

Chapter 18: Observation Overruled Without Appeal 1977-1979 ¹

THE CONTROVERSY OVER THE Navy's antenna ended with an administrative victory that wore the costume of scientific resolution. The crucial work was not done in a laboratory and did not involve rebutting evidence point by point. The crucial work was done by constructing a process that decided — before evidence could matter — what counted as a legitimate question, what counted as mechanism, and what counted as credibility. The Academy committees did not merely weigh uncertainty, they converted it into the appearance of consensus. Dissent could be acknowledged as a voice and still be denied as a basis for action, because the institution had already decided where legitimacy began and ended. The endpoint was an authoritative declaration that there was nothing to fear—not because risk was disproved, but because risk was made institutionally illegitimate. The public saw the Academy's end-products, not the machinery that generated them.

Handler did not win by meeting Becker's evidence on equal terms; he won by constructing a procedural world in which the possibility of electromagnetic risk was defined out of existence before any committee convened and before any report was drafted. The boundaries of legitimate science in Handler's world were choosing the right people for his committees, constraining the questions posed, managing the documents and deadlines, and letting the desired outcome present itself as the natural result. What the public encountered was not his machinery, but the end-product: the Academy's imprimatur, the prestige of institutional objectivity, and the reassuring proclamation that there was nothing to fear, which was made possible only because his system ensured that nothing threatening could be acknowledged. That outcome established more than a verdict about electromagnetic energy, it demonstrated a reusable administrative method. Once consensus was manufactured and laundered into policy through the Academy's cultural authority, the same method could be used to resolve problems unrelated to disputes the Navy wanted quieted and toward scientific issues Handler wanted resolved in a particular way.

Handler's interaction with Becker showed, in miniature, how biomedical empiricism could be left to drift without institutional mooring while the Academy and media flattened the dispute into a polite difference of opinion, and Handler could publicize Academy committee outputs as neutral arbiters rather than instruments of his design.

The antenna affair ended with an outcome that looked like scientific closure but depended, in its decisive steps, on Handler's institutional design. The controversy didn't turn on who had the better scientific evidence but rather on who controlled the procedures that determine what counted as a legitimate question and a legitimate answer.

Handler's next target was not a national controversy about anthropogenic electromagnetic energy. It was a category of biomedical experimentation Handler despised at a location he did not control and influenced only slightly — emergent biomedical research inside the Veterans Administration (VA), as exemplified by Robert Becker's regeneration and bioelectromagnetism projects. The research service at the VA — in sharp distinction to other federal agencies that funded biomedical research — supported emergent biomedical research, which has a serious affront to Handler because it constituted a living institutional challenge to

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his ideology, biochemical reductionism. Although Handler's target changed, his method did not. Here too, his central move was not a direct scientific refutation but rather construction of an evaluation environment in which certain kinds of research did not count as high quality, by definition, and therefore should not survive. Handler utilized the technique throughout his tenure as the head of the Academy, and had only recently employed successfully, at least in his eyes, to orchestrate the result the Navy desired. He commenced to employ it against his new target, emergent biomedical research inside the VA.

Handler's technique consisted of a sequence of steps, each of which he presented as ordinary professionalism. His FIRST STEP was to secure a neutral-appearing entry point into the VA's internal functioning. As it happened, the Congress, however inadvertently, provided Handler with an ideal opportunity to get inside the VA. In the mid-1970s, as the Veterans Administration struggled under the weight of scandal, understaffing, and the complex legacy of Vietnam, the Congress turned to the National Academy of Sciences for help. A new law compelled the VA to invite the Academy in, ostensibly to answer managerial questions about resources: how many beds, how many nurses, how many doctors do the hospitals need? The same law also mandated that the VA contract with the Academy to study its biomedical research program and determine what medical resources the VA needed and what research it should support, if any. In the antenna controversy, Handler used Academy procedures to make a category of risk — health harms from anthropogenic electromagnetic energy — administratively difficult to recognize as legitimate; here he would use the same procedural leverage to make a category of research — emergent biomedical work exemplified by Becker — administratively difficult to sustain. The object changed from *risk* to *research*, but the mechanism was the same — define legitimacy through putative standards, and the conclusions follow.

Handler's SECOND STEP was to restrain and focus the VA's research affairs. In the Academy's biomedical research report, delivered in 1977, Handler turned the congressional mandate into a weapon. Rather than merely complying with the oversight mandate, Handler opportunistically leveraged it to advance the policy direction he desired. He framed the Academy's advice as administrative in nature so that it seemed like a management issue rather than an ideological matter, thus converting the mandate into a putatively legitimate pathway — that actually consisted of his scientific standards — by which the VA would judge its research. The advice created the machinery for oversight, which is power when the overseer controls the terms of evaluation, which Handler did. His position at the Academy gave him the authority to appoint the committee, choose its chairman, define its remit, and shape the vocabulary in *which quality, priority, scientific, and excellence* would be discussed. Since the Academy could define and set all relevant standards, it could determine what would count as legitimate research inside the VA, and what would not.

STEP THREE was about how that control was made to look non-ideological. It was the presentation step — the way the process was framed so that it appeared to be ordinary good governance rather than a program designed to eliminate a particular target — a class of health risks in the antenna case, a class of research in the VA case.

In both instances — as in many earlier Academy reports — the objective was to make the standards look like common sense. Step three was intended to make the standards and committee process look like what Handler called “just professionalism. “Words like *neutral*,

technical, managerial, merit review, modernization, reform, excellence did the heavy lifting. They made it sound as if the process were merely improving quality, when in fact it was also selecting which kinds of research could survive.

This was where the logic Handler employed when managing the antenna issue was most visible in the context of emergent biomedical research — a form of experimentation Handler scorned. The antenna issue was not chiefly about a scientific debate being settled by evidence. It was about a category being foreclosed by institutional procedure. Handler's logic was that if he could define the evaluative framework — what counted as evidence and mechanism and a legitimate question — then he could control the permissible conclusions without needing to refute specific experiments. That logic was most discernable at the point where the committee process was explicitly portrayed as neutral and professional, because that was exactly how procedural power hides itself. It was the moment where the reader could see that the process was doing the argument's decisive work while claiming it wasn't doing any argumentative work at all. Handler's decision-making system — his entire committee-and-standards pipeline — was intended to project itself as neutral, objective, and expert.

In the VA instance, Handler wanted the reader to believe: he obeyed a neutral congressional mandate as the entry point; the situation was analyzed by means of the Academy's unbiased decisional structure which controlled the committee's scope and questions considered; scientific concepts including *quality, priority, excellence* were used to form judgements. The clearest expression of that procedure was the Academy's biomedical research report's recommendation that the VA mimic the NIH-style external merit review, and the report's use of *quality* as a pivotal decisional term. The committee process was intentionally constructed to ensure that certain outcomes were far more likely than others; the rules were written in a way that favored one kind of claim and disadvantaged another, but the nature and impact of the rules were hidden by a rhetorical mask. The intentional construction of the committee process was to ensure that certain outcomes were far more likely than others. The committee's procedures — the rules of the game — were written in a way that favored one kind of claim and disadvantaged another, but they were hidden by a rhetorical mask. The committee professed neutrality and presented itself as modernizing, improving standards, ensuring rigorous peer review, aligning with NIH best practices — terms that sounded non-ideological, but weren't. Handler presented the procedures as the Academy's normal path for producing responsible advice — its committee conducted a general review of the pertinent scientific literature, deliberated on the evidence and the draft report presented by the Academy's staff, modified its language as appropriate, and laced it with the moral tone of expertise. The objective was to sound inevitable.

Only after the first three steps were in place did the setting come into full view: securing a neutral-looking entry point — capitalizing on the congressional mandate that invited the Academy into VA hospital management and VA research evaluation; converting entry into control — using Academy leadership to control committee composition, scope, and vocabulary, and standards; making the standards look like common sense — framing the whole operation as professional reform and modernization so that the ideological content was not visible as ideology. Once that is understood, the role of the VA specifics comes into full view. Philip Handler was a consultant to the Veterans Administration throughout his career; his contact point was the research service in the VA's Washington, D.C. Central Office. Within the

VA, two research orientations coexisted. The two sections had comparable budgets, and the research they supported was considered by the VA to be of equal quality and importance, at least as early as 1964, the year Robert Becker received the VA's Middleton Award, the highest honor conferred annually by the VA in recognition of outstanding achievement in research related to the VA's mission. Handler had an informal relationship with the head of the section of the research service that supported biochemical studies — who came and went over the years and occasionally asked for his advice — but was never consulted by the head of the research service section that supported emergent biomedical studies. The relationship benefitted both parties: Handler was compensated for his services, and the VA could tell congressmen that any needed advice concerning its research projects was readily available from eminent sources. Handler's influence on the VA research service operated through bureaucratic structures and people, not overt directives, as did his influence on the National Institutes of Health and the National Science Foundation — the nation's major reductionistically-oriented research institutions, and later, the National Academy of Sciences.

From Handler's perspective, research at VA was a paradox. On the one hand, the VA was a treasure trove of patients—a stable, deeply characterized patient population, ideal for clinical trials and biochemical experimentation concerning drug efficacy. On the other hand, it housed research programs shaped by investigators who thought in terms of systems and emergent properties, not just molecules; Becker was the most successful such investigator supported by the VA. Handler objected to emergent studies; his long-term goal was to discipline research at the VA, stripping it of any ideological challenges to biochemical reductionism while continuing to harness its patient base for drug trials and biochemical studies. Over the years, he was forced to tolerate non-reductionist projects in the VA because they were politically popular—it was relatively easy for politicians and the public to see the relation between emergent research projects and their objectives—but he regarded Becker's project as a uniquely dangerous threat. He had serious institutional support within the VA, almost twenty years of funding, many publications in prestigious national and international journals, and was officially recognized by the VA as one of its best scientists. His work on bioelectromagnetism and regeneration directly contradicted the prevailing biochemical dogma — long promoted by Handler — implying electromagnetic energy could organize healing and that cellular behavior might be governed by system-wide dynamics, not just local molecular interactions. In Handler's eyes, Becker's success was intolerable — not just scientifically wrong but also dangerous because it provided a coherent emergent-level alternative to reductionism with institutional stability, visibility, and federal support.

At this point, the analogy to the antenna issue becomes concrete because that's where the same kind of move can be seen being applied to a different target. In the antenna case, the objective was to set up an authoritative evaluative framework so that risk could be treated as illegitimate or unworthy of institutional action. In the emergent-research case, the objective was to set up an authoritative evaluative framework so that emergent research could be treated as low-quality or illegitimate science. The analogy becomes concrete precisely when the reader shifts from the general description of method to the operational goal inside VA — not to abolish regeneration research, not to denounce Becker publicly, but to purify the portfolio of research by adopting standards that will predictably push

emergent work out. That's where the similarity stops being abstract and becomes procedural: it's the same kind of framework-control doing the same kind of foreclosure.

The antenna problem for Handler was not merely that some individuals claimed risk existed. The problem was that a claim of risk, if recognized as legitimate, could force institutional attention, funding shifts, and regulatory consequences. His solution was to shape the institutional environment so that the risk category could not achieve official standing. The same logic appeared in the context of his opposition to the VA's support for emergent biomedical phenomena, but applied to research rather than risk. A simple mapping revealed the parallel between the two cases. When the target category was that electromagnetic energy causes health risks, Handler's problem was that if the category were treated as legitimate, it would force attention, caution, funding, regulation, embarrassment, and policy constraints. His solution logic was not fight each claim forever; instead, control the institutional criteria so that the risk category cannot achieve official standing. The mechanism of his solution was Academy authority and its derivative, committee authority, which framed and controlled vocabulary and produced the end product. When the target category was the legitimacy and importance of emergent bioelectromagnetism and regeneration research, the problem for Handler was that if the category were treated as legitimate, it would become a living alternative to biochemical reductionism. That was an especially dangerous outcome for Handler because it had VA institutional support and appealed to the Congress and the public. Handler's solution logic was not attack Becker's experiments head-on, but rather control the institutional criteria so that emergent work was classified as unacceptable or non-competitive under the new standards. The mechanism of his solution was an Academy report that called for exclusive reliance on reductionistic experimentation, defined quality biomedical research in reductionist terms, and called for external reviewers aligned with that worldview.

In both cases, Handler's decisive move was to control the institutional framework that decided legitimacy so that the target category — risk in one instance and emergent science in the other — became impossible to accept inside the official system, even if it can be observed or argued for outside it. The problem was not that Becker was personally prominent. The problem was that his work, if treated as legitimate high-quality science, made emergent explanations visible and viable inside the federal biomedical system. Handler's solution was not to argue Becker out of existence, but rather to design an environment in which Becker's work could not survive.

STEP FOUR was to separate purification of the VA's research service from the appearance of attack. Handler's long involvement with the VA research meant he did not have to move crudely, which he had done historically when confronted with scientific behavior inconsistent with his ideology. He did not have to denounce Becker by name in public, nor did he have to argue that VA research should be dismantled.

On the contrary, he recognized the VA's unique asset: millions of veterans concentrated in a nationwide hospital system — ideal for clinical trials and drug testing. Only a fool would argue that research at the VA should disappear. Handler's goal was more subtle.

VA research had to be purified — stripped of emergent-level heresies like Becker's, and brought into alignment with the reductionist orthodoxy Handler had already entrenched at other federal research agencies. To accomplish that, Handler needed machinery.

STEP FIVE was to use the Academy to define research quality in a way that decided outcomes without naming specific scientists as targets. In 1977, Handler released the required Academy report on biomedical research in the VA. It allowed him to enter into the VA's internal life wearing a neutral mask — the Academy as helper, advisor, evaluator. As head of the Academy, Handler was in complete control of the report; he appointed the committee, chose its chairman, and had the first and last word on the intellectual vocabulary in which *quality*, *priority*, and *excellence* were defined.

The Academy machinery described in the report worked in stages. First, it wrapped the VA research program in the language of reform and modernization. It praised the idea of intramural research, extolled the VA's unique patient base, and affirmed that a pluralistic system of funding ought to be preserved. On its face, that sounded like a defense of variety. In practice, *pluralism* meant research conducted in more than one federal institution but governed everywhere by the same reductionistic criteria.

Then came the key recommendation — the VA's research portfolio needed rigorous merit review by external experts and evaluation according to the standards Handler had entrenched at the National Institutes of Health and the National Science Foundation years earlier. A sizeable fraction of existing projects funded by the VA were said to be of “unacceptable quality.” The statement would have been banal if *quality* were neutral, but it wasn't. In Handler's world, it meant adherence to molecular causation, mechanistic chains, and the orthodox hierarchy of explanation.

Whatever could not be made to fit that template was, by definition, inferior science.

This was Handler's way of precluding an outcome, just as he had done in the antenna issue. In that case, certain observations could exist and still be treated as irrelevant, because the authorized system could deny them interpretive standing. In the context of emergent experimentation, certain research programs could exist and still be treated as what Handler called “low quality,” because the authorized system defines quality in a way that excludes them.

STEP SIX in Handler's evaluation technique was to route elimination through “external review,” so the act looks impersonal and therefore legitimate. Marguerite Hays now had what she needed. When Becker's present VA grant came up for renewal, his proposal need only be sent out to reviewers selected, directly or indirectly, from Handler's circle — reviewers who regarded bioelectromagnetism as fantasy, regeneration as a curiosity to be explained away, emergent field theories as quasi-mystical. It would not matter how many papers Becker had published or what fame and recognition he had achieved. Under the evaluative language imported by the Academy report, his work would simply not count as high-quality science. The reviews were certain to be harsh, dismissive, and even contemptuous. Hays could point to them and argue that this was not personal; it was the Academy's standard, NIH's norm, the new meritocratic regime for VA research. The system could look rigorous—advisory councils, external reviewers, an explicit commitment to excellence—and still remove the VA's internal challenge to the molecular worldview. That, from Handler's standpoint, was the point. The Academy report did not say “Becker must go.” It did not mention bioelectromagnetism, emergent physiology, or limb regeneration. It did something more effective: it redrew the boundaries of legitimate inquiry inside the VA so that Becker's work—and anything like it—

could not survive. The political machinery that would destroy him was quiet, procedural, and couched in the language of improvement.

STEP SEVEN was enforcement—public-facing and internal—when the technical machinery encounters political resistance. Handler could not be certain that the biomedical report would eliminate all of the emergent studies the VA supported, or that Becker’s program would be terminated. There was high-level evidence that Handler’s goal might not be achieved. VA Administrator Max Cleland — who lost three of his four limbs in Vietnam — told several senators and congressmen that the VA was conducting studies on the possibility of limb regeneration and explicitly pointed to Becker’s research, urging them to contact Becker for further information. Political interest in regeneration increased the VA’s research budget and led the VA to adopt regeneration research as one of its research priorities. Handler responded to those unforeseen developments with what amounted to a plan to blackmail the VA to follow the research plan described and recommended in the Academy’s biomedical report.

In the mandated Academy’s medical program report that dealt with how the VA managed its hospitals, doctors, nurses, and medical services, the Academy addressed the question whether the Veterans Administration should be closed, and answered it in what seems to be the affirmative. It did so using highly dense text that arguably could support other interpretations. But the Academy’s press release that accompanied the public release of the medical program report explicitly suggested the Academy committee recommended closing the VA. That was a political impossibility, which the calculating Handler surely knew and the resulting nationwide uproar quickly showed.

Ultimately, Handler officially decried any interpretation of the Academy report that suggested the VA should close; Administrator Cleland agreed the VA would adhere to the research criteria outlined in the biomedical report; the VA remained intact as a clinical institution but a seriously compromised as a research institution. Handler got what he wanted — control of the VA’s research — and Cleland got what he wanted — a financially stable institution that served the clinical needs of veterans.

By the 1970s, Robert Becker had gathered a persuasive body of experimental evidence supporting a new understanding of regenerative biology. In a series of studies anchored in bioelectric measurement, surgical intervention, application of man-made electromagnetic energy, and physiological observation, Becker demonstrated that electrical signals at injury sites played a pivotal role in initiating and guiding regenerative growth. The signals were measured, manipulated, and in some cases, used to stimulate regeneration in mammals previously believed incapable of such feats.

Becker's core discovery was that electrical phenomena were not incidental by-products of injury, but informatory forces arising from the organism's nervous system. He boldly conceptualized that an analog control system, electrical in nature, was superimposed upon and evolutionarily predated the familiar digital nervous system of impulses and neurotransmitters. Unlike the rapid-fire digital signals of the central nervous system, this analog network operated slowly, continuously, and locally, electrically shaping the behavior of cells. In salamanders, the signals orchestrated full limb regeneration. In mammals, Becker found that reintroducing or

amplifying them could partially restore regenerative capacity by mimicking the ancient physiological language long silenced in higher vertebrates; his observations were confirmed by others who replicated his results.

Becker's conception of regeneration was sweeping. He did not confine his interest to the restoration of amputated limbs, but rather extended his experimental approach to other tissues. For Becker, regeneration encompassed the body's potential ability to replace lost tissue with histologically and functionally identical tissue.

Bone healing, for instance, was not merely a repair process — it was a regenerative phenomenon deeply related to electromagnetic signaling. The success achieved by physician-scientists, including Becker, in electrical stimulation of bone regeneration resulted in a major clinical break-through in the 1970s in the treatment of bone diseases. Becker demonstrated that the bioelectric characteristics of fracture healing mirrored those of limb regeneration in amphibians. The mass of undifferentiated cells central to limb regrowth which formed and functioned under the influence of electrical signaling at the injury site was similar to the cell mass that formed in fractured bone, suggesting to Becker that electrical signaling was the key factor in regeneration throughout the animal kingdom.

Becker extended his regeneration inquiry to the spinal cord — a subject of enormous significance to the patient population served by the VA, many of whom suffered debilitating spinal injuries. He undertook preliminary experiments in spinal-cord regeneration, again using salamanders as his model system because they were naturally able to restore motor and sensory function after surgical spinal transection, and began uncovering the bioelectric underpinnings of this remarkable capacity. His objective, grounded in laboratory data, was to identify bioelectric triggers that could someday be leveraged in humans to treat spinal trauma. Even more provocative were Becker's forays into organ regeneration. He discovered that when half of a salamander's heart was surgically removed, the injury healed and the missing portion was reformed by tissue structurally and functionally indistinguishable from the original — true regeneration. In follow-up experiments he studied the role of electrical signaling in heart morphogenesis and the mechanisms responsible for cardiac regrowth.

Historically, novel biomedical research could be contemplated and pursued within the VA because it encouraged and promoted research by physician-scientists that was foreseeably applicable to human disease. Becker's regeneration research was possible because the VA was the only major source of financial support in the United States for direct studies of emergent biomedical phenomena, which was exactly the reason, two decades earlier, he went to work at the VA hospital in Syracuse the same month he completed his residency training in orthopedic surgery ². The National Institutes of Health (NIH) and the National Science Foundation (NSF) were not plausible alternatives, for Becker's promising avenue of regeneration. Both institutions automatically rejected research proposals that did not conform to two conditions precedent — presentation of what they called "preliminary data," a gimmick that required the grant applicant to do the proposed experiment prior to receiving funding, and conformation to reductionistic molecular orthodoxy — a condition for funding first elaborated when Handler was a leader of the institutions. Working for the VA, Becker could produce preliminary data because

² Becker the Researcher. Andrew A. Marino. Cassandra Publishing, 2017.

he had a staff of scientists and technicians, a fully functional research laboratory, and needed no specific permission from the VA's Central Office to conduct initial studies of tissue regeneration —thus, his innovative ideas were not still-born. Not only was Becker free to criticize and ignore the NIH and NSF requirement of molecular orthodoxy, he was funded by the VA specifically to perform research that was not based on their orthodoxy. In a speech, he emphasized the importance of allowing such opportunities. He said, "While no one can dispute the value of research that explains what was previously unexplainable, research that renders unexplainable what was previously satisfactorily explained is equally valuable, and we must always have a place for destruction of cherished dogma."

Philip Handler was an extreme, unapologetic reductionist who believed all biomedical phenomena were ultimately explicable in terms of the molecular chemistry of DNA, proteins, and enzymes. In his eyes, they were the only acceptable factors for explaining life and all other biophenomena. Concepts like bioelectric signals or cybernetic control networks struck him not as ideas to be tested, but as threats to be neutralized. He did not seek to understand the evidence Becker produced; oppositely, he worked to make it vanish and avoided engaging with the published data or experimental protocols in Becker's regeneration research or that of other scientists working in the area. Handler turned away on ideological grounds and worked to suppress Becker's findings — not by scientific rebuttal, but by institutional erasure.

Handler was an authoritative figure in the biomedical research establishment who parlayed his stature into successfully establishing reductionism — the antithesis of Becker's research — as the norm in American biomedical research. In 1976, Handler repeatedly punished Becker for his obdurate adherence to system-level research projects, which were the opposite of Handler's preferred molecular-level design.

He orchestrated the cancellation of Becker's VA Medical Investigatorship, which effectively slowed his research progress because it increased his clinical duties. This move followed Handler's success in securing cancellation of Becker's NIH training grant for teaching residents and medical students how to perform biomedical research, and cancellation of his NIH a grant to study the scientific basis of acupuncture — a field viewed by Handler with even more disdain than regeneration. This limited foray of twelve grants nationally by NIH into research involving emergent biomedical phenomena had occurred in the face of Handler's opposition only because it was expressly ordered by President Richard Nixon. All three moves hurt Becker, and the messages he received from various sources, especially his contacts in Central Office, suggested Handler had no intention at stopping his anti-Becker campaign until his laboratory was closed. Handler had been hostile to the idea of physician-scientists ever since the early days of his career when his idea to lengthen medical school by two years for intensive training in biochemistry was soundly rejected by the dean of the medical school where he worked. Thereafter, Handler's enmity toward physicians both as medical doctors and scientists was palpable; it led to his choice to self-treat his many medical problems and his view of Becker as a black beast.

Handler's role in reducing Becker's VA support to work in his laboratory was not the result of scientific review but rather Handler's sabotage, carried out through his backchannel influence within the VA's research service — connections that were strengthened following the Academy's evaluation of the VA's biomedical research program. Becker was acutely aware that

Handler's influence at the VA was increased following the Academy's biomedical report, which required the VA's research service to adopt the grant-review standards and policies of the NIH and the NSF. Together with Handler's deep resentment regarding Becker's research in the area of health risks due to anthropogenic electromagnetic energy, and his characterization of the Academy's Sanguine committee as a stacked deck, the implications for his career were ominous.

In mid-1976, Becker was asked by an editor of *The Saturday Evening Post* to write an article for the *Post*. His plan for the article was approved by the *Post* and Becker provided a manuscript, *The Failure of Scientific Medicine*. The *Post* editors wrote they were "excited" by the "precision of your argument and the unarguable logic of your approach." They described it: "Never lifeless, never pompous or overly pedantic, it provided a new and intriguing perspective in a most thought-provoking manner."

In the manuscript (see Appendix 1), Becker wrote that modern biomedical research has achieved enormous technological power but has lost understanding of life, and thus the so-called scientific medicine of the post-1950 era is failing to address degenerative disease. Medicine has become mechanical, treating the body as a machine rather than a living system, and the research establishment has become authoritarian through peer-review and funding control. The NIH-funded research apparatus suppresses ideas that challenge prevailing dogma and mechanistic biomedicine — life as biochemistry and molecular machinery — which has become entrenched, even though living organisms possess properties that cannot be explained by molecular mechanisms alone.

The most important of these properties is integrated control of growth and healing. Amphibians — especially salamanders — possess extraordinary regenerative ability that mammals lost during the evolutionary process. This regenerative ability is clearly linked to the nervous system, but not to nerve impulses. There exists a primitive, organism-wide data transmission system distinct from the nerve-impulse system that operates through electrical properties of organized biological material.

Bone healing and tissue regeneration are controlled by this electrical system.

Cancer and arthritis represent failures of growth control, not merely chemical dysfunction. Understanding regeneration in amphibians is the proper path to understanding human disease. However, the research establishment refuses to pursue this path because it contradicts molecular dogma, which is repeating the authoritarian errors of pre-scientific medicine. Peer review now enforces conformity rather than encouraging discovery and biomedical research that incorporates principles from solid-state physics, electrical organization, and cybernetics. Life negates entropy through integrated control systems not yet understood, or even seriously studied. The uniqueness of life lies in this control and communication system, not in chemistry alone. True progress requires open-minded investigation of these non-molecular control systems.

Electrical stimulation experiments already show partial regeneration in mammals. Medicine's future lies in restoring growth control, not replacing body parts with prosthetics or machines. Understanding control systems could transform treatment of cancer, arthritis, spinal injury, and organ failure. The current biomedical establishment, however, actively prevents such

exploration. A revolution in biomedical thinking is required, not more application of 1950s mechanistic concepts.

The Post's Executive Editor, realizing Becker was calling for a restructuring of peer review and research priorities, and arguing for the study of life across the evolutionary spectrum, not just at the molecular level — asked Philip Handler for an evaluation of the suitability of Becker's article for publication. Following a highly negative review by Handler, the article was never published.

Becker's research had already placed him in quiet opposition to powerful currents within American biomedicine. What he was articulating went beyond experimental anomaly and into the realm of first principles: a challenge to the very framework through which modern medicine understood life. His work had already forced a confrontation that was deeper than disagreement over data. His experiments on electrical control of healing and regeneration did more than introduce an unfamiliar physiological mechanism; they exposed a widening gap between what living systems demonstrably do and what the governing vocabulary of modern biomedicine allowed investigators to acknowledge. That vocabulary had not arisen accidentally. It had been shaped, defended, and institutionalized by partisans, especially Philip Handler, who functioned as a representative voice for NIH-style peer review and, through his leadership of the Academy, as a guardian of what counted as legitimate biomedical explanation.

Handler's achievement was to equate scientific rigor with biochemical reductionism. Under this settlement, a biological phenomenon was considered real only insofar as it could be translated into molecular terms. The authority of science rested on maintaining that discipline through funding, publication, and peer review. What could not be made to fit this framework was not merely unproven; it was treated as unscientific in principle. This was the Objective-facts doctrine in practice — the conviction that biological truth was already bounded by the molecular vocabulary that defined it.

Becker's interpretation of regeneration stood in direct violation of that boundary. He argued that the central clinical problems of the age—cancer, arthritis, failure of repair, stress-related disease — were not fundamentally chemical malfunctions but failures of physiological control. That control, he maintained, operated at an integrated, organism-wide level by means of the electrical properties of living tissue and could not be reduced to chemistry alone. The implication was not simply that a new mechanism had been found, but that the reigning explanatory hierarchy had misplaced its emphasis. Molecular reactions, in Becker's view, were subordinate expressions of a higher level of biological organization that governed growth, form, repair, and pattern. To accept such a proposition would have required acknowledging that the molecular monopoly Handler had worked to entrench was insufficient to describe the most important functions of living systems. Becker's thesis therefore did not merely add to the research agenda; it rearranged the conceptual order on which that agenda depended. It suggested that the most fruitful path toward understanding disease lay not in deeper molecular penetration or technological substitution of failing parts, but in restoring the organism's intrinsic regulatory capacity.

Where Handler's vision of progress favored mechanical intervention and biochemical manipulation, Becker's favored recovery of the body's own governance.

This difference was not a matter of emphasis but of worldview. Handler's model treated conformity to reductionist explanation as the essence of scientific seriousness, and used institutional authority to enforce that standard. Becker's model treated empirical evidence of integrated control as a signal that the standard itself was too narrow. In his view, science required expansion of its explanatory language to match biological reality. In Handler's view, science required protection of its language from precisely such expansion, because expansion threatened to dissolve the boundary between disciplined explanation and speculation.

From Handler's standpoint, Becker's position was an attack on scientific legitimacy. A biology not anchored in molecular explanation seemed to him indistinguishable from heterodoxy. From Becker's standpoint, the insistence that life be interpreted exclusively through chemistry represented a form of sanctioned blindness that diverted medicine away from the very phenomena most relevant to healing. The conflict was therefore not about whether Becker's observations were accurate, but about whether the framework that defined acceptable science could accommodate what those observations implied. Handler's denunciation of Becker's work followed naturally from this clash. Becker had crossed from presenting anomalous findings to challenging the interpretive sovereignty of biochemical reductionism itself. By asserting that growth control and regeneration depended upon an electrical level of organization beyond molecular chemistry, he implicitly questioned the adequacy of the intellectual order Handler defended.

Handler's response was not analytical refutation but categorical dismissal — an assertion that such thinking lay outside the bounds of science. In doing so, he exercised the authority of paradigm over evidence, using the credibility of his position to prevent Becker's argument from receiving a fair hearing. This episode revealed the nature of Handler's role in the broader suppression of regeneration research. Acting as a defender of an ideologically certain understanding of what biomedical science ought to be, he used the power associated with NIH-style peer review and Academy leadership to enforce the molecular monopoly. Becker, lacking comparable institutional voice, could not compel engagement on equal terms. The decisive act was Handler's: an assertion that the boundaries of science were his to define, and that Becker's challenge to biochemical reductionism must be silenced rather than examined.

By early 1978, the Veterans Administration was under sustained pressure to reconsider both the direction and intellectual premises of its research program. The VA had accepted the recommendations in the National Academy of Sciences' report on biomedical research, which was framed as a corrective and urged conformation to NIH-style standards of peer review and scientific evaluation. Yet in Congress, among veterans' advocates, and within parts of the VA leadership itself, there was a growing sense that the VA's research program had produced few tangible clinical gains. The VA's Rosalyn Yalow won a Noble Prize in Medicine for co-developing a biochemical technique using radioactive isotopes to measure minute concentrations of substances in the blood and used it to prove diabetes was caused by the body's inefficient use of insulin, but the VA's research program was stagnated in areas where veterans' needs were greatest. Senator Alan Cranston, Chairman of the Senate Veterans' Affairs Committee, was among those who had reached this conclusion. Decades of biochemical research within the VA and the NIH had yielded great detail about molecular and cellular aspects of the injuries

suffered by veterans, but left amputees and spinal-injury patients with little more than prostheses and no hope of tissue restoration because relevant biochemical experimentation had never even been undertaken.

Cranston's interest in limb and spinal-cord regeneration reflected not a fascination with speculative science, but a pragmatic dissatisfaction with the prevailing system of biomedical research. After encountering Becker's regeneration studies, Cranston became convinced the traditional methods of research in areas relevant to the problems of veterans were insufficient, and that progress required not better execution of the same paradigm but willingness to explore approaches that lay outside it. This dissatisfaction was shared — from a different vantage point — by VA Administrator Max Cleland, whose own experience as a triple amputee due to injuries in Vietnam had sharpened his awareness of the limits of prosthetic devices and symptomatic management. He was receptive to the idea that regeneration research — however unconventional — might represent a path beyond the clinical plateau veterans' medicine had reached. Encouraged by Cranston and reinforced by interest from other political figures and private advocacy groups, Cleland authorized the VA to convene a conference to examine regeneration as a legitimate scientific and clinical domain. The decision to hold a regeneration conference thus reflected a provisional judgment that NIH-style research, for all its rigor, had not delivered proportionate benefit in certain areas of human injury and repair, and that alternative approaches deserved serious consideration.

From the outset, this challenge to the orthodox biomedical research blessed by Handler was met with arguments couched in the language of responsibility. Handler and his supporters maintained that research must be evaluated solely by peer scientists, must involve multiple biochemical sub-specialties, and must conform to established standards of scientific rigor, which they argued were neutral principles that served as safeguards against error and fraudulent claims. However, they did not mention that by *peers* they meant investigators trained within and committed to a reductionist molecular framework, and by *biochemical sub-specialties* they meant investigators who shared methodological assumptions. The competence, experience, reasoning, and empirical record of individual investigators mattered less than their alignment with an accepted explanatory style.

The conference, which took place in mid-1978 and addressed limb and spinal-cord regeneration, brought together VA and NIH investigators and congressional staff. Becker was given a prominent role, reflecting both his scientific standing and the political interest surrounding his work. In his presentation, Becker reviewed decades of experimental evidence — his and others — that demonstrated induced regeneration in mammals; he emphasized bioelectric control systems, arguing that limb and neural regeneration were mechanistically related not fundamentally distinct processes. Other presenters acknowledged the importance of nerve supply, whole-organism context, and non-molecular signaling in regenerative phenomena. The subsequent general discussion among the conferees revealed disagreement over interpretation and implication of the experimental data, but not over whether the regenerative phenomena themselves existed. The conferees agreed Becker's experimental results were valid and at least partially substantiated, but the discussion exposed a deeper problem: existing evaluative frameworks were ill-equipped to assess phenomena that operated at the level of system-wide regulation rather than molecular interaction. At this point,

as if on cue from Handler, Marguerite Hays, a physician-administrator within the VA research service and Handler's main contact point therein, asserted her administrative authority.

Hays was responsible for managing the conference, but was not a researcher and had no independent experience in regeneration science. Her authority was bureaucratic but, within the VA bureaucratic structure, her authority extended to interpretation. Hays was closely aligned with the NAS report's recommendations and with the broader effort —long championed by Handler—to bring all federally funded biomedical research into conformity with NIH standards. Those standards, while presented as methodological safeguards, had the practical effect of blocking direct study of emergent biomedical phenomena, and of excluding system-level inquiries from sustained financial support. In her remarks, Hays dismissed the conference's scientific significance with the assertion that there was “no evidence that anything more is known now than before.” Her speech was highly critical of Becker's research, and echoed a familiar rhetorical stance long used by Handler to minimize work that challenged molecular orthodoxy without directly engaging its empirical claims.

Essentially functioning on his behalf, Hays did not simply express skepticism and disdain, she attempted to set boundaries beyond which Becker's research should not proceed.

Becker was not permitted to respond at the conference, but did so in a post-conference letter to Hays' superior that revealed the deeper significance of Hays' intervention. Becker conveyed his perceptions of the meeting, explained his core position on regeneration research, and formally objected to the posture adopted by Hays. He framed regeneration as a mature, empirically advancing field whose credibility had strengthened over decades, culminating in demonstrations that regenerative capacity can be restored in animals, including mammals, ordinarily lacking it. Becker underscored that the meeting's scientific value laid precisely in acknowledging the accumulation of such evidence and in recognizing that the question is no longer whether the phenomenon is real, but how it can be deliberately elicited and understood. He rejected what he perceived as a recurrent theme during the meeting: the claim that neural regeneration should be treated as fundamentally different from limb regeneration. Becker said he saw “no dichotomy,” and that there was no evidence for such a division; he defended the opposite hypothesis — that the same underlying mechanisms may govern both limb and neural regeneration, and that the proper way to resolve the question is empirical: “The only way is to try it.” He treated regeneration as a generalizable clinical principle with system-wide relevance.

Becker pivoted from science to institutional design, arguing that regeneration research was under-supported and “fragmented” across disciplines, making it inherently difficult to fund and coordinate, and insisting that the VA was its proper institutional home. He said clinical applications were developing and that one application, bone growth stimulation, was already “here.” He contended that an agency with a clinical mission should “support, encourage and direct” the program, and identified the VA as “ideal.” Becker emphasized the modest cost, minimal risk, and the disproportionate potential benefit of regeneration research. He noted that the conference produced multiple suggestions for program mechanics that “all merit consideration,” and that he regarded the conference as capable — if properly appreciated and understood —of generating actionable next steps.

The center of Becker's letter was his reaction to Hays' closing remarks. He called them “incredible” and stated explicitly that he hoped they did not reflect a preconceived official position. Becker carefully but clearly characterized her message as a

sweeping denunciation of his work and the work of his colleagues as trivial, erroneous, fabricated, and “obviously lacking in significance. “He objected not only to the substance of the critique but to its procedure and forum. He contended “it would have been proper” for Hays to state her opinion in a manner where it could be answered rather than to deliver it “in silence.” The gist of Becker’s accusation was that Hays’ remarks effectively were a kind of administrative pronouncement rather than a scientific argument, presented in a manner that foreclosed the reciprocal give-and-take that constitutes legitimate evaluation. He added that the very fact the meeting was convened indicated regeneration was regarded, “in some minds at least,” as significant and worthy of serious consideration, thus underscoring his view that Hays’ dismissiveness was discordant with the conference’s convening rationale.

In closing, Becker pointed to his scientific record, “over 110 publications” during the past twenty years,” and then delivered the letter’s most revealing institutional judgment. It is “discouraging,” he wrote, that administrative staff within his own organization “still prefers to blindly adhere to dogma rather than be receptive to new ideas and evidence.” Yet he ended by expressing appreciation for the opportunity to present his research, and for the “balanced, fair and unbiased” manner the conference was designed and managed by the head of the research service, thus treating Hays’ remarks as a form of administrative disparagement inconsistent with open scientific evaluation.

The conference showed that Becker’s work had become difficult to ignore. Scientifically, it had accumulated a substantial empirical record. Politically, it had attracted congressional interest and VA support. Publicly, it had begun to reshape how regeneration was discussed beyond the laboratory. Each of these developments carried implications that extended well beyond Becker himself, and posed a continuing problem for Handler Together, they challenged the sufficiency of molecular explanation, threatened to reopen questions about system-level regulation, and intersected uncomfortably with Becker’s parallel work on electromagnetic fields — research that suggested anthropogenic electromagnetic energy could interact with the body’s endogenous control systems for growth and healing. That evidence stood in quiet but direct contradiction to Handler’s repeated assurances such energy was biomedically harmless. In Handler’s eyes, the stakes were scientific, institutional and reputational. He had spent decades consolidating a model of biomedical research that privileged molecular reductionism and marginalized direct studies of emergent biomedical phenomena. He had shaped the NIH and the NSF into the dominant arbiters of biomedical legitimacy, and the VA, at least before the Academy biomedical research report, represented one of the few remaining federal spaces where alternative approaches might take root. The conference — and the political support that made it possible — signaled that the VA might not fully bow to the NAS report’s demand for reliance on NIH criteria to evaluate proposed biomedical research.

Compounding this was a more personal affront — Becker said on national television the Academy’s Sanguine Committee Handler appointed by Handler was a stacked deck. The remark damaged Handler not because it was inflammatory but because it was plausible. It exposed, in plain language, the alignment between committee composition and predetermined outcome.

The regeneration conference made the outline of a looming conflict visible.

Political authority had opened the door to scientific exploration; administrative authority had moved to close it again. Becker's work had gained momentum at precisely the moment when its implications became highly uncomfortable for Handler and those who had supported his definition of the boundaries of acceptable inquiry.

What followed did not begin within the VA, but rather unfolded in public.

Several weeks after the VA conference considered whether regeneration research belonged on a biomedical research agenda, that question was abruptly thrust into general view. *Saturday Review* magazine published a feature-length article that framed regeneration as a coherent, empirically grounded scientific program centered on the work of Robert Becker. The article avoided technical biomedical concepts and did not rest its case on the authority of experts. Instead, it presented regeneration as a long-observed biological capacity — well documented in nature, partially elicited in laboratories, and already producing tangible results in clinical settings. Regeneration was regarded as a latent biological process that modern biomedical research had neglected rather than disproved.

The heart of the article was a clear logical progression. First, it established regeneration as a real and orderly phenomenon in animals, emphasizing that lost structures were not merely replaced by scar tissue, but rebuilt with correct form and orientation. Second, it identified a persistent explanatory problem: how cells know *what* to rebuild and *where*. Rather than resolving that problem by appeal to molecular detail, the article introduced Becker's central insight — that regeneration appears to be governed by organism-level control systems, specifically electrical ones, that coordinate growth across tissues. Crucially, the article did not present Becker as a theorist in search of evidence. He appeared instead as an empirical investigator who moved repeatedly between observation and intervention. Readers were shown that Becker's ideas were tested not only in animals, but in practical medical contexts — most notably in the healing of previously non-healing human bone fractures through controlled electrical stimulation. These cases functioned rhetorically as anchors: regeneration was not merely promised; it had already occurred, in modest but unmistakable form.

The article's reasoning unfolded as a kind of detective story in the sense that it guided the reader step-by-step through a process of inference. Seemingly disparate observations—severity of injury, nerve supply, electrical changes at wound sites—were treated as clues. Each step built toward a unifying inference: that injury generates electrical signals which, under the right conditions, do not merely accompany healing but direct it. The article emphasized that these signals operate at extremely low intensities, suggesting a regulatory role rather than a blunt mechanical one.

Regeneration, in this telling, depended less on chemical force than on informational control. What made the article particularly powerful for a general audience was its restraint. Becker himself was portrayed as cautious, even conservative, about timelines and applications. Full limb regeneration in humans was framed as a distant goal, while nearer-term possibilities—such as spinal cord repair or improved orthopedic reconstruction—were presented as plausible extensions of work already done.

The primary obstacle identified was not conceptual failure, but lack of institutional support and funding. This narrative logic produced an implicit but unmistakable contrast. On one side stood an investigator who struggled continuously for modest resources, who published

empirical results, and whose claims rose or fell with observable outcomes. On the other side — never named, but clearly implied — stood a dominant biomedical framework that promised revolutionary results through molecular mastery, yet repeatedly deferred their realization to an unspecified future.

The article did not attack that framework directly; it simply made its limitations visible by juxtaposition.

Equally significant was what the article suggested without asserting. By treating electrical control and regulation as a fundamental organizing principle of growth and healing, it opened the possibility that external electromagnetic influences might interact with the body in ways medicine had underestimated. These implications were handled cautiously, framed as questions rather than conclusions, but they were unmistakable. The body was presented not as a chemically closed system, but as one sensitive to informational and environmental cues — a view at odds with assurances that such influences were biologically inconsequential. In this way, the article accomplished what the VA conference could not. It translated a contested scientific program into a coherent public narrative, one that emphasized results over rhetoric and evidence over authority. Becker emerged not as a dissenter seeking attention, but as a methodical investigator whose work exposed gaps in prevailing explanations.

The article did not call for revolution; it simply asked why a line of research that worked should be marginalized by institutions that answered to no comparable standard of demonstrable success.

By the time the article circulated nationally, regeneration research was no longer an internal administrative problem. It had become a public challenge to biomedical orthodoxy — one framed in plain language, grounded in observation, and difficult to dismiss without appearing to dismiss results themselves. What followed revealed how such challenges were answered when they could no longer be managed quietly.

The logic of the *Saturday Review* article quietly subverted Handler's reductionistic framework, which privileged conformity to molecular standards when evaluating the need for or the worth of research, and demanded reliance on the abstract notion of peer review to determine what research was respectable. The article, in contrast, privileged empirical results, coherent logical explanations, and beneficial outcomes. Where Handler treated system-level biophenomena as methodologically suspect unless reducible to biochemical components, the article treated them as legitimate objects of inquiry precisely because they organized observable biological behavior. And where Handler implicitly assumed biomedical progress flowed downward — from genes to molecules to tissues — the article reversed the explanatory direction, presenting regulation of healing as an emergent process that orchestrates molecular events rather than waiting to be explained by them.

Within the context of biomedical governance, the article did not merely popularize regeneration research; it rendered visible the philosophical commitments underlying competing models of biomedical science. In other words, the article did not just popularize regeneration research; it highlighted the philosophical commitments underlying competing models of biomedical science. In doing so, it exposed how Handler's ideology had come to substitute methodological preference for demonstrated effectiveness. The public visibility altered the terms of the dispute.

What had previously been managed through reports, committees, and administrative judgment was now framed as a matter of credibility before a national audience. The article's favorable reception ensured that regeneration research — and Becker himself—could no longer be dismissed quietly or procedurally, and must be dealt with in a more direct fashion. It was at this point that Handler's attitude toward Becker reached its most bitter level — up to that time; his strategy shifted from containment to counterattack. Deprived of the ability to marginalize Becker's work through internal mechanisms alone, Handler committed to defending the prevailing biomedical order by means of discrediting Becker more aggressively than previously.

HANDLER'S INITIAL EXPLICIT CENSURE of Becker's approach to biomedical research occurred in the context of the Sanguine issue, and was delivered not through formal scientific rebuttal or institutional review, but through public accusation under the seal of the Academy. His second attack was delivered covertly when he blocked publication of a magazine article that described the clinical consequences of blindly adhering to biochemical reductionism as the sole basis of medical research. His third attack was delivered surreptitiously during the VA's regeneration conference with the help of Marguerite Hays, his VA contact, who leveled "incredible" criticism against Becker's research when she addressed the conferees. Employing language routinely used by Handler when berating an investigator whom he considered to be a danger to science, Hays denounced Becker not based on evidence or analysis, but rather on her administrative authority. Handler's fourth attack on Becker came shortly after the *Saturday Review* article was published. It was not a formal scientific rebuttal or institutional review but rather an arranged increase in intensity of disparaging remarks by Lionel Jaffe who, historically, Handler used as stalking horse to undercut the credibility of Becker's regeneration research.

Jaffe's untoward remarks about Becker's research began at a 1973 conference on electrically mediated growth mechanisms in living systems (see Chapter 4), during which Jaffe also leveled criticism at similar research of other physician-scientists. Subsequently, Jaffe and co-workers in his laboratory at Purdue University, writing in scientific journals, systematically made false and misleading comments about Becker's regeneration studies, and that of others. Becker ignored the criticism, judging it valueless and based only on misplaced respect for Handler's ideology that Jaffe expressed at the conference, which Becker rejected because it conflicted with empirical evidence. Becker's regeneration and electromagnetic-energy research was pregnant with the implication that Handler had based his career on a fatally flawed model of how living systems worked. But what Handler saw was Becker weakening the scientific endeavor and helping to cause the monotonic nosedive in the public's esteem for science that was occurring nationally. Handler's goal was to elevate both science and recognition of the National Academy of Sciences as a national authority on a broad range of social issues, and he regarded Becker as a major obstacle who had to be silenced.

In the early 1970s, Lionel Jaffe was essentially the only non-industry scientist in the U.S. who shared Handler's disapproval of directly studying emergent biomedical phenomena in the context of electromagnetic signaling; Jaffe believed they should be studied at the atomic and molecular level, employing the measuring device he invented. He was rewarded by Handler, who used his influence to help Jaffe secure federal grants to build the device and fund what he called a "National Laboratory" where interested investigators would come and use the device. Few did, and Jaffe used the device mainly to measure electromagnetic signals in the vicinity of

the eggs of seaweeds. The quid pro quo for Handler's largess was Jaffe's willingness to systematically level strategic, not merely rhetorical, claims that denigrated Becker's regeneration research, advancing Handler's agenda while shielding him from direct exposure as the benefiting party. Handler thus opposed Becker's work indirectly by deploying Jaffe as the visible critic while remaining in the background, positioned as if he were a neutral arbiter of scientific standards.

Handler's instrumental use of Jaffe began at the 1973 conference, where he was essentially the only advocate of reductionistic experimentation to further understanding of electrical regulatory systems. Jaffe continued to criticize Becker's research as wrong-headed and lacking scientific merit — a conclusion, however erroneous, Becker recognized Jaffe was quite entitled to. In subsequent presentations and publications, however, Jaffe moved beyond good-faith scientific discourse; his comments became progressively harsher and more reckless. In a 1977 publication he suggested Becker had falsified the results of an experiment. He said it was "truly astonishing" that partial limb regeneration in rats could be caused by electrical stimulation, and that he believed "these claims very difficult to reconcile with the established information about regeneration." In a paper in the Proceedings of the National Academy of Science [1977](#), Jaffe dismissed Becker's work as specious because he hadn't used the measuring device in Jaffe's so-called National Laboratory. Following the regeneration article in the *Saturday Review*, however, Jaffe's sporadic comments metastasized into a full-blown effort designed to reframe Becker's empirical challenge to reductionism as scientific misconduct.

The *Saturday Review* article did something that had previously not been accomplished in any forum: it rendered Becker's regeneration research intelligible, coherent, and empirically grounded for a general audience without trivializing its scientific content. By proceeding inductively — assembling observations, experiments, and reasonable inferences—the article allowed readers to see regeneration not as speculative fantasy, but as a legitimate biomedical phenomenon whose explanatory framework was at the level of system-wide regulation rather than isolated molecular events. The achievement upset Handler, who was invested in enforcing reductionist orthodoxy, but could not be undone by means he directly controlled. What followed was an attempt to characterize the article as untrustworthy and secure a retraction. The instrument chosen for the task was a letter to *Saturday Review* editor Carll Tucker, signed by Joseph Venable, a junior member of Lionel Jaffe research staff at Purdue University but crafted within Jaffe's laboratory.

The letter was designed to ape responsible scientific skepticism while functioning, in effect, as a prosecutorial brief against Becker's credibility. In its opening move, the letter juxtaposed the regeneration article with an earlier *Saturday Review* discussion of pseudoscience, implicitly placing Becker's work within that category by proximity alone. By associating regeneration with pseudoscience at the threshold, Venable shifted the burden of proof, requiring Becker to establish legitimacy rather than requiring critics to demonstrate error. The classification preceded the evidence.

From there, the letter moved to Becker's photographic documentation, not to demonstrate falsification, but to insinuate visual deception. The frog images were said to "suggest" a regenerative sequence that might mislead the reader. No fraud was alleged, no experimental inconsistency demonstrated. Instead, implication itself became the charge. The

tactic was reputational: if the presentation could be made to seem misleading, Becker's authority could be undermined even while the experiments themselves remained intact.

The same pattern governed Venable's treatment of regeneration speed. Becker's reports of rapid regenerative response in mammals were not shown to be impossible or contradicted by data. They were declared "implausible in the extreme" by comparison to expectations derived from other organisms. Novelty was thus transformed into excess. The accusation relied not on falsification, but on incredulity — a rhetorical substitution of expectation for evidence.

The most consequential maneuver appeared in the letter's discussion of experimental controls. Here, Venable retroactively imposed an idealized standard of causation — one demanding molecular isolation and narrowly defined control conditions — and then declared Becker's work deficient for failing to meet it. Under such a standard, system-level regulatory phenomena could never count as causal by definition. Becker's experiments were not shown to be wrong but rather were declared inadmissible — not good-faith methodological rigor but a veto masquerading as scientific principle.

Venable then escalated from his appraisal to moral insinuation. Becker's refinement of terminology as data accumulated — a normal feature of experimental science — was reframed as terminological manipulation undertaken to preserve conclusions. The distinction between correction and deception was deliberately collapsed, and the reader was invited to infer dishonesty without any demonstration of it.

The attack widened further with Venable's treatment of Becker's theoretical interpretation of regeneration. Selective citation and credential policing replaced substantive engagement. Becker was portrayed as mishandling the literature, misquoting authorities, and venturing beyond his intellectual competence. The point was not to resolve a biological question, but to strip Becker of standing by suggesting he could not be trusted even to interpret evidence, much less than produce it. At this stage, Becker ceased to appear merely mistaken and was recast as a danger to the credibility of others in the field — essentially, this move justified suppression as protection. Silencing Becker was framed not as punishment, but as stewardship.

The letter concluded by condemning Becker's engagement with the popular press — communicating with the public was treated as ethical failure. The unstated premise was clear: science that persuades the public before institutional authorities have sanctioned it must be suspect. Visibility became transgression. Throughout the letter Venable cloaked aggression in moral language and invoked a duty to expose "bad science," recasting his attack as reluctant service to scientific integrity — his rhetoric absolved the cloaking before he did it

What makes the letter's speciousness unmistakable is not merely its argumentative weakness, but its routing. Becker was not informed. Instead, the letter was copied to a National Institute of Health (NIH) official with administrative authority over Becker's pending grant application. The timing was acute. Becker had submitted an application to the NIH for support and was preparing a renewal grant application to the VA research service for his regeneration research. The head of the service was departing, and Handler had recommended that Marguerite Hays — who had already made clear her desire to terminate Becker's funding — be promoted to that position.

The unchallenged letter prejudiced his NIH application, and an adverse signal from NIH could cascade directly into the VA's decision-making regarding Becker's grant application. The letter was also copied to Norman Cousins, emeritus editor of *Saturday Review* and a long-time associate and admirer of Philip Handler. Cousins' prior role in soliciting articles by and about Handler made him an ideal backstage intermediary — someone positioned to exert quiet editorial influence without appearing partisan.

Seen in full, the Vanable letter was not a scientific rebuttal. It was an attempt to manufacture doubt, to transform empirical challenge into reputational liability, and to reassert institutional control over a line of research that had escaped procedural containment. *Saturday Review* declined to publish Vanable's letter or to retract the article. Publicly, Becker's work stood. But the conflict between Becker and Handler — Jaffe's controller, who was Vanable's controller — had entered a new phase.

A month after the *Saturday Review* article made regeneration intelligible and credible, the *Washington Post* published a newspaper article that made it politically visible. Regeneration appeared in the national daily — whose readership was accustomed to institutional conflict and public consequence — framed as a problem of governance and accountability, not an unfolding story in a magazine of ideas. Where *Saturday Review* emphasized coherence and empirical accumulation, the *Post* emphasized stakes — injured veterans, stalled medical progress, and bureaucratic resistance — and did not ask readers to understand biomedical inference or adjudicate biomedical mechanism. Becker appeared in the *Post* not as a speculative theorist, but as a physician-scientist confronting institutional systems that privileged methodological conformity over demonstrable outcome. The *Post* asked the reader to notice a discrepancy: why a line of research that had already produced practical benefit — however limited — struggled for institutional support, while vast resources continued to flow to approaches that promised transformation but delivered little beyond incremental refinement. *Saturday Review* had shown that regeneration could be understood. *The Washington Post* suggested that its marginalization required explanation. Together, the two articles transformed Becker's work into a public question that required an institutional response to an empirical challenge, public visibility converge, and what good science is, considering that their definition is no longer uncontested.

The appearance of regeneration research in the *Washington Post* produced an immediate and striking divergence in response. On one side stood public officials who recognized in Becker's work precisely the kind of biomedical research federal funding was meant to foster. On the other stood Handler and a network of institutional actors for whom that visibility was a threat, not a promise. The high point for Becker came swiftly. Senator John Glenn rose on the floor of the United States Senate to commend the *Washington Post* article and to underscore its implications for medicine and public policy. Glenn described the account as "fascinating and exciting," emphasizing that regeneration had already demonstrated clinical utility in healing non-union bone fractures and might one day extend to damaged limbs, joints, and even spinal cords.

He explicitly framed the work as an illustration of the value of research whose payoff could not be captured by short-term cost accounting or adherence to particular ideologies. Glenn observed that progress often emerged from the convergence of disparate lines of inquiry pursued over long periods of time, guided as much by curiosity as by immediate application.

Glenn pushed back against the reductionist funding ideology that demanded rapid, narrowly defined results and discounted system-level discoveries whose value lay in what they made possible, not in what they immediately delivered.

He asked that the *Washington Post* article be entered into the *Congressional Record*, thereby formalizing Becker's work as part of the national legislative discourse. Within days, Glenn followed with a personal letter to Becker, noting that he had circulated the article among colleagues. The tone was unambiguous: fascination, approval, and encouragement offered not from the margins but from the center of national political life. For a brief moment, Becker's regeneration research occupied an unusual position — both empirically credible and publicly affirmed, insulated by visibility rather than exposed by it.

The counterstroke came quickly and destructively. Soon after the *Saturday Review* and *Washington Post* articles had established Becker's public standing, a press release concerning his regeneration research was issued through the Office of Public Information at Purdue University, authored by Lionel Jaffe. He called Becker's research "plain ordinary fraud," an accusation not hedged by methodological caveat or framed as scholarly dissent, but stated baldly and disseminated under the imprimatur of a major university. The extraordinarily severe charge was not based on a formal finding of misconduct, journal retraction, or investigative process. Jaffe bypassed every mechanism of scientific adjudication and entered directly into the public domain an inflammatory, conclusive assertion of Becker's moral and professional failure, and did so without proper evidentiary foundation, procedural fairness, or institutional restraint. Even though — following a letter Becker sent to the dean at Purdue University — Jaffe acknowledged in a letter to Becker that the press release was the result of "careless talk," that it was "garbled," and that he deeply regretted its issuance, the apology could not undo the damage done. Public accusations of fraud by an authoritative institution linger permanently in the minds of colleagues, funders, and institutions, especially when they align with existing institutional currents, and the institution does not issue a retraction.

The implications of the Purdue press release that Becker feared were soon manifested. The head of the VA's research service resigned and Marguerite Hays, Handler's recommended choice, was promoted to replace him, and she told Becker that his pending application for continued VA funding would likely be denied. Almost simultaneously, the National Institutes of Health (NIH) formally notified Becker that his NIH grant application to continue regeneration research had been rejected.

With NIH funding denied, ominous signals from the VA regarding renewed support, and his scientific reputation publicly scarred, there seemed no realistic institutional pathway forward for Becker. Funding decisions had been rendered, administrative succession completed, and the machinery of biomedical review had returned to its familiar posture. Nothing in the sequence of events that brought Becker to this point had addressed the evidence itself — his observations remained and experiments still stood. What had been interrupted was not a failed program, but a successful one whose implications exceeded the tolerance of the system charged with judging it. Becker had spent two decades building an empirical program whose claims rose and fell on observable results. He had endured skepticism, debate, and criticism, all of which he regarded as proper. What he had not anticipated — until it unfolded — was the degree to which success

itself could provoke retaliation, and how readily institutional power could be mobilized to end a line of inquiry without ever confronting its evidence directly.

Regeneration research had captured public imagination, attracted senatorial praise, and demonstrated tangible clinical promise. But it had also exposed the limits of tolerance within the biomedical establishment. There was no scientific refutation, but rather a quiet consequential resolution — the triumph of institutional orthodoxy over empirical challenge, and the forced withdrawal of a scientist whose work had briefly suggested that another path was possible.

Becker's predicament revealed a deep pathology in the federal system for supporting biomedical research — institutional power. In a research culture increasingly governed by ideology rather than inquiry, success itself could provoke retaliation. Empirical traction—especially when it emerged outside sanctioned frameworks—became a liability. Institutional power, once mobilized, did not need to confront inconvenient findings; it needed only to deny them standing to exhaust their advocates and then wait out their time. Institutional power reflected the maturation of an ideology that had come to dominate federal biomedical research — one that equated rigor with reductionism, legitimacy with conformity, and authority with correctness. Under that regime, the role of institutions was not to follow evidence wherever it led, but to define in advance where evidence was allowed to go. Philip Handler was not the sole inventor of this order, but he perfected it, defended it, and enforced it with a discipline that left little room for deviation

Handler's ideology was strongly established as the norm for federal biomedical research. It appeared sufficiently powerful to prevail despite the absence of merit that it do so. In a research culture increasingly governed by ideology rather than inquiry, success itself could provoke retaliation. Empirical traction—especially when it emerged outside sanctioned frameworks—became a liability. Institutional power of the type developed by Handler, once mobilized, did not need to confront inconvenient findings; it needed only to deny them standing, to exhaust their advocates, and to wait out their time. His power reflected the maturation of his ideology that had come to dominate federal biomedical research—one that equated rigor with reductionism, legitimacy with conformity, and authority with correctness. Under that regime, the role of institutions was not to follow evidence wherever it led, but to define in advance where evidence was allowed to go. Philip Handler was not the sole inventor of this order, but he perfected it, defended it, and enforced it with a discipline that left little room for deviation.

Handler reasoned from observation to illegitimacy and employed the reusable machinery of foreclosure. The outcome — that observations were overruled without permitting an appeal — was not an aberration confined to the fate of regeneration research at the Veterans Administration. It was the predictable result of a governing logic Philip Handler had already refined and successfully deployed in earlier controversies, most recently the Academy's handling of anthropogenic electromagnetic risk during the Sanguine–Seafarer affair. In both cases, Handler's decisive achievement was not the refutation of inconvenient evidence but the construction of an institutional environment in which certain categories of evidence, inquiry, and explanation could not attain legitimacy at all. The antenna controversy showed this logic at work in the domain of environmental and public-health risk.

Handler's reductionist convictions — formed decades earlier and hardened into doctrine — led him to reject, as a matter of principle, the possibility that electromagnetic energy could exert biomedically meaningful effects except through immediate heating or shock. Effects that were, probabilistic, delayed, system-wide, or mediated through regulatory dynamics rather than biochemical reactions were ruled out *a priori* as scientifically meaningless in the sense of not being proper objects of scientific experimentation. He treated the absence of conclusive proof not as an invitation to inquiry but as decisive evidence of the nonexistence of causal consequences. Risk, unless expressible as a certain molecular mechanism, was deemed non-factual and therefore unworthy of institutional attention.

Exactly the same reasoning governed Handler's campaign against the VA's direct studies of emergent biomedical phenomena. In Handler's eyes, Becker's regeneration and cybernetic electromagnetism work was a threat, but not because it lacked rigor or productivity; on the contrary, it was well funded, widely published, experimentally grounded, and officially celebrated by the VA itself. The threat was it demonstrated the viability of emergent-level research within a federal biomedical institution, which directly contradicted Handler's core ideological claim that all legitimate biomedical knowledge must be reducible to molecular hypotheses. As with electromagnetic risk, the problem was not that the evidence was weak, but that its acceptance would force recognition of a form of causality Handler's worldview could not accommodate.

There was a striking procedural symmetry between the antenna controversy and the suppression of VA regeneration research. The parallel was how outcomes were decided before evidence mattered. In the case of the antenna, Handler did not ask whether electromagnetic energy caused harm; he asked whether there was clear, conclusive proof of harm, a deliberately distorted question that guaranteed a negative answer. He then staffed Academy committees with individuals whose professional and economic commitments aligned with that framing, constrained the scope of inquiry to thermal mechanisms, and laundered uncertainty into consensus through institutional authorship. Dissent was permitted to exist only as noise, never as a basis for action. Risk was not disproved; it was defined out of existence .

In the VA case, the same machinery was redeployed with the target shifted from risk to research. Here, Handler transformed a congressional mandate for administrative review into a mechanism for ideological purification. By defining *quality*, *excellence*, and *scientific merit* in reductionist terms and recommending NIH-style external merit review, he constructed evaluative criteria under which emergent research could not compete by definition. No direct attack on Becker was required. Once the standards were installed, the outcome followed automatically. Regeneration research became administratively illegitimate, not because it failed, but because it no longer counted. In both cases, the decisive move was control of the framework that determined legitimacy. Handler's genius — such as it was — lay in making ideological exclusion appear as professional common sense. The process announced itself as neutral governance, modernization, or reform, while silently enforcing a metaphysical boundary about what kinds of knowledge were allowed to exist.

Handler's ideological roots were scientism, certainty, and war on emergence. The deep continuity between the antenna and regeneration cases laid in Handler's scientific ideology, which he articulated in the earlier stages of his career. Handler believed that science — properly understood as reductionist biochemistry — was not merely one way of knowing but

the *only* legitimate source of human knowledge. Facts, in this view, were certain; uncertainty was not provisional knowledge but epistemic failure. Phenomena that could not yet be reduced to molecular mechanisms were not examples of immature science awaiting better tools; they were, by definition, not science at all. This stance had two exceedingly corrosive effects. First, it systematically biased biomedical research against exploratory, observational, and systems-level inquiry — the very approaches required to study emergent biophenomena such as regeneration or bioelectromagnetic regulation. Second, it created a two-staged moral hierarchy within science itself (See Chapter 11). University-based scientists — university employees insulated from industrial or military constraints — were relatively free to pursue the truth about biomedical risks and benefits, independent of external pressure. In sharp contrast, Industry and military scientists — employees, grantees, or consultants of industry or the military — were constrained to produce narrowly framed results that reliably aligned with institutional interests — if they didn't, that work never appeared in a journal, and their employment was jeopardized. When they did, typically, their results were published not to provide new knowledge but to refute the results of university scientists.

This division of science was largely the consequence of Handler's successful instantiation of his ideology within the federal agencies responsible for funding biomedical research, which he accomplished during the years he worked there as an influential authority. He entrenched the orthodoxy that uncertainty — the absence of biochemical knowledge — was tantamount to the conclusion there was no risk and or causal connection. This achievement ensured uncertainty would always be resolved in favor of existing powers, industry and the military; their scientists were easily able to confirm uncertainty by finding nothing.

Handler's denial of electromagnetic health risks and his suppression of regeneration research were two examples of his war against emergence. In both cases, the presence of suggestive evidence triggered not curiosity but institutional defense. Observation itself became dangerous because—if allowed to stand— it could compel further study, regulatory caution, restrictions on military activity, or conceptual revision. Handler's response was to build systems in which observation had no standing unless it arrived already packaged as molecular certainty.

Handler's suppression of Becker's regeneration and electromagnetic research was not the endpoint of his campaign against Becker. His method— manufacture institutional legitimacy and find it lacking — was proven successful and could be used at will. A confrontation that developed late in 1980 over high-voltage powerlines was his most extreme application. Handler was to display an intense determination to defend his methodology that far eclipsed his alliance with Jaffe against Becker, or his use of the Academy's authority to preserve the fiction that uncertainty regarding the Navy's antenna meant safety. What united these episodes was not merely misconduct or bias, but a coherent ideological project: the elimination of emergent biophenomena from official biomedical reality. In Handler's world, what could not be reduced could not be real; what could not be certain could not be known; and what could not be known could be safely ignored. The cost of that worldview—to science, to public health, and to the integrity of inquiry itself—was already visible by 1979. It remained only to see how much further Handler was willing to go to defend it.

