Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America

EDWIN LAYTON

American technology went through a scientific revolution in the 19th century. Technological knowledge was uprooted from its matrix in centuries-old craft traditions and grafted onto science. The technological community, which in 1800 had been a craft affair but little changed since the middle ages, was reconstructed as a mirror-image twin of the scientific community. The artisan was replaced in the vanguard of technological progress by a new breed of scientific practitioner. For the oral traditions passed from master to apprentice, the new technologist substituted a college education, a professional organization, and a technical literature patterned on those of science. Equivalents were created in technology for the experimental and theoretical branches of science. As a result, by the end of the 19th century, technological problems could be treated as scientific ones; traditional methods and cut-and-try empiricism could be supplemented by powerful tools borrowed from science. This change was most marked in the physical sciences and civil, mechanical, and electrical engineering, the subject of this paper. But similar changes were taking place at the same time in the relations of chemistry, biology, geology, and other sciences to their corresponding technologies. The result might be termed "the scientific revolution in technology."

The significance, indeed the very existence, of the scientific revolution in technology has been obscured by a commonly accepted model of the relationships between science and technology. In essence, this holds that science creates new knowledge which tech-

DR. LAYTON, of the graduate program in the history of science and technology at Case Western Reserve University, is the editor of *A Regional Union Catalog of Manuscripts Relating to the History of Science and Technology Located in Indiana, Michigan, and Ohio,* and the author of *The Revolt of the Engineers: Social Responsibility and the American Engineering Profession.* This paper was originally presented at the symposium on "Science in 19th-Century America," at Northwestern University, 1970.

nologists then apply. Jacob Bigelow articulated a common faith in 1831 when he asserted:

> Our arts have been the arts of science, built up from an acquaintance with principles, and with the relations of cause and effect . . . we have acquired a dominion over the physical and moral world, which nothing but the aid of philosophy could have enabled us to establish. . . . The labor of a hundred artificers is now performed by the operations of a single machine. We traverse the ocean in security, because the arts have furnished us a more unfailing guide than the stars. We accomplish what the ancients only dreamt of in their fables; we ascend above the clouds, and penetrate into the abysses of the ocean.

And he concluded that "the application of philosophy to the arts may be said to have made the world what it is at the present day."[1] But when Bigelow came to enumerate the specific instances in which "philosophy" or science had transformed technology, he noted such inventions as the magnetic compass, the printing press, gunpowder, the clock, glass, the cotton gin, the steam engine, and textile machinery. Yet in none of these cases is the influence of science on technology obvious; certainly in none is their relationship explained satisfactorily by the common model.

That this view of science-technology relations has continued into the 20th century was demonstrated by Vannevar Bush and other architects of America's recent science policies. Bush held that basic research, though undertaken without thought of practical ends, generates knowledge of nature's laws which provides the means of technological progress. He maintained that "basic research leads to new knowledge. It provides scientific capital. It creates the fund from which the practical applications of knowledge must be drawn. New products and new processes do not appear full-grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science." Bush concluded that, "today, it is truer than ever that basic research is the pacemaker of technological progress."[2]

Inspired by Bush's model of the relation of science to technology, the Department of Defense, from 1945 to 1966, invested about $10 billion in scientific research, of which approximately one-quarter went for basic or undirected research. A growing skepticism con-

[1]Jacob Bigelow, *Elements of Technology* (Boston, 1831), p. 4.

[2]Vannevar Bush, *Endless Horizons* (Washington, D.C., 1946), pp. 52-53. See also John R. Steelman, *Science and Public Policy* (Washington, D.C., 1947), pp. 4-5.

cerning the technological value of this enormous expenditure caused the department to undertake an investigation, Project Hindsight. This study took eight years and consumed some forty man-years of time on the part of thirteen teams of scientists and engineers who analyzed the key contributions which had made possible the development of the twenty weapons systems that constituted, in large part, the core of the nation's defense arsenal. Some 700 key contributions or "events" were isolated. They were classified as being either technological or scientific. If the latter, they were further subdivided into basic and applied-science "events."[3]

The preliminary results of Project Hindsight, which were released in November 1966, came as something of a bombshell to the scientific community. Of all "events," 91 percent were technological, only 9 percent were classed as science. Within the latter category 8.7 percent were applied science; only 0.3 percent, or two "events," were due to basic or undirected science.[4] Predictably, the publication of these results produced a spate of indignant letters to the editors of *Science*.[5] Many of these missed the point. The investigators had not sought to show that science has no influence on technology. What they did demonstrate was that the immediate, direct influence has been small; they showed that the traditional model of science-technology relations is in need of revision. To correct the misunderstanding of Project Hindsight, a subsequent study, TRACES, demonstrated the dependence of five recent innovations on prior scientific work. The question, therefore, is not whether science has influenced technology, but rather the precise nature of the interaction.[6]

[3]Chalmers W. Sherwin and Raymond S. Isenson, "Project Hindsight," *Science* 156 (June 23, 1967): 1571-77.

[4]Ibid.; D. S. Greenberg, "'Hindsight': DOD Study Examines Return on Investment in Research," *Science* 154 (November 18, 1966): 872-73; Philip H. Abelson, "Project Hindsight," *Science* 154 (December 2, 1966): 1123.

[5]See the collection of letters in "How Perceptive Is Hindsight?" *Science* 155 (January 27, 1967): 397-98. See also Helen L. Hayes, "Project Hindsight: Basic Research," *Science* 154 (December 23, 1966): 1504; Allen M. Lenchek, "Project Foresight," *Science* 155 (January 13, 1967): 150; Lee Leiserson, "Project Hindsight," *Science* 157 (September 29, 1967): 1512; and Robert M. Lukes, "Masquerade of Undirected Research," *Science* 159 (January 5, 1968): 34.

[6]Illinois Institute of Technology Research Institute, *Technology in Retrospect and Critical Events in Science*, 2 vols. (n. p. [Chicago], 1968). While generally adhering to the traditional model of science-technology relations, the authors of this study noted that "a better understanding needs to be achieved concerning the two-way influence between science and technology. The tracings revealed cases in which mission-oriented research or development effort elicited later nonmission research, which often was found to be crucial to the ultimate innovation" (1:22).

The results of Project Hindsight are surprising only if one assumes the validity of the received model of science-technology relationships. This model is not so much false as misleading. It assumes that science and technology represent different functions performed by the same community. But a fundamental fact is that they constitute different communities, each with its own goals and systems of values. They are, of course, similar in that both deal with matter and energy. But these similarities should not be overstated. Each community has its own social controls—such as its reward system—which tend to focus the work of each on its own needs. These needs determine not only the objects of concern, but the "language" in which they are discussed. These needs may overlap; but it would be surprising if this were a very frequent occurrence. One would expect that in the normal case science would beget more science, and technology would lead to further technology. This is precisely the finding of Project Hindsight.

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The difficulties of the traditional model may be illustrated by the relationship, or lack of one, between Newtonian mechanics and the "golden age" of mechanical invention in America in the 19th century. An enthusiastic group of scientists, technologists, and reformers in America, as in Europe, were attempting to foster the application of science to technology. Among them was James Renwick, professor of natural experimental philosophy and chemistry at Columbia College. He wrote two books that were intended to bridge the gap between art and science. The first, *The Elements of Mechanics,* published in 1832, was a conventional exposition of the science of mechanics. In it Renwick followed a well-trodden path in treating systems in equilibrium by the principle of virtual velocities.[7] The second book, his *Applications of the Science of Mechanics to Practical Purposes,* published in 1842, surveyed the field of mechanical technology, including prime movers, clocks, and various types of machinery.[8] But despite Renwick's earnest efforts, the principles of the first book did not carry over to the second to any significant degree.

A mechanic interested in designing a water wheel would have found the methods and principles of the first book of little value,

[7]James Renwick, *The Elements of Mechanics* (Philadelphia, 1832), p. viii.

[8]James Renwick, *Applications of the Science of Mechanics to Practical Purposes* (New York, 1842). Five of the ten chapters deal with what we would now class as mechanical engineering, two with civil engineering (railroads and canals), one with both civil and mechanical technology (wheels and roads), one with marine engineering, and one with mining.

even if he could have understood them. But the same mechanic would have found valuable assistance in Renwick's second book. John Smeaton, the 18th-century British engineer, had used the experimental methods of science to derive a set of "maxims" or design principles for this type of prime mover. Two of these may be quoted as examples:[9]

> In a given undershot wheel, if the quantity of water expended be given, the useful effect is as the square of the velocity,

and

> In a given undershot wheel, if the aperture whence the water flows be given, the effect is as the cube of the velocity.

Neither could be classed as laws of nature; they were lawlike statements about man-made devices. They were not logical deductions from the science of mechanics; they constituted the germ of a new technological science.

Far from constituting a unity, Renwick's two books pointed to two quite different lines of technological development. Technology might, as suggested by his first book, build directly on the foundations of science. The science of mechanics could be extended to create new, technologically oriented sciences such as the strength of materials and hydraulics. Or, following Smeaton, technologists might borrow the methods of science to found new sciences built on existing craft practices.

To some extent inventors helped to develop technological sciences. Oliver Evans attempted to apply scientific methods to technology in his *The Young Mill-Wright and Miller's Guide* published in 1795. Evans began with a survey of the principles of mechanics, and he was able to derive useful design principles directly from them. But his chief reliance was on the application of scientific methods, rather than deductions from existing laws. He derived a set of "rules" for designing mills, including a critical examination of Smeaton's "maxims." But Evans attempted to go further and he devised a set of rules for making inventions in any field. These amounted to applying scientific methods to technology. Included were the discovery of fundamental principles, making deductions from these principles, and testing the results by experiment.[10]

[9]Renwick, *Applications of the Science of Mechanics,* pp. 49-50; John Smeaton, "An Experimental Enquiry concerning the Natural Powers of Water and Wind to Turn Mills, and Other Machines, Depending on a Circular Motion," *Philosophical Transactions* 51 (1759-60): 118-20. I have retained Renwick's phrasing.

[10]Oliver Evans, *The Young Mill-Wright and Miller's Guide* (Philadelphia, 1795), pp. 1-70, appendixes 1-2.

It is, of course, very difficult to discover which works were read by specific inventors; it is even harder to establish a correlation between particular inventions and prior published information. But it is easy to show that there was a vast increase in the volume of written, more or less systematic technical information available to American inventors in the course of the 19th century.[11] This was part of a worldwide movement that had its origins in the great encyclopedias of the 18th century. Oliver Evans, much of whose inventive career came before 1800, recalled that the chief impediment for the inventor was the lack of reliable published information.[12] By the middle of the 19th century, through the efforts of men like Evans and Renwick, this barrier to invention had been largely removed.

Inventors might apply scientific methods; but despite the work of a few like Evans, the inventor was ill-adapted to the task of building up technological sciences. Scientists, on the other hand, had the necessary skills, and they played a vital role in stimulating the development of engineering sciences. But scientists lacked the lasting commitment and the intimate knowledge of technology and its needs that was required. The bulk of the effort to build technological sciences, therefore, fell on the engineering profession itself.

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The engineering sciences, by 1900, constituted a complex system of knowledge, ranging from highly systematic sciences to collections of "how to do it" rules in engineering handbooks.[13] Some, like the strength of materials and hydraulics, built directly on science; they were often classed as branches of physics. Others, such as the kinematics of mechanisms, evolved from engineering practice. In either case, their development involved the adoption by engineers of the theoretical and experimental methods of science, along with many of

[11]There are two excellent guides to this literature: Eugene S. Ferguson, *Bibliography of the History of Technology* (Cambridge, Mass., 1968); and Brooke Hindle, *Technology in Early America* (Chapel Hill, N.C., 1966).

[12]Eugene S. Ferguson, ed., *Early Engineering Reminiscences of George Escol Sellers* (Washington, D.C., 1965), p. 38.

[13]Hunter Rouse and Simon Ince, *History of Hydraulics* (Iowa City, Ia., 1957); Eugene S. Ferguson, *Kinematics of Mechanism from the Time of Watt* (Washington, D.C., 1962); Stephen P. Timoshenko, *History of Strength of Materials* (New York, 1953); Isaac Todhunter, *A History of the Theory of Elasticity*, 3 vols. (New York, 1960); James H. Potter, ed., *Handbook of the Engineering Sciences*, 2 vols. (Princeton, N.J., 1967) is a useful modern survey. For a view of the engineering sciences similar to the one taken in this paper, see James Kip Finch, "Engineering and Science: A Historical Review and Appraisal," *Technology and Culture* 2 (Fall 1961): 318–32.

the values and institutions associated with their use. By 1900 the point of origin made little difference; the engineering sciences constituted a unity. Those derived from practice took on the qualities of a science in their systematic organization, their reliance on experiment, and in the development of mathematical theory. At the same time, sciences like the strength of materials gradually diverged from physics, assuming the characteristics of an autonomous technological science.

The separation of the engineering sciences from physics may be illustrated by the strength of materials and its sister disciplines, the theory of elasticity and the theory of structures. They were the first of the engineering sciences to be cultivated extensively, both in Europe and America. The reasons for this were twofold. The intractable nature of materials constituted one of the most important barriers to the development of technology. The 19th century saw many new uses for materials like iron and steel; a scientific study of their properties would enable designers to avoid costly failures. But another reason for the early emphasis on this science was that it represented one of the most promising avenues for the application of science to technology. It could draw upon a sophisticated body of physics accumulated since the time of Newton. Thus, it attracted scientists and others inspired by the vision of a scientific technology.

Both in Europe and America scientists played a key role in fostering the development of the science of the strength of materials. But once it was established, technologists dominated its further development, although scientists continued to make important contributions. Scientists such as Hooke, Euler, Young, and Coulomb did much to lay its foundations; it is worth remembering that the second of Galileo's "two new sciences" was the strength of materials. But once it reached the stage of being technologically useful, its development was undertaken by engineers. A critical institutional innovation was the development of engineering colleges in which technology would be pursued in the manner of science. The Ecole Polytechnique was the pioneer, widely imitated both in Europe and America. A group of polytechnicians, notably Louis Marie Navier (1785–1836) and Barrie de Saint-Venant (1797–1886), reformulated and extended this science.[14]

As the strength of materials moved from the community of sci-

[14]Frederick B. Artz, *The Development of Technical Education in France, 1500–1850* (Cambridge, Mass., 1966), pp. 81–86, 151–66, 230–53; Timoshenko, *History of Strength of Materials*, pp. 67–80, 135–41, 229–42.

ence to that of technology, it went through an important transformation. Its ties with physics were weakened, and it developed in ways uncharacteristic of the basic sciences. At the same time, its range of technological usefulness was gradually expanded. Scientists tended to explain their findings by reference to the most fundamental entities, such as atoms, ether, and forces. But these entities cannot always be observed directly. To be useful to a designer, however, a formulation must deal with measurable entities, particularly those of importance to the practical man. These need not be fundamental in the scientific sense. The scientists who had done so much to found a science of the strength of materials—notably Young, Coulomb, and Poisson—strove to found this study on the same ontological basis as classical mechanics—that is, they sought to explain their results in terms of molecules and the forces between them. Although not without interest, these efforts were not wholly successful. They were also needless complications from the technological point of view. A few of the engineers poineering in this field, including Navier and Saint-Venant, continued this quest, but in the end the attempt was abandoned.[15] Instead, engineers were content with a simple macroscopic model—for example, viewing a beam as a bundle of fibers.

In America as in Europe the foundation of the science of strength of materials owed much to scientists. Led by Alexander D. Bache, the Franklin Institute in 1830 undertook an investigation of the causes of steam boiler explosions for the federal government. This study was itself one of the first significant attempts to use scientific methods to investigate technological problems in America.[16] One aspect of this multifaceted effort was a systematic study by Walter R. Johnson of the strength of the metals used in boiler construction. This involved building the first testing machine in America and conducting a well-conceived and highly fruitful series of experimental tests.[17] Scientists also fostered the use of mathematical

[15]Timoshenko, *History of Strength of Materials,* pp. 104–7, 231–32.

[16]Bruce Sinclair, *Early Research at the Franklin Institute, the Investigation into the Causes of Steam Boiler Explosions, 1830–1837* (Philadelphia, 1966); John G. Burke, "Bursting Boilers and the Federal Power," *Technology and Culture* 7 (Winter 1966): 1–23.

[17]"Report of the Committee of the Franklin Institute . . . on the Explosion of Steam Boilers . . . Part II . . . ," *Journal of the Franklin Institute* 19 (February 1837): 73–109; ibid. (March 1837): 156–93; ibid. (April 1837): 241–77; ibid. (May 1837): 28–31; ibid. (June 1837): 409–51; ibid. 20 (July 1837): 1–31; ibid. (August 1837): 72–113. See also George E. Pettengill, "Walter Rogers Johnson," *Journal of the Franklin Institute* 250 (August 1950): 93–113.

theory for the study of materials. William Barton Rogers, although primarily a geologist, was well grounded in physics and mathematics. His *An Elementary Treatise on the Strength of Materials* published in 1838 was the first American book in this field.[18]

But perhaps the greatest contribution of scientists like Rogers was to foster institutions to encourage the marriage of science and technology. Rogers himself was one of the foremost; he had apparently become concerned with technology when he lectured at the Maryland Institute, a Baltimore mechanic's institute, in 1827. His treatise on the strength of materials was produced as part of an effort to found a school of engineering at the University of Virginia. While this venture did not succeed, Rogers did not give up. In 1846 he drew up a plan for a "polytechnic school" for Boston—a dream finally realized in 1861 with the chartering of the Massachusetts Institute of Technology.[19] Rogers was, of course, not alone in his vision of a scientific technology. Led for the most part by chemists and geologists, the scientific schools attached to Harvard, Yale, and other colleges instituted engineering programs. Benjamin F. Green reorganized Rensselaer into a polytechnic school after 1847. It was the first to concentrate almost exclusively on engineering, and the first to go beyond one-man departments in this area—a vital step in encouraging the specialization required for the development of the engineering sciences.[20]

While scientists like Rogers, Bache, Renwick, and Green did much to found the scientific study of materials in America, its systematic development lay principally with the engineers themselves. An important role in this was played by West Point, the first American engineering school. It was reorganized after 1818 by Sylvanus Thayer on the model of the great French engineering schools. One graduate, Dennis Hart Mahan, was sent to France to complete his engineering education; on his return, he taught civil and military engineering to cadets from 1832 to 1871. Mahan produced in 1837 the first American textbook based on French engineering practice, *An Elementary Course of Civil Engineering.* Over 15,000 copies of this work were sold; it had an important impact on the teaching of

[18]William Barton Rogers, *An Elementary Treatise on the Strength of Materials* (Charlottesville, Va., 1838).

[19]Emma Rogers, *Life and Letters of William Barton Rogers,* 2 vols. (Boston, 1896), 1: 40-54, 259-62, 420-27.

[20]Samuel Rezneck, *Education for a Technological Society* (Troy, N.Y., 1968), pp. 78-110; Palmer C. Ricketts, *History of the Rensselaer Polytechnic Institute* (New York, 1895), pp. 69-112.

engineering in America.[21] It included a brief survey of the strength of materials. Although Mahan limited himself to elementary mathematics, his treatment of this subject was distinctly professional, in striking contrast to the purely qualitative discussion of the strength of materials in Bigelow's *Elements of Technology*. It is perhaps significant that Bigelow sought explanations for the properties of materials in molecules and forces between them but Mahan made no reference to these fundamental entities of physics. Mahan had read deeply in the European literature and he particularly recommended the works of Navier to his students. A few apparently followed his advice. West Point engineers did much to establish a tradition of the scientific study of engineering in America.[22]

Mahan's work, and the technical works which followed it, provided a basis for introducing European methods into ordinary engineering practice in America. But creative contributions, the founding of a science, required money for laboratories and equipment as well as men trained to use them. The federal government played an important role in supporting experimental investigations in the second quarter of the 19th century. Federal funds had made possible the Franklin Institute's studies of boiler explosions. The same testing machine was used for another pioneering investigation, the study of the causes of the disastrous explosion of a cannon on the U.S.S. *Princeton*.[23] But the institute lacked the funds for developing a sustained program of research. Army officers, particularly in the Ordnance Department, to some extent filled in the gap. A series of experiments on the strength of cannons by Maj. William Wade and Capt. Thomas Jackson Rodman were among the first American contributions to attract European attention. Wade's testing machine was apparently the second to be built in America?[24] Other ex-

[21]George W. Cullum, "Dennis H. Mahan," *Biographical Register of the Officers and Graduates of the U.S. Military Academy at West Point*, 7 vols. (Boston, 1891), 1:319-25.

[22]Dennis H. Mahan, *An Elementary Course of Civil Engineering* (New York, 1837), pp. vii, 44-53, 86-104; Bigelow, *Elements of Technology*, pp. 43-53.

[23]Lee M. Pearson, "The 'Princeton' and the 'Peacemaker': A Study in Nineteenth-Century Naval Research and Development Procedures," *Technology and Culture* 7 (Spring 1966): 163-83.

[24]U.S. Ordinance Department, *Reports of Experiments on the Strength and Other Properties of Metals for Cannon, with a Description of the Machines for Testing Metals, . . .* (Philadelphia, 1856). On Wade's testing machine, see Chester H. Gibbons, *Materials Testing Machines* (Pittsburgh, 1935), pp. 27-28. For a critique of the work of Wade and Rodman, see Todhunter, *History of the Theory of Elasticity*, 2 (pt. 1): 688-96. For Rodman's later work, see U.S. Ordnance Department, *Reports of Experiments on the Properties of Metals for Cannon, and the Qualities of Cannon Powder . . . by Captain T. J. Rodman* (Boston, 1861). (Hereafter cited as Rodman, *Reports of Experiments*.)

perimental investigations were carried out by John Dahlgren and Benjamin F. Isherwood of the navy.[25] But the government was unwilling to make a long-range commitment for research not directed to an immediate mission. In 1872 the American Society of Civil Engineers requested that the government undertake tests of the properties of American iron and steel. Congress authorized a study and created a board of seven engineers to supervise the work. But the president placed the control of the program with the Ordnance Department, which differed with the civilian engineers. In 1878 Congress turned the testing machine over to the army and dissolved the board. The army refused to cooperate with the civilian engineers on the grounds that they lacked the necessary funds.[26]

Large business ventures were also in a position to undertake scientific studies. The proprietors of Lowell supported James Francis's hydraulic experiments; but for his studies of the strength of cast iron he had to rely on European data.[27] The building of the Eads bridge necessitated the adoption of systematic testing of materials, and this practice gradually spread through the steel industry. But these tests were usually geared to the needs of particular projects.[28] Thus, while business and government did much to encourage the adoption of experimental methods in technology, they were unwilling to carry out basic research on a sustained basis.

What engineering needed was not just short-term studies directed to specific problems, but a broad and continuous program of basic research in laboratories specifically dedicated to developing the engineering sciences. Robert Thurston, one of the founding fathers of mechanical engineering in America, was perhaps the foremost champion of basic research in the engineering sciences. He wanted

[25]Edward William Sloan, *Benjamin Franklin Isherwood, Naval Engineer* (Annapolis, Md., 1965); Clarence S. Peterson, *Admiral John A. Dahlgren, Father of U.S. Naval Ordnance* (New York, 1945). Both Isherwood and Dahlgren were active in applying experimental methods to derive design principles for engineering. Isherwood was concerned with the designs of marine steam engines, of screw propellers, and of other subjects. Dahlgren, along with Rodman, used experiment to design the bottle-shaped cannon used in the Civil War.

[26]Charles W. Hunt, *Historical Sketch of the American Society of Civil Engineers* (New York, 1897), pp. 82–83; William F. Durand, *Robert Henry Thurston* (New York, 1929), pp. 79–81.

[27]James B. Francis, *Lowell Hydraulic Experiments* (Boston, 1855), p. xi; Francis, *On the Strength of Cast-Iron Pillars* (New York, 1865), pp. 1–17. Francis derived a classic set of design principles for turbines in the former work (pp. 44–52).

[28]Carl W. Condit, *American Building Art: The Nineteenth Century* (New York, 1960), pp. 9, 139–140; Gibbons, pp. 31, 34–40.

engineering laboratories established in connection with engineering schools. The rise of research-oriented universities and technical institutes after the Civil War gave him his opportunity. He founded two of the earliest and best-known engineering laboratories in America, at Stevens Institute of Technology and Cornell University. Thurston devised two new testing machines and made important discoveries of the properties of materials. With his massive, three-volume work, *The Materials of Engineering,* the experimental study of the strength of materials reached maturity in America.[29]

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Although the experimental approach to technology was readily adopted in America, theory tended to lag behind. American technologists generally lacked the advanced mathematical training needed to make contributions to a sophisticated field like the theory of elasticity. American engineers also tended to pride themselves on their practicality, and regarded mathematical theory as of little real value. The theoretical approach had to prove its utility to be adopted. The difficulty lay with the limitations of existing theory. Although the strength of materials had developed into a science by the 1830s, the range of application of its theory was limited. Very elegant solutions for a limited number of problems were available; but most problems were not solvable.[30] Many problems were indeterminate; they could not be solved because the number of unknowns was greater than the number of equations. Unfortunately, the indeterminate cases included some of the ones most frequently met in American engineering practice: the continuous beam and the truss bridge.[31]

From the 1830s to the 1870s there was a major effort, both in Europe and America, to extend the range of applicability of the engineering sciences. This effort met with remarkable success; by 1880 it was possible to attack a wide range of problems by mathematical theory. In America much of the effort went into the analysis of truss bridges. Squire Whipple's *An Elementary and Practical Treatise on Bridge Building,* the first version of which appeared in 1847, was a homespun product developed apparently in complete innocence of

[29]Durand, pp. 65–73, 236–40; Robert H. Thurston, "On the Necessity of a Mechanical Laboratory," *Journal of the Franklin Institute* 70 (December 1875): 409–18; Robert H. Thurston, *The Materials of Engineering,* 3 vols. (New York, 1883).

[30]Timoshenko, p. 231.

[31]Condit, pp. 6–9, gives a particularly clear sketch of the development of the strength of materials with emphasis on the problem of indeterminate structures.

European work. Whipple employed no mathematics other than elementary geometry and algebra; he did not use the calculus or even trigonometry. While he expressed his results algebraically, the argument was basically geometrical, giving his work a quaint, 17th-century flavor at times. But Whipple's work was a remarkable achievement. He derived mathematical and graphical methods by which he was able to analyze correctly truss bridges which were indeterminate by the usual methods.[32]

A second American effort to establish a mathematical theory for bridges was that of Herman Haupt, whose *General Theory of Bridge Construction* appeared in 1853. A West Point graduate, Haupt had some familiarity with European theory. The British scientist Thomas Young, upon whose work Haupt relied heavily, assumed, like many scientists, that stresses were ultimately reducible to forces between particles. On this assumption, Haupt sought to resolve the forces operating on a beam to a single resultant force acting at the center of an equivalent geometric figure. Unfortunately, stresses are not forces and they cannot be combined in this manner. Although Haupt's assumptions were open to question, his approximations were doubtlessly a vast improvement over the rule-of-thumb methods still in general use. A correct theory of stresses, developed at about the same time by European engineers, did much to further the separation between engineering sciences and physics. It was no longer helpful to attempt to base this engineering science on the fundamental assumptions of physics, atoms, and forces.[33]

Whipple's and Haupt's use of graphical methods to extend the range of engineering science was prophetic of one of principal lines of advance in the science of the strength of materials. In 1866 the Swiss engineer Karl Culmann developed an important graphical

[32]Squire Whipple, *An Elementary and Practical Treatise on Bridge Building,* 4th ed. (New York, 1883). See also Squire Whipple, "On Truss Bridge Building," *Transactions of the American Society of Civil Engineers* 1 (1868–71): 239–44.

[33]Herman Haupt, *General Theory of Bridge Construction* (New York, 1853). Haupt's assumption was that "the weight of any body may be supposed concentrated at its center of gravity; and, in general, any number of parallel forces may be replaced by a single force called the resultant. In the present case . . . the sum of all the forces upon the fibres . . . will be the same, as if a single force equal to its area was applied in the direction of a line passing through its center of gravity" (p. 20). Each normal stress is always accompanied by two components of shearing stress acting at right angles. These cannot be combined by a parallelogram of forces to give a single resultant. In modern terms, forces behave like vectors, but stresses behave like tensors. See also Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts* (London, 1807), pp. 135–52.

method in which stresses were represented by segments of circles. Culmann's use of circle diagrams was extended by the German engineer Otto Mohr and others, resulting in a great increase in the range of usefulness of the strength of materials.[34] The development of graduate-level work at American universities after the Civil War produced engineers who had the training to develop and apply methods of mathematical theory to materials. Henry Turner Eddy, a graduate of Yale's Sheffield Scientific School who received his Ph.D. from Cornell, was among the first of this new generation of scientific technologists. In 1878 he published an extension of the new graphical methods in his *Researches in Graphical Statics*. It was one of the first American engineering books to be translated into German; Florian Cajori, the historian of American mathematics, called it "the first original work on this subject by an American writer."[35]

The expansion of the range of application of the engineering sciences was accompanied by a tendency away from analytic solutions, a reliance on approximations, and, to some extent, a lessening of mathematical rigor. A given problem in the strength of materials might be solved rigorously by the theory of elasticity or it might be treated by less rigorous graphical methods. American engineers, beginning with Rodman, pioneered still less rigorous empirical methods using strain gauges and models. The selection of technique depended on economic as well as technical factors, since rigorous treatment, when possible, often involved more time and effort. The development of hierarchies of methods of variable rigor, along with the importance of economic factors in determining their use, served to distinguish the engineering sciences from physics where only the most rigorous methods were normally admitted.[36]

By 1900 the American technological community was well on the way to becoming a mirror-image twin of the scientific community.

[34]Timoshenko, pp. 190–97, 283–88. See also Hans Straub, *A History of Civil Engineering* (Cambridge, Mass., 1964), pp. 197–202. Not all of the changes were in the direction of lessening rigor; Saint-Venant for one was opposed and his development of the "semi-inverse" method extended rigorous analytic solutions.

[35]Florian Cajori, *The Teaching and History of Mathematics in the United States* (Washington, D.C., 1890), p. 177; Henry Turner Eddy, *Researches in Graphical Statics* (New York, 1878); idem., *Neue Constructionen aus der Graphischen Statik* (Leipzig, 1880); Arthur E. Haynes, "Henry Turner Eddy," *Minnesota Engineer* 20 (March 1912): 104–7; *Dictionary of American Biography*, s. v. "Henry Turner Eddy."

[36]Eddy's and Mohr's methods rested on rigorous mathematical proofs. But graphical methods usually involved simplifying assumptions about the distribution of stresses. For Rodman's pressure gauge, see Rodman, *Reports of Experiments*, pp. 299–300 (see n. 24 above).

The rise of engineering sciences had played a vital role. They gave technology equivalents to the theoretical and experimental departments of physical science. They were fostered by engineering colleges which, by 1900, had virtually displaced apprenticeship as a means of training engineers. Scientifically inclined engineers like Thurston played an important role in the founding of professional engineering societies after the Civil War, and an even more important role in producing worthwhile technical literature for engineering journals to publish. But despite the structural similarities between science and technology, the two were further apart in some respects. In many important areas engineering and physics had ceased to speak the same language.

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In the case of mirror-image twins there is a subtle but irreconcilable difference which is expressed as a change in parity. Between the communities of science and technology there was a switch in values analogous to a change in parity. One way of putting the matter would be to note that while the two communities shared many of the same values, they reversed their rank order. In the physical sciences the highest prestige went to the most abstract and general—that is to the mathematical theorists from Newton to Einstein. Instrumentation and applications generally ranked lowest. In the technological community the successful designer or builder ranked highest, the "mere" theorist the lowest. These differences are inherent in the ends pursued by the two communities: scientists seek to know, technologists to do. These values influence not only the status of occupational specialists, but the nature of the work done and the "language" in which that work is expressed.

An indication of the gap between science and technology is provided by two discoveries, one by Henry Rowland the American physicist and the other by Francis Hopkinson, a British electrical engineer. Rowland, starting from an idea of Faraday, published a paper on magnetic permeability in 1873. James Clerk Maxwell, to whom Rowland sent the paper, recognized its importance, and arranged to have it published in *Philosophical Magazine.* Hopkinson in 1879 published the results of his investigation of the efficiency of electric dynamos. By graphing his results, he discovered the "characteristic curve" of the direct-current dynamo, a vital key to rational design. Hopkinson could show, for example, how the Edison dynamo could be radically improved by simply changing the dimensions of some of its parts. It was not discovered until several years later that, in a certain sense, Rowland and Hopkinson had made the same discovery.[37]

There was an irony in the fact that Rowland had "discovered" a key to the design of electric dynamos without realizing it. For Rowland's only earned degree was in engineering; and while he had transferred his primary loyalty to physics, his laboratory at Johns Hopkins was an important center for the training of electrical engineers. Rowland missed the significance of his discovery because he was looking for a law of nature, not a design principle. Each man expressed his work in the terms appropriate to his quest; Rowland discovered a relation between the entities of electromagnetic theory; Hopkinson between basic engineering parameters, such as the input and output of a dynamo. The method of approach, the argument, and the form of presentation differed, each according to its purpose and the audience for which it was intended. The two might be considered equivalent because the engineering variables of Hopkinson could be expressed as functions of the electromagnetic entities employed by Rowland.[38]

Perhaps no scientist has had a greater impact on technology than James Clerk Maxwell. But his influence was indirect, since few engineers could understand him. It required a creative effort almost equal to Maxwell's own by the British engineer Oliver Heaviside to translate his electromagnetic equations into a form usable by engineers.[39] Yet Maxwell was one of those scientists who consciously attempted to contribute to technology. Thus, he developed an important method for solving indeterminate problems in the theory of structures. But this work, too, had to be "translated" for technologists. A British engineer, after quoting Maxwell's conclusions, commented that "few engineers would, however, suspect that the two paragraphs quoted put at their disposal a remarkably simple and accurate method of calculating the stresses in a framework."[40]

The cases of Rowland and Maxwell suggest how the interchange between science and technology may take place. For information to pass from one community to the other often involves extensive

[37]James E. Brittain, "B. A. Behrend and the Beginnings of Electrical Engineering, 1870-1920" (Ph.D. diss., Case Western Reserve University, 1969), pp. 6-18.

[38]Ibid., p. 41.

[39]James E. Brittain, "Heaviside and the Telephone: A Case Study of the Interaction of Science and Technology in Nineteenth Century Telephony" (Master's essay, Case Western Reserve University, 1968), pp. 1-3, 7-14. See also Oliver Heaviside, *Electrical Papers*, 2 vols. (Boston, 1925).

[40]Quoted in Timoshenko, p. 203.

reformulation and an act of creative insight. This requires men who are in some sense members of both communities. These intermediaries might be called "engineer-scientists" or "scientist-engineers," depending on whether their primary identification is with engineering or science. Such men play a very important role as channels of communication between the communities of science and technology. It is perhaps significant that of American physical scientists of the 19th century, Joseph Henry, Alexander D. Bache, Henry Rowland, and J. Willard Gibbs were all trained as engineers. Administrators of scientific agencies of government and those engaged in teaching science to engineers would be more effective if capable of understanding and reconciling the competing demands of science and technology.

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It is worth noting, however, that the relationship between science and technology is a symmetric one. That is, information can be transferred in either direction. The flow of technology into science in the form of instrumentation has long been recognized; but the traditional model does not provide for the possibility that technological theory might influence science. Yet the rise of engineering sciences such as the theory of elasticity and hydrodynamics did have an influence on science. The theory of elasticity provided a means of constructing models of the ether, a favorite occupation of Lord Kelvin, among others. The maturing of hydrodynamics was one cause of the proliferation of vortex theories of matter in the second half of the 19th century. Thermodynamics presents a somewhat more complex interaction. This science began with the French engineer Carnot as a design principle. It was not a law of nature but a statement of the limits of the efficiency of the steam engine. Its development relied on a simple macroscopic model—Carnot's ideal heat engine; it did not rely on molecular hypotheses. It was discovered in the engineering literature by scientist-engineers like Kelvin, Rankine, and Helmholtz, and translated by them into the language of science. As thermodynamics was absorbed by physics, Carnot's ideal heat engine tended to be replaced by the molecular model of statistical mechanics.

Because of the status differentials, one would expect engineers with the appropriate training to attempt some work directed at the scientific community. Theory ranks high in science but low in engineering. Many examples of American engineers contributing to basic science could be cited. De Volson Wood used elasticity considerations in an attempt to determine the density, pressure, and specific heat of the ether. Although one of the weakest of his works, Wood apparently took inordinate pride in it since he published an expanded version as a book. Henry Turner Eddy did some interesting papers in which he concluded from kinetic considerations that the atom must have some form of internal motion and he postulated the existence of a subatomic particle.[41] The only earned degrees of J. Willard Gibbs and Henry Rowland were in engineering. Considering the low status of academic theorists in engineering, not to mention the low status of engineers as a group, their identification with physics is not surprising.

The most important influence of technology on scientific ideas, however, was more indirect. Engineering sciences did not postulate unobservables. Their example was, therefore, a challenge to physics. They contributed to the critical reexamination of the foundations of physics which took place in the latter 19th century. But the engineers themselves contributed little to this movement; it was carried forward by physicists under the banners of positivism and energeticism. The influence of technology on science, like that of science on technology, was an indirect, "second-order" effect.

The coupling of science and technology in the 19th century had at least two important social consequences in the twentieth. It accelerated the pace of technological change, and consequently of social dislocation. It also encouraged engineers to adopt a self-image based on science which served to discourage them from assisting society in meeting the problems they had done so much to create. The scientific self-image caused engineers to portray themselves as logical thinkers, free of all bias and emotion, and it promoted a sort of "above-the-battle" neutrality on the part of the profession. Although engineers gave lip service to the idea of social responsibility, their definition of this responsibility served to prevent effective action. When faced with an actual social problem, engineers have sought objective or "scientific" solutions. In practice, this made the discovery of methods of social engineering a precondition to social action, thus substituting an impossible task for a difficult one. The delusive quest for social engineering led more than one engineer down the blind alley of technocracy.

The reversal of "parity" between science and technology further

[41]David R. Topper, "The Development of the Kinetic Theory of Gases in America: An Analysis of the Ideas of Three Key American Figures prior to Gibbs" (Master's essay, Case Western Reserve University, 1968), pp. 35–62; De Volson Wood, *The Luminiferous Aether* (New York, 1886).

reduced the engineers' ability to respond effectively to social problems. The scientific community has been better able to act on social issues because those with the greatest prestige were in universities where they were relatively free from pressures from corporations and government. The engineers who enjoyed a corresponding independence lacked sufficient prestige to lead their profession. Prestige and power in engineering went to the "doers," not the "theorists." This had the practical effect of giving the control of the engineering profession to men who were linked by ties of self-interest to those who were using, and in some cases, misusing technology. This conflict in interest between the leaders of the profession and rank-and-file engineers did much to frustrate the legitimate professional aspirations of American engineers.[42]

[42]The author's *The Revolt of the Engineers: Social Responsibility and the American Engineering Profession* (Cleveland, 1971) deals with the engineering profession's concern for social responsibility.