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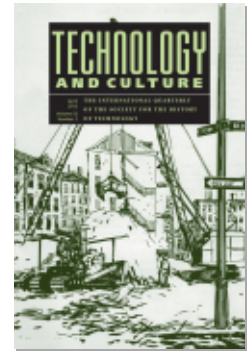
Bell and Gray: Just a Coincidence?

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Bell and Gray

Just a Coincidence?

BERNARD S. FINN

Among the controversies surrounding Alexander Graham Bell's telephone patent, the most intriguing has concerned his relationship with Elisha Gray. Was it simply a coincidence that both men filed applications with the United States Patent Office—Bell for a patent, Gray for a caveat—covering electrical transmission of voice sounds, on the same day? If not, was Bell's claim for a variable-resistance transmitter using water as a medium improperly “borrowed” from Gray's caveat and even more improperly inserted into Bell's patent document? Or—less frequently argued—did Gray borrow from Bell?

These questions are the focus of three books published since 2000, by Seth Shulman, A. Edward Evenson, and Burton Baker. This review will address the degree to which the arguments of these three authors are successful. Because I want to conclude with some broader comments about popularization, I shall also refer to another recent book, by Charlotte Gray.¹

By Way of Background

It may help to begin by mentioning a few significant dates. Starting in 1873, Bell and Gray (along with Thomas Edison and others) independently began developing versions of a “harmonic telegraph” whereby tuned iron reeds, vibrating near electromagnets, induced electrical currents at differ-

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1. Seth Shulman, *The Telephone Gambit* (New York: W. W. Norton, 2008, pp. 256, \$24.95); A. Edward Evenson, *The Telephone Patent Conspiracy of 1876* (Jefferson, N.C.: McFarland, 2000, pp. 269, \$39.95); and Burton Baker, *The Gray Matter: The Forgotten Story of the Telephone* (Saint Joseph, Mich.: Telepress, 2000, pp. 145, \$14.95). Charlotte

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ent audio frequencies into a single wire which then could carry several separate telegraph messages at the same time. The signals were separated at the other end by reed receivers tuned to the same frequencies. This led each of them to conceive of a device with an untuned reed (or diaphragm) receiver that could respond (less efficiently) to all audible frequencies. Bell turned one of his untuned instruments around so that it could also act as a transmitter, and in June 1875 he tried unsuccessfully to transmit and receive intelligible voice signals. On the basis of this experiment he began preparing a patent application that fall; the application mainly concerned improvements in harmonic telegraphy but included a claim for voice transmission by “undulating” currents. A copy of his draft was given to George Brown on 25 January 1876. Brown was about to leave for England, and it was deemed important that he should file for a patent there before an application was made in the United States. Available evidence indicates that Brown’s document, which survives, was identical to a version Bell had notarized on 20 January for submission to the U.S. Patent Office. It contained no reference to a liquid transmitter or to a variable-resistance transmitter.

Gray conceived the essentials of his variable-resistance liquid transmitter in late 1875 and filed for his caveat on 14 February 1876. Bell’s lawyers had been holding Bell’s application, waiting for word that Brown had been successful in Britain. Without receiving any such confirmation, they filed it in the U.S. Patent Office on 14 February. Bell later admitted learning the essence of Gray’s variable-resistance claim on a trip to Washington when he met the patent examiner (an improper communication) on 26 February; an amendment requesting that unrelated items be changed was submitted on 29 February; and the patent was granted 7 March. In this final version it included a clause claiming credit for a variable-resistance transmitter, with specific reference to a device using acidulated water.

Immediately after his return to Boston, Bell began what for him was an unprecedented series of experiments with a metal-water electrical contact, and he achieved successful voice transmission using a liquid transmitter on 10 March. Gray tested versions of his system (liquid transmitter, induction receiver) unsuccessfully in July and successfully later in the year, but his interests were consumed by work related to telegraphy, and especially to the harmonic telegraph. Bell demonstrated a now-successful version of his 1875 induction telephone at the Centennial Exhibition in Philadelphia in July—making no mention of the liquid experiment—and in 1877 he launched a company using this design. By 1878 the induction transmitter had been replaced by a variable-resistance device using carbon, based on the inventions of others.

In the numerous lawsuits that followed, the most serious challenges to

Gray, *Reluctant Genius: Alexander Graham Bell and the Passion for Invention* (New York: Arcade Publishing, 2006, pp. 320, \$25).

Bell's patent were claims that his achievement had been anticipated by various other inventors, including Philipp Reis, Antonio Meucci, Daniel Drawbaugh, and Elisha Gray. None would prevail. Subsequent investigators—among the authors whose books are under review here, Baker is especially effective—have argued that the courts erred and that Bell's claims should at least have been limited, as they were in other countries.

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A separate line of attack was pursued in some of the court cases: that Bell had improperly appropriated Gray's concept of a variable-resistance liquid transmitter. This charge was invariably dismissed, but it has now been raised again by Baker, Evenson, and Shulman—with new and effective arguments.

Why should we care? If the detractors are right, we would have cause to make a judgment about the character of Bell and/or his attorneys—of some significance because of the iconic stature of the Bell name. At an institutional level we would gain insights into the working of the Patent Office. And, if the changes fundamentally affected the nature of the patent, we would conclude that the wrong group of people became rich.

These are all interesting potential conclusions, but historians have generally avoided their pursuit. Part of the reason is that the traditional version, with substantial support from the telephone company (AT&T) and the Bell family, has become imbedded in popular mythology. If they treat it at all, professional historians are much more likely to cite 14 February as a dramatic example of simultaneous invention rather than as a possible instance of collusion.

My own research was conducted in the mid-1960s with the help of Elliot Sivowitch at the Smithsonian and was based on original artifacts and laboratory notebooks. This investigation showed that, whatever the reason Bell began his experiments on a liquid transmitter, he had good reason for abandoning them less than a month after his great success.² My concurrent examination of the various patent drafts and the Bell papers led me to believe (albeit with insufficient evidence to assert) that some level of chicanery had occurred, but that it was not significant enough to affect Bell's patent claims.

Robert Bruce published his definitive biography of Bell in 1990.³ In his research, Bruce had dug more deeply into court records and Bell correspondence. Failing to find anything approaching a smoking gun (and he

2. Bernard Finn, "Alexander Graham Bell's Experiments with the Variable-Resistance Transmitter," *Smithsonian Journal of History* 1, no. 4 (winter 1966): 1–16.

3. Robert Bruce, *Alexander Graham Bell and the Conquest of Solitude* (Ithaca, N.Y., and London, 1990).

wasn't really looking for one), he let the coincidence story stand. David Hounshell, in these pages in 1975, provided evidence that Gray probably anticipated Bell in conceiving both a liquid transmitter and an induction receiver, though not an induction transmitter.⁴ But Hounshell's main concern with the events surrounding 14 February was that they reinforced the image of Gray as a traditional professional inventor who was absorbed in his own telegraphic work and failed to see any commercial value in a speaking instrument, and who therefore failed to take timely steps to protect his position.

The amateur historians have seen things differently. Obsessed with the desire to set the record straight, to right an old wrong, and to establish who was really first, a few of them have spent untold hours of research in attempts to unearth some combination of material that will prove that Gray was the good guy and Bell was not. More than seventy years ago, Lloyd Taylor, a physicist at Oberlin College, was stimulated by Gray apparatus stored in his department (after Gray's death in 1901, half of a substantial collection had been given to the college, the other half to the Smithsonian). Taylor assembled a significant amount of documentary material and wrote an article (published) and a book-length manuscript (unpublished) passionately advancing Gray's right to be called the inventor of the telephone.⁵ But he added nothing new to help untangle the events surrounding 14 February. Now, in the past few years, three more amateurs—a lawyer, an engineer, and a journalist—have brought their talents to bear and produced strongly persuasive arguments that go a long way toward doing just that.

Burton Baker, the lawyer, has made an exhaustive study of court proceedings and government investigations. His detailed analysis of the events surrounding 14 February provides evidence that Gray's application arrived at the Patent Office ahead of Bell's—a matter that should not have been important but became so. His conviction that Bell's application lacked the variable-resistance clause is based largely on surviving patent drafts and inconsistencies in court testimonies by Bell and others. He concludes that the appropriate changes in the application were made after Bell's encounter with the examiner on 26 February. Baker finds particularly compelling Bell's notebook drawing of the 10 March experiment, where the speaker is shown "nose down" above the transmitter in a manner identical to Gray's caveat drawing (p. 108). Personally, I find this less than startling, especially since on a previous notebook page, where Bell sketched a preliminary version of the liquid transmitter, the speaker is shown talking horizontally into a differently designed mouthpiece. Of greater concern is Baker's statement that "There is no question of the practicality of his [Gray's] transmitter" (p.

4. David Hounshell, "Elisha Gray and the Telephone: On the Disadvantages of Being an Expert," *Technology and Culture* 16 (1975): 133–61.

5. Lloyd W. Taylor, "The Untold Story of the Telephone," *American Physics Teacher* 5 (1937): 243–51.

33). The liquid transmitter was in fact fatally flawed, a matter to which I shall return below. Baker does a major service, however, in arguing that, in the decades that followed, a variety of forces conspired to influence government investigations and the judicial system in ways that supported AT&T's telephone monopoly, and in reproducing a hundred pages of patent specifications and other documents.

Although A. Edward Evenson's background is that of an engineer, the strength of his argument lies in his analysis of interpersonal relationships among the principal players in our story, especially within the patent culture in Washington. Much of this apparently was gleaned from court and congressional documents as well as from Bell's papers at the Library of Congress and Gray's at the Smithsonian (where surprisingly he did not ask to see the extensive instrument collections for both inventors). Evenson's picture of a close-knit fraternity of patent lawyers and patent examiners is highly believable—though frustratingly lacking in documentation—as is his analysis of Bell's frame of mind both during the critical days of 1876 and subsequently when he discusses the events in letters and testimony. Evenson paints the same basic picture as Baker, but in the end he concludes that Bell's attorneys probably learned of Gray's plans over the weekend before the caveat was filed (14 February was on a Monday) and took it upon themselves to alter Bell's application before rushing it over to the Patent Office. This creates certain problems, though not problems that are insurmountable. How are we to explain, for instance, a surviving patent draft that includes the critical items in the margins, apparently in Bell's handwriting? Evenson's analysis has the advantage of exonerating Bell from complicity in this initial act, and of giving credibility to his account of being surprised at what he learned from the examiner on 26 February, though very shortly thereafter he would have to have known what had happened. Evenson comes close to admitting that all of this was of little practical consequence, since transmissions over Bell's liquid transmitter were drowned out by noise at more than a few volts. But he also says that "Gray had invented a workable telephone" (p. 119). In fact, since it suffered from the same problem, it was no more "workable" than Bell's.

The books by Baker and Evenson, which appeared almost simultaneously, were greeted by a deafening silence from the reviewing media. Not so Seth Shulman's *The Telephone Gambit*. As a journalist, Shulman is well aware of the need for publicity, as is his publisher. And he has the kinds of contacts that assist in the process. He also has much more at stake because he specializes in making science understandable to the general public. He had certain advantages over his predecessors: in addition to being able to refer to their works, he had a year's appointment at the Dibner Institute in Cambridge, Massachusetts, where he had access to an excellent library and could view from his office those portions of the Bell papers that the Library of Congress had recently made available online. In the course of starting a

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project comparing Bell and Edison, he was looking at Bell's notebooks and was struck by the sudden interest in water. Further investigation revealed the "virtually identical" Bell-Gray nose-down drawings, and he concluded that he "might have discovered something that had eluded generations of historians" (pp. 35, 36).

Shulman describes his pursuit of proof that Bell had indeed received critical information from Gray. Most of his time was spent using the facilities of the Dibner Institute, though he did visit Baker in Michigan and went to see the Taylor papers at Oberlin and some telephone instruments at the Science Museum in London. He apparently did not go to Washington to check the Bell collection at the Library of Congress (only a portion has been digitized) or to view the Bell instruments at the Smithsonian or to check out the Gray papers and Gray instruments which many years ago were given to the Smithsonian by Oberlin. Still, in an easy-flowing narrative he argues that Bell was indeed the guilty party and deserves at most only partial credit for inventing the telephone.

Unfortunately, Shulman's shortcuts in research contributed to some important errors. Four stand out. First, the description of a liquid transmitter on the first page of text does not match the experimental instrument of 10 March, which did not look "something like" the illustration on page 12 (or on the cover). The pictured device was probably constructed for demonstrations in early May and subsequently taken to (but not shown at) the Centennial Exhibition in Philadelphia. This is a common error, and Shulman seems to be victim of a pitfall that he warns against himself (p. 163ff.): popular history is often larded with accounts that creep into the literature and are difficult to dislodge. (The primitive 10 March instrument is depicted in Bell's notebook on a page reproduced—very poorly—by Shulman on p. 36.) Second, in his analysis Shulman states that the resistance changes as the tip of the needle (attached to a diaphragm) "moves alternately closer [to] and further from a separate metal contact immersed in the cup" (p. 12). Conceptually, this is the way Gray's instrument worked; for Bell the varying resistance came from changes in the degree of surface contact between the needle and the water. Third, Shulman finds no evidence in the notebook that Bell was dissatisfied with his liquid device, which flies in the face of expressions of frustration (in the notebook) as Bell failed to get consistent results and in seeming desperation within a month turned back to his induction instrument. Fourth, for a popular work, there are a reasonable number of endnote citations, but there are gaps in attributions. For example, Shulman seems to borrow much more from Evenson than is acknowledged, especially regarding activities in and around the Patent Office. Even if he arrived at this information independently, he should have indicated where others anticipated him.

One citation that Shulman can be forgiven for not checking further is a memo in the Library of Congress speculating that, when the Bell collection

was in the National Geographic Society archives, Robert Bruce was the only historian who used it “to any great extent” (p. 210). From personal experience I know this to be untrue.

But by far the biggest error that all three authors make is in claiming that the origins of the liquid-transmitter concept are of any consequence in determining who was “the inventor” of the telephone. The device was in fact worthless. Shulman, like Evenson, expresses the view that the “variable transmitter’s signal could be easily amplified” by increasing the voltage (p. 176). This in spite of the fact that in the same paragraph he cites my experiments that show that the liquid transmitter was operating at the edge of practicality. Raising the voltage even slightly increases the amount of gas generated to the point where static drowns out the voice signal—something Bell himself noted. (Something Bell did not note, which may have caused part of his frustration, is that an inadvertent reversing of battery polarity would cause hydrogen to be emitted at the contact point in amounts twice as great as oxygen, so that an experiment that worked marginally well one day might have been a disaster the next.)

In any case, Bell’s claim to the liquid transmitter was disallowed, as was his wider claim to variable resistance. What he was granted by the Patent Office, later upheld by the courts, was a remarkably broad claim to transmission of voice by undulating currents, as well as to the induction forms of transmitter and receiver. One can argue, and numerous other claimants did argue, that these rights were given erroneously. But they were in no way dependent on the liquid-transmitter clause. Indeed, one might contend that Bell, or his lawyers, did his cause a great disservice by slipping this language into his application and jeopardizing the overall patent.

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The Problems of Making History Popular

In what has been written thus far I have expressed admiration for contributions to the body of historical evidence that have been made by amateur historians. To this should be added an appreciation for their assistance in spreading the fruits of our professional research to an audience much larger than we generally hope to reach—a process frequently termed “popularization.” Shulman fits into this latter category, but an even better recent example is Charlotte Gray, who has added Bell to her list of Canadian biographical studies. My concern is the degree to which these works ignore or misrepresent established historical information. Gray, for instance, has produced a generally competent, readable book. But when addressing the matter being discussed here, she includes some unconscionable errors. She states, for instance, that Bell’s breakthrough experiment in May 1875 was limited to showing that one tuned reed could respond to another without the benefit of a battery in the circuit. She ignores the much more important

discovery that a damped untuned reed could be seen and heard to respond to the currents produced by a tuned reed. (Shulman, incidentally, makes this same mistake.) More surprisingly, she writes that the demonstrations at the Centennial Exhibition were with a liquid transmitter and a tuned-reed telegraph receiver.

Casting technical historical material into a popular form requires more than a fluid style. There are at least three potential challenges to overcome. One is making the technology accurate and clear. Even if an author understands the technical problems, which is frequently not the case, he or she often finds it difficult to translate them into a form that does not impede the flow of the narrative. Second, there is the need for a plot. Not all historical sequences lend themselves easily to being cast into a form that will keep the reader's attention. One way of getting around this is to make the author's *quest* the plot. This is a standard technique, used regularly and effectively in magazines ranging from *National Geographic* to *The New Yorker*. And it is used here by Shulman. But there are pitfalls, the most important being that the story is invariably tailored to the excitement of the chase. Compromises inevitably have to be made, and material ignored—all too frequently in ways that distort the conclusions. Third, there is the problem of references. Most popular writers want to provide some evidence that they have conducted significant research, and often they want to suggest additional readings. But there is also a strong impetus not to clutter up the account and not to take the extra time required to include meaningful citations. In the present group of books, Baker has no citations and only a brief bibliography; Evenson has a reasonable number of endnotes (with some gaps, as mentioned above) but no bibliography; Shulman is good at referencing quotations but not general statements, and he imbeds a short list of references among his acknowledgments; Gray has no citations or bibliography, providing instead a list of source readings for each chapter.

Professional historians rightly feel frustrated when popular writers inadequately respond to these challenges—as was apparent in a recent exchange in *Technology and Culture* between John Larson and Frederic Schwarz.⁶ Mixed in with frustration is a sense of betrayal and lost opportunity. Our own research is not being used effectively to achieve a broader level of public understanding. But the situation is not beyond improvement.

Proposed Guidelines

Responsibility for the failure to incorporate decent scholarship into popular accounts arguably lies with several parties. Authors in search of

6. John Larson, "What Are We Doing Wrong?" *Technology and Culture* 47 (2006): 803–7; Frederic Schwarz, "We Should All Be Friends," *Technology and Culture* 48 (2007): 407–10.

information may be lazy or in a hurry, or they may just not know where to look. Or they may have difficulty boiling the facts down to a readable account without losing meaning. All too many editors (and publishers, especially those engaged in producing popular works) tend to be lax in checking on authors. And historians, even if they are asked, may decline to assist in the process because the professional rewards are often small. As a means of addressing this pervasive and important problem, I suggest the development of what might be called *A Guide to Inclusion of Good History of Technology in Popular Presentations*. I do so in the hope that it will encourage others (perhaps a committee of SHOT) to expand on them, refine them, publish them, and promote their use.

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Items that might be included are: standard bibliographies and other means of gaining access to recent historical literature; suggestions of ways to handle citations, bibliographies, and source notes, and of how to frame cautionary statements when shortcuts have been taken; examples from the popular literature where seemingly complex technical information has been described clearly and effectively. Exhibits and video (or film) productions have their own special needs and difficulties. There is no reason, however, for the former not to mimic the latter and systematically include display panels of credits and comments. For all media, but especially video and film, the internet is an obvious repository for more extensive information—bibliographies, annotated scripts, authors' comments—for which separate guidelines could be proposed.

To establish such guidelines is clearly not enough. I suggest that they be endorsed by SHOT, published in accessible locations, and made known to publishers. Actual adoption is more difficult, but can, I believe, be achieved if members of SHOT, when asked to participate in projects as expert advisors or reviewers, require that in return the guidelines be followed. And in printed reviews we would draw attention to the extent to which they had been followed.

My own experience suggests that authors will be pleased to have such guidance. Print publishers, and other media producers, are a more problematic group. But the demands are not great, and there are meaningful rewards, both in simplifying what can be a vexatious internal decision-making process and in reducing unwanted external criticism. The rewards for professional historians will be greatest: guidelines will help to assure that our assistance does not go unheeded, that our own works are more accurately and effectively represented at the popular level, and that both as individuals and as a profession we receive recognition for our contributions.