[8]

OPENING A CLOSED BOX

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All science, as this paper explains, relies on models, but this is more *obvious* for the study of complex systems than, say, mycology, so it is only fitting that we begin this volume with one of the first self-conscious articulations of how and why scientists use models. A few words of scene-setting are in order before discussing the paper itself, about our authors, their larger interdisciplinary collaborative project, and how this paper both fit into that project and opened new pathways.

A. Rosenblueth and N. Wiener, "The Role of Models in Science," *Philosophy of Science* 12 (4), 316–321 (1945).

Arturo Rosenblueth

Arturo Rosenblueth Stearns (1900–1970) was a Mexican neurophysiologist.[1] Born in Ciudad Guerrero, Chihuahua, to a Hungarian Jewish immigrant father and a Mexican-American mother, he studied medicine, specializing in physiology, first in Mexico City and then (after an interlude when, lacking funds, he made a living playing piano in restaurants) in Berlin, and finally Paris. There he was educated in the tradition of physiology descending from the great Claude Bernard (1813–1878), where biomedical investigation was blended with sophisticated ideas about scientific method and philosophy (Bernard [1865]1927).[2] In 1927 he obtained his medical de-

[1] More or less brief accounts of Rosenblueth's life and work can be found in all of the biographies of Wiener cited below. There are few dedicated studies in English, so I have relied on Salmerón (1978), Guadalajara Boo (2012), and especially on the works of Ruth Guzik Glantz (2015, 2009). (I am grateful to Prof. A. E. Owen for help with these references.) I have been unable to consult Guzik Glantz (2018); an English translation is very much to be desired.

[2] Some aspects of this tradition include: a focus on characterizing the functional role of anatomical structures, and then on characterizing the physical and (especially) chemical mechanisms through which those functions are implemented; an embrace of the method of conjectures and refutations (long before Karl Popper coined that apt phrase) rather than inductive generalization; a focus on crucial experiments where the diverging consequences of differing ideas could be subjected to empirical test; an

gree from the Sorbonne, returning to Mexico City as a professor of physiology at the National School of Medicine. In 1930, however, a Guggenheim fellowship took him to Harvard Medical School, where he was to spend the next fourteen years under the auspices of the celebrated Walter B. Cannon (1871–1945). Cannon—an academic "grandchild" of Bernard—was a giant of American science, who investigated the chemical bases of an immense range of physiological processes, and coined many concepts still in common (if anonymous) use, such as "fight or flight" reactions, and "homeostasis" (itself an elaboration of Bernard's "stability of the internal environment").

Rosenblueth's work with Cannon focused on the physiology of the nervous system, specifically the conduction of electrical impulses ("spikes") through neurons, and the action of the sympathetic nervous system. In both cases the goal was to work out the chemical mechanisms underlying the observed phenomena—very much in line with the Bernard tradition. While scientifically productive, his position at Harvard was precarious, reliant on Cannon's patronage. Cannon, for his part, tried to secure Rosenblueth a permanent position at a US university (including nominating Rosenblueth as Cannon's own successor at Harvard), but failed, in part due to explicit antisemitism. In 1944, Rosenblueth returned to Mexico City, where he held a series of increasingly distinguished positions at the Instituo Nacional de Cardiología and the Centro de Investigación y de Estudios Avanzados del Instituto Politécnico Nacional. It is characteristic that his final publication during his lifetime was a book titled *Mind and Brain: A Philosophy of Science* (Rosenblueth 1970).

Our other author gives us a memorable sketch of his "companion in science" (Wiener 1955, 171):

> *Arturo is a burly, vigorous man of middle height, quick in his action and in his speech, who paces rapidly up and down*

insistence on causal determinism, with the implication that any variability or (apparent) role for chance was merely a sign of some neglected, systematic causes. We can see traces of all of this in the essay, including its examples, its ideas about progressive refinement and elaboration of models, and the use of formal models themselves, to deduce the consequences of assumptions so that they might be compared to experimental realities.

> *the room when he is thinking. No one who sees him in the Mexican environment can doubt that he is a true Mexican, though the greater part of his genetic heritage comes from other countries, particularly Hungary. Arturo and I hit it off well from the very beginning, though to hit it off well with Arturo means not that one has no disagreements with him, but rather that one enjoys these disagreements.*

Norbert Wiener

Wiener (1894–1964), for his part, is a more celebrated, even mythologized, figure.[3] His father, Leo Wiener, was a polyglot Jewish polymath from what was then imperial Russia and is now Belorussia, who (after adventures in eastern and western Europe, central America and Missouri, where he married Norbert's future mother), became professor of Slavic languages at Harvard. Among Leo's *many* eccentric theories were ideas on education, which he implemented with young Norbert; because or despite of those interventions, Norbert proved to be "an infant prodigy in the full sense of the word": "I entered college before the age of twelve, obtained my bachelor's degree before fifteen, and my doctorate before nineteen" (Wiener 1953, 3). That doctorate was in philosophy, specifically mathematical logic; it was followed by what we'd now call a post-doc at Cambridge University, with the great logician and philosopher Bertrand Russell (1872–1970). Just as decisively, Cambridge was also where Wiener was introduced to the more conventional branches of higher mathematics, from the lectures of the celebrated G. H. Hardy (1877–1947).

Returning to the US, Wiener decisively failed to find satisfactory academic employment in philosophy, despite publishing a number of papers in mathematical logic now regarded as fundamental. After floundering as (among other things) a hack encyclopedia writer, a

[3] In addition to his (well-written and *largely* reliable) memoirs, Wiener (1953) and Wiener (1955), Wiener has been the subject of at least four full-length biographical studies: Heims (1980), contrasting him (favorably) with John von Neumann; Masani (1990), emphasizing his mathematical accomplishments; the popularizing Conway and Siegelman (2005); and Montagnini (2017), emphasizing his role as a philosopher and pioneer of interdisciplinarity.

"computer" at the US Military's Aberdeen Proving Grounds, a private in the US Army, and a reporter for a Boston newspaper, he finally became a professor of mathematics at the Massachusetts Institute of Technology, where he remained for the rest of his life. There he began to produce works in pure and applied mathematics which secured his place as one of the leading mathematicians of the twentieth century. Two aspects of his inter-war work stand out here:[4] his theory of Brownian motion and his contributions to harmonic analysis. The former provided the first fully-coherent theory of a random process in continuous time, putting earlier heuristic work by Einstein, Langevin and other physicists on a firm mathematical footing, and paving the way for the modern theory of stochastic processes [@von-Plato-modern-prob]. In harmonic analysis, his work was many-sided, but one key part was understanding what happens when we try to decompose random signals into superpositions of sine waves, and relating the properties of the resulting Fourier spectrum to the statistical distribution of signals. While pursuing these more theoretical undertakings, Wiener was also, encouraged by the MIT environment, collaborating with engineers on very practical issues like the design and analysis of switching systems and analog computers.

Interdisciplinary Collaboration and Cybernetics

It was in 1933, when Rosenblueth was at Harvard Medical School and Wiener was at MIT, that the two men met, introduced by a former student of Wiener's, the Mexican physicist Manuel Sandoval Vallarta. Rosenblueth, who had an interest in philosophy of science dating back to reading Poincaré (2001) as a student, ran an informal seminar on "scientific method," mostly but not exclusively attended by other biologists in the area. (It seems to have often been as much dinner party as seminar.) Wiener began attending, and quickly became both a leading participant and a personal friend to Rosenblueth. The two shared a belief that

[4] At a technical level, both of these contributions relied on the newer ideas about integration developed by Lebesgue, Borel and others, which we now know as "measure theory." Thus the issue with Wiener's model of Brownian motion was really "what does it mean to have a probability distribution over trajectories that evolve continuously in time?" (and not just a distribution over positions at a finite set of times). Similarly, the issues in harmonic analysis came down to "how do we make sense of situations where the Fourier transform of a signal needs a *continuous* set of frequencies?"

"divisions between the sciences were convenient administrative lines for the apportionment of money and effort, which each working scientist should be willing to cross whenever his studies should appear to demand it" (Wiener 1955, 171). It was in this period that the two men began thinking about "the application of mathematics, and in particular of communications theory, to physiological method" (Wiener 1955, 173).

With the arrival of World War II, Wiener pursued a project for the US military on automatic anti-aircraft fire control. An anti-aircraft gun needs to fire at where a plane *will* be, rather than where it is now, so the future trajectory of the target needs to be extrapolated or predicted from its past, which was itself observed noisily. Since this was all to be done with analog equipment, the trajectory of the plane, the data and the predictions could all be thought of as continuous functions. Wiener set up the problem of finding the optimal predictor as a least squares problem, and used his work in harmonic analysis to show how to find the optimal linear solution. The result was a general theory of (linear) "extrapolation, interpolation and smoothing of stationary time series" (Wiener 1949). (Parallel work was done independently, and simultaneously, by Kolmogorov in the USSR.) Prototype predictors based on these principles were actually built and greatly impressed competent observers, but did not see use in the war, the army settling on simpler alternatives.

During the course of this work, Wiener became seized by an analogy between the principles of negative feedback control, long used in automatic machinery (Mayr 1986; Mindell 2004), and biological phenomena of self-regulation and even purposive behavior. (When you are building a gun that adjusts itself to shoot planes out of the sky, it is probably hard to avoid talking about what the gun is *trying* to do.) Together with his engineering collaborator Julian Bigelow, Wiener approached Rosenblueth to see if there was anything to the analogy biologically, especially neurologically. At a high level of generality (and vagueness), of course negative feedback is akin to Cannon's notion of homeostasis, but the trio went beyond that. Control theory already provided the tools to analyze the *failures* of feedback, including those due to over-correction, those due to excessive delay, and so forth. Rosenblueth was able to provide fairly convincing neurological examples of *pathological* conditions corresponding to these different failures of feedback. In terms of the present paper: if one thinks of (say) visually-guided reaching as being governed by the same abstract, formal model as an electro-mechanical servomechanism, then one should *expect* to see certain kinds of pathological motion when there is too much delay between seeing and moving. Observing this pathology then strengthens one's confidence in the aptness of the abstract model.

These considerations led to a truly seminal paper, Rosenblueth, Wiener, and Bigelow (1943), which analyzed *some* kinds of "teleological" (goal-directed, purposive) behavior as the result of feedback mechanisms.[5] This line of thought was a key ingredient—arguably *the* key ingredient—in the synthesis that Wiener presently dubbed "cybernetics," and descried, in the subtitle of his classic book (Wiener 1948), as the study of "control and communication in the animal and the machine," and, we might add, "in society," too.[6] The book, dedicated to Rosenblueth and largely written during Wiener's visits to Mexico City, had an enormous impact. This was helped along by a series of conferences funded by the Macy Foundation devoted to these ideas, whose participants included many of the leading figures in American natural and social science, and in which Rosenblueth was a prominent participant.

This "movement" (Heims 1991) or "moment" (Kline 2017) was in some was astonishingly successful—it is "why we call our age the information age"—but ultimately failed to create an autonomous, self-propagating discipline, not least because by the 1960s it found itself pursuing some very strange notions and blind alleys. It would be an excellent thing to see a proper *scientific* assessment of its contributions

[5] It is a common misconception that Rosenblueth, Wiener, and Bigelow (1943) *identified* purpose and feedback. The text actually makes it clear that many kinds of purposive behavior can't be controlled by negative feedback "in the course of the behavior" (Rosenblueth, Wiener, and Bigelow 1943, 19–20), e.g., when throwing at a target, the muscles in the hand and arm have to move too quickly for nerve impulses from the eye, or even from the proprioreceptive sensors in the arm, to travel to the brain and back out to the muscles (Rosenblueth, Wiener, and Bigelow 1943, 20). Of course on a larger time scale, feedback can *improve* throwing, and, as they say, there is a kind of negative feedback involved in *stopping* purposive behavior once the aim has been met.

[6] There was a long tradition of claiming that organisms, even human beings, were machines (de la Mettrie 1994; Loeb 1912), or at least of seeking mechanistic explanations for biological phenomena (Bertoloni Meli 2019). People like Rosenblueth and Wiener were very aware of a long tradition of claiming organisms *were* machines, or at least *acted* like machines.

and limitations in light of modern knowledge. Instead, I will just remark here that in emphasizing information, computation, dynamical systems modeling, processes of circular causation, aggressively abstractions or analogies across physical, technological, biological and social domains, and general disdain for traditional disciplinary boundaries, cybernetics was clearly a predecessor to the field of "complex systems" that formed in the 1980s, and arguably even a linear ancestor.

It would be a mistake to see cybernetics as *just* a project of grand theorizing. It is characteristic of Rosenblueth and Wiener that they felt it important to demonstrate the worth of these ideas by showing how they could solve concrete problems—ones already recognized as worthy problems by scientific communities. Much of their collaborative time in the later 1940s was thus spent on devising, and fitting to data, stochastic models of neurophysiological processes, most successfully the input-output behavior of the synapses between neurons (Rosenblueth *et al.* 1949), and of the propagation of waves of activity in cardiac tissue (Wiener and Roseblueth 1946), a pioneering study of what are now called "excitable media" (Greenberg and Hastings 1978).

Modeling, and the Paper

The essential argument of the paper is that "no substantial part of the universe" is so simple that the human mind can grasp it without "abstraction," "replacing the part of the universe under consideration by a model of similar but simpler structure" (316). These models are, primarily, "formal or intellectual" affairs, which allow us to reason logically from the premises of about that structure, to conclusions about what we should see in the world, or *would* see under such-and-such conditions, or *will* see if we do something. Much of the art of the scientist thus consists in formulating, manipulating and revising models, with the full understanding that the model does not, and should not, try to capture the full richness of the world.

We can now see where this paper fits in with Rosenblueth and Wiener's larger collaborative project.[7] Like many ventures into philosophy by scientists, this essay is (in part!) about justifying what the authors were already doing. In 1945, Rosenblueth and Wiener were deep into their neurophysiological studies, and engaged in just this process of model-building and model-revision. These models *deliberately* abstracted from many details of the physiology. Similarly, the 1943 paper on teleology likewise abstracted away many implementation details between "servomechanisms" and organisms. One goal of the paper was to argue that such abstraction was, in fact, a virtue—or, rather, inevitable, and so better done thoughtfully than in denial.

While physical models of scientific hypotheses are literally ancient,[8] the conscious recognition of "formal or intellectual" models is a comparatively recent thing. There is a transition somewhere between, say, Newton, who thought of himself as describing "the system of the world," and, say, Bohr, putting forward a model of atomic structure that was frankly *merely* a model. Even when Newton works through what we would see as a model of single planet orbiting the sun (uninfluenced by the gravitation of other planets, etc.), he did not think of it that way. Bohr, quite manifestly, did. The roots of this shift are beyond tracing here, but some aspects can be pointed out: an increasing comfort with approximation, with partial descriptions and with generalizations of limited scope, and with a certain division of cognitive labor. To borrow an example from Rosenblueth and Wiener, someone doing acoustics can (usually) take mechanical properties of air for granted, without having to worry about *why* it has just those properties, or even whether those properties would alter under extreme conditions, secure in the knowledge that someone else is studying those issues. (Whether this *reflects* the social organization of the scientific community, or on the contrary whether that organization is an outcome of this sort of problem-solving approach, is a deep question.)

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[7] There is very little secondary literature on this paper; the biographies of the two authors cited above mention it only in passing or not at all, and it did not spawn the lineage of commentaries and critiques as Rosenblueth, Wiener, and Bigelow (1943).

[8] Physical models of the orbits of the planets date back to the Hellenistic age, if not before, the most famous (now) being the artifact called the "Antikythera mechanism" (Jones 2017).

By the middle of the twentieth century, then, the conscious use of models was a prominent part of natural science, but also one that required some explication and defense. Rosenblueth and Wiener actually portray "material" models, such as scale models, biological "preparations," or what we'd now call "model organisms," as secondary to formal models, though this logical order reverses the order of historical development and of their own exposition. For Rosenblueth and Wiener, material models are practical expedients, for when the "part of the world" of interest is awkward to work with: too large, too small, too expensive, too hard to manipulate, too apt to bite experimenters. Nonetheless, for them, the scale model of (say) an airplane in a wind tunnel only lets us draw inferences about the full-sized airplane in the sky because there is a common formal model of the aerodynamic situation, and both the larger and the small physical systems are regarded as relevantly similar to this formal model. They admit that this formal model may only be implicit in the mind of (some) investigators, but insist on its importance.

One consequence of this view of models is that it is hard to say that models are *true* or *false*, so much as *more or less accurate*, and that in particular ways or respects. This, in turn, suggests that there can be multiple good models of the same system, either at different levels of approximation or for different aspects of the same physical process. This paper emphasizes the importance of levels of approximation, as in modeling sound transmission: from a simple linear model treating air as a homogeneous and incompressible fluid, to one including compressibility and shock-waves, to an anticipation of (ultimately quantum) molecular hydrodynamics.

Rosenblueth and Wiener liken this process of elaborating models to that of "opening" a "closed box." The "closed" boxes are the unarticulated parts of the model which are treated "functionally," that is, like mathematical functions defined as relations of inputs to outputs. "Open" boxes are elaborated parts of the model treated "structurally," specifying a structure or mechanism that implements the function, and gives some details about how inputs get turned into outputs. Which parts of the model need to be opened up, and which can be left closed, is again part of the modeler's art and varies with the goals and resources of the investigation. The temptation to (as it were) open all the boxes, and model everything in complete detail, is one they are clearly familiar with, but want the reader to avoid: a fully articulated model becomes as complex as the original, and so useless to a merely human intelligence.

This brings us to one of the more interesting features of this paper, for the present audience. They begin by asserting that "no substantial part of the universe is so simple that it can be grasped and controlled without abstraction," and end by talking about how increasingly refined models "approach asymptotically the complexity of the original situation" (316, 320). But they never actually *define* either "complexity" or "simplicity." If we were to try to back out a complexity measure from the way they use words like "simple" and "complex," we'd land on something like "number of explicitly articulated details or processes." To modern ears, this suggests some sort of description length, which would certainly fit our authors' pre-occupations with information theory, and, one could even say, would look forward to the Kolmogorov–Solomonoff notion of complexity as algorithmic information content (Kolmogorov 1965; Solomonoff 1964; Li and Vitányi 1997). But perhaps this mere anachronism.

Despite being published in *Philosophy of Science*, then and now a leading journal for that subfield, this paper, for all its insight and good sense, made little impact on philosophy. It was much later, really in the 1980s, that philosophers of science became interested in the role of models in science. Thus Giere (1988), to take one justly influential example, defines "models" as, basically, what our authors call "formal models" (78—80), adding that there are also "hypotheses" which "claim a similarity between the models and real systems . . . [and a] specification of [the] relevant respects and degrees [of similarity]" (81). Such accounts of models and modeling have been extremely influential in contemporary philosophy of science, and have begun to filter back out to the thinking of working scientists (e.g., Burch 2018) and the more thoughtful textbooks (Ashworth, Berry, and Bueno de Mesquita 2021). This is all, wittingly or not, following in Rosenblueth and Wiener's footsteps.

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