociobiology debates authored by two of the most prominent members of SSG and SftP—Wilson’s respected Harvard colleagues Richard Lewontin and Stephen J. Gould—support this suspicion.14

Wilson attempted to acquit himself of the extreme charges leveled by SSG and SftP in the popular and scientific press that sociobiology resembled scientific policies pursued by early-20th century eugenicists.15 Wilson’s defense against what seem to be overblown, politically motivated claims may have worked for him as an individual scholar. For sociobiology as an disciplinary approach, however, the charges of fascism masquerading as legitimate science appeared to stick.16 Despite assurances of historians that the debate between “naturists” and “nurturists” ended with a compromised consensus in the days leading up to the Second World War,17 the acrimony surrounding the popularization of sociobiology in 1975 highlighted still smarting, though decades old,


social and scholarly wounds. Perhaps understandably, then, the subsequent locking of horns between advocates and opponents of sociobiology (rebranded in the 1990s as “evolutionary psychology”) continued through the 1970s, '80s, and '90s—eclipsed, though not ended, only by the broader so-called “Science Wars” of the late 1990s. ¹⁸

In this chapter, I describe the origins of mid-twentieth century sociobiology and the telling theoretical changes that transformed sociobiology from a relatively innocuous analogy to a disputed, deterministic “third phase” of the neo-Darwinian Synthesis. I do so with an eye toward the final years of Waddington and Bateson’s lives in the 1970s. I argue that it is the 1960s–70s framing of this debate over the relative influence of genetics on human behavior by Bateson and Waddington that best accounts for their relative marginalization, the demise of their form of organicism as a viable alternative to both reductionism and “obscurantist” holism/vitalism/culturism, and the triumph of the gene-centric understanding of human behavior currently in vogue in popular science media. Unfortunately, the very problem that Bateson and Waddington were most equipped to address—the confluence of biology and culture—was the domain from which their attempted non-reductionist resolutions were excluded.


Ironically, given its subsequent history, sociobiology arose out of a collaboration between ecologists, geneticists, and animal behaviorists who aimed at increasing public awareness regarding natural resource and animal habitat destruction. Sociobiology, in other words, had its roots in the fledgling post-World War II American environmentalist movement. This connection is quite apparent when we observe the context in which the term “sociobiology” was minted.

Beginning in September 1946, scientists from the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine, sponsored conferences on animal behavior, attracting some of the foremost specialists of animal sociology in the United States. A subset of this group met together with Fairfield Osborn (son of former AMNH president, Henry Fairfield Osborn), president of the New York Zoological Society (NYZS), to establish a new research center at Jackson Hole, Wyoming. Jackson Hole was chosen because, according to Osborn, animals lived in an “undisturbed” habitat there. Efforts to establish the Jackson Hole research station led to a much larger conference in 1948, now under the combined sponsorship of the widely respected American Society of Zoologists and the Ecological Society of America. Another large meeting two years later, this one a collaboration between the New York Academy of Sciences and the NYZS, resulted in a volume of conference proceedings, edited by noted animal behaviorist John Paul Scott of the Roscoe B. Jackson Memorial Laboratory. The 1950 meeting had two goals:

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One is to remedy the deplorable lack of scientific control over destructive social phenomena such as warfare, crime, and poverty…. The other aim is to promote the conservation of natural resources, which has been strongly advocated by Fairfield Osborn…. In any case, the two aims are so closely interrelated that one can hardly be reached without the other.

In the wake of another World War, and with Western-Soviet relations turning bellicose, any hope of conserving animals and their natural habitats seemed predicated upon limiting human destructiveness.²⁰

Fairfield Osborn, son of famed paleontologist, eugenics supporter, and director of New York’s American Museum of Natural History, Henry Fairfield Osborn, wrote Our Plundered Planet with an acute sense of this darkening global political and ecological situation.²¹ In Plundered Planet, an early example of modern conservationist or environmentalist literature, Osborn appealed both to modern science and to ancient Judeo-Christian tradition in his argument for the conservation of water, soil, plant life, and animal life “from protozoa to mammals.” Osborn stressed human interconnectedness with nature without appealing to nature-mysticism as earlier environmentalists such as John Muir had done. Instead, Osborn crafted his argument using the scientific terminology of the day: “Nature represents the sum total of conditions and principles which influence, indeed govern, the existence of all living things, man included... [M]an, despite the extraordinary mental accomplishments...as been, is now and will continue to be a part of nature’s general scheme.”²² Osborn

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²² Osborn, Plundered Planet, p. viii.
referenced the irreducible holism of ecology: “Remove any essential part and the machine breaks down.”\textsuperscript{23} Then he extended this ecology-inspired holism to humans:

The basic similarity, from a biological point of view, of all peoples on the face of this earth has only recently been made clear recently through the development of anthropology and genetics.... The saying, ‘We are all brothers under the skin’ has a basis in scientific fact.\textsuperscript{24}

This intra-species relatedness meant, for Osborn, that there was no defensible way to “naturalize” war: related animals knew nothing of war or even murder. In fact, carnivores—who, in any case, could hardly be considered murderers—only account for about one percent of total animal populations in their natural equilibria. Human wars like the First and Second World Wars were unique to humans rather than anything mirroring the larger animal kingdom. According to Osborn, a qualitative boundary between animals and humans could be clearly identified: humans are acquisitive and aggressive; animals, typically, are not.\textsuperscript{25} Significantly, then, scientifically trained early American environmentalists, like Osborn and others, believed that human aggressiveness—illustrated dramatically in wars and mass killings and displayed in more mundane activities like irresponsibility with natural resources resulting in mismanagement, waste, and animal extinction—was not part of our animal heritage. Aggressiveness resulting in the slaughter of other human races and whole animal species appeared to be a form of biological deviance unique to humans. This point was to be an

\textsuperscript{23} Osborn, \textit{Plundered Planet}, p. 48.

\textsuperscript{24} Osborn, \textit{Plundered Planet}, pp. 24–5.

\textsuperscript{25} Osborn, \textit{Plundered Planet}, pp. ix, 23–4.
extremely interesting one for later discussions regarding the relation of animal to human behavior.

Books like Osborn’s *Our Plundered Planet* directly informed scientific research like John Paul Scott’s fieldwork at the Roscoe B. Jackson Laboratory in Maine. There Scott and nine research assistants observed three litters of dogs (each of a different breed) that were allowed to live through a full life cycle (each group produced at least one litter) more or less undisturbed by human intrusion. They supplemented this study with another, more controlled laboratory study of 17 litters (7 breeds) and a third set of observations of 70 dogs in “kennel conditions.”²⁶ To this set of data on domestic dogs, Scott’s research group added an intensive study of literature on wolf behavior in the wild. After conducting these comparisons from 1946–48, Scott concluded that the obvious deviations between domestic dog and wild wolf behavior appeared not because of any genuinely novel biological change but resulted from the “exaggeration and suppression of basic behavior patterns.”²⁷ Though the physical appearance of beagles, for instance, differed dramatically from their wild lupine cousins, their underlying behavior patterns did not. According to Scott, canid nature, whatever that might entail, had not altered substantially even after centuries of artificial selection.

Scott’s goal in these experiments had been two-fold. Obviously, he intended to clarify domestic dog behavior with respect to wolf behavior. He also hoped, quite explicitly, to make bolder claims regarding the application of this kind of work with


animals to behavioral patterns observed by sociologists who study humankind. Unfortunately, as Scott pointed out, no sociologist studied humans “in general”—by the twentieth century, even anthropologists localized their studies to one particular ethnic group or another. For decades, perhaps since the Rivers and Haddon Torres Strait expedition in 1898, social scientists found it impractical to make serious claims about any non-trivial behavior shared by all humans. Yet Scott attempted just that with his “sociobiology”: social animals, dogs and wolves in this case, were to give researchers a new lens with which to peer into the terra incognita of human behavior. Because “wolves and dogs show certain similarities to human beings,” Scott insisted a new generation of animal-human behaviorists could begin to resolve the now hackneyed nature/nurture question, “If such general traits exist, how much are they influenced by heredity and how much by environment?”

Neither Scott nor any of the other participants who attempted to make telling correlations between humans and animals at that initial sociobiology event in 1950, the “Methodology and Techniques for the Study of Animal Societies” conference, related their work to Darwin’s Descent of Man; nor did they seem interested in any other work of the Victorian era, biological or anthropological. In fact, this group of animal behavioral scientists seemed only vaguely interested in the nearly century-long debate already behind them about just this issue: is social behavior “nature,” or is it “nurture?” They certainly were interested in demonstrating the connections between humans and ecology—connections that seemed increasingly destructive, bleak, and inevitable. Perhaps they believed that if they could demonstrate the kinship of humans and especially “higher” animals, that scientific realization would reorient the apocalyptic course that humanity seemed to have charted for itself in the twentieth century. At a
minimum, they believed, as Osborn had suggested, that wanton destruction found no basis in behavior inherited from animal ancestors.

But for all their insinuation that “simpler” bio-social systems, such as those among domestic dogs and wolves, would unlock a new understanding of human bio-social behavior, nowhere did this group of scientists assembled for the Methodology and Techniques for the Study of Animal Societies conference in 1950 go out of their way to pin behavior to particular biological models. They left “heredity,” like “environment,” an unopened black-box. They did not address theoretical questions like how this behavior originated biologically or why it changes, if it indeed does. In fact, as in Scott’s study of canid behavior, it mattered little whether one approached behavior as a Darwinian or a Lamarckian—changes in environment might well have been the cause of traits later artificially selected by humans for domestication.28

From a historical perspective, animal behavior scholarship, at least of the variety that contributed to this initial deployment of the term sociobiology in 1950, stood on nearly the same philosophical ground as the work of Charles Darwin and T. H. Huxley a century earlier: analogies between human and animal behavior is made to do the heavy lifting for implied homology. In other words, similarities in appearance between modern social behavior of canids and the social behavior of humans are meant to imply common evolutionary roots. This comparison was not fully examined at the 1950 conference, though entomologist T. C. Schneirla, a major figure at this conference, did seem to be

aware of the problem and addressed it in his other papers. As a side issue, though a crucial one for the history of the sociobiology debate, by the 1950 conference, there was not an endorsement of one particular biological path or mechanism to which behavior was attributed; no participant advocated the explanation of animal behavior in terms of changes in allele frequencies, for instance.

9.2 Ethology: From Analogy to Implicit Determinism, 1950s–70s

J. P. Scott continued to work on the behavioral patterns observable in domestic dogs and their wild cousins. In the mid-1960s, Scott and long-term collaborator, John L. Fuller, published their classic study of the social behavior of canines. They claimed that their study would shed important light on human social behavior as well, especially because humans, dogs, and wolves had coexisted, perhaps even coevolved, for such a long period of time—well before humans developed what would now be considered “civilization.” Scott and Fuller even went so far as to claim that, given their relatively short generational period—roughly one-tenth of a human generation—the past and future evolution of dogs offered behavioral scientists something akin to the “genetic pilot experiment for the human race.”

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31 Scott and Fuller, Genetics and the Social Behavior of the Dog, p. 397.
Despite the excitement of its earlier investigators, however, sociobiological research had migrated from canines to other, seemingly more fruitful, research organisms and methods. In the United States, and much to the consternation of those like Scott and Schneirla who studied wild populations, laboratory settings were thought to provide more authoritative results. Observations and experiments with captive primates, especially, promised to open a window into human behavior. Even in Europe, where funds for housing and feeding groups of chimpanzees and orangutans for the purposes of studying human social behavior were understandably scarce following World War II, the comparative study of animal behavior in controlled confines, even if not in a formal laboratory, developed into a highly respectable discipline known as ethology.\textsuperscript{32}

European ethology, also known as behavioral physiology (\textit{Verhaltensphysiologie}), was primarily championed by a Dutch ornithologist, Nikolaas “Niko” Tinbergen, an Austrian entomologist, Karl von Frisch, and Konrad Lorenz, a Viennese anatomist and amateur bird-watcher, beginning in the 1930s. After meeting Tinbergen in 1936, Lorenz became something of a recognized aficionado on both bird and fish behavior and, increasingly, the public face of ethology. Upon returning to his home in 1948, following his enlistment as a medic with the Eastern Front Wehrmacht, and his subsequent capture by Soviets and forced medical duty in Armenia for several years, Lorenz and

psychologist Erich von Holst set up a behavior research institute funded by the Kaiser-Wilhelmsgesellschaft (now the Max-Planck-Gesellschaft). Explicitly, their institute studied behavioral patterns in non-human animals: collaborating often with Tinbergen, Lorenz intensely followed the lives of jackdaws, rooks, and graylag geese, reporting on their lives with exquisite clarity and aesthetic flourish for both scholarly and popular audiences. Implicitly, however, Lorenz had long oriented his work in light of his beliefs about the implications of animal behavior for human civilization. Though his extraordinarily popular King Solomon’s Ring familiarized readers with the lives of animals in home-spun, radically anthropomorphic terms, Lorenz, much like Darwin in Descent of Man, intended his analogies to run in both directions:

You think I humanize the animal? Perhaps you do not know that what we are wont to call ‘human weakness’ is, in reality, nearly always a pre-human factor and one which we have in common with the higher animals? Believe me, I am not mistakenly assigning human properties to animals: on the contrary, I am showing you what an enormous animal inheritance remains in man, to this day.

And if I have just spoken of a young male jackdaw falling in love with a jackdaw male, this does not invest the animal with human properties, but, on the contrary, shows up the still remaining animal instincts in man. And if you argue the point with me, and deny that the power of love is an age-old instinctive force, then I can only surmise that you yourself are incapable of falling a prey to that passion.\

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14 Konrad Z. Lorenz, King Solomon’s Ring: New Light on Animal Ways, trans. Marjorie Kerr Wilson (New York: Time, Inc, 1962) [Originally published by Thomas Y. Crowell, 1952], p. 170. So popular was King Solomon’s Ring that a decade after its initial publication in the United States, the publishing giant Time, Inc. bought the rights and republished it as a trade paperback using a custom typeface, Granjon, designed by the famed type foundry Atlantic Linotype, and an experimental plastic-impregnated cover—both extremely costly investments for a paperback reissue. But justifiable, according to Time’s editor (see “Editor’s Preface” and “A note about this book”): King Solomon’s Ring had already sold nearly 50,000 English-language copies since its initial publication, an exceptionally high number for a book of its kind.
As a professional, scholarly observer of animals, Lorenz gained notoriety for his uncanny ability to deconstruct the barriers between himself as observer and his animal subjects.\(^5\)

This bit of blurred vision, which he modeled after famed German *Umwelt* theorist Jakob von Uexküll, contributed directly to his more crucial blurring of the boundary between animals and humans.\(^6\)

Lorenz was not convinced, as were many other European and American scientists of the day, that animal behavior provided an interesting, but ultimately abstract and inapplicable, analog to human behavior.\(^7\) Lorenz instead envisioned specific linkages between behaviors mutually possessed by organisms as diverse as his aquarium-bound angelfish, precocious jackdaws, and humans. Aggressive behavior, often taking the form of territoriality, particularly intrigued Lorenz. After the commercial and professional success of King *Solomon's Ring*, he published *On Aggression* to great fanfare in 1963 (English translation, 1966).\(^8\) Initially, the work received a cordial reception as scientific work able to be read by those without ethological training. And it was written by a scientist with homespun charm.\(^9\) But over the course of the 1960s, scholars and

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6 Lorenz, “Autobiography.”


9 Lorenz carefully crafted this reading of his work and his scientific image as “foster-mother of ducks.” See, for instance, his interview in *Life* magazine: “An Adopted Mother Goose: Filling a Parent’s Role, a Scientist Studies Goslings’ Behavior,” *Life* 39 (July/Aug. 1955): 73–8; and Marga Vicedo, “The
popular readers alike became more critical. After *On Aggression* especially, readers began to view all of Lorenz’s ethology in a less favorable light. Even the once whimsical *King Solomon’s Ring* was thought to shelter a more sinister philosophy of biological determinism, even concealed fascism.\(^{40}\)

Though the clamor in the mainstream press of the 1960s surrounding Lorenz’s work never reached a pitch as shrill as that which followed Wilson’s *Sociobiology* in 1975, social scientists publicly disagreed with one another and with Lorenz regarding the value of ethology when applied to the human animal. Some of the most assertive opponents congregated, not surprisingly, in the anthropological and psychological communities of the US and UK. Fellow European scientists, on the other hand, stood fast in their support of *On Aggression*. For instance, both Niko Tinbergen, who moved to Oxford after the war, and Karl von Frisch, whose Rockefeller Foundation-supported entomological institute flourished in post-war Munich, remained supportive both of ethology in general and of Lorenz’s insights into human aggression specifically. Tinbergen and his wife had by this time begun applying his ethological techniques to the study of autism in children.\(^{41}\) If nothing else, the growing divisions between ethologists

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\(^{40}\) Though he successfully rehabilitated his image after the war, Lorenz was a supporter—though not an ardent one—of the *Blut und Boden* (blood and land/soil) social hygiene movement in Third Reich Germany. In his “Durch Domestikation verursachte Störungen des arteigenen Verhaltens” (*Zeitschrift für Angewandte Psychologie und Charakterkunde* 59, no. 2 (1940)), Lorenz cited his own ethological work in support of his observations of “domestication degeneracy” in city dwellers, in part because of the “high productive rate of the moral imbecile” and the “socially inferior human material” that inhabited urban areas. Georg Breuer, *Sociobiology and the Human Dimension* (Cambridge: Cambridge University Press, 1982), pp. 234–36.

in Europe and social scientists in America, including some of the early sociobiologists like Scott and Schneirla, demonstrated the lack of unified methodology or common philosophy among those who investigated seemingly similar problems in animal behavior at mid-century.

Lorenz’s work was controversial in part because, unlike studies by fellow animal behaviorists that focused on a range of typical behaviors limited to one or a small set of organisms, he began the study that would eventually become *On Aggression* by fixating on a single behavioral trait and looking for examples of the trait anywhere he could find them. Lorenz’s observational techniques were not haphazard. To the contrary, he had accumulated an impressive record of scholarship before *On Aggression* based on painstaking observations.\(^4^2\) But his critics immediately protested that his observations were occasionally arbitrary, his gaze wandered toward observations that seemed to confirm what he already believed to be the case, and that his analysis tended to obscure as many issues as clarify them.\(^4^3\) Lorenz’s made his purpose quite clear: it was to “naturalize” the historically romanticized portrayals of the *Naturphilosophie* of Goethe and Kant. In a phrase strangely reminiscent of Darwin’s concluding lines in the *Origin of Species*, Lorenz hoped to retain, as he saw it, “transcendental” inspiration while extirpating supernatural content:

Never has natural explanation of one of its marvelous processes exposed nature as a charlatan who has lost the reputation of his sorcery; natural causal associations have always turned out to be grander and more awe-


inspiring than even the most imaginative mythical interpretation.
...[F]or [the scientist] there is only one miracle, namely, that everything, even the finest flowerings of life, have come into being without miracles; for him the universe would lose some of its grandeur if he thought that any phenomenon, even reason and moral sense in noble-minded human beings, could be accounted for only by an infringement of the omnipresent and omnipotent laws of one universe.
...Would [Kant], who did not yet know of the evolution of the world of organisms, be shocked that we consider the moral law within us not as something given, a priori, but as something which has arisen by natural evolution, just like the laws of the heavens?**

Lorenz’s metaphysically naturalistic revision of the Kantian moral a priori, in other words, could retain the same sort of force it held for Kant without explicit grounding in anything beyond empirical evidence.

Complicated human morality, according to Lorenz, had itself developed gradually from fundamentally similar, if simpler, animal behavior. Indeed societal “ought,” to turn the famous Humean guillotine on its head, was derived from a pre-human “is.” Humans were aggressive to one another because “aggression,” far from being morally or ethically impermissible, furnished the basic approach to the primary problem of existence: how to ensure one had the best possible chances to survive and reproduce. Aggression—despite the fact that humans abhorred it when it was practiced as “war,” its most grandiose and inefficient form—was hardwired. Evolution, in fact, could not work without aggression. And when it came to evolution, Lorenz expanded “aggression” to include intra-species territoriality accompanied by intimidation, blustering displays, and occasional violence. To evolve, Lorenz claimed, species required an innate drive not, as others had suggested, for mutual aid but for mutual dislike.

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Little of this rewriting of ethics from an evolutionary perspective was new with Lorenz or *On Aggression*. It inspired such vocal resistance in the 1960s for two reasons. First, Lorenz’s popular book seemed to foreground especially vicious laissez-faire Malthusianism. Lorenz’s depictions of blatant, often violent, intra-specific competition in a diverse panoply of animal societies—“nature red in tooth and claw”—appeared to undercut appeals like those of prominent ecologists and other animal behaviorists that animals lived in relatively peaceful dynamic equilibrium with each other and their environments (compare to Osborn’s claims above). Historically, this fear was warranted: generations of anti-Darwinians had decried an apparent appeal to the inevitability of *bellum omnium contra omnes* at the core of Darwin’s project. Works like *On Aggression* more than confirmed old suspicions that “social Darwinism” and Darwinism were of a piece.\textsuperscript{45}

Secondly, Lorenz’s *On Aggression* joined a growing list of publications that purported to explain complicated human issues—the marrow of the social sciences—by reducing them to, hypothetically, much more soluble biological problems. Robert Ardrey’s Territoriality Trilogy (*African Genesis*, 1961; *The Territorial Imperative*, 1966; and *The Social Contract*, 1970) and Desmond Morris’s *The Naked Ape* (1967) exposed disciplinary tensions already present, though at a less intense level, in the reception of the work of ethologists. Were Anglo-Americans like Ardrey and Morris merely articulating

a desire implicit in the work of Tinbergen, Lorenz, and other European ethologists to conquer sociology, anthropology, and psychology, subjugating them under the banner of zoology or physiology? By 1973, the year he shared the Nobel Prize for Medicine or Physiology with Tinbergen and Von Frisch, Lorenz had repudiated this reading of his work in particular: ethology hoped to extend gently the Darwinian paradigm to the social sciences, not bring them under a greatly augmented zoology.⁴⁶ He did not clarify his distinction between extending biology to the social sciences and cannibalizing them, however. And his most vocal supporters definitely did not second his recognition of the sovereignty of the social sciences. To his chagrin, Lorenz conceded in the early-1970s that his detractors might justifiably find his claims regarding the perspicuity and applicability of ethology threatening, especially when appearing as supportive planks in the works of popularizers like Ardrey and Morris.⁴⁷

Regardless of Lorenz’s qualms about its use, Robert Ardrey and Desmond Morris found evidence for their arguments in On Aggression and in Niko Tinbergen’s Herring-Gull’s World.⁴⁸ Morris, a zoologist by training and primatologist by profession, devoted only one of eight chapters specifically to the behavior of “fighting.” Yet his widely read comparison of human and ape behavior, anatomy, and physiology drew extensively from European ethologists in general, and Tinbergen and Lorenz in particular. As was the case with other zoologists who entered the domain usually


occupied by social scientists, Morris opened his work with a salvo directed at two groups: those intellectual ostriches who would rather bury their proverbial heads than “contemplate their animal selves,” and “specialist[s]” who might take offense at Morris’ “zoological invasion.” Morris intended to put psychologists, sociologists, and anthropologists on notice: though “vertical, hunting, weapon-toting, territorial, neotenenous,” and endowed with unparalleled intelligence, the “Naked Ape” was tightly tethered to its simian ancestry and, therefore, was rightly the provenance of zoology. Morris did not conceal the fact that he regarded the social sciences as living on borrowed time. Eventually, zoological techniques would suffice to ferret out the major aspects of human behavior. But Morris further implied that, as far as zoology was concerned, genetically determined behavior inherited from our ancient ape ancestors could account for every idiosyncrasy of human existence. Even seemingly irreducible issues regarding religion and submission, on the one hand, avarice and war, on the other, ultimately originated in our ape ancestry, itself determined by randomly associating DNA. Because, for Morris, human behavior mapped so efficiently onto animal behavior, and because animal behavior could be reduced so easily to extremely slow changing, biologically-determined, programmed genetic responses to biochemical stimuli, the only solutions to human behavioral problems lay in controlling human reproduction—


namely through elective contraception and abortion, though involuntary eugenics could not be ruled out.\footnote{52 Morris, \textit{Naked Ape}, pp. 173–78.}

On the other side of the Atlantic, author Robert Ardrey largely concurred with Morris’ assessments regarding both the cause and the solution to problems of human behavior, especially aggression. Though, like Morris, Ardrey planted the roots of all human behavior in the primate past, he also found extensive analogues among other animal species. In this way, Ardrey—a Hollywood screenwriter by trade, though with some undergraduate training in physical anthropology—functioned throughout the 1960s as an American mouthpiece for many of Lorenz’s European ideas. Territoriality excited Ardrey as it did Lorenz; the drive for adequate space for feeding and reproduction justified, even if it did not fully explain, all manner of animal and, by extension, human behavior. Ardrey extrapolated the implications of animal territoriality for humanity further than Lorenz, however.

In the second book of his influential trilogy, \textit{The Territorial Imperative}, Ardrey moved from a relatively innocuous definition of territory as “space, whether of water or earth or air, which an animal or group of animals defends as an exclusive,” to an abstract force: “the inward compulsion in animate beings to possess and defend such a space…. [A]n inherent drive to gain and defend an exclusive property.” Using this quite flexible definition of “territory,” Ardrey then argued the following: (1) territoriality appears innate for socially complex animals; (2) the widespread appearance of territoriality indicates that it is an adaptive trait; (3) since territoriality is a widespread, adaptive behavior, it must also be adaptive for humans; (4) indeed, humans have notions of the
“pursuit of property,” “national defense,” and the like, so they must have retained the adaptive trait for territoriality from their non-human ancestors.\textsuperscript{53} Ardrey then employed this somewhat loose reasoning against theologians and religious leaders as well as scholars in the arts, humanities, social, and political sciences who argue for the inhibition of human territorial acquisitiveness. For humans, according to Ardrey, the internal drive for territory renders all other human behaviors secondary.

With territoriality came dominance, another “internal drive,” this one an “upward-pressing force that seeks competition, strives for superiority…”\textsuperscript{54} By his third book, \textit{The Social Contract}, Ardrey sought to explain “alphaness” in animals and humans and its social complement, “followership.” The duality was, of course, just a detail of the territoriality scheme that also spawned language, dress, eating habits, and the division of labor between frugal, home-making women and competitive, hunting men. Hunting, organized around a notion of dominance, was especially important for Ardrey’s account of hominid evolution. As with Morris’ \textit{Naked Ape}, Ardrey cited hunting as the proving grounds for the methodology of warfare. But territory-inspired leadership and followership also led to social cohesion, a seemingly counterintuitive outcome for Ardrey. For though followers must outnumber leaders, any social group must contain a fair number of strong alpha personalities. All that competition between dominant males located together in their hard-won territory must engender some enmity between individuals who need each other to provide resources for the followers in the group, especially females and their young. Relying on the ethological observations of Lorenz


and the threadbare arguments of Victorian-era determinists Herbert Spencer and his American apostle William Graham Sumner, Ardrey created the formula $A=E+H$ (i.e., group *Amity* equals dominant, hunting male *Enmity* plus external *Hazards* to the group). By this bit of social algebra Ardrey meant to convey his supposition that the ability of any group to overcome naturally occurring competition among the dominant members in its own territory stood in direct proportion to impending threats from outside the group. As Ardrey described it, only the image of greater *foreign* hostility could balance the ever-smoldering hostility of *domestic* individuals toward each other.  

(Whether his balanced hostility model was ultimately inspired by the omnipresent cultural tensions of the Cold War or those of his own household, he did not say.)

Here Ardrey articulated, and purported to resolve, a seemingly obvious paradox at the heart of explanations that gathered under the canopy of ethology: how could violently competitive instincts deriving from territoriality, an absolutely fundamental drive, result in the often-corroborated observations of strong social cohesion? As we observed earlier, social cohesion could only present a paradox if scholars agreed that aggression played a universal biological role; animal behaviorists like John Paul Scott and advocates of environmental conservation like Fairfield Osborn did not believe aggression to be the norm in animal societies. As we will observe below, however, the paradox of aggression leading to social cohesion—which by the 1960s came to be called the problem of altruism—would dominate discussions of evolution, ethics, and the philosophy of biology both before and after E. O. Wilson introduced his variety of sociobiology.

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9.3 The Aggression and Territoriality Debate, 1966–73

Of the claims attributed to ethology by the mid-1960s publications of Lorenz, Ardrey, Morris, and others, the most controversial was that human aggression seemed to be incorrigibly instinctual, a product of untold generations of aggressive primates, and predicated on the drive for territory—itself as close to a biological law as had ever been proposed. Concomitant with this assertion were two other claims, possibly even more inflammatory: behavior is overwhelmingly genetically determined, and biological scientists are better equipped to address human behavior than those scientists, like psychologists, sociologists, and anthropologists, who have previously studied it. While non-geneticist authors of the neo-Darwinian Synthesis like Ernst Mayr and G. G. Simpson had learned to incorporate the methodology and discoveries of population genetics without jeopardizing the autonomy of their own fields of study, social scientists had not been incorporated into the making of neo-Darwinism. Therefore, they had not adequately addressed as a community of scholars the strong sort of biological determinism, increasingly coupled with genetic reductionism, introduced by ethology.⁵⁶

Given the enormous popularity of works like *King Solomon’s Ring*, *The Naked Ape*, and *The Territorial Imperative*, many scholars chose to voice their opposition to the encroachment of ethology in popular journals and magazines like *Scientific American*,

The Nation, Encounter, and, especially, The New York Times Magazine. Animal behaviorist—but decidedly not an ethologist even after reading many of Lorenz’s works—Sally Carrighar was prompted to write to the NYT Magazine after watching a popular BBC documentary on animal territoriality and reading the work of British ecologist John Hurrell Crook. After bringing her readers’ attention to the philosophical problem of naïve empiricism highlighted by William Whewell’s aphorism, “There is a mask of theory over the whole face of nature,” Carrighar chided both Ardrey and Lorenz for extrapolating from grossly inaccurate hypotheses about animal behavior. Reading from behind their distorted “mask of theory,” ethologists rendered a largely peaceful, communal animal world seem individualistic, competitive, and territorial. Though she was careful not to conflate “natural” with social “good,” Carrighar worried that the public influenced by ethologists would fall into this naturalistic fallacy; any initiative to train bad aggression leading to bullying, physical abuse, and worse, out of our behavior would be stifled if we believed it was “innate and instinctual.” Cultural anthropologist Geoffrey Gorer seconded Carrighar’s points in the NYT Magazine, noting that, according to his own ethnographic research, humans were not aggressive “by nature” any more than were other primates. That some humans were aggressive indicated only that culturally-induced violent behaviors were proscribed, though rarely for extended durations or without clear constraints. Anthropologists Edmund Leach of


Cambridge and Ralph Holloway of Columbia University agreed with Carrighar and Gorer and charged the ethologists and popularizers like Ardrey with disingenuously skewing their reports of observations of caged, tamed, and domesticated animals—which the ethnologists portrayed as existing their “natural” state.⁵⁹ Even respected Scottish zoologist S. A. Barnett added his voice to the chorus in the American popular press, faulting Lorenz’s *On Aggression* in particular for over-reliance on obviously faulty analogies, especially when other ethologists, including Tinbergen, admitted the tenuousness of their data.⁶⁰

Ironically, sociobiology’s “founders,” John Paul Scott and T. C. Schneirla, expressed some of the most forceful critiques of Lorenz, Ardrey, Morris, and others.⁶¹ In his January 1967 article for *The Nation*, Scott extolled Lorenz’s adroit sketches of animal behavior. Clearly, Lorenz observed more carefully and described his observations with greater precision and artistic flourish than other scientists of animal behavior, Scott included. Care in observation came with a price, however. For instance, Scott detected a glaring lack of breadth in Lorenz’s reading: *On Aggression*, like Lorenz’s other works, returned again and again to the pre-World War I work of ornithologist by training, psychologist by professional appointment, Wallace Craig, and the late 19th/early 20th


⁶¹ Scott edited the *Methodology and Techniques for the Study of Animal Societies* volume, to which Schneirla contributed. Both organized and attended the 1950 collaborative meeting between the NYAS and the NYZS at which Scott coined “sociobiology.”
century studies of University of Chicago zoologist and administrator, Charles Otis Whitman. Scott’s worry was not so much that the works of Craig and Whitman that Lorenz relied upon were dated (though that did concern Scott a bit), but that Lorenz borrowed too heavily and unquestioningly from their implicit mechanistic biases, which more recent studies had been careful to avoid. While both Craig and Whitman, like Lorenz, presented their work as if it was conducted on wild animals, in reality their works were largely based on studies of organisms in relatively carefully controlled settings. Craig, especially, believed the setting of the animal in the experiment, in other words the environment, mattered little; a bird kept inside a house showed few distinctions from a bird kept outside in an enclosure or a bird found in the woods. Certainly different aspects of behavior might be emphasized in these different settings, but behavior itself, because intrinsic to the organism-qua-machine, would not alter appreciably. At least, this was how Scott believed Lorenz read Craig. And in the context of aggression, this meant for Scott that Lorenz placed territoriality and aggression as part of the basic operating instructions of most organisms, co-present with the fundamental practices of resource acquisition and reproduction, and prior to any experiences that would alter behavior acquired during the organism’s life. Scott found this portrayal of

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animal behavior incomplete at best, and misleading in the extreme if applied to social mammals, especially humans.  

T. C. Schneirla, one of the foremost American comparative psychologists of the day, saw in the work of Lorenz and other ethologists—to say nothing of Ardrey, Morris, and other popularizers—oversimplification bordering on the pathological. Though his reviews of Lorenz’s *Evolution and Modification of Behavior* and *On Aggression*, which appeared in *Natural History* in 1966, did not stoop to name-calling, Schneirla clearly disapproved of the books. More significantly, he detested their apparently positive reception from the scientific community along with the largely laudatory attention in the popular press. While the University of Chicago Press published *Evolution and Modification of Behavior*, and while the book’s audience was intended to be scientists and intellectual peers, Schneirla identified some of the same errors of fact and judgement that he spotted in the writing intended for popular audiences. It concerned Schneirla that even Lorenz’s scientific peers seemed to be giving him a pass on shoddy work.

According to Schneirla, this shoddiness was evident in at least two different ways. First, Lorenz overzealously employed the results of “deprivation” or “isolation” experiments to get at “native” or innate behaviors. Isolation, as the ethologists saw it, was “neutral”; Schneirla saw it as anything but: “Believing that ‘[deprivation/isolation experiments] can only tell us directly what is not learned,’ the author must assume that he knows beforehand the developmental significances of these ‘natural’ conditions. This,
however, is a major aspect of the problem under study."\textsuperscript{65} Without having anything like a “control” or “normal” for animal behavior at any level, ethologists were actually developing a specified experimental animal in a strictly artificial environment. It followed that behavior under these new conditions would deviate significantly from whatever was “natural.” Comparative psychologists no longer relied upon isolation or deprivation as a means by which to get at instinctual behavior, and it disappointed Schneirla that Lorenz seemed utterly unfamiliar with any of this recent work. Secondly, Lorenz reinforced the false nature-nurture dichotomy, even while denying he was so doing, by shifting to his preferred terms, “phyletic information” and “individually acquired information,” and aiming his investigations on distinguishing between them.\textsuperscript{66} As Schneirla pointed out, Lorenz used “information” in two radically different senses here. By phyletic information, Lorenz meant, and sometimes explicitly said, the genome—“nature.” Individuals acquired additional information through training or learning—“nurture”—but this could not be passed on in any direct and reliable sense to their phyletic descendants. Individually acquired information might be important for ethology to study, but only inasmuch as it allowed scientists to distinguish the truly meaningful (i.e., what nature has programmed the animal to do) from the epiphenomenal (i.e., incidental variations in behavior). Schneirla regarded even Lorenz’s foundational assertion that “native” behavior was genetically coded and, therefore, that it merely supervened upon DNA as seriously misleading: “The implications that the genes

\textsuperscript{65} Schneirla, “Instinct and Aggression,” p. 195.

\textsuperscript{66} Schneirla, “Instinct and Aggression,” p. 194.
rigidly ‘program’ the animal’s learning is opposed by the results of many experiments, as well as by evidence from animal training.”

Lorenz reacted with surprise to these sorts of negative reviews in the American popular and scholarly press of the mid-to-late-1960s and quickly adopted two responses. On the one hand, he donned the role assigned to him in the 1950s around the publication of *King Solomon’s Ring*: a folksy Viennese natural philosopher who worked with gaggles of geese and coral reefs full of vibrant fish. On the other, Lorenz posed as a tough-minded analytical scientist, especially in scholarly works like his two-volume *Studies in Animal and Human Behavior* (1970), railing against what he took to be deliberately obfuscatory appeals to human exceptionalism made by artists, scholars in the humanities, and social scientists. He reserved particular scorn for those practitioners of behaviorism that had ruled American psychology since the days of Watson and Skinner. But though these targets were not exactly scarecrows, Lorenz dodged the majority of the accusations leveled by fellow animal behaviorists like Scott and Schneirla and failed to deal with

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Interestingly, Mead was one of the few anthropologists to oppose Lorenz in his discussions of “natural” maternal instinct and did so on the grounds that there were cultural feedbacks that seemed to militate against some of the claims Lorenz made about the innateness of female behavior regarding infants. Mead’s comments can be found in: World Health Organization, et al., eds. *Discussions on Child Development: A Consideration of the Biological, Psychological, and Cultural Approaches to the Understanding of Human Development and Behavior* (London: Tavistock, 1971), vol. 1, p. 228; and, see also, Margaret Mead, “Closing the Gap Between the Scientists and the Others,” *Daedalus* 88, no. 1 (1959): 139–46.

substantive challenges to the conjoining of biology and human behavior made by anthropologists like Margaret Mead, among others.

Schneirla in particular feared that the increasingly explicit genetic reductionism that he saw at the heart of Lorenz’s ethological project would grow to envelop comparative animal psychology—a fear that even his committed followers agree has been borne out over the last forty years. Schneirla’s own work with organisms ranging from ants to cats lent itself to a dramatically different philosophy of behavior. He ardently disagreed with simplistic accounts of innate, instinctual, or native drives rooted in genetic programs.

A useful way of stating this problem is to say that although heredity influences all behavior in all animals, its influence is more direct in some animals (evidently the more primitive) than in others and may be very different on different phyletic levels. This interpretation contradicts the traditional view that the ‘instinctive’ and the ‘learned’ may be clearly distinguished in animal behavior, with the latter increasingly prominent in higher animals.

Instead, posited Schneirla, behavior evolves by “reorientations.” Organisms rebuilt and extended evolutionarily earlier behavioral patterns in their entirety. In the argument over aggression and territoriality, this reorientation would mean that aggressiveness would need to be interpreted in the context of the overall “species-typical” behavioral complex—a necessary baseline Lorenz and the ethologists certainly had not established. Behavioral development in “psychologically superior” animals like mammals remained equally influenced by heredity. It was just as false, in other words, to draw a line


between mammals and the rest of the animal kingdom with the assertion that mammals were somehow exempt from biology. But the relationships between biology and behavior in psychologically more complex organisms were increasingly indirect and “plastic” in comparison with protozoa, sponges, insects, and the like. Schneirla accounted for this plasticity by postulating a series of psychological “levels” within the make-up of any individual and extending beyond the individual to the kin-group, hive, clan, or even species. In a revealing disclosure, he pinned his account of levels to contemporary biological theories of epigenetics as formulated by C. H. Waddington and the cybernetic methodology he encountered as a core member of the Macy Foundation Conferences alongside Mead and Bateson.

Though comparative psychologists like Schneirla formulated philosophically sophisticated critiques of ethnography's research program, psychology's resistance to the over-simplified “nativism” displayed by Lorenz and other ethologists during the late 1960s was piecemeal at best. Neuropsychologists touting the ethological line that human brains were neither “whole” nor fully primate, let alone uniquely human, gained a devoted following. Other well-known psychologists—Carl Rogers was but one

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71 Schneirla, “Psychology, Comparative,” p. 49.


73 E.g., Paul D. MacLean and V. A. Kral, A Triune Concept of the Brain and Behaviour (Toronto: Published for the Ontario Mental Health Foundation by University of Toronto Press, 1973).
example—simply ignored the reducibility question as much as possible. Psychology’s response did not set the tone for all social sciences, however.

9.4 Anthropologists Respond to Ethology, 1967–1973

Unlike their fellow social scientists in psychology, anthropologists responded to ethology in a more uniformly negative fashion. This response was no doubt partly due to the long-standing acceptance of an implicit human exceptionalism connected with the concept of culture, as we discussed earlier. Yet, it was clear by the late-1960s that not every anthropologist eschewed biology in favor of culture-only explanations of human behavior. Though he had long since moved past formal affiliations with any anthropology department in the United States, Gregory Bateson remained committed to explanations that took both biology and human cultural behavior equally seriously. He was not alone. Alex Alland, Jr., a respected physical anthropologist with an appreciation for genetics, resisted ethology’s innatist or nativist explanations but did not dismiss genetic influences entirely. And somewhat ironically perhaps, it was the widely read work of a cultural anthropologist, Clifford Geertz, that seemed to calm the turbulent waters between ethnography and ethology in 1973. Indeed, by the time Boas’ most influential students (including Margaret Mead) began retiring from academic life in the early 1970s, anthropology appeared to have come to terms with combinatory biological and cultural explanations for most, if not all, features of the human experience.

Alex Alland, Jr. represented the growing rapprochement between genetics and anthropology. In preparation for his 1967 book *Evolution and Human Behavior*, Alland consulted the established pillars of the Modern Synthesis, Ernst Mayr, G. G. Simpson,
and Theodosius Dobzhansky. Furthermore, Alland attached his book explicitly to the intellectual wave of interest in the relation of anthropology and genetics propagated, according to Alland, not by ethology but by popular works like Waddington’s *The Ethical Animal* and technical anthropological monographs by neo-Marxists like Marvin Harris. Alland’s *Evolution and Human Behavior* represented an attempt to reach the broader anthropological community with the practical implications of genetics and evolutionary biology for their own work. The 1967 work revealed a reliance upon the neo-Darwinians that, if anything, appears retrospectively naïve. With respect to purportedly “simple” behavior of animals, Alland presumed that geneticists “had clearly demonstrated that many behavioral characteristics are genetically determined and that these characteristics follow the classical genetic [i.e., Mendelian] model…. ” More complex human behaviors likewise seemed to have genetic bases. Citing a 1964 collaboration between Julian Huxley, Ernst Mayr, and psychologists Humphrey Osmond and Abram Hoffer, Alland was prepared to count as settled the notion that even complex human mental diseases like schizophrenia could be viewed as genetic adaptations.  

By genetic adaptations, Alland meant, following Huxley and Mayr, that a trait could be described as the “*mean survival value* of a gene.” Though Alland would go on to heavily qualify the explanatory efficacy of much ‘genes-for’ language in

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75 Alland, *Evolution and Human Behavior*, p. 130.


anthropology, such devotion to the Modern Synthesis could hardly be counted as merely a covert endorsement of typical human exceptionalism.

Yet Alland was among the most vehement opponents of ethology’s territorial claims on the behavioral sciences. His *Human Imperative* (1972) attacked Ardrey’s *Territorial Imperative* and Morris’ *Naked Ape* along with the corpus of Lorenz and other ethologists. Primarily, Alland criticized them for oversimplifying anthropology. To Morris’ charge that ethnography merely chronicled “bizarre mating customs, weird ritual procedures, and strange kinship systems”—in other words, the “cultural blind alley[s]” of ethnic groups on the verge of extinction at the hand of European colonial expansion—Alland forcefully responded:

Anthropologists have not spent years away from the comfort, both physical and mental, of their own culture, just to document insignificant distortions of the ‘normal’ pattern of human existence. As humanists, it is true, anthropologists have cataloged the customs of the world’s dying peoples to preserve the richness of the human condition, but as scientists they have had other goals. First and foremost among these has been to demonstrate once and for all that major behavioral differences among the world’s people are due to culture and not biology. This work has gone a long way in destroying the racial myths which guided European and American thinking about human nature since the time of early exploration.

Anthropology was not, according to Alland, a frivolous past time without scientific merit for explaining “real,” i.e., Western and urban, behavior. In fact, as Alland implied, assertions like Morris’ recalled the deterministic conjunction of race and biology undergirding historical accounts of “human nature.” While Morris and other ethologists

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oversimplified the purpose of ethnography, they also implicitly bolstered older prejudices regarding the hierarchical relationship of Europeans to other ethnic groups.

Anthropologists also concurred that the gene-centric biology ethologists utilized in their explanations of human behavior was much too simple. Though Alland had endorsed a relatively unsophisticated version of genetically-determined behavior in *Evolution and Human Behavior*, *Human Imperative* drew from a body of biological work beyond that of Mayr, Huxley, Dobzhansky, and Simpson. However, Alland continued to view his work as a defense of “Darwinian principles in both biology and the social sciences” while simultaneously distancing human behavior from the “overly simplistic approaches” of Lorenz, Ardrey, Morris, and others who would “reduce human behavior to the level of instincts.”80 Affirming the philosophical speculations of existentialists like Karl Jaspers and Erich Fromm, psychologists like Omar K. Moore and Alan Anderson, and the works of human ecologists like Gunnar Myrdal, Alland positioned human culture as “man’s major behavioral adaptation.” With a “capacity for culture” biologically programmed, though not culture itself, humans were free through culture from “strictly biological controls over the development and maintenance of behavioral systems.”81 Genes played an important role at the beginning of the overall cultural system, since they “are responsible for man’s capacity to acquire culture.” But unlike the dictums of the ethologists, who largely parroted the language of gene-centrism, the directional arrow between genes, individual behavior, the environment, and culture pointed in both directions:


Thus different environmental conditions (including education) can produce the same behavioral results on different genetic backgrounds. Individuals with very similar genetic backgrounds can be very different. We must not forget that anthropology has already demonstrated that major (even minor) behavioral differences between human groups depend upon culture.

This does not exclude the possibility that culturally based behavior affects genetic structure and that alterations in genetic patterns may facilitate cultural adaptation to specific environments.\(^2\)

Convinced that ethologists had overstepped scientific warrant when asserting the superiority of biological explanations to cultural ones, Alland went further: not only did culture evolve, it did so as efficiently as any genome.

In human evolution the genetic process has given birth to a new and more efficient form of code system which is capable of rapid change and which can be transferred from organism to organism through learning and is thus freed of slower somatic pathways. This new code system, culture, is as responsive to selective forces as the old one, DNA, and interacts with it to some extent.\(^3\)

Anthropologists were justified in studying “mere” culture, then. For, far from being slaves to their genes, culturally endowed humans could simply opt out of the biological system by inventing some not-strictly-biological adaptation. Lorenz, Morris, Ardrey, and others might be correct in assuming that “territoriality” or “aggression” ruled the animal world. But, according to Alland, humans were under no such compulsion.

Despite works like Alland’s *Human Imperative* that purported to put both human and animal behavior in proper relation to one another and to genetics, the debate over the role of innate behavior in humans continued almost unabated until Clifford Geertz joined the fray. Geertz (1926–2006) visited Bali in the late-1950s with his wife,


\(^3\) Alland, *Human Imperative*, p. 152.
Hildred. The most significant anthropologists to visit that location before them were the recently married Bateson and Mead. So, understandably, the Geertzes viewed their fieldwork through Bateson’s lens: Mead and Bateson’s Balinese Character (1942) had been the first attempt to extensively document through photography the practices of an aboriginal people. There were more connections between the couples: Clifford Geertz became a sensation in the social sciences for analyzing one of Bateson’s favorite cultural moments from his own 1930s expedition, a Balinese cockfight, using Gilbert Ryle’s “thick description” rather than Mead and Bateson’s notion of “character” to report it. Mead thought Bateson’s treatment in the late-’30s too abstract; Geertz constructed a pleasing middle ground between Bateson’s detailed photographer’s eye and his broad theorizing.

Likewise, Geertz tried to forge a middle path between biological determinism and the human exceptionalism that dominated anthropology until the 1960s. In his contribution to Sol Tax’s ambitious 1964 Voice of America radio series on up-and-coming anthropologists, Geertz grappled with a new theory of the origins of culture. Rather than assuming biology led the way and culture followed epiphenomenally from a larger brain, Geertz stressed the parallel development of cultural features and innate,

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84 Beginning in the 1970s, Hildred Geertz began assembling Margaret Mead’s collection of Balinese artwork created when Mead and Bateson were in Bali. This artwork was eventually published two decades later in Hildred Geertz, Images of Power: Balinese Paintings Made for Gregory Bateson and Margaret Mead (Honolulu: University of Hawaii Press, 1994).


86 “Clifford and Hildred Geertz...elaborated] that middle distance that was lacking in our own work, for Gregory was so intent on the faces and the hands of cockfighters that sometimes we had no pictures of the cockfight at all.” Mead, Blackberry Winter, p. 239.
biological tendencies. According to Geertz, by 1964 the “generic constitution” of developing *Homo sapiens* “now appears to be both a cultural and a biological product.”87 Rather than treating the evolution of culture and the evolution of human anatomy and physiology as serial, anthropologists should picture them “emerging together in complex interaction.” Taking this interactionist perspective seriously, scientists would dramatically reinterpret the evolution of humanity because it suggests that man’s nervous system does not merely enable him to acquire culture, it positively demands that he do so if it is going to function at all. Rather than culture acting only to supplement, develop, and extend organically based capacities genetically prior to it, it would seem to be ingredient to those capacities themselves. A cultureless human being would probably turn out to be not an intrinsically talented though unfulfilled ape, but a wholly mindless and consequently unworkable monstrosity.88

Circular causality, as Bateson would have said, appears even in the evolution of *Homo sapiens* and human culture. While biology makes culture, culture makes biology. Geertz’s most important contribution toward settling the nature-nurture divide for social scientists was undoubtedly his “Growth of Culture and the Evolution of Mind.”89 The task he set before himself in the essay was not small; Geertz believed anthropologists must “…find some way in which we can rid ourselves of such a thesis [the development of mental humanity before cultural] without at the same time undermining the doctrine of psychic unity…..”90 In ridding anthropology of a notion of


88 Geertz, “Transition to Humanity,” p. 44.


seriality, Geertz disputed work like that of the new cultural evolutionists (e.g., Leslie A.
White, Marshall Sahlins, etc.) who, like L. H. Morgan and E. B. Tylor long ago,
postulated segregable stages in hominid evolution. “[R]eciprocally interrelated” brain
development and patterned, transferrable social behavior (i.e., culture) could be traced
back to the earliest mammals; primate nervous systems seemed to be “higher” than other
organisms simply by virtue of the number of neurons rather than any qualitative
distinction. So the neurological capacity for something approximating culture, the
superorganic feature of humanity often used as the bastion of human exceptionalism
from Boas onward, existed long before any perceived leap into either humanity or actual
culture use. To Geertz, this meant, first, that culture could not have been superadded
like some vital force bequeathed to an australopithecine predecessor of *Homo sapiens* and,
secondly, that culture did not reduce to biology.  

91 Culture, in other words, *emerged*
unpredictably from the interactions between levels of massed neurons.

Thick-description, the concept for which Geertz was best known—though it
belonged to Ryle—required this kind of biologically informed anti-reductionism.
Anthropology, and by this Geertz meant the study of humankind in its broadest sense,
did not stop by assessing ontology, questions beginning with ‘what is…?’ Rather
anthropologists were after *meaning*, questions that begin from ‘why should anyone care?’

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91 Richards (*Darwin and the Emergence of Evolutionary Theories of Mind and Behavior, Science and*
knowing it Geertz employed a version of James Baldwin’s so-called organic evolution in this essay, which
Richards describes as a “battle against biological reductionists.” I would amend this claim by mentioning
that Geertz’s attack here is not against biological reductionism exclusively, but two dominant
anthropological tropes: White’s 1950s–60s culturism deriving from the Victorian evolutionists we reviewed
in chapter two (above) and Malinowski’s functionalism. In fact, in my reading of Geertz, he is actually
affirming the role of neurological development as an important co-development with culture—a mutual
bio-cultural embeddedness that White discounted or ignored.
But this “obvious truth” had been obfuscated constantly over the history of anthropology either by those who appealed culture as a superorganic vital force or by those who claimed culture reduced to “the brute pattern of behavioral events we observe in fact to occur in some identifiable community or other….⁹² The domain of meaning lay just between these hostile states. Hence, the thick-description approach required some degree of philosophical rigor to balkanize it.

Geertz’s popularity for employing thick-description in ethnography is what made heavily theoretical, at least by anthropology’s standards, essays such as “Growth of Culture” so persuasive to other professionals in the social sciences. And with the publication of his extraordinarily widely read collection, The Interpretation of Cultures (1973), Geertz became something of a spokesperson for “interpretive,” “symbolic,” and “semiotic” approaches to anthropological issues.⁹³ He billed “Growth of Culture” as an explicitly theoretical framework for much of the more popular detailed ethnographic essays. Thus, I would argue, by the early-1970s anthropologists and other social scientists largely accepted the “third-way” conjunction of non-reductive biology and non-superorganic, non-exceptionalist culture. Though it did not sail under the old organicist flag, the “new” organismic with its concern for integrated levels and systems irreducible to simplistic gene inputs persisted into the 1970s.

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9.5 Conclusion

Sociobiology claimed to invalidate the appealing biology-culture détente intrinsic to Geertz’s and Alland’s anthropology. Moreover, the mid-1970s sociobiologists’ insistence that animal behavior was in principle explicable in terms of simple gene changes ignored evidence from Waddington’s epigenetics and Schnierla’s comparative animal psychology. It bypassed Bateson’s cybernetic analysis of communication patterns in organisms, to say nothing of J. P. Scott’s canine-inspired version of sociobiology. In short, the sociobiology represented most obviously in Wilson’s 1975 milestone *Sociobiology: the New Synthesis*, effectively put an ellipsis between the gene-determined development at the heart of the Modern Synthesis and observable animal behavior. The message seemed to be that levels, systems, and the like—concepts featured at conferences like Koestler’s 1968 Alpbach Symposium as well as Bateson’s Wenner-Gren Conference the same year—could be safely ignored.

One of the more robust objections to simplistic genetic determinism from within evolutionary biology fell in the mid-1960s, concomitantly with the popularization of ethology by Ardrey. First Hamilton (1964) and then Williams (1966) critiqued the notion of group selection. 94 Group selection had been offered by Wynne-Edwards in the early-1960s to account for altruism—potentially a major problem for neo-Darwinism. 95 But Hamilton and Williams each demonstrated how, at least in principle, atomic units acting only for their own best interest could explain behavior we typically believe to be

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altruistic; group selection was unnecessary. Hence, even before the publication of Richard Dawkins’ *The Selfish Gene*—which appeared roughly at the same time as Wilson’s *Sociobiology*—many evolutionary biologists found it unproblematic to substitute “is actually the case” for “possible in principle.” Dawkins simply made the slide from individual organism to individual gene less painful.

Implicit in all of this discussion over genes and behavior before 1975 was the knottiness of any answer to the question “Are humans animals, without remainder?” Before 1975, few scientists offered an unqualified “yes” to this question. However, scientists after World War I also frequently dismissed those who offered unqualified ‘no’s. Prior to the extension of the Modern Synthesis to complex human behavior by ethologists and sociobiologists, the answer appeared as a variation on “yes-but” or “no-however.” The trend, given our longer-scale perspective, seems to be that advocates of the “yes-but” position increased in number and prominence as the Modern Synthesis gained rhetorical strength through mid-century.96

*Relating* complex human behavior to animal behavior was neither novel nor especially controversial by the 1970s (think of J. B. Watson’s *Behaviorism*97); reducing human behavior to animal behavior was new with ethology and sociobiology. An additional novelty about Wilson’s mid-’70s sociobiology was the extent to which he openly espoused an unqualified ‘Yes’ to another plaguing question in evolutionary biology.

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biology: “Is animal behavior reducible to genetics?” Because Wilson, Dawkins, and other sociobiologists persuasively answered “yes” to this latter question—despite decades of theoretical work on this very issue by Waddington and the other members of the TBC, among others—as well as “yes” to the question of whether human behavior could be reduced to animal behavior—despite the work of Bateson, Mead, and Geertz, among others—it effectively appeared as if human behavior could be reduced to genetics without remainder. In other words, the door was open to speaking in terms of “genes-for” any number of complex traits. While Wilson, for one, might have had reservations regarding with the implications of these answers, the answers themselves were imbedded within his initial imperialistic claim about biology swallowing the social sciences.98

CONCLUSION

THE NATURE OF MINDS AND OTHER GRAY MATTER(S)

Philosophy of one kind or another cannot be avoided or evaded or given up like sin in Lent. . . It’s no good saying ‘Okay, but I’ve got beyond that stage. I can do without one.’ You can’t, any more than you can do without your DNA genes, although mankind has in important ways ‘gone beyond them’. Some sort of philosophy is a prerequisite for humanity.

—C. H. Waddington (1977)\textsuperscript{1}

If I am right, the whole of our thinking about what we are and what other people are has got to be restructured. This is not funny, and I do not know how long we have to do it in. If we continue to operate on the premises that were fashionable in the pre-cybernetic era, and which were especially underlined and strengthened during the Industrial Revolution, which seemed to validate the Darwinian unit of survival, we may have twenty or thirty years before the \textit{reductio ad absurdum} of our old positions destroys us.

—Gregory Bateson (1970)\textsuperscript{2}

To summarize this dissertation, I turn to the works published by Waddington and Bateson at the end of their careers. Here we find their most wide-ranging attempts to summarize the organicist position they learned in the 1920s and ’30s and articulated through multiple disciplines and in multiple countries for the next forty to fifty years.

\textsuperscript{1} Waddington, \textit{Tools for Thought}, pp. 16–17.

Both Bateson and Waddington adopted a tone of ‘crisis’ and ‘hope’ in these works: a strident call to adopt a more nuanced, holistic philosophy of organisms, societies, and evolution in general combined with optimism that the younger generation would indeed recognize the unsustainability of the older reductionist views. Yet they clearly tempered their optimism—conventional wisdom seemed to militate against the acceptance of a new organicist paradigm in either the life or social sciences.

10.1 Conventional Wisdom or A New Paradigm?

Two years after his death in 1975, Basic Books published Waddington’s *Tools for Thought*. A summary more than a manifesto, *Tools for Thought* nevertheless revealed the serious misgivings Waddington harbored about the trajectory of twentieth century science. Unlike some of his peers—unlike Francis Crick, perhaps—he expressed genuine skepticism that American-led science would be able to address some of the most pressing problems in the planet’s future, those like poverty and hunger on the one hand, overpopulation and natural resource destruction on the other. American science channeled far too much of its intellectual energy into finding answers to short-term techno-scientific questions, some of which actually exacerbated more difficult and longer term problems.\(^1\) Waddington believed that scientists rarely effected longer-term positive social and ecological change even when they hoped for it, because they began from a deficient “worldview.” Whether scientists knew they were acting out of a defective philosophy hardly mattered. In fact, trained to value the creation of new articles and

experiments, even if these merely recapitulated “forgotten” studies, he rather doubted American scientists would be able to self-identify any particular philosophical stance. Instead, he surmised, most followed the “Conventional Wisdom of the Dominant Group.”

“COWDUNG”—Waddington’s “memorable, appropriate and accurate” (and deliberately confrontational) acronym for this conventional scientific outlook—in his view took on an especially pernicious form in the “positivist” United States. By and large, American scientists regarded philosophy as frivolous and denied acting on anything other than the implications of brute fact in their own scientific work. However, Waddington found that many scientists on both sides of the Atlantic actually subscribed to a very basic worldview traceable to Descartes and beyond to the ancient Greeks. Like Descartes and Democritus, most scientists held to the view that the world “essentially consists of things, and that any changes we notice are really secondary, arising from the way things interact with each other.” Other fields might be appropriate for assessing “soul” or some such immaterial force; those fields and that subject matter would not be considered science in the Democritean-Cartesian view. Waddington found a contrary worldview more persuasive. The world, explicated Waddington, “consists of processes,

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1 Waddington, Tools for Thought, p. 16.

2 Philosophers of science had been largely interested in Logical Empiricism for many years by the middle of the twentieth century. The influence of Logical Empiricism likely also has relevance for this story. See, George A. Reisch, How the Cold War Transformed Philosophy of Science to the Icy Slopes of Logic (Cambridge: Cambridge University Press, 2005), pp. 344–68.
and the things that we discern are only stills out of what is essentially a movie.” He identified this view in the work of A. N. Whitehead and beyond to Heraclitus.7

He realized, however, that the Democritean-Cartesian view dominated the sciences; and it was the “common sense” view held by the vast majority of the Western public. Part of the issue undoubtedly had to do with the scale of each science. In geology, astronomy, chemistry, and the like, “It seems much simpler to regard [chemicals, rocks, etc.] not as processes but as things, and to get down to the practical problem of finding out how these things interact with one another, to bring about the essentially secondary processes…. “There are,” Waddington admitted, “many contexts in which the ‘thing’ view is the sensible one to adopt.” But the Democritean-Cartesian scientists did not remain strictly chemists or astronomers. In the early twentieth century, they came into biology and the social sciences; their numbers increased rapidly in the WWII years during which molecular biology germinated. And for these reasons, the thing view, when applied to living organisms usually treated them “as a set of chemical and physical interactions going on between essentially unchanging things…. ” Waddington had, of course, been surrounded by advocates, witting and unwitting, of Democritean-Cartesian “materialism” for his entire post-TBC career.8

This physico-chemical treatment “may indeed lead us to discover a reasonably good account of how the body works as a machine from minute to minute, taking in

6 Waddington, Tools for Thought, p. 18 (emphasis in original).

7 Waddington was certainly correct that he was following the path blazed by Whitehead in particular. Whitehead (Science and the Modern World, p. 18) spent his Lowell Lectures attacking “scientific materialism” as “entirely unsuited to the scientific situation at which we have now [1925] arrived.” Fifty years later, Waddington found himself facing the same opponents and making similar claims.

8 Waddington, Evolution of an Evolutionist, p. 11
food, digesting it, excreting the waste, using the energy of substances it absorbs to carry out various other processes and so on....” But Waddington thought this perspective, no matter how conducive to momentary description and control, got nowhere near the truly interesting questions of organization, development, sentience, and even evolution:

But still, powerful though this [thing] approach is, it has so far really only been successful in connection with some of the questions we want to ask about living things, not all of them. It has given us little understanding of embryonic development; little except some rather empty theories about evolution; and hardly anything at all about the mind.10

Theories about evolution and humans, minds, and the interactions of minded humans had been just the sorts of topics at which Waddington’s processualist approach had been aiming since the 1930s. COWDUNG, at least of the dominant American variety, remained largely devoted to an especially reductionist version of Democritean-Cartesian thing thinking, however. And Waddington could not pretend in this context that his views had been persuasive.

Aside from the feelings displayed in his COWDUNG acronym, Waddington rarely acknowledged his distance from the scientific mainstream in print. He did, however, wonder openly about the resistance to holistic processualism in general. Why had the process view—the organicism held by Whitehead, Woodger, Needham, Bateson, and so many others—not been successful in displacing reductionism? No data, he thought, could univocally determine the truth of either reductionism or anti-reductionism. “Philosophies,” or worldviews, adjudicated between these perspectives. So Waddington looked to the particular worldview of American science for the answer to

9 Waddington, Tools for Thought, pp. 18–19.
10 Waddington, Tools for Thought, p. 20.
why organism did not sprout. There, he found a persuasive, action-oriented, consumerist ethic:

[R]eductionism is a recipe for action...if you are confronted with a complex situation, for instance a living system, your best bet to get some sort of pay-off or other is to look for the physical or chemical factors which can influence the phenomenon in question.¹¹

Democritean-Cartesian reductionism, in other words, made for “lousy philosophy,” according to Waddington. But when what is called for is making a “quick (scientific) buck by discovering some useful practical information,” reductionism trumped multi-level, processual organicism. The holistic processual worldview, he protested, was the “method for making major advances in human comprehension,” like those of “Darwin, Freud, Einstein [and] the quantum physicists.”

On the other hand, reductionism promised finality or predictability, the supposition that scientific discoveries grasped “the whole of what is contained in the reality independent of ourselves.” He regarded this promise as hollow, since it rested on the “Fallacy of Misplaced Concreteness in its simplest form”—a fact American scientists would not see.¹² For Americans, the persuasiveness of reductionist science for action, control, product development, etc., could not be refuted. Indeed, American-dominated fields of astronomy, nuclear physics, chemistry, and now molecular biology were much more precise than the “fuzzy” European-supported sciences of cosmology, psychology,

¹¹ Waddington, Tools for Thought, p. 23.

anthropology, and embryology—even if the latter set could be rendered more quantitative by computers and topography.\textsuperscript{13}

One significant shortcoming of reductionism could not be overlooked, however. Reductionism’s most significant oversight, Waddington thought, was that it treated much of the richness of human society throughout its history as either entirely meaningless or as an epiphenomena of physico-chemical processes. In the Democritean-Cartesian view, things only had value or worth if something superimposed that worth on them externally, as it were. From Waddington’s processual perspective, value or worth constituted the processual nature of nature itself, rather than being superimposed. By this, Waddington referred to a universal principle of emergence—differences in arrangement becoming differences in kind—a topic under consideration in the original Theoretical Biology Club discussions.

Waddington’s worry about the persuasiveness of reductionism had grown just since the 1960s. In his correspondence with Arthur Koestler after the 1968 Alpbach meetings devoted to examining the pervasive reductionism in life science, he was much more confident that the “new paradigm” held by himself, Needham, Woodger, even Paul Weiss, was on its way toward being “adopted by leading biologists.” He attributed the gradual acceptance of the “new” organicist worldview—at that point a full thirty years after the TBC meetings—not to the spread of von Bertalanffy’s “systems approach,” which Waddington regarded as “too sloppy,” but to the persistence of interest in Whitehead and dialectical materialism in the UK. There “reductionism” meant “the attempt to find some simpler elements in terms of which the complex can be

\textsuperscript{13} Waddington, \textit{Tools for Thought}, p. 22.
understood.” Waddington found that formulation of reductionism complementary, or at least not adversarial, to his more holistic worldview. In the United States, however, an alternate—wrongheaded in his view—definition of reductionism reigned: American reductionism “now implies that we start with a full knowledge of the simple elementary units, and, on the basis of this knowledge, can determine what the character of the apparently complex really must be.” The processual organicist, on the other hand, …starts at the other end, as it were; from the basis that we know very little about such elementary units as atoms, molecules, or genes, and what little we do know has been learnt from the study of the complexes into which they enter. We must therefore be prepared to accept additions to our knowledge about them; but equally it is good tactics to refuse to accept any alleged new insight into the properties of our basic units until the evidence really forces us to do so.14

For Waddington, organicists must first be good empiricists. Assumptions that fundamental genetic units give us insight into behavior, for instance, need to be rigorously tested rather than presumed to be true until demonstrably falsified. And any behaviors that can be shown to have a strong genetic basis must still be viewed as integrated parts of “complexes”—structures, more generally—rather than master molecules.

Waddington made this very point—that whole structures determine behavior rather than genes—in a review of Wilson’s Sociobiology for the New York Review of Books in August 1975.15 In one of the most ironic episodes of his career, the anti-sociobiology Science for the People (SftP) organization took his review to be largely

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14 “Comment on Bertallany” [sic], attachment, CHW to Arthur Koestler, 21 Sept 68, UEL-CHW MS 3062.10.

supportive of sociobiology and launched their opposition using Waddington’s review as a springboard.\textsuperscript{16} What Waddington’s critics missed, and what subsequent reprintings of the review leave out, is that his review was of \textit{two} books: Wilson’s \textit{Sociobiology} and \textit{Biogenetic Structuralism} by Charles Laughlin and Eugene G. d’Aquili.\textsuperscript{17} The overall tone of Waddington’s \textit{dual} review is actually critical of Wilson’s genetic reductionism—\textit{Sociobiology: the New Synthesis} was for Waddington a sterling example of the Democritean-Cartesian view. Unfortunately, Waddington died on September 26th, days after SftP published their own condemnatory review of Wilson’s \textit{Sociobiology} in the \textit{New York Review of Books}. Waddington was unable to extricate his own “Mindless Societies” essay from Wilson’s promotion of sociobiology. Historians of this episode have ignored both the substance and the real nature of Waddington’s review and, therefore, have promoted the concept that he was largely supportive of Wilson. However, if we look at Waddington’s full review in the context of his other mid-1970s work, we do not see a member of the “old guard” supporting the defense of his scientific territory after he had retired from the field of battle. Rather, we see a still-active scientist struggling to both his science and his worldview taken seriously by those at the center of the scientific mainstream.

A series of heart attacks in the late-1960s forced Waddington to cut back on his very full traveling and speaking schedule. But he continued to write reviews of books

\textsuperscript{16} Allen, et. al., “Against Sociobiology.”

\textsuperscript{17} The most widely read reprinting of Waddington’s essay appears in Arthur Caplan, ed. \textit{The Sociobiology Debate: Readings on Ethical and Scientific Issues} (New York: HarperCollins, 1978). This version excises the entire second half of the essay, which reviews Laughlin and d’Aquili and connects their discussion of structuralism to Waddington’s Whiteheadian structuralism to critique some of the central themes in Wilson’s \textit{Sociobiology}.
and popular articles even from his hospital bed.\textsuperscript{18} He was motivated in part by the fear that the scientific adoption of organicism that he had once assumed would come to pass was in fact slipping away. Once his generation had quit the field, would the “pre-reductionist Whiteheadian thought” and the deep-seated implications this view had for “general equality [and] freedom” find no one to champion them?\textsuperscript{19} His public lectures and popular articles had already reoriented toward the dire consequences of a worldview that ignored holism and development in favor of the conformity of “units of personnel” which could be more efficiently processed and moved from point to point. In “The Autocatalytic Loss of Freedom,” for instance, Waddington stressed that the push for control over the human and non-human environment inherent in the reductionist worldview was already leading to depersonalization and the curtailing of optimum human flourishing in megapolis environments.\textsuperscript{20}

And in his own version of the Alpbach symposium, “Biology and the History of the Future” held in New York in 1970, Waddington assembled social and life scientists, urban planners, even musicians, to discuss how a processual, structuralist, organicist worldview could combat the rash of social problems that seemed to be sweeping over the Western world.\textsuperscript{21} Consensus at the conclusion of the conference, not surprisingly perhaps, was that the reductionist worldview, though necessary to deal with enormous

\textsuperscript{18} CHW to Margaret Mead, 17 Oct 1973 and Mead to CHW, 19 Oct 1973, LC-SPE/MM Box B17, Folder 2; CHW to GB, 12 Jan 74, UEL-CHW MS 3040.3.

\textsuperscript{19} CHW to Margaret Mead, 17 Oct 1973, LC-SPE/MM Box B17, Folder 2.


aggregates of genes, organisms, people, etc., forced the scientist to ignore individualities, developmental processes, and patterns. Reductionism promoted action, but left out a great deal of important particularities. While probably acceptable, even crucial, when dealing with physico-chemical units, this kind of reductionism could not really grasp the intricacies of growing living things, let alone behavior, social dynamics, or constantly-in-flux ecological systems.

This acute awareness of what genetic reductionism, in this case, brushed aside motivated Waddington’s overlooked or misunderstood critique of Wilson in 1975 as well. In a tone that may have proved to be too subdued for the SftP group, Waddington faulted Wilson for merely waving his hands at genetic assimilation and phenotypic adaptation—Waddington’s favored evolutionary explanations. This was a mistake, according to Waddington, because phenotypic adaptation is merely “another way of referring to learning, by a phrase broad enough to encompass modification of physical structure as well as of behavior.” Wilson refers neither to learning nor to semantic communication, the crucial core of learning between beings more complex than ants (Wilson’s experimental organism of choice and sociobiology’s exemplar society). The origin and development of symbolic communication, from Waddington’s point of view, is more fundamental, and more problematic, for sociobiology than altruism. Even more crucially, Wilson discounted “mentality” or mind on the way to downgrading phenotypic adaptation/learning and semiotic communication. Waddington believed Wilson was “guilty of at the very least a certain incoherence, in indulging himself in a delicious mental frisson of Camusian alienation, while denying [higher organisms]…even a hint of mentality.” For Waddington, Wilson and the other sociobiologists were deliberately obviating direct discussions of semiotic communication, phenotypic
adaptation and learning, as well as any mention of mind, because they did not want to admit the pervasiveness of structure and the importance of development. If they took mind and all that followed from it more seriously, sociobiologists would be forced to concede the inadequacy of their reductionist metaphysics.\footnote{Waddington, “Mindless Societies.”}

In “Mindless Societies” and his 1972 and ’73 Terry Lectures at the University of Edinburgh, Waddington hinted that his definition of “mind” had something to do with evolving biological structure, itself tied to Whitehead’s notion of the “scientific object.” But he had no precise concept of how this relationship could be schematized. His friend Gregory Bateson had been hosting his own conferences, attempting more carefully to define these relationships. In the summer of 1968, while Arthur Koestler readied the Alpbach symposium, Bateson organized a conference on the “Effects of Conscious Purpose on Human Adaptation.” The conference concerned the interconnections of biological structure, mind, pattern, and holism in the vein of Whitehead’s processual philosophy. But, in part because it was supported by the Wenner-Gren Foundation for Anthropological Research, Bateson’s conference extended Waddington’s diagnoses about the problematic foundations of sociobiology in the life sciences. Held at the somewhat remote Burg Wartenstein, Austria, from July 16–25, the conference focused specifically on the problematic tendency of human technological systems to amplify short-term successes into long-term drawbacks. At least that was its initial draw.

Mary Catherine Bateson, the only child of Gregory Bateson and Margaret Mead, immortalized “Effects of Conscious Purpose on Human Adaptation” in her narrative, Our Own Metaphor. According to her retelling, the attendees assembled not simply because they were interested in new information about cybernetics or human evolution—something that seemed on offer, given the promotional materials about the conference and Gregory Bateson’s own interests at the time. Rather, they sensed in his invitation, or “Memorandum,” a call to identify and correct “systematic distortions of view which, when implemented by modern technology, become destructive of the balances between individual man, human society, and the ecosystem of the planet.” Bateson, according to his daughter, had become a kind of prophet.

It is somewhat more difficult to say, however, exactly what kind of prophet he was. In his various attempts to formulate a coherent position paper leading up to the official Memorandum, Bateson labored to synthesize evolutionary thought and behavior from Darwin to contemporary ethology, with Whiteheadian philosophy and other still less well defined ideas. An early draft for his position took as its title “Consciousness vs. Nature.” In it, Bateson defined “nature” as not “red in tooth and claw” but a complex network in which competition and mutual dependency are closely combined.” A later version stressed interdependence even more: “Personally, I would take a larger view and say that in all evolving systems, the ultimate unit of survival is not organisms but

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organisms plus environment. (Conservationists, please note.)" These initial drafts seem to be authored by a semi-romantic environmentalist enamored with the old notion of species or group cooperation found in works like Peter Kropotkin’s Mutual Aid. By the July conference itself, however, Bateson had transformed into a slightly more detailed, technically minded, and less optimistic host and interlocutor. Two things had changed in the intervening months since his first communications. First, Bateson had ceased believing that the problem of besieged ecosystems could be solved by just talking about the content of the problem. Somehow the context of the discussion had to change as well. Secondly, he had begun questioning whether his initial research questions were broad enough. He had started thinking about human minds versus natural systems. But was he already making an assumption about how those things were and were not connected with one another and with the greater environment? Perhaps, as he suggested to Wenner-Gren liaison officer Lita Osmundsen just after the first Burg Wartenstein conference, their next conference needed to have loftier goals:

We might probe the implication of what seems to me to have been the central conclusion of our first conference, that all action based upon untrue premises is irrational even though it may be adaptive in the short time perspective; that mind is immanent and not transcendent; the premise of transcendent mind is wrong, therefore all decisions based upon this premise are irrational and lead in the end to pathologies of the total system, reductionism, destruction of the ecosystem, etc., etc.27

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26 GB to Wenner-Gren conference participants, 12 Mar. 1968, UCSC-GB MS 98, Box 36, Folder 1478.

27 GB to Lita Osmundsen, 13 Sept. 1968, UCSC-GB MS 98, Box 36, Folder 1478.
The notion that “mind” is immanent rather than transcendent—part of the system rather than above the system—proved to be the cornerstone of his final, synthetic evolutionary work, *Mind and Nature.*

*Mind and Nature* opens a window onto the intellectual and cultural person of Gregory Bateson more than any of his other works. Like *Naven,* it is peppered with guilty admissions of uncertainty, poignant asides, dry wit, and cleverly turned phrases; unlike the tentative tone in his 1936 anthropological monograph, *Mind and Nature* exudes confidence even in its uncertainty and occasional opacity. The man that breathes through the text in *Mind in Nature* is scholarly, to be sure—even polymathic. But his erudition springs from older and more deeply dug wells than some of his contemporaries. The Bateson of *Mind and Nature* is passionate about the applicability of his worldview—about the potentially apocalyptic consequences that follow from not adopting his worldview, in fact—while simultaneously eschewing the “action man” impulsiveness of, say, a Carl Rogers. He is the man who brings a crab to class because, though it is the right teaching tool, no one else would do so.

However, despite the seemingly counter-cultural sagacity of its author, none of the content in *Mind and Nature* was particularly novel. Moreover, little of it originated with Bateson. He admitted this, of course: he saw his role as that of a latter day Johann

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28 Interestingly, Bateson first made this point when confronted by a dissatisfied Wenner-Gren conference member, Fred Attnave, a psychologist from the University of Oregon. All in all, Attnave regarded Bateson’s conference as “too fluffy,” without the kind of reporting of experiments and reading of papers that Attnave expected. He did receive something “at the hard intellectual level,” however. As Bateson reminded him, they had a collective epiphany: “namely, that all decisions based upon the premise of transcendent mind are *ipso facto* irrational. That, and the beginnings of a realization of how this irrationality ramifies through our civilization and links up with our maltreatment of both the negro and Lake Erie.” Bateson reminded Attnave that these were, indeed, the main points of the conference. Fred Attnave to GB, 26 Aug. 1968, and GB to Fred Attnave, 30 Aug. 68, UCSC-GB MS 98, Box 36, Folder 1480.
Gottfried Herder, or perhaps a Charles Darwin—great synthesists—rather than a Gregor Mendel or a William Bateson—rigorous, even punctilious, fact-finders. Though Gregory Bateson conducted experiments, of sorts, his chief contribution would be to bring together disparate intellectual territory under the same banner and to adopt and clarify ideas that were already buzzing about in the air.29

Bateson placed his clarification (or, more properly, reconfiguration) of neo-Darwinian evolutionary theory at the heart of Mind and Nature. He very much saw himself as a formidable dissenter from the Modern Synthesis.30 And, like Waddington’s “post-Darwinism” critique, Bateson underscored the importance of both genetic assimilation and epigenetics to the whole evolutionary process from monad to man. He emphasized the degree to which this evidence was being excluded from neo-Darwinism. Bateson leaned heavily upon Waddington’s Drosophila work in order to make his own evolutionary arguments, just as Waddington had referenced the Balinese ethnographic work of Bateson and Mead in his own forays into social science, urban planning, and ethics.31 Each clearly saw the scientific work of the other as supporting similar theoretical positions in parallel realms. In his reconfiguration of neo-Darwinism, Bateson upheld the direct influence of environment upon the genome typically held to be neo-

29 Margaret Mead highlighted this depiction of Bateson in contrast to herself—in part to explain why she was not as widely known or well-regarded as she was. Margaret Mead, “End-Linkage: A Tool for Cross-Cultural Analysis,” in About Bateson: Essays on Gregory Bateson, ed. John Brockman (New York: Dutton, 1977), pp. 171–2.


31 Including, prominently, Science and Ethics (1942) and The Ethical Animal (1961). See: CHW to Mead, 15 Apr 1959, where Waddington thanks Mead for her help in straightening out this aspect of his work (LC-SPE/MM, Box B17, Folder 1).
Lamarckian. Interestingly, he even criticized Arthur Koestler for repopularizing neo-Lamarckism, and for improperly posing William Bateson as his ultra-Darwinian foil, in *The Case of the Midwife Toad.* However, like Waddington, he left open the question regarding whether and how quickly environmental (which he explicitly termed “ecological”) conditions would impact upon the creation of phenotypes from the genome and, therefore result in phyletic evolutionary change, albeit indirectly.

There are always at least two levels of process working in organismal development and phyletic evolution, suggested Bateson: (a) the morphological/structural or “epigenetic;” and (b) the phenotypical or “adaptive.” Each level presents a “maze of tests” to which any novelty, internal or external, is subject. The evolving organism, then, is the junction between two interacting cybernetic systems: one between the internal mechanics of the developmental system; and the other between the phenotypic result of that system and the ecosystem of climate, resources, other organisms. In the grand “Janus-faced” process of evolution the two interlocking systems of form and process, or calibration and feedback, work to hold deep structures constant while allowing constant creation and dissolution of more superficial traits. Bateson called this a

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32 “…if Lamarckian inheritance were the rule or even at all common, the whole system of interlocking stochastic processes would come to a halt.” G. Bateson, *Mind and Nature,* p. 142.


34 “No doubt the substitution of genetic for somatic control (regardless of the question of heredity [Darwinian or Lamarckian]) will always diminish the flexibility of the individual. The option of somatic change in that particular characteristic will be wholly or partly lost. But the general question still remains: Does it never pay to substitute genetic for somatic control? If this were the case, the world would surely be a very different place from that which we experience. Likewise, if [pan-adaptive] Lamarckian inheritance were the rule, the whole process of evolution and living would become tied up in the rigidities of genetic determination. The answer must be between these extremes…” G. Bateson, *Mind and Nature,* p. 144.

discontinuity in logical typing. In practice this discontinuity prevents immediate fixation of adaptive somatic traits into nearly permanent genomic ones and, vice versa, filters or “interprets” genomic change so that mutations in DNA cannot be strictly determinative.\textsuperscript{36} Though genes can be many things, they cannot be the basis for morality.\textsuperscript{37}

When applied to the notion of “learning,” Bateson saw interesting consequences in his interlocking system metaphor. For instance, he insisted that when speaking of human cognitive traits,

\begin{quote}
…it is nonsensical to ask: Is the given characteristic of that organism determined by its genes or by somatic change or learning? There is no phenotypic characteristic that is unaffected by the genes. The more appropriate question would be: At what level of logical typing does genetic command act in the determining of this characteristic? The answer to this question will always take the form: At one logical level higher than the observed ability of the organism to achieve learning or bodily change by somatic process. Because of this failure to recognize logical typing of genetic and of somatic change, almost all comparisons of “genius”…degenerate into nonsense.\textsuperscript{38}
\end{quote}

Here Bateson attempted to capture the same intuition as that of Waddington about the paucity of sociobiology when tied to naïve gene-centrism. What genes determine, thought both Bateson and Waddington, are genotypic \textit{spectra, domains, ranges, or habits}, not phenotypic characteristics.\textsuperscript{39} Characteristics—the actual units exposed to selection,

\begin{quote}
\textsuperscript{36} G. Bateson, \textit{Mind and Nature}, p. 71.

\textsuperscript{37} The first chapter of E. O. Wilson’s Sociobiology is entitled “The Morality of the Gene.” For one of the most philosophically persuasive arguments against the sociobiologists’ “normative biologist,” see Donald T. Campbell, “Comments on the Sociobiology of Ethics and Moralizing,” \textit{Behavioral Science} 24, no. 1 (1979): 37–46.


\textsuperscript{39} G. Bateson, \textit{Mind and Nature}, p. 207.
\end{quote}
the driving force of evolution—are results of processes that, though strongly influenced by genes, cannot (or should not) be expressed univocally in terms of genetics. For Bateson, there were too many “logical types” between DNA and soma, between genes and skin. Organisms and therefore evolution itself, could only be comprehended as an interlocking, hierarchical, structural system. Changes began at “concrete” levels—the soma—and moved to “more abstract” levels—the genes.\textsuperscript{40} As evo-devo advocates say presently: phenotype leads, genotype follows.

In summary, Bateson and Waddington argued—in contrast to their contemporaries on both sides of the anthropology-biology divide—against two popular positions. First, they asserted that biology \emph{is} of major importance to humans. Culture, in this case, cannot be understood \emph{merely} in terms of culture. But biological determinism is likewise naïve. Thus they also argued that any sociobiological counterproposal is no less misleading: human organisms cannot be univocally described in terms of their genes, nor the study of human behavior subsumed under the study of genes in populations. Human mentality, culture, behavior, or whatever term we choose to use is multi-leveled, processual, emergent, and pluralistic in its constituents. The organic holism in our scientific theories is a reflection of the real state of the world; reductionism is a useful heuristic, a necessary fiction of analysis, and the occasionally problematic result of our epistemological limitations as finite, evolved beings made for social interactions.

\textsuperscript{40} G. Bateson, \emph{Mind and Nature}, p. 147.
10.2 The Paradox of Legacies

Paradoxically, while C. H. Waddington has in more recent decades become a kind of patron saint of evo-devo for his observations of genetic assimilation, developmental constraint, canalization, etc., historians of the life and social sciences (outside of psychiatry and communications theory) almost completely ignore similar claims made by Gregory Bateson. Why should this be the case?

Surely the novelty of their ideas presented a reception problem for both of them equally. And, in any case, that novelty was not truly novel: though seemingly “before” or “after” their time (depending on the judge’s perspective), both Waddington and Bateson produced work consistent with other respected Oxbridge-trained organicists of the 1920s (e.g., G. Evelyn Hutchinson, Joseph and Dorothy Needham, Dorothy Wrinch, etc.). They were, in other words, both Whiteheadian structuralists expounding the organicist views of the 1920s forged in the TBC and in their interactions with one another and with Mead. Certainly, their scientific concepts conflicted with the mainstream in evolutionary biology and anthropology of the 1940s through ’60s, but, again, this seems to be a shared problem, not one that would fall unequally on Bateson.

One major distinction between Bateson and Waddington is the way in which their views were received toward the end of their lives. Or, more properly, who took up their concepts and who opposed them. We have already seen that both Waddington and Bateson were somewhat misunderstood by contemporaries in the fields that they thought most central to their interests—Waddington by Francis Crick and June Goodfield Toulmin, Bateson by Carl Rogers and the client-centered psychologists. Curiously, this mattered far more for Waddington than Bateson in the shorter term.
Waddington, despite his broad connections across biological subdisciplines, received no formal *Festschrift*; no major biography was ever published of his life and works. The one major biographical notice that was published, in the *Biographical Memoirs of the Royal Society*, was handled by a colleague at the University of Edinburgh, Alan Robertson. Robertson was not especially familiar with Waddington’s pre-War work and evidently did not think highly of his more speculative biology.  

Other colleagues were less skeptical of his science, but still did not see Waddington as a major figure. David R. Newth, developmental biologist at the University of Glasgow and editor of the *Journal of Embryology and Experimental Morphology* found Waddington’s musings interesting. But he was not uniformly admiring of Waddington; he later apologized to Joseph Needham for the statement in Newth’s “remembrance” that “Waddington was casual to the point of irresponsibility in matters that failed to interest him.”  

In part, Newth accounted this lack of interest to Waddington’s multifaceted background in geology and genetics along with experimental embryology. Waddington, to put it another way, did not have the pedigree of a typical senior scientist; Newth did not comment on Waddington’s consistent pursuit of evolutionary and structural concepts across disciplinary boundaries.  

Yet accounting for the evolution of structure was clearly the organizing theme of Waddington’s life. In the mid-1970s, he assembled, edited, and published the papers that seemed most definitive of his legacy as *Evolution of an Evolutionist*—an intentionally assertive title given his peripheral standing in the scientific community at the time.

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42 D. R. Newth to J. Needham (no date), CUL-JN M.95.
Excepting his brief introductory autobiographical sketch where he pronounced the influence of Cambridge biology and Whitehead (but not the Needhams, the TBC, or Gregory Bateson) on his subsequent interests, Waddington selected the papers that would position himself simultaneously as an orthodox biologist and a heterodox contender with the neo-Darwinian establishment. He interleaved, for example, the pre-war canalization work and mid-1950s papers explaining genetic assimilation of acquired characters, with a more or less standard rehearsal of developmental genetics for “good friend” Theodosius Dobzhansky’s seventieth birthday *Festschrift*.43 When we consider foundational theoretical biologists of the late-1960s—published in the impressive four-volume collection *Theoretical Biology*—the Waddington of *Evolution of an Evolutionist* seems decidedly conservative.44 Those unfamiliar with the rest of his career or the robustness of the neo-Darwinian backdrop could be forgiven for regarding him, as philosopher Marjorie Grene apparently did, as a regular biologist but perhaps not a “wholly orthodox mechanical materialist.”445


In the paper written after the Waddington-organized 1967 International Union of Biological Scientists meeting in Serbelloni, Italy, Grene actually lumped Waddington with other conventional neo-Darwinians including John Maynard-Smith. Waddington had attacked, on more than one occasion, the neo-Darwinism that Maynard-Smith claimed “has not as yet been refuted.” Clearly Waddington and Maynard-Smith believed they had significant theoretical conflicts with one another. Grene, perhaps reading out of Waddington’s “unaggressive character” saw the two as fairly closely related. “Almost everyone used, constantly and as self-evident, the term ‘biological’ as synonymous with ‘functional,’ ‘adaptive’, ‘conducive to survival’—strictly, in evolutionary terms, conducive to leaving descendants—or ‘produced by Natural Selection’…even Waddington, though not *quite* orthodox,
Broadly speaking, the image of Waddington impressed upon the world both by his colleagues and by himself at the end of his life and career was the image of a philosophically astute biologist with a plethora of “social parts” that were kept “in more-or-less water-tight compartments.” His other, more unorthodox interests—as a Cambridge organicist with laboratory training in Weimar Berlin and interest in bridging the mechanism-vitalism debate, as a comrade of anthropologists like Margaret Mead and Gregory Bateson, as an adroit observer and promotor of modernist architecture and artwork, as a concerned voice for global environmentalism, and so on—were relegated to footnotes. In other words, Waddington remained a scientist. And, largely because of his marginal status to the neo-Darwinian synthesis, his work has retained only historical interest.  

Gregory Bateson, by contrast, became an intellectual figurehead. Though plagued with funding problems throughout his career (at least in part because of his unwillingness to settle down into a traditional academic position), Bateson traveled over the course of the 1950s and '60s from the San Francisco Bay region to the Caribbean islands to Hawaii and back. His first established academic post since the pre-war years appears in his summarizing notes as an interesting mutant of the same species.” Marjorie Grene, “Bohm’s Metaphysics and Biology,” in Towards a Theoretical Biology—2. Sketches, ed. C. H. Waddington, I.U.B.S. Symposia (Chicago: Aldine Publishing Company, 1969), p. 65. It seems Grene’s critique at Serbelloni spurred Waddington to disclose his Whitehead-inspired processual structuralism more vociferously in his final works. Perhaps her noted irascibility even inspired Waddington’s own controversial “COWDUNG” critique (which admittedly was somewhat out-of-character for him) in Tools for Thought.

46 Alan Robertson to GB, 7 May 1976 and return, 19 August 1976 UCSC-GB MS 98, Box 36, Folder 1447.

47 He seemed to sense his growing irrelevance. In his final article, published posthumously, Waddington decried the widespread popularity of reductionist ontologies. Thankfully, Waddington had read Whitehead and had grasped his “major thesis” against “The Bifurcation of Nature” into Mind and Matter, set against one another as two totally separable and incomparable essences” before Whitehead had been “pushed out of fashion by Wittgenstein and Russell.” Waddington, “Fifty Years On,” p. 21.
was as an anthropologist at the University of Hawaii in 1969. He came back to mainland North America as a faculty member of the new Kresge College at the University of California, Santa Cruz in 1974. Entirely unexpectedly, he found himself regarded by radical youth in northern California not as the eccentric, elderly British dolphin trainer in Hawaiian shirts who quoted spontaneously from *Principia Mathematica*, but as the resident philosopher of an emerging radical movement.⁴⁸

Initially, this role fit him nearly as awkwardly as his attire.⁴⁹ He found the “touchy-feely jargon” of his California students “horrible” and their academic motivation questionable.⁵⁰ But over his years at the University of Hawaii (until 1973) and then at UC Santa Cruz (until 1978), he grew to enjoy the attention. Just before joining the UCSC faculty, he had published his first major collection of papers as *Steps to an Ecology of Mind*.⁵¹ Using a construct of six more coherent themes, stretching from his early anthropological work to the communications and epistemological studies of the 1960s, the collection clearly illuminated processual-structuralist fibers running through his otherwise farraginous career. This collection launched him into a kind of academic stardom, at least among younger scholars in the social sciences. A few of them began to

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⁴⁸ In the preface to the 1972 edition of *Steps to an Ecology of Mind* (see n. 45, below), Bateson’s student Mark Engel commended Bateson to his youthful peers: “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” (p. ix).

⁴⁹ “I begin to feel embarrassed when I stand up in a big auditorium where a third of the audience have a sort of primary assumption that they will agree with me. It is…rather strange and improbably that ideas which my father was groping for should have become almost fashionable. And it feels very strange for me, accustomed to be a voice crying in the wilderness, to find myself more famous than infamous.” GB quoted by David Lipset, *Gregory Bateson*, p. 281.


organize a *Festschrift* for Bateson, an honor that he initially resisted. But as momentum
gathered, Bateson envisioned an “Introduction to Gregory Bateson” instead of a
traditional end-of-career book.\(^2\) In the middle of this project, which eventually became a
less ambitious *About Bateson*, germs for another project grew. Bateson initially called it
“The Evolutionary Idea.” But in the rich soil of new relationships with the UCSC
faculty, undergraduates and others outside, it sprouted and grew into two projects: *Mind
and Nature*, and a never-completed manuscript that Mary Catherine Bateson eventually
sculpted into *Angels Fear*.\(^3\) American intellectuals on the west coast began identifying
Bateson with a widespread trend of structuralist and “general systems” perspectives in
the social sciences.\(^4\) (Claude Lévi-Strauss made this observation years earlier, connecting
his work with Bateson’s structuralism.\(^5\)) Bateson debated with luminaries in public
settings. A 1975 exchange with Jonas Salk inspired famed psychologist Rollo May to
exclaim that “In future centuries when the theory of humanistic psychology is written, I
am sure we will find it very ‘Batesonian’.”\(^6\) So popular had Bateson become by the mid-

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\(^2\) John Brockman to GB, 16 July, 16 August, and 23 August, 1974, UCSC-GB MS 98, Box 22,
Folder 955.

\(^3\) Notebooks 43 (1970), 51 (1973), and 52 (1973), UCSC-GB MS 98, illustrate the effects of these
relationships on his final publications quite well. Gregory Bateson, and Mary Catherine Bateson, *Angels
Fear: Towards an Epistemology of the Sacred* (Cresskill, N.J.: Hampton Press, 2005 [originally printed by
Macmillan, 1987]).

\(^4\) See the exchange between GB and Anthony Wilden (at UC San Diego), 8–21 October 1969,
UCSC-GB MS 98, Box 37, Folder 1497. Wilden’s “Project 1971–72” was eventually published as, Anthony
2003).

\(^5\) “Works such as your Social Structure of the Iatmul and *Naven* have always loomed large in my
thinking and I was especially glad [o] hear the other day that a French translation of the latter is
contemplated by a publisher in Paris.” Claude Lévi-Strauss to GB, 21 May 1967, UCSC-GB MS 98, Box 19,
Folder 845.

\(^6\) Rollo May to GB, 17 April 1975, UCSC-GB MS 98, Box 21, Folder 923.
1970s that only rapidly failing health pulled him away from the classroom and the lecture circuit in 1978.

Ironically, it was this ten-year string of successes in Hawaii and California that destined Bateson for future marginalization from histories of evolutionary biology and from the debate over sociobiology in the late-1970s—the arenas upon which he was attempting to have a major impact. Despite his desire to be taken seriously as an evolutionary scientist, New Age advocates were the ones who extolled his holistic approach as something springing from latent mysticism and promoted Bateson’s mind-nature hylozoic monism as their own. \(^\text{57}\) Though initially published by Macmillan, his late theoretical works *Mind and Nature* and *Angels Fear* were reprinted under Bantam’s “New Age” inexpensive paperback imprints alongside popular books like Robert Pirsig’s *Zen and the Art of Motorcycle Maintenance*, Louis Thomas’ *The Medusa and the Snail*, and John Heider’s *Tao of Leadership*. So even as “action men” in psychology like Carl Rogers misunderstood Bateson as supporting the notion of an organized “self,” as we saw in chapter five, New Age advocates such as John C. Lilly and others saw his work as a kind of transcendental philosophy resembling the thought of Aldous Huxley and Timothy Leary. \(^\text{58}\) Bateson’s critics saw this connection with the New Age movement as reflective of the hollow core of his philosophy and science. For them, treatises such as *Mind and

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\(^\text{57}\) This treatment as New Age guru—rather than cybernetician, psychologist, or anthropologist began as early as 1962, when John C. Lilly convinced Bateson to set up his dolphin communication lab in the Virgin Islands. Lilly promoted Bateson as a kind of scientist interested in traditionally extra-scientific issues, partially in order to solicit funds for the Communication Research Institute of St. Thomas. See correspondence between GB and John Lilly from 1961–63, UCSC-GB MS 98, Box 20, Folders 856 to 858.

*Nature* emphasized merely that “process is important and that everything is connected to everything else….,” Consequently, “the book is indeed a grand but empty synthesis of everything,” “pretentious and muddled,” and so on.\(^{59}\)

In large part, it was Bateson’s transdisciplinary “grand synthesizing” and reliance upon holism that inspired criticism. Historically, American scientists and philosophers have looked at holism quite skeptically.\(^{60}\) But even those that supported his holism or anti-reductionism (at least in part) sensed the impact of “eclectic theories and mystical philosophizing” on Bateson as he convalesced in the “intellectual lotus-land” of the greater San Francisco region.\(^{61}\) Not being an accepted member of mainstream evolutionary biology, anthropology, or psychology relegated Bateson to “mainstream” New Age theorist status. From there, his concepts of organicism—and those of Waddington’s—he promoted and connected to the critique of neo-Darwinistic sociobiology—could be safely ignored.

### 10.3 Summation

It has been an argument of this dissertation that the point of disconnect between Bateson, Waddington, and those more centrally located in the social and life sciences—

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the extra-scientific reason for their marginalization—has to do with an intransigent conflict in worldviews. Bateson and Waddington on the one hand (and here they were joined by a few others over the mid-twentieth century) consistently held to a version of holism. I have alternately called their version of holism organicism when applied to individual organisms or developmental sequences (“occasions of experience,” in Waddington’s terminology) or processual structuralism when applied to evolving systems and social networks. More recently, historians and philosophers of science have called this style of holism emergent or non-reductive physicalism (which they occasionally also call organicism). The holism of Waddington and Bateson—whatever we choose to name it—contrasted with a generally mechanist-reductionist program in the British, and especially American, life sciences throughout the last century.

But it also contrasted with the implicit dualism in the social sciences—both sides of the intractable “radical inconsistency” in Western thought Whitehead pointed out in

62 Needham maintained that organicism persisted through the twentieth century—largely through the work of Waddington—and was undergoing a rebirth because of the work of neo-Darwinism’s challengers in the last two decades of the century. Joseph Needham, “Foreword,” in Beyond Neo-Darwinism: An Introduction to the New Evolutionary Paradigm, ed. Mac-Wan Ho, and P. T. Saunders (London: Academic Press, 1984).

1925. From the debates between Johann Gottfried von Herder and Immanuel Kant in the eighteenth century, through Alfred Russel Wallace’s compromise with the London “anthropologicals” in the nineteenth, into the work of A. C. Haddon, W. F. R. Rivers, and the structuralists, functionalists, and Boasians after them, anthropology struggled to classify humans and human culture relative to non-human animals. Bateson and Waddington’s organicism emphasized the bio-psychological unity of all aspects of humans with their non-human predecessors. Culture, though crucial to understanding *Homo sapiens*, was but one part of the human phenotype, and need not be framed in its own effectively biology-free terms, as even “evolutionists” such as Leslie White had insisted.

Sociobiology was an overt attempt at unified biology and the life sciences. But Wilson and other sociobiologists made this unification by fiat: by taking the unique qualities out of social behavior, even in humans, and reducing social structures to perturbations in hormones, neural networks, and, ultimately, strands of DNA. “Consilience”—Wilson’s favored term for the unity of life and social sciences—really meant “assimilation.” Social terms would be translated into biological terms without remainder. But the sociobiologists’ version of biology was gene-centric; the genes, selfish; and the organism, epiphenomenal. Though perhaps interesting to observe in a museum or zoo, the selfish-gene-organism was merely a sophisticated way for DNA to replicate itself in various niches across the biosphere. Form, mind, person, organism—these were all intricate and beautiful ways of dressing-up biomolecules. Though they were at the

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ends of their lives by the time Wilson popularized sociobiology—drastically altering it from its original formulation by J. P. Scott and T. C. Schneirla—Waddington and Bateson followed colleagues interested in systems biology, organicism, and structuralism in dismissing sociobiology as another manifestation of ancient reductionism run amok.

Reductionism, as Waddington expressed quite clearly, has its place among the great worldviews. Like holism, reductionism has been an expansive organizing concept, something larger and more persuasive, even if more ephemeral, than a reified theory or idea—perhaps a kind of tacit perception of the world. 66 Empirical evidence seems to confirm reductionism. However, Bateson, Waddington, and their fellow travelers found plenty of evidence on which to rest their holistic organicism as well. One moral is clear in this case: empirical analysis alone cannot adjudicate between these views in the end. Holism and reductionism are meta-theories, larger than any body of evidence, underdetermined by science—even the kind of science that spans generations. 67 Or perhaps it is better to describe worldviews as cultural concepts acting as practically immovable cores of multiple “research programs”; though reductionism and holism may


67 June Goodfield (Toulmin) exposts this more cynically, “The arguments for reductionism and antireductionism seem irrelevant to what is actually done in the laboratory, mere echoes from the sidelines whose impact and influence are effectively nil.” June Goodfield, “Changing Strategies: A Comparison of Reductionist Attitudes in Biological and Medical Research in the Nineteenth and Twentieth Centuries,” in Studies in the Philosophy of Biology: Reduction and Related Problems, ed. Francisco J. Ayala and Theodosius Dobzhansky (Berkeley, CA: University of California Press, 1974), p. 65. It seems that here again Goodfield takes onboard the perspective of Peter Medawar—who critiqued her paper in its symposium form before it was published. When she asked Medawar whether theory mattered even in selecting concepts, research questions, etc., “I made the suggestion…that the attitude with which a man approached biology might help him to focus on certain problems that otherwise might be ignored. Medawar’s “answer was a flat negative…it made no difference at all. A man was a good scientist or he was not” (see June Goodfield’s “Postscript in the light of discussions at Serbelloni,” pp. 85–6 in the same volume). She clearly had not recalled her earlier exchanges with Waddington and Needham over how much their belief in holism did influence the organizer science and after.
be viewed in some sort of gestalt framework, they certainly outlast any individual paradigm in the Kuhnian sense. Preference for reductionism or holism transcends nations, ethnicities, even religions, while being integral to all three.

I have intended this dissertation as an exploration of a conflict in a particular corner of the expansive scientific fabric and the impact of that conflict on two scholars who seemed to not “fit” with their colleagues. As it turns out, this is but one episode in a human conversation—sometimes bellicose, occasionally conciliatory—predating Bateson, Waddington, organicism, anthropology, evolutionary biology, science, philosophy, perhaps even human history itself.

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SELECTED REFERENCES

Archival sources


LC-SPE/MM: United States Library of Congress—South Pacific Ethnography/Margaret Mead Papers, Washington, DC, US.

SIA: Smithsonian Institute Archives, Washington, DC, US.


UCSC-GB: University of California, Santa Cruz, McHenry Library—Gregory Bateson Papers, Santa Cruz, CA, US.


Published sources


———. “Social Structure of the Iatmul People of the Sepik River (Concluded).” *Oceania* 2, no. 4 (1932): 401–58.


[———. *With a Daughter’s Eye: a Memoir of Margaret Mead & Gregory Bateson*. New York: Perennial, 1984.]


———. “Speciation as a Stage in Evolutionary Divergence.” *The American Naturalist* 74, no. 753 (1940): 312–21


———. Organism and Environment as Illustrated by the Physiology of Breathing. The Silliman Lectures at Yale University for 1915. Yale University Press, 1917.


Kirschenbaum, Howard, and Valerie Land Henderson, (eds.) *Carl Rogers—


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Morgan, Lewis Henry. League of the Ho-De-No-Sau-Nee, or Iroquois. Rochester, NY: Sage, 1851


———.


