

DEFINING A DISCOVERY: PRIORITY AND METHODOLOGICAL  
CONTROVERSY IN EARLY NINETEENTH-CENTURY ANATOMY

by

CARIN BERKOWITZ\*

*Chemical Heritage Foundation, Beckman Center for the History of Chemistry,  
315 Chestnut Street, Philadelphia, PA 19106, USA*

In the early nineteenth century, Charles Bell and François Magendie engaged in a decades-long priority dispute over the discovery of the roots of motor and sensory nerves. The constantly recalibrated arguments of its participants illuminate changes in the life sciences during that period. When Bell first wrote about the nerves in 1811, surgeon-anatomists ran small schools out of their homes, natural theology was in vogue, exchanges between British and French medical practitioners were limited by the Napoleonic Wars, and British practitioners typically rejected experimental physiology and vivisection. By the end of Magendie's career, medical science was produced in the laboratory, taught through artfully produced performances of the sort at which Magendie excelled, and disseminated through journals. It is not entirely clear which historical character, Bell or Magendie, 'won' the dispute, nor that they even had clear and consistent positions in it, but what is clear is that one style of science had won out over the other, and over the course of the dispute, pedagogy lost pride of place in medical science.

**Keywords: Charles Bell; François Magendie; priority dispute; anatomy;  
vivisection; publication**

In 1811 Charles Bell, a surgeon-anatomist trying to make his living in the crowded London medical marketplace, had a little book about the nerves and brain printed for distribution to his friends and other members of the scientific community.<sup>1</sup> The book contained what Bell considered to be a great discovery on the workings of the nerves and brain—the discovery that spinal nerves had a double root corresponding to two distinct and differently functioning parts of the brain, and that the double root betrayed two different kinds of nerve (later determined to be motor and sensory) that were ensheathed together in the periphery of the body. As Bell himself described it:

considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connexions with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiments, and of proving, at the same time, that nerves of different endowments were in the same chord, and held together by the same sheath.<sup>2</sup>

\*carin.berkowitz@gmail.com

The experiments that Bell mentions so casually were both technically difficult and hard for Bell to stomach, according to his own accounts in personal correspondence. In the text, however, they are presented as unremarkable. Bell did not apologize for them or emphasize the difficulty of the surgery required to conduct them. Bell's focus was clearly on the logic of the system of the nerves he had constructed and not on its experimental proof. Bell publicized his work by distributing this printed text to as many important people as he could afford (100 copies were made), and lecturing about it in his anatomy classes.

By 1823, when the Frenchman François Magendie claimed a very similar discovery for himself, the medical world had changed in many ways, among them the emphasis on fact, the role of vivisection, and the significance of publication in periodicals. Credit for a discovery regarding the roots of the nerves was soon claimed by several anatomists, and those would-be discoverers presented overlapping, but not identical, definitions of their innovation. An agreement over what was actually discovered (though not who discovered it) emerged out of conflict, negotiation and revision. Because the discovery was contested, first by Magendie and later by Bell's own student and countryman Herbert Mayo, it offers an opportunity to evaluate the ways in which British medical practitioners and physiologists defined themselves over time in relation to the French and to each other.

Although Bell received much acclaim from his contemporaries for his work on the nerves and eventually was knighted for his discovery,<sup>3</sup> his work was also criticized by many, and it changed shape often. It was continually being defined and redefined by Bell, his student-advocates, his defenders and his detractors.<sup>4</sup> And although the dispute is quite a famous one, historians have ignored the competing and changing definitions of science that were at stake, flattening the changing contours of the debate into stable opposed positions and actions.<sup>5</sup> Thus this priority dispute also contributes to broader literature on priority disputes in the history of science. There is a secondary literature on issues of priority in science that, in its more theoretical guise, arises chiefly from Robert Merton's work in the 1950s, in which Merton argued for the functional value of priority attribution in science and attempted thereby to explain the occurrence of disputes over the matter; but most subsequent literature on priority issues has focused on the specifics involved in particular disputes, in which the significance therefore lies in matters distinct from the general category 'priority dispute' itself (such as the Newton–Leibniz dispute over the calculus).<sup>6</sup> But although many such pieces focus on individual disputes, their merits, their stakes and their victors, too few see the nature and matter of the disputes themselves as evolving and fluid.<sup>7</sup> The complicated disputes surrounding the substance of the discovery of the roots of motor and sensory spinal nerves, never really acknowledged by the historical actors themselves, demonstrate the role and significance of priority and priority disputes and the evolving nature of the knowledge being disputed or claimed, as well as allowing us to glimpse the changing nature of medical science during that period.

#### THE ORIGINS OF THE CONTROVERSY

Charles Bell began his career in Edinburgh, the city in which he was born. Barred from practice at the Edinburgh Infirmary and from a teaching position at the university for political reasons (he and his brother were not well liked by the leading surgeon in town),

Bell moved to London in 1804 to establish his career.<sup>8</sup> In London's free medical marketplace, he began offering anatomy classes to artists and surgical and medical students, first in his own home and later at the Great Windmill Street School of Anatomy, which had belonged to John and William Hunter. Bell's pedagogy provided the foundation for his science and his reputation. His classes, popular and well attended,<sup>9</sup> relied on the sort of dissection and induction that Bell himself claimed to use in his research. Bell relied on his classes to disseminate his ideas, and they played a crucial role in Bell's discovery, which was developed through his plans for lectures on the nerves and brain, and was fine-tuned and publicized through his lectures.

When he began his work on the brain and nerves, Bell conceived of this work as his path to fame and fortune. On 21 May 1807 he wrote to his brother George, 'I am casting about for a subject to make something new of. I have been thinking about the brain . . .'<sup>10</sup> And later, on 31 November, Bell wrote, 'My surgical books and lectures you will soon see eclipsed by my character as an anatomist and physiologist. I really think this new view of the Anatomy of the Brain will strike more than the discovery of lymphatics being absorbents.'<sup>11</sup> He had only three students and little income at the time, so this emphasis on fame and discovery was more than a little bold.

In 1810 Charles Bell described his progress on the discovery he set out to make, detailing experiments on the spinal nerves in a letter to his brother:

It occurred to me that there were four grand divisions of the brain, so were there four grand divisions of the spinal marrow; first, a lateral division, then a division into the back and fore-part. Next it occurred to me that all the spinal nerves had within the sheath of the spinal marrow two roots—one from the back part, another from before. Whenever this occurred to me I thought that I had obtained a method of inquiry into the function of the parts of the brain.

Experiment 1. I opened the spine and pricked and injured the *posterior* filaments of the nerves—no motion of the muscles followed. I then touched the anterior division—immediately the parts were convulsed.

Experiment 2. I now destroyed the posterior part of the spinal marrow by the point of a needle—no convulsive movement followed. I injured the anterior part and the animal was convulsed.<sup>12</sup>

This letter seems to describe in narrative fashion the development of an idea from anatomy—that the spinal nerve was divided in ways that corresponded to the divisions of the brain, and that one part was responsible for motion and the other for sensation—and then the simple test of that idea through vivisection, first injuring both filaments and then destroying the posterior filament and injuring the anterior to confirm that initial idea. This idea and experiment formed the basis of Bell's claim to discovery and are the essence of his short 1811 booklet, *Idea of a New Anatomy of the Brain*.

Bell intended from the start to lecture on his discovery to publicize it. In an 1807 letter to his brother, Bell wrote:

My new Anatomy of the Brain is a thing that occupies my head almost entirely. . . . My object is not to publish it, but to lecture it to my friends, to lecture it to Sir Joseph Banks' coterie of old women, to make the town ring with it, as it is really the only new thing that has appeared in anatomy since the days of Hunter.<sup>13</sup>

However, to spread the word to colleagues and solicit their feedback, Bell also had a ‘little book’ printed. From that 1811 self-published treatise, we get a sense of how he conceived of his discovery before it was contested. In that initial work, Bell described the basis of his idea:

the cerebrum and cerebellum are different in function as in form; that the parts of the cerebrum have different functions; and that the nerves which we trace in the body are not single nerves possessing various powers, but bundles of nerves, whose filaments are united for the convenience of distribution but which are distinct in office as they are in origin, from the brain.<sup>14</sup>

His work had the added virtue of simplicity, an advantage that Bell would rely upon in defence of his contribution throughout his lifetime.

In the introduction to *Idea of a New Anatomy*, Bell asserted that the more one knew about previous work on the anatomy of the brain, the more confused one became. A hodgepodge of previously established facts and theories, many of which contradicted each other, made the brain like a maze. In great part because of his faith in Natural Theology, such confusion was unacceptable to Bell, who placed a high premium on the elegance of systems of anatomical parts working together. Thus, he wrote to his brother:

I establish thus a kind of circulation, as it were. In this inquiry I describe many new connections. The whole opens up in a new and simple light; the nerves take a simple arrangement; the parts have appropriate nerves; and the whole accords with the phenomena of the pathology, and is supported by interesting views.<sup>15</sup>

The elegance and simplicity of a circulatory system of the nerves appealed to Bell as a parallel to Harvey’s circulation of the blood.

Bell went on to detail his findings on the brain, the cerebellum and the double roots of the spinal nerves. He began the body of the text by describing the anatomy of the brain itself, noting the ways in which the cerebrum and cerebellum were clearly anatomically distinct—in form, in colour and in vascular structure—and remained distinct in various species of the animal kingdom. He therefore concluded that every nerve with a double function must have a double root, a connection to both the cerebrum and the cerebellum. In *Idea of a New Anatomy*, Bell called these double functions ‘sensible’ and ‘insensible’, and later, ‘nerves of sense’ and ‘nerves of motion’. The cerebrum, he claimed, united the body with the world, containing the system of nerves bearing sensory impressions from the outside world to the mind and the separate nerves that carried the force of will to the body, while the cerebellum handled the nerves responsible for basic functions of the body. Bell ended the piece by summarizing his new theory of the nerves: ‘Through the nerves of sense, the sensorium receives impressions, but the will is expressed through the medium of the nerves of motion.’<sup>16</sup> The book is short and laid out in a sketchy fashion, but it is clear that Bell thought the stakes of his work were great.

To put such ideas to the test, Bell developed two experiments, described only briefly in *Idea of a New Anatomy* in the fashion depicted above—one in which he opened the spine and ‘pricked and injured the posterior filaments of the nerves . . . then touched the anterior division’, and one in which he ‘destroyed the posterior part of the spinal marrow by the point of a needle . . . [and] injured the anterior part.’<sup>17</sup> The experiments were technically difficult—Bell found it hard to injure one filament without also injuring the other, and getting the spine open without damaging its contents required delicacy and dexterity—but

he was able to demonstrate results. Bell described the injury and destruction of the posterior root as causing no movement, whereas that of the anterior root caused the animal to convulse.

It is important that Bell says of these experiments that they were not conclusive but merely provided encouragement that his system was correct.<sup>18</sup> Bell's anatomical physiology required that anatomical and philosophical reasoning precede, and to some extent supersede, vivisection experiments. Although he became more explicit about these methodological priorities later in his career, Bell established them from the outset, writing within *Idea of a New Anatomy*, 'If I be correct in this view of the subject, then the experiments which have been made upon the brain tend to confirm the conclusions which I should be inclined to draw from strict anatomy.'<sup>19</sup>

Despite his requests for comments on his 'little manuscript', Bell received very little attention for the work at the time and was disappointed by the lack of feedback.<sup>20</sup> In the 10 years after its printing, Bell continued to work and lecture on the brain and nerves, but did not publish anything on them himself until a paper he gave before the Royal Society, in July 1821, entitled 'On the Nerves', which summarized his work to date and discussed the functions of the fifth and seventh nerves. Publication became an increasingly important measure of good science as the circulation of scientific periodicals expanded dramatically in the 1820s.<sup>21</sup> Thus, with dramatic changes in a medical science that began to embrace vivisection and laboratory methods and began to recognize publication in journals as a more fitting way of conveying of new discoveries than classroom teaching, when others took up the subject of the nerves a decade after Bell's little pamphlet and articulated their ideas in ways that represented new trends in anatomy and physiology, Bell found his priority disputed and his methods and conclusions under attack.

#### THE CONTROVERSY: CHALLENGES FROM ABROAD

Perhaps the strongest threat to Bell's priority came from abroad, from the French physiologist François Magendie. Historians and physiologists have tried to sort out the dispute and to issue credit for the discovery in a variety of ways.<sup>22</sup> L. S. Jacyna and Edwin Clarke pronounced Magendie the victor in a passage that is representative of many twentieth-century historians and scientists seeking to base priority on correctness of facts and methodology, saying:

It is now widely accepted that although Bell made the first experimental observations on spinal root properties, his claims for full priority cannot be allowed, for two reasons. First, his pioneer, but sole, investigation was incomplete and the results he obtained did not warrant the conclusions deduced, which in any case were mainly erroneous. Second, and of much more sinister significance, is the damning evidence against Bell that, in an attempt to establish his leadership, he dishonestly appropriated Magendie's correct opinions and in light of them deceitfully emended his earlier publications before reprinting them in order to support his case.<sup>23</sup>

Others have been more even-handed, giving Bell credit for an idea and for an initial theory, while assigning Magendie credit for completing what Bell started.<sup>24</sup> It is necessary to look to his nineteenth-century contemporaries to find accounts that credited Bell entirely, but when one does that, one can find assignments of credit to Bell from both sides of the English Channel.<sup>25</sup>

Magendie published his first account of motor and sensory nerves in 1822. In September 1821 John Shaw, who was Bell's nephew and assistant, had travelled to Paris to convey Bell's work to French anatomists. He explained Bell's system to Magendie and, when asked, provided a demonstration using a horse as the experimental subject. Shaw had previously only performed the demonstration on an ass and seems to have been confused by what he saw when he cut away the skin on the horse's face: the demonstration did not go as planned and the nerves that Shaw cut failed to cause the expected paralysis of the lip.<sup>26</sup> Still, Magendie was intrigued and asked Shaw for a copy of his new laboratory manual and Bell's paper delivered before the Royal Society, both of which he received.

In June 1822 Magendie published an article in the journal he founded, *Journal de Physiologie Expérimentale et de Pathologie*, entitled 'Experiments on the Functions of the Roots of the Spinal Nerves'.<sup>27</sup> In the article he stated that he had long wanted to try an experiment on spinal nerves but that he had had difficulty opening the spinal cord without killing, or at least seriously injuring, the animal, until someone had brought him a litter of eight puppies. The puppies' spinal cords were more malleable, and he had been able to open the vertebral canal without destroying its contents, allowing him to cut first the posterior and then the anterior roots separately and then to sever both together. From these experiments, the first of which produced an animal whose limbs convulsed but were devoid of sensation, the second of which produced flaccid but sensitive limbs, and the third of which produced limbs with neither sensation nor motion, Magendie deduced that 'the posterior roots seem to be particularly destined for sensibility, while the anterior roots seem to be especially connected with movement.'<sup>28</sup>

After Magendie's article was published, Shaw, who had received a copy from Magendie, wrote to the Frenchman, saying that Bell had performed the same experiment 13 years earlier and, shortly thereafter, sent Magendie a copy of Bell's 'little book', *Idea of a New Anatomy*.<sup>29</sup> The priority dispute was taking shape.

Magendie's first attempt to settle it appeared immediately in an article that recounted the events thus far:

One sees by this citation of a work which I could not know of, since it had not been made available to the public, that Mr. Bell, led by his ingenious ideas on the nervous system, had been very near to discovering the functions of the spinal roots; nevertheless, the fact that the anterior roots are designed for movement, while the posterior roots belong more particularly to feeling, appears to have escaped him: it is, therefore, to having established this fact in a positive manner that I must limit my claims.<sup>30</sup>

The passage is notable for the fact that Magendie seized on Bell's lack of publication as well as factual errors as the basis of his claims to credit.

Bell's responses to Magendie were presented in his own later monographs on the nerves and came initially in the form of criticisms of Magendie's methodology. He incorporated one of his earliest public reactions to Magendie's work into *An Exposition of the Natural System of the Nerves* (1825),<sup>31</sup> a volume that detailed work on the nerves that had been presented before the Royal Society. In it he wrote:

In France, where an attempt has been made to deprive me of the originality of these discoveries, experiments without number and without mercy have been made on living animals; not under the direction of anatomical knowledge, or the guidance of just induction, but conducted with cruelty and indifference, in hope to catch at some of the accidental facts of a system which, it is evident, the experimenters did not fully comprehend.<sup>32</sup>

This passage gets to the heart of Bell's critique of Magendie: Magendie's emphasis on 'accidental facts' and his vicious pursuit of those facts through uninformed vivisection were deeply flawed. Such claims could have garnered the support of a significant portion of the British medical community, a community that reluctantly tolerated occasional vivisection but considered it to have limited value compared with dissection.<sup>33</sup> That the first British anti-cruelty legislation, Martin's Act, was passed in 1822 with the support and testimony of British medical professionals, and mentioned Magendie directly, demonstrates the popularity of the anti-vivisectionist cause in Britain at the time that Bell and Magendie were beginning their dispute.<sup>34</sup>

Bell's cause, like that of the anti-vivisectionists, took on nationalistic tones, such that Bell even wrote, in an 1823 letter to his brother, 'You may send for the "Medical Journal"—the last number of the yellow book—if you please, where you will find some strictures in my favour and against the French. They, you know, have accused me of taking from them!'<sup>35</sup> He spoke of the French and of Magendie almost interchangeably during the early years of the dispute, partly because, in the shadow of the Napoleonic Wars, Bell trusted that his fellow Britons would share his opinions about the French and their style of medical science. But because the flow of English medical students studying in Paris, which had slowed to a trickle during the war years, increased significantly with peace re-established,<sup>36</sup> Bell found the medical community around him—its methods and its sympathies—shifting.

Bell continued to insist that Magendie had stolen his idea and that Magendie had then pursued it in a way that was improper methodologically, but his attacks became more specific and more detailed, no longer taking for granted that other British anatomists would rally around opposition to vivisection or to the French. Just a few years after the unequivocal passage above about experiments 'without number and without mercy', Bell claimed, in an 1828 lecture before the College of Surgeons,

that my experiments upon the fifth nerve, and the seventh, were repeated before him; that the rationale of these experiments was explained to him; that he had a little work put into his hands, in which these experiments upon the roots of the spinal nerves were described . . . yet I am constrained, in this place, to say that he may not have understood these experiments upon the seventh, or on the fifth . . . that he may even, in short, have employed his fingers, those 'pickers and stealers,' as Shakespeare calls them, without the control of his head—without intention or ideas of any kind—with a perfect purity that belongs to entire ignorance.<sup>37</sup>

This passage reflects the same vitriol as before, but this time Bell equated Magendie's experiments with his own. By that point it was no longer effective to rail against vivisection on moral grounds.

Just a year later, in a letter to the editor of *London Medical Gazette*, Bell seemed to confirm that experimental physiology was taking hold in British medicine, writing, 'How often shall I have to make an apology for not believing in the opinions of experimenters?'<sup>38</sup> He went on to argue that he had tried precisely the same experiments that Magendie did 'with every assistance possible. . . . My experiments on this subject entirely failed. . . . When, therefore, twelve years afterwards, I addressed the Royal Society, I put all these experiments aside, and founded my reasoning upon that which was not only correct but was easily ascertained to be so.'<sup>39</sup> Magendie's work, Bell said, was subject to mistakes—the procedures that Magendie had followed could not possibly have

allowed him to distinguish between sensory and motor nerves.<sup>40</sup> Here, Bell was refining and combining his earlier arguments—Magendie stole his experiments, but those experiments did not work (as Bell knew from his own experience), so Magendie must also have stolen his conclusions. With a subtle shift in rhetoric, Bell made himself the original thinker but also the expert on a methodology that he had earlier condemned but that had become increasingly popular among his colleagues.

Bell's argument changed several more times within his lifetime. In the next revision of the argument, in 1834, he praised Johannes Müller for deciding the controversy experimentally, saying:

He has repeated the experiments with the utmost care, insulating the distinct roots, and observing the effects when they are variously irritated. He has shewn that by experimenting upon frogs, the conclusions which I had announced are confirmed in a manner which admits of no question or doubt; and that one root—the anterior—is for motion alone, and the posterior for sensation alone.<sup>41</sup>

Bell's acceptance of experiments as conclusive in a debate in which he had rejected experimentation as improper and unsuccessful from the outset could be seen as merely convenient—Bell favoured experiment when it helped his cause—and surely there is some of that self-interest at work here, but Bell was also evolving, responding to a general shift in the British medical community.

The final twist in the argument between Bell and Magendie came in the form of an admission of error on Bell's part. In his 1834 clinical lecture on diseases, published in *London Medical Gazette*, Bell stated:

My experiments proved the portio dura to be the nerve of motion to all the muscles of the side of the face, with the exception of the muscles of the jaws; . . . with regard to the lips, I was led into a mistake in my first experiments, which Magendie corrected. I thought that the lips, besides obtaining the power of motion principally from the branches of the portio dura, were also, to a certain degree, under the control of branches prolonged from the motor root of the fifth pair: and this I conceived was for the purpose of associating the lips and the cheeks in the combined actions of mastication. I was in error as to the particular branch which is so prolonged to the cheeks—an error into which I should not have fallen, had I examined with more care, before my first experiments, the anatomy of the roots of the fifth pair, as it is given in several of the best German authors . . . .<sup>42</sup>

Here, again, Bell was positioning himself as an experimentalist, saying that he had made mistakes in the definitive experiments he had performed, mistakes that Magendie had corrected. But in displaying his cosmopolitanism and recognizing the science of the Continentals, saying he should have studied work being done in Germany better before beginning his experiments, Bell allies himself with German *anatomy* and not *physiology*. This was a unique admission of a mistake by Bell—and one that did not minimize Magendie's work. Still, it ends with Bell's insistence on the importance of anatomy before experimentation, signalling that he was still committed to the primacy of what he called 'higher anatomy', which he thought superior to a strictly experimental physiology, even though programmes like his were going out of fashion.

Bell and his students carried forth the dispute with Magendie with vigour, but the dispute really piqued the attention of a British medical community that was becoming divided along generational and political lines when Bell's former student, Herbert Mayo, claimed the discovery for himself and also allied himself methodologically with Magendie.

## THE CONTROVERSY: CHALLENGES FROM WITHIN GREAT BRITAIN

While Bell's British contemporaries took different sides in his debate with Magendie, perhaps the most brutal betrayal by one of Bell's own countrymen came from his own student, Herbert Mayo. Mayo studied with Bell from 1812 to 1815, first at the Great Windmill Street School of Anatomy and later in the wards of the Middlesex Hospital.<sup>43</sup> Bell's and Mayo's careers remained closely intertwined, as often happened with ambitious pupils and their teachers in early-nineteenth-century London. Mayo became a house surgeon alongside Bell at the Middlesex Hospital in 1818, and in 1826 he and another of Bell's students, Caesar Hawkins, bought the Great Windmill Street School of Anatomy from Bell. As is clear, both from such a web of connected careers and from letters by medical men of the time describing the significance of knowing established members of the medical community, teachers found positions for their students, relatives and friends and helped them to gain a foothold in a competitive medical marketplace. This makes Mayo's conflict with Bell over the functions of the facial nerves even more significant. It also provides a revealing contrast with the dispute between Bell and Magendie.

In 1821, Bell published an article entitled 'On the Nerves; Giving an Account of Some Experiments on Their Structure and Functions, Which Lead to a New Arrangement of the System' in *Philosophical Transactions of the Royal Society*, in which he focused on the facial nerves—those same nerves that had been so troublesome in Shaw's demonstration for Magendie. In the article, Bell divided the nerves into a symmetrical system and superadded or irregular nerves. As previously mentioned, he focused particularly on the trigeminus, or fifth, pair of cranial nerves (figure 1), and the facial, or seventh, pair of cranial nerves (figure 2), declaring that the fifth pair belonged to the symmetrical system, whereas the portio dura of the seventh pair (which was divided at the time into the portio dura and the portio mollis) belonged to the superadded, or respiratory, nerves. The fifth pair was important to Bell essentially because it resembled the spinal nerves—Bell called the fifth pair 'the spinal nerves of the head'—and conversely, the scheme of spinal nerves was important because it explained the complicated fifth pair of cranial nerves. According to Bell's scheme, the fifth pair had sensory branches that, like the ganglion-filled sensory nerves of the spinal cord, emerged from a ganglion, while also having a small motor root that bypassed the ganglion. He traced the origin of this discovery to the process of teaching and demonstrating in front of his students and house pupils.<sup>44</sup>

The idea of a symmetrical system of nerves and a distinctly superadded system of nerves helped Bell to explain both of the similar paths of two nerves that seemed to have motor functions, which would be a redundancy not plausible to someone who favoured the elegance of a designful Creator. Bell's Natural Theology was one that suggested economy in a Nature full of systems, created by God, meant to be comprehensive and complete, and also without wastefulness. To have nerves that carried impulses in both directions simultaneously or more than one set of nerves that performed the same function would lack the sort of simplicity and precision that Bell ascribed to his Creator God. The symmetrical system of the nerves also explained the pathology of partial paralysis of the face—nerves from the symmetrical system would produce paralysis of voluntary motion, whereas those of the superadded system would paralyse respiratory (and therefore involuntary) functions.<sup>45</sup> Thus, according to Bell, the portio dura was the respiratory nerve of the face and 'all those motions of the nostril, lips, or face generally, which accord with the motions of the chest in respiration, depend solely on this nerve', as did the muscles of

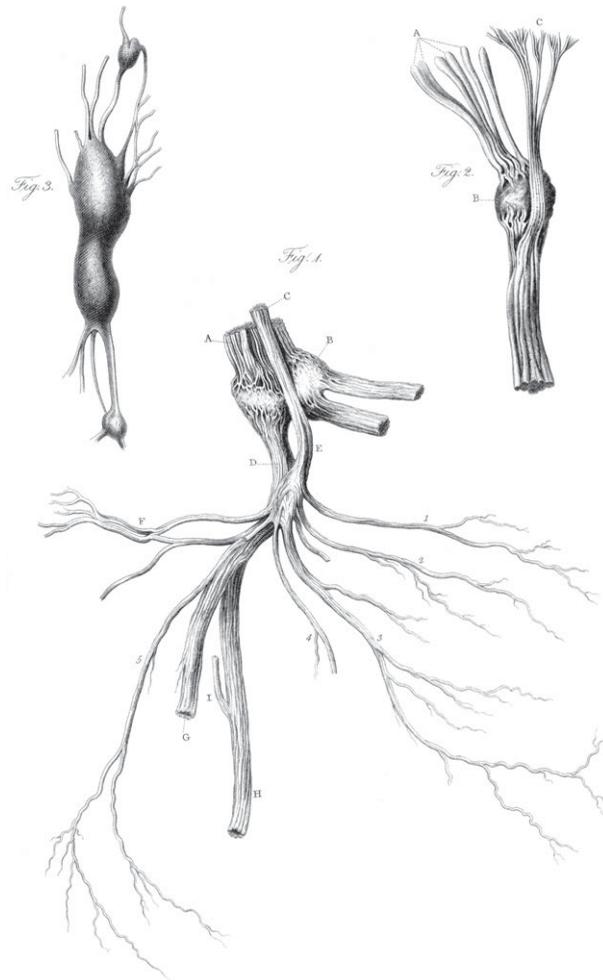


Figure 1. In this drawing by Bell, the large nerve in the middle of the page here (Bell's Fig. 1) is the fifth nerve, dissected out. It shows both the ganglion (B) and the motor root passing in front of the ganglion (C). The smaller nerve above and to the right of the fifth nerve (Bell's Fig. 2) is the ganglion of a spinal nerve, placed in the drawing to show its similarity to the ganglion of the fifth nerve. The nerve to the left of the fifth nerve (Bell's Fig. 3) is the ganglion of a sympathetic nerve and is meant to show the difference between ganglia of the sympathetic nervous system and those of the 'symmetrical system' (of which the fifth nerve and the spinal nerves were a part).<sup>56</sup>

expression, which Bell believed to be related.<sup>46</sup> Without a functioning portio dura, the parts of the face could not coordinate with the lungs or produce expression (which Bell considered to be mostly involuntary).

Herbert Mayo rejected Bell's system of respiratory nerves as well as his assignment of functions to the fifth and seventh cranial nerves. When cutting the fifth cranial nerve, Mayo noted no loss of muscle tone, but on cutting the facial or seventh cranial nerve 'the lips immediately fell away from the teeth, and hung flaccid, and the nostrils lost all movement.'<sup>47</sup> Thus whereas Bell had declared the fifth nerve a motor nerve because when



Figure 2. In this image, also drawn by Bell, A, which is right under the ear, is the portio dura at its origin, with its principal branches cut. B, which is pinned down in front of the face, is the trunk of the portio dura, the seventh nerve, and C, pinned immediately next to it, is the third branch of the fifth nerve, which joins the portio dura, showing how complex and interwoven the nerves of the face are.<sup>57</sup>

he cut it the ass appeared unable to eat, Mayo concluded that it was a sensory nerve and that the animal did not eat with its lips because it could not feel the food, but that when the food was placed on its tongue, it could still devour its oats. Similarly, Mayo found that Bell's experiments on the seventh nerve did not go far enough: when cutting both sides of the seventh pair of cranial nerves, Mayo found that not merely respiratory functions but all motor functions ceased, leading Mayo to declare the seventh pair a general motor nerve. Mayo had clearly adopted Magendie's methods.

Charles Bell did not answer these accusations himself, but Alexander Shaw, who was the brother of his former assistant, John (who had died in 1827), and of Charles's wife, Marion, did so at great length, presumably on Bell's behalf, saying:

I believe I am not altogether destitute of that ancient virtue which enforces respect and deference to the aged; but let me ask what was the example afforded me by Mr. Mayo himself when he was a young man, commencing his professional career—eight years ago? What was his conduct towards Mr. Bell, his senior by many years—his teacher, and in whose house he had resided? . . . he who commenced by opposing in the most reckless manner all that Mr. Bell had done on the subject of the nerves, and who afterwards claimed as his own the most essential and prominent parts of his preceptor's discoveries. He was protected from Mr. Bell's animadversions merely because he was a very young man, and had been his house pupil.<sup>48</sup>

In addition, Shaw asserted that Mayo had omitted discussion of both Bell's work and John Shaw's work on the nerves in *Anatomical and Physiological Commentaries* and that if he had had true evidence regarding their falseness, he would have demonstrated that he had repeated

previous experiments and found them lacking. Finally, Shaw demanded that Mayo—who had said that, if priority were to be allocated to Bell or to Magendie, he preferred Magendie—give some reason for his choice.<sup>49</sup>

Although Shaw wrote extensively in response to Mayo, Bell did not answer Mayo directly. Shaw acted as Bell's representative in this controversy much as Clarke did for Newton, and as Huxley would later do for Darwin.<sup>50</sup> But by the end of the exchange, Bell felt it necessary to distance himself from the whole mess, allowing the editors of *London Medical Gazette* to write: 'We are authorized, by Mr. Bell, to contradict the insinuation that he is the concealed opponent in the controversy between Mr. Mayo . . . and Mr. Shaw. He has neither written nor dictated any thing on the subject in dispute.'<sup>51</sup>

Bell never did directly address Mayo's betrayal, either in published work or in surviving private letters. Mayo, in contrast, continued to write on the nerves and to dispute Bell's findings. In 1834 he wrote to the editor of *Medical Quarterly Review* that 'Magendie, by ingeniously using very young animals in his experiments, succeeded in obtaining a positive result, and in realizing the discovery, which is honestly his', describing the discovery of the difference in function between spinal nerves with and without ganglia. And in case it was not enough to credit Magendie, Mayo discredited Bell, saying, 'Sir Charles Bell's various publications, in which he claims or assumes credit for discoveries to which he is not entitled, the following words of Seneca would form an excellent motto: "Ista pro ingenio finguntur, non ex scientiae vi".'<sup>52</sup>

In 1839, after Bell had returned to Edinburgh and had grown increasingly distant from the London medical scene, Alexander Shaw published *Narrative of the Discoveries of Sir Charles Bell in the Nervous System*,<sup>53</sup> an extensive defence of Bell's priority. In it, Shaw focused almost entirely on attacking Mayo, reserving only one of six chapters for Magendie. Much of his case against Mayo had to do with establishing the dates on which various contenders were working on aspects of the discovery, the extent to which Mayo's work was done under the supervision or at the instigation of Charles Bell, and Mayo's personal conduct toward his mentor, but he also discussed vivisection at length and redefined, yet again, the substance of Charles Bell's innovation.

Shaw's explanation of why vivisection was a method inappropriate to physiological research on the nerves demonstrates both similarities to Bell's views on the matter and also clear differences. Shaw argued the point on technical, not ethical, grounds. He described in rather excruciating detail the process whereby experiments on the nerves would be conducted:

He [the physiologist] has in the first place, to cut extensively through the skin; then he must carry his knife through several successive layers of thick and tendinous muscles: after that, he has to apply his saw to a chain of irregularly formed bones; and having divided these bones, he must introduce his levers and bone scissors into the interior of the vertebral canal, to tear and break up the fragments, and disclose parts contained within. Now, can it be supposed that, after suffering from the tortures of such a proceeding, there is any animal, however submissive to the infliction of pain or high in its courage that could endure the further and concluding parts of the experiment with such a degree of patience as to admit of correct observations being made in regard to the amount of sensibility appertaining to either of the roots? When the membrane investing the spinal marrow has been slit up and the roots displayed, can it be supposed that the animal is in such a condition as to enable us to judge satisfactorily whether its struggles and cries result from the severity of the wound inflicted, or depend on the fresh injuries that we commit on the roots of its nerves?<sup>54</sup>

In other words, Shaw made the claim that there was no way that the nerve roots would be exposed and the animal left unaltered, and said that although one could demonstrate which roots were responsible for motion through vivisection, it would be impossible to show which were responsible for sensation. To determine which was the root of sensation, Shaw suggested, Bell had to return to anatomy and to the argument that the human body is an elegant and purposeful system without unnecessary redundancies. According to this argument, circulation of nervous impulses would require that the posterior root be for sensation, so that one root carried the will of the brain out to the body while the parallel root returned sensory perceptions to the brain.

After declaring vivisection inappropriate for investigations of the nerves, thereby undermining the methods of Bell's detractors, Shaw went on to make the case that the sheer act of focusing on the roots of the nerves was Bell's innovation and that it was far more significant than whatever Magendie or Mayo did afterward. He wrote:

Here, then, is the simple explanation of the principle on which all these new discoveries have been based. It consists, I repeat, in supposing that, to investigate the functions of the nervous system successfully, we must devote our attention, not to the trunks, as was formerly done, but to the roots of the nerves. Accordingly, whoever was the first to suggest and follow out that new method of prosecuting the subject, must be declared the true originator of the recent improvements in this department of physiology.<sup>55</sup>

By constituting the discovery as a particular approach or experimental focus, Shaw could redefine the terms of the debate, denying Mayo and Magendie priority by definition and making their work seem derivative. It was a defence that allowed for the support of vivisection, for, as Shaw pointed out, without the focus on the roots there could be no revealing experiments on the nerves. Shaw's account also placed vivisection early in the narrative of Bell's work—as he told it, Bell discovered the function of one root by vivisection and then resorted to philosophical principles to assign a function to the other root. Shaw's defence of Bell was written for a different audience of medical scientists from that for which Bell had written his own work. Where Bell assumed that his early audience was made up largely of anti-French and antivivisectionist surgeon-anatomists, Shaw was writing to convince the new physiologists and continental medical scientists.

It is no wonder that Magendie is often credited as the winner of this dispute: his was a modern medical science—his results were built on experiment and published in a journal, whereas his rivals' were built on deduction and publicized in the classroom. But that reading is a little too neat. Bell repositioned his work throughout the 30-year period of their controversy, co-opting portions of each others' arguments while at the same time maintaining their opposition to the other would-be discoverers, and each time subtly changing the substance of the discovery itself. And ultimately it probably did not matter terribly much that Bell did not decisively win his priority dispute. Participation was enough. The dispute itself brought him fame, and more than a decade after it began he was knighted, so it is hard to make the claim that Bell suffered personally for his loss. Instead, the dispute marks the death throes of a style of medical science that was rooted in induction from anatomy, natural theology and natural philosophy and located in the classroom, and one that was soon to be history.

## ACKNOWLEDGEMENTS

I would like to thank Robert Fox for his kind encouragement, and Michael Gordin for commentary he gave on a version of this paper delivered at the ICHSTM in Manchester (July 2013).

## NOTES

- 1 Charles Bell, *Idea of a New Anatomy of the Brain: a facsimile of the privately printed edition of 1811, with a bio-bibliographical introduction* (Dawsons of Pall Mall, London, 1966).
- 2 *Ibid.*, p. 21.
- 3 Charles Bell, *Letters of Sir Charles Bell, Selected from His Correspondence with His Brother George Joseph Bell* (J. Murray, London, 1870), p. 323 (12 October 1831).
- 4 Such changing definitions of discovery and evolving accounts by actors themselves of their place within their fields seem a fairly natural part of science. Much as Bell's evolving work on the nerves has been flattened into a single position or theory, historians have collapsed the temporality and evolution of Lister's theories of what we might anachronistically term 'infection', as Christopher Lawrence and Richard Dixey have described. Lister's changing pathological theories, which were shaped in relation to various schools of thought around him, were later elided by Lister and his followers into the 'Listerian revolution' and made to have a crucial role in the history of modern aseptic surgery. For more, see Christopher Lawrence and Richard Dixey, 'Practising on principle: Joseph Lister and the germ theories of disease', in *Medical theory, surgical practice: studies in the history of surgery* (ed. Christopher Lawrence), pp. 153–216 (Routledge, London, 1992).
- 5 See, for example, Paul F. Cranefield and Charles Bell, *The way in and the way out: François Magendie, Charles Bell, and the roots of the spinal nerves: with a facsimile of Charles Bell's annotated copy of his Ideas of a New Anatomy of the Brain* (Futura Publishing Company, Mount Kisco, NY, 1974); Gordon Gordon-Taylor and E. W. Walls, *Sir Charles Bell, his life and times* (E. & S. Livingstone, Edinburgh, 1958); J. M. D. Olmsted, *François Magendie, pioneer in experimental physiology and scientific medicine in nineteenth century France* (Schuman's, New York, 1944).
- 6 See, for instance, Robert K. Merton, 'Priorities in scientific discovery: a chapter in the sociology of science', *Am. Sociol. Rev.* **22**, 635–659 (1957).
- 7 Augustine Brannigan, *The social basis of scientific discoveries* (Cambridge University Press, 1981); David Philip Miller, *Discovering water: James Watt, Henry Cavendish and the nineteenth century 'water controversy'* (Ashgate, Aldershot, 2004).
- 8 Gordon-Taylor and Walls, *op. cit.* (note 5), pp. 13–311; L. S. Jacyna, *Philosophic Whigs: medicine, science, and citizenship in Edinburgh, 1789–1848* (Routledge, London, 1994), pp. 116–117.
- 9 Pauline Mazumdar, 'Anatomy, physiology and surgery: physiology teaching in early nineteenth-century London', *Can. Bull. Med. Hist.* **4**, 119–143 (1987), at p. 128.
- 10 Bell, *op. cit.* (note 3), p. 96 (21 May 1807).
- 11 Bell, *op. cit.* (note 3), p. 117 (31 November 1807).
- 12 *Ibid.*
- 13 Bell, *op. cit.* (note 3), p. 118 (5 December 1807).
- 14 Bell, *op. cit.* (note 1), p. 4–6.
- 15 Bell, *op. cit.* (note 3), p. 117–118 (5 December 1807).
- 16 Bell, *op. cit.* (note 1), p. 20.
- 17 Bell, *op. cit.* (note 3), p. 170 (12 March 1810).
- 18 Bell wrote, 'To this end I made experiments which, though they were not conclusive, encouraged me in the view I had taken', Bell, *op. cit.* (note 1), pp. 21–22.

- 19 Bell, *op. cit.* (note 1), p. 33.
- 20 Bell, *op. cit.* (note 3), p. 275 (10 June 1822).
- 21 For more on general changes in print culture in the nineteenth, see, for a few examples, Thomas Broman, 'J. C. Reil and the 'Journalization' of Physiology', in *The literary structure of scientific argument: historical studies* (ed. Peter Dear), pp. 13–43 (University of Pennsylvania Press, Philadelphia, PA, 1991); James A. Secord, *Victorian sensation: the extraordinary publication, reception, and secret authorship of vestiges of the natural history of creation* (University of Chicago Press, 2000), pp. 115–152; Alex Csiszar, 'Seriality and the search for order: scientific print and its problems during the late nineteenth century', *Hist. Sci.* **48**, 399–434 (2010).
- 22 See Cranefield and Bell, *op. cit.* (note 5); C. B. Jorgensen, 'Aspects of the history of the nerves: Bell's theory, the Bell–Magendie law and controversy, and two forgotten works by P. W. Lund and D. F. Eschricht', *J. Hist. Neurosci.* **12**, 229–249 (2003); Olmsted, *op. cit.* (note 5).
- 23 Edwin Clarke and L. S. Jacyna, *Nineteenth-century origins of neuroscientific concepts* (University of California Press, Berkeley, CA, 1987), p. 111.
- 24 For other accounts of the priority dispute, see Pierre Flourens, '1858 Memoir of Magendie, Translated by C. A. Alexander', *Annu. Rep. Smithsonian Instn.*, p. 100 (1866); John E. Lesch, *Science and medicine in France: the emergence of experimental physiology, 1790–1855* (Harvard University Press, Cambridge, MA, 1984); Mazumdar, *op. cit.* (note 9); Gillian Rice, 'The Bell–Magendie–Walker controversy', *Med. Hist.* **31**, 190–200 (1987).
- 25 For accounts of this dispute that favour Magendie, see Cranefield and Bell, *op. cit.* (note 5); Olmsted, *op. cit.* (note 5). And for accounts that favour Bell, see Alexander Shaw, *An Account of Discoveries of Sir Charles Bell in the Nervous System* (J. Murray, London, 1860); Amédée Pichot, *The Life and Labours of Sir Charles Bell* (R. Bentley, London, 1860).
- 26 Gordon-Taylor and Walls, *op. cit.* (note 5).
- 27 François Magendie, 'Expériences sur les fonctions des racines des nerfs rachidiens', *J. Physiol. Exp. Pathol.* **2**, 276–279 (1822).
- 28 Translated by Olmsted in Olmsted, *op. cit.* (note 5), p. 102.
- 29 A narrative account of these events can be found in François Magendie, 'Expériences sur les fonctions des racines des nerfs qui naissent de la moëlle epinière', *J. Physiol. Exp. Pathol.* **2**, 366–371 (1822).
- 30 *Ibid.*
- 31 Charles Bell, *An Exposition of the Natural System of the Nerves of the Human Body* (Spottiswoode, London, 1825).
- 32 *Ibid.*, pp. 3–4.
- 33 For more on vivisection in Britain, see Adrian J. Desmond, *The politics of evolution: morphology, medicine, and reform in radical London* (University of Chicago Press, 1989), pp. 183–192; Richard D. French, *Antivivisection and medical science in Victorian society* (Princeton University Press, 1975); Nicolaas A. Rupke, *Vivisection in historical perspective* (Croom Helm, London, 1987); Anita Guerrini, *Experimenting with humans and animals: from Galen to animal rights* (Johns Hopkins University Press, Baltimore, MD, 2003), pp. 70–93.
- 34 Hansard's Parliamentary Reports from 1825 states that 'there was a Frenchman by the name of Magendie, whom he [Mr. Martin] considered a disgrace to Society. . . . Mr. Martin added that he held in his hands the written declarations of Mr. Abernethy, of Sir Everard Home (and of other distinguished medical men), all uniting in condemnation of such excessive and protracted cruelty as had been practised by this Frenchman.' See T. C. Hansard, *Hansard's Parliamentary Reports*, 24 February (London, 1825).
- 35 Bell, *op. cit.* (note 3), p. 281 (8 December, 1823).
- 36 Thomas Neville Bonner, *Becoming a physician: medical education in Britain, France, Germany, and the United States, 1750–1945* (Oxford University Press, New York, 1995), pp. 98–103.

- 37 Charles Bell, 'Lectures on the Nervous System Delivered at the College of Surgeons', *Lond. Med. Gaz.* **1**, no. 21, pp. 617–622 (1828).
- 38 Charles Bell, 'Letter to the Editor on the Nervous System', *Lond. Med. Gaz.* **1**, no. 21, pp. 691–692 (1829).
- 39 *Ibid.*, p. 691.
- 40 For another version, see Sir Charles Bell, 'Clinical Lecture on Diseases of the Nerves of the Head', *Lond. Med. Gaz.* **13**, 697–702 (1834), at p. 699. Bell writes: 'When M. Magendie performed the experiments upon the spinal nerves, I saw that he went a great deal too far—farther than he was entitled to go by his premises. I saw that he was stating what he could not state from experiments, because his experiments were the same as mine. I had made out part of the subject—viz. that which related to the functions of the posterior roots,—by inference, and then confirmed the whole by the decisive experiments upon the fifth pair. He pretended to make the thing clear by experiments upon those nerves which I had puzzled at in vain, . . .'
- 41 Bell, *op. cit.* (note 40).
- 42 *Ibid.*, p. 699.
- 43 Gordon-Taylor and Walls, *op. cit.* (note 5), pp. 105–133 and 40–41.
- 44 Alexander Shaw, *Narrative of the Discoveries of Sir Charles Bell in the Nervous System* (Longman, London, 1839), pp. 88–94.
- 45 Charles Bell, 'On the Nerves; Giving an Account of Some Experiments on Their Structure and Functions, Which Lead to a New Arrangement of the System', *Phil. Trans. R. Soc. Lond.* **111**, 398–424 (1821).
- 46 Charles Bell, *The Nervous System of the Human Body: Embracing the Papers Delivered to the Royal Society on the Subject of the Nerves* (Longman, Rees, Orme, Brown & Green, London, 1830), p. 43.
- 47 Herbert Mayo and Johann Christian Reil, *Anatomical and Physiological Commentaries* (printed for Thomas & George Underwood, London, 1822), pp. 107–120.
- 48 Alexander Shaw, 'Letter to the Editor, Mr. Shaw in Reply to Mr. Mayo', *Lond. Med. Gaz.* **4**, 12–14 (1829).
- 49 *Ibid.*
- 50 Steven Shapin, 'Of gods and kings: natural philosophy and politics in the Leibniz–Clarke disputes', *Isis* **72**, 187–215 (1981); Adrian J. Desmond, *Huxley: from Devil's disciple to evolution's high priest* (Addison-Wesley, Reading, MA, 1997).
- 51 Editor, 'Controversy Concerning the Nervous System', *Lond. Med. Gaz.* **4**, 60 (1829).
- 52 'These things are contrived by cleverness, not by the power of knowledge'. Herbert Mayo, 'To the Editor of the Medical Quarterly Review', *Med. Q. Rev.* **2**, 451 (1834).
- 53 Shaw, *op. cit.* (note 44).
- 54 *Ibid.*, p. 45.
- 55 *Ibid.*, p. 7.
- 56 Charles Bell, 'On the Nerves of the Face', *Phil. Trans. R. Soc. Lond.* **119**, pl. ix (1829).
- 57 *Ibid.*, pl. viii.