Peter Galison

The Americanization of Unity

It was late 1946. Returning from mobilization, scientists around Cambridge—as elsewhere in the United States—were streaming back to the university. Philipp Frank, who had helped usher in the scientific philosophy of the Vienna Circle and was now a lecturer in the Harvard department of physics, set out a plan for Warren Weaver at the Rockefeller Foundation entitled "The Institute for the Unity of Science: Its Background and Purpose." It is immensely tempting and indeed historically useful to read this manuscript backwards, to see in it the tree whose seed had been planted in late-night discussions at the *Arkadenkaffee,* to track its manifold roots back to the early 1920s in Berlin and Vienna. On such a reading, the revised, now American, Unity of Science movement would chiefly be a revivification of the older Viennese one. Surely there were common concerns: both movements sought to rid philosophy of "superfluous" metaphysics and replace it with a clarity, precision, and empiricism for which science provided the template. Indeed, both in the prewar and postwar Unity of Science efforts, modern science, and not only physics, loomed large. There was near-unanimity that Boltzmann, Mach, Einstein, and Bohr had done much more than rewrite the rules for physics; they had set a new agenda for philosophy. Observability, causality, and probability now reigned where *Geist und Volk* once had.

This essay, however, will take up the new Institute for the Unity of Science that emerged in postwar America, not exclusively through its distant root-ends, but in its immediate envi-

Peter Galison is the Mallinckrodt Professor of History of Science and of Physics at Harvard University.

ronment. It will not focus so much on the scientist-philosophers of the interwar German-speaking world of modernism and Marburg neo-Kantianism, but rather on the squadrons of American scientists returning in 1945 from war research that had given science both a new form of work and a novel place for physics, chemistry, engineering, psychology, and sociology in the world. The objective, in short, is to elicit a double vision: a picture of postwar unity that is both the extension of the Vienna Circle and, at the same time, a philosophical outlook squarely located in the scientific concerns of an age of computers and nuclear power.[1]

Gerald Holton, a participant from early on, has recalled that the assembly of the American Unity of Science movement at Harvard began with Philipp Frank's organization of an "Inter-Scientific Discussion Group" in 1944. The group rapidly expanded, with such speakers as the polymathematician Norbert Wiener, biophysicist John Edsall, and sociologist Talcott Parsons coming to talk about a wide range of topics from biophysics and computers to the psychoanalysis of social systems.[2] Even before the war was over, Frank and his colleagues began dreaming of a new Institute for the Unity of Science.

Beginning with the familiar lament that science had grown ever more specialized, Frank gestured in his December 1946 report toward those who argued that every attempt at integration would descend into superficiality. True, there were those, including Harvard President James Conant, who were concerned about the political consequences of an education insufficiently wide to undergird liberal democracy. But Frank was worried: with quack prescriptions for unity lurking on one side, and popular science and Hollywood movies calling from the other, good scientists understandably wondered where to turn. Working against this fragmentation, Frank contended, was another, deeper tendency. In the world of the late twentieth century, "cross connections" were growing, not shrinking: "The domains of facts which can be derived from one and the same set of principles have not become smaller but larger." As the cross-connected scientists pulled fields together, domains of the special sciences "merged." Chemistry and physics provided an example. Fifty years earlier, no physicist could truly understand chemistry; now, physical chemistry and chemical physics had entered the scene: "Today general chemistry is just a part of nuclear physics. The physicist has an easy road into the very heart of chemistry." So it was in geometry, where general relativity guided the physicist into the heart of mathematics. And just as mathematical biophysics had joined biology and physics, behaviorism had sealed the union between psychology and biology. Throw in F. S. C. Northrop's unification of political and religious ideologies and their links with the physical sciences, Frank contended, and one was on the way to a universal *pass-partout* at the physicists' disposal.[3]

As Frank represented the problem, one difficulty was that of language. While the various special fields of science held much in common, the bridges between them were blocked by gross and fine differences in meaning that were unfortunately confused with differences over matters of fact. "The situation reminds [us of] the Biblical story of the tower of Babel. Because of the confusion in human language the tower of science cannot grow into . . . heaven." Logical positivism, now given a less Viennese and more cosmopolitan pedigree, was advanced in America by J. B. Stallo and Charles Sanders Peirce, William James, and John Dewey, and on the Continent by such luminaries as Henri Poincaré and Ernst Mach; now the whole (according to Frank) had been cast into a more "modern" formulation in work on both sides of the Atlantic. Percy Bridgman introduced operationalism, and Charles Morris bound the Americans to the core of the Vienna Circle that included Otto Neurath, Rudolf Carnap, Ludwig Wittgenstein, and Moritz Schlick.[4] In the classical (1930s) formulation of the unification project, Frank and his allies had been after a semantical goal, above all: to show that the special sciences could all be put into a language of everyday life. This continued in some versions of Frank's postwar philosophy.[5] Now, in this 1946 program, Frank wanted more—a "socio-psychological analysis" or "pragmatic" approach to supplement the logico-empirical one that previously had been the exclusive goal. As Frank put it:

> By adapting these approaches a vast field of research is opened up. "Hybrid fields" like "mathematical biophysics" or "math-

> ematical economics" are no longer isolated cells where some awkward professors may enjoy their strange fancies but by the application of logico-empirical and socio-psychological analysis these "cross-connections" become the roots of new developments leading towards the integration of human knowledge and human behavior. These queer cross-connections become the avanguards [sic] of the science of the future.[6]

Only by such bonds could the investigation establish the connection between "contemporary physics on one side and contemporary religion and politics on the other side with contemporary philosophy being the intermediate link."[7] In another document from the same time, Frank listed some of the goals of a sociology of science—it would include the conditions under which discoveries were made, but also "intervention of the government in science," and "contemporary merging of science and technique."[8]

Warren Weaver had thrown the weight of the Rockefeller Foundation (along with some modest resources) behind the prewar Unity of Science movement run by Neurath, Carnap, and Morris. In the much-changed postwar world, Weaver heard Frank out and recorded in his diary on December 13, 1946 that "the Unity of Science Movement has been in a somewhat chaotic state since the death of Otto Neurath [late in the war], this being the more true since N[eurath] ran all of the business of the organization in a very individualistic and indeed almost dictatorial way."[9] To the old commitments of the Unity of Science movement (an encyclopedia, a journal, a bibliography, and conferences on unified science) Frank now wanted to add the role it might play in "modern American movements in general education." Writing to Weaver in January of 1947, Frank explained that his course aimed to show just what the "principle of relativity" sanctioned in the wider world of culture, ethics and truth—and what it did not. Only through such a critical examination could the scientist know whether the theory of quanta justified the belief in "freedom of the will" or advanced the reconciliation of science and religion. So girded against misinterpretation, the student of science could venture out against the raft of pseudoscientific or the pseudoreligious interpretations of science. When coupled with an understanding of the historical situatedness of science, such as the Copernican Revolution and "similar conflicts," Frank contended that the science student would have an "inner track" in grasping current relations between science, religion, and government.[10]

At stake, Frank argued, was the fate of the world. Ideologies—combinations of philosophical with political creeds—underpinned both the right wing with its organismic metaphysics and the left wing with its dialectical materialism. Prominent "cardinals of the church" espoused their Thomism (so Frank continued), while political leaders including Lenin plunged his followers into dialectical materialism. Only the student with logico-empirical analysis in one hand and socio-psychological analysis in the other could navigate these waters, for only with a deep understanding of the scientific process in context could the student grasp the idea that a chemical formula like H_2SO_4 was not an isolated fragment of knowledge but rather a "flaming manifesto to mankind."[11]

Weaver bought the manifesto. Recorded among the foundation's deliberations are the considerations that moved them. Above all, the Board cited the ever-expanding "cross- and inter-connections" between pairs of disciplines that now seized "more and more common ground": physics and chemistry, astronomy and physics, biology and psychology, among others. These, the panel judged, were "domains of experience . . . explainable from one and the same set of basic principles." Accordingly, in December 1947, the Rockefeller Foundation designated some $9,000 for the Unity of Science movement covering three years of support (though it was not, for technical reasons, delivered until July of 1949).[12] Led by directors Rudolf Carnap, Charles Morris, Philipp Frank, Milton Konvitz (a lawyer from Cornell), and Hans Reichenbach (then at the University of California, Los Angeles), the group took every opportunity to proclaim their limitless ambition—they would re-establish ties with Europe, train a generation of politically astute scientists, link the working scientific disciplines together, and reform philosophy.

Could this unification take place? If so, would it reflect a unified nature or a unified science? A confidential Rockefeller Foundation report to the trustees (dated March 1949) meditated on this metaphysical dilemma. "We have physical experiments, chemical experiments, biological experiments, and other specialized techniques, but it is important to remember that classification into these categories is man's invention. Whether it is also nature's, we don't know." One school of scientists, the report continued, supposed such metaphysical unity did obtain: "a universe of matter and energy whose interactions under certain conditions produce motion, radiation, and the other effects which we label physical, and under different conditions produce the nightingale's singing and other behavior which we call biological."[13] From Alfred North Whitehead to George Sarton, this metaphysical commitment to the unity of nature became an oft-repeated creed.[14]

Not everyone agreed, as the foundation's 1949 report made clear. Herbert Dingle, for one, argued that this sort of reductionistic metaphysical unity could not be guaranteed. The Rockefeller trustees would have read in the report that the metaphysical unity of nature was not a sure thing, according to Dingle:

> We aim at it; we hope we shall achieve it; but we must recognize the possibility that nature may be essentially dual, or even multiple. . . . We do not ignore the organic unity of nature when we consider laws of motion apart from those of economics, let us say. We simply avail ourselves of the fact that we can make progress by admitting that, at present, motion and economics are disconnected subjects of study. We hope that we shall unify them, but to let our thinking be influenced by the assumption that they are essentially one seems indefensible.[15]

That said, the report went on to laud Maxwell's unification of electricity and optics, along with Einstein's of mathematics and physics (through general relativity). But the list did not stop there. Of crucial import were biophysics, biochemistry, psychophysics, psycho-physiology, and social psychology; moreover, the report noted, "other borderland sciences are fields that seem likely to contribute new data for a unitary picture of nature." In the process of this joining together of "borderland" disciplines in pairwise links, concepts that were superfluous would drop by the wayside. Einstein's geometrical dynamics made "gravitational force" a dead letter; the quantum theory of the chemical bond rendered "chemical force" obsolete; and Maxwellian electrodynamics left fundamental optical hypotheses as nothing but a fifth wheel.[16] Would this *piecewise* integration extend all the way from mathematics to sociology? If it did, would the knowledge pyramid reflect a "natural" order of things? Steering a midcourse between metaphysical dualism and metaphysical unity, Herbert Feigl argued for establishing such connections "without premature attempts at complete unification."[17]

Partial connections (such as that afforded by chemical physics) would take place through the "master key" of semantics, "the study of the meaning of words and other symbols." Just as disposing of "chemical force" was a conceptual advance, so too would be a clarification of the myriad of often obsolete terms plaguing biology—"entelechy," "vital force," "mechanism," "holism," and "entity"—not to speak of similar vestiges of an earlier physics, including "absolute space," "absolute time," "simultaneity." Only a rigorous operationalism could effect this purge of the superfluous. Quoting Feigl approvingly, the report continues, "The possibility of a reconstruction of all factual sciences on the basis of a common set of root terms enables us to speak of the reducibility of all sciences to a common, unitary, interscientific language."[18]

In an attempt to deliver just such a "basic operational dictionary," Frank and MIT's Karl W. Deutsch began a composition in the fall of 1952.[19] Containing nineteen different categories, with three hundred terms, the sweep of the project is stunning.

Table 1. Frank and Deutsch, Basic Operational Dictionary (Outline)

I.	Basic Notions	XI.	Physiological Concepts
II.	Sets, Groups, Order, Structure	XII.	Organism
III.	Constructs (of physics)	XIII.	Mechanism
IV.	Prediction	XIV.	Learning
V.	Logic and Semantics	XV.	Biology
VI.	Psychology	XVI.	Ethics
VII.	Communications Engineering and Theory	XVII.	Religion
VIII.	Sociology and Anthropology	XVIII.	Chemistry
IX.	Economics	XIX.	Aesthetics
X.	Political Science		

Source: Frank, "Report on the Dictionary of Operational Definitions" (September 1952, RG 1.1, 100 Unity of Science, 1952–56, Box 35, folder 285, Rockefeller Foundation Archives).

In all, there would be three hundred "basic concepts." These would include not only standard physics notions like mass, matter, energy, space, time, and field but also (picture Carnap's horror) such hard-to-imagine-operationalized concepts as love (under psychology) or God, belief (faith), soul, and damnation (under religion). (I cannot help but wonder here whether salvation is excluded deliberately or whether it is operationalized under a negative disposition of damnation.)

In those cases where the operational definitions were clear from usage, they would be drawn from "scientific writing." If not, then views would be drawn from writers with an appropriate "operational viewpoint." If both were absent, then experts would provide "paper and pencil operations"; if even these were not possible, then "hypothetical operations," analogous to procedures that could be performed, would be utilized. By example: "real" might refer to that which is "familiar from repetitive, gross, bodily experience." Alternatively (Frank wrote), "we mean by 'real' things from which we can continue to learn, overriding past symbols and traditions." "Reality" is signaled by "structural coincidence" between sensations and impersonal records. "Sensations" track back to "traces" within the nervous system and are therefore impermanent and not easily verifiable, whereas "instrument records" are external, more easily verifiable, and forever.[20]

One could study these three hundred greatest hits in the concept parade almost mechanically, finding here and there the bits and pieces of prewar Vienna Circle concerns. Starting with "sets, groups, order, and structure," one could discern the elements of the new formal logic and set theory of Frege and Russell that so impressed the group back in the 1920s: class, universals, group, model, order, congruence make their appearance here. Under "prediction" we could track back many of Reichenbach's or Carnap's concerns in their extensive writings on probability: "equipossibility," "limit of relative frequencies," "degree of assent," or Frank's own youthful dissection of the causality notion that had so impressed Einstein. Here, too, we find vestiges of the old Vienna Circle's fascination with Freudian psychology (the list includes id and ego) and the frequently discussed *gestalt* concept that arose in discussions among Carnap, Neurath, Wittgenstein, and Schlick; we also see elements of economic theory (utility, market, profit, labor, capital, efficiency) that engaged many among the left wing of the Circle. Religious concerns, anathema to Carnap, could no doubt be laid (in part) at Charles Morris's door, as his "Paths of Life" drew him ever more into contemplation of the great world religious leaders and their thought.[21]

Then, too, we are not surprised to find on a list drawn up by Frank, the positivistic biographer of Einstein, the terms "mass," "matter," "energy," "space," or "time," under "constructs (of physics)." These Einstein-revised notions were read by the Vienna Circle as prototypical positivistic moves. Space was defined through the laying out of rigid measuring rods, and time by the readings of identically calibrated clocks; notions of mass and energy were correspondingly revised. As we now know, Einstein demurred when presented with Frank's positivistic rendition of his work; as far as I can tell these protestations were to no avail.[22]

But there is more on Frank's list than its vaulting ambition, more even than the sum of prewar interests. In particular, several of the categories are *not* ones we would have found even among Neurath's wildest hopes. "Communications engineering and theory" had no role in the world of Schlick, Carnap, Neurath, or Frank years before. This category breaks down as shown in Table 2.

Table 2. Communications Engineering and Theory

1. Message
2. Information
3. Signal
4. Channel
5. Circuit
6. Network
7. Recognition
8. Noise

Source: Frank, "Communications Engineering and Theory" (1952, RG 1.1, 100 Unity of Science, 1952–56, Box 35, folder 285, Rockefeller Foundation Archives).

While pieces of this list were discussed together in various sectors of the radio-technical or telephonic industries before the war, their pride of place in the basic concepts of the world is altogether new. And for just cause—these are some of the starting points of the new sciences loosely grouped under cybernetics, informatics, and the burgeoning wartime radar laboratories. By the time Frank composed this list in September of 1952, cybernetics had already become one of the central issues preoccupying the meetings of the Institute for the Unity of Science. On January 19, 1951, Wiener and Rosenblith launched the Institute's "Cybernetics and Communications" study group with two meetings devoted to a "systematic examination of fundamental concepts," beginning with feedback, noise, entropy, and information.[23] Just to give a few examples of the scope of the study group's concerns, consider the sessions organized around the linguist Morris Halle on the "Entropy of Language," building on the formal ties Claude Shannon and Wiener had established between entropy in statistical mechanics as $k \log \Omega$ and information defined as negative entropy. (Wiener himself had published directly on the links between cybernetics and these other fields in his "Speech, Language, and Learning."[24]) Two further meetings of the Institute also fit squarely into the Wienerean framework: R. D. Luce spoke on "Communication and Learning in Small Task-Oriented Groups," and M. Rogers addressed "Some Applications of Information Theory to Psychology." Indeed, the cybernetic track remained one of the most active topics at the Institute for several years. Many of these gatherings took place in the department of electrical engineering at MIT.[25] Why, the reader may wonder, would anyone be discussing small-group learning, language, and psychology in MIT's E.E. department? In a sense, the answer to that question lies at the bottom of a fundamental change in the meaning of "unity" in the Institute's conception of "Unity of Science." But to get there we need to step back, both philosophically and historically.

* * *

The central point here is that the Central European prewar notions of unity differ strikingly from the ideas of unity that emerged from American collaborative war work. Before the war the slogan "unity of science" carried various meanings, as we know from the careful work of Nancy Cartwright, Jordi Cat, Richard Creath, Michael Friedman, Lola Fleck, Thomas Uebel, and others.[26] Carnap, for example, had at least two senses of the notion of unity. In his early work there is a kind of autopsychological foundationalism, the search for an *Aufbau* built upon the "bedrock" certainty of one's sense impressions here and now, and encompassing pyramidically the whole of human knowledge, including psychology. In a sometimes uneasy tension with this notion of a whole structure for science lies another in which the "basis" is not the autopsychological but rather the heteropsychological, the commonly shared experience. Upon this, too, Carnap could erect the whole edifice of the sciences. Finally, as Friedman has so interestingly shown, Carnap seems—at least by the time he wrote *Logical Syntax of Language* (1934) and perhaps the *Aufbau* itself—to have a full-blown conventionalism that allows him to make it a matter of indifference how the *Aufbau* is grounded: the whole point is to secure objectivity by way of the relations between its elements, and to avoid any reliance upon private experience.[27]

Neurath's notion of unity is not the same as any of these. It is not a "building up" (*Aufbau*) on rock-bottom autopsychological foundations, it is not an *Aufbau* on heteropsychological foundations, and it is not a conventional or "structural" *Aufbau* either. As Cartwright, Cat, Fleck, and Uebel note, Neurath's most distinct notion of unity aimed not at the expression of all science in the language of physics, but at the creation of a heterogeneous jargon capturing pieces of social science, ordinary language, and physics.[28] Metaphorically, he did not want a pyramid of knowledge but a coordinated encyclopedia. The hierarchy of a Comtian picture of science would be replaced by the orchestration of different instruments, each distinct but brought together to accomplish something bigger than any could do individually.[29] Famously, Neurath invoked the image of a forest fire to illustrate how necessary it was to organize the various sciences into an effective unit. Surely, Neurath insisted, one would need to know about climatological and chemical laws to understand the pattern of a mass conflagration. But without the coordination of these physical laws with sociological ones, there could be no prediction. Presumably, the "orchestrated" effort would survey human behavior to find out the circumstances in which humans tended to cause fires. Only then, chemical and climatological knowledge in hand, would the coordinated science be able to predict how the fire spread.[30]

The sciences of World War II were worried about massive fires, all right, but they were in the business of causing them—not reckoning their probability of accidental occurrence. More importantly, American scientific war work was characteristically not merely aggregative (chemical knowledge + climatological information + sociological aspects of behavior = knowledge of fire propagation) but instead involved the formulation of entirely new combinations of disciplines. Take Norbert Wiener, who participated in many of the Institute's meetings and whose work (as we have seen) launched the Institute's central and sustained inquiry into the new science of cybernetics. Long before World War II, Wiener had been a precocious and nearly omnicompetent scholar who moved easily between his home field of mathematics and the adjacent ones of Birkhoff's ergodic theorem, Kolmogorov's axiomatization of probability theory, and a variety of philosophical inquiries. The German air assault on Britain changed that. More than a year before Pearl Harbor, Wiener threw himself completely into the problem of antiaircraft fire-control; suddenly he was neck-deep in engineering and vacuum tube work. Looking for a collaborator in March of 1941, he saw no need for a pure mathematician—he required someone already immersed in computing, communication engineering, and vacuum tube work. Anyone without a feel for engineering, without at least competence in putting together radio sets, ought not apply: "There is nothing in abstract algebra or topology . . . which would prepare one in any way to cooperate in engineering design."[31] And this (according to Wiener) was not only true for his project; it was equally so for just about every piece of crucial war work, from ballistics to cryptography. Already—almost a year before America's war had begun—Wiener insisted that science had to remake itself, realigning old subjects and creating new ones.

Swerving erratically through the sky as they skirted flak, Nazi bombers were hard to hit; a shot from the ground took ten or fifteen seconds to reach altitude, and by then the bomber was headed somewhere else. Responding to this difficulty, Wiener launched what became a new science, one devoted to the electromechanical replication of the human capacity to predict: "Since our understanding of the mechanical elements of gun pointing appeared to us to be far ahead of our psychological understanding, we chose to try to find a mechanical analogue of the gun pointer and the airplane pilot."[32] With electronics, Wiener radically reorganized the way gun laying was done by putting together a "predictor," a device that would take the measure of the pilot's last moves, calculate the statistical likelihood of his future actions, and launch a shell to destroy him where he would be. In a sense that rapidly became altogether explicit for Wiener, the antiaircraft predictor precisely duplicated the intention of the pilot in the flow of electrons. Psychology was not being *superadded* to electromechanism; psychological notions were *supplanted* by the circuitry. Immediately, Wiener began considering what this picture of psychology would mean.

Edwin Boring, the Harvard psychologist (and an early member of the Inter-Scientific Discussion Group) took note. On November 13, 1944, he wrote to Wiener that he planned a "pretty complete list of psychological functions" that he hoped Wiener could duplicate by means of electrical systems. "Symbolic process" would, in electrical input/output terms, come down to "a delayed, adequately differential reaction"; "introspection" on the electronic breadboard ultimately became a reaction to a reaction. Boring laid down the challenge: could Wiener translate each of these stimulus-response pairs into his own "black-box" of electrical relations? "Generalization"? "Abstraction"? Each term that Boring put into behaviorist form would, he hoped, then find its expression in circuitry. "I do not know that you can [do it], but I should be betting on you."[33]

With lightning speed Wiener generalized his predictor's supplanting of intention. Even before the war was over, he had begun to make fundamental and broad use of terms previously confined to specialized aspects of telephonic engineering: signal, noise, feedback, and control flew far ahead of antiaircraft fire.

The physiological model was rife with feedback systems—a circumstance brought home to him by his medical collaborators. There was, for example, the clinically well-known purpose tremor in which a voluntary act such as reaching for a pencil launches an uncontrollable oscillation of overshoot and undershoot. As reformulated by Wiener, the purpose tremor became a particularly salient instance of the more general functioning of the brain—in this case a disordering of the normal feedback cycle between brain, muscle, effector organ, outside world, receptor organ and back to the brain.[34] This work linked him crucially to a variety of medical personnel, including Walter Cannon at the Harvard Medical School, an active member of the Institute for the Unity of Science. At the same time, Wiener became increasingly interested in the role such feedback circuitry might play in electronic computing. With John von Neumann, Wiener organized early and important meetings that helped usher in plans for computers being formulated toward the end of the war.

These discussions were pivotal for von Neumann. In 1945 and 1946, von Neumann did his fundamental work on the digital computer, abstracting from the particular form of electronic realization of the system and putting together a brainlike composite of "organs," leading to the stored-program computer. Wiener, von Neumann, and their associates moved back and forth between the language of logic, the language of electronics, the language of neurophysiology, and the abstracted language of computer functions. Much of this—including von Neumann and Goldstine's first lecture on flow diagrams—came together in January of 1947 at the Harvard Symposium on Large-Scale Digital Calculating Machinery. H. H. Aiken, builder of the electromechanical Mark I and one of the leading experts on the computer, brought the results a few days later to the Inter-Scientific Discussion Group.[35] Here were the new "borderland" sciences in action.

This working out of a conjoint picture of behaviorist psychology and the feedback-predictive circuitry explored by Wiener, neurological studies, and electronic computation became an enduring concern for the Unity of Science group. The same day that Boring penned his fourteen-concept challenge to Wiener, Boring reported in another letter that he and Wiener had both just been present at the Inter-Scientific Discussion Group.[36] Wiener himself was an active member of the group, speaking at one of the very first gatherings in December of 1944 on Birkhoff's ergodic theorem and again on February 14, 1945, on "The Brain and Computing Machine": it was not accidental that the Institute had elevated cybernetics into one of its central topics.[37] The discipline of "cybernetics" (named after the war by Wiener) set out and emphasized certain concepts such as feedback and control, gave it a more developed formal presentation, and linked the whole to information theory and computational strategies. With enormous, perhaps overwrought enthusiasm, physiologists, sociologists, anthropologists, computer designers, and philosophers leapt on the cyberwagon. Even the anthropologists Margaret Mead and Gregory Bateson rewrote the framework of their work in light of the new concepts.[38] Recalling Frank and Deutsch's 1952 list of basic concepts in communication theory (message, information, signal, channel, circuit, network, recognition, and noise), we now perceive in it the elements of shared starting concepts. We see precisely the kind of piecewise unification and cross-connections proclaimed by the Institute for Unified Science.

At the close of the war, Frank had only to look around the corridors of Cruft Laboratory in his own physics department to find his colleagues, fresh from the war effort, brimming with enthusiasm about the new interdisciplines to be explored. Frank himself had spent part of the war preparing Navy officers in physics for their work with radar—along with E. C. Kemble, I. Bernard Cohen, Gerald Holton, Roy Glauber, and Frederic de Hofmann. In the latter part of the war, Frank moved to Columbia University where he did classified applied mathematics work. Among Frank's other physics department colleagues, each had his own stories, his own witnessing of disciplinary recombination. Indeed, their interdisciplinary duties were typically several. The acoustician F. V. Hunt ran the multimillion-dollar Harvard Underwater Sound Laboratory, drawing together electronics, oceanography, physics, ship operation, and much else.[39] Wendell Furry had codirected a research project on the thermal diffusion of gases subject to molecular force laws, work executed on the differential analyzer at MIT and eventually put to application on the Manhattan project.[40] E. C. Kemble had been on the Alsos mission to determine how far the Nazis were in their quest for the bomb; his conclusion was that the ability of American scientists to retrain themselves into engineers had been the key to a narrow victory over the Germans. It was the Allies' great fortune, Kemble concluded, that the Nazis had remained rigidly hierarchical and protective of their division of the pure and applied disciplines.[41] (Both Kemble and Alsos leader Samuel Goudsmit attended the early Inter-Scientific meetings.) Kenneth Bainbridge was returning from the Manhattan Project, out of which the new field of "nucleonics" was to combine nuclear physics, engineering applications, and a myriad of electronic, chemical, metallurgical, mathematical, calculational, and even medical techniques. Now many of these physicists joined forces with colleagues from such fields as metallurgy, chemistry, and engineering to run Harvard's interdisciplinary Committee on Nuclear Sciences.

In the Manhattan Project itself, physicists had learned how to think about matter very differently. There was no way to avoid seeking simultaneously to understand the metallurgy, shock wave behavior, and nuclear physics of imploding plutonium. It is against this background that we must read Frank's 1946 plea cited at the beginning of this paper: "Today general chemistry is just part of nuclear physics. The physicist has an easy road into the very heart of chemistry." In 1928 the specialty "nuclear physics" did not exist as such; the utterance would have been meaningless.

Physicists Edward Purcell, Wendell Furry, Curry Street, and Julian Schwinger spent their war years based further down Massachusetts Avenue at the MIT Radiation Laboratory, where quantum mechanicians had made common cause with radio engineers and industrialists to produce the new field of microwave physics. Once physicists had held themselves aloof from the grubby details of engineering; now a new generation of American physicists learned from radio engineers how to think about black boxes, input/output analysis, effective circuits, breadboards, signal-to-noise ratios, and mass production. Fueled first by the war, from Stanford to MIT microwave physics burgeoned where before the war no such field had existed. Whether one looked up to radioastronomy or down to particle accelerators, whether one turned to the practical features of solid-state physics or to the moments probed by nuclear magnetic resonance, the new techniques of war-inspired short-wavelength physics were reformulating how people went about their scientific business and with whom they spoke. So when Purcell came to his first Inter-Scientific Discussion Group in March of 1946, interscientific coordination would have been more than a programmatic gesture; it had been his main work for the bulk of a still-young scientific career.[42]

And the list goes on: Harvard physicist John Van Vleck (also a member of the Institute for the Unity of Science) spent the years of conflict working on radar countermeasures to foil German air defense in Harvard's Radio Research Laboratory, far from his usual specialty and in concert with engineers. I. I. Rabi, a leader of the overall radar effort, also became a member of the Unity of Science movement. The high-pressure experimentalist Percy Bridgman, inventor of operationalism and one of the strongest American prewar boosters of the Unity of Science movement (having also served on the Inter-Scientific Discussion Group steering committee), spent his war years at the ORDC Ordnance section, where he worked with the Watertown Arsenal on the pressure effects of projectiles on steel, on polymers of new plastics for possible use inside internal combustion engines, and on the physics and chemistry of explosions and incendiary explosives.[43] Nor was war-driven interdisciplinary coordination restricted to physicists, chemists, and biologists. For his part, philosopher W. V. Quine, another powerful supporter of the prewar Unity of Science movement, joined other philosophers, engineers, and mathematicians to decipher intercepted messages and pinpoint the location of the German submarine wolfpacks.[44] And astronomer Harlow Shapley, who ran a substantial research effort on complex new lenses for aerial photography, went on to become a member of the board of trustees for the Institute for the Unity of Science. Indeed, one only has to look at the roster of the Inter-Scientific Discussion Group to find Cambridge addresses that had not existed before, addresses that joined fields as well as people: the Electro-Acoustic Laboratory, the Systems Research Laboratory, the MIT Radiation Laboratory, the Psycho-Acoustic laboratory, the Underwater Sound

Laboratory, the Fatigue Laboratory. Looking back at the list of participants in the Inter-Scientific Discussion Group and the Institute for the Unity of Science in this context, suddenly it reads differently.

Take Stanley S. Stevens, the Harvard psychologist, active in the Inter-Scientific Discussion Group and then on the board of trustees of the Institute for Unified Science and on its program committee.[45] Stevens had worked in and with a variety of interdisciplinary wartime laboratories; it is worth pausing to review what they were and how they worked. Back in late 1940, when the War Department was first gathering project titles for immediate research, it turned certain problems over to the National Defense Research Committee. One issue raised by officers at Wright Field was the effect of noise and vibration on aviators. Information had come back that the noise and vibrations of airplanes so exhausted the pilots on their deep-penetration bombing missions over Germany that they crashed on landing back in East Anglia. Leo Beranek, an acoustician, got the job of developing measures to address the airplane noise problem, first by deciding how quiet airplanes needed to be and then by developing or finding lightweight materials to do the job. It is perhaps illustrative of the times that when Beranek proposed a budget of $3,000 per year, the request was denied—as Beranek recalled in April of 1945, the military indicated that if "we would multiply the figure by ten, [they] would talk business."[46] Forty thousand dollars were set aside for the first seven months. In addition to Stevens (from psychology) and Beranek, F. V. Hunt represented the Harvard physics department and the acoustician P. M. Morse came from MIT. It was a complex problem, involving the analysis of the principles of sound absorption and then its development into a workable product—fiberglass AA, produced by Owens-Corning Fiberglass company. Next the group began a systematic analysis of how to predict noise levels from the blueprints of planes, so that engineers could intervene early enough to minimize the problem. By April of 1941, the sound team had also begun to explore communication in airplanes, a task at once electrical (analysis of amplifiers and microphones), acoustical (insulation of headsets) and psychological (determination of which sounds were intelligible, development of codified patterns of speech). Two laboratories, both interdisciplinary, collaborated extensively in this effort: the Electro-Acoustical Laboratory (directed by Beranek) and the Psycho-Acoustic Laboratory (headed by Stanley Stevens).[47] These labs did not always find the coordination of different fields easy; as one wartime report put it, "The bringing together of men of different experience, different training and different interests presented a sizable problem of integration."[48] That "integration" was hard won over the next year and a half. But by 1944, interscientific collaboration had become part of everyday life for Stevens and his associates.

Problems addressed by the psycho- and electro-acoustic labs included designing earphones, headphones, and microphones in oxygen masks that would allow radio communication while not impeding visibility or oxygen flow; positioning a microphone for best communication in a noisy environment (not, it turned out, near the throat as the Army Air Forces were doing—even the hand-held microphone worked better). While the majority of the electro-acousticians scrambled to ensure quiet in the cockpit and on the bridge, Gerald Holton, secretary of the Inter-Scientific Discussion Group, spent his war days teaching radar while mixing acoustics, chemistry, and physiology in designing gas masks that wouldn't silence a sergeant's barked orders.[49] Physiology as well as psychology entered the picture when the electro-acoustic and psycho-acoustic labs began to address the problem of communication at high altitude; as the bombing levels rose in 1942, it became apparent that inter-crew communication was failing above 25,000 feet. Electronics experts, radio wizards, and doctors joined together to solve the problem. Volunteer conscientious objectors were (virtually) lofted in depressurization chambers to examine the effects of rarified atmosphere on the human voice. The results of these experiments showed that the problem lay in neither the equipment nor the faculty of hearing; instead, it was learned that the human voice drops to a mere 1/60th of its sea-level intensity when the speaker reaches 35,000 feet. Combined with new instruments and a new, codified set of mandatory speech protocols, interphone talk soon became considerably clearer. By the time Stevens began attending the Inter-Scientific Discussion Group on December 18, 1945, he would have recognized interdisciplinary (systems) coordination as both essential and effective.

Building on L. J. Henderson's and Elton Mayo's prewar fatigue laboratory, the war years saw an extension of studies to a wider domain of scientific fields and a broader spectrum of military applications. In 1943, an interdisciplinary team constructed a "fatigue chamber" at the Harvard Business School and began to make operational the myriad of physio-psychological effects associated with noise, all the while working to alter airplane and communication-gear design. How, precisely and repeatably, did the noise in a cockpit or on the bridge of a battleship impede response time, the execution of multiple tasks, or attentiveness to spoken orders? Addressing questions of this last sort fell to another laboratory, the Systems Research Laboratory, tasked with simulating a ship's combat information center in the heat of battle. Again, the task was irreducibly interdisciplinary as time and motion experts had to learn to work with radio engineers, physicists, psychologists, and radar-display personnel. Out of the chaos of a ship under attack, the team created a new order: they moved instruments, altered display panels, choreographed physical movements, and rewrote patterns of speech. This was a "systems" laboratory, where "systems" meant for participants a focus not on isolated pieces of equipment but on people operating equipment "as an integral part of an organization." These researchers were not aiming to discover how a particular radar component handled frequency, pulse width, repetition rate, and lobe pattern; they were after answers to other questions: When operated by typical personnel, how many target fixes can the radar handle per minute? What is the normal degree of error in these fixes? How much time lag is there between the appearance of the pip on the scope and the dissemination of range and bearing by the operator? What are the details—such as the location and size of controls, and the types of cursors—that delay getting each fix?[50]

Conjoint questions like these forced a specific form of unity among the various disciplines, a unity predicated on assembling diverse methods, professions, and patterns of work into the production of pragmatic solutions to immediate problems. Again and again, these interscientific laboratories rendered "operational" their solutions to applied wartime problems, at every step comingling psychological categories, physical principles, and engineering practicalities.

* * *

Put before your mind's eye two very different pictures. First, evoke the Vienna Circle in full bloom. Its adherents saw themselves as opposing the forces of irrationality and joining hands with the modern in architecture, city planning, and, at times, Austro-Marxist politics.[51] Never politically powerful or even institutionally secure, as time went on their voices were increasingly drowned out by the array of nationalistic forces pitted against them. The drive to a "Unity of Science Movement" was, for Neurath, Carnap, and their allies, part and parcel of a struggle to bring together a rationality and objectivity that would halt racial and nationalistic assaults from dominating the world. Their opponents were Austrian clericalism, entrenched traditional philosophy, and, later, Nazism. Just the title of a typical philosophy paper in the mid-1930s shows just how much metaphysics the "old" philosophy could cram into a single article: "Godliness and the Character of the '*Volk*.'" Whether through an *Aufbau* of Carnap's sort or through a physicalist thing language, the Vienna Circle's goal was to squeeze out of the world of the meaningful all that counted as metaphysics. And metaphysics was for them not some limited concept, but the alive, well, and dangerous movements for Godliness, *Volk*, mysticism, and *Deutschtum*. Even the philosophy of Heidegger, they believed, was infected by metaphysics.[52]

On the side of rationality was, above all, the new science and logic. And among the sciences, none served better and more epigrammatically than Einstein's 1905 paper on the electrodynamics of moving bodies. Indeed, if I had to choose one moment in the history of science that the Vienna Circle would have emblazoned on their banner, it would no doubt have been that most famous of all lines penned by that twenty-six-year-old patent clerk: "The introduction of a 'light-aether' will prove to be superfluous. . . ." That unapologetic stripping down, not unlike the Bauhaus architects' removal of ornamentation in their

Dessau headquarters, was for the Vienna Circle a move towards victory over everything they detested in philosophy, in politics, and in culture. Modern physics could ground itself in the specifiable, measurable world of function—and so, the left wing of the Vienna Circle believed, could the rest of the social and human sciences. Theirs was, as they often insisted, more than a philosophical movement; it was the search for a new *Lebensgestaltung.*

The unity in the prewar "unity of science" movement had to be papered over to a certain extent. Carnap's conception of his unifying scheme changed over time. Protocol statements, often deployed as essential in securing unity, were understood differently by Carnap and Neurath, with Neurath always insisting that he only meant that these statements were the last to be given up. Even when unity was to be a purely linguistic exercise—unity as expressibility in terms of objects describable in space and time—there were differences of understanding. Sometimes this language appears as the language of physics, sometimes (especially in Neurath) it refers to a mixed jargon, drawing, without possibility of reduction, from many different sciences (including the social sciences). But variously as the programs for scientific unity were construed, there was a shared sense that the project of *Erkenntniswissenschaft* would find a new and better formulation. Philosophy would aid the other disciplines in cutting the unnecessary or destructive and identifying the modern strategy of epistemic austerity.

Now move your mental image ahead to 1947, from Vienna and Berlin to Cambridge, Massachusetts. The scientific banner flying overhead is not that of relativity and quantum mechanics—though these might occasionally be invoked by Frank. Instead, the banner announces the riveting new, war-boosted interdisciplines: cybernetics, computation, neutronics, operations research, psycho-acoustics, game theory, biophysics, electro-acoustics. The old enemies of interwar Vienna are gone or vanquished: Austrian clericalism and the hollow vestiges of the Habsburg empire do not figure very large in Cambridge, and fascism has been slain, in no small measure (in their scientists' eyes, at least) because of scientists' intervention. Now these same tools that had won the war promised the world. Cybernetics, with its nonlinear feedback, was celebrated as offering a way to rewrite the social sciences as well as the sciences; the computer's logic was thought to be universal and capable of doing everything from weather forecasting to nuclear-weapons design, from the resolution of longstanding problems in number theory to modeling the human mind.

The unification these scientists had in mind was a unification through localized sets of common concepts, not through a global metaphysical reductionism. Were the mathematical and technical features of feedback, control, black boxes, flow diagrams, or extensive forms of a game "reducible" to nuclear physics? Hardly. Even posing that question about the kinds of problems facing the Institute seems hopelessly inappropriate. With the kind of power these scientists felt they had at war's end, fretting about ontological reductionism must have seemed almost beside the point. As the chemist E. Bright Wilson wrote to Holton, the Institute secretary, in 1950: "The phase of the Institute's work in which I am particularly interested is that which deals with scientific method in its most practical and least philosophical senses."[53] The Americanization of unity just after World War II was not sited around an isotypic picture language, a physical language, an *Aufbau*, or an orchestration. It was planted around the new sciences of Los Alamos, the MIT Radiation Laboratory, the stored-program computer of the Institute for Advanced Study in Princeton; this was to be a science unified in pieces, grounded in common widely applicable concepts, and promising a power beyond dreams.

One last contrast: When the Vienna Circle faced off against theology in their manifesto, they saw mystic obscurantism as a rising threat; however misunderstood or powerless they were, the Vienna Circle aimed to cast millenia of such speculation to the winds. When the organizers of the Institute for the Unity of Science sent out its first flyers, they made "Science and Faith" and "Science and Values" early and longstanding objects of study.

In one of the first meetings of the Institute for the Unity of Science, a prominent participant probably spoke for many in observing that the public now saw scientists as authorities comparable to the high priests of ancient cults. But the truly staggering feature is not the prominent positive role accorded truth and values; it is that in these first months of the *pax Americana*, this group of scientists, humanists, and philosophers could take on God and Morality as problems—and fully expect to solve them.

ACKNOWLEDGMENTS AND SOURCES

It is a pleasure to acknowledge the assistance of Rockefeller Archive Center, especially Thomas Rosenbaum, the archivist. Documents from the Rockefeller Foundation Archives are identified in the notes by the designation "RFA". Gerald Holton provided records from his time as secretary of the Inter-Scientific Discussion Group (later the Institute for the Unity of Science). These are grouped into numbered folders and are referred to in the notes as "GHP". Documents identified as "LBP" are from the privately held papers of Leo L. Beranek. Finally I would like to thank the Harvard University Archives (HUA) for use of the papers of Sterling Dow. A shorter, earlier version of this essay appeared in the *Deutsche Zeitschrift für Philosophie*.

ENDNOTES

[1]From Carnap's handbook; cited in Holton, "From the Vienna Circle to Harvard Square: The Americanization of a European World Conception," in F. Stadler, ed., *Scientific Philosophy: Origins and Developments* (Boston: Kluwer Academic Publishers, 1993), 47–73.

[2]Holton, "On the Vienna Circle in Exile," in W. DePauli-Schimanovich, E. Köhler and F. Stadler, eds., *The Foundational Debate: Complexity and Constructivity in Mathematics and Physics* (Boston: Kluwer Academic Publishers, 1995), 269–292; Holton, "From the Vienna Circle to Harvard Square." Also, see P. Masani to Holton, 5 April 1991, in which Masani recalls getting Frank, Shapley, LeCorbeiller, Birkhoff, Leontief, Bridgman and Uhlenbeck together for what became the "Inter-Scientific Discussion Group" in "1943 or 1944."

[3]Frank, "The Institute for the Unity of Science: Its Background and Its Purpose." 1946, RG 1.1, 100 Unity of Science, Box 35, Folder 281, RFA.

[4]Wittgenstein himself, as is well known, was often anything but happy with the idea of being grouped together with the Vienna Circle.

[5]Frank, "Background and Purpose."

[6]Frank, "Background and Purpose," 11.

[7]Ibid.

[8][Frank], untitled, 1945–46, folder 4, GHP.

[9]"Interview: WW [Warren Weaver], Friday, December 13, 1946, Professor Philipp G. Frank," RG 1.1, 100 Unity of Science, Box 35, Folder 281, RFA.

[10]Frank to Weaver, 7 January 1947, RG 1.1, 100 Unity of Science, Box 35, Folder, 281, RFA.

[11]Frank to Weaver, 7 January 1947, pp. 7, 42–43, RG 1.1, 100 Unity of Science, Box 35, Folder 281, RFA.

[12]Resolution RF 47131 set a start date of 1 January 1948; this was emended to 1 July 1949 in RF 49085, both in RG 1.1, 100 Unity of Science, Box 35, Folder 281, RFA. The delay was due to the difficulty in establishing the proper tax status for the organization.

[13]"Toward Integration of the Sciences," March 1949, excerpted from "Confidential Report to the Trustees," p. 6, RG 1.1, 100 Unity of Science, Box 35, Folder 281, RFA.

[14]Whitehead, Lowell Lecture 1925, published as *Science and the Modern World* (New York: Macmillan Co., 1937); see also George Sarton, *Introduction to the History of Science* (Baltimore, Md.: published for the Carnegie Institution of Washington by the William and Wilkins Co., 1947–1948). Both cited in "Toward Integration," ibid.

[15]"Toward Integration," 7.

[16]Ibid., 7–8.

[17]Ibid., 11.

[18]Ibid., 13.

[19]Frank to Weaver, 29 September 1952, RG 1.1, 100 Unity of Science, Box 35, Folder 285, RFA.

[20]Ibid.

[21]Morris, *Paths of Life* (New York: Harper and Brothers, 1942).

[22]On Einstein's protest that his work was not properly understood as purely positivistic, see Holton, "Einstein, Mach, and the Search for Reality," in *Thematic Origins of Scientific Thought: Kepler to Einstein*, rev. ed. (Cambridge, Mass.: Harvard University Press, 1988), 261–262.

[23]Steering Committee to participants, 11 January 1951, folder 3, GHP.

[24]Wiener, "Speech, Language, and Learning," in *Journal of the Acoustical Society of America* 22 (1950): 696–697, reprinted in Wiener, *Collected Works*, ed. P. Masani, 4 vols. (Cambridge, Mass.: MIT Press, 1976–), vol. 4, 200–201; original references to Shannon include "A Mathematical Theory of Communication," *Bell Systems Technical Journal* (3) and (427) (1948).

[25]Institute for the Unity of Science, AAAS, 20 January 1953, from "The Steering Committee, to The Cybernetics and Communications Group of the Institute for the Unity of Science," RG 1.1, 100 Unity of Science, Box 35, Folder 285, RFA.

[26]The secondary literature on the history of the Vienna Circle in general and on matters relating to "unity" in particular is vast and growing. For an introduction to the material see Galison and Stump, *The Disunity of Science* (Stanford: Stanford University Press, 1996); Thomas Uebel, ed., *Rediscovering the Forgotten Vienna Circle* (Dordrecht: Kluwer, 1991); F. Stadler, ed., *Scientific*

Philosophy (1993); R. N. Giere and A. Richardson, eds., *The Origins of Logical Empiricism*, Minnesota Studies in the Philosophy of Science, volume 16 (Minneapolis, Minn.: University of Minnesota Press, 1996); and N. Cartwright, J. Cat, L. Fleck, and T. R. Uebel, *Otto Neurath: Philosophy Between Science and Politics* (Cambridge: Cambridge University Press, 1996).

[27]M. Friedman, "Carnap's *Aufbau* Reconsidered," *Noûs* 21 (December 1987): 521–546; and R. Creath, "The Unity of Science: Carnap, Neurath and Beyond," in Galison and Stump, eds., *The Disunity of Science*.

[28]J. Cat, N. Cartwright, and H. Chang, "Otto Neurath: Politics and the Unity of Science," in Galison and Stump, eds., *The Disunity of Science*.

[29]N. Cartwright, et al., *Otto Neurath*, 167–188.

[30]Ibid, 173–74.

[31]Galison, "Ontology of the Enemy," *Critical Inquiry* (1994): 228–266, 234.

[32]Ibid., 240.

[33]Ibid., 247.

[34]Wiener, *Collected Works*, vol. 4, 169–73.

[35]Program Committee of Interscience Discussion Group to participants, 7 January 1947, folder 2, GHP; Herman Goldstine, *The Computer from Pascal to von Neumann* (Princeton: Princeton University Press, 1947), 267.

[36]Boring to Masani, 13 November 1944, folder 1, GHP.

[37]Masani to Wiener, 6 December 1944; Masani to Ducasse, 6 February 1945, both folder 1, GHP.

[38]Steve J. Heims, *The Cybernetic Group* (Cambridge, Mass.: MIT Press, 1991).

[39]See "History of Harvard's War Contracts," in Sterling Dow Papers, HUA, and Dow interview with Eric Arthur Walker, S.D., Associate Director of the Underwater Sound Laboratory, 18 June 1945, Sterling Dow Papers, HUA.

[40]"Statement of War Activities," 1 February 1946, W. Furry folder, Sterling Dow Papers, HUA.

[41]"When asked how the Germans did so well despite their failure to use academic scientists to advantage, Kemble observed that the analytical engineers in Germany came close to beating us, in spite of the fact that they didn't use their pure scientists to advantage. If they had had somewhat more intelligent overall leadership, things might have come out differently. These engineers had originally good academic training." Interview with Edwin Crawford Kemble, 22 June 1945, Kemble File, Sterling Dow Papers, HUA.

[42]Attendance sheets for the early meetings of the Inter-Scientific Discussion Group can be found in GHP, folder 2.

[43]Bridgman, "Two major jobs for OSRD, Others for Watertown Arsenal," 1 May 1945, P. Bridgman folder, Papers of Sterling Dow, HUA.

[44]On Quine's war work, see David Kahn, *Seizing the Enigma* (Boston: Houghton Mifflin Company, 1991), 241 ff.

[45]See, e.g., Program Committee to "Dear Sir" (participants), 24 October 1950, folder 3, GHP.

[46]Beranek, as narrated to S. Dow, "History of Electro-Acoustic Laboratory (Formerly called: Research on Sound Control)," 9, LBP.

[47]Beranek, "Electro-Acoustic Laboratory," ibid.

[48]Beranek, "Our Laboratory During the War," Report Covering Activities Between December 1, 1940, and July 1, 1944, Cruft Laboratory, Harvard University, Cambridge, Massachusetts, 9.

[49]See L. Beranek folder, Papers of Sterling Dow, HUA; Holton, informational communication, 28 March 1996.

[50]"Systems Research Laboratory: Wartime Organization and Purpose of the Systems Research Laboratory, Harvard University," 5, LBP. On the prewar Fatigue Laboratory, see Bard Clifford Cosman, "The Human Factor: The Harvard Fatigue Laboratory and the Transformation of Taylorism," Harvard Senior Thesis, 1983; Richard Gillespie, *Manufacturing Knowledge: A History of the Hawthorne Experiments* (Cambridge: Cambridge University Press, 1991).

[51]Galison, "Aufbau/Bauhaus: Logical Positivism and Architectural Modernism," *Critical Inquiry* 16 (1990): 709–752; Galison, "Constructing Modernism: The Cultural Location of Aufbau," in Giere and Richardson, eds., *Origins of Logical Empiricism*, 17–44.

[52]For a discussion of the fierce struggle between Carnap and Heidegger, see M. Friedman, "Overcoming Metaphysics: Carnap and Heidegger," in ibid., 45–79.

[53]E. Bright Wilson, Jr., to Holton, 6 October 1950, folder 3, GHP.