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Luigi Galvani
and the Debate on Animal Electricity, 1791–1800

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Summary
Galvani's discovery provoked an animated debate that lasted for about a decade. So far, historians have studied only the controversy between Volta and Galvani. I show that a more extensive examination of the response to Galvani's treatise reveals a number of important issues that were characteristic of the contemporary physics and physiology but have not much attracted the attention of historians. In particular, the analysis shows the need to reappraise Galvani's role in establishing animal electricity.

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1. Introduction

Modern medicine owes some of its most spectacular successes to the use of electricity produced in the human body. A symbol of the modern age, bioelectricity at the same time has a long history. One of the unresolved problems in this history is how and when the concept of bioelectricity (or 'animal electricity', in old terms) was established. I intend to present here a new approach to this problem and in particular to concentrate on the role of Luigi Galvani (1737–1798), Professor of Obstetrics in Bologna.

In 1791 Galvani described a new phenomenon: a frog's leg in a nerve–muscle preparation contracted every time the muscle and the nerve were connected by a metal arc, which usually consisted of two different metals. To explain the new phenomena, which became subsequently known as 'galvanic phenomena' or 'galvanism', Galvani supposed that the contractions were produced by a flow of a fluid (later named 'galvanic fluid') between the muscle and the nerve. He suggested that this fluid was electrical and identical with the so-called 'nervous fluid', which was held to be the cause of all motions and sensations in animals.

For the next decade galvanic phenomena became a favourite topic of study for many scientists. An animated debate began about the nature of the phenomenon in which Alessandro Volta's opposition to Galvani is best known. Volta suggested that the electrical fluid originated not in the animal organs but at the contact of two different metals ('contact electricity'). This idea eventually led him in 1800 to the discovery of the electric pile, which immortalized Volta's name and shifted the focus of electrical investigations from physiological topics to physico-chemical ones.

Galvani's place in science has been summarized by I. Bernard Cohen as follows:

> To be sure, a major source of interest in this work [*De viribus*] will always be its stimulation of Volta to inaugurate a great revolution that affected every branch of physical science. Yet, Galvani inaugurated a new era in our knowledge of the physiology of nerves and muscles, and this must not be obscured by Volta's more far-reaching experiments.[1]

However, it is still not very clear what Galvani's contribution to physiology comprised. We know better what he did not accomplish than what he did. As Hebbel E. Hoff pointed out, Galvani was not the first who studied electrical stimulation of muscles or the physiological effect of a bi-metal, nor did he invent the electrical hypothesis of muscular contractions.[2] According to Hoff, Galvani deserves credit for his 'constant and indefatigable attempts to establish the truth of a current scientific hypothesis.'[3] On the contrary, W. Cameron Walker denied any purpose in Galvani's investigations and contrasted them to the work of Volta, 'directing experiments towards the attainment of a definite end'.[4] Where all historians have agreed is that Galvani's evidence for animal electricity was faulty, except for the case of a nerve directly contacting the muscle ('all-animal circuit').[5] They are also agreed that Volta correctly explained galvanic phenomena and refuted animal electricity. Some of them see the crucial argument against animal electricity in the discovery of the pile,[6] while others believe that Volta won the dispute even earlier.[7]

Consequently, there has been a tendency to lessen the importance of Galvani's work. Walker, for instance, stated that 'Galvani's discovery cannot be ranked as one of great scientific achievement.'[8] Karl Rothschuh tried to distinguish between the 'discovery' of animal electricity, which he attributed to Galvani, and its 'proof' accomplished by Carlo Matteucci (1811–1868) and Emil Du Bois-Reymond (1818–1896) in 1842–43. In his view, Galvani could not have given such a proof because the 'objective' means for registering the animal currents (galvanometers) were unavailable at his time.[9]

[1] I. Bernard Cohen, 'Introduction', in L. Galvani, *Commentary on the Effects of Electricity on Muscular Motion*, translated by Margaret Glover Foley (Norwalk, 1953), p. 41. (This translation will be cited hereafter as *Commentary*.)

[2] H. Hoff, 'Galvani and pre-Galvanian electrophysiologists', *Annals of Science*, 1 (1936), 157–72.

[3] Ibid., p. 169.

[4] W. C. Walker, 'Animal Electricity before Galvani', *Annals of Science*, 2 (1937), 84–113 (p. 111).

[5] Edmund Hoppe, *Geschichte der Elektrizität* (Leipzig, 1884; reprint: Wiesbaden, 1969), p. 118; Hoff (footnote 2), 169–70; Bern Dibner, *Galvani–Volta, A Controversy that led to the Discovery of Useful Electricity* (Norwalk, Conn., 1952), pp. 21, 29.

[6] A. Wolf, *A History of Science, Technology, & Philosophy in the 18th Century*, second edition revised by D. McKie (New York, 1961), I, 260.

[7] Sydney Gill, 'A Voltaic Enigma and a Possible Solution to it', *Annals of Science*, 33 (1976), 351–70.

[8] Walker (footnote 4), 111.

[9] K. E. Rothschuh, 'Von der Idee bis zum Nachweis der thierischen Elekttrizität', *Sudhoffs Archiv für Geschichte der Medizin*, 44 (1960), 25–44 (pp. 25, 36–41).

This approach ignored a number of questions, such as: (1) why Galvani's evidence appeared to be superior to that of his predecessors; (2) why Galvani's treatise met such an enthusiastic response; (3) why a number of scientists accepted neither Galvani's nor Volta's theories; and (4) why animal electricity survived its refutation by Volta. It seems there were two reasons for not addressing these questions. First, historians had considered Galvani's theory from the point of view of modern physiology and thus found it to be unworthy of interest. Second, they over-simplified the response to Galvani's discovery by reducing it to the Galvani–Volta controversy.

In fact, the division of the participants in the debate on the nature of galvanic phenomena into 'Galvanists' and 'Voltaists' is misleading, since very few scientists fully adopted either theory. The variety of views on the subject can better be described as different combinations of the responses to the principal points of Galvani's theory, namely that: (1) galvanic fluid originates inside the animal body; (2) it is a sort of electricity; and (3) it is identical with the nervous fluid. Thus, sometimes different critics of Galvani had very little in common among themselves.

I intend to argue that it was Galvani who established animal electricity as a scientific theory, and that this theory was neither refuted nor fully abandoned. In particular, I shall clarify: (1) what made Galvani's contribution to animal electricity different from that of his predecessors; (2) why initially physiologists became enchanted, and later disappointed, with animal electricity; (3) what was the actual meaning of Volta's 'refutation' of animal electricity; (4) what was the role of Volta's pile in the fate of Galvani's theory; and (5) how Galvani's contemporaries evaluated his discovery. The discussion concentrates on the period between publication of Galvani's treatise (1791) and Volta's discovery of the pile (1800). To determine the influence of Volta's discovery on the study of animal electricity, I shall also briefly review the decade from 1800 to 1810.

Being concerned with the establishment of a scientific concept, I am interested in the *common* views of large groups of scientists and their causes. It seems that none of the theories of galvanic phenomena was associated with a particular age, occupation, or nationality of scientists. Thus, an analysis of the intellectual aspect of the debate seems to be adequate to my task, at least in the first approximation. The problem of the origin of a *difference* in views of individual scientists or small groups of them is also of a considerable interest, and in this case a discussion of political, social, professional, national, and other non-scientific factors becomes indispensable. Although I touch upon some of them here and there, a comprehensive externalist analysis of the history of galvanism is beyond the scope of this paper.

2. Animal electricity before Galvani

Pre-Galvanian electrophysiology has been the subject of several studies, on which I rely here, except for a few details and some conclusions.[10] Briefly, the situation in the field was the following. Scientists had actively explored the connections between

[10] See footnotes 1, 2, 4, 9, and also W. D. Hackmann, 'The Researches of Dr. Martinus Van Marum (1750–1837) on the Influence of Electricity on Animals and Plants', *Medical History*, 16 (1972), 11–26, and his 'Electrical Researches', in *Martinus van Marum: Life and Work*, edited by R. J. Forbes, 6 vols (Haarlem, 1969–1976), III, 329–78; J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley, 1979), pp. 353–4, 491; R. W. Home, 'Electricity and the Nervous Fluid', *Journal of the History of Biology*, 3 (1970), 235–51; and Margaret Rowbottom and Charles Susskind, *Electricity and Medicine: History of Their Interaction* (San Francisco, 1984), pp. 15–30.

electricity and life since the 1740s. Physiologists found that electricity can stimulate muscular motions. Physicians applied this effect for the treatment of paralysis and other diseases. It had been proved that the shock obtained from the electric eel (*Gymnotus electricus*) or from the *Torpedo* was produced by electricity. The hypothesis had been brought forth that not only a few species of fish but all animals possess an innate electricity that participates in various life processes, muscular contractions being only one of them.

Numerous facts had been brought forth in favour of this hypothesis. Among them were sparks produced by animals or humans when being touched, a glow surrounding sometimes the heads of people and birds, and the effect of electricity on the germination of plants and on the heart-beat. Pierre Bertholon de St. Lazare (1742–1799), Professor of Physics in Montpellier, collected many of these facts in his influential book *De l'électricité du corps humain.* There were objections, however, that in many of these phenomena the sparks were due to friction electricity accumulated in clothes, stockings, or hair rather than animal electricity. Some cases still remained unexplained. One of them was the phenomenon observed by Domenico Cottugnio (1736–1822), Professor of Anatomy in Naples, in 1784. When he cut with a knife a muscle of a live mouse, grasped in his hand, he received a violent concussion.[11] Antonio Maria Vassalli (1761–1825), Professor of Philosophy in Turin, who was primarily interested in the effect of electricity on germination, discovered in 1790 another 'strange' phenomenon in animal life: the electrization of urine.[12]

The exact extent of Galvani's awareness of his predecessors' work is difficult to establish, for he gave very few references. However, even if he knew everything, it is difficult to agree with Hoff and Walker that his discovery was 'natural' and 'obvious', being prepared by the development of physiology and physics of the eighteenth century.[13] Their own facts contradict this idea.

First, the electrical hypothesis of muscular contractions was not very popular; such eminent physiologists as Robert Whytt (1714–1766), a Scottish physician; Albrecht von Haller (1708–1777), Professor of Botany, Anatomy and Medicine in Göttingen; Leopoldo Caldani (1725–1813), Professor of Theoretical Medicine and Anatomy in Padua; and Felice Fontana (1730–1805), Professor of Physics in Pisa, strongly opposed to it in the 1750s. One of the primary objections was that since all animal organs and tissues are conductors, electricity cannot pass from a nerve to a particular muscle without spreading to all neighbouring muscles. Lacking positive evidence in favour of the electrical hypothesis, its proponents concentrated on eliminating the other hypotheses of muscular contractions, such as the chemical and vibrational ones.

Secondly, most of the experiments with electricity applied to living things had nothing to do with the hypothesis of animal electricity. Indeed, physicians frequently tried to cure paralysis with electricity, but they were not interested in discovering the mechanism of the treatment. Physiologists also performed, especially during the 1740s and 1750s, a number of experiments to verify some physical and physiological

[11] 'D. Cottunio to G. Vivenzio, 2 October 1784', in G. Vivenzio, *Teoria e pratica della elettricità medica del Sig. T. Cavallo*, translated from English (Naples, 1784), p. 157. See the English translation of this letter in T. Cavallo, *A Complete Treatise on Electricity*, fourth edition, 3 vols (London, 1795), III, 6–8; and an excerpt from it in Walker (footnote 4), 104.

[12] A. Volta, 'Memoria prima sull' elettricità animale', *Giornale fisico medico*, 2 (1792), 146–87 (p. 150); also *Le opere di Alessandro Volta*, 7 vols. (Milano, 1918; reprint: New York, 1968), I, 15–35 (pp. 19–20). Hereafter this edition will be referred to as '*Opere*.'

[13] Hoff (footnote 2), 169; Walker (footnote 4), 84.

theories. For instance, Jean Antoine Nollet (1700–1770), a French physicist, and Giovanni Battista Beccaria (1716–1781), Professor of Physics in Turin, studied the change of weight of the electrified animals and humans, to check whether it was connected with the evaporation of water by electricity.[14a] Most of the experiments on muscular stimulation, in particular those of Leopoldo Caldani and Fontana aimed to verify Haller's theory of irritability and sensibility. According to it, only nerves possess sensibility and only muscles possess irritability. To produce a muscular motion, a stimulant should be applied to a nerve; but the nerve itself cannot move, only the corresponding muscle moves. The theory was demonstrated with mechanical, chemical, and heat stimulators, and the question was to determine whether electricity as an irritant would produce any different result. Among the exceptions was the experiment of a French physician Nicolas Philippe Ledrus (1731–1807), known as Comus, who made a plate for an electrical machine from dried nerves. The fact that the plate was electrified by friction proved, in his view, the identity of electricity and the nervous fluid.

Third, the analogy itself was not very helpful because by 1780 neither of the two concepts was sufficiently developed. The nervous fluid was supposed to be a fast and subtle agent, unobservable either by the senses or by a microscope, and with a particular affinity to nerves. As to electricity, in the seventeenth and early eighteenth century physicists viewed it only as a peculiar *property* of a body to attract other bodies after being rubbed with a cloth.[14b]

The discovery that this property can be transmitted from one body to another made them think of electricity also as of a *cause* of phenomena. The very term 'conductor of electricity' says that electricity was likened to a fluid. Whether the same fluid could account for so different phenomena as friction electricity, atmospheric electricity, the electricity of the crystal tourmaline, and the electricity of a few species of fish, was a matter of debate. Some scientists believed that the causes of all these phenomena were simply different modifications of the same fluid, while others treated them as distinct fluids.[15] The former group referred to the necessity of minimizing the number of causes, whereas their opponents suspected that such reduction might have led to a confusion of phenomena. They agreed, however, that to compare 'electricity' to the nervous fluid one had to select only those features that were common to all electrical phenomena. In other words, 'electricity' had to be defined as a very fast and penetrating agent, which existed in two states (positive and negative) and could be transmitted only by particular substances. However, the nervous fluid, as described above, certainly resembled such an 'electricity' no more than light or fire. In fact, some scientists believed that light, fire, and electricity were just different appearances of the same agent.[16]

Thus, the analogy between electricity and the nervous fluid could hardly be a very promising research subject before Galvani's time. This does not mean that the vagueness of this analogy made it totally fruitless, for as we shall now see, another

[14a] J. Nollet, *Recherches sur les causes particulières des phénomènes électriques*, new edition (Paris, 1754), pp. 366–91; G. Beccaria, *Dell' elettricismo artificiale e naturale* (Turin, 1753), pp. 124–35.

[14b] See Niels H. de V. Heathcote, 'The early meaning of electricity: some *Pseudodoxia Epidemica*', *Annals of Science*, 23 (1967), 261–75.

[15] P. Musschenbroek, *Introductio ad Philosophiam Naturalem*, 2 vols. (Leiden, 1762), I, 289; F. Fontana, *Traité sur le venin de la vipère*, 2 vols (Florence, 1781), II, 244.

[16] J. Nollet, *Essai sur l'électricité des corps*, third edition (Paris, 1753), pp. 119–37; J. Jallabert, *Expériences sur l'électricité* (Paris, 1749), pp. 263–4; and John Freke, *Treatise on the Nature and Property of Fire* (London, 1747), pp. 24–7.

vague analogy between electricity and the fluid emitted by some fish turned out to be successful.

It is commonly believed that the existence of electricity in fish was established in the 1770s by John Walsh and other English scientists. In fact, it was known earlier; in the 1770s it simply received more publicity and additional experimental support. The ambiguity in the dating of this discovery has no other reason than a change in the definition of electricity. After it was found in 1745 that a discharge from the Leyden jar produces a strong shock, some physicists suggested that the cause of the shock obtained from the electric eel was not mechanical, as had been thought before, but electrical.[17] Soon, however, physicists stopped viewing the shock as a sufficient proof of the presence of electricity. Joseph Priestley, for instance, considered the primary feature of electricity to be its ability to propagate only through particular substances (conductors).[18] The same was the opinion of Frans van der Lott. For this reason, when he discovered in 1761 that the fluid emitted by the electric eel passed through iron, lead, tin, copper, silver and gold and was stopped by sealing wax, he concluded that it was electricity. Peter van Musschenbroek made these experiments widely known.[19] In 1773 Walsh conducted similar experiments with the torpedo and arrived at the same conclusions. He also demonstrated the opposite polarity in the electrical organs of the torpedo and obtained an electrical spark from the electric eel.

The experiments of Walsh, Henry Cavendish and others appeared quite convincing. However, as Cavallo recalled in 1786, the failure of Walsh and others to obtain electrical sparks from the torpedo made some scientists question its electrical nature, and the situation did not improve until Walsh obtained a spark from the electric eel.[20] It is worth noting, however, that Walsh observed a spark from the electric eel only once, and perhaps because of that he published nothing about this experiment (it became known from some of the eyewitnesses). However, this circumstance did not deter a number of scientists from attributing the electrical nature not only to the electric eel but even to the torpedo, since, as Cavallo said, 'it would be scepticism to doubt, of the property of the torpedo being derived from the same cause as that of the gymnotus.'[21]

Thus, although electrical polarity and the sparks produced by several species of fish supported their electrical character, the identity of the conductors of the fish fluid with those of electricity still remained the main condition for that characterization even in the 1780s. As we shall see further, that provided experimentalists with an opportunity for verifying the analogy between electricity and the nervous fluid.

Establishing the electrical nature of the torpedo caused Fontana to suggest in 1781 that perhaps the electrical hypothesis of muscular contractions was worth further investigations. He said that

> not only the mechanism of the muscular motion is unknown, but we cannot even imagine anything that could explain it, and it seems that we are forced to

[17] J. N. Allamand, 'Kort Verhaal van de Uitwerkzelen, welke een Americaanse Vis', *Verhandelingen uitgegeeven door de Hollandse Maattschappy der Weetenschappen te Harlem* 2 (1755), 372–9; see also Walker (footnote 4), 89–90.

[18] J. Priestley, *The History and Present State of Electricity*, third edition, 2 vols (London, 1775; reprint: New York, 1966), I, 3–9.

[19] Frans van der Lott, 'Kort Bericht van den Conger-Aal, ofte Drilvisch', 6, ibid. (footnote 17), pt. 2 (1761), 87–95. P. Musschenbroek, *Introductio ad philosophiam naturalem*, 2 vols (Leiden, 1762), I, 290.

[20] T. Cavallo, *A Complete Treatise on Electricity*, third edition, 2 vols (London, 1786), II, 300–1.

[21] Ibid., p. 301.

> have recourse to some other principle, if not the ordinary electricity, at least *something analogous* to electricity. The electrical gymnotus and the torpedo make the thing if not probable, then at least possible, and one could believe that this principle follows the most common laws of electricity. It can be even more modified in the nerves than it is in the torpedo and the gymnoti. The nerves would be the organs destined to conduct this fluid and perhaps also to excite it; but all that still remains to be done. It is necessary first to ascertain by decisive experiments that the electric principle really takes place in contracting muscles. It is necessary to determine the laws which this fluid obeys in the animal body, and after all that there will still remain to be discovered what it is that excites this principle in us and how it does.[22]

However, Fontana's call for launching a special programme for the experimental demonstration of the electrical character of the nervous fluid had not changed the situation, for six years later George Fordyce said in his Croonian Lecture about the hypotheses of muscular contractions that 'no argument from *fact* has been employed to prove any one of them: I shall therefore leave them as mere *chimeras of the brain*'.[23]

Could the analogy between electricity and the nervous fluid have been established before Galvani? To some extent, yes. Here is an example of a simple experiment that was available then for this purpose. The experiment consists in cutting the crural nerve in a nerve–muscle preparation and connecting the two parts through various substances. If by stimulating mechanically or chemically the part of the nerve remote from the muscle one could excite contractions, then the intermediary substance transmits the nervous fluid. In this way one could have checked whether the conductors of the nervous fluid were the same as of electricity. If they were, then, according to the then prevailing criterion, the experiment would have proved the electrical nature of the nervous fluid.

This opportunity had been missed: no one performed a similar experiment before Galvani. Perhaps this fact will not appear so strange if it is recalled that most of the evidence supporting animal electricity was chance observation with a very few purposeful experiments. And even the most amazing among them attracted little interest. For instance, Volta was the only one who repeated Vassalli's observation of the charged urine, while Cottunio's mysterious phenomenon of a 'mouse-torpedo' remained unchecked until 1792.

Such indifference to the testing of animal electricity was typical of both supporters and opponents of animal electricity, although for different reasons. The former group, which included Bertholon, Vassalli, and others, believed that evidence for animal electricity was sufficient. Their antagonists, on the contrary, became more and more suspicious of the idea of animal electricity after discovering how many effects, attributed to it, were caused by frictional or atmospheric electricity. Their strategy was to let their adversaries prove the involvement of animal electricity instead of themselves trying to demonstrate its absence. For instance, when Volta found that Vassalli was right about the impossibility of explaining the electrization of urine by the known laws of electricity, he said that this argument is insufficient to acknowledge animal electricity, and that additional proofs are necessary to overcome his scepticism.[24] It

[22] Fontana (footnote 15), 244–5, italics added.
[23] G. Fordyce, 'The Croonian Lecture on Muscular Motion', *Philosophical Transactions*, 78 (1788), 23–36 (p. 26), italics added.
[24] Volta (footnote 12), 21.

seems that neither of the two groups considered the resolution of the controversy to be of any practical importance, for there was no connection between this theoretical debate and actual physiological investigations.

Certainly, Galvani's awareness of experiments on electrical stimulation and on electric fish was one of the prerequisites for his discovery. However, as shown above, the status of the electrical hypothesis of muscular motions was not such that could easily attract a researcher. There was nothing 'natural' therefore in Galvani's decision to adopt his hypothesis and actively defend it. Indeed, it appeared to be rather unusual for that time. This impression becomes even stronger after the study of Galvani's work and the response to it.

3. *De viribus electricitatis in motu musculari*

The earliest known work of Galvani on electrophysiology is dated 1780 and deals with electrical stimulation of nerves and muscles. At that time he developed a number of experiments, devices, and techniques that were modified only slightly or not at all in his treatise. One of them was the famous frog's nerve–muscle preparation (hind legs together with uncovered crural nerves and part of the vertebra) which served not only Galvani but also many other physiologists afterwards.

It is important to note that at the very early stage of his investigation of electrical stimulation of muscles and nerves Galvani had already been concerned with the verification of the electrical hypothesis. In 1781 he conducted a series of experiments to determine whether the nervous fluid is partly or fully electrical. He did not uncover any connection between the nervous fluid and electricity and switched to establishing the empirical laws of electrical stimulation, disregarding the nature of the nervous fluid.[25]

The new era began in April 1786 when he discovered that atmospheric electricity also produced muscular contractions.[26] While pursuing this investigation he discovered in September 1786 that a frog hung on an iron railing by an iron hook fastened to its spinal cord contracted not only in a storm but even in serene weather, especially when one pressed the frog against the railing.[27] When he placed a frog on a metal plate in his room and pressed the hook against it, he again obtained contractions. When a piece of an insulator was placed between the nerve and the muscle, no contractions occurred. Galvani concluded that the phenomenon was indeed electrical but different from atmospheric electricity. According to the views of his time, he believed that to produce a flow of electricity between a nerve and a muscle, there must be an electrical imbalance in these organs or in the adjacent metals. He supposed first that this electricity was produced by metals.[28] Soon, however, he decided that an imbalance of electricity cannot exist under ordinary conditions in a single piece of metal.[29] (In fact, he had two metal pieces, but the result was the same since they contacted one another.) The idea that *different* metals could produce a new electrical effect could have hardly occurred to Galvani, for he observed contractions even with identical metals. Thus, in his unpublished paper 'On animal electricity' dated 30

[25] *Memorie ed esperimenti inediti di Luigi Galvani* (Bologna, 1937), p. 287.
[26] Ibid., p. 387.
[27] Ibid., p. 33.
[28] Ibid., pp. 397–403.
[29] Ibid., pp. 36–7.

October 1786 he said that the electricity in question belongs to animals and not to metals.[30]

It took him five years to decide that his case was strong enough to present to the public, first as a journal article and then as a book.[31] A comparison with his early works shows that the first three parts of his treatise follow the sequence of his discoveries. An entirely new subject appeared only in the fourth part: various conjectures about the distribution of electricity in animal bodies and also about medical applications of electricity.

I shall now outline Galvani's most important results in both theory and experiment, as they were presented in his published treatise. First, the contractions occurred when a muscle was connected with a corresponding nerve through a communicating arc, which consisted solely of conductors of electricity. Secondly, when all conductors were made of the same metal the contractions were weak or totally absent, whereas they were much stronger when different metals were applied. Galvani never stated that a single metal cannot stimulate a muscle. As mentioned above, in his very first successful observation of galvanic stimulation he employed iron and iron, and not iron and brass as described in his treatise. Third, muscular contractions became stronger when a nerve was wrapped in a metal foil (the 'armature'). Fourth, contractions also occurred when an arc touched a nerve's armature and the bare nerve, or the armed portion of a muscle and its bare surface.[32] Fifth, muscles (or nerves) after being cut and subsequently connected with a conductor, retained their ability to react to a bi-metal.[33] Sixth, nerves, muscles, and other animal organs conducted electricity only when moist.[34] Seventh, the new phenomenon of muscular contractions was inherent in various types of animals.

Galvani was anxious to prove that there were no other causes of his phenomena than animal electricity. To exclude frictional electricity, that might have been on his body, from passing to a frog, he provided the communicating arc with a glass handle. A glass plate on which he placed the frog might have been easily electrified, so he substituted marble for glass. To isolate the frog from atmospheric electricity he submerged it in water. All these precautions did not stop contractions; thus, Galvani concluded that the new phenomenon had nothing to do with either atmospheric or artificial electricity.[35]

He could not know, of course, that some of his observations, such as contractions produced without any arc by a mere touch of a metal or a dielectric to an armed nerve (I shall call them the 'open circuit' phenomena), indicated the presence of an external source of electricity.[36] In all probability, the nerve was stimulated by an electric pulse that was produced by the sudden change of an electric field surrounding the frog. Galvani, however, as well as other proponents of the electrical nature of galvanism, thought of a continuous stream of electricity. Thus he needed a *closed* circuit and he assumed that it could be closed through the moist external surface of the nerve.[37]

[30] Ibid., p. 38.
[31] L. Galvani, 'De viribus electricitatis in motu musculari commentarius', *De Bononiensi scientiarum et artium instituto atque academia*, 7 (1791), 363–418.
[32] Galvani, *Commentary*, p. 65.
[33] Ibid., p. 67.
[34] Ibid., pp. 66–7.
[35] Ibid., pp. 68–9.
[36] Ibid., pp. 64, 69, 78, 81.
[37] Ibid., p. 81.

There was also the possibility that by pressing a metal against a nerve or a muscle one could irritate it mechanically. To eliminate this as a possible cause of his phenomenon, Galvani modified his experiments. He laid the nerve and the muscle on two plates of different metals that did not touch one another. When the arc was applied to these plates instead of to the animal parts, the contractions still occurred. In another experiment, Galvani immersed the frog's feet in a glass of water and the crural nerve in another glass. When an arc touched the water's surface in both glasses, the muscles contracted again.[38]

In his theoretical constructions Galvani often used the analogy between the nerve–muscle preparation and a Leyden jar: the external and internal surfaces of a muscle accumulate electricity of opposite sign as do the surfaces of a Leyden jar (a nerve contacts the internal surface). When the two surfaces are connected by a metal arc, electricity flows through it from one to the other until the electrical equilibrium is restored. (Incidentally, Galvani's name for the connector, 'arca conduttore', is the same as that for the discharger of a Leyden jar). Galvani assumed that the conducting core of a nerve is isolated by its oily cover from the other organs, and this prevents the flow of the electric nervous fluid from dissipating.[39]

Two conditions are required, according to Galvani, to excite the muscular contractions: (1) something must summon the nervous fluid from the muscle to the nerve and provoke its flow; (2) when the nervous fluid flows out of the nerve, something must absorb it and either convey it back to the muscle or carry it off elsewhere and dissipate it. To extend his theory from a nerve–muscle preparation to a nerve–muscle system in a living animal, Galvani had to find a biological equivalent of his communicating arc. Again he gave this role to the moisture on the external surface of the nerves.[40]

By carefully preparing his arguments and making all possible tests of his theory before publishing it, Galvani apparently hoped to avoid a controversy. He hardly expected the storm that followed the publication of his treatise.

4. The early response, 1792–1793

That Galvani's experiments, so curious and easy to perform, attracted the general attention is no wonder. What is surprising, however, is how many scientists entered the public dispute over the explanation of that experiment. The number of authors and publications is unusually large for so short a period of time. Their backgrounds and occupations varied. First, at that time there was no narrow specialization in science; thus, when applying to a person such names as 'physicist' or 'physiologist', I simply emphasize his prevailing interests at that period. Another reason for the heterogeneity of this group was that Galvani's discovery occurred at the junction of physics and physiology. I shall name those who made a significant contribution to the debate on animal electricity.

The group of 'physicists' comprises Volta, Vassalli, Giovanni Aldini (1762–1834), Professor of Physics in Bologna; Floriano Caldani (1772–1836), Professor in Padua; Giovaccino Carradori (1758–1818), Professor of Philosophy in Pistoya; Tiberius Cavallo (1749–1809), physicist from London; Georg Christoph Lichtenberg (1742–

[38] Ibid., p. 68.
[39] Ibid., p. 76.
[40] Ibid., p. 82.

1799), Professor of Physics in Göttingen; Friedrich Albrecht Carl Gren (1760–1798), Professor of Chemistry and Physics in Halle; Karl Friedrich Kielmeyer (1765–1844), Professor of Chemistry in Stuttgart; William Wells (1757–1817), a physician from London; Christoph Heinrich Pfaff (1773–1852), Professor of Medicine in Kiel; Giovanni Fabbroni (1752–1822), chemist, engineer, and physician; Alexander von Humboldt (1769–1859); Edward Ash (d. 1829), a physician from Oxford; and Johann Wilhelm Ritter (1776–1810), physicist from Jena.

The list of the 'physiologists' includes Galvani, Eusebio Valli (1755–1816), a physician from Pavia; Leopold Vacca Berlinghieri (b. 1768), Professor of Physics in Pisa; Alexander Monro (Secundus) (1733–1817), Professor of Anatomy in Edinburgh; Richard Fowler (1765–1863), a physician from Salisbury; Johann Caspar Carl Créve (1769–1853), Professor of Medicine in Mainz; Johann Bernhard Jacob Behrends (1769–1823), a physician from Mainz; Johann Christian Reil (1759–1813), Professor of Medicine in Halle; Johann Christoph Leopold Reinhold (1769–1809), Professor of Medicine in Leipzig; Bassano Carminati (1750–1830), Professor of Pathology in Pavia; Gottfried Philipp Michaelis (1768–1811), a physician from Hannover; Carlo Giulio (1757–1815), Professor of Anatomy in Turin; Francesco Rossi, a surgeon from Turin; Jean Francois Nicolas Jadelot, a physician from Paris; and Jean Claude Delametherie (1743–1817), the editor of the *Journal de Physique*.

This sort of division may be useful when comparing different goals set by scientists in their investigations and sometimes their different techniques. For instance, for the physiologist Galvani a frog was the objective of his study, while the physicist Volta saw in it only a sensitive instrument. Galvani was concerned with a discovery of what makes life *different* from inanimate nature, whereas Volta was interested in finding their *common* features. The division has a limited value, however, for sometimes placing a scientist in one of the two groups does not clearly account for his research style, as in the cases of Galvani, Aldini, Humboldt, or Wells. Nor does it allow us to predict the response to Galvani's discovery. I shall now outline the early reaction to Galvani's treatise leaving the details and references to the following sections.

The treatise apparently reached its readers only in the spring of 1792. Their first reaction was disbelief, and the first comments appeared not earlier than his results were confirmed.[41] Soon new experiments by Valli, Volta, Gren, Berlinghieri, and others followed. News about the discovery quickly spread from Italy to France, Germany, and England. Everyone praised Galvani, but there was no unanimity about his theory. Initially, Valli, Luigi Brugnatelli, and Volta fully supported Galvani's explanation of the new phenomenon through animal electricity. A few months later, however, Volta began to argue that the electricity in question originated outside the animal, namely, at the contact of different metals. Independently of him, Reil also supported this hypothesis. Volta supposed that the contact electricity was also responsible for the sour or alkaline taste produced by a bi-metal applied to the tongue. (He was then unaware that this experiment was described long ago by Johann Georg Sulzer (1720–1779) who attributed the effect to mechanical vibrations.[42]) However, Lichtenberg was rather sceptical about its electrical nature, and Fabbroni attributed the effect to a chemical action. In Gren's view, it was premature to draw any physiological conclusions from galvanic experiments.

[41] 'Intorno all'influenza dell'elettricità nel moto muscolare', *Giornale fisico medico*, 1 (1792), 280–1.

[42] J. Sulzer, 'Recherches sur l'origine des sentiments agréables et desagréables', *Histoire de l'Académie Royale des Sciences et Belles Lettres, Berlin*, 8 (1752), 350–90 (p. 356).

These reviews of Galvani's treatise stimulated the first wave of comments. Aldini defended Galvani, while Carradori supported Volta. Monro, Fowler, and Crève criticized both Galvani and Volta and questioned the electrical nature of galvanic fluid. Monro insisted that this fluid is not the nervous fluid but only a stimulus to it.

Thus, within a year after scientists had learned about Galvani's discovery they had already developed all the views on galvanism that they defended during the following decade. There were three major issues to dispute: (1) is galvanic fluid an electricity or not? (2) does galvanic fluid originate inside or outside an animal? and (3) is galvanic fluid the same as the nervous fluid or is it only a stimulus to the latter? Different combinations of the possible answers to these questions created a variety of views on galvanism. Very few adopted all of Galvani's answers to these questions; however, many agreed with him at least in one of these points. Two conclusions follow from the initial response to Galvani. First, his explanation of muscular contractions through the electrical hypothesis was not accepted as an obvious one. Second, as compared to other theories at the time, the reception of Galvani's theory was quite favourable.

I shall now examine in detail each of the three disputes, beginning with the one that has attracted most attention from historians, which is that between Galvani and Volta.

5. Does the galvanic fluid originate within or outside an animal?

The dispute between Galvani (aided by his nephew Aldini) and Volta has been the subject of numerous discussions. Explicitly or otherwise, most historians have approached it as the prehistory of the electric pile.[43] By contrast, my primary task will be to find how this dispute affected the fate of animal electricity.

By 1792 Volta was already a famous physicist known for his investigations of electricity, and it was natural for Italian scientists to seek his judgement of Galvani's experiments. Volta, on his side, was also anxious to verify Galvani's claims, for he himself had been interested in animal electricity as early as 1782.[44] He considered, however, only the electricity of several species of fish to be proven. He was very sceptical about the experiments of Cottunio, Vassalli, and some others as demonstrating animal electricity as a general property of the animal world.

In his first papers on galvanism Volta confirmed Galvani's experiments and fully agreed with him that the phenomenon was electrical. However, some details of Galvani's theory provoked his criticism.[45] He denied the possibility of an inherent imbalance of electricity between muscles and nerves, since these organs as well as surrounding tissues are conductors. Besides, he obtained contractions by connecting two points of the same nerve (Volta does not give credit to Galvani for this experiment). From Galvani's point of view, this should mean an imbalance of electricity in a nerve. Thus, concluded Volta, an alleged 'Leyden jar' may consist of other organs than a nerve and a muscle and Galvani's idea of the imbalance of electricity in animal bodies is false.

Secondly, Galvani's theory could not explain why different metals produce stronger contractions than did identical ones. To account for this, Volta assumed, contrary to Galvani, that in resting muscles electricity exists in a state of equilibrium.

43 See footnotes 5 and 7.

44 A. Volta, 'Lettera a M.me Le Noir de Nanteuil, 14 May 1782', *Opere*, I, 8–12.

45 A. Volta, 'Memoria prima', *Opere*, I, 15–35; 'Memoria seconda sull' elettricità animale', ibid., pp. 41–82.

This equilibrium can be disturbed by an act of will, or by external electricity, or by an application of different metals. According to the then popular theory, electricity was a fluid that pervaded all bodies. In Volta's view, every metal has a specific affinity to electricity; thus, different metals attract different quantities of electricity from animal organs. When these metals contact one another, electricity flows through the nerve restoring the electrical equilibrium and producing contractions. Two identical metals attract the same quantity of electricity and the equilibrium is preserved. Thus, according to Volta, metals are the 'motors' of electricity while the animal preparation is a mere passive conductor and a very sensitive electroscope. I shall call this theory of Volta's, which considered both animal organs and metals essential for producing electricity, his 'first theory'.

Many scientists, antagonists and supporters of animal electricity alike, shared Volta's criticism of the 'Leyden jar' model and tried to find a substitute for it. Valli supposed that electricity participates in living processes and thus is never at rest, providing an excess of electricity at one point relative to another.[46] This model seemed too vague compared to Volta's so many, in particular Aldini and Carradori, opted for the latter.[47]

Volta's model, however, was also flawed. First, it could not explain why contractions, although weak, nonetheless appeared in experiments with identical armatures. Second, a natural objection arose: what would happen if the identical armatures were connected by two different metals? Volta first came upon this problem when he studied stimulation of sensory nerves. In his unpublished letter to Martinus van Marum of 11 October 1792 Volta described how he applied two identical armatures to the tip and the back of his tongue and connected them to different metals immersed in a glass of water.[48] He felt the same acid taste if there were no armatures at all and concluded that in this case it is not the armatures that move the nervous fluid but the metals that contact the water. Before Volta's new ideas became known in England, Wells conceived in 1795 a similar experiment with motor nerves as an argument against Volta's theory.[49] He also remarked that if armatures attract electricity from animal organs, there is no reason why the excess of electricity does not flow back through the conducting animal tissues.

Volta inferred from this experiment that metals attract electricity from water and not from the tongue, which meant to him that electricity could be produced without animal organs. He did not yet deny the existence of animal electricity but only doubted whether it was the same electricity that one observed in many galvanic experiments:

> What still remains to be decided, and this is perhaps the most important, is whether after all these experiments prove not a true and proper animal elec-

[46] E. Valli, *Experiments on Animal Electricity with Their Application to Physiology, and Some Pathological and Medical Observations* (London, 1793).

[47] G. Carradori, 'Lettera . . . al Sig. Cav. Felice Fontana sull' elettricità animale', *Giornale fisico medico*, 2 (1793), 50–64; G. Aldini, 'De animalis electricae theoria ortu atque incrementis', in *Aloysii Galvani de viribus electricitatis in motu musculari commentarius cum Johannis Aldini dissertatione et notis* (Mutinae, 1792)', iii–xxvi. The references will be given to its English translation in Galvani, *Commentary on the Effect of Electricity on Muscular Motion*, translated by Robert Montraville Green (Cambridge, Mass., 1953), pp. 1–20 (p. 3). This book will be cited hereafter as '*Galvani on Electricity*'.

[48] A. Volta, 'Lettera seconda a van Marum, 11 October 1792', *Opere*, I, 133–41.

[49] W. Wells, 'Observations on the influence, which incites the muscles of animals to contract in Mr. Galvani's experiments', *Philosophical Transactions of the Royal Society of London*, 1795, 246–62 (pp. 249–50).

> tricity, which really depends on *vital forces* and the structure, but simply an *artificial electricity excited by a means hitherto unknown*; whether, I say, such an animal electricity proper exists, *as I maintain*, and by which experiments it can be established.[50]

Perhaps Volta came upon the idea of this artificial electricity even earlier, for in his letter to Cavallo of 13 September 1792 he said that most of the phenomena that have been attributed to animal electricity 'are in fact the effects of a very weak artificial electricity, which is undoubtedly excited by a simple application of two armatures of different metals'.[51]

Albeit reluctantly, he acknowledged that 'the animal electricity . . . exists and cannot be entirely abandoned'.[52] When discussing, for instance, the application of a bi-metal to a nerve he was still thinking in terms of the 'motion of the electric fluid inherent to the nerve itself'.[53]

Hence, in the fall of 1792 Volta admitted the independent existence of both animal electricity and contact electricity. The latter caused, in his view, most of the galvanic phenomena, while animal electricity was responsible for few of them. I shall call this theory Volta's 'second theory'.

It is worth noting that the idea that metals can produce electricity was not a new one. As mentioned above, Galvani and Reil had thought of it.[54] Abraham Bennet devoted a number of experiments to prove that the contact of different metals can produce electrical charges,[55] and the observer of the *Monthly Review* gave him the credit for the result.[56] Volta's real contribution consisted in a systematic application of this hypothesis and creating on its basis a theory (or rather several theories) of contact electricity.

Until 1794 Volta continued to admit the existence of animal electricity, for he needed it to account for galvanic stimulation with a single metal. Then he found a way to explain the latter phenomenon too. In his first letter to Vassalli of 10 February 1794, Volta stated that two pieces of the same metal or two ends of a metal wire cannot be perfectly homogeneous and therefore they behave as different metals. He discovered experimentally that a difference in temperature, hardness, or polish, artificially created in a metal, enhanced muscular contractions. With his theory now covering all galvanic phenomena, Volta said that to keep Galvani's theory would be an unnecessary multiplication of causes.[57]

Aldini immediately responded with a new version of the single-metal experiment: he used mercury, which was free from the heterogeneity suggested by Volta, and succeeded.[58] He also showed that muscular contractions could be stimulated with charcoal. A shy and mild person, Galvani shunned public polemics; thus he decided to defend his theory anonymously. In his book published in 1794 he gave a strong

[50] Volta (footnote 48), 141, italics added.

[51] A. Volta, 'Account of some discoveries made by Mr. Galvani . . . In a letter to Mr. Tiberius Cavallo', *Philosophical Transactions of the Royal Society of London*, 1793, 10–44. Also *Opere*, I, 173–208 (p. 180).

[52] Ibid., p. 180.

[53] Ibid., p. 181.

[54] F. Gren, 'Bemerkungen über die sogennante thierische Elektrizität', *Journal der Physik*, 6 (1792), 402–10 (pp. 409–10).

[55] A. Bennet, *New Experiments on Electricity* (Derby, 1789).

[56] *Monthly Review*, 11 (1793), 421–2; 13 (1793), 300–301.

[57] A. Volta, 'Lettera prima al Sig. Vassalli, 10 February 1794', *Opere*, I, 263–8 (p. 268).

[58] G. Aldini, *De Animali Electricitate. Dissertationes Due* (Bologna, 1794), dissertation I, pp. 5–9.

blow to Volta's theory by describing contractions that were obtained with a circuit consisting solely of a nerve and a muscle.[59]

In his second letter to Vassalli later in 1794 Volta analysed Aldini's experiments with mercury and charcoal and found both of them defective.[60] He believed that the effect was due to a difference in chemical content of the substances involved. In the case of the mercury, he suggested, it could be the different oxidation of the surface of mercury and its inner parts. The excitation with an all-animal circuit phenomenon was, according to Volta, the result of mechanical pressure. However, after repeating the all-animal experiment himself, Volta realized his mistake and decided to modify his theory. Before, he had stated that contractions take place only when the circuit contains two conductors of the first class (metals and solid bodies) separated by a conductor of the second class (liquids and humid bodies). In an unpublished letter to Banks of 30 March 1795 Volta remarked that contractions in an all-animal circuit were stronger when the nerve and the muscle were wetted with different liquids.[61] He concluded that in this experiment the contact of two liquids was the source of electricity and, therefore, not only can different metals be *motors* but also other heterogeneous substances. In his view, in the all-animal experiment electricity was produced by the contact of a muscle and a nerve with animal fluids (blood, saliva, or others). I shall call this view Volta's 'third theory'.

Few people learned about Volta's new ideas in 1795; the third theory became widely known only in 1796–97 from the publication of Volta's third letter to Vassalli and his letters to Gren. In those letters he systematized the results of his empirical study of galvanic circuits that excited muscular contractions. Among them he singled out as the simplest the following circuits: (1) two different conductors of the first class and one of the second; (2) two different conductors of the second class and one of the first; and (3) three different conductors of the second class.[62] The last case was the basis for the third theory that was created principally to explain the contractions produced without any metal.

When publishing his third theory Volta dated its origin back to 1792.[63] Indeed, in his unpublished letter to Tommaselli, Volta had conjectured that not only metals but all different conductors can disturb electrical equilibrium.[64] That was a natural extension of his original hypothesis about metals that disturb electrical equilibrium by attracting electricity from animal organs (or other bodies). Since the only property of metals utilized in this hypothesis was their conductivity, it could have been applied to other conductors as well. At this stage, however, Volta had no evidence to support this hypothesis nor did he feel any need to pursue it further.

Despite its generality, the third theory was not the last of his theories of contact electricity. While acknowledging all heterogeneous substances as capable of producing electricity, he never abandoned the hope of proving the exceptional role of metals.

59 'Supplemento al trattato dell'uso e dell'attivita dell'arco conduttore', pp. 3–23 (pp. 4–6); added to *Dell'uso e dell'attivita dell'arco conduttore nelle contrazione dei muscoli* (Bologna, 1794). Has been attributed to Galvani with possible assistance of Aldini; see John F. Fulton and Harvey Cushing, 'A Bibliographical Study of the Galvani and the Aldini Writings on Animal Electricity', *Annals of Science*, 1 (1936), 239–68 (pp. 260–62). The description of this experiment is translated in Dibner, *Galvani–Volta*, pp. 50–1.

60 A. Volta, 'Seconda lettera al Sig. Vassali [1794]', *Opere*, I, 271–81.

61 A. Volta, 'Lettera al Cav. Banks, 30 March 1795', *Opere*, I, 251–7 (p. 255).

62 A. Volta, 'Lettera prima al Prof. Gren di Halla, 1 August 1796', *Opere*, I, 395–413; see also *Neues Journal der Physik*, 3 (1797), 479–81.

63 A. Volta, 'Lettera terza al Sig. Vassali, 27 October 1795', *Opere*, I, 289–301 (p. 297).

64 A. Volta, 'Risposta alle domande dell' Abate Tommaselli [1792]', *Opere*, I, 113–8 (p. 117).

Not content with his empirical result that metals were more active *motors* than humid bodies, he was anxious to justify it theoretically. Before 1797, as shown above, Volta considered the production of electrical current in a circuit with metals to be caused by a difference in their attraction of electricity from a humid substance. However, in his second letter to Gren of August 1796 he said,

> One can *consider* this *mutual contact* of two different metals as the immediate cause that set the electric fluid in motion, instead of attributing this power to the double contact of these metals with the humid conductors . . .[65]

According to Volta, this principle allows one to explain all the phenomena without resorting to animal electricity. As it was first presented, the new principle was simply a more convenient manner of describing the production of galvanic electricity rather than a new physical law. For instance, let us say that the metal *A* gives momentum (+3) to electricity in the wet substance *a*, while the metal *Z* produces momentum (−2), ('+' stands for clockwise, '−' for counterclockwise). Thus, the resulting motion of electricity will depend on the net momentum (+1). According to Volta, this may be described *as if* the true cause of the motion of electricity was the mutual contact of the metals *A* and *Z*, in the sense that *A* acts on electricity contained in *Z* with the force (+3), whereas *Z* acts on electricity in *A* with the force (−2).

Before Volta sent this letter away, however, he obtained the evidence that transformed his hypothesis into a new theory (the 'fourth theory'): different metals produce electricity through their *mutual* contact.[66] By applying a very sensitive electrometer (the 'doubler of electricity') he demonstrated that two different metals after contacting one another showed electrical charges of different signs, from which he deduced that a contact of different metals produces electricity. In fact, the metals before being connected to the doubler contacted one another through a wet substance. Thus, what Volta assumed to be the electromotive force of a bi-metal was, in fact, that of a galvanic cell. Incidentally, he promised to repeat these experiments without any humid bodies involved, but never did.

The results of Volta's experiments with a doubler of electricity were not very convincing. Only Delamétherie agreed that they proved Volta's fourth theory.[67] Cavallo stated that his experiments with the doubler showed no sign of the contact electricity.[68] John Cuthbertson, a London instrument maker, repeated Volta's experiments and obtained different results. In his view, Volta's technique was unreliable.[69] John Bostock (1773–1846), a physician from Liverpool, did not mention them at all and attributed the discovery of contact electricity to Bennet.[70] Frederic Cuvier stated that Volta's work on the pile aimed to present a definite proof of his theory, which was still doubted by many scientists.[71] In other words, before 1800, many

[65] A. Volta, 'Lettera seconda al Prof. Gren, [August 1796]', *Opere*, I, 417–31 (p. 418); see also *Neues Journal der Physik*, 4 (1797), 107–35 (p. 109), italics added.

[66] Ibid., pp. 128–35.

[67] J. Delamétherie, 'Discours préliminaire . . . Du Galvanisme', *Journal de Physique*., 46 (1798), 37–44 (p. 43).

[68] Cavallo (footnote 11), III, 29–30.

[69] J. Cuthbertson, 'An Examination of Sig. Volta's Experiments which He Calls Fundamental, and upon which His Theory of Galvanism Rests', *Nicholson's Journal of Natural Philosophy*, 2 (1802), 281–9.

[70] J. Bostock, 'Outline of the History of Galvanism', *Nicholson's Journal of Natural Philosophy*, 2 (1802), 296–304 (p. 301).

[71] F. Cuvier, 'Rapport sur le galvanisme, fait à l'Institut National', *Journal de Physique*, 52 (1801), 318–21 (p. 318).

scientists refused to accept Volta's experiment with a doubler of electricity as the decisive proof of his theory.

Thus, according to Volta's fourth theory, in the circuits with metals, only metals can be *motors*, whereas the role of the wet substances is that of passive conductors of electricity. It is not clear whether Volta saw any contradiction between the fourth and third theories, for he never published a systematic account of his theoretical views. He might have neglected the active role of wet substances as negligible compared to that of metals.

It is worth noting that Volta never believed in chemical interaction between metals and liquids. The contact electricity 'predicted' by him was discovered only in the twentieth century; it had nothing to do with galvanic phenomena.[72] Although erroneous, his fourth theory might have helped Volta to discover his pile. Since he believed that the direction of the momentum given to electricity depends on the orientation of bi-metals relative to passive conductors, he could have realized that a proper connection of the bi-metals could increase this momentum.

Thus by 1797 both Galvani's and Volta's theories covered *all* galvanic phenomena. In order to compare them historians have resorted to modern physiology, namely to the fact that although both the electrochemical difference of potentials and the biological difference of potentials participate in galvanic phenomena the former is much greater than the latter. They concluded from this that Galvani was right only in his explanation of the all-animal circuit, where he discovered the so-called 'injury current', and wrong in all other cases, in which Volta's explanation was correct.[73]

This argument is deficient in two points. First, the injury current, which appears when an injured and uninjured part of a muscle are connected, was discovered by Carlo Matteucci and Emil Du Bois-Reymond in the 1840s. These scientists were concerned with *specific* currents in muscles and nerves, while for Galvani animal electricity was a uniform entity. He found in the all-animal experiment a confirmation of animal electricity and he did not search for its origin. Thus, although this experiment is proper to exhibit the injury current, Galvani should not be credited for what he never thought of.

Second, the bio-potentials (those, for instance, which produce the injury current exist practically in every preparation) are negligible as compared to those of electrochemical origin only in the circuits with some bi-metals. However, in circuits with a single metal, and some others, the bio-potentials are comparable in magnitude with the electrochemical ones. Thus, for these cases, there is no reason to consider Galvani's explanation to be less true than Volta's. J. F. Fulton, who represented the view of modern physiologists, believed that both were right:

> Galvani had asserted from the beginning the existence of animal electricity, but he denied that of metals. Volta, on the other hand, disallowed the existence of animal electricity and asserted that of dissimilar metals. It is now evident that both assertions were correct, and both denials incorrect.[74]

It is important to note that the possibility of the involvement in galvanic phenomena of more than one source of electricity was not strange to scientists at that time. Galvani himself discussed an addition of animal current and the current produced by

[72] See, for instance, B. I. Bleaney and B. Bleaney, *Electricity and Magnetism* (Oxford, 1957), pp. 85–7.

[73] Giulio C. Pupilli, 'Introduction', in *Galvani on Electricity*, (footnote 47), XIII; W. Walker (footnote 4), 111.

[74] J. Fulton, *Muscular Contraction and the Reflex Control of Movement* (Baltimore, 1926), p. 37.

a Leyden jar.[75] In Valli's view: 'The action of artificial electricity, as a stimulant of the nerves, does not become an argument against the theory of Professor Galvani, for that does not exclude the influence of the native electricity'.[76] Fowler stated: 'That this influence, . . . whatever it be, is not derived from the metals alone, but that animals at least contribute to its production . . . is . . . rendered highly probable by what I have already urged'.[77] However good, these statements were nonetheless only guesses. A two-causes theory of galvanic phenomena would have been too complicated for developing at that time.

If modern science does not justify the bias towards Volta's theory, let us see whether his contemporaries had better reasons. To prove the truth of their explanation of phenomena, Galvani and Volta had to use different strategies. Volta's task was to demonstrate the contact electricity by direct experiment. He thought to accomplish it, at least for metals, in his experiment with a doubler of electricity. On the contrary, to prove the existence of animal electricity, Galvani had to eliminate *all* other possible causes of muscular contractions. This problem was much more difficult, if feasible at all, for he could not exclude the unknown causes. As to the known irritants, Galvani had repudiated their possible intervention before he announced his discovery. He did that quite satisfactorily, except for electrical phenomena in the open-circuit experiments which were far beyond the resources of physics of his time. When Volta suggested the involvement of a new stimulant—the electricity produced by metals and other solids—Galvani, aided by Aldini, excluded the new one too by obtaining contractions in an all-animal circuit devoid of inorganic matter.

When two theories explain different aspects of the same phenomenon, it is difficult to bring evidence *against* them. That was what happened in the case of animal electricity versus contact electricity. Galvani and Aldini challenged Volta's early theories with new experiments, such as the single-metal circuit or the all-animal circuit. After 1795 this common tactic was useless, for Volta's third theory was so general that it became invincible. Volta, in his turn, did not offer any experiment to invalidate animal electricity. His alternative explanations of Galvani's experiments could not fulfill this function. For instance, although he showed that the heterogeneity, artificially created in a metal, may be large enough to produce contractions, he was unable to prove that in the observations by Galvani, Aldini, and Humboldt the heterogeneity was as big as in his own experiments.

It was hardly possible to decide between the two theories on the basis of the number of phenomena being explained. Galvani's theory, for instance, could not explain the role of different metals, or why there was no contractions when a nerve and a muscle were connected with a conducting liquid (water).[78] Volta's theory, in turn, had a no less serious defect, for it could not explain why an electrization by friction of the armature of a nerve did not produce muscular contractions.[79] Certainly, Volta's third theory would have given him the advantage, had he proved it in all its generality. But he never tried to do that.

[75] L. Galvani, 'Lettera al Sig. Prof. Don Bassiano Carminati', *Giornale fisico medico*, 2 (1792), 131–45. See also *Galvani on Electricity*, pp. 90–1.

[76] Valli (footnote 46), 175.

[77] R. Fowler, *Experiments and Observations Relative to the Influence Lately Discovered by Galvani and Commonly Called Animal Electricity* (Edinburgh, 1793), p. 58.

[78] C. Pfaff, 'Abhandlung über die sogenannte thierische Electrizität', *Journal der Physik*, 8 (1794), 196–280 (pp. 263–4).

[79] A. Vassalli, 'Lettre à J. C. Delamétherie sur le galvanisme, et sur l'origine de l'électricité animale', *Journal de Physique*, 48 (1799), 336–9.

Consequently, Galvani's and Volta's theories had to appear of equal plausibility to their contemporaries, and so it was for a while. If, as will be shown, they later changed their opinion, that was because they believed that Volta's theory met *new* challenges more successfully than Galvani's.

While debating whether the galvanic fluid originates inside or outside an animal, Galvani and Volta had no doubt about its electrical nature. However, many did not share this view. We shall see in the next section how they treated the problem of the internal versus external origin.

6. Is galvanic fluid an electrical or non-electrical fluid?

Historians have implied that most scientists adopted the idea of the electrical nature of galvanism as something natural. As shown above, establishing the identity of different fluids with electricity before Galvani was a complicated matter, for it depended on the changing definition of electricity and on the methodological views of the scientists. So it remained during the 1790s. In contrast to the fluid produced by a few species of fish, galvanic fluid was supposed to be a general property of the animal world; thus, scientists were more cautious when making a judgement about its nature. Many arguments that were used in the dispute on electric fish were repeated in the controversy over galvanic fluid being electricity. A number of scientists, Wells for instance, still believed that to establish the electrical nature of a fluid, it was sufficient to prove that it is transmitted through the same substances as electricity.[80]

However, Galvani decided to give more proofs of the electrical character of galvanism. He compared some features of galvanism with friction electricity and others with the electricity of the torpedo.[81] In particular, he pronounced the following properties as common to animal electricity and ordinary (friction) electricity: (1) the conductors of the two agents are the same, as well as their non-conductors; (2) both fluids seek an easier path, such as an arc, an angle, or a point; (3) both are of two kinds, positive and negative; (4) animal electricity adheres to the muscles for a long time, as does ordinary electricity to bodies; (5) the renewal of animal electricity is spontaneous and is not limited to a short time; and (6) the strength of animal electricity considerably increases when an armature is employed. The last point implies an analogy with a Leyden jar, which accumulates more electricity when its surface is larger. The fifth point also relates to this analogy and stresses a difference between, rather than the similarity of, the two fluids.

According to Galvani, the following features were common to animal electricity and electricity of fish: (1) both need an electrical circuit from one part of the animal to the other; (2) neither affects an electroscope; (3) neither needs a preliminary device (heating, rubbing, and so on) to be excited. However, he adds, while electricity of fish can produce a shock or a spark, animal electricity cannot.

The supporters of the electrical hypothesis of galvanism added several physical arguments to Galvani's (the physiological ones will be discussed in the next section). They said that both agents: (1) are very fast (Valli, Aldini, and Halle); (2) have a great penetrating ability (Aldini, Halle); (3) produce muscular contractions; (4) produce a

[80] Wells (footnote 49), 260. The situation in the 1790s will be understood better if bearing in mind that as late as 1833 Michael Faraday was preoccupied with demonstration of the identity of 'electricities' observed in different phenomena; see his *Experimental Researches in Electricity* 3 vols (London, 1839–1855; reprint: New York, 1965), I, 76–102.

[81] Galvani, *Commentary*, pp. 77–8.

similar taste (Volta), (5) are similarly affected by heat (Valli); and (6) are the most powerful stimuli of muscles.

Their opponents emphasized the following differences between galvanic fluid and electricity: (1) galvanic fluid does not affect an electrometer (Fowler, Cavallo); (2) unlike electricity in the Leyden jar, it produces no sparks and gives no shocks to people (Cavallo, Kielmeyer); (3) galvanic fluid can flow through an open circuit (Monro, Fowler, Humboldt, Jadelot); (4) it can be stopped by such conductors of electricity as bone, fire, and rarefied air (Humboldt); (5) metals better transmit electricity than charcoal, but for galvanic fluid the reverse is true (Cortambert)[82]; (6) unlike the electricity of the torpedo, galvanic fluid does not depend on the will (Fowler); (7) unlike the electricity of the torpedo, galvanic fluid does not give a shock to people (Fowler); (8) unlike frogs, the torpedo does not contract itself under the action of its own fluid (Fowler); (9) a shock from the torpedo does not pass through a brass chain, while galvanic fluid does (Fowler); (10) the sensation of taste produced by electricity and galvanic fluid is not the same (Fowler); (11) if galvanic fluid were animal electricity, it would be exhausted after the first connection of a muscle and a nerve as in a Leyden jar (Fowler, Pfaff); (12) in Sulzer's phenomenon galvanic fluid has less penetrating ability than electricity and it acts continuously while electrical effect should be instantaneous (Fabbroni); (13) galvanic phenomena take place whether an animal is isolated or not; (14) if galvanic fluid were electricity, it could not be accumulated in nerves surrounded by conducting tissues (Cortambert); and (15) galvanic fluid cannot be an external electricity, since electrifying by friction a nerve's armature does not produce contractions (Vassalli).

The proponents of the electrical hypothesis tried to counter the objections with new experiments and reasonings. For instance, Valli wrapped in the same armature the nerves of fourteen frogs. When he connected the armature with the muscles, the muscles contracted and his electrometer showed the presence of electricity.[83] Fowler failed to obtain Valli's effect and questioned his result, nonetheless some scientists accepted it as true. Volta argued that galvanic fluid is electricity of a very low tension (as in a slightly charged battery of Leyden jars), and that is why it does not produce sparks and shocks and does not repel the ball of an electrometer. This explanation had a flaw, however, for the torpedo shows at the same time the signs of both high tension (shocks) and low tension (no sparks). According to Wells, the experiment with an electrometer is inconclusive, for this instrument does not react to a discharge of a Leyden jar too.[84] In modern terms, this means that an electrometer is an improper instrument to measure an electric current.

Let us discuss in more detail the most popular arguments against the electrical origin of galvanism. In Monro's view, for instance, the similarity of muscular contractions excited by atmospheric electricity and bi-metals does not require the identity of the two agents, thus 'Prof. Galvani and Dr. Valli have allowed preconceived theory to conduct their experiments, instead of allowing their experiments to conduct their theory'.[85] Monro observed that contractions occurred even when a communicating

[82] Cortambert, 'Extrait d'une mémoire sur le galvanisme', *Mémoires de la Société Medicale d'Emulation*, 1 (1797), 232–6.

[83] E. Valli, 'Lettres sur l'électricité animale', *Journal de Physique*, 41 (1792), 66–77, 185–202, 435–7; 42 (1792), 74–5 (p. 77).

[84] Wells (footnote 49), 261–2.

[85] A. Monro, 'Experiments Relating to Animal Electricity', *Transactions of the Philosophical Society of Edinburgh*, 3 (1794), 231–9 (p. 236).

arc connected an armed nerve and another metal which contacted not the muscle itself but other organs of the animal or did not touch the animal at all. This phenomenon, in his view, strongly opposed galvanic fluid being electricity. In fact, in some of the cases he did not take into account the conductivity of animal tissues which allows contact to be made with the muscles indirectly, while in others he dealt with the phenomenon of the 'open circuit', which he did not understand.

Having failed to repeat Cottunio's observation with a mouse and that of Valli with an electrometer, Fowler correctly suggested that both authors deceived themselves.[86] However, he was not always right himself: he concluded, for instance, that a single metal or charcoal does not produce contractions. Fowler concluded that galvanic phenomena are primarily due to some *non-electrical* action of metals, with an additional contribution from animal organs.[87]

Créve cut a nerve in half and put a drop of oil between the two parts: contractions continued. Then he observed contractions in a frog wholly submerged in oil. From these and other similar experiments (some of which were those of the 'open circuit' type) Créve concluded that galvanic phenomena have nothing to do with electricity.[88]

Humboldt was one of the leading critics of the theories of Galvani and Volta. He improved several of Galvani's and Aldini's experiments and confirmed the possibility of stimulating a muscle with a single metal (mercury) or with an all-animal circuit. He argued that in the latter case the circuit can consist of only two substances—a nerve and a muscle—while Volta's theory required three. Many experimenters observed the open-circuit phenomenon, but it was Humboldt who first claimed it to be the strongest argument against the circulation of electricity in a galvanic circuit. In one of his experiments he cut the crural nerve of a frog, separated the two parts by 2 mm and applied an arc to the muscle and the cut-off nerve: contractions continued. Humboldt conjectured that the special atmosphere surrounding nerves, imagined by Reil, is capable of closing the gap in the circuit.[89] In another experiment he was able to excite contractions by simply touching a nerve's armature with a metal that had no connection with the muscle.[90]

Philipp Michaelis suspected that it was moisture absorbed by a glass plate that made the connection in Humboldt's experiments.[91] In 1797, however, Humboldt confirmed his original results and gave more details about his observations.[92] He emphasized that the physiological conditions of animal organs, namely their sensitivity or excitability, plays an important role in the outcome of galvanic experiment. He found, for example, that the radius of the 'nerve atmosphere', estimated by the gap in a circuit, depends on the 'vitality' of a preparation, and it drops from 2 mm to zero in 5–8 minutes after a frog is prepared; thus, the experiments can succeed only with very fresh preparations.

[86] Fowler (footnote 77), 47, 51.

[87] Ibid., pp. 56, 58.

[88] C. Créve, 'Beitrage zu Galvani's Versuchen . . .' *Journal der Physik*, 7 (1793), 323–31; 'Extrait . . . de la découverte du Prof. Créve, sur la nature de l'irritation metallique', *Mémoires de la Societe Medicale d'Emulation*, 1 (1797), 236–7.

[89] A. Humboldt, 'Ueber die gereizte Muskelfaser, aus einem briefe an Hrn. Blumenbach', *Neues Journal der Physik*, 2 (1795), 116–23 (pp. 122–3).

[90] A. Humboldt, 'Neue Versuche über den Metallreitz . . . Aus einem Briefe an den Herrn Blumenbach', *Neues Journal der Physik*, 3 (1796), 165–84 (pp. 177–8).

[91] P. Michaelis, 'Ueber die gereizte Muskelfaser', *Neues Journal der Physik*, 4 (1797), 1–27.

[92] A. Humboldt, *Versuche über die gereizte Muskel- und Nervenfaser nebst Vermuthungen über den chemischen process des lebens in der Thier- und Pflanzenwelt*, 2 vols in 1 (Berlin & Posen, 1797), pp. 42–48, 82–88, 213–221.

Humboldt had followers in studying the role of the physiological factors involved in galvanic phenomena. After comparing all the properties of electricity and galvanic fluid, Reinhold concluded in 1798 that 'if this galvanic fluid exists, it is of different nature than electricity, and it is only provided by living beings'.[93] In 1799 Jadelot confirmed several difficult observations of Humboldt, including that with an open circuit. His conclusion was that galvanic fluid resides in animal bodies and is not electrical; metals or other external bodies just set this agent in motion.[94]

We see that the arguments for the non-electric nature of galvanism were substantial and they were not properly corrected before 1800. That was due to lack of understanding of both the new kind of electricity and of the mechanism of the nervous action. Scientists were unaware that a slight modification of an experiment, which remained undetected, could have completely changed the result of some difficult observations from positive to negative. Accordingly, each side interpreted these observations to its advantage.

In other cases, not the experimental result but its interpretation was the subject of debate, and opinions varied according to the criteria applied. The supporters of the electric hypothesis were content with a lesser amount of evidence in its favour than were their rivals. For instance, the 'pro-electrical' party was content with galvanic fluid stimulating the muscles of some animals, while the 'anti-electrical' group insisted that if galvanic fluid is electricity, it must excite human muscles as well. When disputing Sulzer's phenomenon, the former asserted that the taste produced by electricity and galvanic fluid was the same, while the latter claimed that it was not exactly the same. However convincing their criticism of the electrical theories was, its authors realized that it would not solve the problem; thus, they launched a search for an alternative solution which led them to chemistry.

Fabbroni apparently was the first to assert that galvanic phenomena depend on the chemical action of metals, which decompose the humid substance in the circuit and animal lymph.[95] He said that if the instantaneous muscular contractions is an effect of electricity, the continuity of the taste produced by a bi-metal indicates a chemical rather than electrical cause.[96] In 1796 Créve found that two different metals or a metal and charcoal decompose water surrounding a nerve or a muscle. In his view, if oxygen combined with the metal and hydrogen with caloric, the latter union produces an electric substance which is the most immediate cause of the stimulation (here he referred to Gardini's 'demonstration' that the electric fire is composed of hydrogen and caloric).[97]

Ash informed Humboldt that when zinc and silver plates wetted with water contacted one another, the zinc became oxidized and the silver was covered with a fine white powder. He observed similar effects with lead and mercury and also with iron and copper.[98] Humboldt repeated these observations and found them supporting his own theory.

[93] J. Reinhold, *De Galvanismo*, 2 vols (Leipzig, 1798), II, 72. Also see P. Sue, *Histoire du galvanisme*, 2 vols (Paris, 1802), I, 193.

[94] J. Fr. N. Jadelot, 'Discours préliminaire', in A. Humboldt, *Expériences sur le galvanisme, et en général sur l'irritation des fibres musculaires et nerveuses*, traduction par J. Fr. N. Jadelot (Paris, 1799), pp. i–xxxviii (pp. viii, ix, xxxiv).

[95] Humboldt (footnote 92), 462–3.

[96] G. Fabbroni, 'Sur l'action chimique des différens metaux entr'eux, à la temperature commune de l'atmosphère, et sur l'explication de quelques phénomènes galvaniques', *Journal de Physik*, 49 (1799), 348–57.

[97] Humboldt (footnote 92), 462–4.

[98] Ibid., pp. 472–5.

According to Humboldt's theory, 'galvanic phenomena are the phenomena of irritation. Certain substances set in communication with excitable organs and arranged in chains or in other ways make these organs pass from rest to a condition of action'.[99] He agrees with Galvani that there is a discharge of a fluid between muscle and nerve, which results from an accumulation of this fluid and its unequal distribution in animal organs, but he denies its electrical character. In his view, the particles of different elements, such as phosphorus, nitrogen, hydrogen, oxygen, and others have a tendency to approach one another and unite, and this tendency is modified by the distance between the particles and the cooperation of caloric, electricity, and possibly light. Normally, the attractions of various elements balance each other in such a way as to create an equilibrium, which corresponds to a relaxed fibre. Addition or subtraction of chemical substances destroys the equilibrium and thus produces a contraction of the fibre: that explains chemical stimulation. Referring to the fact that blood is oxidized by a muscular action which in turn depends on galvanism, Humboldt conjectured that the role of galvanic fluid is in helping hydrogen and nitrogen to combine with oxygen, which is supposed to be a precondition for a muscular contraction.[100]

Humboldt based his belief in the connection between galvanism and chemical processes on the hypotheses of others about the compound character of electricity. According to Gren, it contains a combustible substance and an acid; Lichtenberg believed that it includes caloric, oxygen, and hydrogen; and Lampadius suggested such components as caloric, phlogiston, light, and a phosphorescent base. Humboldt agreed with them about caloric and light but totally denied that electricity contains any chemical substances. He stated that an electric spark simply assists in uniting the chemical substances existing in vegetable or animal organs, rather than releasing an additional chemical component. In this way he explained why electricity changes the colour of petals or produces an acid taste when applied to the tongue.[101]

It is premature, Humboldt says, to argue about whether galvanism is a modification of electricity when so little is known about electricity itself: the phenomena of electric fish, for instance, are as obscure as the galvanic ones. He is referring here to the inability of the electricity of fish to affect the electrometer or produce a spark (except for the electric eel). He concluded that 'not all that metals are conductors of and that glass isolates should be regarded as electricity'.[102] Thus, Humboldt's criterion of electricity was very different from that of Walsh and Wells.

Pfaff severely criticized Humboldt for the improper, in his opinion, use of chemistry in physiology.[103] Others, on the contrary, were very interested in the connection between galvanism and chemistry. Johann Friedrich Ackermann (1726–1804), Professor of Medicine in Kiel, for instance, supposed that galvanic action consisted in the passage of oxygen from one metal to another through a muscle.[104] Ritter discovered that the chemical processes on zinc and bismuth plates immersed in water or an alkaline solution went differently when the circuit was closed from when it was open. In his view, galvanism is a more complex concept than those of electricity and

[99] Ibid., p. 355.
[100] Ibid., pp. 397–400.
[101] Ibid., pp. 444–5.
[102] Ibid., p. 452.
[103] C. H. Pfaff, 'Ein Beytrag zu Alex. von Humboldts 2tem Bande der Versuche über die gereizte Muskel- und Nervenfaser', *Nordisches Archiv für Natur- und Arzneywissenschaft*, 1 (1799), 17–43.
[104] F. L. Augustin, *Versuch einer vollstandigen systematischen Geschichte der galvanischen Electricitat und ihrer medicinischen Anwendung* (Berlin, 1803), p. 36.

chemistry, and galvanic action incorporates in itself electrical and chemical actions.[105]

Thus, there was no more unanimity among the opponents of the electrical theory than among its supporters. In particular, they also argued whether galvanic fluid originates outside animal bodies (Fabbroni, Ackermann) or within them (Humboldt, Reinhold, Jadelot). Contrary to what is usually implied, not many scientists, judging by published sources, supported the electrical nature of galvanism by 1800. Their number was relatively greater among physicists than among physiologists. Physicists were very sceptical about the arguments presented by their physiologist opponents, for they distrusted their skill (Fowler, for instance) in carrying out experiments on electricity. It seems that the national factor had some impact too: the opposition to the electrical hypothesis was somewhat weaker in Italy (Fontana, Fabbroni) than in Germany (Humboldt, Crêve, Reinhold, Ackermann). Whether the cause was the authority of Volta, a leading electrician of that time, is still unclear.

Moreover, the supporters of the electrical hypothesis could not agree whether they had proved their identity of galvanic fluid and electricity or only their analogy. As shown above, an 'analogy' meant acknowledging the existence of many electricities, and Galvani himself introduced galvanic fluid as one of the family of electricities. Using the analogy with different gases, Cavallo supposed that 'there may be several sorts of more subtle fluids essentially different from each other, yet bearing some analogy to the electric fluid.'[106] Kielmayer held a similar opinion.[107] According to the Commission on Galvanism of the Paris Academy of Science, Volta's experiments proved only an analogy between the galvanism and electricity.[108] The weakening of the claim from the 'identity' to the 'analogy' between galvanic fluid and electricity accounted for the numerous differences between the two fluids. In some cases this idea stimulated new discoveries, while in others it rather delayed them.

Since physics did not provide enough evidence to resolve the dispute on the nature of galvanism, scientists turned to metaphysical arguments. An explicit and very popular one was the call for minimizing the number of causes. It was applied to remove animal electricity as the rival of contact electricity (Volta, Reil), as well as to substitute electricity for galvanism (Wells).

Another possible reason for scientists adopting or rejecting a particular theory appears to have been their attitude towards such issues as vitalism and mechanism. Few explicit statements of that type can be found, however. Erasmus Darwin said, for instance, that 'animal contraction is governed by laws of its own, and not by those of mechanics, chemistry, magnetism, or electricity'.[109] Gren once remarked:

> The name *animal electricity* appears to me not well chosen, for it leads to the cause which perhaps does not exist at all. One should not use this name because of its association with this weird *magnetizer* [Mesmer]. Since *true* physicists

[105] F. Augustin (footnote 104), 42; J. W. Ritter, *Beweis dass ein bestandiger Galvanismus den Lebenprocess in dem Thierreich begleite* (Weimar, 1798), pp. 65, 172.

[106] Cavallo (footnote 11), III, 72.

[107] K. Kielmayer, 'Versuche uber die sogenannte animalische Electrizitat', *Journal der Physik*, 8 (1794), 65–77.

[108] Halle, 'A la Classe des sciences mathématiques & physiques de l'Institut National, des premières expériences faites en floreal & prairial de l'an 5, par le commission nommée pour examiner & verifier les phénomènes du galvanisme', *Journal de Physique*, 47 (1798), 392–401, 441–68 (p. 466).

[109] E. Darwin, *Zoonomia*, 2 vols (London, 1794), II, 65–6.

assumed the identity of electrical and magnetic matter, they may wish to establish a connection between *animal electricity* and *animal magnetism.*[110]

There are several interesting points here. First, that Mesmer is not a 'true' scientist. (Valli called him a 'celebrated impostor', Humboldt referred to 'Mesmer's charlatanism'.[111]) Secondly, that, contrary to imaginary 'animal magnetism', animal electricity is a real physical phenomenon. Third, it appears from the context of this passage that Gren objects to the term 'animal' as implying something specific exclusively to animal beings. This may give a clue to understanding Volta's struggle against animal electricity.

The terms 'vital' or 'animal' can mean two things: (1) the phenomenon originated inside the animal organs; (2) that it is due to specific forces that act only in animal bodies and not reducible to the known physical laws. The first option is unlikely in Volta's case. He could not have denied the possibility of electricity inherent in animals, for, as mentioned above, he knew as early as 1782 that the electricity of the torpedo was its innate property. Therefore, in this case 'animal' stood for Volta for 'non-physical' and not for 'internal'. As soon as he found in 1800 a physical explanation of the electricity of the torpedo, it ceased to be 'animal' for him and became just contact electricity, one of several 'physical' electricities.

As applied to frogs and other animals, the term 'animal electricity' had different meanings at different times. When Volta was not yet sure that his contact theory was applicable to experiments with a single metal or with an all-animal circuit he treated 'animal' electricity as an internal electricity of vital origin, opposing it to the external contact electricity. At this stage his tactic was to emphasize the unimportance of 'animal' electricity as the cause of only a very few phenomena. The situation changed after Volta offered his third theory. The 'animal electricity' became a physical phenomenon for him, and from then on he applied this term only when criticizing those who continued to treat this agent as of 'vital' or non-physical origin.

That Volta did not distinguish a contact of two metals from that of a nerve and a muscle means that he did not expect from animal matter any peculiar action different from that of an inanimate substance. Thus, it seems that it was Volta's reductionism rather than physical arguments that precluded him from admitting the possibility of an electricity which could exist only in animal bodies.

7. Is galvanic fluid the same as the nervous fluid?

For a long time physiologists had been concerned with the role of the nerves in animal life. They believed that the understanding of the nervous act would provide a scientific basis for the treatment of nervous diseases, and perhaps of others as well. Thus, they considered the following questions to be of the utmost importance: (1) what is the physical nature of the 'vital spirit' or 'nervous fluid' which had been held responsible for all motions and sensations? (2) what is the source of the 'nervous fluid'? and (3) are the so called 'involuntary' muscles, such as the heart or intestines, indeed independent of nervous activity?

All efforts to answer these questions were unsuccessful, partly because of the lack of proper experimental technique. The most difficult among these problems certainly

[110] Gren (footnote 54), 408–9.
[111] Valli (footnote 46), 215; Humboldt (footnote 92), 453.

was the first. What was known about the nature of the nervous fluid was that it must be very fast, subtle, fluid, and imperceptible by the senses. Many agents might have fitted so vague a description, and indeed in various theories of muscular contractions the nervous fluid was compared either to a gas, or vibrations in the aether, or electricity. The objections to the electrical hypothesis were no less solid than those to the others.[112]

If the nature of the nervous fluid was very obscure, its chief seat seemed to be established—the brain. However, observations of muscular contractions in decapitated animals prompted a search for additional sources of the nervous fluid. Blood appeared to be one of them, for it was found that cutting the arteries leading to a muscle paralysed it in the same way as cutting nerves. Georg Prohaska suggested that nerves themselves could also be such a source, since an irritation of a nerve in animal parts separated from the body also produced muscular contractions. The dispute about the cause of the motion of the heart was particularly animated. By referring to the well known influence of emotions on the heartbeat, Thomas Willis (1621–1675), Richard Lower (1631–1691), Herman Boerhaave (1668–1738), and Robert Whytt (1714–1766) asserted that the brain rules the heart through the nerves. Their opponents Caldani, Haller, and Fontana responded to this that although the heart reacted to direct irritation of its muscles, it was indifferent to a stimulation of any nerves leading to it, which indicated that the heart was not governed by the will. This result could hardly have been satisfactory, for another question appeared: what are all these 'cardiac' nerves for?

After having learned that in many cases galvanic fluid is a more powerful stimulus than the others, physiologists supposed that galvanic stimulation might have helped them to resolve all their difficulties. They considered two possibilities: (1) galvanism is the same as the nervous fluid; and (2) galvanism is only a stimulus to the nervous fluid. The partisans of animal electricity upheld the former view. The latter opinion had a broader range of supporters. Those who defended the external origin of galvanic fluid naturally considered this agent to be a stimulus, although they differed about its nature: for Volta and Pfaff it was electrical, whereas for Monro, Darwin and Fowler it was non-electrical. Vitalists, such as Monro, also supported galvanism being a stimulus, for they objected to the analogy of the nervous fluid with any physical agent.

Another popular argument against the identity of the galvanic and nervous fluids was the action of a ligature on a nerve (Pfaff, Cortambert). It was discovered that tying a nerve paralysed the muscle but did not prevent it from contracting when stimulated with a bi-metal. This implied that the ligature stopped the 'nervous fluid' but not galvanic fluid; therefore, the two agents were different. Valli showed, however, that this argument was weak, for, when applied near the entrance of a nerve into the corresponding muscle, the ligature stopped galvanic stimulation too.[113]

As to a possible source of the 'nervous fluid', Valli excluded the circulatory system, for he demonstrated that stimulation of the blood vessels does not produce muscular contractions.[114] Fowler denied the validity of Valli's result, for his own experiments showed that an interruption of the circulation affected galvanic stimulation more strongly than cutting its communication with the brain.[115]

[112] Home, footnote 10.
[113] Valli (footnote 83), 72.
[114] Ibid., p. 189.
[115] Fowler (footnote 77), 103–5.

The attempts at galvanic stimulation of the heart gave contradictory results: Volta, Valli, Behrends, Klein, Pfaff, and Xavier Bichat acknowledged their failure, whereas Fontana, Giulio, Fowler, Créve, Humboldt, and Jadelot claimed success.[116] Caldani stated that the negative result refuted Galvani's theory, since, as found by Behrends, the 'cardiac' nerves did not penetrate into the heart's muscle, and thus the electrical chain was broken. Contrary to Caldani, Fontana denied the electrical nature of galvanism after having *succeeded* with the heart stimulation. Unfortunately, he never explained his point of view.[117] Carradori found one more argument against animal electricity: the heart made to move from rest did not stop after the chain was broken, while, according to Galvani, the cessation must be instantaneous.[118]

The confusion in the interpretation of the experiments with the heart had two reasons. First, the successful observations were conducted differently from the unsuccessful: both metals contacted the muscle of the heart, and not the nerve and muscle. Secondly, it was not clear how strong the contractions had to be in order for the effect to be considered positive. Bichat, for instance, believed that the effect of the stimulation of the heart must be as strong as of voluntary muscles; thus, he ignored weak contractions.[119]

While aiming primarily to discover the nature of life, physiologists did not ignore the possibility of solving, with the aid of galvanic stimulation, a variety of minor problems where the nature of galvanism was irrelevant. It had been recognized at a very early stage that the effect of galvanic stimulation depends on the 'degree of vitality' of animals or animal organs. Thus, galvanic stimulation was used as an instrument for a qualitative observation of the extinction of life in intact animal bodies or in separate organs subjected to various external influences. In this way Valli compared the effect of death under different conditions (by poisoning, drowning, suffocating, starving, vacuum, electricity, and by heat and cold). As a side effect, he found a possibility of reviving dead animals; at least he succeeded sometimes in resuscitating chickens after drowning.[120] Créve offered to introduce galvanic stimulation into medical practice to distinguish 'true' death from an 'apparent' one, such as in partial asphyxia.[121]

Fowler applied galvanic stimulation to check Fontana's theory that opium and some other poisons produced no effect whatever when applied immediately to nerves and muscles alone, but destroy life by acting on some agent in the blood. He did not find any evidence that poison affected the nervous fluid.[122] Some of Fowler's findings gave a new momentum to the debate among physiologists on whether muscles can be stimulated directly without the assistance of their corresponding nerves. He argued, for instance, that although anatomists were unable to discover nerves in earthworms and leeches, galvanic stimulation shows their existence.[123]

[116] Sue (footnote 93), I, 145.

[117] F. Caldani, 'Lettera le quale si esaminano alcune riflessioni circa le nuove ricerche sulla elettricità animale', *Giornale fisico medico*, 2 (1795), 3–20 (pp. 13–4); F. Fontana, 'Articolo di lettera all' Ab. Giuseppe Mangili', ibid., 4 (1792), 116–18 (p. 118).

[118] G. Carradori, 'Lettera sopra l'elettricità animale scritta al Sig. Cav. Felice Fontana', *Giornale fisico medico*, 3 (1795), 225–7.

[119] X. Bichat, *Recherches physiologiques sur la vie et la mort* (Paris, 1800), pp. 393–9, 419–23.

[120] Valli (footnote 83), 72–4, 177, 192–5.

[121] C. Créve, *Vom Metallreize, einem neuentdeckten untruglichen Prüfungsmittel des wahren Todes* (Leipzig, 1796).

[122] Fowler (footnote 77), 136–9, 154–6.

[123] Ibid., pp. 64–8.

Humboldt discovered that the connection between the power of muscular contractions and the 'degree of vitality' of animal organs can be useful not only for purely physiological research, but also in solving the puzzle of the nature of galvanism. He stated that one cannot succeed with a given frog in all kinds of experiments, for some phenomena can be observed only with preparations of the highest sensitivity.[124] In this way he explained why experiments with all-animal circuits and with a single metal succeeded so seldom. Reinhold followed him in acknowledging the role of the physiological condition of animal organs.

Thus, contrary to physicists, physiologists were interested not so much in explaining galvanic phenomena as in finding something useful for physiology. For this reason Volta's theory could not fully satisfy them. Indeed, physiologists did not object to contact electricity as the cause of galvanic phenomena, for it provided them with a new powerful stimulant of muscles. Volta's theory, however, did not solve the problem of the existence of animal electricity. Except for the torpedo, he never applied his third theory to live animals, although he asserted that a contact of different animal substances may create an electric current as well as a bi-metal. Probably Volta suspected that such electricity would have been too feeble to be observed. He had indirect evidence for that: only a very few frogs reacted to the current made by the contact of a nerve and a muscle. The torpedo became an exception because its laminated structure suggested the possibility of a multiplying effect similar to that in the electric pile.[125]

This created an impression that Volta denied electric currents circulating in animal bodies as a general property of the animal world, which did not appeal to many physiologists. Galvani's theory, on the contrary, asserted the existence of animal electricity and its identity with the nervous fluid, which was very important in the justification of electrical treatment for various diseases.

Despite all the successes in the study of galvanic phenomena, at the end of the eighteenth century physiologists began questioning whether their involvement in these investigations was productive. J. J. Sue, for instance, stated in 1797 that all the efforts undertaken until then to understand the nature of the nervous fluid were useless, and it was even unknown whether this fluid moves in a circle.[126] Delamétherie shared this mood in 1798:

> Can one conclude from these various experiments that the galvanic fluid, whatever it is, electrical or another, is the principle of the animal movement? Is it the cause of the irritability of the animal fibre? No, these experiments, interesting as they are, cannot yet authorize this consequence.[127]

Was the concept of galvanism itself to blame for these difficulties, or the way it was applied? Some physiologists began to suspect that they had been asking the wrong questions. Jadelot said,

[124] Humboldt (footnote 92), 22–7.

[125] In Alexander Mauro's view, Volta had no right to compare his metallic pile to the torpedo (see his 'The Role of the Voltaic pile in the Galvani–Volta Controversy Concerning Animal *vs* Metallic Electricity', *Journal of the History of Medicine and Allied Sciences*, 24 (1969), 140–50 (p. 149)). In fact, Volta based this analogy on his third theory (not the fourth) which was valid for organic conductors too.

[126] J. Sue, 'Recherches physiologiques et experiences sur la vitalité', *Journal de Physique*, 46 (1798), 226–35 (p. 277).

[127] See footnote 67.

> It would be foolish to want to explain by this means *all* the phenomena of the nervous system; but unquestionably this discovery offers real progress in the study of nerves, whose action remains to this day as unknown as it is interesting. . . . But if it is necessary to renounce forever to learn on what the action of nerves depends immediately, the attention of physiologists and physicians at least must be concentrated on various agents able to exercise an influence on them, and galvanism is evidently one of the most powerful of these agents.[128]

The most radical solution was offered by the reviewer of the *Monthly Review*:

> It may excite surprize that the wonderful discovery of Galvani has added so little to our stock of knowledge. Perhaps, experimenters have given their inquiries an *unprosperous direction.* Would it not be advisable to suspend, for a while, our physiological researches, and to employ the exquisite sensibility of the animal electrometer to correct and extend our ideas in the infant science of electricity?[129]

Thus, physiologists realized that their enthusiasm about the application of Galvani's discovery was premature. Lack of knowledge of the properties of electricity on one side and anatomy and physiology on the other often led them to contradictory results. Gradually they understood that they must stop asking questions such as, 'How do the nerves function?' or, 'Is the nervous fluid electrical?' and concentrate instead on more practical and solvable problems. Some of them abandoned electricity altogether, while others switched to physical electricity, which began to flourish after the new discovery by Volta.

8. The crisis

In 1800 Volta presented to the Royal Society a paper in which he described a new source of electricity that consisted of a number of plates of silver and zinc separated by pasteboard soaked in salt water, and arranged in the sequence: silver, zinc, water, silver, zinc, water, and so on.[130] This 'pile' produced shocks and sparks, decomposed various substances, and affected an electrometer—there was no question about an electric nature of the fluid involved. Some scientists decided that these experiments confirmed the truth of Volta's theory. Although no one described their reasoning, it can be reconstructed without difficulty.

First, since all the effects produced by a pile took place even in a circuit with no animal tissues, scientists assumed that the electricity of the pile had a physical and not an animal origin. Secondly, they supposed that the action of the pile was simply a multiplied action of one of its component bi-metals; that is, it was due to the contact electricity. Thirdly, as the fluid produced by a single bi-metal in a circuit without a pile was galvanic, it appeared natural to suggest that the same fluid acted in a circuit with a pile. And finally, since the latter agent was contact electricity, so was the former.

[128] Jadelot (footnote 94), p. vi, italics added.
[129] 'Transactions of the Royal Society of Edinburgh, vol. III', *Monthly Review*, 19 (1796), 129, italics added.
[130] A. Volta, 'On the electricity excited by the mere contact of conducting substances of different kinds', *Philosophical Transactions of the Royal Society of London*, 1800, 403–31.

More precisely, as in other cases that have been discussed above, what was established was analogy rather than identity. Galvanic fluid became one of several electricities, different from, say, frictional electricity. Only in the late 1830s was it proved that all the 'electricities' have the same properties and differ only quantitatively. However, the actual problem with this reasoning is not that the result is slightly weaker than one might wish it to be, but that its logic is faulty.

Indeed, its third step is completely wrong, for it ignores the presence of animal organs in a circuit. A nerve–muscle preparation can be harmlessly excluded from a circuit with a pile, but not from the circuit without a pile, where it is the only indicator of electricity. But maintaining animal parts in a circuit leaves the possibility of interference by animal electricity. Thus, experiments with a pile cannot at all advance the solution of Galvani's original problem.

Besides, Volta himself emphasized that the electricity of the pile was continuous, distinct from the instantaneous electricity produced in circuits without a pile. This certainly implied a considerable difference between the two fluids. He might have alleviated this objection by supposing that different detectors of electricity, such, for instance, as a nerve and a heated wire, react differently in time to an electric current passing through them. However, he had never done this, leaving the possible objection unanswered.

No one had noticed either discrepancy. Somehow, many of Volta's former adversaries persuaded themselves that the new experiments proved his theory. Did they deceive themselves? If they did, one of the possible causes of such a deception might have been an unfortunate use of the term 'galvanic'. Before 1800 this term was applied to phenomena of muscular motion or sensation produced with some solid and liquid substances involved, none of which was known before as a stimulant.[131] Physiologists believed that galvanic phenomena revealed the 'nervous fluid'. On the contrary, most experiments with the electric pile did not involve any animal organs, and focused on physico-chemical problems. Nonetheless, the old term 'galvanism' was applied to them as if its meaning was the same as before.

There is also a possibility that the term 'galvanic' was preserved deliberately. Those who decided for some reason to adopt Volta's theory after 1800 might have wished to acknowledge that he was right from the very beginning when identifying galvanism with contact electricity. However, in this case their reason had to be entirely independent of the pile, which is unlikely. It is clear from various sources that it was the discovery of the pile that swung prevailing opinion in favour of Volta's theory. For instance, in 1799 Vassalli thought that 'there is still nothing certain about the galvanic fluid'.[132] Delamétherie's annual reviews of works on galvanism show when the change occurred. Between 1798 and 1800 he devoted as much attention to the opponents of the electrical origin of galvanism (Fontana, Fowler, Humboldt, Jadelot, and Fabbroni) as to its supporters (Galvani, Volta, Wells, and Vassalli), although he himself had always preferred the latter view.[133] In 1801, however, he concluded his description of the experiments with the pile with William Nicholson's remark that 'there is no doubt left any longer that galvanism must be put into the

[131] Jadelot (footnote 94), p. v.

[132] A. Vassalli, 'Lettre à J.-C. Delamétherie sur les phénomènes de la torpille', *Journal de Physique*, 46 (1799), 69–72 (p. 69).

[133] J. Delamétherie, 'Du galvanisme', *Journal de Physique*, 46 (1798), 39–44; 48 (1799), 18–20; 50 (1800), 18–20.

number of electrical phenomena'.[134] In 1802 Delamétherie himself asserted that Volta's opinion 'is becoming today almost generally accepted'.[135]

Thus, after 1800 a misunderstanding of the correct relation between the phenomena observed with and without the pile made many scientists to believe that Volta's theory had won. This did not happen overnight, however. In 1801–1803 a number of authors still questioned the nature of galvanic fluid produced by the pile, and it took time to prove beyond doubt that this fluid had the properties of electricity.[136] However, while accepting the electrical nature of galvanism, which was common to both Galvani's and Volta's theory, some scientists disagreed that Volta's theory refuted animal electricity.

9. Animal electricity between 1800 and 1810

When comparing the publications on animal electricity during the decade following Volta's discovery with the preceding one, a significant increase can be seen both in absolute numbers and relative to other topics on electricity. As in the previous decade, the research in the field was concentrated in two major directions. Some works focused on theoretical questions aiming to discredit Volta's theory and support animal electricity. Others dealt with the application of galvanism as a stimulus and they offered to increase the effect of galvanic stimulation by substituting the pile for the bi-metal.

In the first group, Aldini's work was particularly important. He performed numerous experiments with all-animal circuits that contained parts of different animals.[137] Lehot showed that muscular contractions take place not only at the make but also at the break of a galvanic circuit. None of the theories was able to explain this phenomenon, and Lehot suggested that it was due to accumulation of galvanic fluid in animal organs.[138] Thomas Buntzen, a physician from Copenhagen, successfully repeated Humboldt's experiment with an open circuit, in which a pile replaced a bi-metal.[139] Probably by analogy with Volta's pile, he built a battery using the frogs' nerves and muscles (nerve, muscle, sponge, nerve, muscle . . .) and observed simultaneous contractions in all of them when the circuit was closed.[140] He considered this result to be in favour of animal electricity. From their experiments with the torpedo Humboldt and Gay-Lussac concluded that nerves play a very important role in producing electricity, which refuted Volta's idea that electricity of the torpedo was due to the contact of a number of aponeurotic plates and of albumen-gelatine pulp.[141]

Thomas Young favoured the electrical nature of the nervous fluid.[142] William

[134] J. Delamétherie, 'Du galvanisme', *Journal de Physique*, 52 (1801), 9–11 (p. 11).

[135] J. Delamétherie, 'Du galvanisme', *Journal de Physique*, 54 (1802), 15–9 (p. 16).

[136] F. Cuvier, 'Rapport', p. 321; E. G. Robertson, 'Sur le fluide galvanique', *Annales de Chimie* 37 (1801), 132–50 (p. 145); N. Gautherot, 'Mémoire sur le galvanisme', *Annales de Chimie*, 39 (1801), 203–10 (p. 204); and A. Vassalli, 'Extrait d'un ouvrage: Expériences et observations sur le fluide de l'éctromoteur de Volta', in Cassius, Larcher Daubancourt, and De Saintot, *Précis succinct des principaux phénomènes du galvanisme* (Paris, 1803), p. 5.

[137] G. Aldini, *An Account of the Late Improvements in Galvanism* (London, 1803), pp. 3–18.

[138] C. Lehot, 'Extrait d'un mémoire du citoyen Lehot, sur le galvanisme', *Annales de Chimie*, 38 (1801), 42–65.

[139] T. Buntzen, 'Verschiedene Galvanische Versuche mit ausserordentlich machtigen Säulen', *Annales der Physik*, 15 (1803), 340–9.

[140] T. Buntzen, 'Eine galvaniche Batterie aus Froschpraparaten', *Annales der Physik*, 25 (1807), 155–7.

[141] A. Humboldt et J. Gay-Lussac, 'Expériences sur la Torpille', *Annales de Chimie*, 56 (1805), 15–23 (p. 23).

[142] T. Young, Lecture notebooks, University College, London, MS Add 13, 20/21r.

Wollaston and Everard Home also shared this view. In 1809, having been inspired by Humphry Davy's experiments, all three suggested independently that electrical decomposition of animal substances may be responsible for secretions. Wollaston demonstrated that even one silver–zinc pair was sufficient to separate alkali from a salt solution and make it pass through a bladder.[143] Home showed that a low-voltage pile separated alkaline and acid components from blood and supposed that the electricity necessary for electrolysis is supplied by the nerves.[144]

Among the experiments of the second group were Ritter's observations of the influence of the pile on human senses.[145] General attention had been attracted to galvanic stimulation with a pile of freshly killed animals. When Aldini stimulated the head of an ox or a dog, separated from the body, all muscles moved so that the head appeared to come back to life.[146] Vassalli, Giulio, and Rossi observed a similar effect with the heads and trunks of decapitated criminals.[147] J. A. Heidmann, a physician from Vienna, offered to apply a pile to determine a 'true' death from an 'apparent one'.[148] J. Tourdes, Professor of Medicine in Strasburg, observed that galvanic fluid produced contractions in the fibrin of blood similar to muscular ones. In the opinion of Ludwig Wilhelm Gilbert, Professor of Physics in Halle and the editor of the *Annalen der Physik*, that was a proof that muscles, which have fibrin as a component, can be stimulated directly, without the mediation of nerves.[149] Heidmann found that this movement was due to a chemical action and could have been obtained without galvanic stimulation.[150] In his view, galvanic phenomena were caused by chemical processes and not by a contact of metals.[151]

Thus, some physiologists maintained their allegiance to Galvani's theory even after the discovery of the pile. But if only a few scientists supported animal electricity after 1800, had this concept gained anything compared to the pre-Galvanian era? The answer to this question is connected with the evaluation of Galvani's role in physiology.

10. Assessing Galvani's discovery

We are now in a better position to judge the overall significance of Galvani's contribution to physiology. Usually, historians have done this from the point of view of modern science. My account is based on contemporary evaluations. However different, all the comments on Galvani emphasized the novelty and importance of his

[143] W. Wollaston, 'On the Agency of Electricity on Animal Secretions', *Philosophical Magazine*, 33 (1809), 488–90.

[144] E. Home, 'Hints on the Subject of animal Secretions', *Philosophical Transactions of the Royal Society of London*, 1809, 385–91.

[145] J. Ritter, 'Versuche und Bemerkungen über den Galvanismus der Voltaischen Batterie', *Annales der Physik*, 7 (1801), 431–46, 447–84.

[146] See footnote 137, pp. 54–87.

[147] Vassalli, Giulio, et Rossi, 'Rapport presenté à la Classe des Sciences exactes de l'Academie de Turin le 27 Thermidor 'Sur les expériences galvaniques faites les 22 et 26 du même mois, sur la tête et le tronc de trois hommes, peu de temps après leur decapitation'. *Journal de Physique*, 55 (1802), 286–96.

[148] J. Heidmann, 'Resultate aus meinen Versuchen mit der zusammengesetzen ungleichartigen Metallverbindung, oder mit der Voltaischen Säule', *Annalen der Physik*, 10 (1802), 50–6 (p. 56).

[149] J. Tourdes, 'Aus einem Briefe an Volta', *Annalen der Physik*, 10 (1802), 499. Gilbert, footnote, ibid., 499–500.

[150] J. Heidmann, 'Nichtigkeit der Versuche von Tourdes und Circaud, über die Reizbarkeit des Faserstoffs durch galvanische Electricität', *Annalen der Physik*, 17 (1804), 1–14 (pp. 13–4).

[151] J. Heidmann, 'Eintheilung der festen und flüssigen Leiter einer galvanischen Kette, nach dem Grade ihrer galvanischen Action und ihres chemischen Wirkungsvermögens', *Annalen der Physik*, 21 (1805), 85–107 (p. 107).

results. Valli, for instance, said: 'The discovery of M. Galvani so *surprized* me and appeared to me of such a *great importance* that I immediately decided to repeat his experiments'.[152] Brugnatelli said that Galvani's experiments 'marvelously proved the influence of electricity on muscular motion'.[153] Volta went even further by saying in May 1792 that it was

> one of those great and brilliant discoveries that deserve to make an epoch in the annals of physics and medicine . . . The existence of the true and proper animal electricity . . . is what has just been *proved with certainty* in the third part of this treatise by means of many well arranged and accurately described experiments.[154]

This agrees with Galvani's self-evaluation:

> although many distinguished scholars published the same theory long ago, nonetheless we were amazed at our good fortune in being the first to hold in our hands, as it were, this electricity which is concealed in the nerves, and to draw if forth from the nerves and to set it practically before our eyes.[155]

In other words, Galvani claims here that he transformed animal electricity from a speculation to a fact. Some of his contemporaries also contrasted the 'conjectures' of his predecessors with Galvani's reasoning that was based on exact experiments.[156]

As shown above, before Galvani, scientists were not concerned with critical examination of evidence in favour of animal electricity. It seems that this hypothesis appeared ridiculous to some and plausible to others, but a hypothesis all the same. This status of a conjecture could probably explain the lack of debate on the subject. A conjecture is not binding: one can either accept it or argue against it, or simply ignore it. But when Galvani announced that animal electricity is no longer a speculation but the *truth*, no one could remain indifferent.

Another point in Galvani's treatise that attracted great interest was the promise of a breakthrough in the experimental study of the nervous fluid. Among the opponents of animal electricity there were capable experimentalists who tried to avoid speculation and deal only with empirical evidence. For them, the whole concept of the animal electrical fluid was not worthy of attention for, locked inside the animal body, it seemed inaccessible to their instruments. Galvani opened a window into this world by providing a new, convenient technique for its examination by physical means. Physiologists adopted this technique immediately, even before they had made up their minds about Galvani's explanation of the new phenomenon. Not only did the intensity of the debate on animal electricity suddenly greatly increase but its whole character changed. In contrast to the speculative argument then so typical of discussions of theoretical problems, every response to Galvani was based on experiment.

As mentioned above, some historians have implied that Galvani's discovery consisted solely of a new phenomenon of muscular stimulation. If this is true, let us imagine that Galvani published a description of this phenomenon in a short note without any theory and the additional experiments following from it. What would have been its chance of receiving the same attention? In my view, insignificant. First,

[152] Valli (footnote 83), 66, italics added.
[153] See footnote 41, p. 280.
[154] Volta (footnote 12), 15, italics added.
[155] Galvani, *Commentary*, p. 79.
[156] 'Ragguaglio delle sperienze del Sig. Luigi Galvani . . . estratto da una lettera diretta al Sig. Conte Prospero Balbo', *Giornale fisico medico*, 2 (1792), 94–109 (p. 95).

this suggestion is corroborated by historical precedents, two of which are close to our topic. Sulzer correctly stated that the contact of different metals produces an acid taste, and he conjectured that the effect was produced by mechanical vibrations. This was in a footnote to a paper by him on another subject. Although the effect was interesting and easy to reproduce, no response followed Sulzer's note. In another case, Jan Swammerdam (1637–1680) described contractions of a frog's leg which might have been attributed either to mechanical stimulation of the nerve or to an action of the silver and brass which contacted a nerve and a muscle of the frog.[157] It was natural for Swammerdam to opt for the former solution, since the physiological action of electricity was unknown at the time. However, the English translation of his book was published in 1758 when this action was well known, yet no one offered such an explanation. Although Sulzer's and Swammerdam's experiments did not provoke their due response, they were noticed. Soon after Galvani and Volta attributed these phenomena to electricity, the names of Sulzer and Swammerdam entered the history of animal electricity.[158]

Secondly, there is little doubt that the readers would have ascribed Galvani's phenomenon to mechanical stimulation. Galvani himself suspected this and showed in his treatise how to obviate such a possibility. Despite that, two years later Volta stated that in the case of the all-animal circuit the stimulation was mechanical and not electrical as Galvani suggested.

Thus, the eighteenth-century scientists would hardly have accepted a brief description of one more phenomenon of muscular contractions as a discovery. To claim a discovery, Galvani had to show that (1) the effect was permanent, that is, that it could be observed as many times as necessary in a variety of conditions; (2) the phenomenon was new and could not be produced by mechanical or chemical action, or by frictional electricity; (3) the effect could be explained by a theory that was applicable to other phenomena as well; and (4) it had an important practical application. That was the strategy that Galvani followed. After working on his treatise for five years he still was not fully satisfied with it. However, his presentation was skillful enough to convince scientists in a very short time of the importance of his discovery and plausibility of his theory. The sudden change of attitude towards the experiments of Vassalli and Cottunio illustrates this. Little known before the publication of his treatise, they were immediately acknowledged (without any verification) afterwards as the first observations of the true animal electricity. When in 1792 Valli decided to repeat Cottunio's experiment, he *expected* to get a violent shock and was surprised by the negative result.[159]

As galvanic investigations proceeded, many scientists found Galvani's arguments imperfect and adopted other explanations of galvanic phenomena. Nonetheless, even those who disapproved animal electricity continued to view Galvani as a great discoverer. More than once he was compared to Harvey. As Humboldt said:

> the name of Galvani will never perish; the future centuries will profit from his discovery, and, as Brandes says, 'they will recognize that physiology owes Galvani and Harvey its two principal grounds'.[160]

[157] J. Swammerdam, *The Book of Nature or, the History of Insects*, translated from Dutch and Latin by Thomas Floyd (London, 1758), p. 127.

[158] *Galvani on Electricity*, p. 14; footnote 107, p. 76.

[159] See footnote 83, p. 95.

[160] Humboldt, (footnote 92), p. 361. See also Charles Caldwell, 'Appendix', in J. F. Blumenbach, *Elements of Physiology*, translated from Latin by C. Caldwell (Philadelphia, 1795), p. 217.

The 'two principal grounds' are presumably blood circulation and nervous action.

Thus, it seems that in the eyes of his contemporaries, Galvani's principal achievement was to provide a physical foundation for neurology. His experiment was perceived as the proof of the existence of a particular fluid (galvanic fluid) which originates in animal bodies, is unequally distributed in different organs, and irritates nerves when flowing through them. In other words, Galvani proved what people had for long conjectured: the existence of the 'nervous fluid'.

In such a form, Galvani's result was acceptable to many. His other major result—the demonstration of the electrical nature of the 'nervous fluid'—drew sufficient support only in 1792. Although limited, this favourable reaction is important to us. Indeed, scientists who only a few months earlier looked down upon animal electricity as a useless conjecture began to view it as a well established scientific theory. And this reappraisal was irrevocable. Scientists might abandon this theory for a while, having been attracted to other subjects or simply being unprepared to pursue it as yet, but this does not change the new view on animal electricity.

May we treat this change of status of animal electricity as equivalent to its demonstration? There are reasons to answer this question positively. Usually, historians have evaluated Galvani's experiments by modern standards and found them insufficient to prove his theory. The credit for the proof is given to Matteucci and Du Bois-Reymond. This approach, however, seems neither fair nor fruitful. It does not, for instance, compare the intellectual capacities of Galvani and, say, Du Bois-Reymond. The experiments of the latter were more definitive because they were based on a knowledge of electricity that did not exist in Galvani's time. Nor does it explain why Galvani's theory was not abandoned altogether after 1800. To understand the fate of animal electricity between Galvani and Du Bois-Reymond, I prefer to speak of its 'demonstration' or 'establishing' in the eyes of scientists of that time. It turns out that none of the supporters of animal electricity intended to prove its existence.[161] What they tried to do was to invent new methods of detecting animal electricity, to discover different kinds of animal currents, and to study their properties. Since no one at the time mentioned any 'crucial' experiments to 'prove' animal electricity, we have to admit that either animal electricity was established before that period or after it. The latter suggestion leaves unanswered two questions: (1) what did Matteucci and Du Bois-Reymond accomplish to change the perception of animal electricity so suddenly? and (2) what was the motivation for their predecessors to work in this field? The former assumption appears more plausible; thus it was Galvani who motivated a group of scientists to work on animal electricity. When, then, was this theory established? The answer depends on how to define the group whose opinion matters.

If we assume it to be an overwhelming majority of scientists or even a big group of them, then animal electricity was not established even in 1850. Such a conclusion gives us no help in understanding the development of electrophysiology between Galvani and Du Bois-Reymond. In fact, what eventually led to a breakthrough in this field in the 1840s was the result of the persistent efforts of a small group of investigators over several decades. The existence of such a small group of scientists who used Galvani's theory in their investigations and whose work was well known to the scientific community seemed to be sufficient to establish this theory as a respectable scientific subject. Thus by this criterion, it was Galvani who established animal electricity and this took place in the 1790s.

[161] N. Kipnis, 'Animal Electricity in the First Half of the Nineteenth Century', unpublished; part of this paper was presented at the Midwest Junto Meeting, Minneapolis, April 1985.

11. Conclusions

Galvani had chosen the hypothesis of animal electricity not because of its popularity or plausibility; it was he who brought it these attributes. The main point of attraction for physiologists in Galvani's treatise was the new opportunity to study the nervous act and perhaps to solve the mystery of life. Initially, they adopted Galvani's theory of galvanic phenomena. Later, however, several factors made most scientists abandon animal electricity. First, physiologists realized even before the discovery of Volta's pile that animal electricity was too complicated a subject for study at the time. Secondly, some scientists misunderstood Volta's experiments with the pile as refuting Galvani' theory. Thirdly, a new field opened: the application of the pile to physics, chemistry, and perhaps medicine appeared to promise quick success—therefore everyone rushed there.

The decline of animal electricity after 1800 does not depreciate the importance of Galvani's discovery. What he achieved was to raise animal electricity from a speculation to a scientific theory, and without that any further development in electrophysiology would have been impossible.

To physiologists, neither Galvani's nor Volta's theory was sufficiently sophisticated to explain galvanic phenomena. Their hopes of solving the fundamental problems of physiology with galvanic stimulation were not fulfilled, for the problems exceeded the resources of contemporary science. It became clear by 1800 that an anticipated revolution in neurology had aborted. Volta's discovery of the pile added nothing to this result, it simply offered another field for disappointed scientists to enter.

The investigations of galvanic phenomena before 1800 were important to the further development of the theory of nervous and muscular action. Galvani brought forth considerable evidence for the existence of animal electricity. Volta proved the participation of a new kind of artificial electricity. Humboldt showed that a continuous flow of electricity in a closed circuit does not properly represent the propagation of the 'nervous fluid'. He also suggested that both electricity and chemical processes are involved in the nervous activity. Thus, all the competing groups made important contributions to the development of physiology. However, it took several decades to realize all that was accomplished during the 1790s. The major reason for the slowdown of electrophysiological research was not the opposition to animal electricity but the lack of physical foundation for it. In the 1840s physics and chemistry reached the level necessary to proceed with these investigations, and Matteucci and Du Bois-Reymond laid the foundation of modern electrophysiology.

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