Edison and the Pure Science Ideal in 19th-Century America

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Summary. Between 1878 and 1882, key members of the American scientific community played an important role in Thomas A. Edison's work on electric lighting. Impressed by his abilities, these scientists came to regard Edison as a peer and led him to see himself as a scientific man. But Edison's high standing among scientists and the American public and his professed self-image as a scientist provoked America's noted experimental physicist, Henry A. Rowland, to make a "Plea for pure science" before the American Association for the Advancement of Science in 1883.

Although 1979, the centennial of electric lighting, witnessed numerous publications and conferences concerning Thomas A. Edison, his inventions, and his place in the history of American technology, his relationship with the American scientific community in the critical years of the development of his lighting system was largely ignored (*1*, *2*). This relationship and Edison's conception of science are crucial in understanding not only Edison's successful development of electric lighting but also the state of American science in the late 1870's and early 1880's.

Key members of the American scientific community played an important role in Edison's work on electric lighting. Greatly impressed by his abilities, these scientists came to regard Edison as a peer. So, despite an earlier, unpleasant disagreement with scientists, Edison began to see himself as a "scientific man." But the attention accorded Edison by an important segment of the American scientific community (not to mention by a hero-worshipping American public) and Edison's claim to be a scientist provoked Henry A. Rowland, the noted experimental physicist at Johns Hopkins University, to lash out at the "Age of Edison" by making his celebrated "Plea for pure science" (*3*) at the 1883 meeting of the American Association for the Advancement of Science (AAAS).

Although the pure science ideal began to emerge in the mid-1850's in America, Rowland's address brought the ideal into sharp relief with dominant notions of science and utility. For this reason, historians of American science have long regarded Rowland's "Plea for pure science" as the ne plus ultra of pure science rhetoric in the 19th century (*4*-*9*). "American science is a thing of the future," Rowland argued, "and not of the present or past. . . ." Moreover, he went on (*3*, p. 594),

> The proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences, by the name of science. I do not wish to underrate the value of all these things; the progress of the world depends on them, and he is to be honored who cultivates them successfully. So also the cook who invents a new and palatable dish for the table benefits the world to a certain degree; yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the applications of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity.

Rowland even suggested that scientific societies such as the AAAS were responsible for the state of American science (*3*, p. 609):

> When the average tone of the [scientific] society is low, when the highest honors are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind, a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill, and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavor to grow.

The rancor of Rowland's remarks may be fully understood only in the context of Edison's relationship with the physicist (as revealed by surviving correspondence between the two) and with other, key members of the American scientific community (*10*). When these relationships are understood, it becomes clear that Edison provoked Rowland's polemic in particular and the heightened pure science rhetoric of the 1800's in general. Thus while Edison's inventions and his early version of an industrial research and development laboratory (*11*) brought about great progress in American technology, his encounter with the American scientific community in the late 1870's and early 1880's helped to shape a powerful reactionary ideology that resulted in the dichotomy between pure and applied science.

Throughout the 1870's and early 1880's, Edison's vital link with the American scientific community was George F. Barker, professor of physics at the University of Pennsylvania, associate editor of the *American Journal of Science*, member of the National Academy of Sciences (NAS), and president in 1879 of the AAAS. Barker was a crucial factor in Edison's decision of 1878 to devote his efforts to the development of electric lighting. Edison made this decision after a trip he and Barker made—at Barker's invitation—to Wyoming to observe with other scientists a total eclipse of the sun. During the trip, they discussed the problem of electric lighting at length, and when the two returned to the East, Barker arranged for Edison to see a dynamo and arc light built by Moses Farmer and William Wallace. The physicist also accompanied the inventor on his visit to Wallace's manufactory in Connecticut (*12*, *13*).

Edison and Barker had known each other since about 1874, when Barker was a member of the board of managers of the Franklin Institute. That year Edison exhibited an electrical device at the Franklin Institute that caught Barker's attention and led him to invite Edison to

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0036-8075/80/0208-0612$01.50/0

demonstrate his invention before, in Barker's words, the "highest scientific body in the country, the National Academy of Sciences" (*14*). The NAS met that year in Philadelphia, and Edison, for the first time, was present.

It is clear that by 1877 Barker and Edison had established a cordial relationship. Barker often wrote Edison, asking the inventor "to fit him out" with a variety of inventions for his lectures (*15*). More than once Barker asked for Edison's assistance in giving his lectures (*16*) (it is unclear whether Edison ever agreed). In March 1878, Barker wrote E. H. Johnson, Edison's business lieutenant, saying that he and Edison had been "talking all day on the telephone from the University [of Pennsylvania] to Menlo Park" (*17*), and the following month Barker championed Edison's work in a paper given at a Washington meeting of the NAS (*18*). Edison heard this paper, for he had again been invited (probably at Barker's instigation) by the NAS to demonstrate his latest invention, the phonograph. Just before the meeting, Edison had taken his machine (*19*) to the Smithsonian Institution, where he had been called to demonstrate it to a select group of scientists by Joseph Henry, the first Secretary of the Institution and elder statesman of American science.

Although Edison was now being praised by the scientific community, earlier he was a target of criticism. In 1875 Edison thought he had discovered a new force, which he called etheric force. He described his discovery in his laboratory notebook: "In experimenting with a vibrator magnet consisting of a bar of Stubbs steel fastened at one end and made to vibrate by means of a magnet, I was astonished to see peculiarly bright, scintillating sparks issuing from the core of the magnet" (*12*, p. 127). Edison experimented with this phenomenon and soon concluded that he had discovered "a true unknown force" (*12*, p. 128). Rather than investigate this phenomenon thoroughly, consult reputable scientists, and then publish his findings in a scientific journal, Edison immediately disclosed his new force to the world through the New York daily newspapers.

Edison's action suggests that he was not conscious of what had become a norm of scientific practice in America: science and scientific debate were not to be conducted through the public press (*20*). But Edison turned impulsively to journalists throughout his career (*21*). Occasionally he would follow up his news statements with a presentation before a more scientific body. In the case of etheric force, Edison made such an appearance before a quasi-scientific organization in New York (*22*). Apparently this presentation was a disaster. The inventor's newspaper pronouncement touched off a flurry of dispute throughout the world, with some scientists arguing that the observed phenomenon—possibly the transmission of electromagnetic waves—could be accounted for by known scientific principles. Matthew Josephson suggests that this debate, especially his appearance before the New York organization, prompted the inventor to develop an overt "contempt for all mathematicians and physicists" (*12*, p. 136).

Thus at a time when Edison's wounds might have caused him to avoid anyone who called himself a scientist, Barker befriended him and sought to bind him to the scientific community. In 1878, Barker prepared for Edison a paper on the carbon telephone transmitter and invited him to read it at the St. Louis meeting of the AAAS in August of that year (*23*); during the meeting he even convinced Edison to join the AAAS (*24*). As president of the AAAS in 1879, Barker played an instrumental role in getting Edison and his assistant Francis R. Upton to give papers associated with their work in electric lighting at the annual meeting in Saratoga, New York (*25*). That Barker would write a paper for Edison about the telephone for presentation at one AAAS meeting and encourage the inventor to present a paper on electric lighting at the next meeting suggests that the physicist himself tended to place a telephone or an electric light under the name of science. As president of the AAAS, Barker must have believed that the membership would also find scientific merit in electric lighting. Most important, Barker must have convinced Edison that electric lighting was science (*26*).

Edison's association with Upton reinforced this view. At his financial backer's suggestion, Edison hired Upton late in 1878 to aid in the development of electric lighting. Upton had recently earned a master's degree in physics from Princeton University and had spent a year working in Herman von Helmholtz's laboratory at the University of Berlin, the sine qua non for the American physicist. Before Upton took up residence at Edison's Menlo Park laboratory, he conducted a literature search in Boston and New York, for, as he wrote his father, "everything that . . . might contain any hint as to the Electric light" (*27*). In his work on electric lighting, Edison would depend heavily on Upton's scientific abilities, both theoretical and experimental (*28*).

While one may wonder what would have led an academically trained physicist to the workshop of an embattled inventor, a survey of the contemporary scientific literature suggests that electric lighting was a scientifically respectable—even fashionable—topic during the late 1870's and the first half of the 1880's (*29*). Certainly the problems were fascinating, as is clear from the letters Upton wrote to his father during this period. Upton repeatedly told his father that from Edison and electric lighting he was learning more about physics than ever before (*30*). Moreover, he was excited by a unique opportunity to share his and Edison's knowledge with the world: soon after he arrived at Menlo Park, Edison encouraged Upton to write papers (scientific, technical, and even popular) on Edison's work with electric lighting, a challenge to which Upton quickly responded (*31*).

Barker's invitation to Upton and Edison to read papers at the 1879 meeting of the AAAS gave Upton his first opportunity to present the results of their research on electric lighting before a scientific body. Although Edison attended part of the meeting, Upton presented (and probably wrote) Edison's paper "On the phenomena of heating metals in vacuo by means of an electric current" (*32*). Upton also read his own paper, "Methods for testing Faradic machines," in which he had originally planned to reveal the results of his tests on the output and efficiency of the new Edison dynamo that had recently been developed by Edison's team in Menlo Park. Indeed, as he wrote to his father, he had planned to publish his results prior to the meeting (*33*). But when Upton actually read his paper, he noted that "no tests are given on Mr. Edison's [dynamo electric] machines since he does not wish to put on record what he knows are experimental results" (*34*). Upton and Edison thus violated another of the norms of scientific conduct that, as George Daniels argues, had been established in America since the 1850's: "a full and free exchange of knowledge, without regard to personal considerations" (*8*, p. 157). At this level, commercial technology could never be fully regarded as science without seriously violating the norms of science. Apparently Barker never considered such problems, and continued to encourage Edison to regard his work as having a purely scientific character.

Almost from the moment Edison addressed the problem of electric lighting, he made continual pronouncements in the popular press about his successful

strides in the field and about the performance of particular components of his system of electric lighting. These claims repeatedly drew criticism from a variety of individuals, almost all of whom suggested in strong terms that Edison simply did not know his science and was claiming more than well-known physical principles would allow (*35*). Throughout 1878 and much of 1879, Edison's claims often did exceed his carefully guarded results. But when his dynamo reached a critical stage of perfection in the fall of 1879 and when the Menlo Park crew perfected the troublesome incandescent lamp, the performance of these devices for once matched the published claims. Still, however, Edison was criticized by many for making extravagant claims and for being scientifically illiterate. For instance, when Upton claimed, in a full-page article on Edison's dynamo (*36*), that the generator achieved an efficiency of 90 percent, at least three writers criticized Edison (and Upton) by appealing to "scientific" facts (*37*). Edison's announcement about his perfected electric lamp, made initially in the New York *Herald* (21 December 1879) and subsequently in *Scientific American* (10 January 1880), resulted in an equal or even greater amount of disbelief and criticism in the name of science (*38*).

Faced with such extensive debate about the scientific merits of his system of electric lighting, and given his intense desire to commercialize his system as soon as possible, Edison marshaled expert support for his claims from select scientists. Edison's relationship with Barker proved to be crucial to the ultimate success of this endeavor. Although it is unclear whether Barker volunteered his services or whether Edison requested them, Barker nevertheless teamed up with Henry Rowland to test Edison's devices, thereby scientifically authenticating his claims about the efficiency of his lighting system. Evidently, Barker was confident that Edison's claims would be borne out. Shortly before he and Rowland performed the tests, Barker candidly told a visitor that he admired Edison "not only for an inventive genius but for his scientific ability." Moreover, he admitted that he was "not scientific enough to catch Edison trifling" (*39*). In any case, Barker must have been enthusiastic about making the tests, and no doubt his editorial position made it possible for the results, which vindicated Edison, to be published promptly (*40*). Barker also managed to get another "independent" study of Edison's devices (performed by two Princeton physicists) published in the next issue of the same journal, which again verified Edison's claims (*41*).

These are but two examples of Barker's willingness to consider the latest technology as science (*42*). But Barker was not the only American scientist to find fascination in electric lighting. It is clear from the correspondence of scientists such as Barker, Rowland, and Henry Draper (an outstanding physicist) and of inventors such as Edison and Farmer that the American scientific community shared Barker's enthusiasm about telephones and electric lights.

A meeting of the NAS in New York in late 1880 provides an excellent example of this enthusiasm. During the final planning of this meeting, which took place at Draper's famous laboratory, O. C. Marsh, a member of the NAS from Yale, informed Draper that "[William B.] Rogers writes me that I must take charge of the New York Meeting, as he cannot attend. I hope you will help me out of my responsibility by looking after some of the physicists who will give us something good. Cannot you secure Edison?" (*43*). By implication, an appearance by Edison at a NAS meeting ensured success and "something good" in physics.

Draper was well aware of this. Given the idea by Marsh and supported by Barker, he thought it imperative that the NAS have Edison, or at least his scientific novelties, on hand. Therefore he asked, through Barker, that Edison "light up" his new laboratory with the inventor's latest electric light (*44*). Saddled by developmental problems with the electric light, Edison put off Barker's request to loan Draper the necessary equipment until it was too late, and, if we are to believe Thomas Edison, Barker lost his temper over it (*45*). Subsequently, Barker made several comments to a reporter for the New York *Post* comparing Edison's lamp with those of Hiram Maxim and Joseph Swan. The article resulting from this interview suggested that Barker considered Edison's incandescent lamp similar to Swan's, which had been invented 20 years earlier. Moreover, according to the article, Maxim's lamp greatly impressed Barker—he even suggested that it was superior to Edison's lamp and entirely new (*46*). (It was Maxim's lamp that was demonstrated at the NAS meeting.)

Furious, Edison wrote Barker (*47*), asking him if the *Post*'s account of what he had said were indeed true. Barker responded with a letter (*48*) that Edison regarded as "a sorry attempt at hedging" (*46*). Arguing that the article was "only another example of the worthlessness of newspaper reports," Barker gave his own version (*48*):

> On Friday the 19th [of November 1880], after the adjournment of the Academy [meeting], a reporter for the *Post* asked me what I thought of Mr. Maxim's lamp. I replied that I was of the opinion that Mr. M's improvement in carbons was a great step in advance. That my friendship for Mr. Edison led me to regret that he had not been the one to hit upon the new method. That Profs. Draper, Morton, and myself had tested the lamp to our satisfaction and had obtained 650 candles by measurement from one of these carbon loops. I believe this is the substance of what I told him. He took no notes but wrote it out from memory evidently; and a poor memory at that.

Obviously, Barker did not question the propriety of making such comments in the public press. Yet he was well aware of the propensity of the press to distort, misquote, or misrepresent statements. Earlier, he had cautioned Edison about the inventor's press practices: "Excuse me, but really I think your reputation is being lessened by the foolishness of the men whom you allow to write you up" (*49*). Barker may well have recalled these words when trying to explain the *Post* article to Edison.

Edison chose to vent his anger at Barker in an atypical way. He sent Henry Rowland a copy of the *Post* article and a letter in which he told the physicist that the article "showed the true Scientific spirit" and that "the statements . . . are generally absolutely false" (*50*). Perhaps Edison was suggesting that the true spirit of science was either opportunism or traitorism.

Three weeks later, Edison was still steaming. He wrote another letter to Rowland, vividly detailing the falseness of Barker's statements and Barker's general misunderstanding of his electric lighting system. He added that "Barker is now affiliated with the Maxim Co. . . . and I can only expect bitter enimity [sic]" (*51*).

Although he did not substantiate his claim that Barker was now an affiliate of Maxim, Edison carefully if ungrammatically explained his charge that Barker misunderstood his system (*52*):

> You know to effect a commercial subdivision of the Electric light that the multiple arc system is only one permissible for many reasons & to render this available the lamps must have a high resistance. The amount of money invested in copper being directly dependent upon the resistance of the lamps, besides the necessity of low resistance Dynamo station appliances & house wires, my greatest efforts have been to obtain a lamp of the highest resistance and one which will last the longest at such resistance. The high electromotive forces necessary with high resistance lamp

has made this a terrible job but I have worked until I have 160 ohm lamp whose life will average over 300 hours. Now comes in Barker . . . and announces that the greatest advance in electric lighting has been made by Maxim because he exhibited . . . small lamps of 5 ohms resistances which gave 2 & 300 candle power, not knowing that the whole thing was a question of electromotive force.

Edison marshaled other evidence to demonstrate what he considered Barker's ignorance of electric lighting (*53*).

Edison closed with a long postscript that is the single most revealing statement of Edison's self-image as a scientist (*54*):

> Have you noticed lately the utter indiference [sic] of the technical press in giving credit of scientific work to "previous or first publication and public exhibition"[?]. In England a man named Swan has arisen, made a paper lamp of 100 ohms resistance in glass vacuum exactly in shape & in every detail like mine & claims to have done it 20 years ago, has delivered a lecture before the Soc. Tel. Engrs., is highly complimented for his great contribution to the subdivision of the Electric light, etc. Most all the technical press claim it as English; only one makes the remark that it would be interesting to know where Mr. Swan's labors may be found in printed form previous to my publication & Exhibitions. The Daily newspaper press on the other hand say that Swan only exhibited my lamp. *There won't be much protection for a scientific man if his previous publications and exhibition counts for nothing* [italics added].

There can be no question that Edison, at this moment in his career and despite any earlier contempt for science and its practitioners, regarded himself as a scientific man. This certainly helps to explain why, in 1880, Edison began to publish a weekly journal named *Science*, the predecessor of the present journal (*55*).

Rowland's response to Edison's letter is not known (*56*). Perhaps he found the Barker affair and Edison's self-image amusing; perhaps he sympathized with the inventor in all respects. Rowland may have harbored contempt for Barker for embroiling himself in such a controversy and for Edison for regarding his work as scientific. Several other factors involving both Barker and Edison came into play, however, before Rowland let loose in 1883 with his "Plea for pure science."

One such factor may have arisen from Edison's desire to obtain for Rowland a patent on a dynamo armature (*57*). Through conversation with Rowland in 1880, Edison learned that the physicist had built a "magneto machine" some years before. When Werner Siemens applied for a U.S. patent on the dynamo in 1880, Edison, whose own dynamo was much inspired by Siemens', sought desperately to find a way of overcoming Siemens' priority. He wrote to Rowland, "I want to defeat his patent on the ground either that he was not the first inventor, or [that it was in] use for two years previous to his application" (*58*). Edison added that if Rowland would send him a model or a description of his electrical machine, he would file an application for the physicist, pay all costs associated with obtaining a patent, and give Rowland half the profits associated with the patent. In essence, Edison wished only for Rowland to sign his name.

Initially Rowland was sanguine about Edison's proposition, for, as he wrote the president of Johns Hopkins University, he saw it as an opportunity to "at least defeat Siemens and establish my claim to have invented the machine first." Moreover, he thought there was a small chance that a patent would produce enough money for him "to build my own laboratory" (*59*). Although excited and cooperative at first, Rowland eventually became hostile to the idea, particularly as Edison's fame grew to heroic proportions. Early in 1883, Rowland squelched Edison's ploy to defeat Siemens' patent application after Rowland's initial application was rejected. The physicist was unwilling to sign an oath stating the exact date of his invention and to enter into patent litigation (*60*). He seems to have felt that a pure scientist ought not to have to do such things to prosper in America.

A more important factor that may have contributed to Rowland's provocation occurred in connection with the 1881 International Electrical Exhibition in Paris. Edison and his associates regarded the exhibition as an important step toward commercialization of the Edison system, for the judgment of the experts in Paris would largely determine the winners in the struggle to capture the market. As it turned out, Edison achieved everything he could have hoped for at the exhibition, but he may have lost the support of Rowland in the process.

George Barker served as a U.S. commissioner to the exhibition while Rowland attended the International Congress of Electricians as a U.S. delegate. However, Rowland spent a lot of time at the exhibition, and he must have been in contact with Barker (*61*). He may also have been aware of Barker's general activities in Paris; perhaps he was kept informed by Grosvenor P. Lowrey, Edison's Wall Street go-between who was largely responsible for an Edison-Barker rapprochement. Lowrey, well aware of Barker's status in the America scientific community, attempted to reestablish ties between him and Edison in Paris. Lowrey sought Barker out and had a heart-to-heart talk with the physicist. Afterwards, he wrote Edison:

> He [Barker] has in most respects satisfied me; in one or two not quite, but the explanation is one that I cannot gainsay [deny]. He is now definitely retained by me, by a conversation I had with him the day before yesterday, after the termination of his duties as juryman, to be for Edison, day & night, at all times in all countries, and against all persons. There will be a retaining fee to pay him when he gets home, and he will bring you a lot of figures and information concerning everything in the Exhibition, which will be very useful. I shall pay him £100 here for a few days consultation in England with Johnson and our lawyers. Bailey will also pay him a small fee for a report here. On the whole he will not do so badly as he will expect to have considerable employment with us (*62*).

To put it bluntly, Lowrey bought Barker off. Judging from records of 1889, Barker did well indeed. He received a $500 retainer annually and was guaranteed $50 per day for a minimum of 100 days of his services as a consultant (*63*). Thus Barker's proceeds from being "for" Edison were roughly $5500 per year—probably more than his university salary.

Lowrey's letter reveals his other efforts to persuade scientists, particularly the British ones, to be proponents of the Edison system. As Lowrey perceived the situation, winning over Sir William Thomson was most crucial, although he admitted that some concession on Edison's part would have to be made. William Preece and William Crookes would be less problematic. Lowrey concluded that "these English scientific men are, I find, a very close corporation, and stick together like wax, partly from affection, and partly from fear of each other" (*62*). Nevertheless, he felt that by the time he left Europe, he would have the British scientific and technical community singing with praises of the Edison system (*64*).

As Lowrey combed the exhibition, soliciting scientific supporters for Edison, it is likely that he encountered Rowland. Might he not have offered Rowland a retainer too? Moreover, Rowland could hardly have failed to notice Barker's sudden transformation from being a supporter of the Maxim lamp to being an ardent advocate of the Edison system. All of this smacked of opportunism, which an advocate of pure science would have abhorred.

Nor did Rowland find pure science in the electrical apparatus displayed at the International Electrical Exhibition in

Paris. Rather, he found only a lack of the theoretical science necessary to realize the power of science. Rowland could point to the magnetic or electromagnetic design of generating equipment at Paris as notable examples of practice without theory. Later, the physicist would suggest that a man with the proper theoretic vision "should be able to evolve a [perfect] dynamo-electric machine, or any other machine of this kind, out of his own mind before he had tried a single experiment" (*65*). Edison's achievement at the exhibition, which earned the inventor much praise from the public as well as from men of science, may have led Rowland to question the present state of priorities in scientific activity. It is difficult not to connect Lowrey's statement that "nobody for a moment questions now that you [Edison] are the great man" (*66*) with Rowland's 1883 polemic (*3*) against the "American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, and is often lauded above the great originator of the idea."

Therefore, Rowland's "Plea for pure science" was in large part a response to the scientific, technical, and social milieu of the time—and specifically, to Edison's activity in electric lighting. The period was one in which electric lights were regarded as science, not only by the public but by important members of the scientific establishment. Thus Rowland lashed out at the attitudes not only of the American public but of his colleagues as well. Edison's heroic status, his professed self-image as a scientific man, his willingness to use scientists to achieve his personal ends, his consistent tendency to carry his cause before the public through the press, and his relationship with the scientific community (which was cultivated by its prominent members) led Rowland to conclude that the activities of Edison and his kind were vulgar, opportunistic, and even cutthroat, and had somehow become confused with the work of pure science.

Rowland was one of a very few people who developed contempt for the state of science in America in the period (1878 to 1883) that Daniel Kevles has rightly called the "Age of Edison" (*67*). Edison's lay contemporaries were willing to call the inventor a scientist because Edison demonstrated that knowledge was power, that he could do things that had never been done before. For the most part, members of the American scientific community were willing to ascribe scientific merit to Edison's work in electric lighting because they were fascinated by the phenomena it involved and the problems associated with them. Although Edison himself might not follow established scientific norms, the American scientific community welcomed him with open arms, and they convinced him, I believe, that he was indeed a scientific man.

Science did play a crucial role in the development of Edison's system of electric lighting (*68*). Reciprocity held, because Edison's work demonstrated to the American public that science could pay off even though some scientists, notably Henry Rowland, reached a new and disturbing awareness that science in America was not pure (*69*).

References and Notes

1. See, for example, R. Conot, *A Streak of Luck* (Seaview, New York, 1979).
2. Conference on Thomas A. Edison sponsored by the New Jersey Historical Commission, Newark, 19–20 October 1979.
3. H. A. Rowland, in *Physical Papers of Henry A. Rowland* (Johns Hopkins Press, Baltimore, 1901), p. 594.
4. Historians of American science have paid particular attention to the pure science ideal in American history and in so doing have entered a kind of debate. See R. H. Shryock [*Arch. Int. Hist. Sci.* **28**, 3 (1948); I. B. Cohen (*5*); G. H. Daniels, *Science* **156**, 1699 (1967); (*8*); N. Reingold, in (*6*), p. 38; in *The Pursuit of Knowledge in the Early American Republic*, A. Oleson and S. C. Brown, Eds. (Johns Hopkins Press, Baltimore, 1976), p. 33; and D. Kevles (*7*)]. For a helpful essay, see also A. H. Dupree [*Cah. Hist. Mond.* **8**, 613 (1965)]. The pure science ideal did not begin with Rowland. For example, Alexander Dallas Bache (1806–1867) was an ardent advocate of pure research in America, as were many of Bache's fellow members of the Lazzaroni. His 1851 presidential address before the AAAS is in large part a plea for pure science. Good introductions to Bache are N. Reingold [(*9*); *Technol. Cult.* **11**, 163 (1970); and H. S. Miller, *Dollars for Research: Science and Its Patrons in Nineteenth-Century America* (Univ. of Washington Press, Seattle, 1970)].
5. I. B. Cohen, *Science and American Society in the First Century of the Republic* (Ohio State Univ. Press, Columbus, 1961).
6. G. Daniels, Ed., *Nineteenth-Century American Science* (Northwestern Univ. Press, Evanston, Ill., 1972).
7. D. Kevles, *The Physicists* (Knopf, New York, 1978).
8. G. Daniels, *Isis* **58**, 151 (1967).
9. N. Reingold, Ed., *Science in Nineteenth-Century America: A Documentary History* (Hill & Wang, New York, 1964).
10. Although I have never seen a full explanation for Rowland's address, Cohen (*5*, p. 28) suggests or implies that Rowland could have been responding to Daniel Coit Gilman's inaugural address as president of the new Johns Hopkins University: "Thus there can be no doubt that the first president of Johns Hopkins University declared a program in which science and learning were to be geared to practical purposes. It is only by being aware of this quality that we can understand Rowland's bitterness—which must have stemmed from the spirit of science in his own university as well as in his country at large."
11. Kevles [in (*6*), p. 139] quotes *Electrical World* in 1886: "No university [has] the means of research . . . found in the shops of Thomas Edison at Menlo Park."
12. Barker's influence on Edison is recounted by M. Josephson [*Edison: A Biography* (McGraw-Hill, New York, 1959), pp. 175–178].
13. For a biographical sketch of Barker, see *Dictionary of American Biography* (Scribner's, New York, 1928), vol. 1, pp. 601–602. See also *National Cyclopaedia of American Biography* (White, New York, 1898), vol. 1, pp. 532–533; *Pop. Sci. Mon.* **15**, 693 (1879). While some may question Barker's scientific abilities, E. F. Smith suggests [*Am. J. Sci.* **180**, 225 (1910)] that he was "an earnest student of science, thoroughly conversant with its most recent advances and able to render subjects, which were dry and unattractive though important, so simple and so fascinating, that the ordinary layman could comprehend them with ease" (pp. 225–226). Barker's correspondence with Edison and his associates, which is maintained at the Edison Archives (West Orange, N.J.), provides the best picture of his encouragement of Edison to perform electric lighting research. See the letters of 2 September, 16 September, and 10 October 1878. Barker had long been interested in electric lighting and had closely followed the developments in arc lighting of Moses Farmer and William Wallace. See, for example, the letter from M. Farmer to C. Stowell, 7 October 1875 (Moses Farmer Papers, University of California, Los Angeles). The Henry Draper Papers at the New York Public Library also contain letters from Barker concerning his and Draper's early interest in electric lighting. See Barker's letters of 2 July and 15 August 1877.
14. G. Barker, letter to T. Edison, November 1874 (Edison Archives); quoted in N. R. Speiden (*18*, p. 137).
15. See, for example, letters from G. Barker to T. Edison, 10 September 1877, 30 September 1877, 8 March 1878, 16 September 1878, and 29 October 1880 (Edison Archives).
16. See, for example, letters from G. Barker to T. Edison, 8 March and 10 October 1878 (Edison Archives).
17. G. Barker, letter to E. H. Johnson, 31 March 1878 (Edison Archives).
18. N. R. Speiden, *Science* **105**, 137 (1947).
19. It is important to note that the phonograph even won Edison praise from European cultivators of science. Josephson (*12*, p. 166) quoted Du Moncel saying that Edison possessed "more genius than a whole scientific senate." Josephson also quoted an article in *Nature (London)* [**19**, 471 (1879)] in which at least one man's view on Edison as a scientist was reflected: "Mere ingenuity in contriving machines does not add to the sum of human knowledge, and if Mr. Edison were merely a clever inventor and nothing more, I should feel less interest in the man. It is, however, a noticeable feature of his inventions that they, in general, contain some new principles, some original observation in experimental science, which entitles him to the rank of a discoverer" (*12*, p. 166). See Conot (*1*) for the idea that Edison's early phonograph was merely a sensational rather than an important invention.
20. G. Daniels (*8*, p. 159) suggests that this norm had become established in the late 1850's.
21. Conot (*1*) effectively discussed Edison's propensity to take matters into the public press in connection with his invention of the carbon button transmitter. In a newspaper article, he charged British inventor David Hughes with "piracy" and William Preece with "abuse of confidence" and "perfidy." Conot quotes Sir William Thomson [from *Nature (London)* **18**, 355 (1878)]: "By his violent attack in the public journals on Mr. Preece and Mr. Hughes, he has rendered it for the time impossible for either of them or others to give any consideration whatever to his claims." According to Conot, Edison's public press profile "soured his relations with English scientists and electricians for years" (*1*, pp. 114–115).
22. Edison's presentation was before the Polyclinic Club of the American Institute, which Josephson (*12*, p. 129) called a "scientific association." I am not aware of any historical scholarship on this club. Brooke Hindle deals with the American Institute up to 1854 in his essay in *The Pursuit of Knowledge in the Early American Republic*, A. Oleson and S. C. Brown, Eds. (Johns Hopkins Press, Baltimore, 1976), p. 84–116.
23. G. Barker, letter to T. Edison, 12 July 1878 (Edison Archives). The letter reads: "Don't fail to instruct Bachelor to send to me, care J. K. Rees St. Louis, a box containing your newest things, telephone, tasimeter, rheostat etc. involving the carbon button, *for the paper I am to prepare for you to read at the [AAAS] there the 21st of August*" [italics added]. Edison read four papers at the St. Louis meeting of the AAAS: *Proc. Am. Assoc. Adv. Sci.* **27**, 109 (1878); *ibid.*, p. 112; "On the applications of the carbon button" (read but not published; see *ibid.*, p. 123); and "On the principle involved in the microphone and the carbon telephone" (read but not published; see *ibid.*, p. 123). Barker most likely wrote one or both of the latter two papers. Edison clearly wrote the first-cited paper since he performed experiments with his tasimeter at Rawlins, Wyoming, during the eclipse. The paper, Edison noted, was a report to Henry Draper and was presented with his permission. It is interesting to note that in one of the papers that Barker gave at the AAAS meeting in Saint

Louis, Barker mentions Edison in connection with some of his own work in acoustics: "For an improvement upon it [a tuning fork] I am indebted to a suggestion of my friend Dr. Edison, based upon a discovery of his that tellurium when used in this way is excessively sensitive to the electrolytic action of hydrogen" (*ibid.*, p. 119).

24. Note at Edison Archives. Issues of the *Proceedings* of the AAAS after 1882 suggest that Edison was elected a fellow in 1878, but the date may refer only to the date of his initial membership.
25. F. R. Upton, letter to his father, 24 August 1879 (William J. Hammer Papers, Smithsonian Institution, Washington, D.C.).
26. A letter that Barker wrote to Edison on 30 September 1877 is noteworthy in this respect: "Your suggestions relative to the production of alternate heat and cold is, like all your ideas, extremely original and ingenious" (Edison Archives).
27. F. R. Upton, letter to his father, 12 December 1878 (Hammer Papers).
28. This is the conclusion of Edison's most recent biographers, Josephson (*12*) and Conot (*1*). See also C. Derganc, *IEEE Spectrum* **16**, 50 (1979).
29. That almost all of the leading physical science journals carried articles on electric lighting and its scientific problems suggests to me that the topic was respectable. I have gone through the annual reports of the British Association for the Advancement of Science and have been impressed by the intensity of interest in electric lighting, especially on the part of Sir William Thomson. One can find important articles on electric lighting in *Philosophical Magazine, Journal de Physique*, and a wide range of technical journals. Robert Friedel has suggested (personal communication) that John Tyndall's Royal Institution lectures in January 1879 reveal a marked scientific interest in electric lighting.
30. F. R. Upton, letters to his father, 2 March, 13 April, 22 April, 27 April, 1 June, 6 July, 24 August, 31 August, and 16 November 1879 (Hammer Papers).
31. F. R. Upton, letters to his father, 23 March, 21 July, 24 August, 31 August, and 22 November 1879 (Hammer Papers).
32. F. R. Upton, letter to his father, 31 August 1879 (Hammer Papers). Edison's paper was published in *Proc. Am. Assoc. Adv. Sci.* **28**, 173 (1879).
33. F. R. Upton, letter to his father, 24 August 1879 (Hammer Papers). Upton's paper was published in *Proc. Am. Assoc. Adv. Sci.* **28**, 178 (1879).
34. *Proc. Am. Assoc. Adv. Sci.* **28**, 183 (1879).
35. M. Josephson (*12*) and R. Conot (*1*) both treat Edison's critics in their biographies, as does D. A. Hounshell [paper presented at the AAAS Annual Meeting, Houston, 5 January 1979].
36. F. Upton, *Sci. Am.* **41**, 242 (October 1879).
37. E. Weston, *ibid.*, p. 276; C. Seeley, *ibid.*, p. 305; E. Hospitalier, *Electrician* **3**, 293 (1879).
38. F. R. Upton, letter to his father, 21 December 1879 (Hammer Papers).
39. C. H. Ames, letter to S. Farmer, 1 February 1880 (Moses Farmer Papers).
40. H. A. Rowland and G. Barker, *Am. J. Sci.* **19**, 337 (1880). Rowland and Barker mentioned that they had "formed a just and unbiased estimate of the economy of [Edison's] light" (*ibid*, p. 340). In their article, Rowland and Barker concluded the following from the tests: "The great interest which is now being felt throughout the civilized world in the success of the various attempts to light houses by electricity, together with the contradictory statements made with respect to Mr. Edison's method, have induced us to attempt a brief examination of the efficiency of his light. We deemed this the more important because most of the information on the subject has not been given to the public in a trustworthy form. We have endeavored to make a brief but conclusive test of the efficiency of the light, that is, the amount of light which could be obtained from one horse power of work given out by the steam engine. For if the light be economical, the minor points, such as making the carbon strips last, can undoubtedly be put into practical shape" [p. 337].
41. C. F. Brackett and C. A. Young [*Am. J. Sci.* **19**, 475 (1880)] noted that Edison himself had asked them "to make an independent study" of his light and dynamo. Barker noted in a report on progress in physics [*Smithson. Inst. Annu. Rep.*, p. 288 (1880)] that A. M. Mayer, H. Morton, and B. F. Thomas had also performed measurements on some of Edison's lamps and had published their results in *Ann. Chim. Phys.* **20**, 275 (1880).
42. When a New York *Sun* article (16 September 1878) appeared in which Edison claimed to have solved the problems of electric lighting, Barker immediately wrote Edison that he wished "to lecture on the Electric Light," and wondered if Edison would loan him some of his "new things" (16 September 1878, Edison Archives). See also Barker's letters in the Farmer Papers, the Henry Draper Papers, and the Edison Archives. See especially the letters from Barker to Edison of 8 March, 12 July, 10 October, and 14 December 1878, 5 November 1879; and 29 October and 26 November 1880 (Edison Archives).
43. O. C. Marsh, letter to H. Draper, 24 October 1880 (Draper Papers).
44. G. Barker, letter to T. Edison, 29 October 1880 (Edison Archives). Draper's standing as a scientist is treated by N. Reingold (*9*).
45. T. Edison, letter to H. A. Rowland, 13 December 1880 (Rowland Papers, Johns Hopkins University, Baltimore, Md.). Edison wrote: "I forgot to mention that Barker's temper got the best of him because I could not spare my lamps for Draper's reception." It is clear from Barker's correspondence with Edison that Edison simply ignored his request.
46. Edison probably read more into the article than was actually warranted, something that is revealed in his letter to Rowland (*45*).
47. T. Edison, letter to G. Barker, 23 November 1880 (Edison Archives).
48. G. Barker, letter to T. Edison, 26 November 1880 (Edison Archives).
49. G. Barker, letter to T. Edison, 19 January 1879 (Edison Archives).
50. T. Edison, letter to H. Rowland, 24 November 1880 (Rowland Papers).
51. T. Edison, letter to H. Rowland, 13 December 1880 (Rowland Papers). Edison's charges have some validity. In 1881, Barker submitted a 22-page report to the U.S. Electric Lighting Company, which held the patents on the Maxim lamp. Barker concluded that the Maxim lamp was clearly practicable and economical. See "Preliminary report upon the Maxim system of incandescent lighting" in the Moses Farmer Papers, University of California, Los Angeles.
52. T. Edison (*45*). On 19 January 1879 (almost 1 year earlier), Barker demonstrated his misunderstanding of what Edison was up to when he wrote Edison that the external circuit resistance must equal the internal resistance of a dynamo in order to reach a theoretically maximum efficiency (Edison Archives).
53. This other evidence was weighted primarily toward Barker's misunderstanding of carbon transport phenomena in the Edison lamp.
54. T. Edison, letter to H. Rowland, 13 December 1880 (Rowland Papers).
55. Anonymous, *Science* **105**, 142 (1947).
56. There is apparently no letter from Rowland to Edison at this time that can be considered a response.
57. This paragraph is based on letters from T. Edison to H. Rowland, 4 July 1880, undated, 1880, 24 November 1880, 13 December 1880, 1 December 1882, 10 January 1883, and 13 January 1883 (Rowland Papers).
58. T. Edison, letter to H. Rowland, 4 July 1880 (Rowland Papers). In later letters, Edison zealously expressed his desire to crush Siemens.
59. H. Rowland, letter to D. C. Gilman, 1 August 1880 (Gilman Papers, Johns Hopkins University, Baltimore, Md.). See also H. Rowland, letter to Edison, undated, 1880 (Edison Archives).
60. R. Dyer, letter to H. Rowland, 13 March 1883 (Rowland Papers).
61. J. Brittain (unpublished paper) tells us that Rowland made sketches of some of the magnetic circuits of dynamos on exhibit in September 1881 and that these sketches appear in a notebook preserved at Johns Hopkins University. The notebook contains six pages of sketches of magnetic circuit arrangements including Edison's bipolar scheme.
62. G. P. Lowrey, letter to T. Edison, 23 October 1881 (Edison Archives).
63. Minutes, special meeting of the Executive Committee of the Board of Directors of the Edison Electric Light Co., 3 January 1889 (Edison Archives).
64. J. Brittain (unpublished paper) writes that the British Edison Company solicited help from British scientists, including William Thomson, John Hopkinson, and Fleming Jenkins, "as intermediaries and in adversary proceedings with competitors." Edison's use of scientific experts was not unlike that of Alexander Graham Bell with the telephone a few years earlier. See, for example, D. A. Hounshell, *Proc. IEEE.* **64**, 1305 (1976).
65. H. Rowland, *Electr. World* **4**, 145 (1884). Rowland had already delivered his address as the new president of the National Conference of Electricians, an address that made clear his conception of the science-technology relationship and raised the ire of many conference participants and others who read his address in published form. See also *ibid.*, p. 96.
66. G. P. Lowrey, letter to T. Edison, 23 October 1881 (Edison Archives).
67. D. J. Kevles, in (*6*), p. 133; (*7*), p. 47. Henry Ford once used this term in his celebration of Edison. See D. F. Noble, *America By Design* (Knopf, New York, 1977), p. 113.
68. D. A. Hounshell, unpublished paper; C. Derganc, *IEEE Spectrum* **16**, 59 (1979). M. Josephson attempts, although he does not succeed, to come to grips with "Edison and science" in his biography of Edison (*12*).
69. When Rowland died prematurely in 1901, Edison responded (to the Baltimore *Sun*'s request that he comment): "Rowland worked in the field of pure science for which there is no reward except the glory thereof" (T. Edison, letter to Baltimore *Sun*, 16 April 1901, Edison Archives).
70. J. E. Brittain's enormous help in the writing of this article is deeply appreciated. I also thank Edison Archivist R. Abel.