

4

ROSS ASHBY

PSYCHIATRY, SYNTHETIC BRAINS,
AND CYBERNETICS

HAVING DECIDED (HEAVEN FORGIVE ME, BUT IT IS MY CONVICTION) TO FOLLOW IN DARWIN'S FOOTSTEPS, I BOUGHT HIS AUTOBIOGRAPHY TO GET SOME HINTS ON HOW TO DO IT.

ROSS ASHBY, JOURNAL ENTRY, 29 JUNE 1945 (ASHBY 1951-57, P. 1956)

William Ross Ashby (fig. 4.1), always known as Ross, was born in London on 6 September 1903.¹ After failing the entrance exam for the City of London School, he finished his schooling at the Edinburgh Academy between 1917 and 1921 and then graduated from Sidney Sussex College, Cambridge, with a BA in zoology in 1924. He was an unhappy child, incapable of living up to the expectations of a demanding father, and this unhappiness remained with him for many years.² Ashby's father wanted him to pursue a career in either medicine or the law and, opting for the former, on leaving Cambridge Ashby trained at St. Bartholomew's Hospital, receiving the M.B. and B.Ch. degrees in 1928 (qualifying him to practice as a doctor) and the M.D. degree in 1935, both from Cambridge. In 1931 he was awarded a diploma in psychological medicine by the Royal College of Physicians and Surgeons. From 1930 to 1936 he was employed by London County Council as a clinical psychiatrist at Leavesden Mental Hospital in Hertfordshire. In 1931 Ashby married Elsie Maud Thorne—known to her intimates as Rosebud; Mrs. Ashby to others; born in 1908; employed at that point in the Millinery Department at Liberty's



Figure 4.1. W. Ross Ashby. (By permission of Jill Ashby, Sally Bannister, and Ruth Pettit.)

on Regent Street—and between 1932 and 1935 they had three daughters, Jill, Sally, and Ruth.

From 1936 to 1947 Ashby was a research pathologist at St. Andrew's mental hospital in Northampton, an appointment he continued to hold while serving from 1945 until 1947 as a specialist pathologist in the Royal Army Medical Corps with the rank of lieutenant and later major. From June 1945 until May 1946 he was posted to India, in Poona and Bangalore. Returning to England, he became director of research at another mental institution, Barnwood House in Gloucester, in 1947 and remained there until 1959, when he was appointed director of the Burden Neurological Institute in Bristol, succeeding Frederick Golla and becoming Grey Walter's boss. In January 1961, after just a year at the Burden, Ashby moved to the United States to join the University of Illinois (Urbana-Champaign) as a professor in the Department of Electrical Engineering, primarily associated with Heinz von Foerster's Biological Computer Laboratory (BCL) but with a joint appointment in biophysics. He remained at the BCL until his retirement as an emeritus professor in 1970, when he returned to Britain as an honorary professorial fellow at the University of Wales, Cardiff. He died of a brain tumor shortly afterward, on 15 November 1972, after five months' illness.

Ashby's first recognizably cybernetic publication, *avant la lettre*, appeared in 1940. In the mid-1940s he began to make contact with other protocyberneticians, and in 1948 at Barnwood House he built the cybernetic machine for which he is best remembered, the homeostat, described by Norbert Wiener (1967 [1950], 54) as "one of the great philosophical contributions of the present day." The concept of adaptation staged by the homeostat, different from Walter's, will echo through the following chapters. Over the course of his career, Ashby published more than 150 technical papers as well as two enormously influential books: *Design for a Brain* in 1952 and *An Introduction to Cybernetics* in 1956, both translated into many languages. From the homeostat onward, Ashby was one of the leaders of the international cybernetics community—a founding member of the Ratio Club in Britain, an invitee to the 1952 Macy cybernetics conference in the United States, and, reflecting his stature in the wider world of scholarship, an invited fellow at the newly established Center for Advanced Study in the Behavioral Sciences in Palo Alto, California, in 1955–56. After moving to Illinois, he was awarded a Guggenheim Fellowship in 1964–65, which he spent back in England as a visiting research fellow at Bristol University.³

Ashby's contributions to cybernetics were many and various, and I am not going to attempt to cover them all here. Speaking very crudely, one can distinguish three series of publications in Ashby's oeuvre: (1) publications relating to the brain that one can describe as distinctly cybernetic, running up to and beyond *Design for a Brain*; (2) distinctly medical publications in the same period having to do with mental pathology; and (3) more general publications on complex systems having no especial reference to the brain, running roughly from the publication of *An Introduction to Cybernetics* and characterizing Ashby's later work at Illinois. My principle of selection is to focus mostly on the first and second series and their intertwining, because I want to explore how Ashby's cybernetics, like Walter's, developed as brain science in a psychiatric milieu. I will explore the third series only as it relates to the "instability of the referent" of the first series: although Ashby's earlier work always aimed to elucidate the functioning of the brain, normal and pathological, he developed, almost despite himself, a very general theory of machines. My object here is thus to explore the way that Ashby's cybernetics erupted along this line into a whole variety of fields, but I am not going to follow in any detail his later articulation of cybernetics as a general science of complex systems. This later work is certainly interesting as theory, but, as I have said before, I am most interested in what cybernetics looked like when put into practice in real-world projects, and here the natural trajectory runs from Ashby's cybernetic

brain not into his own work on systems but into Stafford Beer's management cybernetics—the topic of the next chapter.

The skeleton of what follows is this. I begin with a brief discussion of Ashby's distinctly clinical research. Then I embark on a discussion of the development of his cybernetics, running through the homeostat and *Design for a Brain* up to the homeostat's failed successor, DAMS. Then I seek to reunite these two threads in an exploration of the relation between Ashby's cybernetics and his clinical work up to the late 1950s. After that, we can pick up the third thread just mentioned, and look at the extensions of Ashby's research beyond the brain. Finally, I discuss echoes of Ashby's work up to the present, in fields as diverse as architecture, theoretical biology and cellular automata studies. Throughout, I draw heavily upon Ashby's handwritten private journal that he kept throughout his adult life and various notebooks, now available at the British Library in London.⁴

The Pathological Brain

When one reads Ashby's canonical works in cybernetics it is easy to imagine that they have little to do with his professional life in medicine and psychiatry. It is certainly the case that in following the trajectory of his distinctive contributions to cybernetics, psychiatry recedes into the shadows. Nevertheless, as I will try to show later, these two strands of Ashby's research were intimately connected, and, indeed, the concern with insanity came first. To emphasize this, I begin with some remarks on his medical career.

Overall, it is important to remember that Ashby spent his entire working life in Britain in mental institutions; it would be surprising if that milieu had nothing to do with his cybernetic vision of the brain. More specifically, it is clear that Ashby, like Walter, belonged to a very materialist school of psychiatry led in Britain by Frederick Golla. Though I have been unable to determine when Ashby first met Golla and Walter, all three men moved in the same psychiatric circles in London in the mid-1930s, and it is probably best to think of them as a group.⁵ It is clear, in any event, that from an early date Ashby shared with the others a conviction that all mental phenomena have a physical basis in the brain and a concomitant concern to understand the go of the brain, just how the brain turned specific inputs into specific outputs. And this concern is manifest in Ashby's earliest publications. At the start of his career, in London between 1930 and 1936, he published seventeen research papers in medical journals, seeking in different ways to explore con-

nections between mental problems and physical characteristics of the brain, often based on postmortem dissections. Such writings include his very first publication, "The Physiological Basis of the Neuroses," and a three-part series, "The Brain of the Mental Defective," as well as his 1935 Cambridge MA thesis, "The Thickness of the Cerebral Cortex and Its Layers in the Mental Defective" (Ashby 1933, 1935; Ashby and Stewart 1934–35).

Such research was by no means untypical of this period, but it appears to have led nowhere. No systematic physiological differences between normal and pathological brains were convincingly identified, and Ashby did not publish in this area after 1937.⁶ After his move to St. Andrew's Hospital in 1936, Ashby's research into insanity moved in several directions.⁷ The January 1937 annual report from the hospital mentions a survey of "the incidence of various mental and neurological abnormalities in the general population, so that this incidence could be compared with the incidence in the relatives of those suffering from mental or neurological disorders. . . . Dr. Ashby's work strongly suggests that heredity cannot be so important a factor as has sometimes been maintained" (Ashby 1937a). The report also mentions that Ashby and R. M. Stewart had studied the brain of one of Stewart's patients who had suffered from a rare form of brain disease (Ashby, Stewart, and Watkin 1937), and that Ashby had begun looking into tissue culture methods for the investigation of brain chemistry (Ashby 1937b). Ashby's pathological work continued to feature in the January 1938 report, as well as the fact that "Dr. Ashby has also commenced a study on the theory of organisation as applied to the nervous system. It appears to be likely to yield interesting information about the fundamental processes of the brain, and to give more information about the ways in which these processes may become deranged"—this was the beginning of Ashby's cybernetics, the topic of the next section.

According to the St. Andrew's report from January 1941, "Various lines of research have been undertaken in connection with Hypoglycaemic Therapy. Drs. Ashby and Gibson have studied the effects of Insulin as a conditioned stimulus. Their results have been completed and form the basis of a paper awaiting publication. They are actively engaged also in studying various metabolic responses before and after treatment by Insulin and Cardiazol. The complications arising from treatment by these methods are being fully investigated and their subsequent effects, if any, carefully observed. It is hoped to publish our observations at an early date." Here we are back in the realm of the great and desperate psychiatric cures discussed in the previous chapter. Insulin and cardiazol were used to induce supposedly therapeutic convulsions in

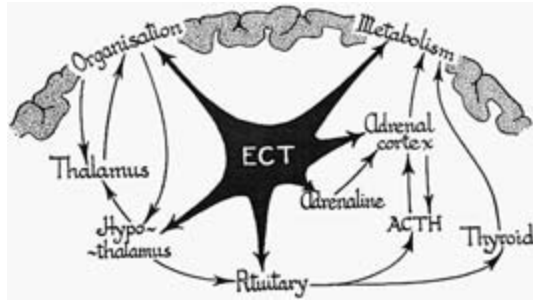


Figure 4.2. "The most important variables affected by E.C.T." Reproduced with permission from W. R. Ashby, "The Mode of Action of Electro-convulsive Therapy," *Journal of Mental Science*, 99 (1953), 203, fig. 1. (© 1953 The Royal College of Psychiatrists.)

mental patients, and we can note that in this work Ashby had moved from his earlier interest in the pathological brain *per se* to the biological mechanisms of psychiatric treatment.

This shift in focus intensified after Ashby's move to Barnwood House in 1947. Not far from the Burden Neurological Institute, Barnwood House was at the epicenter of radical psychiatric cures in Britain. Its director, G. W. T. H. Fleming, was the first author listed, with Golla and Walter, on the first published report on the use of electroconvulsive therapy in Britain (Fleming, Golla, and Walter 1939, discussed in the previous chapter). Ashby had no doubts about the efficacy of ECT: "Electroshock therapy . . . has long passed its period of probation and is now universally accepted as active and effective." "Yet," he wrote, "its mode of action is still unknown." From its introduction there had been speculation that ECT achieved its ends not directly, via the shock itself, but by inducing some therapeutic change in the chemistry of the brain, and this was what Ashby sought to elucidate at Barnwood House, most notably in a long essay on his empirical research published in 1949, which won a prize—the £100 Burlingame Prize awarded by the Royal Medico-Psychological Association. There, Ashby reported on his own observations on fourteen mental patients who had been subjected to ECT and concluded, "The usual effect of convulsive therapy is to cause a brisk outpouring of adrenal chemical steroids during the first few days of the treatment. . . . There is evidence that [this] outpouring . . . is associated with a greater tendency to clinical recovery" (Ashby 1949a, 275, 321). Again, we see the characteristic concern to illuminate the material "go of it"—now to spell out the beginning of a chain of effects leading from the administration of electroshock to modified mental performances. And Ashby followed this up in, for example, a 1953

paper entitled “The Mode of Action of Electro-convulsive Therapy,” in which he reported his own research on rats subjected to ECT, using an assay of his own devising to explore ECT’s effects on the “adenohypophyseal-adrenocortical system” (Ashby 1953a; see also Ashby 1949b for earlier rat experiments on this topic).

It is clear, then, that Ashby was actively involved in a certain kind of clinical psychiatric research well into his fifties, trying to understand the material peculiarities of pathological brains and how therapeutic interventions worked. This was his professional life until he left Britain in 1961, and I will come back to it. Now, however, we can move to a more rarefied plane and explore the development of Ashby’s distinctive cybernetic understanding of the brain.

Ashby’s Hobby

Shortly after Ashby’s death, his wife wrote to Mai von Foerster, Heinz’s wife and a family friend at the University of Illinois:

I came across a very private notebook the other day written in 1951. In it Ross wrote: After I qualified, work on the brain, of the type recorded in my notebooks, was to me merely a delightful amusement, a hobby I could retreat to, a world where I could weave complex and delightful patterns of pure thought, untroubled by social, financial or other distractions. So the work which I had treated for years only as a hobby began to arouse interest. I was asked to broadcast about it in March, 1949. My fear is now that I may become conspicuous, for a book of mine is in the press. For this sort of success I have no liking. My ambitions are vague—someday to produce something faultless.⁸

The notebook in question is “Passing through Nature,” Ashby’s biographical notebook, written between 1951 and 1957 (see note 4).⁹ The broadcast Ashby referred to was a thirty-minute program on BBC radio, “Imitating the Brain,” transmitted on 8 March 1949, for which he was paid twenty-six pounds and five shillings (i.e., twenty-five guineas) plus fifteen shillings and threepence rail fare; the book is *Design for a Brain*, which appeared in 1952.¹⁰ My aim now is to trace out the evolution of the strand of Ashby’s early work that led up to and included *Design*. I am interested in its substance and how it emerged from the hobbyist shadows to establish Ashby’s reputation as one of the world’s leading cyberneticians. In a biographical note from 1962 Ashby wrote that “since 1928 Ashby has given most of his attention to the problem: How can the brain be at once mechanistic and adaptive? He obtained the solution in

1941, but it was not until 1948 that the Homeostat was built to embody the special process. . . . Since then he has worked to make the theory of brainlike mechanisms clearer” (Ashby 1962, 452). I will not try to trace out the evolution of his thinking from 1928 onward; instead, I want to pick up the historical story with Ashby’s first protocybernetic publication. As I said, Ashby’s clinical concerns are very much marginalized in his key cybernetic works, which focus on the normal rather than the pathological brain, but we can explore the interconnections later.

Ashby’s first step in translating his hobbyist concerns into public discourse was a 1940 essay entitled “Adaptiveness and Equilibrium” published in the *Journal of Mental Science*. In a journal normally devoted to reports of mental illness and therapies, this paper introduced in very general terms a dynamic notion of equilibrium drawn from physics and engineering. A cube lying on one of its faces, to mention Ashby’s simplest example, is in a state of dynamic equilibrium inasmuch as if one tilts it, it will fall back to its initial position. Likewise, Ashby noted, if the temperature of a chicken incubator is perturbed, its thermostat will tend to return it to its desired value. In both cases, any disturbance from the equilibrium position calls forth opposing forces that restore the system to its initial state. One can thus say that these systems are able to *adapt* to fluctuations in their environment, in the sense of being able to cope with them, whatever they turn out to be. Much elaborated, this notion of adaptation ran through all of Ashby’s later work on cybernetics as brain science, and we can note here that it is a different notion from the one I associated with Walter and the tortoise in the previous chapter. There “adaptation” referred to a sensitive spatial engagement with the environment, while for Ashby the defining feature of adaptation was finding and maintaining a relation of dynamic equilibrium with the world. This divergence lay at the heart of their different contributions to cybernetics.

Why should the readers of the *Journal of Mental Science* be interested in all this? Ashby’s idea unfolded in two steps. One was to explain that dynamic equilibrium was a key feature of life. A tendency for certain “essential variables” to remain close to some constant equilibrium value in the face of environmental fluctuations was recognized to be a feature of many organisms; Ashby referred to the pH and sugar levels of the blood and the diameter of the pupil of the eye as familiar examples. Tilted cubes and thermostats could thus be seen as formal models for real organic adaptive processes—the mechanisms of *homeostasis*, as it was called, though Ashby did not use that word at this point. And Ashby’s second step was to assert that “in psychiatry its importance [i.e., the importance of adaptiveness] is central, for it is precisely the loss of this

‘adaptiveness’ which is the reason for certification [i.e., forcible confinement to a mental institution]” (478). Here he tied his essay into a venerable tradition in psychiatry going back at least to the early twentieth century, namely, that madness and mental illness pointed to a failure to adapt—an inappropriate mental fixity in the face of the flux of events (Pressman 1998, chap. 2). As we saw, Walter’s *M. docilis* likewise lost its adaptivity when driven mad.

Ashby’s first cybernetic paper, then, discussed some very simple instances of dynamic equilibrium and portrayed them as models of the brain. One is reminded here of Wiener’s cybernetics, in which feedback systems stood in as model of the brain, and indeed the thermostat as discussed by Ashby was none other than such a system. And two points are worth noting here. First, a historical point: Ashby’s essay appeared in print three years before Arturo Rosenblueth, Wiener, and Julian Bigelow’s classic article connecting servomechanisms and the brain, usually regarded as the founding text of cybernetics. And second, while Rosenblueth, Wiener, and Bigelow (1943) thought of servomechanisms as models for purposive action in animals and machines, Ashby’s examples of homeostatic mechanisms operated below the level of conscious purpose. The brain adumbrated in Ashby’s paper was thus unequivocally a performative and precognitive one.

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I quoted Ashby as saying that he solved the problem of how the brain can be at once mechanistic and adaptive in 1941, and his major achievement of that year is indeed recorded in a notebook entitled “The Origin of Adaptation,” dated 19 November 1941, though his first publication on this work came in an essay submitted in 1943 and only published in 1945, delayed, no doubt, by the exigencies of war (Ashby 1945a). The problematic of both the notebook and the 1945 publication is this: Some of our biological homeostatic mechanisms might be given genetically, but others are clearly acquired in interaction with the world. One of Ashby’s favorite adages was, The burned kitten fears the fire. The kitten *learns* to maintain a certain distance from the fire—close enough to keep warm, but far away enough not get to burned again, depending, of course, on how hot the fire is. And the question Ashby now addressed himself to was how such learning could be understood mechanistically—what could be the go of it? As we have seen, Walter later addressed himself to the question of learning with his conditioned reflex analogue, CORA. But Ashby found a different solution, which was his first great contribution to brain science and cybernetics.

The 1945 essay was entitled “The Physical Origin of Adaptation by Trial and Error,” and its centerpiece was a strange imaginary machine: “a frame

with a number of heavy beads on it, the beads being joined together by elastic strands to form an irregular network.” We are invited to think of the positions and velocities of the beads as the variables which characterize the evolution of this system in time, and we are invited also to pay attention to “the constants of the network: the masses of the beads, the lengths of the strands, their arrangement, etc. . . . These constants are the ‘organization’ [of the machine] by definition. Any change of them would mean, really, a different network, and a change of organization.” And it is important to note that in Ashby’s conception the “constants” can change; the elastic breaks if stretched too far (Ashby 1945a, 15–16).¹¹

The essay then focuses on the properties of this machine. Suppose we start it by grabbing one of the beads, pulling it against the elastic, and letting go; what will happen? There are two possibilities. One is that the whole system of beads and elastic will twang around happily, eventually coming to a stop. In that case we can say that the system is in a state of dynamic equilibrium, as defined in the 1940 essay, at least in relation to the initial pull. The system is already adapted, as one might say, to that kind of pull; it can cope with it.

But now comes the clever move, which required Ashby’s odd conception of this machine in the first place. After we let go of the bead and everything starts to twang around, one of the strands of elastic might get stretched too far and break. On the above definition, the machine would thus change to a different state of organization, in which it might again be either stable or unstable. In the latter case, more strands would break, and more changes of organization would take place. And, Ashby observed, this process can continue indefinitely (given enough beads and elastic) until the machine reaches a condition of stable equilibrium, when the process will stop. None of the individual breaks are “adaptive” in the sense of necessarily leading to equilibrium; they might just as well lead to new unstable organizations. In this sense, they are *random*—a kind of nonvolitional trial-and-error process on the part of the machine. Nevertheless, the machine is *ultrastable*—a technical term that Ashby subsequently introduced—inasmuch as it tends inexorably to stable equilibrium and a state of adaptedness to the kinds of pull that initially set it in motion. “The machine finds this organization automatically if it is allowed to break freely” (1945a, 18).

Here, then, Ashby had gone beyond his earlier conception of a servo-mechanism as a model for an adaptive system. He had found the solution to the question of how a machine might become a servo relative to a particular stimulus, how it could learn to cope with its environment, just as the burned kitten learns to avoid the fire. He had thus arrived at a far more sophisti-

cated model for the adaptive and performative brain than anyone else at that time.

The Homeostat

The bead-and-elastic machine just discussed was imaginary, but on 19 November 1946 Ashby began a long journal entry with the words “I have been trying to develop [sic] further principles for my machine to illustrate stability, & to develop ultrastability.” There followed eight pages of notes, logic diagrams and circuit diagrams for the machine that he subsequently called the homeostat and that made him famous. The next entry was dated 25 November 1946 and began: “Started my first experiment! How I hate them! Started by making a Unit of a very unsatisfactory type, merely to make a start.”¹² He then proceeded to work his way through a series of possible designs, and the first working homeostat was publicly demonstrated at Barnwood House in May 1947; a further variant was demonstrated at a meeting of the Electroencephalographic Society at the Burden Neurological Institute in May 1948.¹³ This machine became the centerpiece of Ashby’s cybernetics for the next few years. His first published account of the homeostat appeared in the December 1948 issue of the journal *Electronic Engineering* under the memorable title “Design for a Brain,” and the same machine went on to feature in the book of the same name in 1952. I therefore want to spend some time discussing it.

The homeostat was a somewhat baroque electromechanical device, but I will try to bring out its key features. Figure 4.4a in fact shows four identical homeostat units which are all electrically connected to one another. The interconnections cannot be seen in the photograph, but they are indicated in the circuit diagram of a single unit, figure 4.4c, where it is shown that each unit was a device that converted electrical inputs (from other units, on the left of the diagram, plus itself, at the bottom) into electrical outputs (on the right). Ashby understood these currents as the homeostat’s essential variables, electrical analogues of blood temperature or acidity or whatever, which it sought to keep within bounds—hence its name—in a way that I can now describe.

The inputs to each unit were fed into a set of coils (A, B, C, D), producing a magnetic field which caused a bar magnet (M) to pivot about a vertical axis. Figure 4.4b is a detail of the top of a homeostat, and shows the coils as a flattened oval within a Perspex housing, with the right-hand end of the bar magnet just protruding from them into the light. Attached to the magnet and rotating with it was a metal vane—the uppermost element in figures 4.4b and 4.4c—which was bent at the tip so as to dip into a trough of water—the

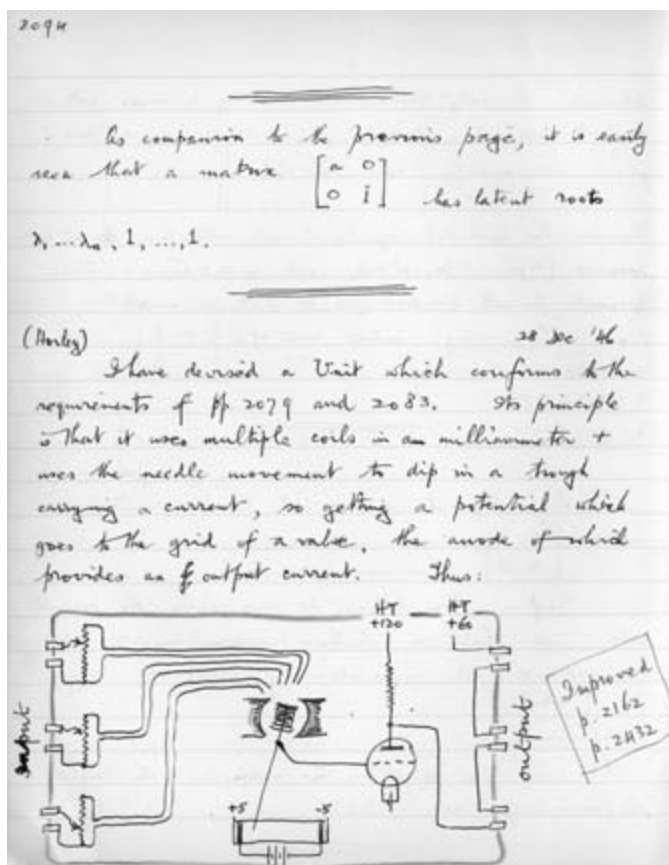


Figure 4.3. Page from Ashby's journal, including his first sketch of the homeostat wiring diagram. Source: Journal entry dated 28 December 1946 (p. 2094). (By permission of Jill Ashby, Sally Bannister, and Ruth Pettit.)

curved Perspex dish at the front of figure 4.4b, the arc at the top of figure 4.4c. As indicated in figure 4.4c, an electrical potential was maintained across this trough, so that the tip of the vane picked up a voltage dependent on its position, and this voltage then controlled the potential of the grid of a triode valve (unlabeled: the collection of elements enclosed in a circle just below and to the right of *M* in figure 4.4c; the grid is the vertical dashed line through the circle), which, in turn, controlled the output currents.

Thus the input-output relations of the homeostat except for one further layer of complication. As shown in figure 4.4c, each unit could operate in one of two modes, according to the setting of the switches marked *S*, the lower row of switches on the front of the homeostat's body in figure 4.4a. For one

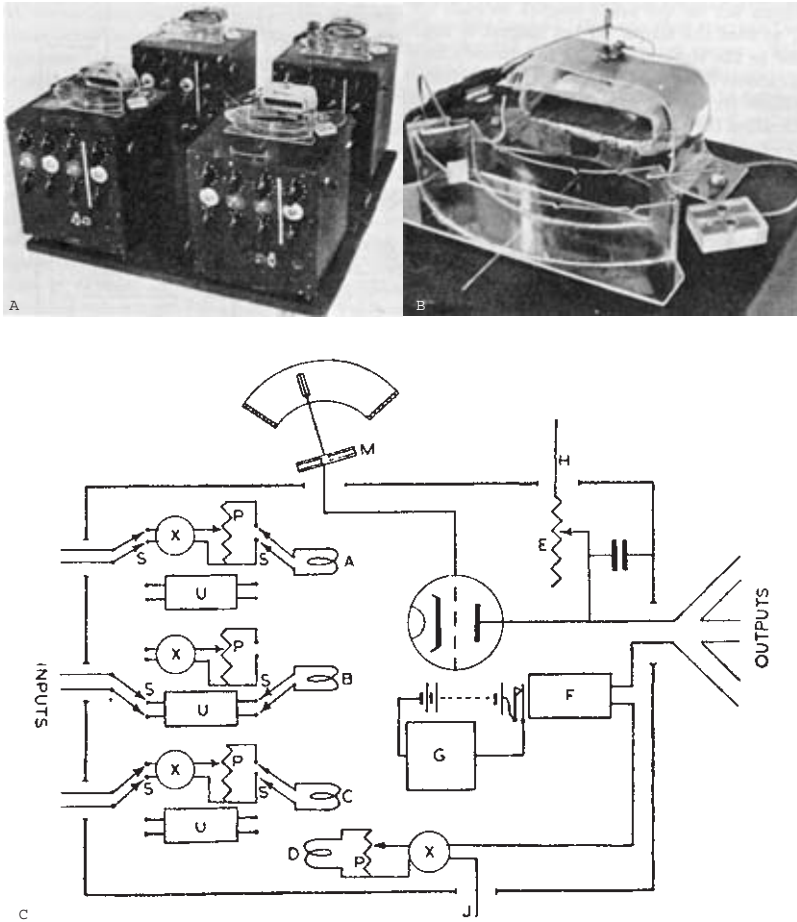


Figure 4.4. The homeostat: a, four interconnected homeostats; b, detail of the top of a homeostat unit, showing the rotating needle; c, circuit diagram. Source: W. R. Ashby, "Design for a Brain," *Electronic Engineering*, 20 (December 1948), 380, figs. 1, 2. (With kind permission from Springer Science and Business Media.)

setting, the input current traveled to the magnet coil through a commutator, X, which reversed the polarity of the input according to its setting, and through a potentiometer, P, which scaled the current according to its setting. The settings for P and X were fixed by hand, using the upper and middle set of knobs on the front of the homeostat in figure 4.4a. More interesting, the switch S could also be set to route the input current through a "uniselector" or "stepping switch"—U in figure 4.4c. Each of these uniselectors had twenty-five positions, and each position inserted a specific resistor into the input

circuit, with the different values of the twenty-five resistances being “deliberately randomised, the actual numerical values being taken from a published table of random numbers” (Ashby 1948, 381). Unlike the potentiometers and commutators, these uniselectors were not set by hand. They were controlled instead by the internal behavior of the homeostat. When the output current of the unit rose beyond some preset limit, relay *F* in figure 4.4c would close, driving the unselector (via the coil marked *G*) to its next setting, thus replacing the resistor in the input circuit by another randomly related to it.

So what? The first point to bear in mind is that any single homeostat unit was quite inert: it did nothing by itself. On the other hand, when two or more units were interconnected, dynamic feedback interrelations were set up between them, as the outputs of each unit fed as input to the others and thence returned, transformed, as input to the first, on and on, endlessly around the loop. And to get to grips with the behavior of the whole ensemble it helps to specialize the discussion a bit. Consider a four-homeostat setup as shown in figure 4.4a, and suppose that for one of the units—call it homeostat 1—the switch *S* brings a unselector into the input circuit, while for the three remaining homeostats the switches *S* are set to route the input currents through the manually set potentiometers and commutators. These latter three, then, have fixed properties, while the properties of homeostat 1 vary with its unselector setting.

When this combination is switched on, homeostat 1 can find itself in one of two conditions. It might be, as Ashby would say, in a condition of *stable* equilibrium, meaning that the vane on top of the unit would come to rest in the middle of its range, corresponding by design to zero electrical output from the unit, and return there whenever any of the vanes on any of the units was given a small push. Or the unit might be *unstable*, meaning that its vane would be driven toward the limits of its range. In that event, the key bit of the homeostat’s circuitry would come into play. As the electrical output of the unit increased above some preset value, the relay would close and drive the unselector to its next position. This, in effect, would change the electrical properties of homeostat 1, and then we can see how it goes. The unit might again find itself in one of two conditions, either stable or unstable. If the latter, the relay would again drive the unselector to its next position, inserting a new resistance in the circuit, and so on and so on, until homeostat 1 *found* a condition of stable equilibrium in which its vane gravitated to the center of its range.

This is the key point about the homeostat: it was a real ultrastable machine of the kind that Ashby had only imagined back in 1941. The uniselectors took

the place of the bands that broke in the fantasy machine of his 1945 publication (with the added advantage that the uniselectors were always capable of moving to another position, unlike elastic bands, which never recover from breaking). Started off in any configuration, the homeostat would *randomly reorganize* itself to find a condition of dynamic equilibrium with its environment, without any external intervention.

The homeostat was, then, a major milestone in Ashby's twenty-year quest to understand the brain as a machine. Now he had a real electromechanical device that could serve in understanding the go of the adaptive brain. It was also a major development in the overall cybernetic tradition then crystallizing around Wiener's *Cybernetics*, also published in 1948.¹⁴ I want to pause, therefore, to enter some commentary before returning to the historical narrative—first on ontology, then on the social basis of Ashby's cybernetics.

The Homeostat as Ontological Theater

ASHBY'S BRILLIANT IDEA OF THE UNPURPOSEFUL RANDOM MECHANISM WHICH SEEKS FOR ITS OWN PURPOSE THROUGH A PROCESS OF LEARNING IS . . . ONE OF THE GREAT PHILOSOPHICAL CONTRIBUTIONS OF THE PRESENT DAY.

NORBERT WIENER, *THE HUMAN USE OF HUMAN BEINGS*,
2ND ED. (1967 [1950]), 54

THERE CAN'T BE A PROPER THEORY OF THE BRAIN UNTIL THERE IS A PROPER THEORY OF THE ENVIRONMENT AS WELL. . . . THE SUBJECT HAS BEEN HAMPERED BY OUR NOT PAYING SUFFICIENTLY SERIOUS ATTENTION TO THE ENVIRONMENTAL HALF OF THE PROCESS. . . . THE "PSYCHOLOGY" OF THE ENVIRONMENT WILL HAVE TO BE GIVEN ALMOST AS MUCH THOUGHT AS THE PSYCHOLOGY OF THE NERVE NETWORK ITSELF.

ROSS ASHBY, DISCUSSION AT THE 1952 MACY CONFERENCE
(ASHBY 1953B, 86-87)

My ontological commentary on the homeostat can follow much the same lines as that on the tortoise, though I also want to mark important differences. First, like the tortoise, the homeostat stages for us an image of an immediately performative engagement of the brain and the world, a little model of a performative ontology more generally. Again, at the heart of this engagement was a process of random, trial-and-error search. The tortoise physically explored

its environment, finding out about distributions of lights and obstacles; the homeostat instead searched its inner being, running through the possibilities of its inner circuitry until it found a configuration that could come into dynamic equilibrium with its environment.

Next we need to think about Ashby's modelling not of the brain but of the world.¹⁵ The world of the tortoise was largely static and unresponsive—a given field of lights and obstacles—but the homeostat's world was lively and dynamic: it was, as we have seen, more homeostats! If in a multiunit setup homeostat 1 could be regarded as a model brain, then homeostats 2, 3, and 4 constituted homeostat 1's world. Homeostat 1 perturbed its world dynamically, emitting currents, which the other homeostats processed through their circuits and responded to accordingly, emitting their own currents back, and so on around the loop of brain and world. This symmetric image, of a lively and responsive world to be explored by a lively and adaptive brain, was, I would say, echoing Wiener, the great philosophical novelty of Ashby's early cybernetics, its key feature.

As ontological theater, then, a multihomeostat setup stages for us a vision of the world in which fluid and dynamic entities evolve together in a decentered fashion, exploring each other's properties in a performative back-and-forth dance of agency. Contemplation of such a setup helps us to imagine the world more generally as being like that; conversely, such a setup instantiates a way of bringing that ontological vision down to earth as a contribution to the science of the brain. This is the ontology that we will see imaginatively elaborated and played out in all sorts of ways in the subsequent history of cybernetics.¹⁶ Biographically, this is where I came in. In *The Mangle of Practice* I argued that scientific research has just this quality of an emergent and performative dance of agency between scientists and nature and their instruments and machines, and despite some evident limitations mentioned below, a multihomeostat setup is a very nice starting point for thinking about the ontological picture I tried to draw there. It was when I realized this that I became seriously interested in the history of cybernetics as elaborating and bringing that ontological picture down to earth.

Three further remarks on homeostat ontology might be useful. First, I want simply to emphasize that relations between homeostats were entirely noncognitive and nonrepresentational. The homeostats did not seek to know one another and predict each other's behavior. In this sense, each homeostat was unknowable to the others, and a multihomeostat assemblage thus staged what I called before an *ontology of unknowability*. Second, as discussed in chapter 2, paradigmatic modern sciences like physics describe a world of fixed en-

tities subject to given forces and causes. The homeostat instead staged a vision of fluid, ever-changing entities engaged in trial-and-error search processes. And a point to note now is that such processes are intrinsically *temporal*. Adaptation happens, if it happens at all, in time, as the upshot of a temporally extended process, trying this, then that, and so on. This is the sense in which the homeostat adumbrates, at least, an *ontology of becoming* in which nothing present in advance determines what entities will turn out to be in the future. This is another angle from which we can appreciate the nonmodernity of cybernetics. Third, we could notice that the brain/world symmetry of Ashby's setups in fact problematized their specific reference to the brain. We can explore Ashby's response to this later, but to put the point positively I could say now that this symmetry indexes the potential *generality* of the homeostat as ontological theater. If the phototropism and object avoidance of the tortoise tied the tortoise to a certain sort of brainlike sensing entity, very little tied the homeostat to the brain (or any other specific sort of entity). A multihomeostat configuration could easily be regarded as a model of a world built from any kind of performatively responsive entities, possibly including brains but possibly also not. Here, at the level of ontological theater, we again find cybernetics about to overflow its banks.



So much for the general ontological significance of the homeostat. As in the previous chapter, however, we should confront the point that Ashby, like Walter, aimed at a distinctly modern understanding of the brain: neither of them was content to leave the brain untouched as one of Beer's exceedingly complex systems; both of them wanted to open up the Black Box and grasp the brain's inner workings. Ashby's argument was that the homeostat was a positive contribution to knowledge of how the performative brain adapts. What should we make of that? As before, the answer depends upon the angle from which one looks. From one angle, Ashby's argument was certainly correct: it makes sense to see the homeostat's adaptive structure as a model for how the brain works. From another angle, however, we can see how, even as modern science, the homeostat throws us back into the world of exceedingly complex systems rather than allowing us to escape from it.

The first point to note is, again, that Ashby's science had a rather different quality from that of the classical modern sciences. It was another instance of explanation by articulation of parts (chap. 2): if you put together some valves and relays and uniselectors *this way*, then the whole assemblage can adapt performatively. Ashby's science thus again thematized performance, at

the level of parts as well as wholes. Second, and again like Walter's, Ashby's science was a science of a heterogeneous universe: on the one hand, the brain, which Ashby sought to understand; on the other, an unknown and cognitively unknowable (to the homeostat) world. Performative interaction with the unknowable was thus a necessary constituent of Ashby's science, and in this sense the homeostat returns us to an ontology of unknowability. And, third, a discovery of complexity also appears within Ashby's cybernetics, though this again requires more discussion.

In chapter 3 we saw that despite its simplicity the tortoise remained, to a degree, a Black Box, capable of surprising Walter with its behavior. The modern impulse somehow undid itself here, in an instance where an atomic understanding of parts failed to translate into a predictive overview of the performance of the whole. What about the homeostat? In one sense, the homeostat did not display similarly emergent properties. In his published works and his private journals, Ashby always discussed the homeostat as a demonstration device that displayed the adaptive properties he had already imagined in the early 1940s and first discussed in print in his 1945 publication on the bead-and-elastic machine.

Nevertheless, *combinations* of homeostats quickly presented analytically insoluble problems. Ashby was interested, for example, in estimating the probability that a set of randomly interconnected homeostats with fixed internal settings would turn out to be stable. In a 1950 essay, he explored this topic from all sorts of interesting and insightful angles before remarking that, even with simplifying assumptions, "the problem is one of great [mathematical] difficulty and, so far as I can discover, has not yet been solved. My own investigations have only convinced me of its difficulty. That being so we must collect evidence as best we can" (Ashby 1950a, 478). Mathematics having failed him, Ashby turned instead to his machines, fixing their parameters and interconnections at random in combinations of two, three, or four units and simply recording whether the needles settled down in the middle of their ranges or were driven to their limits. His conclusion was that the probability of finding a stable combination probably fell off as $(1/2)^n$, where n was the number of units to be interconnected, but, rather than that specific result, what I want to stress is that here we have another discovery of complexity, now in the analytic opacity of multihomeostat setups. Ashby's atomic knowledge of the individual components of his machines and their interconnections again failed to translate into an ability to predict how aggregated assemblages of them would perform. Ashby just had to put the units together and see what they did.

As in the previous chapter, then, we see here how the modern impulse of early cybernetics bounced back into the cybernetic ontology of unknowability. While illuminating the inner go of the brain, homeostat assemblages of the kind discussed here turned out to remain, in another sense, mini-Black Boxes, themselves resistant to a classically scientific understanding, which we can read again as suggestive icons for a performative ontology. Imagine the world in general as built from elements like these opaque dynamic assemblages, is the suggestion. We can go further with this thought when we come to DAMS, the homeostat's successor.

Making much same point, the following quotation is from a passage in *Design for a Brain* in which Ashby is discussing interconnected units which have just two possible states, described mathematically by a "step-function" and corresponding to the shift in a uniselector from one position to the next (1952, 129): "If there are n step-functions [in the brain], each capable of taking two values, the total number of fields available will be 2^n The number of fields is moderate when n is moderate, but rapidly becomes exceedingly large when n increases. . . . If a man used fields at the rate of ten a second day and night during his whole life of seventy years, and if no field were ever repeated, how many two-valued step-functions would be necessary to provide them? Would the reader like to guess? The answer is that thirty-five would be ample!" One is reminded here of Walter's estimate that ten functional elements in the brain could generate a sufficient variety of behaviors to cover the entire experience of the human race over a period of a thousand million years. What the early cyberneticians discovered was just how complex (in aggregate behavior) even rather simple (in atomic structure) systems can be.

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The homeostat is highly instructive as ontological theater, but I should also note its shortcomings. First, like all of the early cybernetic machines including the tortoise, the homeostat had a *fixed goal*: to keep its output current within predetermined limits. This was the unvarying principle of its engagement with the world. But, as I said about the tortoise, I do not think that this is a general feature of our world—in many ways, for example, human goals emerge and are liable to transformation in practice. At the same time, we might note an important difference between the homeostat's goals and, say, the tortoise's. The latter's goals referred to states of the outer world—finding and pursuing lights. The homeostat's goals instead referred inward, to its internal states. One might therefore imagine an indefinite number of worldly projects as bearing on those inner states, all of them obliquely structured by

the pursuit of inner equilibrium. This is certainly a step in the right ontological direction beyond the tortoise.

Second, I described the homeostat as exploring its environment open-endedly, but this is not strictly true. My understanding of open-endedness includes an indefinitely large range of possibilities, whereas the homeostat had precisely twenty-five options—the number of positions of its uniselector. A four-homeostat setup could take on $25^4 = 390,625$ different states in all.¹⁷ This is a large number, but still distinctly finite. As ontological theater, therefore, we should think of the homeostat as pointing in the direction of open-ended adaptation, without quite getting there.

Third, and most important, as the word “uniselector” suggests, adaptation in the homeostat amounted to the *selection* of an appropriate state by a process of trial and error within a combinatoric space of possibilities. This notion of selection appears over and over again in Ashby’s writings, and, at least from an ontological point of view, there is something wrong with it. It leaves no room for creativity, the appearance of genuine novelty in the world; it thus erases what I take to be a key feature of open-endedness. It is easiest to see what is at stake here when we think about genuinely cognitive phenomena, so I will come back to this point later. For the moment, let me just register my conviction that as models of the brain and as ontological theater more generally, Ashby’s homeostats were deficient in just this respect.

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One final line of thought can round off this section. It is interesting to examine how Ashby’s cybernetics informed his understanding of himself. As mentioned above, a multihomeostat assemblage foregrounded the role of time—adaptation as necessarily happening in time. And here is an extract from Ashby’s autobiographical notebook, “Passing through Nature” (Ashby 1951–57), from September 1952 (pp. 36–39):

For forty years [until the mid-1940s—the first blossoming of his cybernetics] I hated change of all sorts, wanting only to stay where I was. I didn’t want to grow up, didn’t want to leave my mother, didn’t want to go from school to Cambridge, didn’t want to go to hospital, and so on. I was unwilling at every step.

Now I seem to be changed to the opposite: my only aim is to press on. The march of time is, in my scientific theorising, the only thing that matters. Every thing, I hold, must go on: if human destiny is to go on and destroy itself with an atomic explosion, well then, let us get on with it, and make the biggest explosion ever!

I am now, in other words a Time-worshipper, seized with the extra fervour of the convert. I mean this more or less seriously. "Time" seems to me to be big enough, impersonal enough, to be a possible object of veneration—the old man of the Bible with his whims & bargains, & his impotence over evil, and his son killing, has always seemed to me to be entirely inadequate as the Spirit of All Existent, if not downright contemptible. But Time has possibilities. As a variable it is utterly different from all others, for they exist in it as a fish lives in the ocean: so immersed that its absence is inconceivable. My aim at the moment is to reduce all adaptation to its operation, to show that if only Time will operate, whether over the geological periods on an earth or over a childhood in an individual, then adaptation will inevitably emerge. This gives to time a position of the greatest importance, equalled only by that "factor" that called space & matter into existence.

This passage is interesting in a couple of respects. On the one hand, Ashby records a change in his perspective on time and change (in himself and the world) that is nicely correlated with the flourishing of his cybernetics. On the other, this passage returns us to the relation between cybernetics and spirituality that surfaced in the last chapter and runs through those that follow. Walter made the connection via his discussion of the strange performances associated with Eastern spirituality, which he assimilated to his understanding of the performative brain and technologies of the self. There are also definite echoes of the East in this passage from Ashby—one thinks of Shiva indifferently dancing the cosmos into and out of existence—though now the bridge from cybernetics to spirituality goes via time and adaptation, the key themes of Ashby's cybernetics as exemplified in the homeostat, rather than technologies of the self.¹⁸

The self does, however, reappear in a different guise in this passage. "The old man of the Bible with his whims & bargains" is the very paradigm of the modern, self-determined, centered, human subject writ as large as possible. And it is interesting to note that Ashby's rejection of this image of the Christian God went with a nonmodern conception of himself. Just as a multi-homeostat setup dramatized a decentered self, not fully in control and constitutively plunged into its environment, so "Passing through Nature" begins (Ashby 1951–57, pp. 1–3) with the story of a meeting in January 1951 at which Warren McCulloch was present. Realizing how important McCulloch was to his career as a cybernetician, Ashby took the initiative and shook hands with him, but then immediately found himself going back to a conversation with someone of "negligible . . . professional importance." "What I want to

make clear is that I had no power in the matter. The series of events ran with perfect smoothness and quite irresistibly, taking not the slightest notice of whatever conscious views I may have had. Others may talk of freewill and the individual's power to direct his life's story. My personal experience has convinced me over and over again that my power of control is great—where it doesn't matter: but at the important times, in the words of Freud, I do not live but 'am lived.'"

By the early 1950s, then, Ashby's understanding of himself and God and his cybernetics all hung together, with questions of time and change as their pivot. I take this as another instance of the fact that ontology makes a difference—here in the realm of spirituality and self-understanding, as well as brain science and much else: time worship and "I am lived" as an ontology of performative becoming in action.¹⁹

The Social Basis of Ashby's Cybernetics

Turning from ontology to sociology, it is evident already that there are again clear parallels between Ashby and Walter. Ashby was telling no more than the truth when he described his early work—up to 1940, say—as having no social basis, as "a hobby I could retreat to": something pursued outside his professional life, for his own enjoyment. Even after 1940, when he began to publish, his work for a long time retained this extraprofessional, hobbyist quality, very largely carried on in the privacy of his journals. In an obituary, his student Roger Conant (1974, 4) speaks of Ashby building the homeostat "of old RAF parts on Mrs Ashby's kitchen table" and of writing his two books "in Dr. Ashby's private padded cell" at Barnwood House.²⁰

When he did begin to publish his protocybernetic theorizing, Ashby submitted his work initially to the journals in which his earlier distinctively psychiatric papers had appeared. His very first paper in this series (Ashby 1940) appeared in the leading British journal for research on mental pathologies, the *Journal of Mental Science*. It appears that there was no great response to Ashby's work within this field, outside the narrow but important circle defined by himself, Grey Walter, Frederick Golla, and G. W. T. H. Fleming, the editor of the journal in question. And one can understand why this might have been: clinical psychiatrists and psychologists were concerned with the practical problems of mental illness, and, besides its oddity as engineering, Ashby's work sticks out like a sore thumb in the pages of the psychiatric journals—his theoretical work offered little constructive input to psychiatric practice (though more on this below).

Conversely, in seeking to create a community of interest for his work, Ashby, like Walter, systematically looked beyond his profession. A journal entry from early June 1944 (p. 1666) records that “several of my papers have been returned recently & it seems that there is going to be considerable difficulty in floating this ship.”²¹ At this point he began writing to other scholars with whom he appears to have had no prior contact about his and their work, and it is notable that none of the people he addressed shared his profession. Thus, the small existing collection of Ashby’s correspondence from this period includes letters to or from the experimental psychologists Kenneth Craik and E. Thorndike in 1944, and in 1946 the anthropologist-turned-cybernetician Gregory Bateson, the eminent neurophysiologist E. D. Adrian, the doyen of American cybernetics, Warren McCulloch, the British mathematician Alan Turing, and Norbert Wiener himself. In most cases it is clear that Ashby was writing out of the blue, and that he identified this extraprofessional and proto-cybernetic community from his reading of the literature. Through these contacts, and also by virtue of something of an explosion in his publication record—around twenty cybernetic essays appeared in various journals between 1945 and 1952—Ashby quickly assumed a leading position in the nascent cybernetic community, though, as we saw in the previous chapter, this was itself located outside the usual social structures of knowledge production. In Britain, its heart was the Ratio Club, the dining club of which Ashby was a founder member; Ashby was an invited speaker at the 1952 Macy cybernetics conference in the United States, and he regularly gave papers at the Namur cybernetics conferences in Europe. As far as knowledge dissemination was concerned, Ashby’s route into the wider social consciousness was, like Walter’s and Wiener’s, via the popular success of his books.

Ashby’s cybernetics largely existed, then, in a different world from his professional life, though that situation began to change in the late 1950s. Through what appears to be a certain amount of chicanery on the part of G. W. T. H. Fleming, who was chairman of the trustees of the Burden Neurological Institute as well as director of Barnwood House, where Ashby then worked, Ashby was appointed in 1959 to succeed Golla as the director of the Burden. His ineptitude in that position—including trying to purge the library of outdated books, setting exams for all the staff, and setting private detectives on Grey Walter—remains legendary in British psychiatric circles, and Ashby was saved from a disastrous situation by the opportunity to flee to the United States (Cooper and Bird 1989, 15–18). Stafford Beer’s diary for 1960 records the circumstances of an offer from Heinz von Foerster to join the faculty of the University of Illinois, made while Beer, Pask, and Ashby were all on

campus for a conference on self-organization—an offer which Ashby understandably accepted without hesitation (Beer 1994 [1960], 299–301).

At Illinois, Ashby's formal position was that of professor in the Department of Electrical Engineering with an associated position on the biophysics committee. His primary affiliation was to von Foerster's Biological Computer Laboratory, the BCL. The BCL was an independently funded operation housed within the Electrical Engineering Department and was, during the period of its existence, 1958–75, the primary institutional basis for cybernetics in the capitalist world.²² At the BCL Ashby became the only one of our cyberneticians to enjoy full-time institutional support for his work, both in research and teaching. Ashby retired from the BCL in 1970 at the age of sixty-seven and returned to England, and Conant (1974, 4) records that “the decade spent in the United States resulted in a host of publications and was in his own estimation the most fruitful period of his career.” It seems clear that this time of singular alignment between paid work and hobby was also one of the happiest periods of Ashby's life, in which he could collaborate with many graduate students on topics close to his heart, and for which he is remembered fondly in the United States (unlike the Burden) as “an honest and meticulous scholar . . . a warm-hearted, thoughtful, and generous person, eager to pass to his students the credit for ideas he had germinated himself” (Conant 1974, 5).

Most of Ashby's cybernetic career thus displayed the usual social as well as ontological mismatch with established institutions, finding its home in improvised social relations and temporary associations lacking the usual means of reproducing themselves. In this respect, of course, his time at the BCL is anomalous, an apparent counterinstance to the correlation of the ontological and the social, but this instance is, in fact, deceptive. The BCL was itself an anomalous and marginal institution, only temporarily lodged within the academic body. It was brought into existence in the late 1950s by the energies of von Foerster, a charming and energetic Austrian postwar emigré, with powerful friends and sponsors, especially Warren McCulloch, and ready access to the seemingly inexhaustible research funding available from U.S. military agencies in the decades following World War II. When such funding became progressively harder to find as the sixties went on, the BCL contracted, and it closed down when von Foerster retired in 1975. A few years later its existence had been all but forgotten, even at the University of Illinois. The closure of the BCL—rather than, say, its incorporation within the Electrical Engineering Department—once again illustrates the social mismatch of cybernetics with existing academic structures.²³

Design for a Brain

We can return to the technicalities of Ashby's cybernetics. The homeostat was the centerpiece of his first book, *Design for a Brain*, which was published in 1952 (and, much revised, in a second edition, in 1960). I want to discuss some of the principal features of the book, as a way both to clarify the substance of Ashby's work in this period and to point the way to subsequent developments.

First, we should note that Ashby had developed an entire mathematical apparatus for the analysis of complex systems, and, as he put it, "the thesis [of the book] is stated twice: at first in plain words and then in mathematical form" (1952, vi). The mathematics is, in fact, relegated to a forty-eight-page appendix at the end of the book, and, following Ashby's lead, I, too, postpone discussion of it to a later section. The remainder of the book, however, is not just "plain words." The text is accompanied by a distinctive repertoire of diagrams aimed to assist Ashby and the reader in thinking about the behavior of complex systems. Let me discuss just one diagram to convey something of the flavor of Ashby's approach.

In figure 4.5 Ashby schematizes the behavior of a system characterized by just two variables, labeled *A* and *B*. Any state of the system can thus be denoted by a "representative point," indicated by a black dot, in the *A-B* plane, and the arrows in the plane denote how the system will change with time after finding itself at one point or another. In the unshaded central portions of the plane, the essential variables of the system are supposed to be within their assigned limits; in the outer shaded portions, they travel beyond those limits. Thus, in panel I, Ashby imagines that the system starts with its representative point at *X* and travels to point *Y*, where the essential variables exceed their limits. At this point, the parameters of the system change discontinuously in a "step-function"—think of a band breaking in the bead-and-elastic machine of 1943, or a uniselector moving to its next position in the homeostat—and the "field" of system behavior thus itself changes discontinuously to that shown in panel II. In this new field, the state of the system is again shown as point *Y*, and it is then swept along the trajectory that leads to *Z*, followed by another reconfiguration leading to state field III. Here the system has a chance of reaching equilibrium: there are trajectories within field III that swirl into a "stable state," denoted by the dot on which the arrows converge. But Ashby imagines that the system in question lies on a trajectory that again sweeps into the forbidden margin at *Z*. The system then transmogrifies again into state IV and at last ceases its development, since all the trajectories in that field configuration converge on the central dot in a region where the essential variables are within their limits.

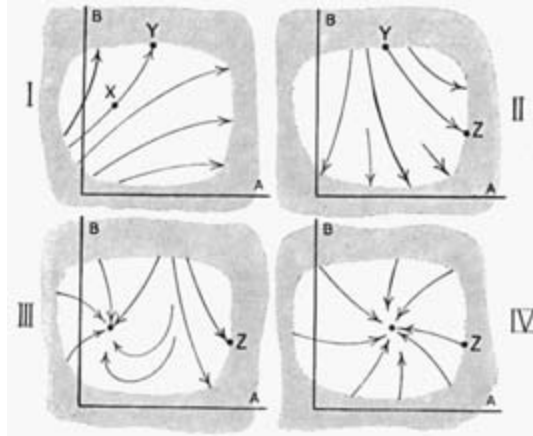


Figure 4.5. Changes of field in an ultrastable system. Source: W. R. Ashby, *Design for a Brain* (London: Chapman & Hall, 1952), 92, fig. 8/7/1. (With kind permission from Springer Science and Business Media.)

Figure 4.5 is, then, an abstract diagram of how an ultrastable system such as a homeostat finds its way to state of equilibrium in a process of trial and error, and I want to make two comments on it. The first is ontological. The basic conceptual elements of Ashby's cybernetics were those of the sort analyzed in this figure, and they were *dynamic*—systems that change in time. Any trace of stability and time independence in these basic units had to do with the specifics of the system's situation and the special circumstance of having arrived at a stable state. Ashby's world, one can say, was built from such intrinsically dynamic elements, in contrast to the modern ontology of objects carrying unvarying properties (electrons, quarks). My second comment is historical but forward looking. In *Design for a Brain*, one can see Ashby laboriously assembling the technical elements of what we now call complex systems theory. For those who know the jargon, I can say that Ashby already calls diagrams like those of figure 4.5 “phase-space diagrams”; the points at which the arrows converge in panels III and IV are what we now call “attractors” (including, in Ashby's diagrams, both point and cyclical attractors, but not “strange” ones); and the unshaded area within panel IV is evidently the “basin of attraction” for the central attractor. Stuart Kauffman and Stephen Wolfram, discussed at the end of this chapter, are among the leaders of present-day work on complexity.

Now for matters of substance. Following Ashby, I have so far described the possible relation of the homeostat to the brain in abstract terms, as both being adaptive systems. In *Design for a Brain*, however, Ashby sought to evoke more substantial connections. One approach was to point to real biological

examples of putatively homeostatic adaptation. Here are a couple of the more horrible of them (Ashby 1952, 117–18):

Over thirty years ago, Marina severed the attachments of the internal and external recti muscles of a monkey's eyeball and re-attached them in crossed position so that a contraction of the external rectus would cause the eyeball to turn not outwards but inwards. When the wound had healed, he was surprised to discover that the two eyeballs still moved together, so that binocular vision was preserved.

More recently Sperry severed the nerves supplying the flexor and extensor muscles in the arm of the spider monkey, and re-joined them in crossed position. After the nerves had regenerated, the animal's arm movements were at first grossly inco-ordinated, but improved until an essentially normal mode of progression was re-established.

And, of course, as Ashby pointed out, the homeostat showed just this sort of adaptive behavior. The commutators, *X*, precisely reverse the polarities of the homeostat's currents, and a uniselector-controlled homeostat can cope with such reversals by reconfiguring itself until it returns to equilibrium. A very similar example concerns rats placed in an electrified box: after some random leaping about, they learn to put their foot on a pedal which stops the shocks (1952, 106–8). Quite clearly, the brain being modelled by the homeostat here is not the cognitive brain of AI; it is the performative brain, the Ur-referent of cybernetics: “excitations in the motor cortex [which] certainly control the rat's bodily movements” (1952, 107). In the second edition of *Design for a Brain*, Ashby added some less brutal examples of training animals to perform in specified ways, culminating with a discussion of training a “house-dog” not to jump on chairs (1960, 113): “Suppose then that jumping into a chair always results in the dog's sensory receptors being excessively stimulated [by physical punishment, which drives some essential variable beyond its limits]. As an ultrastable system, step-function values which lead to jumps into chairs will be followed by stimulations likely to cause them to change value. But on the occurrence of a set of step-function values leading to a remaining on the ground, excessive stimulation will not occur, and the values will remain.” He then goes on to show that similar training by punishment can be demonstrated on the homeostat. He discusses a set up in which just three units were connected with inputs running $1 \rightarrow 2 \rightarrow 3 \rightarrow 1$, where the trainer, Ashby, insisted that an equilibrium should be reached in which a small forced movement of the needle on 1 was met by the opposite movement of the needle on 2. If the

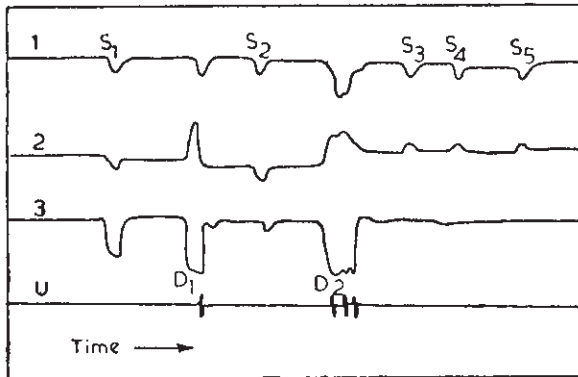


Figure 4.6. Training a three-homeostat system. The lines running from left to right indicate the positions of the needles on the tops of units 1, 2, and 3. The punishments administered to unit 3 are marked D_1 and D_2 . The shifts in the uniselectors are marked as vertical blips on the bottom line, U . Note that after the second punishment a downward displacement of needle 1 evokes an upward displacement of needle 2, as desired. Source: W. R. Ashby, *Design for a Brain: The Origin of Adaptive Behaviour*, 2nd ed. (London: Chapman & Hall, 1960), 114, fig. 8/9/1. (With kind permission from Springer Science and Business Media.)

system fell into an equilibrium in which the correlation between the needles 1 and 2 was the wrong way around, Ashby would punish homeostat 3 by pushing its needle to the end of its range, causing its unisector to trip, until the right kind of equilibrium for the entire system, with an anticorrelation of needles 1 and 2, was achieved. Figure 4.6 shows readouts of needle positions from such a training session.

Ashby thus sought to establish an equation between his general analysis of ultrastable systems and brains by setting out a range of exemplary applications to the latter. Think of the response of animals to surgery, and then think about it *this way*. Think about training animals; then think about it *this way*. In these ways, Ashby tried to train his readers to make this specific analogical leap to the brain.

But something is evidently lacking in this rhetoric. One might be willing to follow Ashby some of the way, but just what are these step mechanisms that enable animals to cope with perverse surgery or training? Having warned that “we have practically no idea of where to look [for them], nor what to look for [and] in these matters we must be very careful to avoid making assumptions unwittingly, for the possibilities are very wide” (1960, 123), Ashby proceeds to sketch out some suggestions.

One is to note that “every cell contains many variables that might change in a way approximating to the step-function form. . . . Monomolecular films,

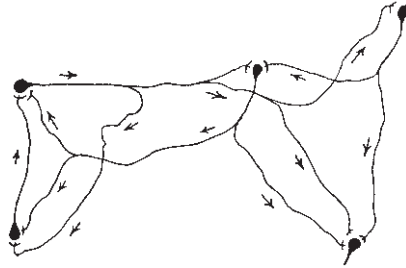


Figure 4.7. Interconnected circuit of neurons. Source: W. R. Ashby, *Design for a Brain* (London: Chapman & Hall, 1952), 128, fig. 10/5/1. (With kind permission from Springer Science and Business Media.)

protein solutions, enzyme systems, concentrations of hydrogen and other ions, oxidation-reduction potentials, adsorbed layers, and many other constituents or processes might act as step-mechanisms” (1952, 125). A second suggestion is that neurons are “amoeboid, so that their processes could make or break contact with other cells” (126). And third, Ashby reviews an idea he associates with Rafael Lorente de Nó and Warren McCulloch, that the brain contains interconnected circuits of neurons (fig. 4.7), on which he observes that “a simple circuit, if excited, would tend either to sink back to zero excitation, if the amplification factor was less than unity, or to rise to the maximal excitation if it was greater than unity.” Such a circuit would thus jump discontinuously from one state to another and “its critical states would be the smallest excitation capable of raising it to full activity, and the smallest inhibition capable of stopping it” (128). Here, then, were three suggestions for the go of it—plausible biological mechanisms that might account for the brain’s homeostatic adaptability.

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The homeostat appears midway through *Design for a Brain*. The preceding chapters prepare the way for it. Then its properties are reviewed. And then, in the book’s concluding chapters, Ashby looks toward the future. “My aim,” he says, with a strange kind of modesty, “is simply to copy the living brain” (1952, 130). Clearly, a single homeostat was hardly comparable in its abilities to the brain of a simple organism, never mind the human brain—it was “too larval” (Ashby 1948, 343)—and the obvious next step was to contemplate a multiplication of such units. Perhaps the brain was made up of a large number of ultrastable units, biological homeostats. And the question Ashby then asked was one of speed or efficiency: how long would it take such an assembly to come into equilibrium with its environment?

Here, some back-of-an-envelope calculations produced interesting results. Suppose that any individual unit had a probability p of finding an equilibrium state in one second. Then the time for such a unit to reach equilibrium would be of the order of $1/p$. And if one had a large number of units, N of them, acting quite independently of one another, the time to equilibrium for the whole assemblage would still be $1/p$. But what if the units were fully interconnected with one another, like the four units in the prototypical four-homeostat setup? Then each of the units would have to find an equilibrium state in the same trial as all the others, otherwise the nonequilibrium homeostats would keep changing state and thus upsetting the homeostats that had been fortunate enough already to reach equilibrium. In this configuration, the time to equilibrium would be of the order of $1/p^N$. Ashby also considered an intermediate case in which the units were interconnected, but in which it was possible for them to come into equilibrium sequentially: once unit 1 had found an equilibrium condition it would stay there, while 2 hunted around for the same, and so on. In this case, the time to equilibrium would be N/p .

Ashby then put some numbers in: $p = 1/2$; $N = 1,000$ units. This leads to the following estimates for T , the time for whole system to adapt (1952, 142):

for the fully interconnected network: $T_1 = 2^{1000}$ seconds;

for interconnected but sequentially adapting units, $T_2 = 2,000$ seconds;

for the system of entirely independent units, $T_3 = 2$ seconds.²⁴

Two seconds or 2,000 seconds are plausible figures for biological adaptation. According to Ashby, 2^{1000} seconds is 3×10^{291} centuries, a number vastly greater than the age of the universe. This last hyperastronomical number was crucial to Ashby's subsequent thinking on the brain and how to go beyond the homeostat, and the conclusion he drew was that if the brain were composed of many ultrastable units, they had better be only *sparsely connected* to one another if adaptation were going to take a realistic time. At this point he began the construction of a new machine, but before we come to that, let me note again the ontological dimension of Ashby's cybernetics.

The brain that adapted fastest would be composed of fully independent units, but Ashby noted that such a brain "cannot represent a complex biological system" (1952, 144). Our brains do not have completely autonomous subsystems each set to adapt to a single feature of the world we inhabit, on the one hand; the neurons of the brain are observably very densely interconnected, on the other. The question of achieving a reasonable speed of adaptation thus resolved itself, for Ashby, into the question of whether some kind of serial ad-

aptation was possible, and he was very clear that this depended not just on how the brain functioned but also on what the world was like. Thus, he was led to distinguish between “easy” environments “that consist of a few variables, independent of each other,” and “difficult” ones “that contain many variables richly cross-linked to form a complex whole” (1952, 132). There is a sort of micro-macro correspondence at issue here. If the world were too lively—if every environmental variable one acted on had a serious impact on many others—a sparsely interconnected brain could never get to grips with it. If when I cleaned my teeth the cat turned into a dog, the rules of mathematics changed and the planets reversed their courses through the heavens, it would be impossible for me to grasp the world piecemeal; I would have to come to terms with all of it in one go, and that would get us back to the ridiculous time scale of T_1 .²⁵

In contrast, of course, Ashby pointed out that not all environmental variables are strongly interconnected with one another, and thus that sequential adaptation within the brain is, in principle, a viable strategy. In a long chapter on “Serial Adaptation” he first discusses “an hour in the life of *Paramecium*,” traveling from a body of water to its surface, where the dynamics are different (due to surface tension), from bodies of water with normal oxygen concentration to those where the oxygen level is depleted, from cold to warm, from pure water to nutrient-rich regions, occasionally bumping into stones, and so on (1952, 180–81). The idea is that each circumstance represents a different environment to which *Paramecium* can adapt in turn and more or less independently. He then discusses the business of learning to drive a car, where one can try to master steering on a straight road, then the accelerator, then changing gears (in the days before automatics, at least in Britain)—though he notes that at the start these tend to be tangled up together, which is why learning to drive can be difficult (181–82). “A puppy can learn how to catch rabbits only after it has learned to run; the environment does not allow the two reactions to be learned in the opposite order. . . . Thus, the learner can proceed in the order ‘Addition, long multiplication, . . .’ but not in the order ‘Long multiplication, addition, . . .’ Our present knowledge of mathematics has in fact been reached only because the subject contains such stage-by-stage routes” (185).²⁶ There follows a long description of the steps in training falcons to hunt (186), and so on.

So, in thinking through what the brain must be like as a mechanism, Ashby also further elaborated a vision of the world in which an alchemical correspondence held between the two terms: the microcosm (the brain) and the macrocosm (the world) mirrored and echoed one another inasmuch as both were sparsely connected systems, not “fully joined,” as Ashby put it. We can

follow this thread of the story below, into the fields of architecture and theoretical biology as well as Ashby's next project after the homeostat, DAMS. But I can finish this section with a further reflection.

Warren McCulloch (1988) notably described his cybernetics as "experimental epistemology," meaning the pursuit of a theory of knowledge via empirical and theoretical analysis of how the brain actually represents and knows the world. We could likewise think of Ashby's cybernetics as *experimental ontology*. I noted earlier that the general performative vision of the world does not imply any specific cybernetic project; that such projects necessarily *add* something to the vision, both pinning it down and vivifying it by specifying it in this way or that. The homeostat can certainly be seen as such a specification, in the construction of a definite mechanism. But in Ashby's reflections on time to equilibrium, this specification reacted back upon the general vision, further specifying that. If one recognizes the homeostat as a good model for adaptation, then these reflections imply something, not just about the brain but about the world at large as well: both must consist of sparsely connected dynamic entities.

We are back to the idea that ontology makes a difference, but with a twist. My argument so far has been that the nonmodern quality of cybernetic projects can be seen as the counterpart of a nonmodern ontology. Here we have an example in which one of these projects fed back as a fascinating ontological conclusion about the coupling of entities in the world. It is hard to see how one could arrive at a similar conclusion within the framework of the modern sciences.²⁷

DAMS

AS A SYMBOL OF HIS INTEREST IN RELATIONS HE CARRIED A CHAIN CONSTRUCTED OF THREE SIMPLER CHAINS INTERLOCKED IN PARALLEL; HE ENJOYED WATCHING MICROSCOPIC ECOSYSTEMS (CAPTURED WITH FISHPOLE AND BOTTLE FROM THE BONEYARD CREEK IN URBANA) FOR THE RICHNESS OF INTERACTION THEY DISPLAYED, AND HE BUILT A SEMI-RANDOM ELECTRONIC CONTRAPTION WITH 100 DOUBLE TRIODES AND WATCHED IT FOR TWO YEARS BEFORE ADMITTING DEFEAT IN THE FACE OF ITS INCOMPREHENSIBLY COMPLEX BEHAVIOR.

ROGER CONANT, "W. ROSS ASHBY (1903-1972)" (1974, 4)

The 1952 first printing of *Design for a Brain* included just one footnote: on page 171 Ashby revealed that he was building a machine called DAMS. In the 1954

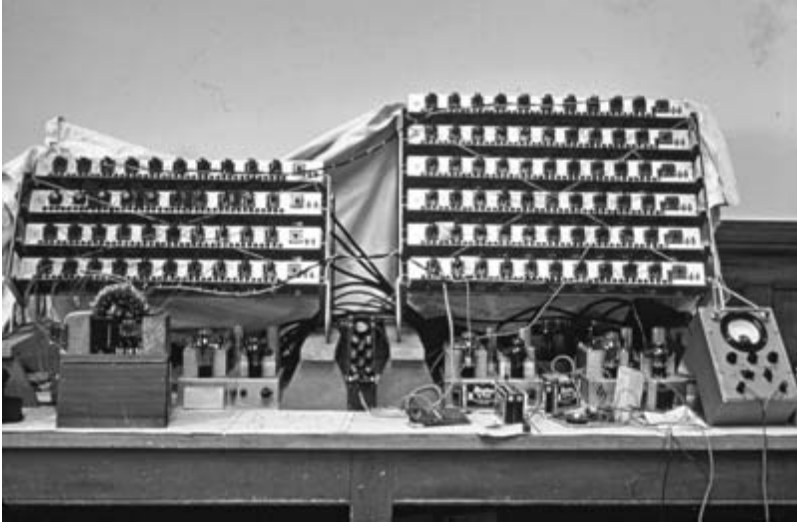


Figure 4.8. Photograph of DAMS. (By permission of Jill Ashby, Sally Bannister, and Ruth Pettit.)

second printing of the first edition the footnote was removed, though the entry for DAMS could still be found in the index and a citation remained on page 199 to the only publication in which Ashby described this device, the paper “Statistical Machinery” in the French journal *Thalès* (Ashby 1951). In the second edition, of 1960, both the index entry and the citation also disappeared: DAMS had been purged from history. Despite the obscurity to which Ashby was evidently determined to consign it, his journal in the 1950s, especially from 1950 to 1952, is full of notes on this machine. It would be a fascinating but terribly demanding project to reconstruct the history of DAMS in its entirety; I will discuss only some salient features.

I opened the book with Ashby’s suggestion that “the making of a synthetic brain requires now little more than time and labour” (1948, 382), and he evidently meant what he said. DAMS was to be the next step after the homeostat. Its name was an acronym for dispersive and multistable system. A *multistable* system he defined as one made up of many interconnected ultrastable systems. A *dispersive* system was one in which different signals might flow down different pathways (Ashby 1952, 172). This gets us back to the above discussion of times to reach equilibrium. Ashby conceived DAMS as a system in which the ultrastable components were linked by switches, which, depending on conditions, would either isolate components from one another or transmit signals between them. In this way, the assemblage could split into smaller

subassemblies appropriate to some adaptive task without the patterns of splitting having to be hard wired in advance. DAMS would thus turn itself into a sparsely connected system that could accumulate adaptations to differing stimuli in a finite time (without disturbing adaptive patterns that had already been established within it).

At the hardware level, DAMS was an assemblage of electronic valves, as in a multihomeostat setup, but now linked not by simple wiring but by neon lamps. The key property of these lamps was that below some threshold voltage they were inert and nonconducting, so that they in fact isolated the valves that they stood between. Above that threshold however, they flashed on and became conducting, actively joining the same valves, putting the valves in communication with one another. According to the state of the neons, then, parts of DAMS would be isolated from other parts by nonconducting neons, “walls of constancy,” as Ashby put it (1952, 173), and those parts could adapt independently of one another at a reasonable, rather than hyperastronomical, speed.

Not to leave the reader in undue suspense, I can say now that DAMS never worked as Ashby had hoped, and some trace of this failure is evident in the much-revised second edition of *Design for a Brain*. There Ashby presents it as a rigorous deduction from the phenomenon of cumulative adaptation to different stimuli, P_1 , P_2 , and so on, that the step mechanisms (uniselectors in the homeostat, neon tubes in DAMS) “must be divisible into non-overlapping sets, that the reactions to P_1 and P_2 must each be due to their particular sets, and that the presentation of the problem (i.e., the value of P) must determine which set is to be brought into functional connexion, the remainder being left in functional isolation” (1960, 143). One can see how this solves the problem of accumulating adaptations, but how is it to be achieved? At this point, Ashby wheels on his deus ex machina, a “gating mechanism,” Γ , shown in figure 4.9. This picks up the state of the environmental stimulus P via the reaction R of the organism to it and switches in the appropriate bank of uniselectors, neons, or whatever that the essential variables (the dial on the right) can trigger, if necessary, to preserve the equilibrium of the system. But then the reader is left hanging: What is the go of this gating mechanism? How does it do its job? Almost at the end of the book, eighty-four pages later, Ashby acknowledges that “it was shown that . . . a certain *gating-mechanism* was necessary; but nothing was said about how the organism should acquire one” (1960, 227). Two pages later, Ashby fills in this silence, after a fashion (1960, 229–30): “The biologist, of course, can answer the question at once; for the work of the last century . . . has demonstrated that natural, Darwinian,

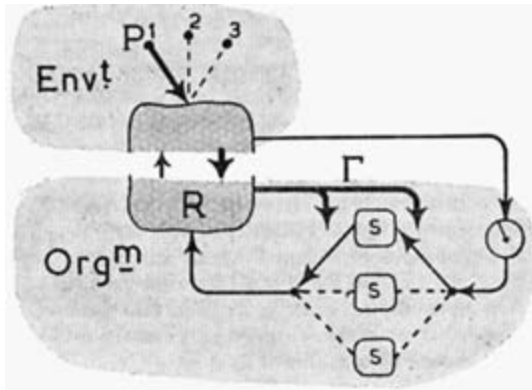


Figure 4.9. The gating mechanism. Source: W. R. Ashby, *Design for a Brain: The Origin of Adaptive Behaviour*, 2nd ed. (London: Chapman & Hall, 1960), 144, fig. 10/9/1. (With kind permission from Springer Science and Business Media.)

selection is responsible for all the selections shown so abundantly in the biological world. Ultimately, therefore, these ancillary mechanisms [the gating mechanism and, in fact, some others] are to be attributed to natural selection. They will, therefore, come to the individual (to our kitten perhaps) either by the individual's gene-pattern or they develop under an ultrastability of their own. There is no other source." Within the general framework of Ashby's approach to the brain and adaptation, these remarks make sense. We need a gating mechanism if multiple adaptations are to be achieved in a finite time; we do adapt; therefore evolution must have equipped us with such a mechanism. But what Ashby had been after with DAMS was the go of multiple adaptation. What he wanted was that DAMS should evolve its own gating mechanism in interacting with its environment, and it is clear that it never did so. To put the point the other way around, what he had discovered was that *the structure of the brain matters*—that, from Ashby's perspective, a key level of organization had to be built in genetically and could not be achieved by the sort of trial-and-error self-organization performed by DAMS.²⁸

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Though DAMS failed, Ashby's struggles with it undoubtedly informed his understanding of complex mechanisms and the subsequent development of his cybernetics, so I want to pursue these struggles a little further here.²⁹ First, I want to emphasize just how damnably complicated these struggles were. DAMS first appeared in Ashby's journal on 11 August 1950 (pp. 2953–54) with the words "First, I might as well record my first idea for a new homeostat [and,

in the margin] found a month ago.” The next note, also dated 11 August 1950, runs for twenty pages (pp. 2955–74) and reveals some of the problems that Ashby had already run into. It begins, “For a time the construction of the new machine (see previous page) went well. Then it forced me to realise that my theory had a yawning hole in it” (p. 2955).

This yawning hole had to do with DAMS’s essential variables, the parameters it should control. In the original homeostat setups all of the currents were essential variables, capable of triggering discontinuous changes of state via the relays and uniselectors. But there was no reason why all of the currents in DAMS should be essential variables. Some of them should be, but others would have simply to do with making or breaking connections. Thus, a new problem arose: how the environment should be supposed to connect to DAMS’s essential variables, and how those variables might act back onto the environment.³⁰ The homeostat offered no guidance on this, and the remainder of this entry is filled with Ashby’s thoughts on this new problem. It contains many subsequently added references to later pages which develop these early ideas further. In a passage on page 2967, for example, one thought is linked by an asterisk to a note at the bottom of the page which says, “May ’51. Undoubtedly sound in aim, but wrong in the particular development used here,” while in the margin is a note in black ink, “Killed on p. 2974,” and then another note, “Resurrected p. 3829,” in red. The next paragraph then begins, “This was the point I reached before I returned to the designing of the electrical machine, but, as usual, the designing forced a number of purely psychological problems into the open. I found my paragraph (2) (above) [i.e., the one just discussed here] was much too vague to give a decisive guide.” The penultimate paragraph of the entire note ends (p. 2974), “I see no future this way. The idea of p. 2967 (middle) [i.e., again the one under discussion here] seems to be quite killed by this last figure.” But then a marginal note again says, “Resurrected p. 3829” (i.e., 17 May 1952).

The substantial point to take from all this is that the construction of DAMS posed a new set of problems for Ashby, largely having to do with the specification of its essential variables and their relation to the environment, and it was by no means clear to him how to solve them.³¹ And what interests me most here is that in response to this difficulty, Ashby, if only in the privacy of his journal, articulated an original *philosophy of design*.

“The relation of the essential variables to a system of part-functions [e.g., the neon tubes] is still not clear, though p. 3074 helps. Start again from first principles,” Ashby instructed himself on 28 January 1951, but a second note dated the same day recorded that DAMS was “going to be born any time”

(pp. 3087–8). Six weeks later Ashby recorded that “DAMS has reached the size of ten valves, and,” he added, “has proved exceedingly difficult to understand.” He continued (14 March 1951, pp. 3148–51),

But while casting around for some way of grasping it I came across a new idea. Why not make the development of DAMS follow in the footsteps marked out by evolution, by making its variations struggle for existence? We measure in some way its chance of “survival,” and judge the values of all proposed developments by their effects on this chance. We know what “survival” means in the homeostat: we must apply the same concept to DAMS. . . .

The method deserves some comment. First notice that it totally abandons any pretence to “understand” the assembly in the “blue-print” sense. When the system becomes highly developed the constructor will be quite unable to give a simple and coherent account of why it does as it does. . . . Obviously in these circumstances the words “understand” and “explain” have to receive new meanings.

This rejection of the “blue-print” attitude corresponds to the rejection of the “blue-print” method in the machine itself. One is almost tempted to dogmatise that the Darwinian machine is to be developed only by the Darwinian process! (there may be more in this apothegm than a jest). After all, every new development in science needs its own new techniques. Nearly always, the new technique seems insufficient or hap-hazard or plain crazy to those accustomed to the old techniques.

If I can, by this method, develop a machine that imitates advanced brain activities without my being able to say how the activities have arisen, I shall be like the African explorer who, having heard of Lake Chad, and having sought it over many months, stood at last with it at his feet and yet, having long since lost his bearings, could not say for the life of him where in Africa Lake Chad was to be found.

This is a remarkable passage of ontological reflection, which gets us back to the cybernetic discovery of complexity from a new angle. Like Walter’s tortoise, the homeostat had been designed in detail from the ground up—the blueprint attitude—and this approach had been sufficient, inasmuch as the two machines did simulate performances of the adaptive brain. My argument was, however, that when constructed, they remained to a degree impermeable Black Boxes, displaying emergent properties not designed into them (the tortoise), or otherwise opaque to analysis (the multihomeostat setup). But it was only with DAMS that Ashby had to confront this discovery of complexity

head-on. And in this passage, he takes this discovery to what might be its logical conclusion. If, beyond a certain degree of complexity, the performance of a machine could not be predicted from a knowledge of its elementary parts, as proved to be the case with DAMS, then one would have to abandon the modern engineering paradigm of knowledge-based design in favor of evolutionary tinkering—messing around with the configuration of DAMS and retaining any steps in the desired direction.³² The scientific detour away from and then back to performance fails for systems like these.

The blueprint attitude evidently goes with the modern ontological stance that presumes a knowable and cognitively disposable world, and Ashby's thoughts here on going beyond design in a world of mechanisms evolving quasi-organically once more make the point that ontology makes a difference, now at the level of engineering method. We can come back to this point in later chapters.

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Ashby never reached the shores of Lake Chad, but one feature of DAMS's performance did become important to his thinking: a behavior called "habituation." In his only published discussion of DAMS, after a discussion of DAMS itself, Ashby turns to a theoretical argument, soon to appear in *Design for a Brain*, that he claims is generally applicable to any "self-switching network, cortex or D. A. M. S. or other, . . . no matter in what random pattern the parts are joined together and no matter in what state its 'memories' have been left by previous activities." This argument has two parts: first, that a system like DAMS will naturally split itself up into subsystems that "*tend to be many and small rather than few and large*"; and second, that such a system becomes habituated to a repeated stimulus, inasmuch as "*it will tend to set its switches so that it is less, rather than more, disturbed by it.*" Then Ashby returns to his machines, noting first that the latter effect had been demonstrated on the homeostat, where, indeed, it is true almost by definition: the first application of any stimulus was liable to provoke a large response—the tripping of the unselectors—while once the homeostat had found an equilibrium configuration, its response to the same stimulus would be small: a damped oscillation returning to the equilibrium state. By 1951, Ashby could also remark that this property "is already showing on the partly-constructed D. A. M. S." (1951, 4, 5; Ashby's italics).

Ashby regarded habituation in his machines as support for his general approach to the brain. "In the cerebral cortex this phenomenon [of diminishing response to a stimulus] has long been known as 'habituation.' It is in fact not restricted to the cerebral cortex but can be observed in every tissue that is ca-

pable of learning. Humphrey considers it to be the most fundamental form of learning” (1951, 5). But, as Ashby put it in *Design for a Brain*, “The nature of habituation has been obscure, and no explanation has yet received general approval. The results of this chapter suggest that it is simply a consequence of the organism’s ultra-stability, a by-product of its method of adaptation” (1952, 152).³³ The significance of this observation is that Ashby had gone beyond the simple mimicry of adaptation to a novel result—discovering the go of a phenomenon that hitherto remained mysterious.³⁴ And in his journals, Ashby took this line of thought still further. Reflecting on DAMS on 22 May 1952 (p. 3829), he arrived at an analysis of “dis-inhibition” (he writes it in quotes): “The intervention of a second stimulus will, in fact, restore the δ -response to its original size. This is a most powerful support to my theory. All other theories, as far as I know, have to postulate some special mechanism simply to get dis-inhibition.”³⁵

If DAMS never reached the promised land and Ashby never quite reached Lake Chad, then, certainly the DAMS project led to this one substantive result: an understanding of habituation and how it could be undone in ultra-stable machines. We can come back to this result when we return to Ashby’s psychiatric concerns.

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I can add something on the social basis of Ashby’s research in the DAMS era and its relation to the trajectory of his research. In the early 1950s, Pierre de Latil visited the leading cyberneticians of the day, including Walter as well as Ashby, and wrote up a report on the state of play as a book, *Thinking by Machine: A Study of Cybernetics*, which appeared in French in 1953 and in English in 1956, translated by Frederick Golla’s daughter, Yolande. De Latil recorded that “Ashby already considers that the present DAMS machine is too simple and is planning another with even more complex action. Unfortunately, its construction would be an extremely complex undertaking and is not to be envisaged for the present” (de Latil 1956, 310). I do not know where the money came from for the first versions of DAMS, but evidently cost became a problem as Ashby began to aim at larger versions of it. On an ill-starred Friday the 13th in September 1957, Ashby noted to himself, “As the RMPA [Royal Medico-Psychological Association] are coming to B. H. [Barnwood House] in May 1960 I have decided to get on with making a DAMS for the occasion, doing as well as I can on the money available. By building to a shoddiness that no commercial builder would consider, I can probably do it for far less than a commercial firm would estimate it at.” Clearly, by this time Ashby’s hobby was turning into a habit he could ill afford and remained a hobby only for lack

of institutional support.³⁶ That his work on DAMS had lapsed for some time by 1957 is evident in the continuation of the note: “In addition, my theoretical grasp is slowly getting bogged down for lack of real contact with real things. And the deadline of May 1960 will force me to develop the practical & immediate” (p. 5747).

Ashby’s strained optimism of 1957 was misplaced. A year later, on 29 September 1958, we find him writing (pp. 6058–60): “The new DAMS . . . having fizzled out, a new idea occurs to me today—why not make a small DAMS, not for experimental purposes but purely for demonstration. . . . The basic conception is that all proofs are elsewhere, in print probably; the machine is intended purely to enable the by-stander to see what the print means & to get some intuitive, physical, material feeling for what it is about. (Its chief virtue will be that it will teach me, by letting me see something actually do the things I think about.) Summary: Build devices for demonstration.” The drift in this passage from DAMS to demonstration machines is significant. After a break, the same journal entry continues poignantly: “The atmosphere at Namur (Internatl. Assoc. for Cybs., 2–9 Sep.) showed me that I am now regarded more as a teacher than as a research worker. The world wants to hear what I have found out, & is little interested in future developments. Demonstration should therefore be my line, rather than exploration. In this connexion it occurs to me that building many small machines, each to show just one point, may be easier (being reducible) than building a single machine that includes the lot. Summary: Build small specialist machines, each devised to show one fact with perfect clarity.” A formally beautiful but personally rather sad technosocial adjustment is adumbrated in this note. In it, Ashby responds to two or possibly three resistances that he felt had arisen in his research. The one that he failed to mention must have been his lack of technical success in developing DAMS as a synthetic brain. The second was the escalating cost and lack of commensurate institutional support for developing DAMS, as just discussed. And the third was what he perceived, at least, to be a developing lack of interest in his research in the European cybernetics community. How far he was correct in this perception is difficult to judge; it is certainly true, however, that youngsters like Stafford Beer and Gordon Pask were bursting onto the scene by the late 1950s—Beer was thirty-four in 1958, Pask thirty-two; Ashby was becoming a grand old man of cybernetics at the age of fifty-four. And all of these resistances were accommodated by Ashby’s strategy. Technically, building small demonstration machines presented him with a finite task (unlike the never-ending difficulties with DAMS as a research machine), reduced the cost to a bearable level, and, socially, positioned Ashby as a pedagogue.

In important respects, Ashby went through with this plan. Especially at the University of Illinois in the 1960s, his demonstration machines became legendary, as did his qualities as a pedagogue.³⁷ It is certainly not the case that he gave up his research after 1958—his “hobby” was always his *raison d’être*—but his major subsequent contributions to cybernetics and systems theory were all in the realm of theory, as foreshadowed in the first quotation above. As a full professor at a major American university, Ashby’s funding problems appear to have been significantly alleviated in the 1960s, and there is one indication that he returned then to some version of DAMS as a research project. In an obituary, Oliver Wells recalled that Ashby’s “love of models persuaded von Foerster to have constructed what was called the ‘The Grandfather Clock’ which was designed as a seven foot noisy model of state-determined complex ‘systems’ running through trajectories of cycles of stabilisation and ‘randomness’” (Wells 1973). One has to assume that nothing significant emerged from this project; like the English DAMS, it was never the subject of anything that Ashby published.

The stars were in a strange alignment for Ashby in the late 1950s. Immediately after the deflationary post-Namur note he added an interstitial, undated note which reads: “Here came the Great Translation, from a person at B. H. to Director at B. N. I. [the Burden] (Appointment, but no more till May ’59)” (p. 6060). But now we, too, can take a break and go back to madness.

Madness Revisited

At the beginning of this chapter I noted that Ashby’s career in Britain was based in mental institutions and that he was indeed active in research related to his profession, publishing many papers on explicitly psychiatric topics. I want now to discuss the relation between the two branches of Ashby’s work, the one addressed to questions of mental illness and the cybernetic work discussed in the preceding sections.

My starting point is Ashby’s 1951 assertion, already quoted, that his cybernetics, as developed in his journal, “was to me merely a delightful amusement, a hobby I could retreat to, a world where I could weave complex and delightful patterns of pure thought.” This assertion deserves to be taken seriously, and it is tempting to read it as saying that his cybernetic hobby had nothing to do with his professional research on pathological brains and ECT. It is also possible to read his major works in cybernetics, above all his two books, as exemplifications of this: there is remarkably little of direct psychiatric interest in them. The preceding discussions of the homeostat and DAMS should

likewise make clear that this aspect of Ashby's work had its own dynamic. I nevertheless want to suggest that this reading is untenable, and that there were in fact interesting and constitutive relationships between the two branches of Ashby's oeuvre—that psychiatry was a surface of emergence and return for Ashby's cybernetics, as it was for Walter's.

We can start by noting that in the 1920s Englishmen took up many hobbies, and theorizing the adaptive brain is hardly the first that comes to mind. If in 1928 Ashby had taken up stamp collecting, there would be nothing more to say. But it is evident that his professional interests structured his choice of hobby. If his cybernetics, as discussed so far, was an attempt to understand the go of the normal brain, then this related to his professional concerns with mental illness, at minimum, as a direct negation rather than a random escape route. More positively, Ashby's materialism in psychiatry, shared with Golla and Walter, carried over without negation into his hobby. The hobby and the professional work were in exactly the same space in this respect. And we should also remember that in medicine the normal and the pathological are two sides of the same coin. The pathological is the normal somehow gone out of whack, and thus, one way to theorize the pathological is first to theorize the normal. The correlate of Ashby's interest in adaptation, in this respect, is the idea going back at least to the early twentieth century, that mental illnesses can be a sign of *maladaptation* (Pressman 1998). Simply by virtue of this reciprocal implication of the normal and the pathological, adaption and maladaptation, it would have been hard for Ashby to keep the two branches of his research separate, and he did not.

The most obvious link between the two branches of Ashby's research is that most of Ashby's early cybernetic publications indeed appeared in psychiatric journals, often the leading British journal, the *Journal of Mental Science*. And, as one should expect, all of these papers gestured in one way or another to the problems of mental illness. Sometimes these gestures were largely rhetorical. Ashby would begin a paper by noting that mental problems were problems of maladaptation, from which it followed that we needed to understand adaptation, which would lead straight into a discussion of tilted cubes, chicken incubators, beads and elastic, or whatever. But sometimes the connections to psychiatry were substantial. Even Ashby's first cybernetic publication, the 1940 essay on dynamic equilibrium, moves in that direction. Ashby there discusses the "capsule" which controls the fuel flow in a chicken incubator and then asks what would happen if we added another feedback circuit to control the diameter of the capsule. Clearly, the capsule would not be able to do its job as well as before, and the temperature swings would be wilder. Although

Ashby does not explicitly make the point, this argument about “stabilizing the stabilizer” is of a piece with the conventional psychiatric idea that some mental fixity lies behind the odd behavior of the mentally ill—mood swings, for example. What Ashby adds to this is a mechanical model of the go of it. This simple model of the adaptive brain can thus be seen as *at once* a model for thinking about pathology, too. Likewise, it is hard not to relate Ashby’s later thoughts on the density of connections between homeostat units, and their time to reach equilibrium, with lobotomy. Perhaps the density of neural interconnections can somehow grow so large that individuals can never come into equilibrium with their surroundings, so severing a few connections surgically might enable them to function better. Again, Ashby’s understanding of the normal brain immediately suggests an interpretation of mental pathology and, in this case, a therapeutic response.

Ashby often failed to drive home these points explicitly in print, but that proves very little. He contributed, for example, the entry “Cybernetics” to the first *Recent Progress in Psychiatry* to appear in Britain after World War II (Fleming 1950).³⁸ There he focused on pathological positive feedback in complex machines—“runaway”—as a model for mental illness, leading up to a lengthy discussion of the stock ways of curing such machine conditions: “to switch the whole machine off and start again,” “to switch out some abnormal part,” and “to put into the machine a brief but maximal electric impulse” (Ashby 1950b, 107). We saw this list before in the previous chapter, and when Walter produced it he was not shy of spelling out the equivalences to sleep therapy, lobotomy, and ECT, respectively. Given a pulpit to preach to the psychiatric profession, Ashby could bring himself to say only, “These methods of treatment [of machines] have analogies with psychiatric methods too obvious to need description” (1950b, 107).

To find more specific and explicit connections between Ashby’s cybernetics and his professional science, it is interesting to begin with a paper I mentioned before, his 1953 essay “The Mode of Action of Electro-convulsive Therapy” (Ashby 1953a). As I said, the body of this paper is devoted to reporting biochemical observations on rats that had been subjected to electroshock, and the theoretical introduction accordingly lays out a framework for thinking about ECT and brain chemistry. But Ashby also throws in a second possible interpretation of the action of ECT:

There is a possibility that E. C. T. may have a direct effect on the cortical machinery, not in its biochemical but in its cybernetic components. . . . It has been shown [in *Design for a Brain*] that one property such systems [of many interacting

elements] will tend to show is that their responses . . . will tend to diminish. When the stimulus is repeated monotonously, the phenomenon is well known under the name of “habituation.” We can also recognise, in everyday experience, a tendency for what is at first interesting and evocative to become later boring and uninspiring. Whether the extreme unresponsiveness of melancholia is really an exaggeration of this process is unknown, but the possibility deserves consideration. What makes the possibility specially interesting is that the theory of such statistical systems makes it quite clear that any complex network that has progressed to a non-responding state can, in general, be made responsive again by administering to it any large and random disturbance. The theory also makes clear that such a disturbance will necessarily disturb severely the system’s memory: the parallel with E. C. T.’s effect on memory is obvious. Whether, however, E. C. T. acts in essentially this way is a question for the future.

This passage is remarkable in at least two ways. First, it does not belong in Ashby’s essay at all. If taken seriously, it undercuts the entire rationale for the biochemical investigations reported there. Second, and more important in the present context, it makes an explicit connection between Ashby’s cybernetics and his work on DAMS on the one hand, and his interest in ECT and its functioning on the other, and we can return to DAMS here.³⁹ A journal entry of 25 August 1951 records that “while working with DAMS I found I was unconsciously expecting it to ‘run down,’ then I realised what was happening, & that my expectation was not unreasonable, was a new idea in fact.” Then follows the first discussion of “habituation” in DAMS (though Ashby does not use the word here): “there is therefore a tendency for the neons to change their average ‘readiness’ from ‘more’ to ‘less.’” And Ashby immediately moves from this observation to a consideration of the antidotes to habituation: “After this initial reserve of changeable neons has been used up the system’s possibilities are more restricted. The only way to restore the possibilities is to switch the set off, or perhaps to put in some other change quite different from those used during the routine. This fact can obviously be generalised to a principle.” As just mentioned, there was a stock equation in the cybernetics of this period between switching off a machine and sleep therapy for mental illness, though Ashby does not comment on this in his note. However, there then follows a quick sketch of the argument that in its response to a new and different input, DAMS will regain its prehabituation sensitivity to the old one, after which Ashby concludes: “Summary: A multistable system tends to lose reactivity, which will often be restored by applying some strong, but unre-

lated stimulus, at the cost of some forgetting. ? Action of E. C. T. (Corollary p. 3464)” (pp. 3434–3437).

This is the argument Ashby relied upon above but did not provide in his 1953 essay on the functioning of ECT, but here we find it right in the heartland of his hobby, engaging directly with his major cybernetic project of the early 1950s, DAMS. And it is revealing to follow this story a little further in his journal. The reference forward from the last note takes us to a journal entry dated 12 September 1951, which begins, “From p. 3464, it is now obvious how we make DAMS neurotic: we simply arrange the envt. so that it affects two (or more) essl. variables so that it is impossible that both should be satisfied.” Page 3464 in fact takes us to a discussion of Clausewitz, which I will come back to in the next section. In this entry, though, Ashby draws a simple circuit diagram for DAMS as subject to the conflicting demands of adapting to two different voltages at once (fig. 4.10) and comments that “both E.V.’s will now become very noisy,” seeking first to adapt to one voltage and then the other, “and the system will be seriously upset. It is now very like a Masserman cat that must either starve or get a blast in the face. The theme should be easily developed in many ways” (pp. 3462–63). We thus find ourselves explicitly back in the psychiatric territory I associated in the previous chapter with Grey Walter and the CORA-equipped tortoise, now with DAMS as a model of neurosis as well as normality and of the functioning of ECT.⁴⁰

Ashby’s journal entry refers forward to another dated 22 September 1951, where Ashby remarks that DAMS will simply hunt around forever when posed an insoluble problem, but that “the animal, however, . . . will obviously have some inborn reflex, or perhaps several, for adding to its resources. . . . A snail or tortoise may withdraw into its shell. . . . The dog may perhaps simply bite savagely. . . . A mere total muscular effort—an epileptic fit—may be the last resort of some species. . . . My chief point is that the symptoms of the unsolvable problem, whether of aggression, of apathy, of catatonia, of epilepsy, etc are likely to be of little interest in their details, their chief importance clinically being simply as indicators that an unsolvable problem has been set” (pp. 3479–81). Here Ashby covers all the bases, at once addressing a whole range of pathological clinical conditions, while dismissing the importance of symptoms in favor of his cybernetic analysis of the underlying cause of all of them—and, in the process, perhaps putting down Grey Walter, for whom epilepsy—“a mere total muscular effort”—was a major research field in its own right.

Habituation and dehabituation, then, were one link between Ashby’s cybernetics and his psychiatry, and, indeed, it is tempting to think that the

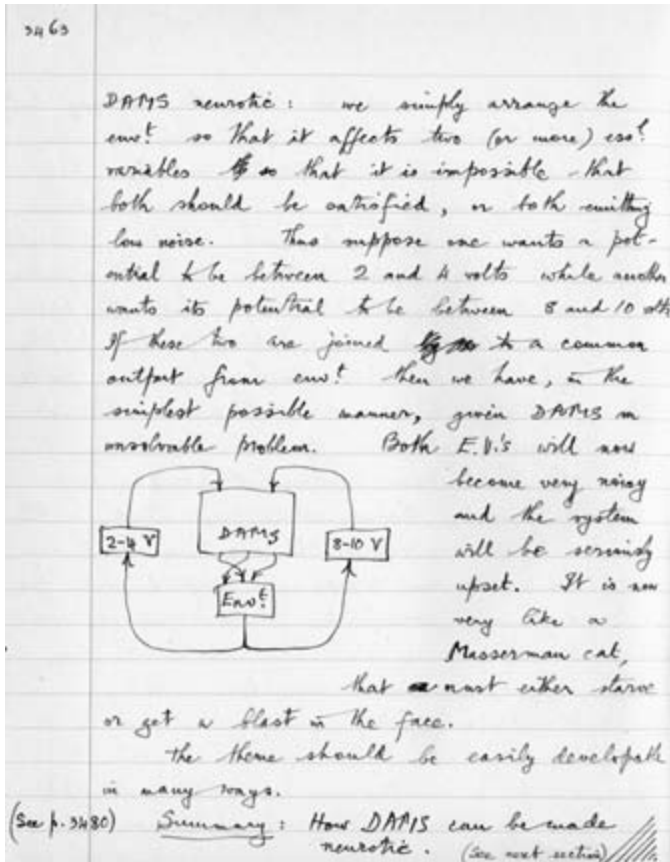


Figure 4.10. "How DAMS can be made neurotic." Source: Ashby's journal, entry dated 12 September 1951 (p. 3463). (By permission of Jill Ashby, Sally Bannister, and Ruth Pettit.)

possibility of this link explains some of the energy Ashby invested during the 1950s in this otherwise hardly exciting topic. But it is worth emphasizing that it was by no means the only possible link that Ashby discerned. To get at the range of his thinking it is enough to look at his published record, and here we can focus on a 1954 paper, "The Application of Cybernetics to Psychiatry" (Ashby 1954).⁴¹ This tentatively outlines several different ways of thinking cybernetically about mental illness. I will just discuss a couple.⁴²

One carried further Ashby's theorizing of the chemistry of electroshock. As mentioned at the beginning of this chapter, Ashby's own measurements had shown, he believed, that electroshock was followed by "a brisk outpouring of steroids." Here the question addressed was this: The level of steroids

in the brain is presumably a quantity which varies continuously, up or down. Insanity, in contrast, appears to be dichotomous—one is either mad or not. How then can a continuous cause give rise to a discontinuous effect? “What is not always appreciated is that the conditions under which instability appears are often sharply bounded and critical even in a system in which every part varies continuously. . . *Every dynamic system is potentially explosive.* . . . These facts are true universally. . . . They are necessarily true of the brain” (1954, 115–16). And Ashby had, in fact, addressed this topic mathematically in a 1947 paper (Ashby 1947). There he considered a complex system consisting of interlinked autocatalytic chemical reactions of three substances, with rates assumed to be controlled by the presence of some enzyme, and he showed by numerical computation that there was an important threshold in enzyme concentration. Below that threshold, the concentration of one of the reacting chemicals would inevitably fall to zero; above the threshold, the concentration would rise to unity. This mathematical result, then, showed in general how discontinuous effects can emerge from continuous causes, and, more specifically, it shed more light on the possible go of ECT—how the outpouring of steroids might conceivably flip the patient’s brain into a nonpathological state.⁴³

The other suggestion was more directly cybernetic. Ashby supposed that when the essential variables exceed their limits in the brain they open a channel to signals from a random source, which in turn pass into the cortex and initiate homeostat-like reconfigurations there (fig. 4.11). Both the source and the channel were supposed to be real anatomical structures (1954, 120): “V [the random source] could be small, perhaps even of molecular size. It won’t be found until specially looked for. The channel U [carrying the random signal to the cortex], however, must be quite large. . . . One thinks naturally of a tract like the mammillo-thalamic . . . [and] of the peri-ventricular fibres . . . but these matters are not yet settled; they offer an exceptional opportunity to any worker who likes relating the functional and the anatomical.” And, having hypothesized this cybernetic channel U, Ashby was in a position to describe the pathologies that might be associated with it. If it was unable to carry sufficient information, the brain would be unable to change and learn from its mistakes, while if it carried too much, the brain would be continually experimenting and would never reach equilibrium—conditions which Ashby associated with melancholia and mania, respectively. Here then, he came back to the idea that he had unsuccessfully explored in the 1930s—that there exists an identifiable organic basis for the various forms of mental pathology—but now at a much greater level of specificity. Instead of examining gross features of brains in

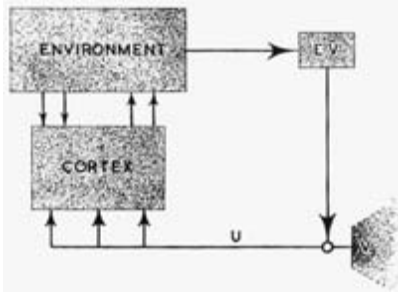


Figure 4.11. The brain as homeostat. Signals from the essential variables (E.V., top right) open the channel *U* to the random source (*V*, bottom right). Reproduced with permission from W. R. Ashby, "The Application of Cybernetics to Psychiatry," *Journal of Mental Science*, 100 (1954), 120. (© 1954 The Royal College of Psychiatrists.)

pursuit of differences, one should above all look for this channel *U* and its possible impairments. This idea that the brain contains a special organ to accomplish its homeostatic adaptations—a whole new kind of bodily structure lying outside the classifications of contemporary medical and biological science—is a striking one. As far as I know, however, no one took this suggestion up in anatomical research.

There is more to be said about Ashby's cybernetic psychiatry, but that will take us in different directions, too, so I should briefly sum up the relation between his cybernetics and psychiatry as we have reviewed it thus far. First, as I said of Grey Walter in the previous chapter, psychiatry was a surface of emergence for Ashby's cybernetics: his cybernetics grew out of psychiatry, partly by a reversal (the normal instead of the pathological brain as the focus of his hobby) but still remaining in the same space (the normal and the pathological as two sides of the same coin). There is no doubt that Ashby's hobby represented a significant detour away from the mental hospital in his thinking; as I said, his cybernetic research had its own dynamics, which cannot be reduced to a concern with mental illness. But still, psychiatry remained very much present in Ashby's cybernetics as a potential surface of return. Especially during his years at Barnwood House, 1947–59, the key years in the flowering of his cybernetics, Ashby was more than ready to see how his cybernetics could grow back into psychiatry. And we should not see this as some cynical maneuver, simply pandering to the profession that paid him. The appearance of psychiatric concerns in his journal—where, for example, his wife and children never get a look in, and where his own appointment to the directorship of the Burden only warranted an interstitial remark—testifies to his own continuing interest in psychiatry. This, I believe, is how we should think of the relation between cybernetics and psychiatry in Ashby's work: psychiatry as both a surface of emergence and return for a cybernetics that was, nevertheless, a scientific detour away from it.⁴⁴

Adaptation, War, and Society

SUPPOSE WE CONSIDERED WAR AS A LABORATORY?

THOMAS PYNCHON, *GRAVITY'S RAINBOW*

We have been following the development of Ashby's cybernetics as a science of the brain, but I mentioned at the start the instability of the referent of his work, and now we can pick up this thread. In the next section I will discuss Ashby's transformation of cybernetics into a theory of everything, but first I want to follow some passages in Ashby's journal that constitute more focused extensions of his cybernetics into the field of *the social*—specifically, questions of war and planning. These interest me for two reasons. First, they are further manifestations of the protean character of cybernetics, spilling over beyond the brain. Second, Ashby's thoughts on war and planning manifest diametrically opposed ways—asymmetric and symmetric, respectively—of imagining adaptation in multiagent systems. This is an important contrast we need to keep in mind for the rest of the book. Ashby assimilated psychiatry to the asymmetric adaptation he associated with warfare, while we will see that Bateson and Laing took the other route, emphasizing a symmetry of patient and therapist (and Beer and Pask also elaborated the symmetric stance). This difference in stance goes to the heart of the difference between the psychiatry of Ashby and Walter and the “antipsychiatry” of Bateson and Laing.

Ashby started making notes on DAMS on 11 August 1950, and one of his lines of thought immediately took on a rather military slant. In the long second note he wrote that day he began to struggle with the central and enduring problem of how DAMS could associate specific patterns of its inner connections with specific environmental stimuli—something he took to be essential if DAMS was to accumulate adaptations. Clearly, DAMS would have to explore its environment and find out about it in order to adapt, and “[when] one is uncomfortable [there] is nothing other than to get restless. (3) Do not suffer in silence: start knocking the env[ironmen]t about, & watch what happens to the discomfort. (4) This is nothing other than ‘experimenting’: forcing the environment to reveal itself. (5) Only by starting a war can one force the revelation of which are friends & which foes. (6) Such a machine does not solve its problems by thinking, just the opposite: it solves them by forcing action. . . . So, in war, does one patrol to force the enemy to reveal himself and his characteristics” (p. 2971).

A year later, we find similar imagery. “A somewhat fearsome idea!” begins the entry for 7 September 1951 (pp. 3451–52):

In evolution, the fact that survival rules everything means that organisms will not only develop those features that help them to survive against their environment but will also force them to develop those features that help them to survive against each other. The “killer” Paramecium, or the aggressive male stag, is favoured as compared with its more neutral neighbours. . . . If the cerebral cortex evolves similarly, by “survival” ruling everything in that world of behaviour & subsystems, then those subsystems should inevitably become competitive under the same drive. . . . In a really large cortex I would expect to find, eventually, whole armies of subsystems struggling, by the use of higher strategy, against the onslaught of other armies.

Ashby was a great reader, and his next note on the following day begins thus (pp. 3452–7):⁴⁵

I have always held that war, scientific research, and similar activities, being part of the organism’s attempt to deal with its environment, must show, when efficient & successful, the same principles that are used by the organism in its simpler & more direct interactions with an environment. I have hunted through the Public Library for some book on the essentials of military method, but could find nothing sufficiently abstract to be usable. So I borrowed “Clausewitz.” Here is my attempt to translate his principles into the psychological. He starts ‘What is war? War is an art of violence, and its object is to compel our opponent to comply with our will.’ Comment: Clearly he means that step-functions must change, and those are not to be ours.

War among the homeostats! It is worth continuing this passage. Ashby remarks that the approximate symmetry between opponents in war (he is thinking of old-fashioned wars like World War II) “is quite different from the gross asymmetry usually seen in the organism-environment relation,” and continues:

Where, then, do we find such a struggle between equals? Obviously in a multi-stable system between adapted sub-systems, each of which, being stable, “tries” to force the other to change in step-functions. . . . If two systems interact, how much information should each admit? . . . If I am wrestling, there is a great practical difference between (1) getting information by looking at my opponent with

open eyes and (2) setting his hands around my throat & feeling what he is going to do. Obviously the difference is due to the fact that effects from the throat-gripping hands go rapidly & almost directly to the essential variables, whereas the effects from the retina go through much neural network & past many effectors before they reach the E.V.'s. In war, then, as discussed by Clausewitz, we must assume that the systems have essential variables. Is this true of the cortical sub-systems? Probably not if we are talking about purely cortical sub-systems. . . . It would, however, be true of subsystems that have each some of the body's essential variables and that are interacting: [see fig. 4.12]. Now we have something like two armies struggling. . . . Summary: The art of war—in the cortex.

What should we make of these ruminations? The first point to note is the extension of Ashby's ontological vision: here warfare and brain processes are understood on the same basic plan, as the interaction of adaptive entities. But second, an *asymmetry* has entered the picture. Warfare, on Ashby's reading of Clausewitz, is *not* a process of *reciprocal* adaptation: in war each party seeks to remain constant and to oblige the other to adapt.⁴⁶ Third, it is evident that in the early 1950s Ashby's cybernetics evolved in a complex interplay between his thinking on war and brain science and his struggles with DAMS. And, furthermore, we can get back to the topic of the previous section by throwing psychiatry back into this heady mix. Figure 4.12, for example, is almost identical to a circuit diagram that Ashby drew four days later, except that there the central box was labeled "DAMS." This latter figure was reproduced above as figure 4.10, which I labeled with a quotation from Ashby, "how DAMS can be made neurotic." We thus return very directly to the topics of psychiatry, once more in the heartland of Ashby's journal. In this phase of his research, then, it is fair to say that DAMS, adaptation, war, and neurosis were bound up together. Ashby's thinking on each was productively engaged with his thoughts on the other.

This line of thought on Clausewitz and war never made it explicitly into Ashby's published writings, and I have not tracked its evolution systematically through his journal, but it makes a striking reappearance seven years later, in the entry immediately following the note that he had just been appointed director of the Burden. On 3 November 1958 he remarked (pp. 6061–2) that

treating a patient is an imposition of the therapist's will on the patient's; it is therefore a form of war. The basic principles of war are therefore applicable. They may actually be very useful, for an opposing army is like a patient in that both are [very complex, inherently stable, etc.]. A basic method much used in war is to use a maximal concentration of all possible forces on to a small part,

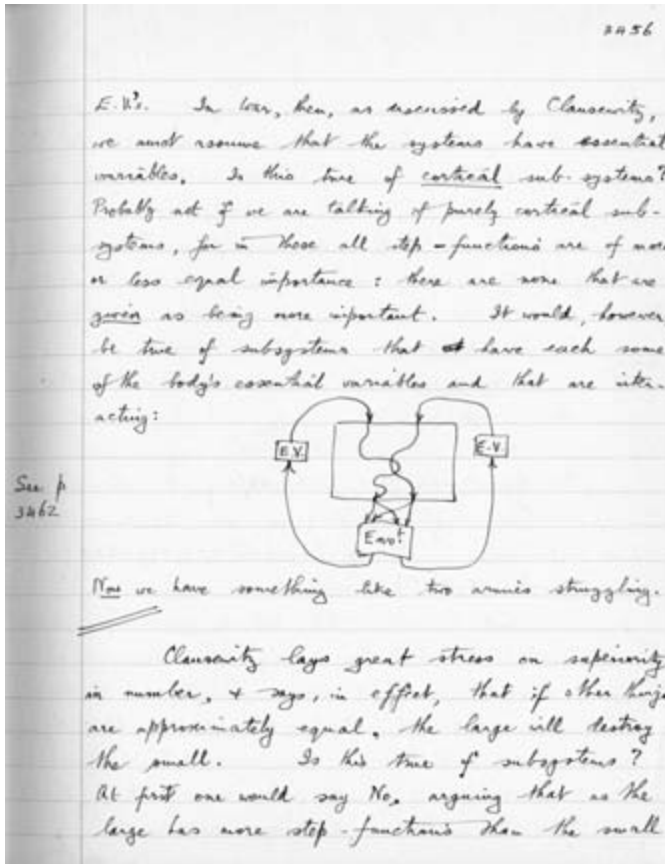


Figure 4.12. War among subsystems in the cortex. Source: Ashby's journal, entry dated 8 September 1951 (p. 3456). (By permission of Jill Ashby, Sally Bannister, and Ruth Pettit.)

to try to get it unstabilised. The gain here may be semi-permanent, so that, with this holding, the forces can then attack another point. With this in mind, a Blitz-therapy would be characterised by:- (1) Use of techniques in combination, simultaneously. E.g. LSD, then hypnosis while under it, & ECT while under the hypnosis. (2) Not waiting to "understand" the patient's pathology (psycho-, somato-, neuro-) but hitting hard & seeing what happens. (3) Get a change anyhow, then exploit it; when it comes to a stop, take violent action to get another change somehow. (4) Get normal every point you possibly can. (5) Apply pressure everywhere & notice whether any part of the psychosis shows signs of cracking. (6) Let the psychiatric team focus on one patient, others being ignored meanwhile. Summary: Blitz-therapy.

LSD, hypnosis and electroshock. . . . As I said of Grey Walter in the previous chapter, Ashby was hardly one of Deleuze and Guattari's disruptive nomads within the world of professional psychiatry, and we can no doubt understand that along similar lines. But this horrendous image of "Blitz-therapy"—what a combination of words!—does help to bring to the fore a characteristic feature of British psychiatry in the 1950s which is worth emphasizing for future reference, namely its utter *social asymmetry*. In Ashby's world, it went without saying that the only genuine agents in the mental hospital were the doctors. The patients were literally that, subject to the will of the psychiatrist, whose role was to apply whatever shocks might jolt the mentally ill into a homeostat-like change of state. In this world, Blitz-therapy and the association between psychiatry and war made perfect sense, psychiatrically and cybernetically. In the next chapter we can explore the form of psychiatry that took the other fork in the road, on the model of symmetric and reciprocal adaptation between patient and psychiatrist.⁴⁷

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One can see Ashby's military musings as a drift toward a more general social elaboration of his cybernetics. War, as Ashby thought of it, following Clausewitz, was an extreme form that the relations between adaptive systems might take on, but it was not the only form. I have been quoting from Ashby's notes on DAMS, psychiatry, and warfare from early September 1951, and right in the middle of them is an entry dated 12 September, which begins, "On arranging a society" (pp. 3460–62): "Here is an objection raised by Mrs Bassett, which will probably be raised by others. May it not happen for instance that the planner will assume that full mobility of labour is available, when in fact people don't always like moving: they may have friends in the district, they may like the countryside, they may have been born and bred there, or they may dislike change. What is to stop the planner riding rough-shod over these 'uneconomic' but very important feelings?" Mrs. Bassett was, I believe, a researcher at the Burden Neurological Institute with whom Ashby later published a paper on drug treatment for schizophrenia (Ashby, Collins, and Bassett 1960). She was evidently also an early spokeswoman for the Big Brother critique of cybernetics, and her argument drove Ashby to think about real everyday social relations:

The answer, of course, is that one sees to it that feedback loops pass through the people so that they are fully able to feel their conditions and to express opinions and take actions on them. One of the most important class of "essential

variables” in such a society would be those that measure the “comfort” of the individual. . . . It is obvious that the original objection was largely due to a belief that the planner must understand every detail of what he plans, & that therefore the Plan must be as finite as the intelligence of the Planner. This of course is not so. Using the principles of the multistable system it should be possible to develop, though not to understand, a Plan that is far superior to anything that any individual can devise. Coupled with this is the new possibility that it can be self-correcting. Summary: Society.

Here we see the usual emphasis on performativity as prior to representation, even in planning—“though not to understand”—and temporal emergence, but expressed now in a much more socially symmetric idiom than Ashby’s remarks on warfare and psychiatry. Now planners do not dictate to the planned how their lives will develop; instead planners and planned are envisaged as more or less equivalent parts of a single multistable system, entangled with one another in feedback loops from which transformations of the plan continually emerge. The image is the same as the vision of evolutionary design that Ashby articulated in relation to DAMS, transferred from the world of machines to that of people—now social designs and plans are to be understood not as given from the start and imposed on their object but as growing in the thick of things.

This is just one entry in Ashby’s journal. He never systematically developed a cybernetic sociology. I mention it now because these remarks can serve as an antidote to the idea that Ashby’s only vision of society was warfare, and, more important, because here he crudely sketches out a symmetric cybernetic vision of society that we shall see elaborated in all sorts of ways in the following chapters.

In conclusion, however, we can note that all traces of hierarchy were hardly purged from Ashby’s thinking. The sentences that I skipped above contain his reflections on just how “the people” should make themselves felt in the feedback loops that pass through them. “The ‘comfort’ of the individual . . . can easily be measured. One simply makes a rule that every protest or appeal must be accompanied by a sum of money, & the rule is that the more you pay the more effective will your appeal be. You can have a sixpenny appeal which will adjust trivialities up to a hundred-pound appeal that will move mountains.” This from a medical professional with a weakness for fast sports cars in a class-ridden society recovering from the devastations of war. It would be nice to think he was joking.

Cybernetics as a Theory of Everything

From the late 1920s until well into the 1950s Ashby's research aimed to understand the go of the brain. But this project faltered as the fifties went on. As we have just seen, Ashby's ambition to build a synthetic brain came to grief over his failure to get DAMS to accumulate adaptations. And, at the same time, as we saw in the previous chapter, the psychiatric milieu in which Ashby's cybernetics had grown started to shrink—as psychoactive drugs began to replace ECT and whatever, and as the antipsychiatric reaction to materialist psychiatry began to gain force. Where did those developments leave Ashby? Did he just give up? Evidently not. His mature cybernetics—that for which he is best remembered among cyberneticians today—in fact grew out of this smash-up, in ways that I can sketch out.

We can begin with what I called the “instability of the referent” of Ashby's cybernetics. Even when his concern was directly with the brain, he very often found himself thinking and writing about something else. His 1945 publication that included the bead-and-elastic device, for example, was framed as a discussion of a “dynamic system” or “machine” defined as “a collection of parts which (a) alter in time, and (b) interact on one another in some determinate and known manner. Given its state at any one moment it is assumed we know or can calculate what its state will be an instant later.” Ashby then asserted that “consideration seems to show that this is the most general possible description of a ‘machine’ . . . not in any way restricted to mechanical systems with Newtonian dynamics” (1945, 14). Ashby's conception of a “machine” was, then, from early on exceptionally broad, and correspondingly contentless, by no means tied to the brain. And the generality of this conception was itself underwritten by a mathematical formalism he first introduced in his original 1940 protocybernetic publication, the set of equations describing the temporal behavior of what he later called a *state-determined system*, namely,

$$dx_i/dt = f_i(x_1, x_2, \dots, x_n) \quad \text{for } i = 1, 2, \dots, n,$$

where t stands for time, x_i are the variables characterizing the system, and f_i is some mathematical function of the x_i .

Since Ashby subsequently argued that almost all the systems described by science are state-determined systems, one can begin to see what I mean by the instability of the referent of his cybernetics: though he was trying to understand the brain as a machine, from the outset his concept of a machine

was more or less coextensive with all of the contents of the universe. And this accounts for some of the rhetorical incongruity of Ashby's early cybernetic writings. For example, although it was published in the *Journal of General Psychology*, Ashby's 1945 bead-and-elastic essay contains remarkably little psychological content in comparison with its discussion of machines. It opens with the remark that "it is the purpose of this paper to suggest that [adaptive] behavior is in no way special to living things, that it is an elementary and fundamental property of all matter," it defines its topic as "all dynamic systems, whether living or dead" (13), and it closes with the assertion that "this type of adaptation (by trial and error) is therefore an essential property of matter, and no 'vital' or 'selective' hypothesis is required" (24). One wonders where the brain has gone in this story—to which Ashby's answer is that "the sole special hypothesis required is that the animal is provided with a sufficiency of breaks" (19), that is, plenty of elastic bands. "The only other point to mention at present is that the development of a nervous system will provide vastly greater opportunities both for the number of breaks available and also for complexity and variety of organization. Here I would emphasize that the difference . . . is solely one of degree and not of principle" (20).

So we see that in parallel to his inquiries into the brain, and indeed constitutive of those inquiries, went Ashby's technical development of an entire worldview—a view of the cosmos, animate and inanimate, as built out of state-determined machines. And my general suggestion then is that, as the lines of Ashby's research specifically directed toward the brain ran out of steam in the 1950s, so the cybernetic worldview in general came to the fore. And this shift in emphasis in his research was only reinforced by the *range* of disparate systems that Ashby described and analyzed in enriching his intuition about the properties of state-determined machines. I have already mentioned his discussions of chicken incubators and bead-and-elastic contrivances (the latter described as a "typical and clear-cut example of a dynamic system" [Ashby 1945a, 15]). The homeostat itself was first conceived as a material incarnation of Ashby's basic set of equations; his analysis of discontinuities in autocatalytic chemical reactions, discussed above, likewise concerned a special case of those equations. In *Design for a Brain* Ashby outlined the capabilities of a homeostatic autopilot—even if you wire it up backward so that its initial tendency is to destabilize a plane's flight, it will adapt and learn to keep the plane level anyway. And later in the book he spelled out the moral for evolutionary biology—namely, that complex systems will tend over time to arrive at complicated and interesting equilibriums with their environment. Such equilibriums, he argued are definitional of *life*, and therefore, "the development of

life on earth must thus *not* be seen as something remarkable. On the contrary, it was inevitable” (233)—foreshadowing the sentiments of Stuart Kauffman’s book *At Home in the Universe* (1995) four decades in advance. Ashby’s single venture into the field of economics is also relevant. In 1945, the third of his early cybernetic publications was a short letter to the journal *Nature*, entitled “Effect of Controls on Stability” (Ashby 1945b). There he recycled his chicken-incubator argument about “stabilizing the stabilizer” as a mathematical analysis of the price controls which the new Labour government was widely expected to impose, showing that they might lead to the opposite result from that intended, namely a destabilization rather than stabilization of the British economy.⁴⁸ This reminds us that, as we have just seen, in his journal he was also happy to extend his analysis of the multistable system to both social planning and warfare.

Almost without intending it, then, in the course of his research into normal and pathological brains, Ashby spun off a version of cybernetics as a supremely general and protean science, with exemplifications that cut right across the disciplinary map—in a certain kind of mathematics, engineering, chemistry, evolutionary biology, economics, planning, and military science (if one calls it that), as well as brain science and psychiatry. And as obstacles were encountered in his specifically brain-oriented work, the brain lost its leading position on Ashby’s agenda and he turned more and more toward the development of cybernetics as a freestanding general science. This was the conception that he laid out in his second book, *An Introduction to Cybernetics*, in 1956, and which he and his students continued to elaborate in his Illinois years.⁴⁹ I am not going to go in any detail into the contents of *Introduction* or of the work that grew out of it. The thrust of this work was formal (in contrast to the materiality of the homeostat and DAMS), and to follow it would take us away from the concerns of this book. I will mention some specific aspects of Ashby’s later work in the following sections, but here I need to say a few words specifically about *An Introduction to Cybernetics*, partly out of respect for its author and partly because it leads into matters discussed in later chapters.⁵⁰

An Introduction to Cybernetics presents itself as a textbook, probably the first and perhaps the last introductory textbook on cybernetics to be written. It aims to present the “basic ideas of cybernetics,” up to and including “feedback, stability, regulation, ultrastability, information, coding, [and] noise” (Ashby 1956, v). Some of the strangeness of Ashby’s rhetoric remains in it. Repeatedly and from the very start, he insists that he is writing for “workers in the biological sciences—physiologists, psychologists, sociologists” (1960, v)

with ecologists and economists elsewhere included in the set. But just as real brains make few appearances in *Design for a Brain*, the appearances of real physiology and so on are notable by their infrequency in *An Introduction to Cybernetics*. The truly revealing definition of cybernetics that Ashby gives is on page 2: cybernetics offers “the framework on which all individual machines may be ordered, related and understood.”⁵¹

An Introduction to Cybernetics is distinguished from *Design for a Brain* by one major stylistic innovation, the introduction of a matrix notation for the transformation of machine states in discrete time steps (in contrast to the continuous time of the equations for a state-determined system). Ontologically, this highlights for the reader that Ashby’s concern is with change in time, and, indeed, the title of the first substantive chapter, chapter 2, is “Change” (with subheadings “Transformation” and “Repeated Change”). The new notation is primarily put to work in an analysis of the regulatory capacity of machines. “Regulation” is one of the new terms that appeared in Ashby’s list of the basic ideas of cybernetics above, though its meaning is obvious enough. All of the machines we have discussed thus far—thermostats, servomechanisms, the homeostat, DAMS—are regulators of various degrees of sophistication, acting to keep some variables within limits (the temperature in a room, the essential variables of the body). What Ashby adds to the general discussion of regulation in *An Introduction to Cybernetics*, and his claim to undying eponymous fame, is the law of requisite variety, which forms the centerpiece of the book and is known to his admirers as Ashby’s law. This connects to the other novel terms in *An Introduction to Cybernetics*’s list of basic ideas of cybernetics—information, coding, and noise—and thence to Claude Shannon’s foundational work in information theory (Shannon and Weaver 1963 [1949]). One could, in fact, take this interest in “information” as definitive of Ashby’s mature work. I have no wish to enter into information theory here; it is a field in its own right. But I will briefly explain the law of requisite variety.⁵²

Shannon was concerned with questions of efficiency in sending messages down communication channels such as telephone lines, and he defined the quantity of information transmitted in terms of a selection between the total number of possible messages. This total can be characterized as the *variety* of the set of messages. If the set comprised just two possible messages—say, “yes” or “no” in answer to some question—then getting an answer one way or the other would count as the transmission of one bit (in the technical sense) of information in selecting between the two options. In effect, Ashby transposed information theory from a representational idiom, having to do with messages and communication, to a performative one, having to do with machines

and their configurations. On Ashby's definition, the variety of a machine was defined precisely as the number of distinguishable states that it could take on. This put Ashby in a position to make quantitative statements and even prove theorems about the regulation of one machine or system by another, and pre-eminent among these statements was Ashby's law, which says, very simply, that "only variety can destroy variety" (Ashby 1956, 207).

To translate, as Ashby did in *An Introduction to Cybernetics*, a regulator is a blocker—it stops some environmental disturbance from having its full impact on some essential variable, say, as in the case of the homeostat. And then it stands to reason that to be an effective blocker one must have at least as much flexibility as that which is to be blocked. If the environment can take on twenty-five states, the regulator had better be able to take on at least twenty-five as well—otherwise, one of the environment's dodges and feints will get straight past the regulator and upset the essential variable. I have stated this in words; Ashby, of course, used his new machine notation as a means to a formal proof and elaboration; but thus Ashby's law.

To be able to make quantitative calculations and produce formal proofs was a major step forward from the qualitative arguments of *Design for a Brain*, in making cybernetics more recognizably a science like the modern sciences, and it is not surprising that much of the later work of Ashby and his students and followers capitalized on this bridgehead in all sorts of ways. It put Ashby in a position, for example, to dwell repeatedly on what he called Bremermann's limit. This was a quantum-mechanical and relativistic estimate of the upper limit on the rate of information processing by matter, which sufficed to make some otherwise plausible accounts of information processing look ridiculous—they could not be implemented in a finite time even if the entire universe were harnessed just to that purpose.⁵³ But there I am going to leave this general topic; Ashby's law will return with Stafford Beer in chapter 6.⁵⁴

Cybernetics and Epistemology

I have been exploring Ashby's cybernetics as ontology, because that is where his real originality and certainly his importance for me lies. He showed how a nonmodern ontology could be brought down to earth as engineering which was also brain science, with ramifications extending in endless directions. That is what I wanted to focus on. But Ashby did epistemology, too. If the Ur-referent of his cybernetics was preconscious, precognitive adaptation at deep levels of the brain, he was also willing to climb the brain stem to discuss cognition, articulated knowledge, science, and even painting and music, and

I want just to sketch out his approach to these topics. I begin with what I take to be right about his epistemology and then turn to critique.

How can we characterize Ashby's vision of knowledge? First, it was a deflationary and pragmatic one. Ashby insisted that "knowledge is finite" (Ashby 1963, 56). It never exceeds the amount of information on which it rests, which is itself finite, the product of a finite amount of work. It is therefore a mistake to imagine that our knowledge ever attains the status of a truth that transcends its origins—that it achieves an unshakeable correspondence to its object, as I would put it. According to Ashby, this observation ruled out of court most of the contemporary philosophical discourse on topics like induction that has come down to us from the Greeks. And, having discarded truth as the key topic for epistemological reflection, he came to focus on "the practical usefulness of models" (Ashby 1970, 95) in helping us get on with mundane, worldly projects.⁵⁵ The great thing about a model, according to Ashby, is that it enables us to *lose* information, and to arrive at something more tractable, handle-able, manipulable, than the object itself in its infinite complexity. As he put it, "No electronic model of a cat's brain can possibly be as true as that provided by the brain of another cat, yet of what use is the latter as a model?" (1970, 96). Models are thus our best hope of evading Bremermann's limit in getting to grips with the awful diversity of the world (1970, 98–100).

For Ashby, then, knowledge was to be thought of as engaged in practical projects and worldly performances, and one late essay, written with his student Roger Conant, can serve to bring this home. "Every Good Regulator of a System Must Be a Model of That System" (Conant and Ashby 1970) concerned the optimal method of feedback control. The authors discussed two different feedback arrangements: error- and cause-controlled. The former is typified by a household thermostat and is intrinsically imperfect. The thermostat has to wait until the environment drives the living-room temperature away from its desired setting before it can go to work to correct the deviation. Error control thus never quite gets it right: some errors always remain—deviations from the optimum—even though they might be much reduced by the feedback mechanism. A cause-controlled regulator, in contrast, does not need to wait for something to go wrong before it acts. A cause-controlled thermostat, for example, would monitor the conditions outside a building, predict what those conditions would do to the interior temperature, and take steps in advance to counter that—turning down the heating as soon as the sun came out or whatever. Unlike error control, cause control might approach perfection: all traces of environmental fluctuations might be blocked from affecting the controlled

system; room temperature might never fluctuate at all. And the result that Conant and Ashby formally proved in this essay (subject to formal conditions and qualifications) was that the minimal condition for optimal cause control was that the regulator should contain a model of the regulated system.

Intuitively, of course, this seems obvious: the regulator has to “know” how changes in the environment will affect the system it regulates if it is to predict and cancel the effects of those changes, and the model is precisely that “knowledge.” Nevertheless, something interesting is going here. In fact, one can see the cause-controlled regulator as an important elaboration of Ashby’s ontological theater. The servomechanism, the homeostat, and DAMS staged, with increasing sophistication, an image of the brain as an adaptive organ performatively engaged with a lively world at the level of doing rather than knowing. This is undoubtedly the place to start if one wants to get the hang of the ontology of cybernetics. But, like CORA and *M. docilis*, the cause-controlled regulator invites us to think about the insertion of *knowledge* into this performative picture in a specific way. The virtue of knowledge lies not in its transcendental truth but in its usefulness in our performative engagements with the world. Knowledge is engaged with performance; epistemology with ontology. This performative epistemology, as I called it before, is the message of the cause-controlled regulator as ontological or epistemological theater; this is how we should think about knowledge cybernetically. Conversely, the cause-controlled regulator is a concrete example of how one might include the epistemic dimension in bringing ontology down to earth in engineering practice. That is what interests me most about this example.⁵⁶

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BASIC RESEARCH IS LIKE SHOOTING AN ARROW INTO THE AIR, AND, WHERE IT LANDS, PAINTING A TARGET.

HOMER ADKINS, CHEMIST, QUOTED IN BUCHANAN (2007, 213)

Now we can return to the critique I began earlier. In discussing the homeostat I noted that it had a fixed and pre-given goal—to keep its essential variables within limits, and I suggested that this is a bad image to have in general. At that stage, however, the referent of the essential variables was still some inner parameter analogous to the temperature of the blood—a slippery concept to criticize. But in his more epistemological writings, Ashby moved easily to a discussion of goals which clearly pertain to states of the outer, rather than the inner, world. An essay on “Genius,” written with another of his students,

Crayton Walker, can serve to illustrate some consistent strands of Ashby's thinking on this (Ashby and Walker 1968).

The topic of "Genius" is more or less self-explanatory. In line with the above discussion, Ashby and Walker aim at a deflationary and naturalistic account of the phenomena we associate with word "genius." But to do so, they sketch out an account of knowledge production in which the importance of *predefined goals* is constantly repeated. "On an IQ test, *appropriate* [selection of answers in a multiple choice test] means correct, but not so much in an objective sense as in the sense that it satisfies a decision made *in advance* (by the test makers) about which answers show high and which low intelligence. In evaluating genius, it makes an enormous difference whether the criterion for appropriateness [i.e., the goal] was decided *before* or *after* the critical performance has taken place. . . . Has he succeeded or failed? The question has no meaning in the absence of a declared goal. The latter is like the marksman's saying he really meant to miss the target all along" (Ashby and Walker 1968, 209–10). And, indeed, Ashby and Walker are clear that they understand these goals as explicit targets in the outer world (and not, for example, keeping one's blood temperature constant): "In 1650, during Newton's time, many mathematicians were trying to explain Galileo's experimental findings. . . . In Michelangelo's day, the technical problems of perspective . . . were being widely discussed" (210). The great scientist and the great artist thus both knew what they were aiming for, and their "genius" lay in hitting their specified targets (before anyone else did).

I can find nothing good to say about this aspect of Ashby's work. My own historical research has confronted me with many examples in which great scientific accomplishments were in fact bound up with shifts in goals, and without making a statistical analysis I would be willing to bet that most of the accomplishments we routinely attribute to "genius" have precisely that quality. I therefore think that while it is reasonable to regard the fixity of the homeostat's goals as possibly a good model for some biological processes and a possibly unavoidable electromechanical limitation, it would be a mistake to follow Ashby's normative insistence that fixed goals necessarily characterize epistemological practice. This is one point at which we should draw the line in looking to his cybernetics for inspiration.

Beyond that, there is the question of how cognitive goals are to be achieved. Once Ashby and Walker have insisted that the goals of knowledge production have to be fixed in advance, they can remark that "the theorems of information theory are directly applicable to problems of this kind" (Ashby and Walker 1968, 210). They thus work themselves into the heartland of Ashby's

mature cybernetics, where, it turns out, the key question is that of *selection*.⁵⁷ Just as the homeostat might be said to select the right settings of its uniselectors to achieve its goal of homeostasis, so, indeed, should all forms of human cultural production be considered likewise (210):

To illustrate, suppose that Michelangelo made one million brush strokes in painting the Sistine Chapel. Suppose also that, being highly skilled, at each brush stroke he selected one of the two best, so that where the average painter would have ranged over ten, Michelangelo would have regarded eight as inferior. At each brush stroke he would have been selecting appropriately in the intensity of one in five. Over the million brush strokes the intensity would have been one in $5^{1,000,000}$. The intensity of Michelangelo's selection can be likened to his picking out one painting from five-raised-to-the-one-millionth-power, which is a large number of paintings (roughly 1 followed by 699,000 zeroes). Since this number is approximately the same as $2^{3,320,000}$, the theorem says that Michelangelo must have processed at least 3,320,000 "bits" of information, in the units of information theory, to achieve the results he did. He *must* have done so, according to the axiom, because appropriate selections can only be achieved if enough information is received and processed to make them happen.

Ashby and Walker go on to deduce from this that Michelangelo must have worked really hard over a long period of time to process the required amount of information, and they produce a few historical quotations to back this up. They also extend the same form of analysis to Newton, Gauss, and Einstein (selecting the right scientific theories or mathematical axioms from an enormous range of possibilities), Picasso (back to painting), Johann Sebastian Bach (picking just the right notes in a musical composition), and even Adolf Hitler, who "had many extraordinary successes before 1942 and was often acclaimed a genius, especially by the Germans" (207).

What can one say about all this? There is again something profoundly wrong about the image of "selection" that runs through Ashby's epistemology and even, before that, his ontology. There is something entirely implausible in the idea of Michelangelo's picking the right painting from a preexisting set or Einstein's doing the same in science. My own studies of scientific practice have never thrown up a single instance that could be adequately described in those terms (even if there is a branch of mainstream philosophy of science that does conceive "theory choice" along those lines). What I have found instead are many instances of open-ended, trial-and-error *extensions* of scientific culture. Rather than selecting between existing possibilities, scientists (and

artists, and everyone else, I think) continually construct new ones and see how they play out. This is also a cybernetic image of epistemology—but one that emphasizes creativity and the appearance of genuine novelty in the world (both human and nonhuman) that the homeostat cannot model. The homeostat can only offer us selection and combinatorics. I have already discussed the homeostat's virtues as ontological theater at length; here my suggestion is that we should not follow it into the details of Ashby's epistemology.⁵⁸

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I want to end this chapter by moving beyond Ashby's work, so here I should offer a summary of what has been a long discussion. What was this chapter about?

One concern was historical. Continuing the discussion of Walter's work, I have tried to show that psychiatry, understood as the overall problematic of understanding and treating mental illness, was both a surface of emergence and a surface of return for Ashby's cybernetics. In important ways, his cybernetics can be seen to have grown out of his professional concerns with mental illness, and though the development of Ashby's hobby had its own dynamics and grew in other directions, too, he was interested, at least until the late 1950s, in seeing how it might feed back into psychiatry. At the same time, we have explored some of the axes along which Ashby's cybernetics went beyond the brain and invaded other fields: from a certain style of adaptive engineering (the homeostat, DAMS) to a general analysis of machines and a theory of everything, exemplified in Ashby's discussions of autopilots, economics, chemistry, evolutionary biology, war, planning, and epistemology. Ashby even articulated a form of spirituality appropriate to his cybernetics: "I am now . . . a Time-worshipper." In this way, the chapter continues the task of mapping out the multiplicity of cybernetics.

Another concern of the chapter has been ontological. I have argued that we can see the homeostat, and especially the multihomeostat setups that Ashby worked with, as ontological theater—as a model for a more general state of affairs: a world of dynamic entities evolving in performative (rather than representational) interaction with one another. Like the tortoise, the homeostat searched its world and reacted to what it found there. Unlike the tortoise's, the homeostat's world was as lively as the machine itself, simulated in a symmetric fashion by more homeostats. This symmetry, and the vision of a lively and dynamic world that goes with it, was Ashby's great contribution to the early development of cybernetics, and we will see it further elaborated as we go on. Conversely, once we have grasped the ontological import of Ashby's

cybernetics, we can also see it from the opposite angle: as ontology in action, as playing out for us and exemplifying the sorts of project in many fields that might go with an ontology of performance and unknowability.

We have also examined the sort of performative epistemology that Ashby developed in relation to his brain research, and I emphasized the gearing of knowledge into performance that defined this. Here I also ventured into critique, arguing that we need not, and should not, accept all of the ontological and epistemological visions that Ashby staged for us. Especially, I argued against his insistence on the fixity of goals and his idea that performance and representation inhabit a given space of possibilities from which selections are made.

At the level of substance, we have seen that Ashby, like Walter, aimed at a modern science of the brain—at opening up the Black Box. And we have seen that he succeeded in this: the homeostat can indeed be counted as a model of the sort of adaptive processes that might happen in the brain. But the hybridity of Ashby's cybernetics, like Walter's, is again evident. In their mode of adaptation, Ashby's electromechanical assemblages themselves had, as their necessary counterpart, an unknowable world to which they adapted performatively. As ontological theater, his brain models inescapably return us to a picture of engagement with the unknown.

Furthermore, we have seen that that Ashby's cybernetics never quite achieved the form of a classically modern science. His scientific models were revealing from one angle, but opaque from another. To know how they were built did not carry with it a predictive understanding of what they would do. The only way to find out was to run them and see (finding out whether multihomeostat arrays with fixed internal settings would be stable or not, finding out what DAMS would do). This was the cybernetic discovery of complexity within a different set of projects from Walter's: the discovery that beyond some level of complexity, machines (and mathematical models) can themselves become mini-Black Boxes, which we can take as ontological icons, themselves models of the stuff from which the world is built. It was in this context that Ashby articulated a distinctively cybernetic philosophy of evolutionary design—design in medias res—very different from the blueprint attitude of modern engineering design, the stance of a detached observer who commands matter via a detour through knowledge.

Finally, the chapter thus far also explored the social basis of Ashby's cybernetics. Like Walter's, Ashby's distinctively cybernetic work was nomadic, finding a home in transitory institutions like the Ratio Club, the Macy and Namur conferences, and the Biological Computer Laboratory, where Ashby

ended his career. I noted, though, that Ashby was hardly a disruptive nomad in his professional home, the mental hospital. There, like Walter, he took for granted established views of mental illness and therapy and existing social relations, even while developing novel theoretical accounts of the origins of mental illness in the biological brain and of the mechanisms of the great and desperate cures. This was a respect in which Ashby's cybernetics reinforced, rather than challenged, the status quo.

The last feature of Ashby's cybernetics that I want to stress is its seriousness. His journal records forty-four years' worth of hard, technical work, 7,189 pages of it, trying to think clearly and precisely about the brain and machines and about all the ancillary topics that that threw up. I want to stress this now because this seriousness of cybernetics is important to bear in mind throughout this book. My other cyberneticians were also serious, and they also did an enormous amount of hard technical work, but their cybernetics was not as unremittingly serious as Ashby's. Often it is hard to doubt that they were having fun, too. I consider this undoing of the boundary between serious science and fun yet another attractive feature of cybernetics as a model for practice. But there is a danger that it is the image of Allen Ginsberg taking LSD coupled to a flicker machine by a Grey Walter-style biofeedback mechanism, or of Stafford Beer invoking the Yogic chakras or the mystical geometry of the enneagram, that might stick in the reader's mind. I simply repeat here, therefore, that what fascinates me about cybernetics is that its projects could run the distance from the intensely technical to the far out. Putting this somewhat more strongly, my argument would have to be that the technical development of cybernetics encourages us to reflect that its more outré aspects were perhaps not as far out as we might think. The nonmodern is bound to look more or less strange.

A New Kind of Science:

Alexander, Kauffman, and Wolfram

In the previous chapter, I explored some of the lines of work that grew out of Grey Walter's cybernetics, from robotics to the Beats and biofeedback, and I want to do something similar here, looking briefly at other work up to the present that resonates with Ashby's. My examples are taken from the work of Christopher Alexander, Stuart Kauffman, and Stephen Wolfram. One concern is again with the protean quality of cybernetics: here we can follow the development of distinctively Ashby-ite approaches into the fields of architecture, theoretical biology, mathematics, and beyond. The other concern is to explore further developments in the Ashby-ite problematic of complexity.

The three examples carry us progressively further away from real historical connections to Ashby, but, as I said in the opening chapters, it is the overall cybernetic stance in the world that I am trying to get clear on here, rather than lines of historical filiation.

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IN ALEXANDER'S VIEW, MODERNITY IS A SORT OF TEMPORARY ABERRATION.

HILDE HEYNEN, *ARCHITECTURE AND MODERNITY* (1999, 20)

Christopher Alexander was born in Vienna in 1936 but grew up in England, graduated from Cambridge having studied mathematics and architecture, and then went to the other Cambridge, where he did a PhD in architecture at Harvard. In 1963 he became a professor of architecture at the University of California, Berkeley, retiring as an emeritus professor in 1998. British readers will be impressed, one way or the other, by the fact that from 1990 to 1995 he was a trustee of Prince Charles's Institute of Architecture. Alexander is best known for his later notion of "pattern languages," but I want to focus here on his first book, *Notes on the Synthesis of Form* (1964), the published version of his prize-winning PhD dissertation.⁵⁹

The book takes us back to questions of design and is a critique of contemporary design methods, in general but especially in architecture. At its heart are two ideal types of design: "unselfconscious" methods (primitive, traditional, simple) and "selfconscious" ones (contemporary, professional, modern), and Alexander draws explicitly on *Design for a Brain* (the second edition, of 1960) to make this contrast.⁶⁰ The key concept that he takes there from Ashby is precisely the notion of adaptation, and his argument is that unselfconscious buildings, exemplified by the Mousgoum hut built by African tribes in French Cameroon, are well-adapted buildings in several senses: in the relation of their internal parts to one another, to their material environment, and to the social being of their inhabitants (Alexander 1964, 30). Contemporary Western buildings, in contrast, do not possess these features, is the claim, and the distinction lies for Alexander in the way that architecture responds to problems and misfits arising in construction and use. His idea is that in traditional design such misfits are localized, finite problems that are readily fixed in a piecemeal fashion, while in the field of self-conscious design, attempts to fix misfits ramify endlessly: "If there is not enough light in a house, for instance, and more windows are added to correct this failure, the change may improve the light but allow too little privacy; another change for

more light makes the windows bigger, perhaps, but thereby makes the house more likely to collapse” (1964, 42).

The details here are not important, but I want to note the distinctly Ashbyite way in which Alexander frames the problem in order to set up his own solution of it, a solution which is arguably at the heart of Alexander’s subsequent career. As discussed earlier, in a key passage of *Design for a Brain* Ashby gave estimates of the time for multihomeostat systems to achieve equilibrium, ranging from short to impossibly long, depending upon the density of interconnections between the homeostats. In the second edition of *Design*, he illustrated these estimates by thinking about a set of rotors, each with two positions labeled A and B, and asking how long it would take various spinning strategies to achieve a distribution of, say, all As showing and no Bs (Ashby 1960, 151). In *Notes on the Synthesis of Form*, Alexander simply translates this illustration into his own terms, with ample acknowledgment to Ashby but with an interesting twist.

Alexander invites the reader to consider an array of one hundred lightbulbs that can be either on, standing for a misfit in the design process, or off, for no misfit. This array evolves in time steps according to certain rules. Any light that is on has a 50-50 chance of going off at the next step. Any light that is off has a 50-50 chance of coming back on if at least one light to which it is connected is on, but no chance if the connected lights are all off. And then one can see how the argument goes. The destiny of any such system is eventually to become dark: once all the lights are off—all the misfits have been dealt with—none of them can ever, according to the rules, come back on again. So, following Ashby exactly, Alexander remarks, “The only question that remains is, how long will it take for this to happen? It is not hard to see that apart from chance this depends only on the pattern of interconnection between the lights” (1964, 40).⁶¹

Alexander then follows Ashby again in providing three estimates for the time to darkness. The first is the situation of independent adaptation. If the lights have no meaningful connections to one another, then this time is basically the time required for any single light to go dark: 2 seconds, if each time step is 1 second. At the other extreme, if each light is connected to all the others, then the only way in which the lights that remain on can be prevented from reexciting the lights that have gone off is by all of the lights happening to go off in the same time step, which one can estimate will take of the order of 2^{100} seconds, or 10^{22} years—one of those hyperastronomical times that were crucial to the development of Ashby’s project. Alexander then considers a third possibility which differs in an important way from Ashby’s third possibil-

ity. In *Design for a Brain*, Ashby gets his third estimate by thinking about the situation in which any rotor that comes up A is left alone and the other rotors are spun again, and so on until there are no Bs left. Alexander, in contrast, considers the situation in which the one hundred lights fall into *subsystems* of ten lights each. These subsystems are assumed to be largely independent of one another but densely connected internally. In this case, the time to darkness of the whole system will be of the order of the time for any one subsystem to go dark, namely 2^{10} seconds, or about a quarter of an hour—quite a reasonable number.

We recognize this line of thought from *Design*, but the advantage of putting it this way is that it sets up Alexander's own solution to the problem of design. Our contemporary problems in architecture stem from the fact that the variables we tinker with are not sufficiently independent of one another, so that tinkering with any one of them sets up problems elsewhere, like the lit light-bulbs turning on the others. And what we should do, therefore, is to “diagonalize” (my word) the variables—we should find some new design variables such that design problems only bear upon subsets of them that are loosely coupled to others, like the subsystems of ten lights in the example. That way, we can get to grips with our problems in a finite time and our buildings will reach an adapted state: just as in unselfconscious buildings, the internal components will fit together in all sorts of ways, and whole buildings will mesh with their environments and inhabitants. And this is indeed the path that Alexander follows in the later chapters of *Notes on the Synthesis of Form*, where he proposes empirical methods and mathematical techniques for finding appropriate sets of design variables. One can also, though I will not go into this, see this reasoning as the key to his later work on pattern languages: the enduring patterns that Alexander came to focus on there refer to recurring design problems and solutions that can be considered in relative isolation from others and thus suggest a realistically piecemeal approach to designing adapted buildings, neighborhoods, cities, conurbations, or whatever (Alexander et al. 1977).

What can we take from this discussion? First, evidently, it is a nice example of the consequentiality of Ashby's work beyond the immediate community of cyberneticians. Second, it is another example of the undisciplined quality of the transmission of cybernetics through semipopular books like *Design for a Brain*. I know of no evidence of contact between Alexander and Ashby or other cyberneticians; it is reasonable to assume that Alexander simply read *Design* and saw what he could do with it, in much the same way as both Rodney Brooks and William Burroughs read Grey Walter. Along with this, we have another illustration of the protean quality of cybernetics. Ashby thought



Figure 4.13. The Linz Café. Source: Alexander 1983, 48.

he was writing about the brain, but Alexander immediately extended Ashby's discussion of connectedness to a continuing program in architecture and design, a field that Ashby never systematically thought about. We can thus take both Alexander's distinctive approach to architectural design and the actual buildings he has designed as further exemplars of the cybernetic ontology in action.⁶² Finally, we can note that Alexander's architecture is by no means uncontroversial. Alexander's "Linz Café" (1983) is an extended account of one of his projects (fig. 4.13) that includes the text of a debate at Harvard with Peter Eisenman. Alexander explains how the cafe was constructed around his "patterns" (58–59) but also emphasizes that the design elements needed to be individually "tuned" by building mock-ups and seeing what they felt like. The goal was to construct spaces that were truly "comfortable" for human beings. This tuning harks back to and exemplifies Alexander's earlier discussion of how problems can be and are solved on a piecemeal basis in traditional architecture, and the last section of his article discusses resonances between the Linz Café and historical buildings (59). In debate Eisenman tries to problematize Alexander's comfort principle and suggests a different, less harmonious

idea of architecture (theoretically inspired). Egged on by a sympathetic audience, Alexander remarks that “people who believe as you do are really fucking up the whole profession of architecture right now by propagating these beliefs” (67)—another marker of the fact that ontology makes a difference. We can return to this theme in a different and less “comfortable” guise when we come to Gordon Pask’s version of adaptive architecture.

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IT IS A FUNDAMENTAL QUESTION WHETHER METABOLIC STABILITY AND EPIGENESIS REQUIRE THE GENETIC REGULATORY CIRCUITS TO BE PRECISELY CONSTRUCTED. HAS A FORTUNATE EVOLUTIONARY HISTORY SELECTED ONLY NETS OF HIGHLY ORDERED CIRCUITS WHICH ALONE CAN INSURE METABOLIC STABILITY; OR ARE STABILITY AND EPIGENESIS, EVEN IN NETS OF RANDOMLY CONNECTED INTERCONNECTED REGULATORY CIRCUITS, TO BE EXPECTED AS THE PROBABLE CONSEQUENCE OF AS YET UNKNOWN MATHEMATICAL LAWS? ARE LIVING THINGS MORE AKIN TO PRECISELY PROGRAMMED AUTOMATA SELECTED BY EVOLUTION, OR TO RANDOMLY ASSEMBLED AUTOMATA WHOSE CHARACTERISTIC BEHAVIOR REFLECTS THEIR UNORDERLY CONSTRUCTION, NO MATTER HOW EVOLUTION SELECTED THE SURVIVING FORMS?

STUART KAUFFMAN, “METABOLIC STABILITY AND EPIGENESIS IN RANDOMLY CONSTRUCTED GENETIC NETS” (1969B, 438)

Now for Stuart Kauffman, one of the founders of contemporary theoretical biology, perhaps best known in the wider world for two books on a complex systems approach to the topics of biology and evolution, *At Home in the Universe* (1995) and *Investigations* (2002). I mentioned his important and explicitly cybernetic notion of “explanation by articulation of parts” in chapter 2, but now we can look at his biological research.⁶³

The pattern for Kauffman’s subsequent work was set in a group of his earliest scientific publications in the late 1960s and early 1970s, which concerned just the same problem that Alexander inherited from Ashby, the question of a large array of interacting elements achieving equilibrium. In *Design for a Brain*, Ashby considered two limits—situations in which interconnections between the elements were either minimal or maximal—and argued that the time to equilibrium would be small in one case and longer than the age of the universe in the other. The question that then arose was what happened in between these limits. Ashby had originally been thinking about an array of interacting homeostats, but one can simplify the situation by considering an

array of binary elements that switch each other on and off according to some rule—as did Alexander with his imaginary lightbulbs. The important point to stress, however, is that even such simple models are impossible to solve analytically. One cannot calculate in advance how they will behave; one simply has to run through a series of time steps, updating the binary variables at each step according to the chosen transformation rules, and see what the system will in fact do. This is the cybernetic discovery of complexity transcribed from the field of mechanisms to that of mathematical formalisms. Idealized binary arrays can remain Black Boxes as far as their aggregate behavior is concerned, even when the atomic rules that give rise to their behavior are known.

The only way to proceed in such a situation (apart from Alexander's trick of simply assuming that the array breaks up into almost disconnected pieces) is brute force. Hand calculation for a network of any size would be immensely tedious and time consuming, but at the University of Illinois Crayton Walker's 1965 PhD dissertation in psychology reported on his exploration of the time evolution of one-hundred-element binary arrays under a variety of simple transformation rules using the university's IBM 7094–1401 computer. Walker and Ashby (1966) wrote these findings up for publication, discussing how many steps different rule systems took to come to equilibrium, whether the equilibrium state was a fixed point or a cycle, how big the limit cycles were, and so on.⁶⁴ But it was Kauffman, rather than Walker and Ashby, who obtained the most important early results in this area, and at the same time Kauffman switched the focus from the brain to another very complex biological system, the cell.

Beginning in 1967, Kauffman published a series of papers grounded in computer simulations of randomly connected networks of binary elements, which he took to model the action of idealized genes, switching one another on and off (like lightbulbs, which indeed feature in *At Home in the Universe*). We could call what he had found a *discovery of simplicity* within complexity. A network of N binary elements has 2^N possible states, so that a one-thousand-element network can be in 2^{1000} distinct states, which is about 10^{300} —another one of those hyperastronomical numbers. But Kauffman established two fundamental findings, one concerning the inner, endogenous, dynamics of such nets, the other concerning exogenous perturbations.⁶⁵

On the first, Kauffman's simulations suggested that if each gene has exactly two inputs from other genes, then a randomly assembled network of one thousand genes would typically cycle among just twelve states—an astonishingly small number compared with 10^{300} (Kauffman 1969b, 444). Furthermore the lengths of these cycles—the number of states a network would pass through

before returning to a state it had visited before—were surprisingly short. He estimated, for example, that a network having a million elements would “possess behavior cycles of about one thousand states in length—an extreme localization of behavior among $2^{1,000,000}$ possible states” (446). And beyond that, Kauffman’s computer simulations revealed that the number of distinct cycles exhibited by any net was “as surprisingly small as the cycles are short” (448). He estimated that a net of one thousand elements, for example, would possess around just sixteen distinct cycles.

On the second, Kauffman had investigated what happened to established cycles when he introduced “noise” into his simulations—flipping single elements from one state to another during a cycle. The cycles proved largely resistant to such exogenous interference, returning to their original trajectories around 90% of the time. Sometimes, however, flipping a single element would jog the system from one cyclic pattern to one of a few others (452).

What did Kauffman make of these findings? At the most straightforward level, his argument was that a randomly connected network of idealized genes could serve as the model for a set of cell types (identified with the different cycles the network displayed), that the short cycle lengths of these cells were consistent with biological time scales, that the cells exhibited the biological requirement of stability against perturbations and chemical noise, and that the occasional transformations of cell types induced by noise corresponded to the puzzling fact of cellular differentiation in embryogenesis.⁶⁶ So his idealized gene networks could be held to be models of otherwise unexplained biological phenomena—and this was the sense in which his work counted as “theoretical biology.” At a grander level, the fact that these networks were randomly constructed was important, as indicated in the opening quotation from Kauffman. One might imagine that the stability of cells and their pathways of differentiation are determined by a detailed “circuit diagram” of control loops between genes, a circuit diagram laid down in a tortuous evolutionary history of mutation and selection. Kauffman had shown that one does not have to think that way. He had shown that complex systems can display self-organizing properties, properties arising from within the systems themselves, the emergence of a sort of “order out of chaos” (to borrow the title of Prigogine and Stengers 1984). This was the line of thought that led him eventually to the conclusion that we are “at home in the universe”—that life is what one should expect to find in any reasonably complex world, not something we should be surprised at and requiring any special explanation.⁶⁷

This is not the place to go into any more detail about Kauffman’s work, but I want to comment on what we have seen from several angles. First, I want to

return to the protean quality of cybernetics. Kauffman was clearly working in the same space as Ashby and Alexander—his basic problematic was much the same as theirs. But while their topic was the brain (as specified by Ashby) or architecture (as specified by Alexander), it was genes and cells and theoretical biology when specified by Kauffman.

Second, I want to comment on Kauffman's random networks, not as models of cells, but as ontological theater more generally. I argued before that tortoises, homeostats, and DAMS can, within certain limitations, be seen as electromechanical models that summon up for us the cybernetic ontology more broadly—machines whose aggregate performance is impenetrable. As discussed, Kauffman's idealized gene networks displayed the same character, but as emerging within a formal mathematical system rather than a material one. Now I want to note that as world models Kauffman's networks can also further enrich our ontological imaginations in important ways. On the one hand, these networks were livelier than, especially, Ashby's machines. Walter sometimes referred to the homeostat as *Machina sopora*—the sleeping machine. Its goal was to become quiescent; it changed state only when disturbed from outside. Kauffman's nets, in contrast, had their own endogenous dynamics, continually running through their cycles whether perturbed from the outside or not. On the other hand, these nets stage for us an image of systems with which we can genuinely interact, but not in the mode of command and control. The perturbations that Kauffman injected into their cycling disturbed the systems but did not serve to direct them into any other particular cycles.

This idea of systems that are not just performative and inscrutable but also dynamic and resistant to direction helps, I think, to give more substance to Beer's notion of "exceedingly complex systems" as the referent of cybernetics. The elaborations of cybernetics discussed in the following chapters circle around the problematic of getting along with systems fitting that general description, and Kauffman's nets can serve as an example of the kinds of things they are.⁶⁸

My last thought on Kauffman returns to the social basis of cybernetics. To emphasize the odd and improvised character of this, in the previous chapter (note 31) I listed the range of diverse academic and nonacademic affiliations of the participants at the first Namur conference. Kauffman's CV compresses the whole range and more into a single career. With BAs from Dartmouth College and Oxford University, he qualified as a doctor at the University of California, San Francisco, in 1968, while first writing up the findings discussed above as a visitor at MIT's Research Laboratory of Electronics in 1967. He was then

briefly an intern at Cincinnati General Hospital before becoming an assistant professor of biophysics and theoretical biology at the University of Chicago from 1969 to 1975. Overlapping with that, he was a surgeon at the National Cancer Institute in Bethesda from 1973 to 1975, before taking a tenured position in biochemistry and biophysics at the University of Pennsylvania in 1975. He formally retired from that position in 1995, but from 1986 to 1997 his primary affiliation was as a professor at the newly established Santa Fe Institute (SFI) in New Mexico. In 1996, he was the founding general partner of Bios Group, again in Santa Fe, and in 2004 he moved to the University of Calgary as director of the Institute for Biocomplexity and Informatics and professor in the departments of Biological Sciences and Physics and Astronomy.⁶⁹

It is not unreasonable to read this pattern as a familiar search for a congenial environment for a research career that sorts ill with conventional disciplinary and professional concerns and elicits more connections across disciplines and fields than within any one of them. The sociological novelty that appears here concerns two of Kauffman's later affiliations. The Santa Fe Institute was established in 1984 to foster a research agenda devoted to "simplicity, complexity, complex systems, and particularly complex adaptive systems" and is, in effect, an attempt to provide a relatively enduring social basis for the transient interdisciplinary communities—the Macy and Namur conferences, the Ratio Club—that were "home" to Walter, Ashby, and the rest of the first generation of cyberneticians. Notably, the SFI is a freestanding institution and not, for example, part of any university. The sociologically improvised character of cybernetics reappears here, but now at the level of institutions rather than individual careers.⁷⁰ And two other remarks on the SFI are relevant to our themes. One is that while the SFI serves the purpose of stabilizing a community of interdisciplinary researchers, it does not solve the problem of cultural transmission: as a private, nonprofit research institute it does not teach students and grant degrees.⁷¹ The other is that the price of institutionalization is, in this instance, a certain narrowing. The focus of research at the SFI is resolutely technical and mathematical. Ross Ashby might have been happy there, but not, I think, any of our other principals. Their work was too rich and diverse to be contained by such an agenda.

Besides the SFI, I should comment on Kauffman's affiliation with the Bios Group (which merged with NuTech Solutions in 2003). "BiosGroup was founded by Dr. Stuart Kauffman with a mission to tackle industry's toughest problems through the application of an emerging technology, Complexity Science."⁷² Here we have an attempt to establish a stable social basis for the science of complexity on a business rather than a scholarly model—a pattern

we have glimpsed before (with Rodney Brooks's business connections) and which will reappear immediately below. And once more we are confronted with the protean quality of cybernetics, with Kauffman's theoretical biology morphing into the world of capital.

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WE HAVE SUCCEEDED IN REDUCING ALL OF ORDINARY PHYSICAL BEHAVIOR TO A SIMPLE, CORRECT THEORY OF EVERYTHING ONLY TO DISCOVER THAT IT HAS REVEALED EXACTLY NOTHING ABOUT MANY THINGS OF GREAT IMPORTANCE.

R. B. LAUGHLIN AND DAVID PINES,
 "THE THEORY OF EVERYTHING" (2000, 28)

IT'S INTERESTING WHAT THE PRINCIPLE OF COMPUTATIONAL EQUIVALENCE ENDS UP SAYING. IT KIND OF ENCAPSULATES BOTH THE GREAT STRENGTH AND THE GREAT WEAKNESS OF SCIENCE. BECAUSE ON THE ONE HAND IT SAYS THAT ALL THE WONDERS OF THE UNIVERSE CAN BE CAPTURED BY SIMPLE RULES. YET IT ALSO SAYS THAT THERE'S ULTIMATELY NO WAY TO KNOW THE CONSEQUENCES OF THESE RULES—EXCEPT IN EFFECT JUST TO WATCH AND SEE HOW THEY UNFOLD.

STEPHEN WOLFRAM, "THE GENERATION OF FORM
 IN A NEW KIND OF SCIENCE" (2005, 36)

If the significance of Kauffman's work lay in his discovery of simplicity within complexity, Wolfram's achievement was to rediscover complexity within simplicity. Born in London in 1959, Stephen Wolfram was a child prodigy, like Wiener: Eton, Oxford, and a PhD from Caltech in 1979 at age twenty; he received a MacArthur "genius" award two years later. Wolfram's early work was in theoretical elementary-particle physics and cosmology, but two interests that defined his subsequent career emerged in the early 1980s: in cellular automata, on which more below, and in the development of computer software for doing mathematics. From 1983 to 1986 he held a permanent position at the Institute for Advanced Study in Princeton; from 1986 to 1988 he was professor of physics, mathematics and computer science at the University of Illinois at Urbana-Champaign, where he founded the Center for Complex Systems Research (sixteen years after Ashby had left—"shockingly, I don't think anyone at Illinois ever mentioned Ashby to me"; email to the author, 6 April 2007). In 1987 he founded Wolfram Research, a private company that develops and markets what has proved to be a highly successful product: Mathematica soft-

ware for mathematical computation. Besides running his company, Wolfram then spent the 1990s developing his work on cellular automata and related systems, in his spare time and without publishing any of it (echoes of Ashby's hobby). His silence ended in 2002 with a blaze of publicity for his massive, 1,280-page book, *A New Kind of Science*, published by his own company.⁷³

The key insight of the new kind of science, which Wolfram abbreviates to NKS, is that “incredibly simple rules can give rise to incredibly complicated behavior” (Wolfram 2005, 13), an idea grounded in Wolfram's explorations of simple, one-dimensional cellular automata. “Cellular automaton” is a forbidding name for a straightforward mathematical system. A one-dimensional CA is just a set of points on a line, with a binary variable, zero or one, assigned to each point. One imagines this system evolving in discrete time steps according to definite rules: a variable might change or stay the same according to its own present value and those of its two nearest neighbors, for example. How do such systems behave? The relationship of this problematic to Ashby's, Alexander's, and Kauffman's is clear: all three of them were looking at the properties of CAs, but much more complicated ones (effectively, in higher dimensions) than Wolfram's. And what Wolfram found—“playing with the animals,” as he once put it to me—was that even these almost childishly simple systems can generate enormously complex patterns.⁷⁴ Some do not: the pattern dies out after a few time steps; all the variables become zero, and nothing happens thereafter. But Wolfram's favorite example is the behavior of the rule 30 cellular automaton shown in figure 4.14 (one can list and number all possible transformation rules for linear CAs, and Wolfram simply ran them all on a computer).

If Kauffman was surprised that his networks displayed simple behavior, one can be even more surprised at the complexities that are generated by Wolfram's elementary rules. He argues that rule 30 (and other rules, too) turn out to be “computationally irreducible” in the sense that “there's essentially no way to work out what the system will do by any procedure that takes less computational effort than just running the system and seeing what happens.” There are no “shortcuts” to be found (Wolfram 2005, 30). And this observation is the starting point for the new kind of science (31):

In traditional theoretical science, there's sort of been an idealization made that the observer is infinitely computationally powerful relative to the system they're observing. But the point is that when there's complex behavior, the Principle of Computational Equivalence says that instead the system is just as computationally sophisticated as the observer. And that's what leads to

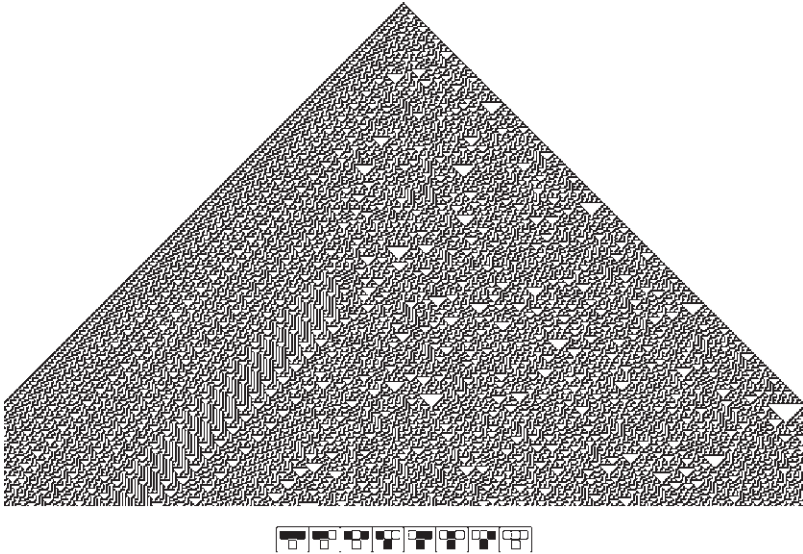


Figure 4.14. Rule 30 cellular automaton. Time steps move from the top downward; 1s are denoted by black cells, starting from a single 1. The transformation rule is shown at the bottom. Source: Wolfram 2005, 4. (Image courtesy of Wolfram Research, Inc. [] and Stephen Wolfram LLC, as used in Stephen Wolfram's *New Kind of Science* © 2002.)

computational irreducibility. And that's why traditional theoretical science hasn't been able to make more progress when one sees complexity. There are always pockets of reducibility where one can make progress, but there's always a core of computational irreducibility.

The classical sciences thus address just those “pockets” of the world where the traditional shortcuts can be made to work, while the reference of NKS is to all of the other aspects of the world where brute complexity is the rule, and much of Wolfram's work has been devoted to bringing this ontological perspective down to earth in all sorts of fields: mathematics; a sort of crystallography (e.g., snowflake structures); studies of turbulence; biology, where Wolfram's discussion echoes Kauffman's.⁷⁵ Having compared the patterns on mollusc shells to those generated by various CAs, Wolfram notes that (22)

it's very much as if the molluscs of the Earth are little computers—sampling the space of possible simple programs, and then displaying the results on their shells. You know, with all the emphasis on natural selection, one's gotten used to the idea that there can't be much of a fundamental theory in biology—and

that practically everything we see must just reflect detailed accidents in the history of biological evolution. But what the mollusc shell example suggests is that that may not be so. And that somehow one can think of organisms as uniformly sampling a space of possible programs. So that just knowing abstractly about the space of programs will tell one about biology

And, of course, reflecting his disciplinary origins, Wolfram also sees the NKS as offering a “truly fundamental theory of physics.” Space, time and causality are merely appearances, themselves emerging from a discrete network of points—and the ultimate task of physics is then to find out what rule the system is running. “It’s going to be fascinating—and perhaps humbling—to see just where our universe is. The hundredth rule? Or the millionth? Or the quintillionth? But I’m increasingly optimistic that this is all really going to work. And that eventually out there in the computational universe we’ll find our universe. With all of our physics. And that will certainly be an exciting moment for science” (27).

We can thus see Wolfram’s work as a further variant on the theme that Ashby set out in 1952 in his considerations of the time to reach equilibrium of multihomeostat assemblages, but differing from the other variants in interesting and important ways. Unlike Alexander and Kauffman, Wolfram has generalized and ontologized the problematic, turning it into an account of how the world is, as well as respecifying it in the domains mentioned above and more. Beyond that, from our point of view, Wolfram’s distinctive contribution has been to focus on systems that do not settle down into equilibrium, that perform in unpredictable ways, and to suggest that that is the world’s ontological condition. His NKS thus offers us a further enrichment of our ontological imaginations. Systems like the rule 30 CA genuinely *become*; the only way to find out what they will do next is run the rule on their present configuration and find out. As ontological theater, they help us to imagine the world that way; they add becoming to our models of what Beer’s “exceedingly complex systems” might be like. If we think of the world as built from CA-like entities, we have a richer grasp of the cybernetic ontology.

It remains only to comment on the social basis of Wolfram’s work. We have seen already that after a meteoric but otherwise conventional career in academic research Wolfram (like Kauffman) veered off into business, and that this business enabled him to sustain his unusual hobby (like Ashby)—providing both a living and research tools. There is the usual improvised oddity here, evident in the biographies of all our cyberneticians. What I should add is that having launched NKS with his 2002 book, Wolfram has since sought to

foster the growth of the field with an annual series of conferences and summer schools. Organized by Wolfram's group, these parallel the Santa Fe Institute in existing outside the usual academic circuits, and one can again see them as an attempt to stabilize a novel social base for a novel kind of science. Nine of the eleven people listed as faculty for the 2005 NKS summer school worked for, or had worked for, Wolfram Research, including Wolfram himself, and the website for the school mentions that, in the past, "some of our most talented attendees have been offered positions at Wolfram Research."⁷⁶ Wolfram also imagines a permanent NKS research institute, supported, perhaps, by software companies, including his own (personal communication). Bios, the SFI, NKS: a nascent social formation for the latter-day counterparts of cybernetics begins to appear here beyond the frame of the usual institutions of learning—a parallel world, a social as well as ontological—a socio-ontological—sketch of another future.