Chapter Four

Circuits, Algae, and Whipped Cream

The Biophysics of Nerve, ca. 1930

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> There will be nothing that the average man sees, hears or buys that will not be controlled, regulated or affected in some important respect by an electronic tube!
>
> —O. H. Caldwell, 1930

The intimate entanglements of electrical technologies and nervous phenomena belong to the better charted territories in the history of the neurosciences. The metaphors of the telegraph, switchboard, battery, or computing machinery make for familiar reading, as do narratives of laboratories in the midst of urban electrification and scientists chasing imponderable fluids, nervous "messages," or "codes": from Leyden jars to Cold War electronics, historians of science have explored at great lengths the careers of animal electricity. Their actors, too, typically were quick to point to the signal importance of such entanglements. "The history of electrophysiology has been decided by the history of electric recording instruments," as the great Edgar Adrian ventured in his *Mechanism of Nervous Action* (1932), his fellow British countrymen still recovering from the Faraday Centenary the year before (generally a cause for the celebration of electric progress, of course).[1]

The very "gadgets" of physiology thus may have fostered the "kind of electrical double-talk concerning nerve" in which everyone indulged. Thus pondered Columbia neurophysiologist Harry Grundfest some twenty years later at the First Macy Conference on the "Nerve Impulse" in 1950—just having lived through yet another (this time electronic) upheaval in the annals of electrophysiology.[2] Nuances apart, historians have mostly found themselves in agreements with such verdicts.[3] And a very similar picture of entanglement will be belabored in the present essay, which deals with the production of electrophysiological knowledge in the proverbial "radio-age" of the 1920s and 1930s. But in doing so, I aim to add a historiographical twist to the story of nervous activity. This essay is less interested, that is, in showing how electrical things shaped, or did not shape, the sciences of the nervous system; nor is it to demonstrate that instruments or metaphors are important—historians of science already know that. Rather, it aims to show how these electrical things, their uses, and the knowledge effects they had may prompt us to reconsider the nature of those very sciences. Thus, below circuits and nerves *will* be featured—along with such items as algae and whipped cream.

The point, if you will, is to defamiliarize us from a neuroscientific past that always already has revolved, however unsophisticatedly, around a coherent object: the mind/brain. The interwar pursuit of bioelectricity, as we shall see, is one such site of apparent incoherence; or, put positively, a site where quite different, more "fractured" kinds of lineage become palpable. The period, to be sure, also saw the origins of electroencephalography, or the infamous "war" of the "soups and sparks," but such episodes do not exhaust the sheer breadth of items that meant puzzles to electrically minded scientists.[4] Attending to the material cultures of electricity that got enrolled in the process helps to see this, as this chapter argues. For these weren't "instruments" so much as aggregations of electrical elements—themselves rhizomatic, omnipresent, and indeed, ready to hand; in turn, they spawned experimental techniques of rearranging, reassembling, and retooling that could be put to multiple uses. The elucidation of the nerve impulse was one of them.

As the (then newly launched) journal *Electronics* advised in 1931, "The best radio designer is the one who draws on and skilfully assembles the existing experiences of the best makes of components and parts."[5] It would have been an apt description for the radio-age physiologist, too; and in many ways, then, this essay is concerned with the inverse problem pursued in this volume by Frank Stahnisch on "materials"; not with the scientific things that migrate, but with the technical things, the components and parts, that were, in a sense, already there.

Prelude: 1939

To enter the interwar sciences of organic circuitry, let us take the story a bit back first. The year 1939, as specialists will know, was important in terms of the nervous impulse. That summer, at the Plymouth Marine Biological

Station in England, young Alan Hodgkin and his (younger still) collaborator Andrew Huxley pulled off the delicate feat of measuring the nervous action potential *across* the surface of a *single* nerve fiber, "intracellularly."[6] Both descendants of the famed Cambridge school of physiology, they promptly unearthed a somewhat puzzling phenomenon that hitherto had escaped physiological investigators. For various (mostly technical) reasons, physiologists traditionally had tended to busy themselves with *extra*cellular measurements; that is, to measurements *along* the surface of a nerve. The phenomenon in question—a "reversal" or "overshoot," as Hodgkin and Huxley labeled it, of the cellular potential during activity—was indeed something unheard of in the annals of electrobiology. Or rather, it was defying the received wisdom (which had it that a cell`s electric potential should vanish, not reverse its polarity).

Across the ocean that summer at Woods Hole, Massachusetts, and not coincidentally deploying a very similar setup—Hodgkin had just recently returned from there—the biophysicists Kenneth Cole and his colleague Howard Curtis broke new ground as well.[7] For their part, Cole and Curtis managed to detect, by similarly "direct" means, a change of resistance of the nerve membrane during activity. Or to be precise, they detected a change of the nerve's "impedance"—crudely, its *alternating* current resistance. This too was no negligible achievement, for little was known about the physical properties of this elusive structure except "indirectly" and by means of speculation; and even less was known about its dynamic behavior. The accompanying record is an iconic one (see fig. 4.1): a photograph of the surface of a cathode ray tube screen, the tube itself connected, via a multistage amplifier, to a microelectrode; the electrode, in turn, carefully inserted into the interior of a squid giant axon.

The subsequent, considerable career of the squid giant axon as an experimental object need not concern us here; neither will Hodgkin and Huxley's rise to fame in the early 1950s or, for that matter, what contemporaries were quick to identify as one of the "brightest chapters of neurophysiology and even biology of all time"—their seminal, computational model of the nervous impulse.[8] It is a seemingly technical detail that interests me here: the change in "impedance" pictured above. Or more precisely, it is the world of biophysical practice it emerged from: an electrobiological world that was stranger, I shall argue, than the received plotline—from Galvani to *Aplysia*—would seem to suggest.[9] While, with hindsight, the records obtained by Cole—much like Hodgkin and Huxley's—do form a central role in the postwar story of protoneuroscientific consolidation, they also point to a past which fits that teleology less neatly. Indeed, as a closer look at the genealogies of the above "change in impedance" will reveal, nervous activity—let alone the activity of brains and minds—was typically not at stake as radio-age scientists worked out the details of "animal" electricity. Rather, at stake

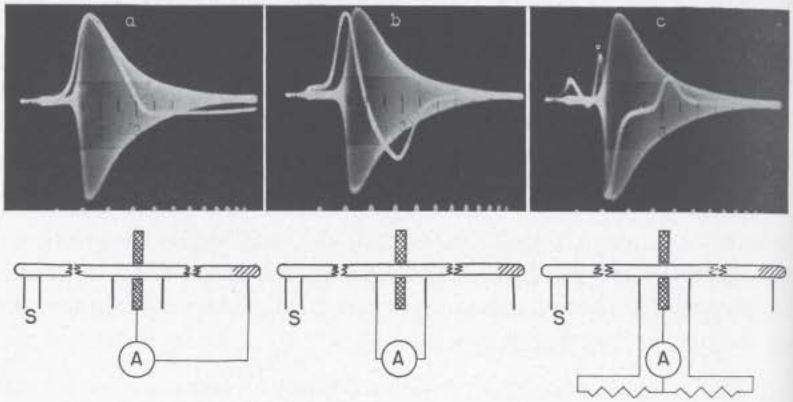

FIG. 9. Double exposures of the impedance change at 10 kc. on each with the action potential in each picture from the circuit shown below it. (*a*) is V, the monophasic potential, (*b*) is V_t, the first derivative or axial current, and (*c*) is V_{tt} the second derivative or membrane current. The time marks at the bottom are 1 millisecond apart.

Figure 4.1. "Impedance" change in the squid giant axon. © 1939, K. S. Cole and H. J. Curtis, "Electric Impedance of the Squid Giant Axon during Activity," *Journal of General Physiology* 22, no. 5 (1939): 655, doi:10.1085/jgp.22.5.649.

were nerves, muscles, hearts, legs, arms, torsos, breast tumors, algae, even microbes and suspensions of "whipped cream"—a range of epistemic objects that may come across as fairly disparate in retrospect; they do have the virtue of making it possible to conjure up a different, less brain-centered image of the neuroscientific past.

In other words, the production of bioelectric knowledge in the interwar period was a considerably more eclectic affair than what the emphasis on places such as Cambridge, eminent figures such as E. D. Adrian, or the more obvious applications—the EEG and the vacuum tube amplifier—tends to imply (important as they were).[10] By the same token, while it was a technique largely alien to "classical" nerve-and-muscle physiology, to zero in on the impedance of nerve was a far from unsystematic (or unobvious) manoeuvre vis-à-vis the study of so-called *excitable* tissue, broadly conceived (as interwar physiologists were inclined to conceive it). Much of this essay will be concerned, therefore, with the diverse range of sites, actors, and excitable "materials" that did serve the advance of bioelectrical knowledge at the time; and much of it will be concerned, consequently, with the technological infrastructure connecting those various dots: the practical culture—and conceptual universe—of the radio (or "wireless"). The incursions of the radio arts into the realms of bioelectricity—or what at first sight might seem to be at best a tangent to the True Story of neuroscience—in fact was a significant, epistemological turning point. In the study of nervous phenomena, it implicated a turn toward an epistemology of "measurement" and (so-called) "models," displacing physiologists' earlier predilection for curves, graphs, and ultimate "laws."[11] From this perspective, the 1939 episode recounted above was less of a beginning but a symptom: a case of well-honed, biophysical techniques implanted into seemingly purer realms of physiological science (we shall return to the episode in conclusion).

In what follows, I approach this gradual shift in techniques and technologies from several angles: as a matter of technological, cultural, and conceptual appropriations—the adoption and creative (re)uses of the "wireless" arts in the biological laboratory; as a matter of practical knowledge (including therapeutic applications of electricity); and, finally, as a matter of transmogrifying biophysical objects, from algae to nerve.

The Laws of Excitation

The significance of "wireless" in this story will turn out to largely revolve around the domestication of alternating currents (AC) at the time. The reason is, by and large, simple. Traditionally, studying nerve (or muscle) scientifically meant deriving *laws*—the laws of excitation. It also meant *stimulating* the nerves (so that their activity could then be recorded). This, in turn, implied—by virtue of their excitatory powers—the use of *direct* (or galvanic) current (DC).

Historians of the physiological sciences, quite understandably, have been enthralled by the production of "inscriptions" in this type of experimentation (or so-called graphic methods), neglecting somewhat the great efforts that had always been devoted to, as it were, the reverse operation: the production of *interventions*.[12] By the 1930s, an immense variety of devices had become available to this very end, littering trade journals, scientific articles, and the catalogues of scientific instrument makers: all were means to generate shape and time currents, from "classic" electro-mechanical devices such as ballistic rheotoms or pendulums to rotating commutators; so-called chronaximeters; "make and break" circuits; arbitrary wave forms etched onto gramophone records; and increasingly so, fully electrical outfits. Neon lights, for example—widely used for advertising—were most suitable for the purposes of "rhythmic" stimulation: faster, more accurate, and more reliable than anything that could be achieved by electromechanical means.[13]

A whole story remains to be written here regarding these devices of synthesis, but for present purposes it will do to point out only the one, central bifurcation. Whereas direct currents were, evidently, eminently suited to excite, alternating currents, curiously enough, had no such excitatory effects when applied to organic tissues (even though they were perfectly suited to electrocuting microbes, rodents, and even elephants, as readers familiar with the late nineteenth-century "war of the currents" will recall).[14] Alternating, high frequency currents were less easily handled, too. As late as 1908, for example, the physical chemist Walther Nernst—always keen to weigh in on electrophysiological matters—declared in his "Theory of the Electrical Stimulus" that, all things being equal, it remained virtually impossible to generate alternating currents "pure" enough to even begin to ground the tremendous amount of speculation surrounding their putative organic action.[15]

But along came the "modern comforts of broadcasting" (as Adrian put it);[16] most notably, of course, along came the vacuum tube. Indeed, the many "triumphs" of the vacuum tube—exemplar of "modern universal instrumentality," as the electric boosters had it—prominently included the "production" of alternating currents—"of any desired frequency."[17] And once reined in, alternating currents swiftly did accrue an increasing range of biomedical uses, too: therapeutic, diagnostic, surgical, and so on. It could, after all, be done and at the very least, there was an "intense modernism" about it (as the growing numbers of medical proponents enthused).[18]

This amounted, one might complain (as some did), to little more than the indiscriminate deployment of an immature technology. But indiscriminate or not, before long these uses did make salient a range of different electrophysiological effects and properties—different, that is, from the typical electrophysiological roster. As we shall see shortly, this roster prominently included the electrical properties—impedance, capacitance, and so forth—of organic materials. It was a subtle but significant departure because, as the Harvard physiologists Hallowell Davis and Alexander Forbes noted in 1936, physiologists were seemingly obsessed with something else entirely; namely, with the properties of stimulation currents and corresponding tissue responses—and hence with said "laws" of excitation.[19] Taking stock, Davis and Forbes then made a point in putting together almost thirty such laws, derived in recent years, as they explained, in the futile hope that the ongoing proliferation of experimental observations might be "covered ultimately by a single law and formula."[20]

Reflecting the daily routines of much physiological practice—stimulating and recording—these formulae and their diagrammatic representations, so-called strength-duration curves, indeed had become ubiquitous, omnipresent in journals, textbooks, and laboratory manuals. But for all the progress in precision and exactitude, the "true picture" of bioelectrical activity, it seemed, was as distant as ever.[21] The popular concepts of the day, traveling under names such as "excitation-time," "temps utile," "chronological coefficient of excitation," "time factor," "Nutzzeit," "Kennzeit," and—most infamously—the "chronaxie"—did not help it;[22] this confusing abundance of inscriptions all were variations on the same empiricist theme: the urge to establish such more or less "arbitrary" but quantitative relationships between the electrical stimulus properties on the one hand (such as its duration), and the responses of irritable tissues on the other (say, the interval between stimulus onset and impulsive response).

Or we should say beliefs that these laws and curves were "arbitrary," crudely empirical, merely "phenomenological" and lacking "immediate physical sense" constituted a lament that was being voiced with increasing frequency by physiologists from across the electrified world. Their practical usefulness being widely acknowledged, nothing in this veritable, DC-driven natural history of curves was at all "direct" or, for that matter, intellectually defensible: so went the gist of these complaints, which picked up steam toward the late 1920s.[23] In the words of one Cambridge physiologist, William Rushton, it was difficult to find a "physical justification" for such formal maneuvers, much less to "find a physical meaning to it."[24] Futile "fitting" of curves ventured the young Bernard Katz;[25] a "Kunstprodukt" [artifice] without "physiological correlate," decried another.[26] Worse, deviations from the ideal, law-derived curves became suspiciously manifest especially for the "smallest times"—by 1930 these were in the range of a millionth of a second, and they were manifest as well in cases of "prolonged" stimulation, when so-called accommodation effects noticeably set in.[27]

In brief, the progress of electrical precision did not straightforwardly translate into progress when it came to making sense of the "quantitative fixation" of tissue excitation.[28] Indeed, even physiologists' best guess in the direction of "physical sense"—Julius Bernstein's so-called membrane theory—was increasingly eyed was suspicion. It had degenerated into a "means of lulling the mind," as the "axonologist" Herbert Gasser complained in 1933.[29] Not too much, of course, should be made of such crisis talk, but as an indicator of tendencies, it is instructive. For there was, significantly, a great deal of movement at the fringes of the "classic" nerve-and-muscle physiology—though this generally still failed to impress those operating at its center. Hans Schaefer's utterly comprehensive, two-volume tome *Elektrophysiologie*, finally completed in 1940, was symptomatic in this regard. The work was, according to Schaefer, confined to the more "theoretical" aspects of the subject (still including some six thousand references), but was unable to consider the "purely clinical-pathological" studies, topics of a mostly electrochemical and electrophoretic nature, the "physiology of shortwaves" and of "high-voltage currents," and "all those things where the electrical [was] only technics [*Technik*]."[30]

Enter Biotechnics

Only "technics"—there was, no doubt, a lot of truth in Schaefer's exclusion principle (and by no means was Schaefer a stranger to the more technical dimensions of his subject matter). But the accumulated effect of such disciplinary stratification and streamlining, which would only intensify over the years to come, was to obscure the extent to which the "theoretical aspects," of course, had never been so neatly separate from the purely practical ones. In this regard, the 1920s and 1930s were distinguished not least by the emergence of a heterogeneous population of actors—Cole and Curtis among them—who would have felt rather comfortable with precisely the latter, technical aspects of all things bioelectrical. It was also a preoccupation usually involving simpler, and less delicate, objects than nerve (or even muscle). And for reasons explored below, these actors thus had little time for excitation laws or general solutions. Indeed measurements such as those pursued by Cole and Curtis demanded a different kind of theory: one based on models rather than laws.

To enter these terrains, consider the biophysical trajectory of Hugo Fricke (1892–1972).[31] His forays into the electrical properties of biological things will turn out to have profoundly shaped and prepared the biophysical vision of the nervous impulse, with which this essay opened. But such genealogies are all too easily obscured, as was already indicated. Operating in a world removed from the centers of the purer kinds of physiological science, Fricke's oeuvre was steeped in tumor cells, bacteria, blood corpuscles, and similarly simple objects. But neither was Fricke disconnected from the biomedical world at large; or rather, as we shall see, he did not remain so always. In 1928 Fricke became the first director of the Walter B. James Laboratory for Biophysics at Cold Spring Harbor on Long Island. For now, however, more revealing is Fricke's background: he had spent the previous decade or so as the resident biophysicist at George W. Crile's Cleveland Clinic Foundation, making a reputation for himself in the area of high-frequency measurements.

Crile, for his part, was a figure as notorious as he was distinguished.[32] "Dr. Crile Suggests That Our Bodies Are Electric Batteries" went a typical headline. Crile's daring biophysical theories predictably failed to enlist much sympathy among the more clear-headed students of living processes (more likely, such students sneered at "Crile's rather loose and uncritical methods of work").[33] Even so, Crile was an accomplished surgeon. And, propelled by a bizarre, electric vision of life as "bipolar," Crile fashioned himself into the role of true biophysical pioneer and enabler.[34] The Cleveland Clinic thus provided, for several (and much renowned) investigators, a first contact with, if not a more permanent home for the borderlands of physics and biomedicine. From here, Otto Glasser, remembered mostly as a biographer of Roentgen, was pushing the case of *Medical Physics* (1944) and *The Science of Radiology* (1933); he had joined Crile's enterprise in 1922 (after quitting his previous job with the German *BASF* concern). Meanwhile, Fricke—trained in engineering and physics in Denmark—had already been enrolled in Crile's sprawling program the previous year. And soon after, Kenneth Cole, then still a graduate student in physics at Cornell, made it on the temporary staff list as well.[35] Fricke, installed as the director of the clinic's biophysical laboratories, developed his work chiefly along two lines (as he later commended himself to the Cold Spring Harbor Laboratories): "the biological effect of radiation and the electric polarization and conductivity of biological cells."[36]

Both of these were subjects considered to be of immense significance by Crile, who had had his biophysical epiphany at least twice. First, in 1887, when Crile witnessed the death through "shock" of a fellow student whose legs had been crushed by a streetcar: "the dramatic picture of failing bodily energies and death."[37] And again, when Crile had to cope with the "intensive application of man to war" at the Western front (Crile being surgical director of the American Ambulance): millions of similar cases of "shock—a violent restless exit," as he reminisced, and phenomena leading him to reason "that man and other animals are physico-chemical mechanisms."[38] Back in Cleveland, Crile promptly initiated a series of biophysical investigations into the electrical conductivity of animal tissues, co-opting the special expertise of one Helen Hosmer, formerly of the General Electric Laboratories. The organism, Crile then inferred, was "operated by electricity"; in particular, as Crile determined, in a state of shock which was marked by a diminished conductivity especially of the brain and an increased conductivity of the liver.[39]

The framework had thus been set. As Crile hit the news with spectacular bioelectric discoveries (as happened every so often),[40] Fricke quietly embarked on figuring out the details. Most notably, toward the mid-1920s Fricke had begun to look into high-frequency measurements of blood, bacteria, and various animal tissues. Finally, here was a "precision method" that had, as Fricke explained, certain "practical implications" as well.[41] Their basic principle was simple enough, and in fact, long established. It left few traces because the goal was silence—the *absence* even of sound: equipped with, say, a telephone receiver (or some other "display"), when measuring with a so-called bridge-circuit (or "null" method) one was required to balance an unknown circuit component (or "arm") against a parallel, *known* one: silence meant balance—or, in electrical terms, "equivalence" (more on which below).[42]

What was new was the sheer range of frequencies at Fricke's disposal. As significant, a "most convenient and uncomplicated material for study" was found in tumors, something readily available at the clinic.[43] By 1926, in a paper on "The Electric Capacity of Tumors of the Breast," Fricke set out how a suspension of such malignant tissue, when injected with such high-frequency currents, "behaves as though it were a pure resistance in parallel with a pure capacity."[44] The diagnostic potential apart—certain types of malignant tumors, it turned out, featured an abnormally high capacitance—Fricke was already teasing out some of the theoretical implications as well.

As attentive readers of Crile's *Bipolar Theory of Life* (also appearing in 1926) would have known, in investigations such as this, "Dr. Fricke ha[d] found that the film which surrounds . . . [biological] cells is in the order of 4/10,000,000 of a centimeter thick." Such "films of infinite thinness," according to Crile, were "peculiarly adapted to the storage and adaptive discharge of electric energy."[45] But Crile's *Bipolar Theory* need not distract us here; for present purposes, what was significant about Fricke's tumor experiments is that here was the germ of a *physical* model of the cell. Fricke, the practical biophysicist, had little time for, or interest in, establishing phenomenological correlations between stimulus and response. Nor, as the next section shows, did his biophysical peers. What mattered most to them were the physical characteristics and properties that now were exposed by rapidly undulating currents.

Medical Physics as a "Model" Science

The timing at any rate was opportune: Fricke's high-frequency forays into the electrical nature of biological membranes occurred at a time when the more business-minded men enthused that thanks to short-wave radio, these "once useless very short waves [were] becom[ing] most valuable."[46] The vacuum tube, and wireless technology generally, then turned from the experimental stage into commercial products. By 1923, 4.5 million tubes were produced annually in the United States, a figure reaching 69 million in 1929, with prices for tubes and materials plummeting. "Kaleidoscopic changes," the trade journals recorded, were underway in the electrical industry.[47] As wave-lengths diminished ever more rapidly, there emerged a true zoo of diodes, triodes, tetrodes, pentodes, thyratrons, magnetrons, rectifiers, and oscillators.

Fricke, too, was impressed. "Earlier investigations were handicapped by the experimental difficulties of producing alternating currents over a wide range of frequencies," he noted. "This difficulty was overcome by the introduction of the audion oscillator, which initiated a period of considerable progress."[48] And as Fricke knew well enough—because it had been done before (albeit with limited success) and because it was being done in many venues elsewhere—"an interesting application" of such very short waves consisted in the calculation of membrane *thickness* (on which these capacities depended). More generally, variations in tissue resistance, when subjected to alternating currents of varying frequency, allowed the physiologist to make inferences concerning the physical properties of the (preferably simple) biological objects so investigated; these included, as seen, their capacitance but also their *impedance*.

Such inferences, it is important to see, followed a somewhat different logic than the pursuit of "laws" by means of bioelectrical *stimulation*. By design, they gravitated toward the kind of physical "sense" that physiologists so sorely missed—or to use the term which would grow in significance in subsequent years: *models*. Nor would they adequately be captured by filing them as a case of neuroscientific metaphor; by this time, the word *equivalence* was in the process of accruing a very definite and exacting meaning in many an electrotechnical field, most notably in telephone engineering and electro-acoustics.[49] Indeed the very idea that organic materials were, somehow, electrically "equivalent" to this or that circuit was something that was being built into the practices of (bio)electrical *measurement* itself; it thus came naturally to anyone hailing from the technical peripheries of academic physiology (or what Schäfer above termed its "Randgebiete" [borderlands]).

From this perspective, a hodgepodge venture in medical physics such as Crile's Cleveland Clinic Foundation was fairly typical of the eclectic, makeshift technical culture that was interwar biophysical science.[50] What it lacked in prestige (or, as some might say, scientific mindedness), it frequently made up for in explorative (ab)use of electrical devices. In this, Crile's enterprise was not so much unlike the Institut für die Physikalischen Grundlagen der Medizin in Frankfurt, the Institut für physikalische Therapie in Vienna, or the Johnson Foundation for Medical Physics in Philadelphia (the latter funded by Eldridge R. Johnson of the Victor Talking Machine Company and headed by engineer-turned-nerve-physiologist Detlev Bronk). And one might add to this list many other venues, small and large, where a growing number of easily available, electrical things—from telephone-condensers, switches, and vacuum tubes to neon lamps, amplifiers, and cathode ray tubes—gradually transformed, often subtly, the face of biomedical science.[51]

What went under the name of "medical physics" was a most adventurous assortment of electrical instruments and gadgetry in particular.[52] It stretched from quartz lamps for home use to (increasingly) off-the-shelf devices for purposes as diverse as electrocardiography, X-rays, myotherapy, light therapy, or ultrashort wave therapy. Courtesy of firms eager to cash in on the modernity and cleanliness of such physical interventions—Radionta, Siemens, the British Hanovia Quartz Lamp Co., GEC, Icalite, Ulvira, Cox-Cavendish Electrical, and many more—there was no shortage of "new and highly technical" forms of treatment and diagnosis (so much so, in fact, as to prompt even the British Medical Association to install a Register of Biophysical Assistants in 1930 to regain some control over the matter);[53] even better, for those skilled enough, "handy, lucid, and comfortable" apparatus oftentimes was easily DIYed by making exclusive use of components, as one physiologist advised, "as [were] being used in radio technics and [were] available everywhere, at relatively low cost and in excellent finish."[54]

Once unleashed and democratized, the proliferation of physical agents at the time—ranging from ions, UV rays, currents of high and ultra-high frequencies to the more controversial items such as radiogens and mitogenetic rays[55]—duly prompted curiosity regarding their physiological actions (if any). Of particular interest here, of course, are those investigations pertaining to the effects of the new, and newly precise, abundance of currents during the 1920s, which, as it was quickly appreciated, were most easily generated by the vacuum tube. A correlate to the ubiquity of the latter, biomedical scientists now increasingly trained their attention onto the various effects that high-frequency currents were found to provoke, or seemed to provoke, after all.

The new ultra-high frequencies especially fueled the biomedical imagination. Still in the early 1920s, as nerve-and-muscle physiologists turned to harnessing their fickle and failure-prone amplifiers for the purposes of analysis,[56] others discovered the tube's powers of synthesis. For example, Joseph Schereschewsky of the Office of Cancer Investigations of the US Public Health Service at Harvard University, was among the first to investigate the therapeutic action of such electrical waves with small animals and "inanimate models"; similar advances were due to the physician Erwin Schliephake in Germany who, in collaboration with the physicist Abraham Esau, took to the thermal "depth-effects" of such short waves, induced by means of a special "vacuum tube sender."[57] More spectacularly, figures such as the exiled Russian engineer Georges Lakhovsky would reveal the new applications of such short wave-length oscillations. Lakhovsky, "the well-known French scientist" (according to *Radio News*) by 1925 had created a *Radio Cellulo-Oscillator*, a device producing currents up to 150 million cycles-per-second. Reportedly, it had a morbid action on plant cells, tumors, and microbes; it also provided the technological substrate for Lakhovsky's many assaults on "orthodox medicine" such as, notably, *The Secret of Life* (1925) and *The Cellular Oscillation* (1931).[58]

More widespread deployment, however, found the less drastic effects of high-frequency currents. Fricke's investigations into the electrical nature of membranes are a case in point; more typically however, these effects concerned the production of heat, or what was known as diathermy. The means were the same, only the objective differed: analysis here, the useful distribution of currents in the body there. As the British textbook *Diathermy: Its Production and Uses* (1928) explained, "To generate a perceptible and measurable amount of heat in the tissues, a current . . . deprived of its power to stimulate the excitable tissues and to cause chemical (electrolytic) change [must be used]. This can be done by making it alternate at an exceedingly high rate. . . . It may be regarded as not less than 500,000 per second."[59] These peculiar thermal effects had first been noted by Tesla as early as 1891, an observation followed up in a more systematic fashion by the French physiologist Arsonval. Beginning in the early 1890s, Arsonval investigated the "sensation de chaleur désagréable" produced, killing many a rabbit along the way through "overheating."[60] It wasn't until several years later that the medical chemist Richard von Zeynek first realized the therapeutic potential of such "Durchwārmung" (or thermo-penetration), while pursuing research in Nernst's laboratories in Göttingen. (Indeed, it was the puzzling, nonexcitatory effects of alternating currents that then had prompted Nernst to develop the aforementioned "exact, mathematico-physical theory of the phenomena of excitation" in 1908).[61] Almost simultaneously, the Berlin clinician Franz Nagelschmidt—later claiming priority—also introduced high frequencies into electrotherapeutic practice, calling it diathermy.[62]

After a slow start, which was hampered, as one leading diathermist opined, by the still mediocre "technic"—and the considerable distrust regarding the utilization of such currents—diathermy "undoubtedly occupied the prime position among the electro-physical therapies" by the late 1920s.[63] As proponents had it, in Germany and elsewhere, high-frequency currents now provided a therapeutic means almost as "natural" as it was "rational"; there was, consequently, "scarcely a region of the body to which it ha[d] not been applied."[64] Meanwhile, as the technique was diffusing, the currents involved reached staggering proportions, or cycles-per-second. The year 1930, for instance, marked the advent of ultra-short wave therapy, when Willis Whitney, director of the General Electric research laboratories at Schenectady, New York, happened on "radio fever": "Men working in the field of a short wave radio transmitter," he found, "were having fever."[65] (Whitney promptly recruited Helen Hosmer, Fricke's former Cleveland colleague who, equipped with "powerful radio equipment," recreated the phenomenon with ease, being able to increase the temperature of both salt solutions and tadpoles by several degrees).

As a correlate to such electric business, the soon immense literature on diathermy and kindred short-wave applications was replete with attempts at elucidating the nature of AC currents, their putative physiological action, or the reality and presence of so-called specific effects (over and above the production of heat, that is); this naturally was a set of concerns typically accompanied by laments concerning the deficient physical and technical understanding of (most) medical practitioners. And it is here that our stories—the story of models-of-nerve and the story of applied biophysics—begin to intersect. For the generation of such physico-physiological knowledge was prompted in no small measure by the imperatives of high-frequency biomedical practice.

Textbooks on the subject thus routinely explained the nature of electricity and its biological effects.[66] But rather than puzzling over electricity's excitatory powers, practical biophysicists deployed a different register than academic physiologists, zeroing in on the spatial and temporal distribution of currents and on the physical properties of tissues such as dielectric constant, resistance, and "polarization capacity." Much like Fricke, who instinctively had turned to simple blood corpuscles and suspension of tumor cells, diathermy enthusiasts gravitated toward protein solutions, gelatine, or meat when investigating the "difficult subject of [bio-] electrical reactions."[67] They thus turned to the use of such simple, physical models when pondering the influence of, say, electrode shape and size on current distribution. Very nearly impossible to fathom in the abstract, high-frequency currents were most easily coaxed, for instance, into leaving their visible traces—or rather, three-dimensional zones (of coagulation) (see fig. 4.2).

In brief, the scientific cultures of bioelectricity that were taking shape around high-frequency currents differed in significant respects from the DC world of stimulus-response curves and "graphic methods." Where the latter stressed the correlations between their electric interventions and the phenomena induced, the former naturally was drawn to the physical properties of organic things; where the latter worried that the specificities of the electrical setups they employed were hopelessly entangled with—or deformed—the phenomena they induced, the former entangled things, as we shall see,

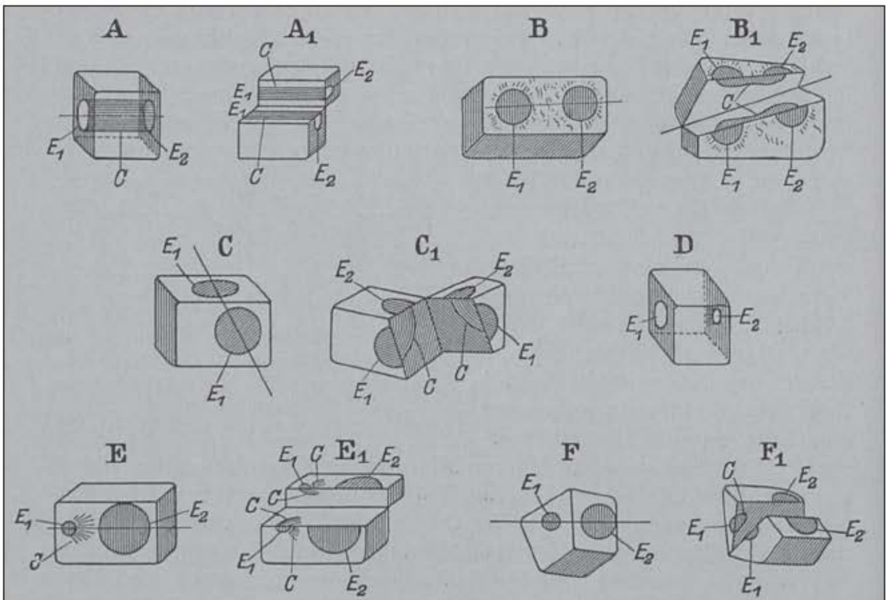

Abb. 41. Schema der Stromverteilung im Gewebe, durch Koagulationszonen (schraffiert) gekennzeichnet.

Figure 4.2. "Coagulated meat." Nagelschmidt, Lehrbuch der Diathermie für Ärzte und Studierende, Berlin, 1921, Springer-Verlag; Abbildung 41, p. 60.

on purpose: at its core resided the production of material "equivalence"—between gelatine and cells, cells and algae, algae and circuits.

To be sure, as theoretical entities, or for the purposes of calibration, quantities such as (tissue) resistance had long been of concern to physiologists. But ultimately, these projects—predicated on the regime of stimulation/response/inscription—had been geared toward other ends and constructs, many of them soon to be derided, as we have already heard, as merely phenomenological, treacherous "laws" and "formulae." In contrast, the techniques of high-frequency stimulation made salient quantities that, or so one said, had real "physical sense."

Circuits

The pursuit of circuit "equivalence" was not nearly exhausted by substituting patients for gelatine, of course. It didn't take long until such empiricist maneuvers were supplemented by a range of more theoretical considerations. As one medical scientist, this one based at the Rockefeller Institute for Medical Research, complained in 1927, hitherto, the spatio-temporal qualities of "heating effects" mainly had been studied "in vitro," preferably through "the coagulation of egg albumin or the cooking of meat and potatoes."[68] By implication, what was known about the physiological action of high-frequency currents was known mostly, and merely qualitatively, by "analogy" with what could be observed when electro-cooking various model-substances. Fortunately, the more abstractly minded biophysicists had already begun to intervene in the matter, turning such modeling-qua-substitution into a more formal affair.

For instance, Jesse McClendon. professor of physiologic chemistry at the University of Minnesota Medical School, already was pushing a more rigorous approach to such current distributions: "The extensive use of high frequency currents for heating the deeper tissues of the human body," as McClendon submitted in 1932, "has made it desirable to obtain more information on the path of the current between the electrodes and the distribution of heat in the tissues."[69] On McClendon's mind, in this regard it was the "localization of heating [that was] important." And therefore, it was essential to know the "seat of the . . . resistance." Like Fricke (who, we will remember, was after conductivity), McClendon thus availed himself to "bridge" circuits (see fig. 4.3)—"most extensively used by physical chemists, industrial chemists, and workers in biological sciences," as an assistant of McClendon's had explained in a 1928 review of the subject (characteristically, this focused especially on the beet root).[70]

Having most extensively studied the electrical properties of sea urchin eggs, muscular tissue, and blood suspensions himself, McClendon was confident that, now, a "true reproduction of the circuit within the cell" could be

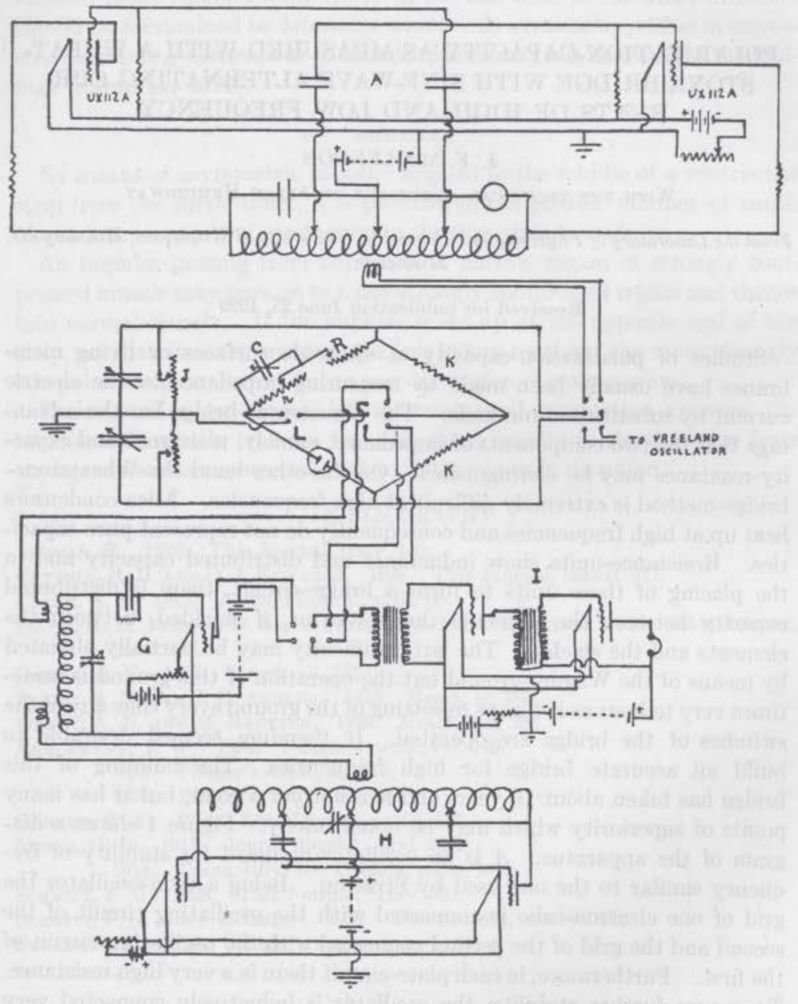

Fig. 1. Wheatstone bridge for measuring polarization-capacity of electrolytes with currents of high and low frequency.

Figure 4.3. Wheatstone bridge circuit (note the parallel "arms" in the center). J. F. McClendon, "Polarization-Capacity as Measured with a Wheatstone Bridge with Sine-Wave Alternating Currents of High and Low Frequency," *American Journal of Physiology* 91, no. 1 (1929): 80.

obtained.[71] Indeed, thanks to the art of frequency control, already a much more complex picture of the conditions in such electro-organic circuits had emerged. Unsurprisingly perhaps, much of this practice-induced theorizing revolved around the frequency dependency of a cell's (certain) electrical properties. Most notably, by the late 1920s there had been revealed the presence of a "capacitance" effect in addition to the tissue resistance; it made itself suspiciously manifest at the far, high-frequency end of the spectrum. And while its causes largely remained elusive, the clear implication was that none of the simplistic, "customary methods of obtaining balance" in a bridge circuit could result in a "true reproduction" of the unknown "circuit" that was the cell. At the very least, the more complex picture would involve, according to what quickly turned into the consensus view, a resistance (the cell interior) in series with a "leaky" condenser (the cell membrane).

Once established, such equivalent-circuit representations could be turned to manifold uses: gauging current distributions and devising means to control and improve them; diagnosing malignant tissue; or, based on measured, empirical values of conductivity, estimating the thickness—the real, physical dimensions, of cellular membranes (a good candidate for the "source of resistance" puzzling not only McClendon).[72] "Equivalence," in other words, was a theoretical concept anchored and honed in practice. Much like the "physical sense" these practical bio-electricians were fabricating, it sprang forth from the worlds they moved in (see fig. 4.4).

Cream, Algae, Nerve

Seen in this light, the turn from "laws" to models was the cumulative effect of a complex assortment of techniques, predicated on a multilayered, material logic of substitution: simple objects replacing complex ones; unknown, organic circuit elements being "balanced" by known, inorganic ones; and a set of diagrammatic and formal tools that themselves were drawn from the investigation of *technical* things: circuits. If my emphasis thus far has been on Fricke, it is because there is a direct line leading from Crile's biophysico-clinical venture to the nervous impulse as it was taking shape in 1939. For both Kenneth Cole and Howard Curtis (and, indirectly, Alan Hodgkin) were deeply familiar with the science of Hugo Fricke; this concluding section will resume their story.

Fricke's own initiation into biophysical research, as we have seen, took place in a world of blood suspensions, breast tumors, pathological conductivity changes, and X-ray dosimetry. Before long, however, Fricke found himself transplanted into the center of academic, "quantitative" biology: Cold Spring Harbor, the renowned home of the eponymous Symposia on Quantitative Biology. The first such gathering, staged in 1933 (five years

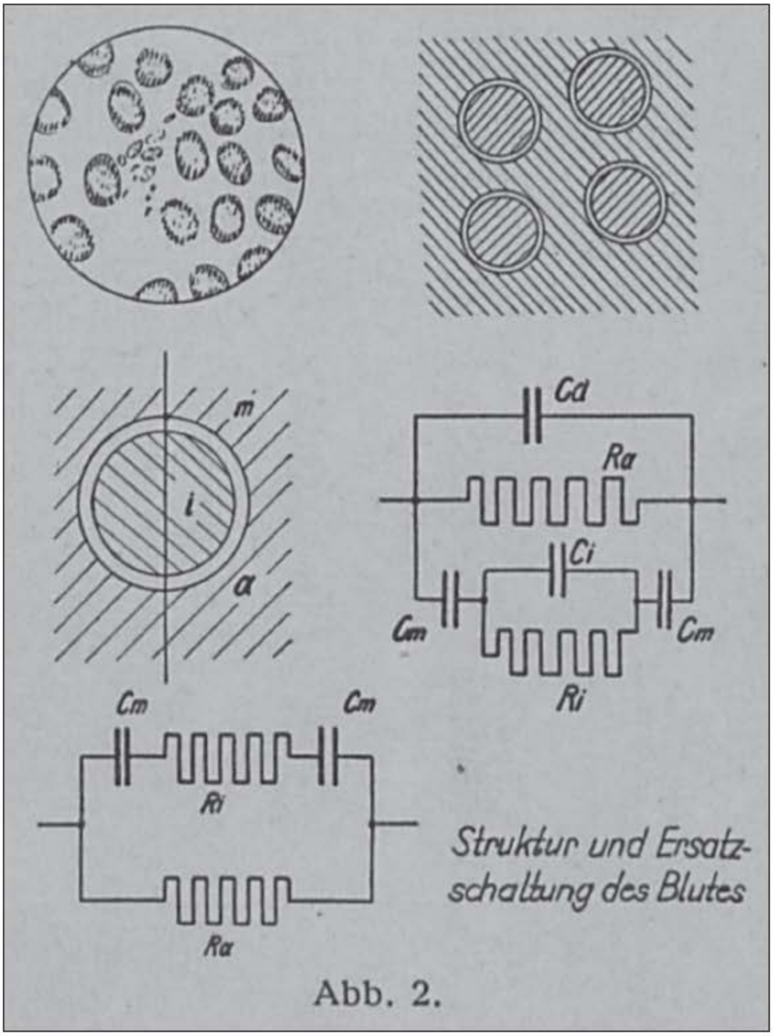

Figure 4.4. “Equivalent circuit of blood,” 1937. B. Rajewsky and H. Lampert, eds. *Erforschung und Praxis der Wärmebehandlung in der Medizin einschliesslich Diathermie und Kurzwellentherapie* (Dresden: Steinkopff, 1937), Abbildung 2, p. 85.

after Fricke's arrival in Long Island), not coincidentally dealt with surface phenomena in a symposium of that name. Participants included the likes of Herbert Gasser, Winthrop Osterhout, Eric Ponder, and Leonor Michaelis, as well as Kenneth Cole—biophysicists, for the most, of then or future acclaim. The "presence of such a group . . . each summer," as the published *Proceedings* announced, would hopefully "aid the Laboratory in its . . . aims of fostering a closer relationship between the basic sciences and biology."[73]

The publications now issuing from Fricke's circle clearly reflected his newly biological environs: "The study of the electric resistance of living cells," as one of Fricke's new students mused in 1931, "has been used chiefly in . . . special investigations on subjects such as the resistance of malignant tumors; but such problems of general physiology as growth or death, in relation to variation of frequency, remain almost untouched."[74] Among those who began to touch them now was Howard Curtis, an electronics-savvy Yale physics graduate who had recently been recruited by Fricke.[75] Meanwhile, too, Kenneth Cole had undergone a similar trajectory, gradually moving into more recognizably physiological territories as he had meandered from an apprenticeship with Fricke at Crile's Cleveland Clinic, via the High Tension Electrical Laboratory at Harvard, on to an assistant professorship in physiology at Columbia University.

Unsurprisingly, then, Cole and Curtis's forays into the sciences of the nervous system would bear the mark of their sometime teacher. Fricke, for his part, retained a preference for the simple, red corpuscle even in Long Island—albeit, as indicated, with a new emphasis. Once he had settled in, Fricke began to move beyond the merely *static* properties of membranes. It was the result of a complex set of factors: progress in high-frequency technique; the interaction with biological students who came to the picturesque location for summer school or more permanently to be "acquainted at first hand," as Fricke said, with the "findings" of biophysics; and not least, the Long Island site—a strategically located nature spot, "easily accessible to biologists residing in, or visiting, New York, and to those in passage to and from Europe."[76] And as if inspired by his new, and less morbid, surroundings, Fricke increasingly trained his attention on the *dynamic* aspects of the cellular life.

With Curtis's aid, the two of them soon were able to observe *variations* in the frequency-dependent, electrical characteristics of the cell as they induced membrane "desintegrations" through swelling in water (osmotic lysis), by way of freezing and thawing, and with various chemicals. "The fact that a change of the frequency dependence takes place," as they reported in 1935, "show[ed] that the injury cannot be due merely to a rupture in the membrane, but must be due to changes in the properties (increased permeability) of the membrane as a whole."[77]

The potential significance of these new horizons was clear enough—one observed physiological *changes*. But making intelligible such behavior was, as ever, difficult. Worse, certain "characteristics" of the natural surfaces were easily "obscured . . . by reason of their lack of homogeneity."[78] Fricke, always the practical biophysicist, therefore turned to even simpler, fabricated systems. His surviving notebooks show him grappling with various "model substances." On December 17, 1934, for instance, Fricke prepared a "heavy suspension of whipping cream in H20." January brought "Lion brand evaporated milk-homogenized" and solutions of "1% of 'Cooper's' gelatin." Or again, suspensions of (relatively simple) yeast cells, he found, also exhibited sudden, drastic, and reversible drops in resistance and capacitance at high frequencies.[79] These sudden changes, they reasoned, were thus unlikely due to "minute disintegration[s]" of the lipoid layer surrounding these cells. Rather, being reversible, such behavior indicated processes that were *functional* in nature. Meanwhile, Fricke struggled with the detailed interpretation of these observations, jotting down calculations next to circuit diagrams and wondering about "condition[s] of equivalence."[80]

But to no avail. While Fricke was able to generate increasingly better guesses at the physical dimensions of these—possibly bimolecular—cellular membranes, no clear "conception as to the origin of the dielectric properties of cell membranes" was in evidence, as Fricke confessed in 1937; and neither was a conception of their *changes*.[81] Indeed, increasingly consumed with problems of radiation biology, it was not for Fricke to carry this particular case forward; rather, it was Kenneth Cole, who by then had teamed up with Curtis, to whom was due the protracted migration of high-frequency measurements into the realms of the nervous.

Cole's biophysical career path was fairly prototypical otherwise. "Accumulating batteries, magnets, and other worn out parts" during his youth already,[82] Cole had been soaking up the wireless arts all his life: at the General Electric Research Laboratory in Schenectady (where he had spent two years after high school); as a physics graduate at Cornell; in the course of a NRC fellowship with Emory Chaffee at Harvard—an authority on vacuum tubes and someone who regularly weighed in on biophysical matters (for instance, on the sterilization of fruit juices, "ultra-violet" therapeutic lamps, iono-atmospheric hygiene, or "diathermy from the view point of physics"); and most fatefully, perhaps, at Crile's biophysical clinic, where Cole went for a summer job in 1925 after responding to a note hung up in the Cornell physics department: "Wanted, at the Cleveland Clinic, two biophysicists."

Unsurprisingly, Cole's first forays into biophysical matters very much (or merely) centered on the technics of bioelectric, high-frequency measurement (indeed, it was a Bell labs engineer—K. S. Johnson, at the time a visiting professor at Harvard—who had introduced the young Cole to the more esoteric dimensions of circuit equivalence). And much like anyone else in this essay, this naturally attracted him toward simple model systems: suspensions of calf blood, cat diaphragms, skin, potato slices, sea urchin eggs, or muscle.[83] What is more, by the early 1930s, Cole had already turned to charting out a bigger picture. In all these cases, the "frequency characteristics of tissues," as Cole informed his biophysical peers at the symposium titled Surface Phenomena at Cold Spring Harbor in 1933, could be traced to a single, "variable impedance element"; its "seat," Cole confidently declared, "is probably the cell membrane."[84]

Paralleling Fricke's own move into the more "active" (or *living*) systems, Cole—by now at Columbia—evidently had begun to venture into more complex terrains as well. In collaboration with a Columbia anatomist, for instance, Cole then turned toward the high-frequency analysis of embryo rat heart muscle cultures—a rather more active thing than potato slices (little could be made, however, of the heaps of confusing data the muscle cultures produced); with Emil Bozler, a German zoologist visiting the Johnson Foundation for Medical Physics in Philadelphia, Cole took on impedance changes during muscular activity and rigor (these proved similarly elusive); and not least, Cole then won the attention of Warren Weaver, who encouraged him to submit a "program of research on the electrical constants of the membrane and cytoplasm of the normal and abnormal cell" to the Rockefeller Foundation.[85]

But Cole did not yet worry much about nervous impulses, let alone the "messages" so broadcast. Indeed, the erratic behavior of the above, complex objects already and all-too-easily sabotaged the aim of investigating the functional changes these objects quite evidently underwent; the organic, bioelectrical action of living things made the analytic task of the bioelectric engineer very difficult indeed (it was [electrically] the "passive" effects that one "always hope[d] to maintain during the measurement," as Cole noted).[86] Ideally, experimental objects should be both, simple *and* living, but not too active: a single cell. The "most direct attack," as Cole had noted in 1933 at the Surface Phenomena gathering, would be to relinquish such complex materials altogether and to measure the impedance "between the interior and exterior of a single cell . . . such that the most of the current traverses the cell membrane."[87]

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Our story has thus come full circle—or has come so almost: although the ideal experimental design seemed clear enough now, regarding the study of nerve (or, indeed, of any living cell), the prospects were still daunting. At the time, only a few electric investigators had felt their way toward single cells. The required minuscule microelectrodes were by and large a technology of the future. Still, biologists routinely worked on isolated organs, whole tissues, entire bundles of nerve fibers, or suspensions. The "single cells" that came into question at all, because they were large enough, weren't even real cells but unicellular algae, tulip spores, and marine eggs. They also were very fragile objects, and measuring them in the way Cole proposed meant "impaling" them—a highly precarious affair. In brief, a great many factors were in place that would have served to render the nerve impulse a far from obvious object of investigation to electrically minded investigators such as Cole: it was too complex; too delicate and "alive"; and thus too unsuited for measuring/modeling.

If, just about six years later, the *New York Times* reported that "Drs. Cole and Curtis" had uncovered "a sort of Rosetta stone for deciphering the closely guarded secrets close to the very borderland of mind and matter,"[88] it was the sudden appearance of two new experimental objects, which principally altered the position, smoothing the transition from the study of tumors, eggs, potatoes, and whipped cream to those very borderlands of mind and matter. One was the giant squid axon—a nerve axon visible to the plain eye, which the young Oxford zoologist John Z. Young rediscovered in 1936 (the same year that Young was touring the East Coast on a Rockefeller stipend).[89] The other was the simplest kind of cell imaginable: *Nitella*. Unearthed on the tropical beaches of Bermuda, here was an uncomplicated, living object—a giant algae—which, as W. J. V. Osterhout, Jaques Loeb's successor at the Rockefeller Institute, had noted in about 1927, exhibited an impulse-like phenomenon when injured: A "wave of some sort," as Osterhout said, "which we may for convenience call a death wave."[90] This death wave, as he perceptively surmised as well, clearly "resembl[ed] action currents of nerve and muscle." It only traveled much more slowly.

And it was this quasi-nerve, which provided the intermediary between complexity and simplicity, between the real thing—the nerve impulse—and the electric passivity of skimmed milk and sea urchins. Indeed, if the possibilities the squid giant axon offered in terms of membrane analysis were plainly obvious, adapting existing techniques was not. But the remedy would have come very naturally to circuit-savvy investigators such as Drs. Cole and Curtis: substitute, simplify, replace the elements in the circuit—such were the powers of circuit equivalence. "The experimental procedure and the technique of analysis," as Cole and Curtis happily conceded, were "fundamentally the same as those used in Nitella during activity."[91]

And we may conclude, then, that a great many disparate things went into the electrical fabrication of the nerve impulse, indeed: *Nitella* but also coagulated meat, sea urchins and lowly plants, high-frequency currents, diagrams, circuitry, the scenes of medical physics, the electronic arts, as well as radio-cultural forms of instrument use. Historiographically speaking, by the mid-twentieth century, the genesis and legibility of the nerve impulse thus not only depended on particular interpretational techniques, but these techniques themselves were embedded in experimental and material cultures that largely would remain invisible were one to adopt the narrow perspectives from academic nerve physiology, of metaphors of "messages" or inscription devices.[92] But neither should this story of the nervous impulse be construed as a story of physicists (or engineers) colonizing biology.[93] The point that I've tried to convey is that interwar bio/medical physics was a more complex, homegrown and heterogeneous subject matter than that. Rather than being imposed from the outside, circuitry-based modeling practices, for one, reflected the variety of medico-physical practices that surged in the interwar period, notably those having to do with high-frequency currents. And more generally, as I have suggested, they reflected the permeation of interwar life-worlds with electrical technologies.[94] It was, after all, a time when "every child dabbled in resonance, filter circuits and distortions" (as one German physiologist put it).[95]

Regarding technique and technology, the turn from "laws" to "models" in neurophysiology is best conceived, accordingly, not in terms of this or that instrument's transformative role—an object and its impact. The arts of "wireless" weren't anything like that; they were a system, a culture of oftentimes DIY-esque bricolage, a set of *Kulturtechniken* (such as reading a circuit diagram). But nothing that could have exerted a single, unidirectional, let alone deterministic influence—the rhetoric of actors notwithstanding.[96] By implication, and curiously enough, "nerve" (let alone brains) may not always be the best guide to the history of the nervous system: regarding the interwar "impulse," circuits, nerve, and whipped cream went together.

Notes

Epigraph. O. H. Caldwell, "The Electron Tube . . . A Universal Tool in Industry," *Electronics* 1 (April 1930): 10–11.

1. Edgar D. Adrian, *The Mechanism of Nervous Action: Electrical Studies of the Neurone* (Philadelphia: Pennsylvania University Press, 1932), 2.

2. Harry Grundfest, "Potentialities and Limitations of Electrophysiology," in *Nerve Impulse: Transactions of the First Conference*, ed. David Nachmansohn, 18–19 (New York: Josiah Macy Jr. Foundation, 1950).

3. See, for example, Timothy Lenoir, "Models and Instruments in the Development of Electrophysiology, 1845–1912," *Historical Studies in the Physical Sciences* 17, no. 1 (1986): 1–54; Robert G. Frank, "Instruments, Nerve Action, and the All-or-None Principle," *Osiris* 9 (1994): 208–35; Laura Otis, "The Metaphoric Circuit: Organic and Technological Communication in the Nineteenth Century," *Journal of the History of Ideas* 63, no. 1 (2002): 105–28; Sven Dierig, "Engines for Experiment: Laboratory Revolution and Industrial Labor in the Nineteenth-Century City," *Osiris* 18 (2003): 116–34; Cornelius Borck, "Electrifying the Brain in the 1920s: Electrical Technology as a Mediator in Brain Research," in *Electric Bodies: Episodes in the History*

of Medical Electricity, ed. Paola Bertucci and Giuliano Pancaldi, 239–64 (Bologna: CIS, 2001); Robert L. Schoenfeld, *Exploring the Nervous System: With Electronic Tools, an Institutional Base, a Network of Scientists* (Boca Raton, FL: Universal Publishers, 2006); Henning Schmidgen, *The Helmholtz-Curves: Tracing Lost Time* (New York: Fordham University Press, 2014).

4. On the EEG, see Cornelius Borck, *Hirnströme: Eine Kulturgeschichte der Elektroenzephalographie* (Göttingen: Wallstein, 2005). On said "war," see Elliot S. Valenstein, *The War of the Soups and the Sparks: The Discovery of Neurotransmitters and the Dispute over How Nerves Communicate* (New York: Columbia University Press, 2005).

5. O. H. Caldwell, "Engineers, Components, Parts," *Electronics* 2 (November 1931): 173.

6. See Alan Hodgkin and Andrew F. Huxley, "Action Potentials Recorded from Inside a Nerve Fibre," *Nature* 144 (1939): 710–11. More broadly, see Alan Hodgkin, *Chance and Design: Reminiscences of Science in Peace and War* (Cambridge: Cambridge University Press, 1992); Max Stadler, "Assembling Life: Models, the Cell, and the Reformations of Biological Science, 1920–1960" (PhD diss., Imperial College London, 2009), chap. 4.

7. Kenneth Cole and Howard Curtis, "Electric Impedance of the Squid Giant Axon during Activity," *Journal of General Physiology* 22 (1939): 649–70; Kenneth Cole and Alan Hodgkin, "Membrane and Protoplasm Resistance in the Squid Giant Axon," *Journal of General Physiology* 22 (1939): 671–87. See also Kenneth Cole, *Membranes, Ions, and Impulses: A Chapter of Classical Biophysics* (Berkeley: University of California Press, 1968).

8. Chandler McC. Brooks, "Current Developments in Thought and the Past Evolution of Ideas Concerning Integrative Function," in *The History and Philosophy of Knowledge of the Brain and Its Functions,* ed. F. N. L. Poynter, 248 (Oxford: Blackwell, 1957).

9. For a recent example of this kind of narrative, see Eric Kandel, *In Search of Memory: The Emergence of a New Science of Mind* (New York: W. W. Norton, 2006).

10. For instance, John Bradley and Tilly Tansey, "The Coming of the Electronic Age to the Cambridge Physiological Laboratory: E. D. Adrian's Valve Amplifier in 1921," *Notes and Records of the Royal Society* 50, no. 2 (1996): 217–28; David Millet, "Wiring the Brain: From the Excitable Cortex to the EEG, 1870–1940" (PhD diss., University of Chicago, 2001); Borck, *Hirnströme.*

11. Regarding the historiography, the production of "curves" (or "inscriptions") has been of signal importance, of course. See, for instance, Frederic L. Holmes and Kathryn Olesko, "The Images of Precision: Helmholtz and the Graphical Method in Physiology," in *The Values of Precision,* ed. M. Norton Wise, 198–221 (Princeton, NJ: Princeton University Press, 1995); Soraya de Chadarevian, "Graphical Method and Discipline: Self-Recording Instruments in Nineteenth-Century Physiology," *Studies in History and Philosophy of Science* 24, no. 2 (1993): 267–91; Philipp Felsch, *Laborlandschaften: Physiologen über der Baumgrenze, 1800–1900* (Göttingen: Wallstein, 2007); and Schmidgen, *Helmholtz-Curves.*

12. Ibid., all citations. More broadly, of course, the appeal of the graphic method owed to the increasing significance that was attached to all things visual. See, e.g., Bruno Latour, "Visualisation and Cognition: Thinking with Eyes and Hands," in *Knowledge and Society: Studies in the Sociology of Culture,* ed. Henrika Kuklick, 1–40 (Greenwich, CT: Jai Press, 1988).

13. See, e.g., Gustav Oppenheim, "Die Schallplatte im Dienste der Elektro-Medizin," *Klinische Wochenschrift* 11, no. 14 (1932): 595–97; Francis O. Schmitt and Otto H. Schmitt, "A Universal Precision Stimulator," *Science* 76, no. 1971 (1932): 328–30; Archibald V. Hill, "Repetitive Stimulation by Commutator and Condenser," *Journal of Physiology* 82, no. 4 (1934): 423–31; cited in Ferdinand Scheminzky, "Über einige Anwendungen der Elektronenröhren in Widerstandsschaltung und der Glimmlampen für die Physiologie," *Pflüger's Archiv* 213, no. 1 (1926): 126–27.

14. Any biography of Edison or Tesla will feature the story of this AC/DC "war." See, for example, W. Bernard Carlson, *Tesla: Inventor of the Electrical Age* (Princeton, NJ: Princeton University Press, 2013).

15. Walter Nernst, "Zur Theorie Des Elektrischen Reizes," *Pflügers Archiv* 122, nos. 7–9 (1908): 275–314.

16. Edgar D. Adrian, *The Basis of Sensation: The Action of the Sense Organs* (London: Hafner, 1928), 39.

17. William H. Eccles, "The New Acoustics," *Proceedings of the Physical Society* 41 (1929): 232–33.

18. Burton Baker Grover, *High Frequency Practice for Practitioners and Students* (Kansas City, KS: The Electron Press, 1925), 3.

19. Halloway Davis and Alexander Forbes, "Chronaxie," *Physiological Reviews* 16 (1936): 407–41.

20. Ibid., 410.

21. Cited in Hans Rosenberg, "Untersuchungen über Nervenaktionsströme," *Pflüger's Archiv* 223, no. 1 (1930): 120–21.

22. On the concept of "chronaxie," see Joy Harvey, "L'autre Côté Du Miroir (The Other Side of the Mirror): French Neurophysiology and English Interpretations," in *Les Sciences Biologiques Ed Médicales En France 1920–1950*, ed. Claude Debru, Jean Gayon, and Jean-Francois Picard, 71–81 (Paris: CNRS Editions, 1994).

23. For instance, Max Cremer, "Erregungsgesetze des Nerven," in *Handbuch der Normalen und Pathologischen Physiologie*, ed. Albrecht Bethe, vol. 9 (Berlin: Springer, 1929); Hans Schaefer, "Neuere Untersuchungen über den Nervenaktionsstrom," *Ergebnisse der Physiologie* 36, no. 1 (1934): 151–248; Walter Eichler, "Über die Abhängigkeit der Chronaxie des Nerven vom äusseren Widerstande," *Zeitschrift für Biologie* 91 (1931): 475.

24. W. A. H. Rushton, "A Physical Analysis of the Relation between Threshold and Interpolar Length in the Electric Excitation of Medullated Nerve," *Journal of Physiology* 82, no. 3 (1934): 483.

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26. Johann D. Achelis, "Kritische Bemerkungen zur Chronaxiebestimmung am Menschen," *Zeitschrift für Neurologie* 130, nos. 5–6 (1933): 233.

27. Cremer, "Erregungsgesetze," 255–56.

28. Friedrich Lewy, "Bericht: Die Chronaxie," *Deutsche Zeitschrift für Nervenheilkunde* 129, nos. 5–6 (1933): 186–87.

29. Herbert Gasser, "Axon Action Potentials in Nerve," in *Cold Spring Harbor Symposia in Quantitative Biology*, vol. 1 (Cold Spring Harbor, NY: CSH Cold Spring Harbor Laboratory Press, 1933), 143.

30. Hans Schaefer, *Elektrophysiologie. I. Band: Allgemeine Elektrophysiologie* (Vienna: Franz Deuticke, 1940), iv.

31. E. J. Hart, "Hugo Fricke, 1892–1972," *Radiation Research* 52, no. 3 (1972): 642–46.

32. On Crile, see *George Crile: An Autobiography*, ed. Grace Crile (Philadelphia: J. B. Lippincott, 1947).

33. Winthrop Osterhout to E. S. Harris, June 13, 1928, folders "Dr Hugo Fricke" (folder 3), Special Collections, Cold Spring Harbor Laboratory, NY.

34. George Crile, *A Bipolar Theory of Living Processes* (New York: Macmillan, 1926).

35. Unless noted otherwise, biographical information on Cole is based on Kenneth Cole, oral interview transcript, National Institutes of Health oral history collection, National Library of Medicine, Bethesda, MD.

36. Hugo Fricke to Harris, July 31, 1928, folders "Dr Hugo Fricke" (folder 3), Special Collections/Cold Spring Harbor Laboratory, NY.

37. Crile, *Bipolar Theory of Life*, 3.

38. George Crile, *A Mechanistic View of War and Peace* (New York: Macmillan., 1915), vii; Crile, *George Crile*, 328, 369–70.

39. Crile, *Bipolar Theory of Life*, 7.

40. For example, "'Suns' in Man's Body Pictured by Crile," *New York Times*, November 26, 1932.

41. Hugo Fricke to Harris, July 31, 1928, folders "Dr Hugo Fricke" (folder 3), Special Collections, Cold Spring Harbor Laboratory, NY.

42. More broadly on the notion of electrical equivalence, see Emily Thompson, *The Soundscape of Modernity: Architectural Acoustics and the Culture of Listening in America, 1900–1933* (Cambridge, MA: MIT Press, 2002); Roland Wittje, "The Electrical Imagination: Sound Analogies, Equivalent Circuits, and the Rise of Electroacoustics, 1863–1939," *Osiris* 28, no. 1 (2013): 40–63.

43. Hugo Fricke and Sterne Morse, "The Electric Capacity of Tumors of the Breast," *Journal of Cancer Research* 16 (1926): 340.

44. Ibid.

45. Crile, *Bipolar Theory of Life*, 15.

46. "The Expanding Short-Wave Spectrum," *Electronics* 3, no. 3 (September 1931), 91.

47. "Raw Materials, Costs—in Tube Manufacture," *Electronics* 1, no. 8 (November 1930), 366.

48. Hugo Fricke, "The Electric Impedance of Suspensions of Biological Cells," in *Cold Spring Harbor Symposia in Quantitative Biology*, vol. 1 (Cold Spring Harbor, NY: CSH Cold Spring Harbor Laboratory Press, 1933), 117.

49. See Stadler, "Assembling Life"; Thompson, *Soundscape of Modernity;* Wittje, "Electrical Imagination."

50. Pnina Abir-Am, "The Biotheoretical Gathering, Trans-Disciplinary Authority and the Incipient Legitimation of Molecular Biology in the 1930s: New Perspective on the Historical Sociology of Science," *History of Science* 25 (1987): 1–70; Lilly E. Kay, *The Molecular Vision of Life* (Oxford: Oxford University Press, 1993); Nicolas Rasmussen, "The Mid-Century Biophysics Bubble: Hiroshima and the Biological Revolution in America, Revisited," *History of Science* 35, no. 109 (1997): 245–93; Soraya

de Chadarevian, *Designs for Life: Molecular Biology after World War II* (Cambridge: Cambridge University Press, 2002).

51. For a more detailed account, see Stadler, "Assembling Life."

52. See C. Thomas de la Peña, *The Body Electric: How Strange Machines Built the Modern American* (New York: New York University Press, 2003); Cornelius Borck, "Electricity as a Medium of Psychic Life: Electrotechnological Adventures into Psychodiagnosis in Weimar Germany," *Science in Context* 14, no. 1 (2001): 565–90.

53. See "A Review of the Medical Curriculum" (1930), ROUGHTON Papers, box 34.60u, American Philosophical Society, Philadelphia, and "Register of Biophysical Assistants," *The Lancet* 215, no. 5570 (May 31, 1930): 1195–96.

54. Wolfgang Holzer, "Modelltheorie über die Stromdichte im Körper von Lebewesen bei Galvanischer Durchströmung in Flüssigkeit," *Pflüger's Archiv* 232, no. 1 (1933): 195–96.

55. For instance, John B. Bateman, "Mitogenetic Radiation," *Biological Reviews and Biological Proceedings of the Cambridge Philosophical Society* 10, no. 1 (1935): 42–71.

56. Frank, "Instruments, Nerve Action"; Bradley and Tansey, "Coming of the Electronic Age."

57. Erwin Schliephake, "Die Biologische Wärmewirkung im elektrischen Hochfrequenzfeld," *Verhandlungen der Deutschen Gesellschaft für innere Medizin* 4 (1928): 307–10.

58. Georges Lakhovsky, "Curing Cancer with Ultra Radio Frequencies," *Radio News Magazine* (February 1925), 1282–83; Georges Lakhovsky, *The Secret of Life*, trans. Mark Clement, 2nd ed. (London: Heinemann, 1939).

59. Elkin P. Cumberbatch, *Diathermy; Its Production and Uses in Medicine and Surgery*, 2nd ed. (St. Louis: C. V. Mosby, 1928), 3–4.

60. Josef Kowarschik, *Die Diathermie* (Vienna: Springer, 1913), 3.

61. Nernst, "Zur Theorie des Elektrischen Reizes," 275–76, 313.

62. Franz Nagelschmidt, *Lehrbuch der Diathermie für Ärzte und Studierende* (Berlin: Springer, 1921).

63. Josef Kowarschik, *Die Diathermie*, 7th ed. (Vienna: Springer, 1930), iii; Hans Henseler and Erich Fritsch, *Einführung in die Diathermie vom Medizinischen und Technischen Standpunkt* (Berlin: Radionta-Verlag, 1929), 5.

64. Elkin P. Cumberbatch, "Uses of Diathermy in Medicine and Surgery," *The Lancet*, February 7, 1931, 281.

65. J. Stafford, "Radio Waves Cause Fever in Patients to Cure Dreaded Paresis," *The Science News-Letter* 18, no. 484 (1930): 36; C. M. Carpenter and A. B. Page, "The Production of Fever in Man by Short Radio Waves," *Science* 71, no. 1844 (1930): 450–52.

66. See, for instance, Kowarschik, *Die Diathermie*; Nagelschmidt, *Lehrbuch*; Cumberbatch, *Diathermy*; Erwin Schliephake, *Ondes Electriques Courtes En Biologie* (Paris: Gauthier-Villars, 1938); Wolfgang. Holzer and Eugen Weissenberg, *Foundations of Short-Wave Therapy: Physics-Technics-Indications* (London: Hutchinson, 1935); Boris Rajewsky and Heinrich Lampert, eds., *Erforschung und Praxis der Wärmebehandlung in der Medizin einschliesslich Diathermie und Kurzwellentherapie* (Dresden: Steinkopff, 1937).

67. "Reviews and Notices of Books," *The Lancet* 215, no. 5551 (1930): 140.

68. Carl A. L. Binger and Ronald V. Christie, "An Experimental Study of Diathermy," *Journal of Experimental Medicine* 46, no. 4 (1927): 571–72.

69. Allan Hemingway and Jesse F. McClendon, "The High Frequency Resistance of Human Tissue," *American Journal of Physiology* 102 (1932): 56.

70. Roe Remington, "The High Frequency Wheatstone Bridge as a Tool in Cytological Studies; with Some Observations on the Resistance and Capacity of the Cells of the Beet Root," *Protoplasma* 5, no. 1 (1928): 353–54; Jesse F. McClendon, "Polarization-Capacity as Measured with a Wheatstone Bridge with Sine-Wave Alternating Currents of High and Low Frequency," *American Journal of Physiology* 91, no. 1 (1929): 83–93.

71. Remington, "High Frequency Wheatstone Bridge," 356–58.

72. A more exhaustive account than I can provide here would include figures such as B. S. Gossling of the GEC Research Laboratories, Wembley, a short-wave therapy expert and someone naturally straddling the "essential differences of outlook between electro-engineering and therapy." It would also have included the likes of Boris Rajewsky, the Russian émigré biophysicist who, as director of the Kaiser-Wilhelms-Institute for Biophysics, put some considerable effort into clearing up the physical foundations of high-frequency interventions. Or again, it would have included figures such as Wolfgang Holzer, a graduate of the High Voltage Institute at the University of Berlin, and also the author of *Foundations of Short-Wave Therapy: Physics-Technics-Indications* (1935), which he had co-written with the medical superintendent of the short wave section at the Vienna University Clinic for Nervous and Mental Diseases. Much like everyone else in the short-wave business, Holzer was primarily after the electrical principles involved in "the action of ultra-high frequency currents on biological materials."

73. *Cold Spring Harbor Symposia in Quantitative Biology* (Cold Spring Harbor, NY, 1933), v.

74. Basile Luyet, "Variation of the Electric Resistance of Plant Tissues for Alternating Currents of Different Frequencies during Death," *Journal of General Physiology* 15, no. 3 (1932): 283.

75. On Curtis, see Raymond E. Zirkle, "Howard James Curtis, 1906–1972," *International Journal of Radiation Biology* 23, no. 6 (1972): 530–32.

76. Hugo Fricke, memorandum "General in Biophysics," August 23, 1930, folders "Dr Hugo Fricke" (folder 3), Special Collections, Cold Spring Harbor Laboratory, NY.

77. Hugo Fricke and Howard Curtis, "The Electric Impedance of Hemolyzed Suspensions of Mammalian Erythrocytes," *Journal of General Physiology* 18 (1935): 836.

78. Hugo Fricke and Howard Curtis, "Electric Impedance of Suspensions of Yeast Cells," *Nature* 134, no. 3377 (1934): 102.

79. See Notebook V, folder "Fricke Notebook, Book V, Nov 28 '34-Dec 18 '35," box 2, Special Collections, Cold Spring Harbor Laboratory, NY.

80. Ibid.

81. Hugo Fricke to Winthrop Osterhout, February 18, 1937, folder "Hugo Fricke," box 2, Osterhout Papers, American Philosophical Society Library, Philadelphia.

82. Biographical information, as indicated, is based on Cole, oral interview transcript, National Institutes of Health oral history collection, National Library of Medicine.

83. Cole began to "duplicate" Fricke's high-frequency bridge while at Harvard.

84. Kenneth Cole, "Electric Conductance of Biological Systems," in *Cold Spring Harbor Symposia in Quantitative Biology*, vol. 1, 107–16 (New York: CSH Cold Spring Harbor Laboratory Press, 1933).

85. Kenneth Cole to Rockefeller Foundation, September 23, 1935, Rockefeller Archives RG.1.1, series 200, box 133, folder 1650; and see, for instance, Emil Bozler and Kenneth Cole, "The Electric Impedance and Phase Angle of Muscle in Rigor," *Journal of Cellular and Comparative Physiology* 6, no. 2 (1935): 229–41. Further details can be found in Stadler, "Assembling Life."

86. Cole, "Electric Conductance of Biological Systems," 114–15.

87. Ibid., 111.

88. "New Clues Found to Life Process," *New York Times*, February 27, 1938, 35.

89. See R. S. Bear, F. O. Schmitt, and J. Z. Young, "The Sheath Components of the Giant Nerve Fibres of the Squid," *Proceedings of the Royal Society of London*, series B, 123, no. 833 (1937): 496–504.

90. W. J. V. Osterhout and E. S. Harris, "The Death Wave in Nitella," *Journal of General Physiology* 12 (1928): 186.

91. Cole and Curtis, "Electric Impedance of the Squid Giant Axon," 650.

92. Owing to post–World War II developments (notably cybernetics), the word *circuits*, as is well-known, increasingly would come to signify codes, messages, and so on; this, however, tends to obscure the wider, richer histories of circuitry, which typically had little to do with cybernetics. On the former, see, for instance, Lilly E. Kay, "From Logical Neurons to Poetic Embodiments of Mind: Warren S. McCulloch's Project in Neuroscience," *Science in Context* 14, no. 4 (2001): 591–614; and Tara Abraham, "From Theory to Data: Representing Neurons in the 1940s," *Biology and Philosophy* 18, no. 3 (June 2003): 415–26.

93. On this topos, see Abir-Am, "The Biotheoretical Gathering"; and Pnina Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's 'Policy' in Molecular Biology," *Social Studies of Science* 12, no. 3 (1982): 341–82.

94. On this point, also see Jeff Hughes, "Plasticine and Valves: Industry, Instrumentation and the Emergence of Nuclear Physics," in *The Invisible Industrialist: Manufactures and the Production of Scientific Knowledge*, ed. Jean-Paul Gaudillière and Illana Lõwy, 58–101 (London: Macmillan, 1998); Kristen Haring, *Ham Radio's Technical Culture* (Cambridge, MA: MIT Press, 2006); Wittje, "Electrical Imagination."

95. Otto Ranke, "Philipp Broemser," *Ergebnisse Der Physiologie* 44, no. 1 (1941): 1–2.

96. Unsurprisingly, therefore, "technological determinism" has been troubling historians of electrophysiology. See Frank, "Instruments, Nerve Action"; Cornelius Borck, "Between Local Cultures and National Styles: Units of Analysis in the History of Electroencephalography," *Comptes Rendus Biologies* 329, nos. 5–6 (2006): 450–59.