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The Taming of the Ray

Electric Fish Research in the Enlightenment, from Walsh to Volta

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To Giulia

Foreword

Two scientists lie behind the birth of this book. One is Giuseppe Moruzzi (1910-1986), the Italian neuro-physiologist who made fundamental contributions to our knowledge of the nervous mechanisms underlying the nervous control of the sleep-waking cycle in vertebrates. From Moruzzi I derived an interest in the study of science history. My work on the history of electrophysiology, on which this book is based, is a follow-up to an article on the electro-physiological work of Carlo Matteucci (1811-1868) that Moruzzi wrote in Italian in 1964. In 1996, with the help of Adriana Fiorentini and Paul Witkovsky, I translated this article into English. It was the start of a lasting interest in the field of science development that, over the past two centuries, has led to our present-day understanding of the involvement of electricity in nerve and muscle function.

The other scientist connected in spirit to this book is Alan Hodgkin (1914-1998). With his studies on the mechanism of nerve conduction, and in particular with the fundamental papers on the giant axon published in 1952 (in collaboration with Andrew Huxley), Hodgkin in a way concluded the research program begun around 1780 by Luigi Galvani (1737-1798) with his experiments on the electrical stimulation of frog nerves and muscles.

I visited Hodgkin at his home in Cambridge in 1997, about a paper celebrating the bicentennial of the death of Galvani. Hodgkin was at that time very ill, but he nevertheless proposed that it would be fitting for Galvani to be commemorated by some important English institution, and suggested that this could be associated with the invention of the Voltaic battery (1799 - 1800). However, he was unable to carry out this plan himself because in December 1998 he died, almost exactly two centuries after Galvani. Responsibility for the project passed to Richard Keynes, who succeeded in attracting the interest of the Royal Institution and of the Welcome Trust, which led to a Symposium in March 2001, held in London, entitled: ‘Electricity and Life: Galvani to Hodgkin’.

When I attended this Symposium, I was strongly interested to the scientific endeavour of John Walsh, the English scientist who in the second half of the 18th century had first provided firm experimental evidence of the electric nature of the shock of electric fish. As this book attempts to show, Walsh’s researches were fundamental to the promotion of interest in the possibility that electricity might play a role in nerve and muscle physiology (mainly through the work of Galvani), and in addition, in fostering important developments in physical conceptions of electricity (through the work of Henry Cavendish and Alessandro Volta).

In spite of the importance of his studies, Walsh is not well known today. I went to London with the secret dream of finding material that could help me draw him out of oblivion. The discovery in the Library of the Royal Society of Walsh’s manuscript ‘Experiments on the Torpedo or Electric Ray at La Rochelle and l’Isle de Ré – in June - July 1772’, made just the day after the Symposium, fulfilled my dreams beyond all hope. Until then my search for unpublished documents on Walsh had been largely disappointed, but now I could read the day-by-day development of the intensive experimental work that on 9th July 1772 led him to write in his journal: «Announced the Effect of the

Torpedo to be Electrical», and to express his pride in discovery with a marginal notation «*Je l'ai donté*», I have tamed it, I have tamed the «indomitable art of the wonderful torpedo».

My book is almost an unavoidable consequence of the discovery of Walsh's manuscript. The journal of the experiments has come down to us thanks to the care of Lord John Arthur Walsh, last Baron Ormathwaite, an indirect descendent of John Walsh, who donated the manuscript to the Royal Society in 1965. This book is also an acknowledgement to him and the other people that preserved the manuscript from the ravages of time and eventually made it accessible to the historian.

On the other hand, I am not inclined to consider the way I came across Walsh's manuscript entirely accidental. I like to think of it as a kind of posthumous gift of Alan Hodgkin, the scientist of the 20th century who, together with Andrew Huxley, in some ways completely 'tamed' the electric force responsible for nervous conduction.

Marco Piccolino, March 2003

Acknowledgements:

This book is part of my researches on the history of electrophysiology that I have been pursuing in recent years in collaboration with Marco Bresadola of the Department of Human Sciences of the University of Ferrara. I wish to express my thanks to Marco Bresadola for helpful discussions during the preparation of the manuscript of this book.

Thanks are also extended to the many persons that have helped me in these years and particularly to the librarians or colleagues that have made my search for old documents and books less arduous. It is difficult to list them all but we wish at least to recall some of those who have contributed to our work. First, Elisabeth Ihrig of the Bakken Institute of Minneapolis, Gianluigi Goggi of the University of Pisa and Rupert Baker of the Library of the Royal Society of London; and then Jean Flouret of the *Académie des Sciences et Belles Lettres* of La Rochelle, Livia Iannucci of the Library of the Institute of Physiology 'G. Moruzzi' of the University of Pisa, Valérie Denier of the *Mediathèque Michel Crepeau* of La Rochelle, Adele Bianchi Robbiati of the *Istituto Lombardo Accademia di Scienze e Lettere* of Milan, Stefano Casati of the *Istituto Museo di Storia della Scienza* of Florence, Danielle Etherington and Anne Young of the John Rylands Library di Manchester, Sarah Bakewell of the Wellcome Institute for the History of Medicine of London, John Simmonds of the Crawford Hansford & Kimber, the librarians of the *Archiginnasio* of Bologna, of the Nebraska University Medical Center and of the Creighton University of Omaha, Nebraska and of many other libraries throughout the world.

I am grateful to Enrico and Gabriele De Angelis, to Katrin Bick and to Marco Grondona for their philological help and to Charles Hindley and Lionel Lovitch for their help in revising the English text of the book. Thanks also to Angela Pignatelli and to Lorenzo Sbrenna for their assistance and friendship.

A note on the quotations, footnotes and bibliography:
In the footnotes we have chosen to insert the originals of most of the quotations from non English sources, a choice which broadens the linguistic characteristic of the text and somewhat reflects the multinational and multilingual character of science and culture in the 'Republic of Letters' of the age of Enlightenment as well as in our time. This book is mostly concerned with English and Italian scientists who, directly or indirectly, published the results of their studies in a variety of languages or made references to works written in different languages. It is written in English, the present-day language of scientific communication, but the author is Italian and it is published in Florence by a publisher whose name is far from being typically Italian.

The bibliography is listed at the end of the book. In general, the references are indicated briefly in the footnotes only when necessary for identifying the pages of quotations or for some other special purpose. However, the page number is not usually given for the quotations derived from articles or other short texts.

For the quotations of passages from the works of John Walsh or Alessandro Volta the abbreviations (as listed below) are followed by the indication of the page number. In the case of Volta's works, the volume number (in Roman ordinals) precedes the page number.

WE: J. Walsh '*Experiments made in La Rochelle and Isle de Ré, June and July 1772*', Library of the Royal Society of London, Ms. 609;

WJ: J. Walsh '*Journey from London to Paris, begun 8th June 1772*', John Rylands Library Manchester, Rylands English Ms. 724.

VEN: A. Volta, *Le opere di Alessandro Volta. Edizione nazionale sotto gli auspici della Reale Accademia dei Lincei e del Reale Istituto Lombardo di Scienze e Lettere*, Milano, Hoepli, 1918-1929, 7 voll.

VEP: A. Volta, *Epistolario di Alessandro Volta. Edizione Nazionale*, Bologna, Zanichelli, 1949-1955, 5 voll.

For the quotations from Walsh's manuscripts, the abbreviation is followed by the page number with L.F. or R.H., to indicate left or right hand pages, respectively. For the passages from the works of Alessandro Volta the abbreviation is followed by the indication of the volume number (in Roman ordinals) and by the page number.

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I PART

THE JOURNEY

A MIDSUMMER JOURNEY TO FRANCE

1772, June 8th. Whitsun Monday. Left London in the afternoon; Crowds of Men and Women returning from Greenwich Park and Maying. The hedges in very full Blow of May. In company with Mr Davies, and my Nephew Arthur in Post Coach. Slept at Dartford 16 Miles.[1]

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[1] See WJ, 1 L. H. The manuscript of John Walsh entitled 'Journey from London to Paris, begun 8th June 1772' conserved at the John Rylands Library Manchester is in the form of a bound notebook (Rylands English Ms. 724). Besides several blank leaves at the end, the manuscript is made up of 108 numbered and handwritten left-hand pages, with the corresponding unnumbered right-hand pages left blank, used only for sporadic annotations. Attached to the front loose endpaper is a large folded sheet, headed «List of visitors a Paris 1772», containing the names, and the addresses, of a total of 33 persons that Walsh intended to visit during his sojourn in the French capital. Many of them were scientists and / or academicians as for instance «*De La Lande*» (i.e. De Lalande), Le Roy «*Darcet*» (i.e. D'Arcet), Du Hamel, Brisson, Bertier, D'Arcy, Daubenton, De Lor, Dalibard, Rouelle, «*le duc de Chaulne*» (i.e de Chaulnes), Trudaine. There was also a number of ladies of high social and intellectual rank (such as «*Mad.me de Corberon,* […] *Lady Lambert,* […] *Mad.me Helvetius,* […] *Mad.me de La Rive,* […] *Mad.me Geoffrin,* […] *Mad.me Greuze*». Very likely Lady Lambert was the wife of Lord John Lambert (1728-1799), a banker, whose name recurs in Walsh's manuscript (also because Walsh uses his address, at the *Quai de Conti,* as a temporary post address in Paris). There is also one, or possibly two, physicians («*M.r Du Bourg, Medecin*» and «*Le Docteur Jem*»), and likely there was a glorious sea captain of breton origin («*M.r Kerguelin*», i.e. Joseph de Kerguelen-Tremarec, 1745-1797), very popular in England (see for instance 'Delandine' Dictionnaire Historique" T. 15, Paris, 1822), and, moreover, one «*M.r Foubert*» (probably Bruno-Nicolas Foubert de Bizy, a military officer who had fought mainly against Austria). One of the most eminent personages in Walsh's list was undoubtedly «*Monsr de Paulmy, / a l'Arsenal*», i.e. Marc-Antoine Le Voyer d'Argenson, *Marquis de Paulmy* (1722-1787) a French State Minister, member of the *Académie Française*, famous for his erudition and for his immense family library which in 1756 was installed in the *Arsenal*. In 1796 this library became a National Library, and with the name of '*Bibliothèque de l'Arsenal*' it is still an active and important library today. As for 'Trudaine', at the epoch of Walsh's visit, lived in Paris both Daniel-Charles (1703-1777), *Conseiller d'Etat, Intendant général des Finances*, member of the *Académie des Sciences,* and his son, Jean-Charles Philibert (1733-1793) who succeeded to some of his father's functions and was also member of the *Académie*. Walsh met both of them. Among the personages that I could not identify with assurance in Walsh's list of visitors are a «*M.r de la Borde*» (it is possible that he is either Jean-Joseph de La Borde, 1724-1794, or his son**,** François Jean- Joseph de La Borde, died in London in 1801, both important Parisian bankers). Of the personages indicated by Walsh as «*Mad.me de Bouquancourt*» and as «*Marquis de Royarine*», I have been not able to find any information in the biographic repertoires consulted. This may be attributed to my difficulty in deciphering in some cases Walsh's writing and / or to possible writing errors of Walsh, who did not master perfectly the French idiom (see later). As to possible writing errors made by Walsh in his list, (probably explained by the sound of French pronunciation to an English ear) it might be that the personage indicated as «*Le Marquis de Sairan*» corresponds to François Jacques Walsh, *comte de Serrant* (ca. 1704-1782), or to the husband of his daughter, Françoise-Elisabeth-Charlotte Walsh de Serrant (died 1793). This was the *Marquis de Choiseul* to whom she had been married in 1775 (which might explain the title of *Marquis* attributed by Walsh to *de Sairan*). These personages are mentioned in various documents of the epoch (as for instance in Horace Walpole's correspondence – see WALPOLE 1937-1983 and in the '*journal*' of the French diplomate Marie Daniel Bourrée de Corberon - manuscript at the *Mediathèque* 'Ceccano' of Avignon. It is possible that the Walsh de Serrants had some remote family connection with John Walsh's family (indeed the Walsh de Serrant originated from an old Irish family of Welsh origin - the 'Walsh') which had moved to France as a consequence of the events connected to the kingdom of James II, see COURCELLES 1820-1822, T. IV, p.). The castle of Serrant (one of the most beautiful *châteaux* of the region of Angers, now a property of the Duc of La Trémoille) belonged originally to an ancient family of the French *Noblesse*. It had been purchased by the exiled Irish gentleman Antoine Walsh, who owned a merchant fleet at Nantes (see LA TRÉMOILLE 1904 and see also WALSH, LE CHEVALIER 1901). One of the most eminent members of the old family of the *Seigneurs* of Serrants was *Guillaume* '*Bautru*' *de Serrant (*or *Séran*, 1588-1665), one of the first members of the *Académie Francaise*. John Walsh actually visited the castle of Serrant (that he spells «*Serrans*») on 4th August 1772, during his journey back after the investigations on torpedo performed at La Rochelle (WJ, 68-69 L.H.). Besides possible writing errors of John Walsh, the problem of the identification of the personages in his visitors list is complicated by the uncertainness of the spelling that characterised the names of the aristocratic families of the epoch. '*Seran*', '*Séran*', *Sérent* might be different spellings of the same family name (see for instance '*L' intermédiaire des chercheurs et curieux*', 1900, vol. 41, p. 64). Bonne Marie Félicité *Marquise de Séran* was loved by Louis XVI and is mentioned for a piquant episode at his court occurred in 1765 (see DUFORT DE CHEVERNY 1970, T. I, p. 260). At the end of Walsh's list, and somewhat separated from the other 'visitors', there are a «*Monsieur Breant - Graveur*», a «*Mad.lle Biheron*» and finally a «*M.r Fossier - Dessinateur*». In BENEZIT's

This is the beginning of a manuscript of about 100 written pages, now in the John Rylands Library in Manchester, describing a memorable 'Journey from London to Paris, begun 8th June 1772' embarked upon by John Walsh, an affluent English gentleman, member of Parliament and fellow of the Royal Society.[2]

Notwithstanding Walsh's public position as a 'Commoner', the journey had no political, diplomatic or related purposes, even though Walsh will eventually incur the charge of espionage and be ordered to leave France immediately (by the way, it was not unusual for scientists to act as spies during

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'*Dictionnaire critique et documentaire des peintres …*' Breant is mentioned just as a «*graveur au burin, né a Rouen, travaillant à Paris vers 1780*» («burin engraver, born in Rouen, working in Paris ca. 1780»). In the same repertoire very scanty information is also available for Fossier, indicated as a «*dessinateur, ornemaniste, vers 1775*» («ornamentalist draughtsman, ca. 1775), whose known drawings served generally for engravings. It might be surmised that Walsh felt the need to contact in advance illustrators and artists for the publication of the results of experiments on the torpedo, particularly because he might have supposed that anatomical material derived from his experiments might not arrive in London in good conditions. However, neither Breant nor Fossier contributed to the beautiful plates that actually illustrated Walsh's (and Hunter's) 1773 article in the 'Philosophical Transactions' (see later). As for «*Mad.^{lle} Biheron*», she was Marie Marguerite de Biheron (1719.1795), famed as an anatomist and creator of wax anatomical models that were used for teaching (particularly obstetrics), and were displayed in a museum at «*Place de l'Estrapade*» (the address actually indicated by Walsh). A recent study has corrected an error concerning Mad.^{lle} de Biheron's name which had been traditionally indicated as '*Marie Catherine*' (see BOULINIER 2001). A visit to Mad.^{lle} de Biheron's Museum was almost obligatory for any Parisian visitor interested to science and culture, and many high ranking personages visited this Museum or were offered exhibitions of Mad.^{lle} de Biheron's anatomical models in special seances at the *Académie des Sciences*. In 1771, the year before Walsh's visit, this had been the case for King Gustav of Sweden and his brother Frederick Adolph, and in 1777 a similar event would occur for the visit of the Austrian Emperor Joseph II (see later). At the end of 1771 Mad.^{lle} de Biheron visited England bearing a letter of recommendation addressed John Wilkes by Denis Diderot who was her admirer and who had placed her in charge of the anatomical education of his daughter (from Diderot's correspondence it seems evident that the anatomical lessons of Mad.^{lle} de Biheron were considered a convenient method of sexual education for the girls of the epoch) (see in DIDEROT 1969-1973, and particularly in vol. VII p. 809, vol. IX pp. 1000, 1004, 1051 and 1125, vol. X pp. 613-615). We might tentatively suggest that Walsh included Mad.^{lle} de Biheron with the '*Graveur*' and with the '*Dessinateur*' because he wished to exploit her talent as an anatomist-artist for the purpose of illustration of his publications, or perhaps he wished to ask her for a wax model of the anatomical preparation that he was planning to bring back from La Rochelle. But of this we have no positive evidence in Walsh's manuscript.
Walsh actually met many of the persons contained in his visitors list, some of them in the occasion of a visit at a seance at the *Académie Royale des Sciences* on 17 June 1772, others in a more or less private occasions (see later).

[2] John Walsh was born in Fort St. George on 1 July 1726 (the date is confirmed by the journal of the experiments where, on 1 July 1772, Walsh annotates: «my birthday, 46 years old»). He died 9 March 1795. He was the cousin of Neville Maskelyne, the Royal Astronomer, and also of Lord Clive who was married to his mother's niece, Margaret Maskelyne. Walsh served in the East India Company since 1742 and, in 1757, he was appointed private secretary to Clive. In 1759 Clive commissioned him to return to London in order to support his plans for reorganizing the administration of Bengal before the English Government. Walsh's richness (estimated to about £140,000) was mostly derived from the considerable fortune amassed as a share of war conquests in India. He was member of the Parliament from 1761 to 1780. He was elected Fellow of the Royal Society on 8 November 1770. In his certificate of election, Walsh is indicated as «a Gentleman well acquainted with philosophical, & polite literature, & particularly versed in the natural history and antiquities of India». (Source of information: NAMIER and BROOKE 1985, the Archives of the Royal Society, and, moreover, the electronic document, now apparently disappeared, compiled in 1990 by Glyn Parry, which was at the web site of the National Library of Wales). In a recent biography of John Wilkes, Walsh is mentioned as a friend and supporter of Wilkes, particularly in the affair of the libel *Number Forty-Five* of the *North Britton* which led to the arrestation of Wilkes (see THOMAS 1996, pp. 26, 29 and 30) . In the 'Dictionary of English Biographies' (BLAKE and NICHOLSB 1990) there are some pieces of incorrect information concerning Walsh, as for instance Walsh's birthday, which is tentatively indicated as 1725 (an error that is endorsed also in the 7th volume of the recent - 1998 - 'British Biographical Index', München, K. G. Saur). Another information present in the 'Dictionary of English Biographies' which could not be confirmed is that, besides in 1774, Walsh had been awarded the Copley medal of the Royal Society on a second occasion, in 1783. A short but accurate biographic sketch of John Walsh is also contained in 'The papers of Benjamin', vol. 19, pp. 160-162 (see FRANKLIN 1960-).

the 18th century)[3]. The journey was not, on the other hand, simply a *grand tour* of one of the most important lands of continental Europe, made by a member of the English upper classes for his self-education, and to make the acquaintance of the customs and fashions of a highly civilised and attractive country, as the France of the Enlightenment undoubtedly was. It may appear to be so from the manuscript, with its rather conventional content of a *journal de voyage*, such as the somewhat prosaic daily observations on local characteristics and the customs of the sites visited. These go from reflections on the attitudes of women, to the character of fishermen (and even comparisons of the local prices of fish with those of England), notes on the way the land was cultivated (the type of grain or other vegetables, the relative state and abundance of crops, manuring methods, the number of horses used in a ploughing unit, etc.), the features of the landscape, the characteristics of rocks and stones, with the accurate enumeration of the types of trees, and the relative number of farms or country houses. The manuscript also contains observations on the animals bred or used for transportation, with sometimes the prices of horses, mules, asses, and stallions, revealing both the connoisseur and the gentleman farmer interested in the economic aspects of farm-breeding. Moreover, Walsh gives frequent details of the state of the roads, whether they were lined by trees, the type of *pavé* used (sometimes comparing the roads in parts of England, as for instance when, on his way to Paris, he writes: «The Pavé to Chantilly miserably bad, as bad as the Chester pavements» (WJ, 14 L.H.), or when he compares a road near Le Mans to the roads of Derbyshire). This seems an understandable interest in a traveller who is progressing either on horseback or by *chaises-postes* and other vehicles undoubtedly sensitive to the state of road surfaces.

As we would expect from a travel journal, there are comments on the quality of the accommodation (good for instance in a house at *La Fricherie* near Poitiers, but rather uncomfortable in other lodgings, such as at *Chantocé* near La Rochelle where Walsh says he slept in «the worst Inn in France next to that at St. Herman», and also bad at La Rochelle and at *La Ferté*). Although Walsh also notices the artistic and historical aspects of the sites visited (in particular during his passage through the region of the Loire with its magnificent *chateaux*, e.g. Blois and Chaumont, or that of the *Duc de Choiseuil* at Amboise), the journal is not particularly noteworthy from this point of view. On the contrary, what may appear remarkable is its lack of references to artistic objects and monuments during, for instance, the week spent in Paris (from June 15th to June 21st). Walsh does indeed linger over monasteries and convents, where he seems to like to stay, describing the organization and the life of the monks in great detail, as in the case of the *Chartreuse* of Val Dieu, and of the nearby Benedictine monastery of *La Trappe*, whose description takes up 20 pages (about a quarter of the entire manuscript). It may even seem that Walsh was attracted to the monastic life, though this is rather abruptly given the lie to by his final words on his sojourn at *La Trappe*:

> How far is the Monastery useful to Society? It confines religious Madmen, and prevents their doing mischief at large. ~~It is an useful Mad house.~~ (WJ, 107 L.H.)

[3] Among the scientists mentioned in this book who certainly acted as spies is Edward Bancroft (1744-1821), an American borne doctor who later settled in England and became Fellow of the Royal Society. Initially Bancroft acted as a spy for Franklin in England, but later he became an English spy mainly in France (see Dictionary of American Biography, vol. I, pp. 563-564).

On the whole, in its main part, Walsh's journal does not seem to be particularly interesting or noteworthy. It could even be said to belong largely to the domain of the ordinary and commonplace, whose documentary importance might depend more on our distance from the epoch concerned, than on the relevance of the events described.

THE SHOCK OF THE TORPEDO: FROM EVIL FORCES AND COLD VENOM, TO CORPUSCLES AND CONCUSSIONS

In spite of appearances, something exceptional was going to occur during Walsh's visit to France, something that would entirely change our perspective on this document, and justify its interest for the history of science at more than two centuries distance. What was really remarkable about Walsh's achievements in France during the summer of 1772 is the subject of another manuscript, to which Walsh refers on page 56 of his journal: «See the Diary of Transactions at la Rochelle & l'Isle de Ré, From June 26th to July 27th». By chance also this second manuscript entitled 'Experiments on the Torpedo or Electric Ray at La Rochelle and l'Isle de Ré – in June July 1772' has been preserved, and is now in London, in the Library of the Royal Society to which it was donated in 1965 by John Arthur Walsh, the 6th Baron Ormathwaite, an indirect descendent of Walsh.[4] (see Fig. 1)

The reason why, more than two centuries ago, in the pleasant springtime atmosphere of a 'Whitsun Monday', John Walsh *Esq.* left his residence in London and underwent the difficulties and fatigues of a long journey abroad, while in London there were «Crowds of Men and Women returning from Greenwich Park and Maying», was not to enjoy a visit to a foreign country, but exclusively to pursue the study of torpedo fish, the subject of the second manuscript. Walsh was intrigued by the amazing capacity of this peculiar aquatic animal to give off a shock very closely resembling the one produced by the common electric devices of those days, and notably by the Leyden jar (or 'phial'), the first electric capacitor in history, invented in rather fortuitous circumstances in the period 1745-1746. In the course of his researches, the similarity of the shock produced by the animal and by the physical device will appear to Walsh so close that eventually he will refer to the fish as the «animated phial».

For the shock of the physical instrument, we can appreciate the extraordinary impression provoked in those who first experienced it from the words of the Dutch scientist Pieter van Musschenbroek in 1746 to his French colleague, the famous naturalist Louis-Antoine Ferchault de Réaumur, member of the French *Académie Royale des sciences*:

> I will tell you of a terrible experiment, that I will advice you not to try personally, nor will do again I, that I have tried it and have survived for God's mercy, not even for all the Kingdom of France.[5]

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[4] Similar to the manuscript of the 'journey to Paris', the manuscript of Walsh's experiments in France, now in the Library of the Royal Society, is also in the form of a bound notebook. Besides a number of blank pages at the end, it consists of 174 handwritten and numbered left hand pages, with the corresponding right hand pages usually left blank, unnumbered and used for annotations (which are, however, more frequent than in the other manuscript)..

[5] See *Académie Royale des Sciences (Paris), Procès verbaux*, a. 1746, vol. LXV, p. 6.

These words may appear *a posteriori* exaggerated, and surely Musschenbroek, one of the most eminent 'electricians' of his age, did not keep to his initial proposal. However, we should remember that in 1746 it was not easy to understand how a violent shock could be produced by a simple glass bottle, filled with water and held in the hand, in which one tried to accumulate 'electric fluid' through a connection with the 'prime conductor' of a friction electrical machine. In time it was realised that similar and even stronger effects could be obtained by substituting the water in the bottle with a metallic lamina that coated its internal face ('internal armature'), and by using a similar lamina for the external coating. The device was called 'Leyden' after the town of Musschenbroek, Allamand and Cuneus, the three scientists who jointly invented it. It became an instrument frequently used for startling members of the upper classes in the literary *salons* of the epoch, as well as more ordinary people who eagerly attended the demonstrations given by 'itinerant physicists', and sometimes by charlatans. Its invention, moreover, provoked an increased interest in the study of electric phenomena with a crop of important discoveries, theoretical and practical, culminating in the epoch-making demonstration of the electric nature of thunder and lightning given by Benjamin Franklin a few years later.

If, at the time of Walsh's journey, the power of the Leyden jar had been known for a quarter of century, the power of a few species of fish to produce strong shocks, a power wonderful and frightening at the same time, had been known for a much longer time. Besides attracting scientific attention, it had been the object of literary works and of artistic representations dating back to Egyptian civilisation. Among these fish, well known to both Greeks and Romans, the flat torpedo was so common in the Mediterranean sea that the Latin poet Claudian dedicated a poem to it, starting with these verses:

> *Quis non indomitam mirae torpedinis artem*
> *audiit et merito signatas nomine vires.*[6]
>
> [Who has not heard of the untamed art of the marvellous torpedo and of the powers that win it its name?]

In the course of history, several explanations had been put forward to account for the ability of the torpedo and similar fish to produce a commotion and numbness in those that came into contact with them, and this power had often been considered as the expression of evil forces of a supernatural kind. Claudian follows a more scientific explanation, initially suggested by Galen on the basis of the similarity between the benumbing power of the fish and the action of cold bodies, and he attributes the 'art' of the torpedo to a cold venom («*gelido veneno*»).[7]

With the scientific revolution of the 17th century, the torpedo had been the object of interest and investigations, and the effects it produced were

[6] See Claudianus 1922.

[7] See for instance GALENUS 1533, p. 10; and 1541, p. 406. Galenus was, however, uncertain as to the true nature of the torpedo's shock. Extracts of torpedo's liver were used for their medical and / or magic power, and were supposed to have anti-aphrodisiac effects (TRALLIANUS 1557, pp. 71-72 and 115-116; LACROIX,1868, p. 84; see, however, REDI 1671, p. 41). For reviews of the old literature on the electric fish see KELLAWAY 1946, WU 1984, MOLLER 1995, MUSITELLI 2002.

accounted for largely on a mechanical basis. In 1671, in a dissertation addressed to the famous Jesuit Atanasius Kircher, Francesco Redi, an eminent member of the *Accademia del Cimento*, assumed that the peculiar organs responsible for the fish shock were of a muscular nature («*musculi falcati*»), and proposed that a sudden contraction of these organs was responsible for the shock.[8] A few years later, in 1678, a pupil of Redi, Stefano Lorenzini, postulated that, at the moment of the shock, the fish emitted a multitude of minute corpuscles («*corpicciuoli*») with great violence, which produced the commotion and the benumbing effect by penetrating deeply into the tissues of the prey (or of the experimenter) and hitting the nerves (Fig. 2).[9]

Réaumur, Musschenbroek's correspondent, also advocated a mechanical explanation, which did not, however, involve anything like the *corpicciuoli* of Lorenzini. The French scientist assumed instead that the torpedo's shock was the consequence of a direct and rapid percussion of nerve trunks similar to that produced in the forearm by the action of a sharp body hitting the nerves in the region of the elbow.[10] According to Réaumur, the shock would be produced by the fish at the moment that the dorsal surface of its body, normally flat or even concave in the preparatory phase, became suddenly convex as a consequence of a contraction of the *musculi falcati* that produced «a movement so prompt that even the most attentive eyes could not perceive it». (Fig. 3).[11]

THE ELECTRIC HYPOTHESIS

This explanation of the torpedo's shock based on a percussive-concussive action was proposed by Réaumur in 1714 on the basis of observations carried out on torpedoes caught on the coasts of Poitou, in France, near La Rochelle. It dominated the first part of the 18th century, and was considered a reference hypothesis also for the similar shock produced by the other species of fish that had come to the attention of the naturalists of the age, notably by two freshwater fish, the eel of Surinam and the catfish (or 'silurus') of the Nile whose effects were even stronger than those produced by the torpedo. However, after the invention of the Leyden jar, it became rapidly evident that the shock produced by all these fish was similar to that produced by the physical device. Apparently this similarity was first recognised by the French naturalist Michel Adanson in the Senegal in the years 1749-1753. Adanson investigated the shock produced by an African freshwater fish, called «*ouaniear*» by the natives, and «*trembleur*» by local French people, because of its capacity to «produce not simply numbness as the Torpedo, but rather a trembling effect very painful in the arms of those that touch it». This fish was very probably the electric catfish or *Malapterurus electricus*, already referred

[8] REDI 1671, pp. 41-42. Francesco Redi (1626-1694), naturalist, linguist and poet, is particularly well known for his study showing the impossibility of spontaneous generation, which paved the way for the successive investigations of Spallanzani and Pasteur. For recent studies on Redi an on his time see BERNARDI and GUERRINI, eds. 1999.

[9] LORENZINI 1678. Stefano Lorenzini (born somewhat after 1652, died after 1700) was a pupil of Redi, Malpighi and Steno. He is famous for the description of the peculiar structures present in some fish (called after him 'ampullae of Lorenzini'), which contain extremely sensitive electroreceptors. For a recent contribution to Lorenzini's biography see GUERRINI 1999.

[10] see RÉAUMUR 1741. Louis Antoine-Ferchault de Réaumur (1683-1753) was one of the greatest naturalists of his epoch, noted particularly for his entomological researches. He was born in La Rochelle. Besides for his interests in natural history, he was known for a series of studies of practical relevance, ranging from metallurgy, porcelain production, egg incubation, thermometry.

[11] «*un mouvement si prompt, qu'il est impossible aux yeux les plus attentifs de l'apercevoi*r». REAUMUR 1741, p. 351.

to in 1615 by a Portuguese Jesuit, Nicolao Goudiño, as an «Abissinian Torpedo», a fish that had been depicted by Egyptians in bas-reliefs dating back to the Old Kingdom (2800-2300 b. C.).[12] As to the comparison between the shock produced by the fish and by the Leyden jar, Adanson wrote:

> Its effect which did not appear to me sensibly different from the electric commotion of the Leyden experiment that I had tried many times, is similarly communicated by a simple contact, with a stick or rod of iron five or six feet long, to such a point that one drops instantaneously all the things that he kept in his hands.[13]

Besides the correspondence of the effects produced on the experimenter, the possibility that the fish shock could be communicated through a metallic body also argued for the similarity with the shock produced by the Leyden jar. Since the studies carried out in 1729-1730 by Stephen Gray, it had been clearly established that electricity could be communicated through a series of materials that were eventually indicated as ‘deferents’ or ‘conductors’, and notably through metals or humid bodies, while it was intercepted by other bodies indicated as ‘insulators’ or ‘non-conductors’ (notably glass, sealing wax, oil and several resins).[14]

Soon after the publication, in 1757, of the *Histoire naturelle du Sénégal*, containing Adanson’s account of the shock produced by the strange African fish, a series of observations were collected on the similar properties of another freshwater fish, similar to an eel, found in the tropical regions of America. These observations were prompted by the Dutch physicist Allamand, colleague of Musschenbroek and one of the discoverers of the Leyden jar.[15] As had occurred with the *trembleur* fish described by Adanson, also in the case of this eel (later called ‘Gymnotus’ or ‘eel of Surinam’), the shock was found to be transmitted through metallic and humid bodies. These characteristics pointed to a possible electric nature of the phenomenon, and seemed therefore to contradict the mechanical interpretation advocated particularly by Lorenzini and Réaumur for the shock of the torpedo. In the case of the latter, the possibility that the shock could be transmitted to the experimenter by some intervening matter had been taken into account by Lorenzini, who, however, excluded it outright, on the basis of a series of experimental observations. In a similar vein, Lorenzini rejected the accounts of fishermen who claimed that the shock was transmitted by the fishing net, or by the harpoons used to catch the fish, or even by simple sea water, as inspired by imagination and fantasy According to Lorenzini «in order that the Torpedo might produce its effects, it was necessary to touch it [directly] in some part of its naked body».[16]

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[12] GOUDIÑO 1615, p. 67.

[13] ADANSON 1757, p. 135: «*Son effet qui ne m’a pas paru differér sensiblement de la commotion électrique de l’experience de Leyde, que j’avois déjà éprouvée plusieurs fois, se communique de même par le simple attouchement, avec un bâton ou une verge de fer de cinq ou six pieds de long, de maniere que on laisse tomber dans le moment tout ce qu’on tenoit à la main.*». Michel Adanson (1727-1806), botanist and naturalist, is one of members of the *Académie Royale des Sciences* that Walsh will meet during his sojourn in Paris (see later). He was also fellow of the Royal Society of London. His journey to Senegal, on which his account of the torpedo shock is based, was made on behalf of the *Compagnie des Indes*.

[14] See HEILBRON 1979.

[15] ALLAMAND 1756, VAN DER LOTT 1762; see KELLAWAY 1946.

[16] Besides by fishermen, the possibility of the shock of torpedo might be transmitted through some intervening body, and notably through the fishing tools, was usually admitted both by classical authors and by more recent ones. In

Lorenzini's conclusions on the lack of transmissibility of the torpedo's shock, by any kind of material, fitted in with his mechanical hypothesis for the shock and especially with his idea that the effect depended on minute corpuscles emitted by the animal and penetrating the body of the experimenter. It was indeed difficult to conceive how these corpuscles could pass through a long rod, or through the chords and threads of the fishing net. On the other hand, in his study of the torpedo, Réaumur found that the shock could be transmitted, though with great attenuation, through a rod, but he did not investigate whether the transmission's efficacy changed according to the material the rod was made of. Réaumur's view of the torpedo's shock as due to a kind of mechanical concussion or vibration could account for the partial transmission of the shock through a relatively rigid body.

To summarise the state of the matter, after the middle of the 18th century evidence was accumulating to suggest the possibly electric nature of the shock produced by some species of fish (notably the Nile catfish and the eel of Surinam), based on the similarity of the effects produced by these fish to the shock of electric devices. However, the reference hypothesis on the nature of the fish shock still remained the mechanical one, based on Lorenzini and Réaumur's studies of the torpedo.[17] In actual fact, the new evidence in favour of the possibly electrical nature of the shock appeared to be rather anecdotal, limited to just a few observations not based on a systematic approach. Moreover, the justification of the mechanical hypothesis was not specifically addressed nor questioned in a direct way in the new investigations.

THE TORPEDO AND THE TORPORIFIC EEL: THE SCIENTIFIC CHALLENGE OF EDWARD BANCROFT

The situation changed in 1769 when Edward Bancroft an American borne doctor (who lately established himself in England), with an interest in natural history, published an account of observations made in equatorial America, in 'An essay on the natural history of Guiana'. Of particular relevance for Walsh's work on the torpedo was Bancroft's description of a peculiar fish «of the eel tribe», referred to as a «Torporific Eel», found «near the coasts and the rivers of Guiana» and especially in the *Essequebo* river. We will therefore quote the part concerning this fish from Bancroft's *Essay* in some detail. After a rather detailed description of the external morphology of the fish, which appears to be superficially similar to a Lamprey, Bancroft wrote:

Claudianus' poem 'Torpedo', should the unfortunate fisherman capture the fish, he would not rejoice for his catch but, due to the passage of the torpedo's «venom» through the fishing line and rod, he would let «go his rebel prey, returning home disarmed without his rod». («*damnosum piscator onus praedamque rebellem iactat et amissa redit esarmatus auena*» (see CLAUDIANUS 1922 and 1985). More than ten centuries after Claudianus, Michel de Montaigne wrote : «*La torpille a cette condition, non seulement d'endormir le membres qui la touchent, mais au travers de filets, & de la scène, elle transmet une pesanteur endormie aux mains de ceux qui la remuent et manient : voïre dit-on dauantage, que si on verse de l'eau dessus, on sent cette passion qui gagne contremont iusques à la main, & endort l'attouchement au travers de l'eau.*» («The torpedo has the property, not only of benumbing any limbs that touch it, but it also transmits a benumbing dullness through the nets or the line to the hands of anyone who moves or handles it; indeed, it is even said that if you pour water on the torpedo, you will feel this sensation rise upward against the flow, all the way to your hand, deadening your touch through the water». (see MONTAIGNE 1652, p. 338).

[17] On a mechanical explanation, inspired by Réaumur, was also based the account of the Torpedo given in 1765 by De Jaucourt in the *Encyclopedie* (article *Torpille*, Tome XVI, pp. 428-431.)

> But the most curious property of the Torporific Eel is, that when touched either by the naked hand or by a rod of iron, gold, silver, copper &c. held in the hand, or by a stick of some particular kinds of heavy *American* wood, it communicates a shock perfectly resembling that of Electricity, which is commonly so violent, that but few are willing to suffer it a second time.[18]

This fish - Bancroft notes soon afterwards - is probably of the same species as the one found by the southern shores of the Amazon river, and already described by «*Mons. de La Condamine*» in his account of a journey into the American interior, as «*une espèce de Lamproie*» [‘a kind of Lamprey’], that has in addition the same property of the Torpedo. The person who touches it by the hand, or even by a stick, experiences a painful numbness [‘*engourdissement*’] in the arms, *&c.* sometimes he can also be overset by it, as it is said».

According to Bancroft, the short description provided by *Mons. de La Condamine* corresponds fairly well to some properties of the ‘Torporific Eel’ of Guiana. Charles-Marie de La Condamine, a botanist and naturalist, was one of the illustrious *savants voyagers* of the Enlightenment and had visited Guiana during a long (1735-1744) expedition to America undertaken to measure the earth’s meridian in near equatorial regions (and thus provide indications on the shape of the globe).[19] With respect to La Condamine’s assertion about the eel phenomena, Bancroft’s only proviso is that, if the shock is to be transmitted through some material (*a baton*, i.e. a stick, as reported by the French scientist) «this must be of few particular kinds of wood, as I could never discover – Bancroft says – any sensation from touching him with oak, ash or indeed any kind of wood trimming in water, which I have tried.». And he continued:

> What affinity there may be between the shock of Torporific Eel and that of Torpedo, I am unable to determine with certainty, having never felt the latter; but from all the particulars which I have been able to collect relatively thereto, I think it is pretty evident both are communicated in the same manner and by the same instruments.
> Some years since the celebrated *Mons*. *De Réaumur* communicated to the Royal Academy of Sciences at Paris a Paper, in which he undertook to demonstrate, that the shock of the Torpedo was the effect of a stroke given by great quickness to the limb that touched it, by muscles of a peculiar structure. To this hypothesis all *Europe* beheld an implicit assent, and M. *De Réaumur* has either to enjoy the honour having developed the latent cause of this mysterious effect. But if we may be allowed to suppose, that is undoubtedly true, that the shock of the Torpedo and that of the Torporific Eel, are both communicated in a similar manner, and by similar means, it will be nowise difficult to demonstrate, that all of M. *De Réaumur*’s pretended discovery is perfect non-entity. You may, perhaps, think it an act of presumption in me, to dispute the authority of a man, whose literary merit is so universally acknowledged; but I am convinced, that an implicit faith, in whatever is honoured with the sanction of a great name, has proved a fruitful source of errors in philosophical research; and whilst I have sense and faculty of my own, am resolved to use them with the freedom for which they are given. Humanity is ever exposed to deception, and charm for novelty may perhaps have precipitated M. *De Réaumur* into an error.[20]

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[18] BANCROFT 1769, p. 192.
[19] LA CONDAMINE 1751. Charles-Marie de La Condamine (1701-1774), naturalist and astronomer, was one of the members of the *Académie Royale des Sciences* that Walsh met during his sojourn in Paris. He became famous also for his crusade in favour of inoculation as a method of prevention of smallpox.
[20] BANCROFT cit., pp. 195-196.

Bancroft continued by exposing those characteristics of the eel's shock that made it unlikely that it could be accounted for on a mechanical basis. He mentions the transmission of the shock through the fishing line when the fish is caught by a hook, the transmission through an iron rod to a chain of people (up to 12) touching each other in a circle («in a manner – he says – exactly similar to that of an electric machine»); and also the transmission of the shock through the water in which the fish is swimming. On the basis of the characteristics listed, Bancroft assumed that the shock was produced by «an emission of torporific or electrical particles», depending on the life of the animal and under the control of its will. Finally, coming back to the torpedo and to *M. De Réaumur*, he concludes provocatively:

> From whence it is self-evident that, either the mechanisms and properties of the Torpedo and those of the Torporific Eel are widely different, or that Mons. *De Réaumur* has amused the world with an imaginary hypothesis: and, from my own observations, as well as the information which I have been able to obtain on this subject, I am disposed to embrace the latter inference.[21]

If one was inclined to recognise the importance of Bancroft's statement, and took it as a veritable scientific challenge, then all that remained to be done was to investigate the characteristics of the shock of the torpedo in order to see whether they corresponded to those of the eel; or, on the contrary, were so different, as to justify, in the case of the torpedo, a mechanical explanation that appeared incompatible with the eel's shock.

This was just what John Walsh decided to pursue through his journey to France in the summer of 1772. Walsh was indeed well acquainted with Bancroft's views on the subject. Both were members of the Royal Society and frequented a circle of 'natural philosophers' in London interested in the study of electrical phenomena. This circle included scientists as eminent as Benjamin Franklin and Joseph Priestley.[22] The reason for travelling to France to investigate the torpedo's shock was due to the difficulty over finding live animals in England. Perhaps we could also be justified in assuming that a further reason might have been the desire to perform experiments on torpedoes from the same region, the *Poitou*, where Réaumur had carried out his experiments more than half a century before. (Fig. 4)

ELECTRICITY AND ANIMAL ECONOMY: FROM ELECTRIC MEDICINE TO ELECTRIC FISH

The importance of Walsh's work with electric fish, beginning with his journey to France in 1772 and culminating, as we shall see, in London with the production of a visible spark from the shock of an electric eel in the summer of 1775, cannot be overstated. It goes far beyond his desire to ascertain a particular point of natural history concerning the curious property of some peculiar fish.

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[21] BANCROFT cit., pp. 198-199.

[22] As Walsh recognises in his 1773 article on the 'Philosophical Transaction', Franklin was influential in devising the plan of the experiments to be performed on the torpedoes at La Rochelle. Among the Franklin's papers there are two short notes undated but surely written before Walsh's journey to France, which attest the relation between Walsh, Franklin and Bancroft. In the first it is written that Walsh asks Franklin for Bancroft's address in London saying that he «is very desirous of making some enquiries concerning the Torporifick Eel» (p. 162, Franklin's papers, vol. 19). From the other note we learn that Franklin had borrowed from Walsh the book of «Laurenzini]» (i.e. Lorenzini) and desired to have it back because «he wishes to look into it for some particulars» (see FRANKLIN 1960-, vol. XIX p. 163)

As a matter of fact, the case of electric fish shock was an aspect of a much wider problem, at the centre of the age's scientific curiosity, namely the possibility that electricity could play a role in the physiology of animal and human organisms. The discoveries on electrical phenomena that had characterised the first half of the 18th century stimulated interest in the possible influence of electricity on living beings. Besides showing that electricity could favour the germination of seeds or the growth of vegetables, induce the contraction of muscles and promote circulation and perspiration, one specific reason for the interest lay in the possibility that electricity could prove useful in medicine for the treatment of various diseases. This seemed an almost unavoidable prospect on the basis of the philosophical conceptions of the time, as is particularly evident from this curious statement in a book published in1744 by Johann Gottlob Krüger:

> Since electricity must have usefulness, and it cannot be looked for either in theology or law, there is obviously nothing left but medicine.[23]

As a matter of fact, from 1740 electricity had been applied in various forms to the treatment of several diseases, and 'electric medicine' had rapidly become important. One could thus easily agree with Joseph Priestley who, in his 'History and present state of Electricity' published in various editions from 1767-1775, noted that «electricity is now become a considerable article in *materia medica* [i.e. therapeutics]».[24] Among the diseases treated more or less successfully by the application of electricity, Priestley listed conditions as diverse as paralysis, apoplexy, deafness and blindness, gout, headache, rheumatic and muscular pain, odontalgia, menstrual disorders, bleeding and mental diseases.

Another important reason for the interest in electricity in the domain of life sciences emerged from the possibility that electricity could be the agent of nervous conduction. In the classical medicine, it was supposed that the nervous function depended on 'animal spirits', mysterious and elusive entities of uncertain physical nature that, flowing through the inner core of nerves, produced sensations or movements according to the final targets they eventually hit, i.e. central regions of the nervous system or muscular tissue. Although animal spirits still remained in some way a reference theory for nervous conduction up to the end of the 18th century, with the scientific revolution of the 17th century their validity was questioned and new hypotheses emerged. It was suggested that nervous conduction depended on a 'nervous fluid' secreted by the brain and circulating in nerve fibres.[25]

With the great development of electrical science in the first half of the 18th century, and in particular with the notion that electricity could flow with great rapidity through long, thin wires and produce vigorous effects, interest developed in the possibility that nerve fluid could correspond in some way to the 'electric fluid', or have some special affinity with electricity. Apparently the first to suggest the possible involvement of electricity in nervous

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[23] «*Da nun aber die Electricität einmahl vor allemahl einen Nutzen haben muste, so sahe man, daß er weder in der Theologie oder Rechtsgelahrheit zu suchen wäre, und da bleibet feyilich nichts, als die Artzeneygelahrheit übrig*» KRÜGER 1745, p. 46. We may perhaps recall here that at about the same epoch the therapeutic properties of *Salix alba* were rediscovered by Rev. Edward Stone on the basis of a somewhat similar providential conception of nature and medicine.
[24] PRIESTLEY 1775 vol. I, p. 472.
[25] PICCOLINO 1997 and 1998; BRESADOLA 1998. PICCOLINO and BRESADOLA 2003.

conduction was Stephen Hales, who in his 'Hemastaticks' published in 1733, established a possible relation between «muscular motion [… a] wonderful and hitherto inexplicable mystery of nature [… and] a vibrating electrical Virtue [… which] can be conveyed and fully acts with considerable Energy along the surface of animal Fibres, and therefore on the Nerves».[26] Hales' suggestion was derived from previous speculations developed by Newton in three famous 'Queries' included in the second edition of the 'Opticks'.[27]

In Italy a 'neuro-electric' hypothesis, based on the assumption of a particular combination between nerve fluid and electrical matter, was suggested by Tommaso Laghi, professor at the University of Bologna, who in 1757 wrote:

> Certainly I don't think it totally opposed to a correct reasoning to hypothesise that the electric matter, widely diffused in any place, is delimited by the nervous liquid secreted by brain glands, so that it could flow through nerves to favour sense and motion.[28]

In particular, according to Laghi, muscle motion was likely to be produced by the force of the electric matter conveyed to muscle fibres by nerve trunks.

OBJECTIONS TO A PHYSIOLOGICAL ROLE OF ELECTRICITY: HALLER AND 'IRRITABILITY'

Laghi expressed his hypothesis during an important debate on the mechanisms of muscle contraction, a debate that inflamed the scientific world, relating to the theories Albrecht von Haller, a physiologist and polymath of Swiss origin who was professor at the University of Gottingen. Haller attributed muscle contractility to a force and to an organisation entirely intrinsic to muscle tissue, and considered the intervention of nerves in muscle motion simply as a stimulatory influence capable of bringing about this intrinsic muscle property that he called 'irritability'. In other words, according to Haller, irritability was the effective cause of muscle contraction, and any external influence, of either a physiological or experimental nature, induced the muscle to contract exclusively by stimulating its intrinsic irritability.[29]

Within the conception of irritability, Haller and his followers were against any theory that considered nerve activity as the effective cause of muscle contraction. It was mainly for this reason that they challenged any electric hypothesis of nerve conduction, by putting forward important objections of both a physical and physiological character. The electric matter, they said, is of such a nature that it tends to spread from a place where it is in excess to a place where is less, if the two places communicate through conductive substances. Since living tissues are electrically conductive, due to

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[26] HALES 1733, pp. 58-59.
[27] NEWTON 1718, Queries 12-14.
[28] LAGHI 1756, pp. 115-6: «*Equidem censeo non admodum a recta philosophandi ratione alienum esse conjectari, materiam electricam longe, lateque diffusam a liquido nerveo in glandulis cerebri secreto determinari, ut per nervos fluat ad sensum, ac motum juvandum*».
[29] HALLER 1768-1775, vol. IV, pp. 254-255; CALDANI 1757; FONTANA 1757. Albrecht von Haller (1708-1777) was one of the main figures of the 18th centuries sciences. Besides for his studies in physiology, anatomy and medicine, and for his immense bibliographic production in these fields, he was known for his writings about physics, chemistry, literature, religion. Apparently Franklin's interest for electricity was stimulated by reading an article of Haller on the subject (see HEILBRON 1977 B and 1979).

their humid nature, no stable imbalance could exist inside animal bodies, and, as a consequence, the force required to move electrical matter through nerves according to physiological necessities could not exist. A somewhat related argument concerned the difficulty over envisioning how the electric flux could be restricted to the specific nerve paths, required by physiological needs. Were the nervous fluid of an electrical nature, argued Haller, we would move all the muscles of the foot when we aimed at moving a single toe, because the electric signal flowing through the nerve fibres directed to that particular toe, would spread to adjacent fibres and thus influence the entire foot. As a matter of fact, when Laghi wrote that «electric matter» might by «delimited by the nervous fluid [...] so that it could flow through nerves to favour sense and motion», he was attempting to face these objections of the Hallerians. The latter criticised Laghi's views by noting that it was difficult to conjecture that the nervous fluid could restrain the electric fluid, and thus confine it to specific nervous paths, and, at the same time, leave it the freedom to flow with the great speed demanded by the physiological needs.

The main objections to an involvement of electricity in nerve function, and, in particular, the difficulty over envisioning the presence of an electric imbalance inside living tissues, and in conceiving how electricity could be restrained inside specific structures, also made it hard to suppose that electricity could play a role in other physiological processes. For the case of electric fish, there was the additional difficulty represented by the conductive nature of the liquid habitat of the animals (fresh or salt water according to the fish species). For a contemporary electrician an 'electric fish' seemed a non-sense, somewhat like a charged Leyden jar plunged in water. We can appreciate the difficulties in conceiving the electric nature of the torpedo's shock from the following passage in a letter from William Henly to William Canton, both famous English electricians, at the time of the debate on the electric nature of the fish's shock:

> Mr Ronayne has made a curious remark upon the supposed electricity of the torpedo: he say, if that could be proved, he saw not why we might not have storm of thunder, & lightning, in the depth of the Ocean. Indeed, I would say, that when a Gentleman can so give up his reason as to believe in the possibility of an accumulation of electricity among conductors sufficient to produce the effects ascribed to the Torpedo, he need not hesitate a moment to embrace as truths the greatest contradictions that can be laid before him.[30]

As we shall see, Henly changed his mind in 1775 when Walsh succeeded in providing decisive evidence on the electrical nature of the fish's shock with the famous experiment in which he succeeded in producing a spark from the discharge of an electrical eel.[31]

WALSH AND THE ELECTRICITY OF THE TORPEDO: FROM INCREDULITY TO ENTHUSIASM

[30] Letter of William Henly to William Canton, dated 21 May [17] 75 Canton papers of the Royal Society, Vol. II, p. 105.

[31] Letter of William Henly to William Canton (undated but probably written in November 1776), Canton papers of the Royal Society, Vol. II, p. 103.

To start with, Walsh himself was reluctant to accept the «electricity of the torpedo», as we can see from his journal of experiments at La Rochelle. On June 30th 1772, four days after his arrival, Walsh was finally able to obtain his first live torpedo and make experiments on it, together with his nephew Arthur, and *Monsieur* Saunier, one of the many gentlemen of La Rochelle who became interested in Walsh's studies and collaborated actively with him. As we learn from the 'journal of experiments', this first live torpedo was captured and studied at Lauzieres, «a small fishing village about a league and half to the northward». It was a «female Torpedo, Marote ou Tremble [the local fishermen's names for the fish] of 3 lb English caught at 8 o'clock». Arthur was the first to experience the shock. Afterwards it was the turn of Saunier and eventually of Walsh himself. This first shock is described by Walsh as follows:

> she gave us both a Shock; his affecting him to the Elbow, mine half way of the past of my arm above the Elbow; both instantaneous in the commencement, and ending; precisely like the Electric shock.! (WE, 6-7)

To that description Walsh adds a note in the margin that testifies his reluctance to admit that the shock could be a genuine electric phenomenon:

> On this my first experiment on the effect of the Torpedo, I exclaimed this is certainly Electricity – but how? (WE, 7 R)

A few days later, however, on July 9th, the incredulity turned to enthusiasm, and during a dinner «with M. S.t Michel, present M. Monier, Dumesnil, Weis, &c.», Walsh could finally announce:

> the Effect of the Torpedo to be Electrical [...] agreeably to our Experiments of this day. (WE, 54)

In rapid succession, on July 12th Walsh wrote a letter to Benjamin Franklin that was eventually to be read to the Royal Society about one year later, on July 1st 1773 (see Fig. 5), and will be included in a famous article published in the same year in the 'Philosophical Transactions'.[32] (see Fig. 6) In a direct way, or via a French translation published by Jean-Baptiste Le Roy in Paris in 1774 in the famous '*Observations sur la Physique*' of the Abbé Rozier, and, moreover, via the other channels of communication that existed in the Enlightenment's 'Republic of Letters', this article reached all the scientific community of the age. It was destined to become a landmark in the revolutionary phase of scientific development that characterised the second half of the 18th century.[33]

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[32] A manuscript of the letter to Franklin is conserved in the Archives of the Royal Society of London. The letter is transcribed faithfully in WE, 71-72 (both left and right hand pages). The letter reported in Walsh's article of 1773 published in the 'Philosophical Transactions' is somewhat modified with respect to the original handwritten text. Walsh published his article on the "Philosophical Transactions" as a separate pamphlet in 1774. Moreover, in 1775 the same article, together with an article by John Hunter's article on the anatomy of torpedo and with another Walsh's paper on English torpedoes published in 1774 on the 'Philosophical Transactions' (see later), was included by the Royal Society in the 'Three tracts concerning the Torpedo, published in the Philosophical Transactions for the years 1773 and 1774', London, W. Bower and J. Nichols.

[33] LE ROY 1776 and 1777. Jean-Baptiste Le Roy (1726-1779) was one of the members of *Académie Royale des Sciences* .that Walsh met during his visit to Paris (see later). He was a physician interested in various aspects of natural philosophy and particularly in electricity. He practiced electrical medicine and was involved in a project of rebuilding

We have seen that Walsh's investigation on the torpedo had in some way been prompted by Bancroft's statement on the correspondence between the shock of the torpedo and that of the 'Torporific Eel'. However, in providing an experimental demonstration of the electrical nature of the torpedo's discharge, and in thus solving the dilemma proposed by Bancroft, Walsh did not simply elucidate a particular aspect of the natural history concerning the singular characteristics of a curious fish. He also provided the first clear evidence of the involvement of electricity in a physiological process of the animal kingdom, and thus became a pathfinder to all future investigation on the role of electricity in animal physiology. As a matter of fact, Walsh's achievement undermined the value of the Hallerian objections to the electric theory of nervous conduction by providing a *de facto* argument against any *a priori* statement. In the Newtonian climate that dominated the science of the 18th century, an experimental demonstration for a given proposition was indeed considered of a stronger probative value than any contrary *a priori* objection not supported by experimental evidence.

ELECTRIC FISH STUDIES IN THE 'WALK' OF PHYSICS AND PHYSIOLOGY

The confidence that life scientists derived from his investigations into the possible involvement of electricity in 'animal economy' prompted Luigi Galvani's studies of the involvement of electricity in nerve and muscle function: these studies were to lay the foundations of modern electrophysiology.[34] Moreover, Walsh's researches on electric fish were also important for advances in physics, because they led to important developments in the physical conceptions of electricity. As we shall see, this was due mainly to the work of Henry Cavendish and Alessandro Volta. Cavendish, one of the most eminent English scientists of the time, took up Walsh's work in his investigations on the mechanisms of electric fish discharge. Cavendish's theoretical studies on the torpedo somewhat anticipated the discovery of the laws of the capacitor by Alessandro Volta. As for Volta, the inventor of one of the most extraordinary devices of human history, the electric (or Voltaic) battery, he derived important inspiration for his invention from electric fish research, as well as further elaboration on the mechanism of its functioning.[35]

In addition to Cavendish, John Hunter, one of the most eminent English anatomists and surgeons of the century, also took an active part in these investigations, performing an accurate anatomical study of some torpedoes and of an electrical eel that Walsh had used for his physiological researches.[36]

That both physiology and physics could benefit from studies of electric fish was just what Walsh himself had somewhat prophetically anticipated when, in the opening of his 1773 article on the «electricity of the Torpedo», he had referred to these studies as:

> a subject not curious in itself, but opening a large field for interesting enquiry, both to the electrician in his walk of physics, and to all who consider, particularly or generally, the animal oeconomy.

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the *Hotel-Dieu* hospital in Paris. He was elected a fellow of the Royal Society of London on 10 June 1773 with the support of Franklin and Walsh. He had an intense correspondence with Benjamin Franklin (see FRANKLIN 1960-).

[34] PICCOLINO 1997 and 1998. BRESADOLA 1998. PICCOLINO and BRESADOLA, 2003.

[35] GLIOZZI 1937; HEILBRON 1977 A and 1979; PANCALDI 1990.

[36] HUNTER 1773 and 1775.

‘TRANSITIONS’ IN THE SCIENTIFIC JOURNEY OF JOHN WALSH

One of the symbolic aspects of a journey is the sense of mutation, the sense of transformation and transition that it conveys. There are many transformations to link to the journey Walsh began on June 8th 1772. One, already noted, is the change it implies from the level of the ordinary and prosaic, to the level of the remarkable and historical; a change that justifies our interest in what Walsh wrote during the period spent in France, in both the journal of experiments and in the *journal de voyage*. Although this latter journal is largely of a non-scientific character, it can nevertheless provide useful information for reconstructing some practical aspects of Walsh’s research, and also for outlining some characteristics of his scientific psychology.

In Walsh’s work at La Rochelle and at the Isle de Ré, the specific transformation from the level of the ordinary to the level of history assumes other connotations. Walsh’s studies attracted the attention of a literary party, comprising various personages typical of a provincial world of the *ancien régime*: the Bookseller, the Apothecary, the Hydrographer, the Naturalist, the Surgeon, many members of the local *Académie* and in particular the Director and the «Perpetual Secretary», who was also the Mayor of the town, the *Inspecteur des Fermes,* the Professor of Rhetoric at the Royal College, the military Governor and *Commandant en chef*, the *Ingenieur*, the *Avocat*, several Colonels and so on. Some of these personalities took an active part in the experiments, others discussed the effects of the torpedo or simply attended Walsh’s demonstrations. As we have already noted, Walsh announced the electric nature of the torpedo during a dinner with some of these personages of La Rochelle. Of many literary and scientific discussions, we often have no records (or no records needed to be taken because of their ordinary character), but these particular ones between Walsh and his acquaintances at La Rochelle are bound to become worthy of historical interest at more than two centuries distance.

The literary party that attended Walsh’s experiments and took an active part in some of them, reminds us of another transformation attested by Walsh’s journey to France and by the investigations begun there. During the 18th century, scientific endeavour gradually changed from a kind of pleasant entertainment for ladies and gentlemen, to a more technical and professional activity requiring specific competence, and which benefited from the association of scientists from varied backgrounds. The case of John Walsh, a ‘natural philosopher’ with an interest in electrical research, who collaborated with such a various and composite world of amateur scientists in the initial phase of his experiments, but eventually became associated with an eminent physicist (Cavendish) and with an expert anatomist (Hunter) in his investigations on electric fish, is an indication of the transition to a modern conception of scientific inquiry. In some ways it prefigures the interdisciplinary collaboration and organization typical of modern scientific research.

We can see another fundamental transformation, the transition from an age in which a singular property of living nature, long considered as pertaining to evil and magic, passes from the *mirabilia* of a *Wunderkammer*, to an object of study in a scientific laboratory. In this transition it may perhaps lose the romantic charm of the legendary and fantastic, but it begins to reveal new unsuspected wonders, opening up insights into the extraordinary mechanisms underlying fundamental life processes.

Through the manuscripts left by Walsh we can witness these transitions as if from behind the scenes, from a perspective that, besides the findings of the investigation, allows us to realize the difficulties and uncertainties the scientist had to face, and at the same time the exciting atmosphere that can accompany the moment of success and discovery.

In the passage of the journal of the experiments in which he announces the electric nature of the torpedo's shock, Walsh adds a note in French, «*Je l'ai donté*» (a slightly incorrect spelling of '*dompté*'), 'I have tamed it', that refers to a comment he wrote in the side page:

> alluding to a verse of the Latin poet Claudian: Quis non indomitam mirae torpedinis artem audiit? (WE 54 R)[37] (see Fig. 7)
>
> [Who did not hear of the untamed art of the wonderful Torpedo?]

The experimental demonstration of the electrical nature of the torpedo's shock is for Walsh like the taming of a wild and obscure force of nature, a force that had appeared at the same time wonderful and frightening, but that, from then onward, was definitely placed under the dominion of human intelligence and human power.

'It is I – Walsh seems to cry in his journal – that did it!'

One of the greatest psychological rewards for the scientist at the moment of discovery is indeed an awareness that he (or she) is the first human being to achieve the new result, to know the new truth, and to dominate the new force.

Although in the published articles on torpedo research Walsh may reveal a certain literary talent, particularly in the subtle humour he reveals in some passages, there is nothing in them of the vivid freshness with which, in his journal of experiments, he writes «*Je l'ai donté*» to express the pride and enthusiasm of the discoverer. Also from this point of view, Walsh's manuscripts are precious documents for the historian. It is a misfortune that Walsh apparently did not leave any direct record of the experiment in which three years later he was able to produce a visible spark from the shock of an electric eel (see later). We can only imagine his emotion and enthusiasm in obtaining this decisive result that succeeded in fully convincing the scientists of the time of the genuinely electric nature of the fish's shock.

WALSH'S RESEARCHES: FROM FAME TO OBLIVION

We have seen that the news of John Walsh's experiments on electric fish circulated widely in the 'Republic of Letters' of the Enlightenment. Besides the articles on the torpedo published in the 'Philosophical Transactions' translated into French and published in a widely circulated journal, references to Walsh's experiments were present in a variety of scientific journals, e.g. 'Memoirs', 'Proceedings of Academies', and scientific publications of various

[37] Walsh follows the version of Claudian's poem '*Torpedo*' that we can read for instance in the '*Carmina*' edited by Platnauer in 1922 for Heinemann & Putnam. The Teubner version edited by Barrie Hall in 1985 reads '*dirae*' instead of '*mirae*', and thus the 'art' of the torpedo is 'dreadful' instead of 'wonderful.

kinds.[38] To the widespread diffusion of the news of Walsh' experiments on the torpedo contributed in particular the Copley medal, awarded to Walsh by the Royal Society, one of the most prestigious honours of what was already one of the most highly thought-of scientific academies in Europe. On the occasion of the award, presented to Walsh on June 30th 1774, the President of the Royal Society, John Pringle, made a long speech, full of praise for Walsh's achievement.[39] This was published by the Royal Society in 1775, and soon translated into French and printed in Paris in the well known journal *Observations sur la Physique*. An Italian version was published in Naples in 1776. Without doubt the publication of Pringle's speech contributed significantly to the diffusion of Walsh's results on the torpedo. (Fig. 8).

As well as in specialised scientific literature, for a long time there was frequent mention of Walsh's researches in medical and physiological textbooks, within the discussion of the possible role of electricity in nervous conduction and in other physiological processes. Among the first authors that mentioned Walsh's experiments on electric fish in medical textbooks was the Genevan doctor Daniel de La Roche, who referred to these experiments in a book of clinical neurophysiology published in 1778.[40] La Roche's book found its way into the personal library of Luigi Galvani, the Bologna doctor whose experimental work on the involvement of electricity in nerve conduction was something of a continuation of Walsh's experiments on electric fish.[41] In 1797, one year before his death, Galvani published the results of a study carried out in 1795 on torpedoes caught off the Italian Adriatic coast, in which he showed that the fish cannot produce a shock after the sectioning of the nerves of the electric organ (Fig. 9). Moreover, we find reference to Walsh's (spelled «*Valsch*») researches in the notes to the lectures given at the University of Pavia by Lazzaro Spallanzani, one of the most important biologists of the 18th century.[42] Spallanzani also carried out experimental researches on the torpedo in the years 1782-1784, and he contributed to the interest in this field both in Italy and abroad after Walsh's studies.[43]

There are frequent mentions of Walsh's researches on electric fish in the work of Alessandro Volta, who, it will be recalled, derived important inspiration from electric fish studies for the invention of his electric battery, although he did not personally study the fish. Volta met Walsh in London in 1782 and from the account of his conversation with the English scientist we shall be deriving some important information on the experiments that Walsh carried out in 1775 on the electric eel.(VEN I, 8-12) Walsh's experiments on both the torpedo and the electric eel are also described in detail by Tiberius Cavallo in 'A Complete Treaty of Electricity', a reference book for the science of electricity in the 18th century, translated into several languages. References to Walsh appear in the fourth edition of Cavallo's book, published in London in 1795.

Mentions of Walsh's experiments on electric fish continued to be made in medical and physiological literature throughout the first half of the 19th

[38] The article on the torpedo published in the 'Philosophical Transactions' by Walsh in 1773 was translated to French by a member of the La Rochelle *Académie*, and published by J. B. Le Roy in 1774 in the *Observations sur la Physique*.
[39] See PRINGLE 1775 A and B, and PRINGLE 1776.
[40] LA ROCHE 1778.
[41] BRESADOLA 1997.
[42] *Università di Pavia, Dipartimento di Biologia Animale, Zoologia, Manoscritti. Fasc. E.*, pp. 225-229; See also SPALLANZANI 1944.
[43] SPALLANZANI 1783 and 1784.

century. Of particular interest is that they appear in two important physiology textbooks of the period, one by Johannes Müller and the other by Friedrich Tiedemann, published in Germany and widely circulated throughout Europe. These were source textbooks for generations of physiologists and medical students.[44]

In general, reference to Walsh's work concerned the experiments on the torpedo carried out in France in 1772, with the subsequent work of Hunter and Cavendish, and the experiments producing a visible spark from an electric eel that Walsh performed for the first time in 1775 in London. This last experiment was of special relevance because it provided decisive evidence on the electric nature of the fish shock, convincing even the most incredulous, for instance William Henly, whose ironical scepticism before seeing the eel spark has already been mentioned.[45]

However, with time the memory of Walsh's work on electric fish gradually declined, especially concerning the experiment of spark production from the eel. In his 1839 paper in 'Philosophical Transactions', Michael Faraday, who in 1838 had himself produced a spark from the discharge of the same fish, wrote that a similar achievement had been obtained before him by Guisan and Fahlberg[46], but he did not mention Walsh. On the other hand, in the section concerning animal electricity of his *Experimental Researches on Electricity - Third Series*, (first published in 1833 in the *Philosophical Transactions* and reprinted in the 1839 and 1849 edition of his collected electrical researches), Faraday said that according to Humboldt, a spark was obtained from an electric eel by «M. Fahlberg of Sweden [...] as Walsh and Ingenhousz had done before him in London by placing the gymnotus in the air, and interrupting the conducting chain by two gold leaves pasted upon glass, and a line distant from each other». But he adds:

> I cannot, however, find any record of such an observation by either Walsh or Ingenhousz, and do not know where to refer to that of M. Fahlberg. M. Humboldt could not himself perceive any luminous effect.[47]

Soon afterwards, Faraday mentions «the dissertation on the progress of mathematical and physical science» written by Sir John Leslie for the seventh edition of the *Encyclopedia Britannica*, published in 1830, in which reference is made to a demonstration in London on an electric fish in which «vivid sparks were drawn in a darkened room».[48] He comments, however, that Leslie «does not say he saw them himself, nor state who did see them; so that the statement is doubtful».

It is easy to understand that Michael Faraday, having succeeded in obtaining a visible spark from an electric eel in his experiment in London in 1838, was inclined to consider any unsupported account of similar achievements in previous experiments somewhat doubtful, particularly if performed in the same city, and more than half a century before.

We can easily imagine that Faraday was especially sceptical of the possibility that Walsh had carried out this experiment, because he had been

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[44] See MÜLLER 1849 and TIEDEMANN 1850.
[45] Letter of William Henly to William Canton (undated but probably written in November 1776), Canton papers of the Royal Society, Vol. II, p. 103.
[46] FAHLBERG 1801 and GUISAN 1819.
[47] FARADAY 1833, p. 47 (see also FARADAY 1839 and 1849 vol. I, Section 7, paragraph 358).
[48] See LESLIE 1835.

unable to find any trace of it in the ‘Philosophical Transactions’, the ideal publication place for a fellow of the Royal Society. We have seen that Walsh had indeed published his paper dealing with torpedo experiments in the journal of the Royal Society. There was thus no reason to suppose that Walsh would not leave any trace of the other experiment in the ‘Philosophical Transactions’, if he really had performed it.

Very likely Faraday could not directly access the work of Fahlberg (originally published in Swedish) or that of Guisan, published in Latin, because of his difficulty over finding the original publications and also because of linguistic obstacles. Had he read Guisan’s work (a doctoral dissertation presented by Ludwig Guisan to the University of Tubingen in 1819, dealing with the experiments made in Cayenna by his father, Samuel Guisan, in 1789) he would have probably gained little insight into Walsh’s achievement with the electric eel. Guisan quotes the article dealing with Walsh’s experiment on the eel, an article published in the *Observations sur la Physiques* in 1776,[49] but he tends to diminish the value of Walsh’s achievement (as well of that of Fahlberg and Ingenhousz who also obtained the spark from the eel). Guisan’s aim was to make it clear that in his father’s experiments the sparks could be seen as emanating from the body of the fish itself, while in the experiments of Walsh, Ingenhousz, and Fahlberg, the sparks were seen only in the separation made in the conductive circuit used to discharge the animal:

> *Fulgor apparet, cum fluidum emanat ex conductore in conductorem, quin, si piscis multum secernit, ex ipso corpore.*
> *Id posterius imprimis est memorabile, quod antea in nullo pisce electrico est observatum,* Walsh, Ingenhousz *nominatim et recentiori aetate* Fahlenberg *[sic] numquam igniculos ex gymnoto elicere poterant, quos, et quidem infirmos, non nisi fluido transmanante ext conductore in conductorem, obserbavant*.[50]

The expression «Walsh, Ingenhousz *nominatim et recentiori aetate* Fahlenberg *numquam igniculos ex gymnoto elicere poterant*» («*Walsh, Ingenhousz* and in a more recent epoch *Fahlenberg*, could never draw sparks from the gymnotus») might well have been particularly misleading for Faraday, if he had tried to read between the lines of Guisan’s work.

The real problem with Walsh’s experiment on the electric eel is that it was never published in written form by its author. Though news of Walsh’s achievement with the eel appeared in an article published in the *Observations sur la Physiques* in 1776, dealing entirely with Walsh’s experiment, it was not written by Walsh, but by Jean-Baptiste Le Roy, a member of the French *Académie Royale*, and one of Walsh’s acquaintances. Walsh met Le Roy in Paris during his journey in France in 1772, and they established a correspondence. As a matter of fact, Le Roy’s article was based on a letter he had received from Walsh, in which the English scientist informed his French colleague of the experiment he had carried out in the summer of 1775.

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[49] LE ROY 1776.

[50] «Light appears when the [electric] fluid emanates from one conductor to another, and even from the body itself, if the fish secretes much fluid. This last finding is particularly memorable, since it could not be observed before in any electric fish whatever; the already named *Walsh, Ingenhousz* and in a more recent epoch *Fahlenberg*, could never draw sparks from the gymnotus, and these they observed, certainly inconsistent, only when the fluid jumped from one conductor to another.» (GUISAN 1819 p. 29).

Walsh's original letter is not available, but fortunately Le Roy reports a few lines of it that are well worth reproducing here:

> C'est avec plaisir que je vous apprends qu'elles m'ont donné *une étincelle électrique*, perceptible dans son passage à travers une petite fente ou séparation pratiquée dans une feuille d'étain collée sur du verre. Ces poissons étoient dans l'air; car cette expérience n'a pas réussi dans l'eau; leur électricité est beaucoup plus forte que celle de la torpille, & il y a des différences considérable dans leur effets électriques.[51] (see Fig. 10 and Fig. 11)

This passage is of great historical interest because it represents the only published record of this memorable experiment written by its author. It is somewhat like the abstract of an work never to be published *in extenso*, as sometimes occurs even in modern times, though not usually concerning particularly memorable achievements. Although Walsh did not publish any paper concerning his experiment, he did make it public by showing it to various colleagues, most of them members of the Royal Society (including its President, John Pringle), in his London house during the summer of 1775, as Le Roy reported. We shall be seeing other reports concerning Walsh's experiments with the eel, some written by people that had actually been present. One of these is in a letter from William Henly, who after attending Walsh's experiment waxed enthusiastic, planning to determine the direction of the spark (i.e. the positive and negative pole of the mechanism underlying its production) by using an apparatus built for this purpose.[52]

Walsh very probably continued his demonstrations of the eel spark for some years afterwards, and some of the eels used for his experiments were kept alive for a long time. It was on these eels that Jan Ingenhousz[53] was able to make experiments in Walsh's house, on visiting London in 1778. An account of these experiments that Ingenhousz carried out with a Flemish colleague, «M. Beerenbroek»[54], is contained in the *Vermischte Schriften* that Ingenhousz published in 1782.[55] It is also referred to by another Dutch scientist, Jan van Swinden,[56] in a book on the relation between electricity and magnetism published in 1784. Walsh did not personally attend these experiments because of intervening duties, but he had a discussion with Ingenhousz from which it

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[51] «It is with pleasure that I inform you that they [i.e. the electric eels] have given me *an electric spark*, perceptible in its passage through a small gap or separation made in a tin lamina pasted on a glass. These fishes were in the air; since this experience has not succeeded in water; their electricity is very much stronger than that of the Torpedo, and there are some considerable differences in their electrical effects.» (Le Roy 1776; an extract of Le Roy's article was published in Italy in 1777; see PICCOLINO and BRESADOLA 2002).

[52] Letter of William Henly to William Canton (undated but probably written in November 1776), Canton papers of the Royal Society, Vol. II, p. 103.

[53] Jan Ingenhousz (1730-1799) Dutch natural philosopher and court physician to the Austrian Empress Maria Theresa is especially famous for his discovery of photosynthesis. In the last part of his life he settled in England.

[54] Very likely Arnould (or Arnold)-Barthélemi Beerenbroek (1751-1825), a physician and politician, born in Antwerp, who spent a long period in of his life in England. He became fellow of the Royal College of Surgeons of England and of the Royal Society of Medicine of Scotland. Beerenbroek translated from English to French, or to Latin, medical works of Percival Pott and of William Cullen.

[55] Ingenhousz had been interested in electric fish and in 1773 he did experiments on torpedo during a journey in Italy at Leghorn that were published in 1775 on the 'Philosophical Transactions' in the form of a Letter to John Pringle (see INGENHOUSZ 1775). He was in correspondence with Franklin, who in a letter to Ingenhousz, dated 18 March 1774, writes that he will send him (then in Vienna) one of Walsh's papers on torpedo «to save postage […] thro' the Hand of the Ambassador» (see FRANKLIN 1960- , vol. 21, p 148).

[56] Jan Hendrik van Swinden (1746-1823) physicist and astronomer, was particularly interested in magnetism and in its relation to electricity. He considered the '*magnetisme animal*' of Messmer to be completely devoid of a scientific basis.

will be possible to draw some significant information of his previous experiments with the eel, that is absent in Le Roy's 1776 article.

Together with that of Fahlberg and Walsh, Ingenhousz' account is one of the three reports concerning the production of a spark from an eel that Faraday mentions in his 'Experimental Researches on Electricity'. We have seen that Faraday could not trace any of these reports and considered them doubtful. Although this was understandable, it is also rather ironic because at the time the eel spark experiment produced an echo that went well beyond scientific literature. In 1777 it led to the publication in London of several pamphlets of a satirical, libertine character in the form of short poems.[57]

Besides Faraday's papers, another source of historical error concerning Walsh's achievement with the eel comes from 'A History of Neurophysiology in the 17th and 18th Centuries' published by Mary Brazier in 1984. In referring to Walsh and the production of a spark from the shock of an electric fish, she writes that «later it was claimed that he had demonstrated this with the Gymnotus». However, she adds, «a discourse on the history of the electric properties of the torpedo, delivered in 1775 to the Royal Society by the eminent Sir John Pringle, mentioned no sparks».[58] Pringle's speech, to which Brazier alludes, was delivered on awarding Walsh the Copley medal for his studies on the torpedo. Although printed in 1775, this speech was delivered to the Royal Society on «November 30th 1774», as clearly indicated in the printed copy (see Fig. 12), that and thus largely preceded Walsh's achievement with the eel.[59] Of course the lack of reference to events to come cannot be taken as negative evidence for their occurrence. Ironically, although mentioning Pringle's speech as «delivered in 1775» in the chapter dealing with electric fish, Brazier reports it as published in 1774 among the references listed at the end of the chapter.[60] It is possible that, besides inaccuracy due to inappropriate recourse to the original documents, Mary Brazier was led astray by reading Faraday's 'Experimental Researches on Electricity', that she undoubtedly consulted, because from it she drew a quotation for her chapter on electric fish.

There are objective difficulties for science historians in their search for old documents, so widely scattered as they are, in numberless libraries all over the world. However, particularly in the field of science history, one should not take for granted what others report without checking the original documents, because, as the scientist and historian David Keilin remarks, in the history of science errors tend usually to survive for much longer than in actual science.[61]

It is difficult to ascertain exactly how things went in the more than two centuries that separate Walsh's studies from the present, and what the relative contribution of Faraday or Brazier was to the matter, but certainly the memory of Walsh and his experiments on electric fish gradually diminished with time. This happened despite the importance and the success of these experiments, sanctioned, as we have seen, by the award to Walsh of the Copley Medal for the year 1774, and by their wide circulation at the end of the 18th century. Walsh is not listed in Gillispie's 'Dictionary of Scientific Biography', nor in recent editions of the *Encyclopedia Britannica*, nor in other scientific

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[57] PERRY 1777 A, B and C.
[58] Brazier 1984, p. 200.
[59] PRINGLE 1775 a (the French translation was published in the same year, while the Italian translation appeared in 1776).
[60] BRAZIER cit., p. 204.
[61] KEILIN 1966.

biographies. Scanty information about him can be found in the English 'National Biographies' and in the 'House of Commons', but this deals to a great extent with Walsh's political activities, especially the projects for the administration of the India of Lord Clive. Clive was his protector and sponsor, for he had been born in India and had been secretary to Clive during his early career in Bengal.

MAKING PUBLIC EXPERIMENTS IN 18TH CENTURY SCIENCE

At least in part, Walsh's decision not to publish the results of his experiments on the electric eel in a written form is responsible for the difficulties encountered by later scientists and historians in finding an unequivocal record of his experiment of spark production from the fish shock, and this may have contributed to the oblivion of this experiment and its author. We do not know why Walsh did not personally write a paper on his electric eel experiments. There are various possible reasons, difficult to ascertain. One is that he postponed the paper to a period in which he could obtain more results on this electric fish or have more convincing explanations for the observations made on the eel, and on the possible differences from the torpedo alluded to in his communication to Le Roy. The project might not have reached a conclusion because of political or private duties of various kinds.[62] Among other things, besides being interested in performing his electric experiments and in fulfilling his political duties, Walsh was actively engaged in private business, particularly in acquiring landed property and very likely in exploiting it, as was commonly the custom with affluent members of the upper classes during the Enlightenment. We have seen how Walsh's manuscript on the journey to France is indeed rich in notes concerning the way the land was looked after and exploited in the regions visited.

It is possible, on the other hand, that Walsh was personally fully satisfied by having definitely proved, through the experiment of spark production, the electric nature of the eel's shock, and the private demonstration of the experiment given to his colleagues and visitors was enough for him. The necessity of having authoritative witnesses for scientific achievements was a characteristic of experimental science in the 18th century, particularly in the case of surprising results that seemed to contradict current scientific notions.

To this characteristic Walsh alluded, at the time of his experiments on the torpedo, in both the journal of his experiments and in the published paper. In the last period spent at La Rochelle, Walsh gave an «exhibition of the electric power of the Torpedo, before the Academy of La Rochelle» and, afterwards, on the two succeeding days, he repeated his experiments «before numerous companies of the principal inhabitants of la Rochelle». In the paper published in 1773 Walsh comments on those performances by saying that one of the reasons for doing them was certainly the wish «to give all possible notoriety to facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science». As already noted, Walsh was well aware of the difficulty of assigning an electric nature to the torpedo's shock within the current conceptions of electric science. «Even the Electrician – he wrote – might not readily listen to assertions, which seemed, in some respects, to combat the

[62] In the years considered, Walsh was encountering many politically difficulties particularly in his attempt to be re-elected to the Parliament, the matter costing him the investment of considerable sums which led to a substantial loss of his fortune.

general principles of electricity». As to the experiments dealing with the «phenomena of the Torpedo», for which «Great authorities had given a sanction to other solutions», different from the electrical explanation, it was therefore necessary to have authoritative witnesses, because of its novelty and unexpectedness. Very probably, a similar principle guided Walsh when, three years later, he was able to produce the spark from the eel's shock.

In modern times the ability to reproduce scientific results in a relatively easy way in different laboratories limits the necessity of direct personal witnesses, and makes it necessary only to publish the experiments accurately in a written form, with appropriate specification concerning the methods, the type of preparation, and the materials used. Walsh was probably so gratified by the demonstration of the eel's spark given to his authoritative colleagues and visitors in his London house, that he deemed it unnecessary to write a detailed paper on these experiments. It can be conceived, moreover, that the rapid and wide diffusion of the news of his achievement, prompted mainly by Le Roy's article, contributed significantly to his not writing a paper himself.

This is probably true, but may not be all the truth. It is possible that the decision not to publish the results reflected the attitude of a time in which science was not yet a professional activity. Henry Cavendish, one of the greatest scientists of the 18th century, did not publish most of his experiments on electricity, some of which were of great importance (among his unpublished electrical researches there was the first measurement of the electric force, an achievement that anticipated Coulomb's studies, and an accurate determination of the electrical resistance and capacitance of several bodies). Fortunately, having collaborated with Walsh in his electric fish researches, in the 'Philosophical Transactions' of 1776 Cavendish did publish his experiments dealing with the mechanism of the torpedo's shock. He did this after having first demonstrated his experiments with his 'artificial torpedo' to a few colleagues (Joseph Priestley, Timothy Lane, Thomas Ronayne, Edward Nairne and John Hunter). This device, made of a wooden or leather model, was capable of imitating the effects of the natural fish, when powered by a large number of Leyden jars, connected in parallel, and charged to a low degree. In particular, it produced a shock when immersed in water, as his eminent colleagues could experience during their visit to Cavendish's laboratory.

As already noted, in this particular case, Cavendish did not content himself with this way of making his experiments public. This was a fortunate choice, because his work on the torpedo opened up paths to the development of an important notion in electrical science. On the other hand, most of Cavendish's other electrical researches did not appear in written form in his time, and were thus largely lost for the advance of science, though rediscovered and published about a century later by James Clerk Maxwell; they were thus recovered at least from the point of view of science history.[63]

The case of Cavendish, as well as the case of Walsh with his experiments on the eel, makes it clear how scientific endeavour was in a transition phase in the second half of the 18th century. Although an experimental science of a modern kind was emerging, scientists might still content themselves with obtaining certain specific results and showing them to a select group of colleagues, within the restricted boundaries of a private *cabinet de physique*: an attitude completely different from that dominating modern science with its need to publish scientific results immediately in a written form, so they may

[63] MAXWELL 1879.

reach the widest possible audience as quickly as possible. From this point of view, the science of Walsh (and Cavendish) still had the flavour of a 'literary' entertainment for the upper classes of the *ancient régime*.

Despite the fact that, with the diffusion of electronic communication, written documents may lose their exclusive character as a form of diffusion and transmission of science and culture in the near future, they have been of great importance for centuries, and still are, because they can usually be read and interpreted rather easily, even long after being produced (a characteristic not granted to electronic documents). Moreover, if they happen to survive the effects of time, written documents can be retrieved and made public, through the work of the historian, even if they were initially conceived for private memory or use, as was largely the case with the two manuscripts of John Walsh we are dealing with, both the one concerning the journey to France, and the other describing the experiments on the torpedo at La Rochelle and at the Isle de Ré. To our knowledge these manuscripts have never been studied by scientists or historians up to modern times.[64]

We will now attempt to describe John Walsh's scientific work on electric fish, starting from these manuscripts, in order to illustrate an exciting phase of 18th century science, that marked the progress of both physiology and physics in a significant way. We hope that our work will help to retrieve from the 'darkness' of history, one John Walsh, *Esq*., a 'natural philosopher' of the Enlightenment, largely neglected by scientists and historians in spite of the importance of his achievements.

WALSH IN FRANCE: IN QUEST OF TORPEDOES

Although, as we have shown at length in the first part of this book, John Walsh's first manuscript, i.e. the one concerning his visit to France, is largely of a non-scientific character, nevertheless in some passages it does reveal, directly or indirectly, the real purpose of the visit, namely the study of the mechanism of the torpedo's shock.

Soon after his arrival in France, at Calais, Walsh visited the local fish market showing the keenest of interest and, in the absence of torpedoes, he took advantage of the arrival of a boat unloading «6 or 700 Rayes of different sort», to carry out a dissection of some of these fish. This appears to be an anticipation of (and a preparation for) the dissection of torpedoes he will carry out at La Rochelle. About a week later, on the way from «Boulogne to Montreuil», we find the first remark betraying Walsh's interest in the torpedo:

> Spoke with Captain Palletti [probably an incorrect spelling of 'Paoletti'], and Captain Lobé, who commanded Coasting Vessels to La Rochelle and Bordeaux &c. They were acquainted with the Torpedo, and assured me that to their one knowledge, there were plenty of them at La Rochelle and La Tesle on the coast of Argenson. (WJ, 8 R.H.).

[64] As said in the foreword, the manuscript was donated to the Royal Society in 1965 by Lord John Arthur Walsh, last Baron Ormathwaite. The fact that the manuscript is conserved at the Royal Society only since 1965 explains why it escaped the accurate search of the documents and books conserved at the Royal Society made by Cameron Walker who, in 1937, published an article on 'Animal electricity before Galvani'. It might that since 1937 no other historian interested in the 18th century investigations on animal electricity has consulted the catalogue at the library of the Royal Society. It must be said, however, that Walsh's manuscript is not listed in 'Book catalogue of the Royal Society', edited by A. J. Clark and published in 1982 by the University Publications of America.

Afterwards, in the pages describing the itinerary to Paris, through Boulogne, Montreuil, Amiens, Breteuil, Clermont, Creil and Chantilly, the journal pursues its usual observations on the country landscape, the conditions of roads and of inns, the way the land was exploited and maintained, farm breeding, persons met with at the coach or horse posts, historical sites, monuments and villages visited, without any reference to torpedoes or to science in general. However, things change upon his arrival in Paris, at the «Hotel du Luxembourg, rue des petits Augustins», on June 15th. On the morning of the next day, Walsh visited Jean-Baptiste Le Roy, who he found engaged in electrical experiments, and discussed laboratory instruments with him. In the evening Walsh was at the *Jardin du Roy*, where he attended a lecture in chemistry and visited a naturalistic collection at the *Cabinet du Roy*:

> a good collection of natural Curiosities, open to all three times a Week. Military mounted there, in Glass Cases, with Label of the name of each Article. (WJ, 16-17 L.H.).

Of the various 'articles' on exhibition, his curiosity was first attracted by a «Male Torpedo, as called, dryed [*sic*] small about 9 Inches»., and, moreover, by «two very small ones in Spirits mention'd from the Isle de Bourbon, two inches in Diameter».

«No Anguille tremblante» - he writes, very probably with some intention to deceive - after having noticed the absence of specimens of the electric eel. Afterwards he noted in the journal:

> Visited M. Jussien [i.e. '*Jussieu*']; talked of the Torpedo, he never saw it or heard much of it; mentions his having heard that the Loadstone took off its Effects. One present mentioned the Eel to be in Salt water at Mauritius. M. Jussien named M. A Surgeon now dead to have collected some particulars of the Torpedo, and to have intended to have refuted M. De Reaumur. (WJ, 18).[65]

We remark here, *en passant*, that in alluding to the power of loadstone to take away the effects of the torpedo, Jussieu was referring to the curious results obtained by the Dutch physician, Godfried Wilhelm Schilling,[66] who in 1770 had reported that a loadstone attracted the electric eel, as if it were a piece of metal, and moreover, deprived it of the power of producing a shock. Jussieu uses the word 'torpedo' instead of '*Anguille tremblante*' when talking about Schilling's experiments. This indicates that the source of his information was Schilling's 1770 essay '*De torpedine pisce*', where the Latin term *torpedo* was

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[65] The Jussieu that Walsh visited at the *Jardin du Roy* was one of the members of an important dynasty of naturalists many of whom had positions at the *Jardin* and gave significant contributions particularly in the field of botanic. The family included the three brothers, Antoine (1686-1758, the founder of the dynasty named professor at the *Jardin* at the age of 23), Bernard (1699-1777) and Joseph (1704-1771), their nephew Antoine Laurent (1748-1836) who completed the new system of plant classification developed by Bernard in opposition to Linneus, and finally Adrien (1797-1833). It is very likely that the Jussieu visited by Walsh at the *Jardin* (and met two days later at the *Académie*) was Bernard, at the epoch of the most eminent member of the *Académie* (Joseph was only associate member of the Institution, and Antoine Laurent had not still been elected). Bernard was *Demonstrateur exterieur des plantes* at the *Jardin*, a position in which he was succeeded just in 1772 by Antoine Laurent, who in 1775 also became *Professeur*.

[66] Schilling (1725-1799) lived for a long time in Paramaribo, capital of the Surinam, in the Dutch Guyana. His interests deal principally with infectious diseases.. The work where he reports his observations on the electric eel is mainly concerned with the description of a tropical disease ('yaws') amply diffused in the Guyana and transmissible through venereal contact.

used generically as the term for all electric fish species. Any uncertainty about the nature of the fish studied by Schilling is removed by the title of the Memoir that he presented in the same year to the *Académie* of Berlin, and that appeared in printed form two years later: *Sur le phénomènes de l'Anguille tremblante*.[67] By the way, Schilling's results (obtained exclusively on the electric eel) were contradicted by the experiments carried out by Ingenhousz on Walsh's electric eels during his visit to London in 1778.[68] Moreover, they were also inconsistent with the experiments carried out on true torpedoes of the Mediterranean sea and published in 1783 by Lazzaro Spallanzani. In no case were these electric fish attracted by magnetic bodies, nor were they deprived of their power to produce a shock.

Coming back to Walsh in Paris, on the day after his visit to the *Jardin du Roy*, he endeavoured to visit several scientists, in particular the naturalist and botanist Michel Adanson, the same Adanson that 15 years before had reported on the similarity of the shock produced by the Leyden jar and the *trembleur* fish of Senegal. Adanson was evidently out, as Walsh writes in the journal: saw «only Madame». (WJ, 18 L.H.).

There are no further mentions of events or discussions concerning electric fish in the following days that Walsh spent in Paris, up to June 21st. The period was, nevertheless, occupied with various activities of a scientific character, including, on June 17th, attendance at a meeting of the *Académie Royale* (with «about 30 Members – as he notes – including D'Alembert, Cassini, D'Aubenton, Jussien (*sic*), De La Condamine, Macquer, Chev.[r] D'Arcy, Le Roy, De Lalande, Gentil, Bailly, Sage, Adanson, Brisson &c.»(WJ, 19 L.H.).[69] Besides the *Académie*, Walsh visited the Astronomical Observatory (headed by Cassini), the private laboratory of Le Roy and the *Cabinet* of the *Duc de Chaulnes*, with his collections, notably his «Egyptian collection» and his «small but good Collection of Instruments for Experime.[ts] in Physick». (WJ, 22).[70]

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[67] See SCHILLING 1770 A and B.

[68] See INGENHOUSZ 1782.

[69] Besides the well known mathematician and philosopher Jean Le Rond D'Alembert (1717-1783), and the aforementioned Jussieu, Charle Marie de La Condamine, Jean-Baptiste Le Roy and Michel Adanson, the other *Académiciens* listed by Walsh are: the astronomer César-François Cassini (very likely Cassini de Thury, 1714-1784); the naturalist and anatomist Louis-Jean-Marie D'Aubenton (1716-1800); the mathematician and physicist Patrick D'Arcy (1725-1779); the mathematician and astronomer Joseph-Jérôme Lefrançais de Lalande (1732-1807), the astronomer Guillaume Joseph Hyacinthe Jean-Baptiste Le Gentil de la Galaisière (1725-1792); the chemist Pierre-Joseph Macquer (1718-1784); the astronomer Jean-Sylvan Bailly (1736-1793), the chemist Balthazar-Georges Sage (1740-1824), the physicist and naturalist Jacques-Mathurin Brisson (1723-1806). Le Roy collaborated with D'Arcy in the construction of an electrometer, and, moreover, he tried medical electricity therapy with both D'Arcy and La Condamine (see *Mèmoires de l'Académie Royale des Sciences* for 1749 and 1755). During the days spent in Paris Walsh met (or tried to visit) other persons, besides those already listed (and the *Duc de Chaulne*). Here is the complete list of other persons visited whose names are annotated in the 'journal de voyage': M. Gobelin, Sir John Lambert, Lord Harcourt, Colonel Blaquier, Lord Camden, M. Rouelle, M. Bougainville, M. Nerville, D'Anville, Jefferys, Ormes, Trudaine.

[70] The *cabinet* of the Duc de Chaulnes was famous in the Europe of the Enlightenment for the richness of his physical instruments, for its naturalistic collections and for its antiquities (which included objects from Greece, Etruria, Egypt, China, old bronzes). It had been established by Michel Ferdinand d'Albert d'Ally, Duc de Chaulnes (1714-1769) *Pair de b France, Lieutenant général* of the King's Army, Governor of Picardy, who had a great interest for physics and natural sciences (he was elected honorary member of the *Académie Royale des Sciences* in 1743). His son, Marie Joseph Louis d'Albert d'Ally (1741-1793), the *Duc* evidently visited by Walsh in 1772, was a traveller interested to natural sciences and to antiquities, particularly of Egypt. He was famous for his experiments on the reanimating effects of ammonia vapours, that eventually he tried on himself, happily with a good success.

As might be expected, Walsh's interest lay in electrical instruments, and he was especially attracted by the electrical machine, with its large «Platteau of better than three feet diameter [...] the form entirely Ramsden's», that he describes with accuracy, and with an appreciation that contrasts with the low opinion he usually had of electrical instruments seen in France. (WJ, 23 L.H.)

In the Duc's *Cabinet*, Walsh also expressed his appreciation of the «Apparatus for flying an Electrical Kite likewise well contrived». That apparatus, however, could not be used for «knowing the Electricity of the Atmosphere» - as he notes - «for want of regular wind». (WJ, 24)

Annotations of a scientific character or connected to science or scientists, in one way or another, occupy the pages on the period spent in Paris almost entirely. The only extra-scientific observation, on June 18, concerns «the Fête de Dieu, from the Church of S.t Genevieve, 8 Bishops walked». As Walsh remarks, «This is the solemnest day of the Year». (WJ, 20-21) There is nothing else, in the journal of his days in Paris, on monuments, museums, churches, or other artistic or historical sites or objects, nor on cultural or political events of any kind (something which is indeed difficult to figure out for a visitor of the Paris of the Enlightenment!)

On June 21st, his last day in the French capital, Walsh wrote a letter to Franklin. He informed his correspondent «to have been at two Meetings of the Academie des Sciences and have had a polite and obliging reception from the Academiciens in general». He goes on:

> I have made no secret of my intentions of prosecuting Experiments on the Torpedo, to which Animal, bye the bye, they are almost strangers here; Brisson only knew that they exist & in tolerable plenty at La Rochelle, in the neighbouring of which Reaumur had a Terre. But I have no occasion for academical information on this point, having met at Boulogne with two plain sensible Captains of Coasters to La Rochelle & Bordeaux, who put me out of doubt of their exceeding plenty at this Season, in the neighbourhood of those places. Whether their Effect be Electrical or not, I persuade myself will be soon obtained. (WJ, 26 R.H.).[71]

In the rest of the letter Walsh portrayed the situation of Electricity in France as being at a «low ebb», though he described the instruments seen in the *Cabinet* of the *Duc de Chaulne* in some detail. He concludes by saying:

> My next will convey to you the event of our Experiments on the Torpedo.

Within about three weeks, Walsh will be able to maintain his commitment to Franklin in a way that will mark a milestone in the «walk of physics» and of «animal oeconomy».

As already mentioned, the decisive experiments that Walsh carried out once he had arrived at La Rochelle, on which his next letter to Franklin was based, are described in a second, separate, manuscript. As to the pages of the diary concerning the journey from Paris to the small town of the Atlantic coast, apart from their usual observations on the country landscape, the characteristics of the places and monuments visited, the inns and the food, they are noteworthy for two reasons. The first is that sometimes Walsh writes his

[71] This letter is reproduced entirely in 'The papers of Benjamin Franklin', vol. XIX, pp. 189-190 (see FRANKLIN 1960-).

notations in French, as if the need to communicate with local people, once he had arrived at his destination, drove him to practice a language that he knows quite well, but that he seems not to have mastered entirely, if one is to judge from the presence of some spelling and grammatical errors in the manuscript.

The other reason of interest is that, after the phrase concerning his arrival on June 26th («At 1 P.M. arrived at La Rochelle»), and before the indication «See the Diary of Transactions at la Rochelle & the Isle de Ré, From June 26th to July 27th», we find three pages of a «Memorandum concerning the Torpedo, made in the Journey from Paris to La Rochelle».(WJ, 53-55 L.H.). This suggests that, as he approached the real destination of his journey, the place where he hoped to obtain an answer to the scientific question that was the focus of his interest, Walsh was trying to make every effort to be fully prepared for his endeavour in order to be able to profit thoroughly from the experimental period.

Before rapidly surveying these pages, it may be worth noting that similar Memoranda were typical of that period, and later, in particular for experiments and observations to be carried out in the course of a journey far from one's domestic laboratory. In the context of Walsh's studies on electric fish it is perhaps appropriate to refer here that an interesting series of annotations concerning experiments on the torpedo was prepared by Lazzaro Spallanzani before his journey from Pavia to Portovenere, on the Italian Riviera near La Spezia. Interestingly, among the indications on the instruments to take, and on the experiments to be carried out, Spallanzani transcribes in his manuscripts passages of Walsh's first article on torpedo (or *Walsch* as he sometimes spells the name), derived from the French version translated by Le Roy.[72]

Coming back to Walsh and his Memorandum, its interest lies in both the experimental programme outlined (on anatomical and physiological investigations), and in his reference to previous authors from which he evidently drew his information concerning the torpedo. He lists Redi, Lorenzini, Kaempfer and Jacobaeus, but *en passant* he refers also to Steno.[73]

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[72] *Biblioteca 'A. Panizzi' Reggio Emilia (Italy) Mss. Reggiani 52*; See also Spallanzani 1935, pp. 121-126. Besides the translation publisehd by Le Roy there exists another unpublished French translation of the 1773 Walsh'a article prepared by a member of the *Académie Royal de La Rochelle 'M. de Villamarais'* upon request of the Académie President Pierre-Henry Seignette (TORLAIS 1959, and see later). The manuscript of this last translation is available at the *Médiathèque Michel Crépeau* of La Rochelle (Ms 313, fol. 52-78).

[73] The reference works implicitly alluded to by Walsh are evidently REDI 1671; LORENZINI 1678; KAEMPFER 1712; JACOBAEUS 1666. Of some of these books Walsh could have consulted either the originals or the English, French or Latin translations. Oligerus Jacobaeus (Holger Jacobsen 1650-1701) a Danish anatomist published his main work, *De Ranis et Lacertis observationes* first in Paris in 1676 and afterwards in Copenhagen in 1686. He made his observations on the torpedo in Italy during a sojourn spent at the court of the Grand Duke of Tuscany, where anatomy and natural sciences were greatly encouraged within the climate of the Galilean scientific revolution. As a matter of fact, starting from about 1656, the torpedo has been an object of attention and investigation for scientists of the Grand Duke court (during the sojourns of the court in Pisa), such as Giovanni Alfonso Borelli, Marcello Malpighi, John Finch, Francesco Redi, Nicolas Steno, Lorenzo Bellini, Stefano Lorenzini (see GUERRINI 1998). As for Steno, almost certainly Walsh referred to the '*Ova viviparum spectantes Observationes*' (see STENO 1675), in which the anatomy of one torpedo is described (very likely the same fish as that studied by Redi, see Guerrini 1999). Steno (Niels Steensen 1638-1687) of Danish origin, was one of the greatest scientists of the 17th century. Besides for his fundamental anatomical discoveries, he is known for his contributions to geology, palaeontology, crystallography. In the Memorandum Walsh mixes English with (a slightly incorrect) French as, for example, when, in reporting annotations from Steno concerning the organisation of the fibres in the Torpedo's organ, he writes «Two kinds of Fibres one of thicker substance and longer orifices, as big as a Goose-quill, soft and white, perpendicular between Back and Belly, joined by transverse Fibres and transverse Nerves therefore called by Stenon the movers, Fibres motrices. at their extremities are fasten'd de petits globules ou de petits vesicules transparentes de la meme substance et couleur que les Fibres. Il y a apparence que ses vesicules tiennent bien de Glandes et que comme elles recoivent des vaisseaux sanguins, elles servent a séparer et a filtrer une humeur onctuese».

During his experiments on the torpedo, Walsh was able to disprove one of the observations reported in the *Amoenitatum exoticarum* published in 1712 by Engelbert Kaempfer (and endorsed also by Linneus in his *Systema Naturae*): the shock of the torpedo is no longer felt if one holds one's breath after deep inspiration at the moment of touching the fish. In his 1773 article on the torpedo,[74] Walsh will allude to this apparently mistaken observation, at the same time praising «the accurate Kaempfer, who so well describes the effect of the Torpedo, and happily compares it with lightning».

Among the experiments Walsh planned in his Memorandum, of particular interest are those concerning the physiological experiments:

> Try it with every Conductor and non-conductor you can think of.
> Try as great distance as you can. (WJ, 53 L.H.).

After about two weeks of intense experimental work, carried out with the help of his nephew, Arthur Fowke, and of Mr. Davies,[75] Walsh will be able to obtain fully convincing results on the differential transmission of the torpedo's shock by conductors and non-conductors, and can thus boldly announce, first to the selected company at La Rochelle, and a few days later to Benjamin Franklin:

> the Effect of the Torpedo appears to be absolutely Electrical (WE, 71 L.H.). (see Fig. 5)

Before entering into the heart of the argument of Walsh's experiments at La Rochelle, there are two other relevant pieces of information in the *journal de voyage*. In Paris during a visit to Le Roy's laboratory, Walsh writes: «[they] know nothing of the Henley's Electrometer and we forgot to bring ours».(WJ, 16 L.H.) In the page concerning the departure from La Rochelle, on July 28th, Walsh writes: «Left La Rochelle at 8 o'clock in the morning, having presented our Electrical Machine, Acon, and all our Apparatus to M. Saunier». (WJ, 57 L.H.) Very probably Walsh bought the electric machine from M. De La Condamine during the days spent in Paris, even though he does not make any mention of it in the page of the manuscript concerning his period in Paris. We find mention of this electric machine on page 30 of the journal of experiments:

> Experiments in artificial Electricity with the Machine bought of M. De la Condamine. That is, a Platteau, with Lane's Electrometer fitted to the Prime Conductor:

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[74] Engelbert Kaempfer (1651-1715), physician, naturalist and voyager made his observations on torpedoes of the Persian gulf during a long journey, as member of a Swedish embassy, which brought him as far as to Nagasaki in Japan. Although not confirmed by Walsh, his observation on the effects of holding one's breath on undergoing a torpedo shock might perhaps be accounted for by the changes of skin electrical resistance that accompany respiratory movements.

[75] Arthur Fowke (Fl. 1756-1775), was the son of Elizabeth, John Walsh's sister, and of Joseph Fowke. Walsh, who did not marry, took charge of the education of Elizabeth's three children (two boys, Francis and Arthur, and a girl, Margaret) after his sister's death. Although the event is not mentioned in the diary of Walsh's journey to France, after the season of the experiments at La Rochelle, Arthur decided to remain to France for a period, enrolling himself in the famous *Académie d'équitation* of Angers. He came back to England at the beginning of 1773. He died few months after going to India in the service of the East India Company with his father. Mr. (David) Davies was tutor to Arthur and secretary to Walsh. Francis Fowke, Arthur's brother, was disinherited by his uncle because of his moral conduct, and Walsh's fortune passed to Walsh's niece, Margaret In accordance with John Walsh's will the husband of Margaret, Sir John Benn, assumed the additional name of Walsh, and his son, Sir John Benn Walsh, was made first Baron Ormathwaite. The name Arthur recurred in the lineage of the Ormathwaite barony. By a singular circumstance, the 6th (and last) baron of the family, deceased in 1984, Sir John Arthur Ormathwaite (the one who donated the 'journal of experiments' to the Royal Society) bore the names of both his ancestors involved in torpedo's experiments in France.

We don’t have much information about the rest of the «Apparatus» to which Walsh alludes. However, besides instruments for dissections, metallic wires and other materials of various kinds necessary to ascertain whether the fish shock could be transmitted selectively through electrically conductive materials, there was a Leyden jar and a flat capacitor of Franklin’s ‘magic square’ type . As we shall see, Walsh will be using the Leyden jar on various occasions at La Rochelle and at the Isle de Ré, to compare the effects of artificial electricity with those of the torpedo, starting from the first days upon his arrival, before he can obtain live torpedoes. Walsh will have recourse to the flat capacitor in a more advanced phase of his investigation at La Rochelle in order to compare the sensation perceived during the torpedo’s shock, with those produced by the discharge of the artificial device.

The «Acon» donated to Saunier with the rest of the apparatus was not an electrical device, but a flat-bottomed boat used by fishermen to load and unload big ships. Walsh used the Acon to transport live torpedoes after they had been fished, and also for keeping them alive ashore.

II PART

LA ROCHELLE AND THE ISLE DE RÉ: A DAY BY DAY CHRONICLE OF AN INTENSE SEASON OF EXPERIMENTS

WALSH'S RESEARCHES AT LA ROCHELLE IN THE 'JOURNAL OF THE EXPERIMENTS'

In describing the investigations that Walsh and his collaborators carried out at La Rochelle and in the Isle de Ré in June-July 1772, we will try to follow the chronological order of the experimental work as Walsh described it in his 'journal of experiments', though we will sometimes go beyond this to compare the passages of the journal with other writings, in order to have a more complete vision of Walsh's experimental methodology and scientific attitude.

Through the day-by-record record of experimental work we hope to obtain insight into Walsh's experimental approach, into his methods, the difficulties he encountered in his research and the way he attempted to tackle them.

We are well aware of the fact that the chronological record of experiments in a laboratory journal does not necessarily tell us 'all the story' of the progress of the investigation, and in some respects may not even be a completely faithful record of the events.[76]

There are things that the scientist does not usually set down in his laboratory notes for objective reasons, such as want of time and the need to avoid interfering with the experiment in progress, or because he does not believe them important at the time, or because of the objective difficulty to set them out in written form. This may be the case, for instance, with the logical development of his working hypothesis and the intimate process of his mental elaboration.

On the other hand, the journal of the experiments may provide information about the scientific work that is complementary to what can be gleaned from the published papers. Since publications are intended for communication purposes, they usually aim at convincing the reader of the relevance and soundness of the results obtained, and they thus tend to give an excessively clear and logical picture of the research, and to trace a more linear progression of its development, placing the difficulties and uncertainties rather in the background. Compared to other examples of laboratory notebooks of the same period (as for instance those of Cavendish, Lavoisier, Galvani, or Volta - to mention four 18th century scientists involved in somewhat related researches),[77] Walsh's 'journal of the experiments' is characterised, on the one hand, by the relative paucity of explicit reflections of a logical or methodological character, and on the other hand, by the presence of frequent references to events related only indirectly to the scientific activity (for instance the problems encountered in obtaining the fish or keeping them alive) or to events of everyday life at La Rochelle (concerning for instance the hotel where Walsh was staying, the people he met, the dinners he went to). These last aspects reflected the fact that Walsh was registering experiments carried out on a special occasion, that of the journey, an occasion that was potentially rich in events of various kinds, worth recording, at least from his own personal point of view. Walsh's journal somewhat resembles the hand-written

[76] For a discussion on laboratory notebooks see HOLMES 1990.

[77] As mentioned, Cavendish's laboratory notebooks have been published by Maxwell (see MAXWELL 1879). For Lavoisier's laboratory notebooks see Holmes 1985; Galvani's notebooks have been partially published in 1937 (see also PICCOLINO and BRESADOLA, 2003); Volta's laboratory notebooks have been published in the monumental '*Edizione Nazionale*" of Volta's works.

notebooks that Spallanzani kept during his journeys as a naturalist to the Adriatic or Tyrrhenian coast of Italy where, among other things, he also investigated the shock of the torpedo. [78]

In the case of Walsh, the general paucity of explicit remarks concerning the logical or methodological aspects of the work depended, at least in part, on the very intensive character of the investigation, concentrated, at it was, into a specific and relatively exceptional period (because it required a sojourn in a different country and because of the temporal problems concerning the availability of fish). On several occasions, the rhythm of the experiments was so intense that there was physically too little time to indulge in comments or annotations, other than the simple record of the experimental work in progress.

From the analysis of Walsh's journal of experiments, besides some specific, personal features of his experimental attitude, other features will emerge that characterise the scientific endeavour of an 18th century 'natural philosopher'. First, those connected to the transitional character of the experimental science of this period. Moreover, we will discover, in Walsh's scientific attitude, an effective exchange between a perspective that, in present day usage, we call 'physiological', and a 'physical' perspective. A similar approach characterised some of the most proficient science of the 18th century, when the limits between different sectors of science were ill defined, and the passage from physics to chemistry and the life sciences was the rule rather than the exception in experimental research, as can be seen from the work of many illustrious people. One of these is Joseph Black, who, starting from a medical investigation into *magnesia alba* to identify a way to dissolve kidney stones, made some fundamental discoveries on the mechanism of transformation of carbonates into hydroxides. Together with the results of the research on 'airs' (gases) of Cavendish, Priestley, Scheele, and others, these discoveries opened the way to the chemical revolution of Lavoisier. Lavoisier himself, within his chemical researches on the mechanism of combustion, eventually studied the gas exchange in lungs, and discovered the fundamental processes of pulmonary respiration. Stephen Hales, the first to measure blood pressure, was interested in botany, and was one of the pioneers of the chemistry of gas (he discovered 'fixed air', i.e. carbon dioxide).

A somewhat analogous path of discovery eventually culminated in the chemical revolution of Lavoisier and the discovery of photosynthesis. As a matter of fact, a fundamental cue to the nature and diversity of gases present in the air and/or released or absorbed in chemical reactions came from the effects they produced on living beings, and thus involved a type of experiment that was both chemical and physical in character and of a biological kind. Some gases supported life, other destroyed it, and, moreover, plants could regenerate air after that the 'vivifying principle' had been exhausted by combustion or by the presence of animals, a regeneration process that, however, occurred only in the presence of light. The chemical revolution and the discovery of the most fundamental process allowing for the emergence of the most complex forms of animal life were thus historically connected, due to the close relations that existed in the 18th century between biological and non-biological science.

The difficulty over separating different domains in the science of the 18th century is evident also in the work of Lazzaro Spallanzani, celebrated for his fundamental studies on the mechanism of reproduction and regeneration, for his researches on digestion and respiration and on the special sense of bats.

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[78] SPALLANZANI 1935.

Although essentially involved in biological investigations, Spallanzani also made important contributions to the fields of chemistry, mineralogy, geology, oceanography and vulcanology.

Henry Cavendish, frequently mentioned in this book, celebrated for the first experimental determination of gravitational constant and for the identification of the composition of water, contributed, as already mentioned, to Walsh's researches on electrical fish. Thomas Young, a medical graduate, contributed to clarifying the mechanism of accommodation of the eye and formulated the trichromatic theory of vision. He was also the first to provide evidence for the undulatory nature of light (and incidentally helped to decipher hieroglyphs).

An effective interplay between physics and physiology, characterised the researches of Luigi Galvani and Alessandro Volta, the two Italian scientists that most contributed to the development of the research field opened up by Walsh with his studies on electric fish. We will discuss in detail the physical and physiological disposition in Volta's electric researches in Part IV of this book.

Another aspect of the interaction between physical and physiological science that characterises Walsh's investigation, and those of many other scientists of the age, concerns the importance of sensory perception as a measure of the events studied, in the absence of other sensitive and adequate methods for detecting and quantifying them. Walsh quantifies the shock of the torpedo by trying to establish that it is felt only by the fingers, or that it arrives up to the elbow or even, in rare circumstances, that it can also affect the shoulders. Moreover, on the basis of a purely sensory estimation, Walsh will give a relative measure of the intensity of the shock produced by the torpedo in water compared to air, as, for instance, he annotates in «the Observation verbatim» written in La Rochelle on July 26th 1772. Here it is mentioned that the shock in water was:

> greatly weaker than in air, perhaps not a quart part of the strength, at least to our sense of it. (WE, 170)

Given the apparently crude and subjective character of these estimations of laboratory events, one may be astonished by the importance and precision of the results obtained by the scientists of the *ancient régime*. Using a method introduced by the famous Italian electrician Giovan Battista Beccaria, Henry Cavendish succeeded in estimating, with impressive precision, the relative electric resistance of water, showing that it was much smaller than that of metals (about 400 million times). He did this by comparing the intensity of the shock perceived when an electric discharge was transmitted through a metallic wire of a given length and section, with that of tubes of various lengths and diameter, full of water.

By estimating the taste sensation produced by a bimetallic arc applied to the tip of the tongue, Alessandro Volta was able to detect, in 1792, a feeble current, too weak to be measured by the physical electrometers of the age. Moreover, on the basis of the same physiological sensor he could establish the polarity of this current. This experiment was a milestone in Volta's path to the discovery of the battery, a path that not only led to the invention of the epoch-making electrical device, but also resulted in an important series of physiological discoveries. Volta was able to show the eminently transient character of the responsiveness of the visual system, that contrasted with the tonic responses of the gustatory and nociceptive system. He anticipated, by about half a century, the fundamental idea of the functional organisation of the nervous system, Johannes Müller's doctrine of 'specific nervous energies'. This stipulates that the physiological effects of nerve stimulation depend on the type of nerve stimulated, and not on the type of stimulus used to achieve the stimulation. Moreover, Volta showed that the effect of the electric stimulation of nerves depends on the polarity of the stimulus, again anticipating important laws of electrophysiology, to be fully clarified only in the 19th century. Finally, Volta came near to envisioning the existence of an 'integration time' in the responsiveness of a sensory system, a time during which there was a partial time summation of the effect of prolonged electric stimulation (see Part IV).

If we consider that these achievements depended to a large extent on an evaluation of physiological responses, often of a sensorial character, induced by simple stimuli, as for instance a tinfoil lamina and a silver spoon, we cannot but suppose that the scientists of the *ancient régime* were endowed with a superior power of sensory and perceptual skill, compared to modern times. This is a likely possibility, if one considers, among other things, the attention to the sensations in the culture of the Enlightenment, and, moreover, the education of the upper classes of the age in the discrimination of gustatory, olfactory, auditory and visual stimuli.

WALSH AT LA ROCHELLE: TORPEDOES COMING

Soon after settling into La Rochelle, in rather unpromising circumstances, Walsh started his search for torpedoes. This did not appear to be an easy task, because contrary to expectations it did not seem particularly easy to find them there:

> At 1 P. M. arrived at La Rochelle; lodged at L'Hotel du Duc de Bourgogne, rue de Minage, a bad Inn; spoken with an Apothecary and a Bookseller: The former knew there were Torpedos at Angoulins, a league off to the South. The bookseller a total stranger to the animal. Stop'd by the Sentinel at the entrance of the new port.
> Visited M. Saunier, and the two Messrs. Weis. They knew nothing of the Torpedo by any name, except that the youngest M. Weis in fishing once with a Seine had met with it. (WE, 1 L.H.)[79]

Next day Walsh went to Angoulins, but there was no good news, because «The man of the Publick house» said «that there were no Torpedos, called by him Marote, till September» Fortunately, things start to change on June 28th when:

> Two fishermen of La Rochelle came to the Inn, who said that they frequently caught La Marote ou Tremble; that the shock was instantaneous; if received on the feet it overset them; if on the hand it left a trembling effect after it, that it could be received through a sword or stick, but not through their Nets in the water. (WE, 3 L.H.)

We can easily imagine how happy Walsh was about the availability of torpedoes, and also about the possibility of interrogating people acquainted

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[79] The Weis, repeatedly mentioned in Walsh's journal of experiments, belonged to a protestant family of Austrian origin. They became extinct in La Rochelle in 1875 with the death of Félix-Thèodore-Achille Weis (source of information: FEUILLERET & RICHEMOND, *Biographie de la Charente-Inférieure ((Aunis et Saintonge)*, Niort & Clouzot, 1875).

with the fish, and thus of at least indirectly verifying the pieces of information on the animal that he had to a large extent gathered from books. The two fishermen soon became the occasion for a first experiment aimed at ascertaining the similarity of the shock produced by the fish, and that of the Leyden jar:

> gave one of them a small Shock with the Leyden Phial and repeated it; he insisted that the Effect was precisely the same with that of the Torpedo. (WE, 3-4 L.H.)

The two fishermen engaged themselves to go fishing torpedoes the next day and to keep the fish alive for Walsh's experiments. One of them, named Cuchon, gave further details about the «Marotes»:

> there were plenty of them from that time, to the last of August; that the young ones began to appear in July; that they were male and female, distinguished, like the Ray, that the shock was instantaneous and what they all join in, that if received on the feet it overset them; that it was frequently felt through their net; and the little ones as well as the large are capable of giving it. (WE, 5 L.H.)

THE FIRST EXPERIMENTS: THE SHOCK OF THE TORPEDO

Cuchon's account was quite accurate, and we can suppose that Walsh felt himself coming ever closer to the object of his desire, the 'wonderful Torpedo' of Claudian's poem, and thus to the possibility of ascertaining the physical nature of its extraordinary power. Things did not go smoothly straightaway, however, because bad weather stopped the fishermen from going out to sea the next day, Monday 29th. However, on Tuesday 30th, Walsh was finally able to get his first live torpedo and thus start his first direct experiments. This was the «female Torpedo, Marote ou Tremble caught at 8 o' Clock», already mentioned, from which Arthur, Saunier and Walsh were all able to obtain a shock with characteristics «precisely like an Electrical Shock». This was the occasion that led Walsh to vividly express his perplexity over the nature of the phenomenon:

> On this my first experiment on the effect of the Torpedo, I exclaimed this certainly Electricity – but how? (WE, 7 R.H.)

On this first «Marote», Walsh and his collaborators carried out further experiments, trying to see whether the shock was transmitted from the person touching the fish to another connected to him, either directly - by joining the hands - or indirectly through a brass rod. On this, the experiment was inconclusive, because one of the people in the circle could not feel the shock. Eventually this torpedo – as Walsh wrote, «refused to give a further Shock, though much handled». (WE, 7-8 L.H.)

This negative result seemed to argue against the identity of the fluid involved in the fish shock and the electric fluid. Genuine electricity circulated easily through 'chains' of many people connected one to another This had been shown in spectacular fashion by Abbé Nollet some decades before, in famous experiments, such as the one at the College of Navarre in Paris where more than 600 soldiers jumped in unison once connected to a source of electricity. However, in his journal of experiments, Walsh did not seem to be disconcerted by the negative result, and apparently accounted for the lack of transmission of the torpedo's shock by noting that the fish «seemed exhausted, and with not much life when returned to the Water». (WE, 8 L.H.)

Fortunately, many «Marotes» were caught that day, and Walsh had the chance to further pursue his experiments on these fish, kept alive in water tubs, one of which - as Walsh noted - was kept «at the Hotel or Inn». The Hotel was to be the place where some of the experiments would be carried out, and, moreover, at the *Duc de Bourgogne* Walsh gave the demonstration of the electric nature of the torpedo's shock to the Members of the local *Académie*, a somewhat splendid conclusion to his researches at La Rochelle, sanctioning their success before the learned society of the place. This way of proceeding should not be considered surprising in an age in which experiments were usually carried out in private houses or in other lodgings, outside public institutions devoted to research or to teaching.

Before doing physiological experiments on the new fish, Walsh made an anatomical study of the first female torpedo, limited, however, to an accurate external description and to a gross dissection of the viscera, without paying any special attention to the structure that he was eventually to call «electric organs» (Redi's '*musculi falcati*'). Besides the description, Walsh has left some schematic drawings of this «Torpedo N. 7» in the journal. For the anatomical study, we can suppose that Walsh did not explore it very deeply, because he was eager to proceed further with the physiological (and physical) study of the shock in the other torpedoes and, possibly, because his ability in anatomical dissections was limited. When possible, during the period in La Rochelle, Walsh generally profited from the collaboration of local people expert in anatomy because of their profession. This was the case with M. Beauregard and M. Ranger, both surgeons, who helped Walsh carry out the torpedo's dissections in various circumstances. In addition, on his return to London Walsh took with him some of his torpedoes (preserved «in brandy») to be studied by John Hunter, one of the most famous anatomists and surgeons of the time.[80]

The new fish were studied in a physiological experiment, «a small female of 2 ½ Inches diameter, immediately touched, refused giving a shock». (WE 9 L.H.) Taking advantage of the presence of «a hollow upon the shore, where there was some Water after the Tide had retired» Walsh put the fish there and thus had an opportunity to observe its behaviour in a relatively natural habitat: the torpedo tried to escape by trying various sorts of movements without much success, and eventually «she made a little bed in the sand, around her, nothing appearing but the two Foramina behind her eyes» (WE, 10 L.H.). In a side page he noted that «this circumstance of it's burying itself in the Sand during the Ebb of the Sea seems to have been unattended to the Naturalists», and quotes a passage from Aristotle to support his contention». (WE, 11 R.H.)

[80] John Hunter (1728-1793), of Scottish origin, a pupil of Percivall Pott, was one of the most influential figures of 18th century medicine in England. He published several works of anatomical, surgical and medical character (on the teeth, on inflammation . on venereal diseases, on digestion, etc.). His famous collection of anatomical and natural history specimens, the 'Hunterian Museum', originally at the Royal College of Surgeons, is now on view in London at the Science Museum. It also includes some of the torpedoes that Walsh brought to England from La Rochelle, preserved «in brandy».

Perhaps due to its permanence in natural conditions, the fish became eventually capable of producing shocks which could be felt by both Arthur and Walsh. Importantly, Arthur noticed that «when the animal was still in the water, it gave a small shock, which reached the first joint of the thumb without affecting the finger at all», whereas when put in the air and touched with both hands, «in 5 five seconds it gave him a Shock reaching above his Elbow, in the muscles of the Arms first holding it and above his wrist in the muscle of the other». (WE, 11 L.H.)

Although not particularly remarked upon by Walsh and his collaborator on this occasion, the experiment provided the first indication that the shock was stronger when the animal was in the air, than when it was in the water. This difference was not obvious to Walsh when, on July 12th, he wrote to Franklin to inform him of the success of his first experiments. In this letter he said that the reason why the experiments were done chiefly in the air was «because the animal was more open to our examination than in Water» (WE, 72 L.H.). (see Fig. 5)

The difference in the intensity of the shock with the fish in the water and the air will emerge clearly in the last phase of Walsh's investigations in France and will be mentioned in the second letter to Franklin written on August 27th in Paris, after the experiments in France (not included in any of of the two Walsh's manuscripts but also be reported, although partially, in the 1773 article published in the 'Philosophical Transactions'):

> The effects produced by the Torpedo, when in air, appeared, on many repeated experiments, to be about four times as strong as when in the water.[81]

A clear explanation of the phenomenon would be given, however, only some years afterwards by Cavendish, in his famous paper on the artificial torpedo, on the basis of the shunting effect of the water, an explanation that seems to anticipate Kirchoff's law of currents division (see later).

Perhaps on this day of experiments, besides the satisfaction of having experienced the shock of the fish for the first time, Walsh may have been reflecting on the variability of the results that could be obtained from a living animal, and on the difficulty that this variability might represent for obtaining reliable information on the nature of the shock in the continuation of his experiments.

DIFFICULTIES OVER THE TRANSMISSION OF THE TORPEDO'S SHOCK

In the evening, after dinner, Walsh attended a meeting of the local Academy, with many members present: among them, besides «M. Rault, Directeur de l'Academie», Walsh met M. Seignette «Secretaire perpetual, et Maire de La Ville»[82] who was to take a very active part in further experiments

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[81] For a complete reproduction of this letter, in which, among others, Walsh congratulates Franklin for his recent election to the French *Académie Royale des Sciences*, see 'The papers of Benjamin Franklin', vol. 19, pp. 285-289 (see FRANKLIN 1960).

[82] Pierre Henry Seignette (1734-1808), lawyer, physicist and pharmacist of La Rochelle was mayor of the town in the period 1771-1775. He made numismatic and meteorological studies. He was a descendant of Jean Seignette who in collaboration with his brother Elie also pharmacist at La Rochelle became famous for his discovery made around 1665 of a chemical compound (lately identified as sodium-potassium tartrate), indicated as '*sel polychreste*' and usually referred to as to '*Sel de Seignette*" (see SEIGNETTE ca. 1675). The crystals of this compound, which was used in

on the torpedo. Among the books and papers of the *Académie*, Walsh was particularly interested in the «Eloge de M. Reaumur, by M. De Villars, Academicien, which he read to the Academy about six weeks before», and notably by the passage in the 'Eloge' where De Villars expressed his doubts about the explanation of the torpedo's shock given by Reaumur, because it could not account «by what means the shock of the Torpedo passed through many persons who held hands, affecting them equally and instantly».(WE, 12 L.H.)[83] We could suppose that Walsh might have reflected on the difference between what De Villars wrote in the 'Eloge' and what he had personally observed the same day, when the shock of the first torpedo investigated seemed to be incapable of circulating even through a chain of just two people. Walsh's attendance at this meeting is proved by the register of the *Académie*, where on this date we read:

> M. Walsh, fellow of the Royal Soc. of London and of the House of Commons has attended the meeting, introduced by the Director.[84]

Next day the problem was a matter of discussion with M. De Villars, who, supposing that Walsh's experiments might not confirm the transmission of the shock through a chain of several people, regretted «that he mentioned anything about the Torpedo in his Memoir».(WE, 13 L.H.) Walsh, De Villars and others went afterwards to the *Hotel du Duc de Bourgogne*, where there was a live torpedo. From this fish, Walsh could draw a shock, but De Villars «felt nothing» by holding Walsh's hand during the shock. We can suppose that the regret of the *Académicien* for what he had written in his '*Eloge*' (without checking up on the phenomenon personally) might have increased on that occasion. However, during a public demonstration of the torpedo's shock held at the *Académie* in the last phase of his stay at La Rochelle, Walsh succeeded in transmitting the shock through a chain of seven and eight people, and this happened, we may suppose, to the great satisfaction of M. De Villars.

The problem of the transmissibility of the torpedo's shock apparently became both Walsh's and the party of *Académiciens*' most urgent concern in the next series of experiments. These were carried out on Torpedo N. 2 in conditions more appropriate to avoid artefacts, and in particular, to exclude the possibility that the lack of transmission might be due to a contact of the members of the chain with the ground. Walsh had recourse to the procedure typically used in classical electrical experiments of the time, based on the use of seats or tables whose feet were made of insulating materials:

> Three persons, insulated on well wiped Glass legg'd Stools, holding hands, the two extreme persons touching the Torpedo, one by holding his flank above and below, the other by seizing his head above and below: the first struck slightly to

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medicine with a variety of indications and notably as a purgative, have piezoelectric properties and are now widely employed in the modern electronic industry; this, *a posteriori*, more than justifying the authors' expectations (*polychreste* meaning 'serving for different purposes').

[83] Girard De Villars (born in 1698) a famous physician of La Rochelle was member of the *Académie* since 1741. He was also librarian of the town. He made observations on mechanism of regeneration and reproduction of animals and also on the classification of the plants of the region, but the manuscript dealing with these studies is apparently lost. Some of the plants he described are, however, quoted in the botanical treatise published in 1846 by Léon Faye. ('*Note sur les progrès de l'étude de la botanique dans le département de la Charente-Inférieure*' Poitiers, Impr. de Pichot).

[84] «*M. Walsh, membre de la Soc. Roy*[le] *de Londres et de la Chambre des Communes, a assisté à l'assemblée, introduit par M. le Directeur*», *Archives Départementales de La Charente Maritime* 103, J. *Archives de l'Académie de Sciences et Belles Lettres et Arts de La Rochelle, Second Registre des délibérations de l'Académie de La Rochelle, 22 Juillet.*

the muscles above the wrist, the other slightly to the breast, the middle person not at all.(WE, 17 L.H.)

In another torpedo the results was similarly rather deceptive, with one of the members of the chain being unable to perceive the shock, in spite of the fact that the trial was repeated five times.

In a further experiment, Walsh «electrified» the animal very probably by connecting it to the electric machine (the one to which he refers in the *journal de voyage*), «when insulated» (i.e. probably placed in air) and drew sparks from it, but nothing particularly happened. The torpedo «appeared as immobile as before, quiet and insensible of these operations. (WE, 18-19 R.H.)

Walsh does not give any indication concerning the rationale of this experiment, and it is possible that it was one of those trials that scientists do simply because, as Lord Adrian has keenly remarked, an essential characteristic of the true experimental scientist, that might possibly lead to important discoveries, is the tendency to manipulate laboratory instruments and preparations, without any apparently purposeful design.[85] In the case of Walsh's experiment with the 'electrified' torpedo, however, it is possible that there was some specific reason for such an experiment. Walsh might have been investigating whether the shock of the torpedo changed in intensity if an external electricity was provided for the animal (experiments on the effect of external current on the discharge of electric fish will be tried in the 19th century, for instance by John Davy - the brother of Humphry - and by Du Bois-Reymond).[86] Or he might have been intrigued by the problem of the conductive nature of animal tissues, a problem that will emerge again in further phases of the researches at La Rochelle (see later).

DIFFERENT SENSITIVITIES TO THE SHOCK

On the same torpedo, Arthur and Walsh separately tried to obtain the shock also by connecting to the animal indirectly through metallic wires or silver spoons. The result was completely different for the two experimenters, as Walsh noted:

> I found no effect from him [i.e. the Torpedo] through two silver spoons, which I held to his back and belly. Arthur presently after touched his back and belly with the two spoons, and received a shock up both his arms equally, affecting him chiefly in the fleshy part of the arm above the Elbow. [...] Tried again the preceding fish, who gave repeated small Shocks that I could not feel with the Spoons, and hardly with my naked finger and thumb: appearing to me little more than a fourmillement, or creeping on the skin; but to Arthur they were distinct shocks with the Spoons to the upper part of his arm at first, and afterwards as the fish weakened, to his wrists. To M. Saunier. And M^{r}. Davies, it was imperceptible, as to me with the Spoons. (WE, 20-21 L.H.)

It therefore appeared that different people could have a drastically different sensibility to the shock, and this might explain some of the experimental variability, and account for the difficulty over having straightforward results in some experiments, as for instance in the transmission of the shock through a chain of people. To the problem of different individual

[85] ADRIAN 1954 (see HODGKIN 1977).
[86] see DAVY 1834 and DU BOIS-REYMOND 1843.

sensibility to the torpedo's shock, Walsh will return in subsequent phases of his research. In this day's journal he makes a marginal note on this subject:

> Arthur was in all our Experiments much more sensible than me, M^r. Davies much less so, till he had a fit of sickness at the Isle de Ré after which he became much more sensible than even Arthur. (WE, 21 R.H.)

Walsh then bought two dead torpedoes at the fish market in order to carry out an anatomical dissection in collaboration with «M. Beauregard a young Surgeon». The study was limited to the examination of «the state of Viscera», and again it did not involve the electric organs. Walsh appears to have been particularly interested in the shape and characteristics of the eggs, and more in general in the reproductive apparatus of the fish. Interestingly, in recording the observation concerning «the two Oviducts», Walsh noticed that Beauregard called them »les Tubes Falloppiens», a detail that shows the different origins of the anatomical culture of the English scientist ('natural history'), from that of Beauregard (the medical tradition).

The same day, other anatomical studies were carried out on some of the torpedoes that had been caught alive for physiological studies, but had not survived long in artificial conditions. About these torpedoes, Walsh wrote:

> One of them of only 2 ½ Inches diameter, with an appearance of Eggs in it; put it into Brandy. On examination of this Torpedo in England it proved to be a young one lately brought forth, having a part of it's own yolk, like what is seen in Chickens just hatched, drawn up into it's Abdomen, and which had not yet been consumed. (WE, 24-25 L.H.).

This detail is interesting since it shows that Walsh, engaged mainly in physiological investigations during his time in France, reserved the most detailed phase of his anatomical studies to his return to England. As already mentioned, most of this study was carried out by John Hunter. Some of the torpedoes studied by Hunter were reproduced in the magnificent plates that illustrates the article published in 1773, in the same issue of the 'Philosophical Transactions' that contained Walsh's paper dealing with physiological investigations of the torpedo's shock (Fig. 13). These torpedoes were brought to England by Walsh after having been put «into Brandy» in France, as happened with the small torpedo just mentioned. By a fortuitous (and fortunate) series of circumstances some of these specimens have been preserved as a part of the Hunter collection of anatomical and surgical objects, and can still be seen in London in the Museum of the Royal College of Surgeons.

As to the 'Brandy' used by Walsh to preserve torpedoes, we can see here how scientists of the past usually made use of materials and objects from everyday life for their experiments (another aspect of a science that was in transition from the style of amateurs and philosophers to the more professional attitude that characterises modern science). In their experiments on the transmission of the torpedo's shock, Walsh and his collaborators made frequent use of metallic spoons and other similar objects. Metallic spoons or other objects of everyday life, and particularly metallic coins, were used by Volta in some of the famous experiments that led him to the invention of his battery.

As we know from his journal of experiments, the two dead torpedoes that Walsh bought at the fish market and studied anatomically with the assistance of Beauregard «were fished at the Isle de Ré by an association of Barges from

Nantes». From a conversation with «the Captain of Barges, Sebastien Bertrand» Walsh obtained useful information concerning both the availability of torpedoes in the Isle de Ré, a small island close to La Rochelle, and the characteristics of these fish, through a channel of communication (fishermen), corresponding to the usual source of information for many old authors who had written about these electric fish, without, in many cases, having had any direct experience of them. Walsh wrote:

> Bertrand acquainted us that there were 11 associated Barges from Nantes, 5 of them on this fishery for the supply of La Rochelle, and there will be more next week; That the Trembles are in much great plenty there, than at Nantes; that they are fished in different depths, from 4 to 12 feet, they are even found on dry sand. He frequently felt the Shock through his net at 6 feet distance: Once chopping a Tremble with a Hache, shocked and forced to drop the Hache; The best way of bringing them is tying them by a string just above their tail fine, and towing them ashore. (WE, 21 L.H.)

Concerning the transmissibility of the torpedo's shock alluded to by Captain Bernard, we may note here *en passant* that his report corresponds to Cuchon's, but contrasts with the words of the first fishermen met with at La Rochelle. According to them the shock «could be received through a sword or stick, but not through their Nets in the water» (WE, 3 L.H.). In the case of the torpedo and of other electric fish, the transmissibility of the shock through the fishing net (and through other materials) had been a controversial issue in the accounts given by old authors. It will be one of the problems Walsh addressed with special care during the late phase of his research in France and later (and after Walsh, by Cavendish as well).

The problem of the transmissibility along a human chain continued to be central to Walsh's concerns (and his collaborators') in the following days of experiments at La Rochelle. As Walsh notes in his journal for July 4th, he and Arthur succeeded in feeling the shock from a torpedo:

> by joining hands, and touching him with metal Spoons, one below, the other above; the fish laid upon a table; dry in the air. (WE, 25 L.H.)

«We were - he adds - both struck slightly in the touching arm». This experiment appeared to be successful, in spite of the fact that the torpedo seemed to be «feeble»: an outcome very likely caused by the animal being in the air, the shock produced not being shunted by water, and consequently flowing more effectively through the bodies of the experimenters. At this moment, however, Walsh seemed actually unaware of the particular effectiveness of the shock when the fish was outside water.

As usually happens in experimental science, the success with the trial of transmission through a human chain was not as complete and satisfactory as might have been hoped. When Mr Davies joined Walsh and Arthur, he was not able to feel the shock, even when his position in the chain was deliberately changed: initially Davies was the central element, then he touched the animal with a silver spoon, while Arthur perceived a distinct shock, in spite of the fact that he had now moved to the centre of the chain.

«Arthur and I - Walsh writes - were shocked two or three times, but Mr Davies still felt nothing.» (WE, 26 L.H.) A bit more successful were the experiments tried on other torpedoes in the evening of the same day, when M.

Seignette, the Academy Secretary and Mayor of La Rochelle, substituted for Davies in the chain.

Even though Walsh does not record any methodological reflections in this phase of his investigation, we can imagine that he was becoming gradually accustomed to the variability of these experiments, in which both the preparation and the experimenter could be the source of variation and unpredictability. In his tendency to repeat the trials, in spite of the initial failure or partial success, and in his effort to modify, in various ways, the experimental arrangements in order to separate what was essential from what was accessory for the result, he reveals characteristics typical of the highest standards of an experimental investigator.

Another important feature of Walsh's scientific work is his tendency to learn from past experiments how to proceed in future ones, a form of learning that is not always recognised explicitly by the experimental scientist. Although, he seemed unaware of the stronger effectiveness of the shock when the fish was in the air, nevertheless, from the further progress of the research it appears that he was falling back more and more on the animal outside water and dried. This is the case for instance with the «small torpedo» studied on July 5th (a Sunday). When touched «with a finger under, and another immediately the soft puffy spongy part» this fish produced «constant feeble shocks like a pulse when the finger was at rest», (WE, 27 L.H.) and gave stronger effects when «the fingers were drawn along the soft part, opposite each other, above and below».(WE, 28 L.H.) By observing this torpedo at the moment of the shock, Walsh remarks that he could not perceive any of the movements that Réaumur had considered as fundamental aspects of the mechanism of the shock.

THE TORPEDO AND THE 'CHARGED PANE OF GLASS'

The next torpedo investigated the same day was also studied «in the open Air». In this fish Walsh, Saunier and Arthur were eventually able to succeed in the experiment of transmitting the shock through a human chain of three elements. The central element of the chain (Saunier) communicated with the other two through silver spoons. In a further trial, Saunier again felt the shock by communicating with Walsh and Arthur «through Wires a yard and half long» (WE, 28 L.H.). Walsh noticed that this happened in spite of the fact that the fish «gave very small Shocks, some so minute, as to be scarcely more than a fourmillement». On this same fish he made another experiment described as follows:

> Holding him by the tail with one hand, and touching him above and below one flank with the other hand, a Shock, which was instantly succeeded by fourmillement, such as we had obtained from a charged pane of Glass by holding the hand almost in contact with it. (WE, 29 L.H.)

Although the experiment may not appear to be particularly noteworthy, this passage is interesting because it is an example of the importance of a visual suggestion in the path of research. With the phrase «pane of Glass», Walsh was referring to an electrically capacitor of the flat type, a device first developed by Franklin (and usually called Franklin's 'magic square') in order to prove that the power of the Leyden jar to accumulate electricity did not depend on its shape, but only on the presence of the glass it was made of. The square capacitor consisted of a glass pane, usually coated on the two sides by metallic laminae, and was normally charged by connection to an electric machine.

With its flat body, the torpedo that produced shocks and '*fourmillement*' (numbness) evoked in Walsh the image of the square capacitor, not only for the effect that it produced but also for its shape. As already mentioned, another source of visual inspiration for Walsh, in his attempt to understand the power of the torpedo, was the Leyden jar. This device was also the mental image that guided Luigi Galvani in demonstrating that an electricity similar to that of the torpedo, but of a weaker force, was present in the muscle and nerves of common animals. In other circumstances, the structure of the electric organ of the torpedo and electric eel was the mental image that guided Alessandro Volta in inventing his electric battery, with its arrangement of disks staked one above the other. (see Part IV)

The importance of images in scientific endeavour is not only because they provide a suggestive visual representation of a hypothesis or a mental elaboration, but also because they usually imply a mechanistic reference capable of stimulating further investigation. In the case of the square capacitor we have an indication of its importance in directing the progress of Walsh's study from the experiment he carried out the next day (July 6th):

> Charging a small Pane of Glass coated on each side, Effects exactly similar in point of sensation, to those obtained from the Torpedo, are produced. The finger touching one surface of the Pane, and the Thumb nearly, but not quite in constant with the other, occasions the fourmillement and engourdissement of the Torpedo, which causes the moment the hand is withdrawn from the Torpedo as with the Pane. On touching the Pane, when it is already charged, and a continual supply is throwing into it, a small Shock is first felt, and that is immediately succeeded by a fourmillement: The same thing happened to me from a Torpedo, I was lifting from one place to another. The fourmillement seems to be the effect of Intention without Ability. (WE, 32-33 L.H.)

We have already alluded to the easy way Walsh passed from animal experimentation to experimentation with physical devices, and we will return to this point again. Re-examining the journal for July 5th, it is evident that Walsh was satisfied by the results obtained during this intensive working Sunday:

> All the Experiments this morning strongly marked the effect to be Electrical, and not arising from a muscular stroke, nor from a refrigidating quality in animal, of which it has nothing. (WE, 29 L.H.)

At this point the electrical hypothesis of the torpedo's shock starts assuming a more substantial connotation: by referring *en passant* to the mechanical theory (of Lorenzini and Réaumur) and to the Galenic hypothesis of the 'cold venom', Walsh seems to be distancing himself from the past. An observation bearing an explicit reference to Réaumur's theory is indeed present in one of the annotations of that day:

> We could perceive nothing of the concavity mentioned by Reaumur; nor that the effect was strong when he raised himself up; as it were to draw the breath. (WE, 27 L.H.)

The experiments of Sunday 5th continued until late in the evening on various torpedoes. The results obtained supported Walsh's increasing confidence in the electric nature of the shock, although the last torpedo investigated «on a table by Candle light - as Walsh writes - refused any Shock whatever». (WE, 31 L.H.)

We may perhaps note here that experimental science is usually based on very intensive and enduring work, and that working at late hours, and during holidays, seems to be the rule rather than the exception for scientific effort, although we rarely find information on this aspect of scientific activity in published papers. From his laboratory notebooks we learn, for instance, that Alessandro Volta worked until very late in the evening, and sometimes spent the entire night with his animal preparations in the period he was trying to replicate Galvani's experiments on animal electricity. As for Galvani, he was extremely busy with his many duties as Professor at the University and at the *Istituto delle Science* of Bologna, as well as with his medical profession, at both private and public levels, so that making experiments or writing scientific papers in holiday periods was almost a necessity for him.

THE TWO OPPOSITELY CHARGED SURFACES OF THE TORPEDO

On July 6th, besides the experiment with the charged «Pane of Glass», made in the morning, Walsh and Arthur also carried out further experiments on live torpedoes in the evening. There is no explicit indication of what the next question was that Walsh tried to answer with these experiments, which were based on relatively small experimental variations. We can gain some insight into Walsh's mental processes by considering the details of the experimental arrangements from his journal records:

> touched with naked fingers the Electrical flank on one side [i.e. one of the electric organs] above and below; Arthur at the same time pressing the other flank above and below with Spoons; we each of us shocked at the same instant; and this several times.
> Arthur holding Spoons above and below on one flank, I touching the other flank with one finger; on taking off I felt nothing, he felt the Shock. Repeated this 5 Times.
> Touched the two upper flanks at once; Nothing: 4 times repeated.
> Touched he two lower flanks; Nothing.
> Touched the upper and under flank; on taking off and putting on my thumb, received the Shock 6 or 7 times; weakly, instantaneously, to the first joint of the finger and thumb.
> On touching the Eyes, which are prominent, he draws them in as the Snail does it's antenna.
> Touched him all over on the upper side; no shock.
> Touched above and below on the same flank; constantly Shocked.
> Touched below on one side and above on the other; Shocks, but not so perfect as when touched above and below on the same side.
> When the finger was changed to the same side, he gave the shock directly.
> His prominent Eyes always sink down, whenever he gives the Shock. (WE, 34-35 L.H.)

From the passage above, it appears that, by manipulating the animal in various ways, probably with the initial aim of verifying whether it was really necessary to establish a circuit in order to perceive the shock, Walsh gradually approached the solution to an important problem, concerning the zones of the animal surface on which the electric difference responsible for the shock was produced. Eventually, as reported in the 1773 paper in the 'Philosophical Transactions', he will be reach the conclusion that the «upper and under surfaces are capable, from a state of equilibrium with respect to electricity, of being instantly thrown, by meer energy, into an opposition of a *plus* and a *minus* state, like that of the charged Phial [i.e. the Leyden jar]».

Even though this idea that the shock is produced between the upper and lower surface of the animal body was to be recognised explicitly in a later phase of the investigation, it was implicit in the resemblance of the torpedo to a «charged Pane of Glass», the image that had just emerged during the most recent phase of the experiments.

With regards to the similarity between the animal preparation and Franklin's square capacitor (usually coated by metallic laminae), it is likely that it was one of the reasons why, in one of the experiments of the same day (and also in some later phases) «a plate of tinfoil» was put under a torpedo, although this was apparently done in order to «collect all the Electricity» - as Walsh writes (WE, 37 L.H.) - and not to increase the electric power of the shock (as would have occurred with the physical capacitor). The manoeuvre, however, did not result in a better electric performance of the animal it was initially tried on.

In the passage quoted, one interesting experiment was when Arthur did not perceive the shock by simply «holding Spoons above and below on one flank» until he touched the fish with a finger, and then took it off. As will appear more clearly in further experiments, the animal needed to be excited, to be stimulated in some way, in order to produce the shock. In other words it did not behave like a constantly charged Leyden jar or a Franklin's square capacitor, that produces the shock whenever the two oppositely charged faces are connected through some conductive body. In the journal of experiments, Walsh will recognise this explicitly on various occasions. In the 1773 paper, he will express the concept that the animal produces the electric effect not constantly, but as a reaction to external stimuli or for its functional necessity, by saying that the apparatus responsible for the shock «is subject to the will of the animal».

Another interesting notation concerns the eye movements that Walsh perceived in the fish at the moment of the shock. These movements, together with other small visible changes of fish attitude in the preparatory phase that precedes the shock, were afterwards conveniently exploited as announcements of the imminent shock, even if the shock might not be perceived due to some peculiarity of the experimental arrangement. In the already mentioned letter to Franklin that he was to write on July 12th, Walsh said:

> Each shock is conveniently accompanied by a depression of the Eyes by which we were ascertained ever of his attempts to give it to non conductors. (WE, 72 L.H.)

In this particular case we can see an instance of that type of learning that the experimental scientist draws from his past experiments, and that becomes an essential tool in directing the progress of his researches.

In most of the experiments tried on the same day (Monday 6th), Walsh and his collaborators (namely Arthur and Seignette) seemed to be well aware that the shock was felt in the most effective way by touching the upper and lower surface of the fish's body, and that no shock was usually felt when the contact was established with only one of the two surfaces. In a rare case in which Walsh «had a stroke from touching underneath only», he supposed that the effect might be due to an artefact. He wrote: «Quere if he did not touch with more than one finger». It seems that, growing more and more confident of the electrical nature of the shock, Walsh was more prepared to attribute the experimental observations that seemed in contrast with his hypothesis, to occasional and unforeseen causes, rather than interpreting them as evidence against his hypothesis.

On other occasions Walsh accounts for unsuccessful results through explanations based on the electric notions of the age. This happens for instance when he himself, Arthur and Seignette, forming a chain communicating with each other through metallic wires, did not feel the shock. Walsh wrote in a margin on this experiment:

> The electricity either passed by other conductors round the Torpedo, which was not insulated; or else from the badness and length of Conduction passed slowly and unobserved. The rapidity of its passage occasions the sensation. (WE, 39 R.H.)

This experiment was carried out on July 7th. Walsh worked the next day as well, although apparently at a less intense rhythm. From the torpedo that he examined on that day, he was able to obtain several shocks, and he took advantage of the occasion to compare the strength of the sensation obtained from the two sides, examined separately, and to ascertain how the intensity of sensation changed by putting the fingers in different positions on the animal's body. Interestingly, after the dinner (in which a «boiled torpedo» was served), he made an experiment with the electrical machine «procuring Shocks, just strong enough to pass through one person, without going through two or three». (WE, 42 L.H.) On this occasion Walsh was again passing from an animal experiment, to an experiment with a physical device, in order to have a possible explanation for his physiological observations. He was trying here to account for the frequent failures obtained in the experiments of transmission of the fish shock through a chain of people. The conclusion he was able to draw from his comparison was, that when the shock was weak, as with an electric machine charged to a low degree or with a feeble torpedo, the shock could not pass through a chain of many people, although it was felt by a single person.

On July 8th there was no experiment. Walsh was probably tired, but on the other hand, he was evidently satisfied psychologically, as appears from some notes of a non-scientific character in the journal for that day, concerning the countryside with its beautiful canals, springs, carps, trees and paths where he had the opportunity to walk after a dinner held at M. Weis' home.

A CRUCIAL DAY: JULY 9TH 1772

These relaxed moments seem to be like a placid *intermezzo* that precedes a more lively phase the next day (July 9th 1772), played at *andante con brio*, of very intensive work that Walsh will conclude with his first public announcement:

> the Effect of the Torpedo to be Electrical (WE, 54 L.H.)

The experiments of this fundamental day of Walsh's researches at La Rochelle were carried out on two torpedoes fished by Captain Bertrand in the morning. Early in the day there was no sign that things would go particularly smoothly. Walsh and Arthur tried unsuccessfully to obtain a shock, by joining hands and touching the first fish studied, one above and the other below, with two spoons. Walsh seemed ready to account for the unsatisfactory result on the basis of the animal's condition: «Torpedo very lifeless», he writes. In the same vein, on noting afterwards the very concave back of the animal in the region of the electric organs, he added that this «seems to be a sign of weakness only». (and not, as Réaumur had supposed, an essential aspect of the mechanism of shock production).

However, things improved and a shock was afterwards felt with a similar experimental arrangement. In one of the subsequent experiments Walsh tried to obtain a physical measure of the shock by using a pith ball electrometer, but without success. Although he accounted for the failure of this trial by noting: «The wind too great to be successful», it is likely he was alluding to this unsuccessful experiment in his first letter to Franklin (written a few days later, on July 12th, and published in a modified form in the 1773 article in the 'Philosophical Transactions'), when he wrote:

> We have not yet perceived any Spark or Noise to accompany the shock, nor that the pith balls are affected in any instance. (WE, 71 R.H.)

In later phases of his researches Walsh will again try to obtain a physical measurement of the torpedo's electricity, but always without success.

Walsh and Arthur, forming a chain, then connected to each other through a metallic spoon, experiencing the shock by one touching the upper surface of the fish and the other the lower. The shock was not felt when both touched only the upper or the lower surface. The experiment was repeated several times with success, even though in one of the trials, in which both touched the ventral surface of the animal, «Arthur thought he felt a little in his finger». Walsh did not, however, appear to be particularly concerned about this unexpected sensation of weak shock perceived by his collaborator. On a subsequent trial in which Arthur touched the lower surface and he himself the upper one, he wrote that both «felt a smartish shock, compared with the other very feeble one».

The idea that the shock was produced by a change in the electric state of the dorsal and ventral surfaces of the animal, an idea that had been emerging implicitly over the previous days, and had directed in a more or less unconscious way the progress of the experiments, thus seemed clearly established.

Taking advantage of the ability of the torpedo examined to give many repeated shocks without much fatigue, Walsh and Arthur were able to clearly confirm the necessity that a circle be formed in order for a shock to be felt by both of them (see Fig. 14). To avoid artefacts that could have undermined the significance of the results obtained, all the elements of these experiments (i.e. the two experimenters and the fish) were properly insulated from the ground.

The passage in the journal describing these experiments is worth reproducing, at least in part, because it conveys the increasing animation and the lively rhythm that characterised the experimental session of this crucial day. The laboratory notes are written succinctly, suggesting that the experiments were being carried out at a pace so rapid that there was little time to set them down. In interpreting these notes, it should be recalled that Walsh and Arthur were able to know when the animal was going to give the shock, even though they could not perceive the commotion. They knew this by observing the small movements of the eyes that preceded the generation of the shock, noticed in previous experiments, and usually indicated as «Signals» of the shock.

> All insulated; not joining hands; one above, the other below; it could not give it; though he attempted it 2 or 3 times.
> Joining, he immediately gave it.
> Unjoined, attempted, could not give it.
> Joined, gave it.
> Repeating it again and again, by taking unjoined two Signals, and the having two shocks joined.
> Tried joined by touching the two different upper sides; could get nothing.
> The 2 under sides: Signal; no effect.
> Opposite sides of the same flank, hands joined, Shock.
> Repeated again, touching the upper and lower flank, without joining; strong Effect by dropping his eyes; no effect,
> Joining hands, immediate Effect. (WE, 47-48 L.H.)

Having clearly established that the shock was felt only when the people participating in the experiment established a circle, Walsh and Arthur next undertook to test experimentally the differential transmissibility of the shock through electrically conductive or insulating matter, a crucial aspect of the electrical hypothesis of the torpedo's shock. Some of these experiments were done on the same torpedo and were reported immediately after the passage quoted, after the line «Joining hands, immediate Effects»:

> Repeated the same, communicating with a Spoon; gave the shock twice.
> Communicating with Glass, Signal; nothing.
> With Sealing Wax; a Signal; Nothing. (WE, 48 L.H.)

Afterwards the experiments were continued on a second torpedo that gave out shocks so powerful, that attempts to take it out of the water were made rather difficult. This torpedo was investigated by Walsh and Arthur again with the idea of ascertaining the different transmission of the shock through conductive and insulating materials. The description of these experiments is reported in detail below:

> Walsh and Arthur communicating with a Spoon, and touching one above, the other below the same flank; a Shock.
> Communicating with Sealing wax, a Signal, felt nothing.
> The Spoon again, immediately a Shock.
> Sealing Wax twice; nothing.
> Arthur put his Thumb upon Walsh' hand while the sealing wax was still in their hands; two signals – nothing.
> Joined hands, immediately a smart Shock.
> All insulated; sealing wax in hand; joining hands by thumbs, pressing harder; Arthur communicating with the under side; felt a very strong Shock. Repeated the same.
> Changed Sides; Walsh, communicating with underside; felt nothing; but Arthur felt a Shock; plainly from being more sensible.
> Joined hands; felt it immediately.
> Communicating with Sealing wax: two signals, felt nothing.
> Communicated by thumbs; placing ball of one upon the joint of the other thumb; two Signals; nothing.
> Full hand within full hand; plain shock.
> Holding Sealing wax in full hands joined; strong Shock.
> Walsh and Arthur communicating by Spoons, and touching above and below the same flank with Spoons, felt it on touching hands. Repeated; the same effect.

> Communicating with Glass, short, at an Inch distance, and touching with Spoons, two Signals, felt nothing.
> Again communicating with Spoon, and touching with Spoons; felt it twice.
> Communicating with Sealing wax, at half an Inch distance; touching with Spoons; two signals, felt nothing.
> fish insulated; single person insulated, touched both flanks above with different hands, felt nothing.
> Touched the same flank with different hands; nothing. In each of these experiments there were two winks.
> Touched the upper and lower side of the same flank with Spoons; Shock, twice.
> Repeated with Spoons; a Shock.
> With Sealing Wax; nothing.
> Repeated with Spoons; Six times.
> With Sealing wax, twice; nothing.
> Tried with 3 persons communicating with Spoons; too weak for the third person to feel the Shock.
> Two persons communicating by a Spoon; felt it. (WE, 49-53 L.H.). (see Fig. 14)

In this lively description one can feel the pervasive excitement of the scientist in those rare moments of his investigative activity in which nature seems to grant him its secrets with apparent ease, when any result obtained appears to be useful and coherent with the hypothesis that he is developing. The experiments are repeated many times with small variations, in an apparently compulsive way, because the inquisitive scientist is never satisfied and requires endless experimental support for his hypothesis. The indefatigable repetition of the trials indicates that, in the early phase of his experiments, Walsh had learned how difficult it could be to collect coherent data in experimental research, and particularly in animal studies, where many variables, difficult to control, could affect the outcome of the experiment and render the search for scientific truth elusive. When, due to some blissful combination of circumstances, all the sources of unpredictability appear to be largely settled, as was the case for Walsh and his collaborators on this fortunate day, it seems a good idea to persevere with in the experimental activity, to obtain all available information and support. One must consider, moreover, that in the 18th century the idea that experiments should be repeated many times and in different ways in order to exclude the interference of accidental circumstances was already a fundamental dogma of experimental methodology in both the field of chemistry and physics, and in that of the life sciences.

TRANSMISSION THROUGH LONG WIRES AND CHAINS CONSISTING OF MANY PEOPLE

The next day of experiments (July 10th) did not promise to be particularly fruitful at first, since the first trials, made on «a fresh torpedo» brought in the morning by Captain Bertrand, gave unsatisfactory results. A weak shock was felt only by a chain formed by two. When the chain was made up of three elements (Walsh, Davies and Arthur), only one of them felt the shock (independently of the position occupied in the circle, this was invariably Arthur, considered by Walsh the most sensitive of the team). A note in the journal seems to account for the unsuccessful result, and at the same time conveys a sense of disappointment for the more ordinary performance of this experimental session:

> The Weather moist and cloudy: the Torpedo feeble and reluctant.(WE, 55 L.H.)

Electrical experiments, as was well known, gave bad results on humid days, and moist weather also negatively affected animal organisms, according to the theory of atmospheric influences of classical medicine.

Walsh's remark seemed to be confirmed by the next trial in which:

> holding two spoons connected by Wires; and touching top and bottom with them; the signal of the Shock was twice made by the Animal, but nothing felt by the holder of the spoon. (WE, 56 L.H.)

However, «a smart shock» was felt afterwards by a single person when he touched «one flank above and below with a finger and thumb of the same hand». Probably the confidence in the power of the fish to produce effective shocks provided by this observation, encouraged Walsh and his collaborators to proceed further with these experiments, which were to provide further evidence of the similarity of the shock produced by the torpedo and one brought about by genuine electricity.

In a first experiment, a shock was felt repeatedly by two people who were «distant from each half a yard, but connected by a Brass Wire; one touched the upper Surface, the other the under of same flank with finger». No shock was felt when the connection was modified by changing «the Wire for Sealing Wax». (WE, 56-57 L.H.)

These results simply confirmed the conclusions reached the preceding day, but a further experiment added new information. The rapidity of the corresponding notations is typical of the passages that in Walsh's journal usually correspond to moments of discovery:

> Connected by ten yards of fine Brass wire; and proceeding as before; felt weak shock repeatedly.
> Laying aside the Wire, and joining hand, the Shocks much stronger.
> Used the short Wire: the Shock weaker than with joined hands.
> Joined hand again: the Shocks strong.
> Used the long Wire again, twisting it three or four times round the hands that held it; stronger shocks than when not twisted.
> Communicating by Sealing Wax. The distance between the hands of the two Persons very small, almost in contact: No Effect. (WE, 57-58 L.H.)

With these experiments Walsh was obtaining evidence that the intensity of the fish shock decreased as the path of its circulation was made progressively more difficult (or, in modern usage, as it resistance was increased), and this was further evidence for the electrical theory of the torpedo's shock. The value of this argument was not, however, discussed in the journal, nor considered in the letter to Franklin of July 12th , where Walsh mentions that the shock could be transmitted:

> through a considerable length of wire by two insulated persons, one touching the lower surface, and the other his upper. (WE, 71 R.H.)

Wishing to stress the event of conduction through a long wire, Walsh probably preferred not to point out that a long wire could make the shock weak. As we shall see, in the last phase of his experiments at La Rochelle, Walsh will become progressively more aware of the power of long circuits to attenuate the effect of the torpedo's shock.

Before leaving the discussion of the results of July 10th, it is perhaps appropriate to recall here what Walsh had written a few weeks before in his 'Memorandum concerning the Torpedo, made in the Journey from Paris to La Rochelle' (see above).

> Try it with every Conductor and non-conductor you can think of.
> Try as great distance as you can. (WJ, 53 L.H.)

At this point of his investigations, Walsh's experiments had been successful in the two main issues that were implicitly addressed in these two lines: the different transmission of the shock through conductive or non-conductive bodies, and the possibility that the shock could be transmitted also by long metallic wires. With the results of the further experiments made in the afternoon, these findings provided experimental evidence in favour of the «torpedinal electricity» (as Walsh will afterwards call it) sufficient to fulfil the commitment he had made to Franklin in his letter from Paris before leaving for La Rochelle:

> My next will convey to you the event of our Experiments on the Torpedo. (WJ, 26 R.H.)

Perhaps the most important finding that Walsh obtained in the afternoon of July 10th, at least from the point of view of his intention to communicate to Franklin the successful results of his investigation, was the transmission through a chain of four people. This was eventually achieved through another torpedo brought «by Madame Gasan's fishermen», as Walsh records in the journal (these fishermen belonged to the rather numerous group engaged by Walsh to search for live torpedoes). The experiment had an immediate echo in the letter to Franklin two days later, where on speaking of the torpedo, he wrote:

> we have been able to convey his shock, though they were very small, through a circuit of four persons.[87] (WE, 71-72 L.H.) (see Fig. 5)

Walsh also tried a chain of five people, but unfortunately «the middle person did not feel the Shock». This unsuccessful experiment was not, however, mentioned in the letter to Franklin.

THE TORPEDO'S DISSECTION, AND EXPERIMENTS WITH ARTIFICIAL ELECTRICITY

Among the other experiments tried the same day, there was a potentially interesting one carried out with the collaboration of M. Beauregard (the Surgeon). It consisted in a dissection of a live torpedo to test how the ability to produce the shock would be affected by the operation. However, the experiments did not appear to be conclusive, and Walsh abandoned this line of investigation. Moreover, it was not alluded to in the published paper. The skin of the animal was progressively removed from different parts of the animal body, and it appeared initially that no shock could be felt when either finger or hand touched the naked parts of the animal body. However, on repeating the

[87] In the letter to Franklin reported in the 1773 article of the 'Philosophical Transactions' there is «direct» instead of «convey».

experiment this negative result was not confirmed, because the shock could be felt with both the hand that touched the intact surface and with the one touching the other part. Walsh seemed apparently unable to draw any clear conclusion from these experiments, and, from the footnotes to the page, he seems to be unaware of the exact circumstances in which they were performed.

As a matter of fact, besides the general conditions of the preparation, a critical aspect of such an experiment was the innervation of the electric organs. The production of the shock is under the control of the nervous system, and in the circumstance of the experiments it was largely the consequence of a reflex activity. No shock could be produced in the absence of a functional integrity of the organs' nerves. This will emerge clearly during the researches of Luigi Galvani in 1795, published in 1797, although some cues to the importance of nerves in the production of the electric discharge had been provided by previous research by Lazzaro Spallanzani published in 1783 and 1784. Among Walsh's observations concerning the dissection of this live torpedo, he saw that the animal was able to give a shock after taking out the heart. It is perhaps worth considering here that in his experiments on the torpedo, Luigi Galvani showed clearly that the ability of the torpedo to give a shock persisted for a rather long time after the removal of the heart, whereas it disappeared suddenly upon cutting the nerves of the electric organs.

The last experiment of this long day was performed at night, and it did not involve a torpedo, but a charged Leyden jar. It was one of those experiments with artificial electricity that Walsh, in his continuous shifting from physiology to physics, used to undertake to ascertain the correspondence between the shock of the 'animated Phial' (the torpedo) and that of the physical one (the Leyden jar). Walsh was evidently intrigued by the fact that, in several circumstances, the shock of the torpedo was not felt by all the elements of the human chain connected to each other. This happened only rarely with the shock produced by artificial electricity in ordinary circumstances. From the experiment that he devised and carried out on that night of July 10th, one can argue that Walsh's opinion on the partial failure with the fish shock was evidently that the electricity of the torpedo is weak. He used a Leyden jar connected to a Lane's electrometer to generate «Small Electric Pulsations» (i.e. weak shocks). The results were the following:

> Seven persons holding Silver spoons, and the extreme touching small Wires, connected with the in, and outside Coatings, were unequally affected. (WE, 65 L.H.).

The difference depended on the different individual sensibility of each person, and also on their position in the chain. Walsh drew the following important conclusion from this experiment:

> In weak Shocks therefore from the Leyden Phial, as well from the Torpedo, Persons in the Circuit are not all equally affected, nor are they all sensibly affected in any degree whatsoever. (WE, 66 L.H.)

We will see, however, that in the prosecution of his study, Walsh will change his mind somewhat as to the intensity of the torpedo shock, because when compared to the shock of physical devices, it seemed to be weak in some respects, but strong in others. The reasons why the torpedo could produce strong commotion with its shock, but did not result in the production of sparks and other evident signs of electric discharge, would be tackled by Walsh, and by Cavendish after him, in such a way as to lay down the grounds for an important advance in the physical notions of electricity.

The experiment with artificial electricity was made in a situation in which the seven people making up the chain were not insulated from the ground, even though, as Walsh wrote in his journal, they were all situated «on a boarded floor above stairs». After his description of the experiment with artificial electricity, he added:

> N.B. To try this Experiment when all the Persons are insulated, to judge what escapes by the floor. (WE, 66 L.H.)

This note is interesting because it shows that, after demonstrating the similarity of the conduction of the shock of the torpedo and the Leyden jar, Walsh was able to try to investigate whether the particular behaviour observed could be accounted for by the known laws of electricity (a partial shunt to the ground, in this case, in the modern usage of the words). Again Walsh uses physics to clarify the mechanism of physiology. In the continuation of the study of electric fish mechanisms, by Walsh, Cavendish and Volta, we will see how the terms of the relation can be reversed, since as Walsh put it in his 1773 article:

> as artificial electricity had thrown light on artificial electricity on the natural operation of the Torpedo, this might in return, if well considered, throw light on artificial electricity.

This passage is actually a re-elaboration of the concluding words Walsh used at the public demonstration of his experiments on the torpedo during the session at the Academy (held for this purpose on July 22nd; see later):

> as artificial Electricity has led to a discovery of some of the operations of the torpedo, the Animal if well considered would lead to a discovery of some truths in artificial Electricity which were at present unknown and perhaps unsuspected. (WE, 145 L.H.)

In both forms, this final statement sounds somewhat prophetic. From the attempt to account for the phenomena of the torpedo, and particularly for the impossibility to obtain sparks and other evident ‘electrical signs’ from it, notwithstanding the strength of the commotion it produced, there will emerge, particularly in the work of Cavendish, an anticipation of the concepts of tension and quantity of charge and of the laws of capacitors. These notions, to be fully developed by Alessandro Volta in 1782, are one of the most important conceptual advances of the 18th century for our understanding of the physical laws of electricity.

In the version reported in the journal of the experiments, the statement conveys the evident confidence of the scientist of a revolutionary age, as Walsh’s undoubtedly was, in the future discoveries of science. The concluding words recall, even in their syntactic structure, the confidence in the progress of science expressed about a century before by Marcello Malpighi, a scientist who made important contributions to the scientific revolution of the 17th century in the domain of the life sciences. In his *De viscerum structura*, first published in 1666, on referring to the microscopic machines that in his mechanical conception of the body functions underlay the most delicate functions of the animal economy, Malpighi had written:

> machines will be eventually discovered not only unknown to us but also perhaps unimaginable to our mind. [88]

Coming back to Walsh's concern with the diversity of the effects of the torpedo's shock on the different people making up the chain, he will return to the subject, assuming that it might depend, among other things, on the multiplicity of the circuits that could be established, particularly when the elements of the chain were not insulated with respect to the ground.

A VERY LARGE TORPEDO AND THE CONDUCTION OF THE SHOCK THROUGH THE FISHING NET

In the afternoon of the next day (July 11th) a very large torpedo («14 ½ Inches broad and 21 long, French measurement») was brought to Walsh by Gabriel Coyau, another of the 'providential' fishermen who supplied Walsh with the precious fish (and whose name is mentioned in the journal). Due to the rather exceptional size of the torpedo, Walsh seems to be particularly eager to learn about the details of its capture, reported in the journal as follows:

> Coyau , on drawing his Nets felt a Shock when the Torpedo was about 12 feet distant, and two or three Shocks more before he got it into his Boat. (WE, 67 L.H.)

This particular episode will be alluded to by Henry Cavendish in his 1776 article on the 'artificial torpedo', where he tries to account for the possibility that the fish shock could spread at a distance through water and other conductors. Cavendish wrote:

> One of the fishermen that Mr. WALSH employed assured him, that he always knew when he had a torpedo in his net, by the shocks he received while the fish was at several feet distance; in particular, he said that in drawing in his nets with one of the largest in them, he received a shock when the fish was at twelve feet distance, and two or three more before he got it into his boat.

We may note *en passant* that Walsh's journal of experiments was probably the source for the details of this episode referred to by Cavendish, details which correspond with great exactitude to those of the journal («12 feet [...] two or three more Shock»). It is very likely, moreover, that Walsh was alluding to the large torpedo brought by Coyau in his 1774 article 'Of Torpedos found on the Coast of England':

> The largest Torpedo I met with in the neighbourhood of La Rochelle, where upwards of seventy passed through my hands, weighted little more than ten pounds, and measured not two feet in length, nor quite sixteen inches in breath:

Notwithstanding its large size, this 'French' torpedo was much smaller than the «enormous British Torpedo» captured in Torbay that Walsh was able

[88] See MALPIGHI 1686 (p. 98 , *De Renibus* Cap. VI).

to study anatomically in November 1773, and which was the subject of his 1774 article:

> This I weighted and measured before it was touched by the dissecting knife, and found it to weigh fifty-three pounds avoirdupois, and to measure four feet in length, two feet and a half in its extreme breadth, and four inches and half in extreme thickness.

Coming to Walsh's researches on the torpedo of La Rochelle, the possibility that the fish when in water could give an actual shock from a distance was one of the most puzzling problems for Walsh as he continued his research. As he will express it in the published 1773 paper, using words already present in the journal of the experiments, the «action [of the Torpedo] in water was a capital desideratum». Water was the habitat of the fish and thus the medium in which it would make use of its electrical power in natural circumstances. However, it appeared difficult to account for an action of the shock at a distance through water, because, in the opinion of 18^{th} century electricians, all the electrical fluid produced by the animal would flow through the layer of water immediately surrounding its body, and nothing would reach distant objects.

To be sure of what Coyau had really felt on drawing in his nets, Walsh repeated the experiment already done with the first fisherman encountered at La Rochelle who had positively reported to have experienced the fish shock (see above) with him:

> Gave Coyau some Shocks from the Leyden Phial, a strong one jumping to the Electrometer two lines, he said was precisely like the Shock this Marote gave him, which affected him to the Shoulders.(WE, 68 L.H.)

On a second occasion, probably on his return to England, at the time he was trying to establish some quantitative correlation between the animal and artificial shock, Walsh tried to remember how big the Leyden jar used on this first occasion was but he was unable to, and he probably regretted it. With a coarse pencil stroke we find in the journal:

> The Phial contained ab^t .
> NB. The Phial on recollection is guessed to contain ab^t. [blank] Inches of coating. (WE, 68 L.H.)

The large torpedo brought by Coyau did not survive until the next day (Sunday 12^{th}). The only fish alive then was a «small Torpedo languid». Although this fish «gave very feeble shocks», it was tested by Walsh and by his collaborators in an experiment in which four people separately (and in parallel) put a finger above and a finger below the fish, in the zone of the electric lobes, two on each side. All the people:

> felt Shocks at the same instant; frequent, small, but distinct; As frequent as the Beat of a Pulse.(WE, 69 L.H.)

It is possible that Walsh performed this experiment to examine the possibility that the electric fluid emitted by the torpedo at the moment of the shock could take various routes. The concomitance of the commotion felt by the four people composing the chain, suggested to Walsh that all the cylinders composing each electric organ, and possibly also those of both organs, were discharged at once. And that, furthermore:

> fresh Electricity is somehow generated by the Animal in the interval of the Shocks, but most probably at the very instant of discharge. (WE, 70 L.H.)

The idea that the electricity might be generated «at the very instant of discharge», an idea that Walsh put forward here for the first time in the journal, is important because it helped him to envisage how the animal could manage to use electricity for the discharge, without dissipating the electric disequilibrium necessary to produce it through its tissues or through the conductive medium in which it lived. According Walsh it was reasonable to suppose that the electric disequilibrium did not exist in the animal in the interval between the shocks, but was generated at the very instant of their production. Walsh will develop this hypothesis, trying to give a likely explanation of how the animal could so rapidly produce the electric disequilibrium, based on a pneumatic analogy. It would then also be possible to explain why the fish shock failed to produce signs for the electrometers of the age (see later).

THE FIRST LETTER TO FRANKLIN: COMMUNICATING THE ELECTRICITY OF THE TORPEDO OUTSIDE LA ROCHELLE

The page of the journal concerning Sunday July 12th ends with the following observation:

> Wrote to Dr Franklin, acquainting him with the Results of our Experiments. (WE, 70 L.H.)

As we have already mentioned, this letter to his illustrious colleague was for Walsh the occasion to fulfil the commitment made to Franklin on June 21st with his first letter from Paris. Less than 20 days from that initial letter, and a little more that two weeks from his arrival in La Rochelle, Walsh was thus able to draw sound conclusions from his experiments. He thus felt confident about communicating the news that the torpedo was electrical to a circle larger than the small dinner party at La Rochelle. Through Franklin, Walsh was indeed beginning to inform the entire ‘Republic of Letters’ of his achievement, since this letter would be included, although in a slightly modified form, in the article published in 1773 in the ‘Philosophical Transactions’ of the Royal Society (Fig. 5). In one of the last of the concluding passages of the letter, Walsh asked Franklin:

> to acquaint Dr Bancroft of our having thus verified his Prediction concerning the Torpedo. (WE, 72 R.H.)

The electricity that Bancroft had assigned to the «Torporific Eel» after his experiments in Guiana, did not appear to belong exclusively to this exotic fish, but was also the basis of the shock of the torpedo, a shock that Réaumur (and Redi and Lorenzini before him) had attributed to a mechanical effect. The «Torporific Eel» will return to the stage of these researches on electric fish, and will allow Walsh to provide further evidence for the electric nature of the fish shock, evidence so decisive as to convince all the natural philosophers of the

18^{th} century, and to have a profound influence on the progress of physiological and physical studies of electricity in the last part of the 18^{th} century.

In the letter to Franklin, Walsh expresses his conviction:

> that the effect of the Torpedo appears to be absolutely Electrical, by forming its circuit through the same conductors with Electricity, for instance metals, Animals and moist Substances; and by being intercepted by the same non-conductors, for instance Glass, and Sealing wax. (WE, 71 L.H.)

In the published article this passage is reported in a slightly modified form:

> that the effect of the Torpedo is absolutely electrical; by forming its circuit through the same conductors with electricity, for instance metals, and water; and by being intercepted by the same non-conductors, for instance glass, and sealing wax.

Apart from minor stylistic modifications, the change at the beginning, from «the effect of the Torpedo appears to be absolutely Electrical» to «the effect of the Torpedo is absolutely electrical» very likely expresses the greater confidence in the electrical nature of the torpedo shock acquired by Walsh in 1773 (and possibly by Franklin too).

As well as on the different transmission of the shock through electrical conductors and insulators, Walsh's conclusion was based on the similarity of the effect produced by the torpedo and Leyden jar:

> The sensations they occasion likewise in the human frame are precisely similar. There is not an engourdissement or fourmillement of the Torpedo that we do not most exactly imitate with the Phial by means of Lane's Electrometer. (WE, 71 R.H.) [89]

At the same time, however, Walsh wrote to Franklin that in his experiments he had not yet succeeded in obtaining from the shock of the torpedo any spark or any effect sensible to the electrometer. The lack of such effects - he suggested - might be due to the scarce vitality of the torpedoes studied at La Rochelle, because they very rarely produced a commotion extending beyond the touching fingers. The experiments - he says - were in progress, and perhaps more conclusive results would be obtained at the Isle de Ré, the next place he planned to go to in his search for torpedoes «fresher taken and of more vigour».

WALSH AT THE ISLE DE RÉ: SEARCHING FOR MORE VIGOROUS TORPEDOES

Most of the new experiments were indeed carried out in this small island, situated at a distance of about three kilometres from the coast of La Rochelle. In the Isle de Ré, with the help of Captain Bernard and his crew, and of other local fishermen, Walsh was eventually able to obtain active torpedoes, useful to gain further insight into the electric nature of their shock.

[89] Text of the letter in the article: «The sensations likewise, occasioned by the one and the other on the human frame, are precisely similar. Not only the shock, but the numbing sensation which the animal sometimes dispenses, expressed in French by the words *engourdissement* and *fourmillement*, may be exactly imitated with the Phial».

On Monday 13th, with some of his collaborators, Walsh moved to the Isle de Ré in Bernard's boat, finding comfortable lodgings «at Madame Riffauds» in a house «near half a mile» or «within Ten yards» from the sea, depending on the impressive tides typical of the Atlantic coast in this region. (WE, 73 L.H.)

Torpedoes are easily found at the Isle de Ré, but the first ones available did not appear to be particularly lively or strong in their shocks. Finally, on July 15th Bertrand brought three large torpedoes, and one of these produced shocks strong enough to allow for transmission in larger human chains than previously at la Rochelle. This torpedo was initially tested on board of Bernard's barge in a first experiment that Walsh described as follows:

> Joined hands, five Persons in Boat, fish in Boat; All felt it; Arthur, the Extreme; touching Back, and happening to stand on a Wet Place near the fish and with wet feet, felt it very strong and heavy in the touching arms; Walsh, the other extreme, touching Belly, felt it in the finger only. (WE, 78 L.H.)

A second experiment was then tried in which the chain was extended to «7 Persons holding hands; Arthur in the middle did not feel it; the others did in the arm next the fish». It seems that, as frequently happens in experimental science, when a success is obtained (five people, one more than in La Rochelle, felt the shock), it is followed soon afterwards by a partly disappointing result (again the middle person, Arthur, did not feel the shock, although he was usually the most sensitive to the fish's shock among Walsh's collaborators).

The event is especially interesting because it was the occasion for two observations written at a distance of about one year, one on the same day as the experiment (July 15th 1772), the other in July 1773 (with no indication of the day). The first observation goes as follows:

> Remark 15.th July 1772.
> To observe in future very particularly as to the Center person being unstruck, and the others struck in the Arms next the fish. (WE, 78 R.H.)

The second runs:

> See Exp. N°. [blank] where the Point is determined that every Person in the Circuit is equally struck, but that the electrick effect often makes more Circuits than one - (WE, 78 R.H.)

The difference between these two observations suggests that after this experiment, Walsh became aware of another possibility to explain why some people in the human chain might not feel the electricity, while the others were affected. This might not depend exclusively on differences in the individual sensitivity of the members of the chain, but could also have to do with the possibility that the fluid took various and unexpected routes and thus bypassed, more or less completely, some members of the chain.

This second observation bears the indication «July 1773», and was thus written at the time of the presentation to the Royal Society of the communication concerning Walsh's experiments on the torpedo (1st July 1773), that eventually led to the article published in the 'Philosophical Transactions'. A long paragraph of the article concerns the possibility that, in its flowing, electricity could form «distinct circuits», and that it could «be divided and subdivided in different channels». Walsh had become progressively more aware of the possibility that electricity could take different routes depending on the spatial arrangement of conductors along a circuit, and that it could flow along unexpected paths, particularly when the elements of the circuit were not insulated with respect to the ground, as in the first experiment of July 15th 1772 at La Rochelle. This possibility emerged again in a particularly clear way in the last experiments to be carried out at La Rochelle (see later).

It could thus be imagined why the central elements of a chain of people might not feel the shock, while the others, situated nearer to the source of electricity, were affected by it.

Other experiments were carried out on July 15th on the same large torpedo, after it had been transported home, in the «grenier» («the loft or Garret of the House» as Walsh explains). In a first experiment made with the animal «placed on a Table with a wet Cloth under him»:

> Six Persons insulated, one extreme touching Back, the other the wet Cloth; all sensible of Shock. This repeated. Depression of Eyes always attended the Shock. Two communicating with sealing wax, no Shocks. Touching thumbs, an immediate Shock.(WE, 79 L.H.)

With this experiment Walsh had increased by one unit his highest score as to the number of persons feeling a shock in a human chain. Eventually he was to arrive at seven or eight, during his last days at La Rochelle.

After the experiment with the chain of six people, Arthur connected himself by one hand, through a metallic wire 13 feet long, to the back of the fish, and by the other hand, through a similar wire, with the wet cloth on which the fish was laid (probably with the ventral surface touching the cloth). He «felt the minutest Shocks, Repeatedly».

Among the other experiments made on that same torpedo, there was one in which, there being «Fish and Person insulated», fifty consecutive shocks could be felt by taking one side of the fish «between finger and thumb». Interestingly the phenomenon occurs with «little or no diminution of the force of the Shocks» (WE, 80 L.H.). Probably Walsh was also taking this experiment into account when, in his second letter to Franklin from Paris on 27th August 1772 (and inserted in the 1773 published article), on speaking of the experiments made at the Isle of Ré, he wrote:

> I observed you in my last [letter], the singularity of the Torpedo being able, when insulated, to give an insulated person a great number of successive shocks: in this situation I have taken not less that fifty from him in the space of a minute and half.[90]

SCIENTIFIC EXPERIMENTS AND MILITARY VIGILANCE

Unexpected events, however, suddenly seem destined to force Walsh to abruptly terminate his experiments that day, and perhaps end all his research on the torpedo. For a moment, this relatively idyllic scene of science ‘in the field’, made up of an English gentleman and his companions, the ‘local’ learned people that collaborate with him, the fishermen that eagerly capture fish for his investigations, with experiments made on a barge or in a ‘grenier’, all this seems destined to end, due to an unforeseen *coup de theatre*.

In the afternoon:

[90] See footnore 82.

> A message by a soldier arrives from M. De Tailler, Lieutenant du Roy, que J'ai la bonté de luy aller parler aujourdhui, a S.[t] Martin, et que Je ne rien observe le long de la Côte. (WE, 80 L.H.)

Although communicated with the polite manners of the *ancient régime*'s military style, this message meant that Walsh was suspected of being a spy, visiting the Isle de Ré possibly in order to study the geography of the island for military purposes, and to have information about its fortifications. Indeed, after a short time, «a Corporal, de la part du Commandant du Fort de la Prée», arrived, forbidding Walsh to take measurements of the Coast, and advising him that, should he not go to see the King's Lieutenant that evening, he would be arrested.

The termination of this rather dramatic *intermezzo* in Walsh's researches at the Isle de Ré is abrupt and apparently unjustified, as was the beginning. After a rather long wait, Walsh was eventually received by the King's Lieutenant, M. Tailler. He was asked to show his passport, and on ascertaining that he did not have one, was ordered to leave the island. However, things changed, since - as Walsh noted in the journal - «without any intercession or concession on my part», the Lieutenant then let him know he could stay. During his last days at La Rochelle, this episode would be the subject of a conversation with M. le Baron de Montmorency, military Governor of La Rochelle, «with great civility on each side», during a dinner in honour of Walsh held at the palace of the «Gouvernement», and was considered by the French gentleman as the consequence of a «Mal-entendu» (misunderstanding) (WE, 148 L.H.).

FORCING THE SHOCK TO PASS THROUGH A SMALL GAP: NO SPARKS FROM THE TORPEDO

On July 16th the experimental activity continued with its usual intense rhythm and with its variety of trials and usual mixture of success and failure. A freshly captured torpedo that seemed very lively nevertheless refused the shock on many occasions leading Walsh to observe:

> The stronger the animal is, the more retentive of his Shock has appeared to be, or at least capable of such retention. (WE, 83 L.H.)

This torpedo, however, once taken home and studied in the 'grenier', gave «tolerable shocks to finger and thumb» and was used for an experiment with the Lane's electrometer, a component of Walsh's electric apparatus. This experiment was evidently intended to see whether the shock produced by the fish could pass across a minute gap of air (i.e. across the separation between the electrometer and the conductor of the machine, a separation that could be controlled in a precise way with the micrometric regulator of Lane's device). With this system, however, whenever the electrometer and conductor «were in the least degree separated, no Effect could be obtained». There were similar results from another torpedo (N. 40) just caught and studied in a condition of insulation with respect to the ground:

> felt the Shock when the Electrometer was in contact; not else. (WE, 81 L.H.)

These results indicated that the fish shock could not pass through the smallest separation in a conducting circuit, a finding that, even though it may appear obvious to us, was difficult to reconcile with the electrical notions of the times. As already mentioned, the electricity commonly produced by electrical machines in the 18th century was characterised by very high voltages (usually more than 10.000 Volts), and could thus easily pass across relatively wide gaps in the circuit of the discharge (of the order of centimetres or more in some conditions). This was because the high voltages led to the ionisation of the air in the gap and consequently to a transmission that was accompanied by a series of manifest events such as sparks, sounds, and odours. These events were considered hallmarks of genuine electricity, and their absence cast uncertainty on the electric nature of the event. Besides producing sparks, Walsh had also been unsuccessful in forcing «torpedinal electricity» to jump across the small separations of the Lane's electrometer in an invisible or otherwise inconspicuous way.

The problem of spark production was dealt with in the next torpedo examined the same day, a «Small Male Torpedo [...] very vigorous and active». Walsh tried an additional protocol in order to force the passage of the shock through a very small gap in the circuit so as to get sparks, but again the experiments proved to be unsuccessful:

> Many endeavours to get the Spark, by means of the minutest separation, made in Tinfoil pasted on Sealing Wax, and the smallest removal of the Electrometer from the Conductor, but without Effect: No Spring of the Electric fluid would ensue: no Spark; no ticking noise. (WE, 86 L.H.)

The result thus appeared to be negative in such a clear-cut way as to exclude any doubt, and as to make any explanation based on some accidental circumstance unlikely. Everything seemed to conspire to exclude complete success for Walsh's endeavours to show the perfect correspondence between the genuine electricity and the particular 'fluid' involved in the shock of the torpedo.

As already mentioned, this apparent failure would turn out to be the grounds of one of Walsh's main contributions to electric fish researches.

In his demonstration of the similarity between the shock of the torpedo and the Leyden jar in producing the same effects and sensations «on human frame», and in being transmitted by the same conductive materials and intercepted by the same insulating materials, Walsh's results on the torpedo undoubtedly superseded the anecdotal reports published by various natural philosophers and amateur scientists of the 18th century, because they represented the outcome of a systematic and rigorous experimental investigation. However, to a large extent they constituted a confirmation and an extension of previous observations.

On the other hand, the way in which Walsh, and after him Cavendish, faced the problem of the negative signs of «torpedinal electricity» (the apparent impossibility to obtain sparks from the shock of the torpedo and to force the shock to pass through «the minutest separation» in the circuit of discharge) represents, a totally new contribution to the advance of electric science, in the field of both physiology and physics. Furthermore, the 1775 experiment in which Walsh would be successful in drawing a spark from the discharge of an electric eel was based on exactly the same protocol employed with the «Small Male Torpedo [...] very vigorous and active» of 16th July 1772.

This success, accountable on the basis of the much higher voltage involved in the discharge of the eel (up to 600 Volts) compared to that of the torpedo (usually less than about 50 Volts), will sanction in the most brilliant way the conclusion of Walsh's efforts to dominate the mysterious power of electric fish, leading him on 9th July 1772 to write in his journal of experiments «*Je l'ai donté*». That the eel produced a stronger shock compared to the torpedo was also manifested by the fact that its shock could affect a chain of 27 people, while the torpedo's shock was not able to pass through a chain of more than 7-8 people.

WALSH'S REFLECTIONS ON THE ELECTRICITY OF THE TORPEDO

With his experiments at the l'Isle de Ré on 16th July 1772, Walsh was probably convinced at that moment of his investigation that a first phase of the study of torpedo shocks was concluded, and that future experiments would mainly confirm and give more precision to the results already obtained. He thus deemed it appropriate to summarise the main conclusions to be drawn from the experiments performed up to that time. This he did by listing a series of «Reflections»:

> That its is necessary to have a communication between the upper and lower Surfaces to have a Shock.
> That Metals, Human Bodies, and Wet Substances are Conductors, Metals preferable to the other two.
> That Glass, and Sealing Wax, intercept the Effect.
> That the animal gives the Shock when he pleases.
> That he gives it, not from a Spot only, but from the whole of the two Honeycombs [i.e. electric organs] at the same Instant: proved by four persons touching him at the same time, above and below; forming each a distinct Circuit. E. E. N. 171.
> That he gives it also from that part of the Skin of his Back, which lies between the two Honeycombs.
> That the upper Skin of the two Honeycombs, and the part between them, seem to be of conducting nature.
> That he renews in himself, insulated on Glass, the impelled fluid.
> That he is capable of doing this fifty times successively by our Experiences.
> That the Intervals being equal, the force of the Shock will be equal; longer interval occasioning stronger, and shorter interval weaker shocks.
> That the fourmillement is an impotence in the animal to give the full Shock; It often follows a heavy shock. It is nothing but numberless small consecutive Shocks.
> That the whole of the Torpedo is a weak Electricity. (WE, 86-88 L.H.)

We have already discussed most of the points covered by Walsh in his «Reflections» of July 16th. What deserves some comment here is his allusion to the power of the fish to renew «in himself, insulated on Glass, the impelled fluid». The way Walsh expresses this concept, especially the phrase «insulated on Glass», suggests that he was considering, albeit implicitly (and rejecting it at the same time) the alternative possibility that the fish might absorb the fluid involved in shock production from outside. Although this possibility would seem totally unlikely in modern times, this was not the case in the second half of the 18th century.

As a matter of fact, the dominant theories of the age concerning the involvement of electricity in physiology and medicine were based on the idea that an electric fluid present in the atmosphere was able to penetrate in the bodies of animals (and plants as well), and in this way influence the activity of the organisms. This was the view of the French scientist Pierre Bertholon and the Italian doctor Francesco Gardini, in 1780 both awarded an important prize for their Memoirs on the relation between electricity and medicine by the *Académie* of Lyon. According to Bertholon:

> this fluid [i.e. electricity] so active and penetrating cannot exist in the atmosphere which surrounds us, and in which we are immersed as fish in the water, without acting on us and thus without having on our bodies some influence.[91]

In the theory of both Gardini and Bertholon, electric fluid was useful for the organism (in a state of health) because of its own properties, independently of the existence of physiological mechanisms capable of putting it into a state of disequilibrium. Although the fluid might be modified inside the body, for instance as a consequence of its contact with the blood and with other fluids, and made perhaps more pure and more adapted to physiological needs, it acted therein with its intrinsic force. As a matter of fact, for Gardini and Bertholon, the electric fluid present in the universe and in the atmosphere was somewhat a re-edition of the elemental fire, one of the four elements of old physics, and indeed it was often indicated as 'electric fire'. The involvement of the electric fluid in physiology and medicine was, in many respects, a revival of the theory of the atmospheric (and astral) influences of classical and medieval medical science.

On the contrary, in the case of the electric fish, the analogy Walsh put forward between the animated Phial (the torpedo) and the physical Phial (the Leyden jar) contained in itself the idea that the fish was endowed with an intrinsic mechanism capable of generating the electric disequilibrium necessary to the action of this fluid (i.e. a mechanism capable of putting «the back and the breast of the animal in different states of electricity»).

Similar to the Leyden jar that needed an electric machine to be charged, the animated Phial required an appropriate device to be charged and to work. However, the fish had this mechanism «in himself», since it could repeatedly discharge shocks even when it was insulated, and thus independently from an external action. What was impressive with the fish compared to the physical device was its ability to re-charge the mechanism responsible for the shock so

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[91] Bertholon 1786, vol. I, p. xiv. Pierre Bertholon (abbé, 1742-1800) professor of physique in Montpellier and afterwards of history in Lyon, was a friend of Franklin. Besides studying the effects of electricity on animal and vegetal organisms, he wrote about a series of subjects of practical relevance (how to prevent firing, on alcoholic fermentation, manufactures, etc.). Francesco Giuseppe Gardini (1770-1816) an Italian doctor of Piedmont, was a pupil of Giovan Battista Beccaria (1716-1781), one of the greatest 'electrician' of the 18th century and the main supporter of the Franklinian theories on electricity in Europe. In one of his experiments performed in Genoa, Gardini mentioned to have obtained a spark from the shock of a big torpedo (but this appeared to be an exceptional event): «[...] *una tantum vice in concussione scintillam vidi, crepitum audivi, & adstantes ipsi quoque observarunt, tunc vero Torpedo non erat in aqua, sed posita supra scamnum electricum, & manu una eandem tangebam, dum digitum alterius manus propre locum medium inter cornua, quae extendebat, appropinquabam, & statim per spatium, quod intererat inter digitum, & corpus torpedinis transiit scintilla dicta crepitans; expertus quoque sum, quod hoc animal ad animae voluntatem percutiebat*».[«[...] only once during the shock I saw the spark, and I heard the noise, an even the attending persons also observed it; the Torpedo indeed was not in water, but placed on an electric stool, and I touched it by a hand, while I approached the finger of the other hand in a place in midway between the extended lobes; and suddenly across the gap between the finger and the body of the torpedo the mentioned spark passed with a crackling noise; I am also convinced that the animal produced the shock by a voluntary action». (Gardini 1792 pp. 100-101). An unequivocal and reproducible generation of a spark from a torpedo's shock was obtained in the first half of the 19th century by Santi Linari (1836 and 1838) and by Carlo Matteucci (1836 and 1844).

rapidly, so as to give on some occasions up to fifty shocks «in the space of a minute and half».

Walsh would refer to a pneumatic analogy in envisioning, in the body of the fish, the presence of a mechanism leading to the electric disequilibrium necessary for shock generation. This idea of an intrinsic production of an electric disequilibrium, that appears implicitly in the journal of experiments as a short 'Reflection', is an important step forward in the progress of the electrophysiological conceptions of the 18th century. It will be central to Luigi Galvani's idea of an electricity present in the body of frogs and other common animals, according to the model of the muscle as an animated Leyden jar. Galvani will refer to the notion of the machine that he derived from the mechanical theories that dominated life sciences in the 17th century, although his machine was not the mechanical machine of Borelli, Malpighi or Baglivi, but a new type of device, inspired by the electric science of the 18th century. With reference to the electric disequilibrium responsible for muscle contraction in his animated Leyden jar model, Galvani wrote in 1794:

> Such a disequilibrium in the animal either must be there naturally or should result from artificium. If it is there naturally, we should admit that in the animal there is a particular machine capable of generating such a disequilibrium, and it will be convenient to refer to this form of electricity as to an animal electricity in order to denote, not an electricity whatsoever, but a particular one referred to a particular machine. [92]

The idea of such a mechanism, of a machine capable of generating an electrical disequilibrium, that in a entered the field of electric researches into living organisms through Walsh and Galvani, prepared the terrain for the foundation of modern electrophysiology. Galvani would not put forward any hypothesis on how his animal electric machine might work. He wrote:

> But what will it be this animal machine? We cannot establish it with certitude; it remains totally occult to the most acute sight; we can do nothing else than figure out its properties, and, from these, somewhat envision its nature.[93]

At more than two centuries' distance from Galvani, we know the details of the structure and function of the hypothetical machine that seemed to be «totally occult to the most acute sight» at the time of the Bologna doctor. This is the cell membrane, with its ionic pumps which create dissimilar concentrations of Na^+ and K^+, with ion selective channels capable of converting the concentration differences into electric potential.[94]

TORPEDO ELECTRICITY: WEAK OR STRONG?

For Walsh and his «Reflections» of July 16th 1772, it is interesting to note that after the last Reflection (»That the whole of the Torpedo is weak Electricity») in the journal of experiments there is an important observation added later, as a «Remark of a future date»):

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[92] GALVANI 1794, pp. 70-71.
[93] GALVANI 1794, p. 76.
[94] See HILLE 2002.

> The Torpedo often gives severe Shocks, his Electricity therefore cannot be deemed weak. May he not have a better Conductor in his Skin, than in any Space, however small. (WE, 88 L.H.)

We don't know exactly when Walsh added this observation to his journal. It is evident, however, that in elaborating his conception of the torpedo shocks as «absolutely Electrical», he did eventually become aware that there was an important obstacle to the perfect similarity between the effects of the torpedo and of the Leyden jar, an obstacle that could not be removed by invoking such a simple explanation as one based on the intensity of the phenomenon.

The shock of the torpedo could be «severe», in terms of the commotion and sensation it produced on the «human frame», yet at the same time it also seemed to be weak, since it was unable to pass across the «minutest separation» in a circuit represented by a small air gap. The shock of a charged Leyden jar, on the other hand, passed across rather wide air gaps without apparent difficulty. Only when the jar was almost completely discharged did its electricity fail to traverse minute separations in the circuit. However, in these circumstances no shock was commonly felt at the moment of reuniting the circuit of discharge. There thus appeared to be an insuperable difference, something of a dilemma: if the torpedo was supposed to correspond to a strongly charged Leyden jar, it was difficult to account for the blocking of shock transmission by a small interruption of the circuit. Conversely, if it was compared to a weakly charged jar, it was difficult to account for the severity of the shocks it might produce.

This type of difficulty accounts for the contrast in the two comments in the journal, one coming immediately after the other:

> That the whole of the Torpedo is weak Electricity
>
> The Torpedo often gives severe Shocks, his Electricity therefore cannot be deemed weak. (WE, 88 L.H.)

As already mentioned, it is just from reflections on this type of difficulty that would emerge the most important theoretical contribution of Walsh (and Cavendish after him) to the electric science of the 18th century.

After writing down his «Reflections», Walsh continued his experiments with Arthur in the afternoon of July 16th on new torpedoes. In one of these experiments, Arthur felt a shock by holding a metallic wire 28 feet long in one hand, and in the other a 13 foot long wire, connected respectively to the lower and upper surface of one of these fish.

The next day (July 17th) was another day of intensive work for Walsh and his collaborators. Interestingly, in some experiments to ascertain the conduction of the torpedo's shock through a chain of people, Walsh introduced a small modification that would result in a greater effectiveness and constancy in the circulation of the electric fluid. Instead of connecting each other either directly by joining hands, or through metallic spoons, the different members of the chain connected to each other by putting a finger or the hand in «Delphi ware Basins of water».

Initially the chain was made up of three people insulated from the ground:

> all three felt the Shock in the touching finger of each hand. - Repeated the experiment several times, without it's failing once. (WE, 94 L.H.)

The experiment succeeded also if the centre person (Arthur) was not insulated. Or if, being insulated from the ground (by «standing on the Board supported by Glass Tumblers»), he connected himself to the other members of the chain via two wires, one in each hand, dipped at their extremes in the water of the two basins. The elements of the chain were increased to four, and the position of some of them was varied with the following result:

> the Shock felt by all the four in the touching finger of each hand, ten times and upwards; no failure ever, on the Animal's depressing his Eyes. (WE, 95 L.H.)

Walsh remarks that this fish: «was capable of giving to a single person, touching him above and below, but feeble Shocks, not reaching beyond the touching finger» (WE, 96 L.H.). Evidently the new way of establishing the contact between the members of the chain made the transmission easier and more constant, and resulted thus in a more effective circulation of the shock even when it was of weak intensity. As we shall see, it will be used by Walsh in his public demonstration of the torpedo's electricity at the Meeting of La Rochelle Academy held on July 22nd.

After the physiological experiment, Walsh makes a dissection of the electric organs (indicated as «Honeycombs», because of their resemblance to «*rayons de miel*» as Walsh remarks), after boiling and putting them in brandy. He can thus observe the columns that make up the organs, which are compared to the «Goose quill» appearances described by «Rhedi [*sic*] &c.» that retain «much of an Hexagonal appearance» (for the sake of precision, Redi speaks of « big Swan quill»[95] in his description of the torpedo organs).

L'Isle de Ré appears to be very generous in the provision of live torpedoes for Walsh's studies. Early in the evening «Twelve Trembles were brought». We can imagine how glad the local fishermen would be to receive «24 Sous, about 13 pence English» for each live torpedo sold to this liberal English gentleman, when «Perhaps their common price in the Market might be Two pence a piece» (WE, 97 L.H.), as Walsh remarks in the journal. One of these torpedoes was used in an experiment in which six people «uninsulated» made a chain by communicating with each other again through the water basins. They all:

> felt the Shock, 25 successive times, each person in the touching fingers; it appeared but little more than a fourmillement. (WE, 98 L.H.)

TRYING AGAIN TO PRODUCE SPARKS FROM THE TORPEDO

Probably because he is assured by this successful result of the electric nature of the torpedo's shock, Walsh decided to try the experiment of producing a spark from the discharge of the fish again, an experiment that had never succeeded in previous trials (and was never to succeed in his future investigations into the torpedo). Curiously, at the beginning the experiment was performed by placing two torpedoes, one above the other, piled in a battery-like arrangement, possibly with the idea that in this way the strength of the

[95] «*grossa penna di cigno*», see Redi 1671, p. 54. Walsh was probably mixing memories from Redi's and Steno's works, because it is the last author who actually speaks of «goose quills» in his article on the torpedo (see Steno 1675).

shock might be increased by the summation of the power of each of the two fish. Apart from this singularity, the method was basically similar to the one used two days before:

> A narrow strip of Tinfoil having been pasted on a Stick of Sealing wax, and a very minute interstice made in the Tinfoil, by only drawing the edge of a sharp knife across it; presented the intersected Tinfoil to the upper Honeycomb [i.e. electric organ] on one fish, and the metal Conductor to the under Honeycomb of the other; The Signal being given by the fish, neither Spark in the interstice of the Tinfoil, nor any sensation of a Shock in the holder, ensued. (WE, 98-99 L.H.)

Afterwards the experiment was continued with only one torpedo, but the result was similar:

> Several Signals, but no Spark; no Shocks. (WE, 99 L.H.)

Only when the hand was placed «within the interstice», i.e. in such a way as to establish an electric continuity on the cut lamina, was the shock immediately felt.

After several unsuccessful trials, Walsh had sadly to come to the following conclusion:

> It appears therefore impracticable to make it to take the least Jump or Spring through a non-conducting medium; the separation in the Tinfoil was so very minute, as to be barely discernible to the naked eye. (WE, 99-100 L.H.)

This conclusion is maintained by Walsh even if, in the course of the same experimental session, «Arthur felt the Shock twice through a separation in Tinfoil pasted on Glass». Walsh assumed that this came «possibly from the dampness of the Glass.». This possibility, however, could not be verified because «the glass wiped, the fish could not be provoked to give the Signal». (WE, 101 L.H.)

The arrangement of three torpedoes stacked one above the other in a ‘pile’ was used for another type of experiment showing the sensibility of these fishes to the shock produced by other members of their species:

> Piled three Torpedos one upon the other; touching the under one above and below, the two upper ones gave a Jump when the under one Shocked. (WE, 100 L.H.)

The torpedoes in the pile did not react, however, when a shock was produced by another torpedo placed on the same wet cloth where the others lay, even though the experimenter established a connection with his hands between the cloth and one of the torpedoes.

It is interesting at this point to consider how the arrangement based on the use of different torpedoes in different spatial relations to each other, that Walsh initiated here with his pile of torpedoes, became the basis of other types of investigations. In the experiments just mentioned it was eventually used to reveal the reactivity of a given torpedo to the shock produced by another. In subsequent experiments, based on a variation of the spatial arrangement of two torpedoes, Walsh would try to ascertain whether the electric organ of one of them was able to conduct the shock produced by the other.

This last problem, as we shall see, is important in Walsh's logical elaboration, especially in relation to his conception of the torpedo as an «animated Phial». The Leyden jar, as well as other devices capable of maintaining or producing an electric disequilibrium, was dependant on the presence of an insulating material that would limit the flow of electric fluid (and thus the dissipation of the disequilibrium). If the torpedo's electric organs worked as Leyden jars, then it was logical to assume that they contained, inside, some layer of insulating material, and, as a consequence, would not allow for the passage of an external electricity across their substance. Walsh was thus trying to derive an unknown physiological process from a paradigmatic model of physics.

About three decades after Walsh the mechanism whereby the torpedo produces its shock was at the centre of Alessandro Volta's thinking. Like Walsh, he was convinced of the possibility that physiological phenomena could be explained on the basis of the laws of physics. The solution that Volta envisioned for the functioning of the electric organs of the torpedo was, however, different from the one Walsh had put forward (and other scientists after him, e.g. Cavendish and Nicholson) on the basis of the model of the «animated Phial», and will be based on a mechanism similar to that of the battery that Volta had invented at the end of 1799 (see later).

In one of the numerous experiments made on July 18th, Walsh made an accidental observation that probably contributed to revive his interest in devising experiments to prove that the fish shock could be effectively transmitted by water. In the journal the experiment is described (and commented upon) as follows:

> Raising the head of one of the Torpedos from the table so as to throw Water under him, the tip of the finger was placed just on the outward edge of the Animal near its nostrils: In this situation at the instant the water was thrown from the other hand, Walsh received a Shock. An extraordinary accident that the fish should give the Shock in the moment that the Water had continuity enough to convey it. The Animal could not be provoked to a repetition. (WE, 101 L.H.)

As to the idea of the «extraordinary accident» that, according to the journal, characterised this observation, one may reflect on the importance that accident and chance can play in scientific discovery. However, accidents of scientific relevance usually only happen to 'prepared minds', as Pasteur has keenly remarked, and, moreover, only in those very peculiar circumstances that the prepared minds have created in their laboratory, often during the most intensive phases of their experimental work.

This particular day (Saturday 18th) was indeed a day of very intensive work for Walsh and his collaborators. By deriving the expression from a Memoir on the torpedo written by Spallanzani in 1783, we could say that none of the many live torpedoes of that day was «allowed to die idly», i.e. without contributing a piece of evidence to the experimental investigation. Many different experiments were tried, and in several passages the record in the journal conveys the excitement and rapidity that characterised the most intensive phases of the work. In some cases, in copying into the journal the notes recorded at the very moment of the experiment as «rough minutes» again, Walsh was evidently unable to interpret the experimental details registered correctly, and was hence obliged to recognise in the journal that this resulted in «No conclusion».

In one of the experiments carried out on this day «the animal gave 50 very strong Shocks, and 17 more in another minute; the two last Shocks very strong»(WE, 102 L.H.). Walsh touched the electric organ («Honeycomb») of one side with the thumb and finger of one hand, and with the thumb and finger of the other hand, the organ on the opposite side. As Walsh remarked, in the course of the generation of these shocks, the fish «grew gradually more languid», a comprehensible effect if one considers that the electric organ discharge in the torpedo is an energetically demanding phenomenon, with a great consumption of power at its acme. In this experiment the shock was felt with both hands even «though only one hand stroked [the fish] at a time».

SHOCK AND FISH MOVEMENTS: AGAINST RÉAUMUR'S HYPOTHESIS

As with previous experiments, Walsh tries to characterise the zones of the fish surface from which a shock can be generated. In some experiments two torpedoes are used, placed on a wet cloth, one near the other, but not in contact, as previously in the 'pile' arrangement. A shock was usually felt when the experimenter touched both torpedoes, but not, for instance, when he touched «the upper Surface of both Honeycombs of the same fish». In one experiment in which were «Touched with two fingers of the same hand, both Honeycombs of one fish, and with the two fingers of the other hand, both Honeycombs of the other fish» the result was: «a Shock, repeatedly». (WE, 105 L.H.) Interestingly, both animals gave the shock at the same time, as proved by their simultaneous «winking». Probably one of the fish was reacting within a short delay to the shock produced by the other and communicated to it through the body of the experimenter. Among the experimental variations tried this day were those in which one, or both, of the two torpedoes investigated were placed in a supine or prone position in order to see whether the position could affect the ability to produce a shock

There are also observations concerning the movement of the animal at the moment of the shock, or just before it. Besides the movements of the eye, frequently recorded in the journal, there is a «slight and sudden movement, like a little Catch», in its cartilagineous part, which seemed to be confined «to the Cartilage enclosing the Electric parts». This movement occurred, however, «while the Electric Parts themselves appear totally at rest».(WE, 107 L.H.) Very likely, it was to these experiments that Walsh would allude in his published 1773 article when he wrote:

> The electric effect [i.e. the fish shock] was never perceived by us to be attended with any motion or alteration in the organs themselves, but was frequently accompanied with a little transient agitation along the cartilages which surround the organs.

A important conclusion is recorded in the journal on this same day, soon after the observation concerning the movement of the cartilage surrounding the electric organs:

> The inferior lateral fins, the Tail , and indeed all below the transverse Cartilage which separate the Thorax from the Abdomen, has no concern in the electric operation, but as a Conductor sometimes. (WE, 107 L.H.)

It is possible that Walsh arrived at this conclusion not only on the basis of the experiments showing that the best places from which the shocks can be drawn were the surface of the animal body above and below the electric organs («Honeycombs»), but also because he saw that the movement of the cartilages was restricted to the zone surrounding the organs. Very probably he interpreted this movement as due to the contraction of the muscles of that region induced by the electricity generated by the organs.

With Walsh, things have radically changed with respect to Lorenzini and Réaumur. Being convinced of the electric nature of the torpedo's shock, the English scientist became inclined to interpret a visible movement in the region of the electric organs not as evidence of a possibly mechanical nature of the shock, but instead as an epiphenomenon of the discharge of the organs. Walsh's increasing confidence in the electrical nature of the torpedo's shock emerges also from the expression «Electric parts», that he used for the first time in this case to indicate the structure responsible for the shock, instead of the more usual «Honeycombs» (generally used in the previous pages of the journal of experiments). Two days later, as we shall see, Walsh will start using the term «electric organs», by which they have been known up to modern times (see later).

WHETHER THE TORPEDO IS ABLE TO CONDUCT THE SHOCK

In the continuation of this long day of investigation, Arthur and Walsh repeated the experiment of the shock transmitted through long wires of metal. Arthur succeeded in receiving «a very severe one above the Elbow», by connecting himself to the animal through two wires (one in each hand) measuring together 40 feet. Afterwards, they again tried the experiment with two torpedoes placed one above the other. The lower torpedo was put on a tinfoil. A shock was felt by touching the tinfoil and the back of the uppermost fish, when this one gave the typical signs associated with the discharge of the organs (winking, and slight general stirring). This again meant that one fish transmitted the shock generated by another fish across its body.

This experiment was followed by a series of others, with two torpedoes placed on a wet board, investigated either together or separately, with the apparent purpose of localising (or better of re-localising) the regions of the fish surface from which a shock can be effectively drawn. In the experiments made on one fish alone, no shock was generally felt when the fingers of the two hands (or of the same hand) were placed on the same surface over the electric organs (for instance over the back), while severe shocks were felt with the two fingers touching the back and the belly respectively, with an intermediate strength of shock when one finger was placed over one of the organs, and the other on a fin. On the other hand, with two torpedoes, a shock could be felt even when the fingers of the two different hands were placed on the same surface of the two fish. We know that Walsh will eventually come to the conclusion, through a great variety of experiments, that the «*plus* and a *minus* state» responsible for the shock of the torpedo was generated between the «upper and under surfaces» of the fish. Nevertheless, he seemed unable to interpret the experiments of this day correctly, at least where they concerned the difference of behaviour when two fingers were placed on the same surface of the two electric lobes of a single fish (with no shock felt), compared to when the two fingers were placed on the same surface of two different fish (the shock usually felt). In his journal of experiments he wrote:

> It is remarkable, that while the Skin of a Honeycomb will not conduct the Electricity of the other Comb of the same animal, it is yet capable of conducting the electricity of another torpedo. (WE, 113 L.H.)

Apparently, in this phase, Walsh was unable to work out that in the first situation (one animal) the shock was not felt, not because of a selective lack of conduction of the «Skin of a Honeycomb» for «the Electricity of the other Comb» of the same fish, but because the shock was discharged simultaneously by the two electric organs of the same animal. The corresponding surfaces of the two «Honeycombs» were in the same electric state ('potential' or 'voltage' in modern words) during the shock, so no electricity could flow through the finger of the experimenter that was connecting them. The shock was felt in the second situation (the experimenter touching the corresponding surfaces of the electric lobes of two different fish) because in this case there was a potential difference between the two surfaces. This was because only one fish discharged the shock at a time, due to the extremely unlikely possibility that a shock would be generated in precisely the same instant by the two fish.

The variety of arrangements used in the experiments of July 18th was so great that it is difficult to describe them in detail. It is, however, worth mentioning one experiment that aimed to ascertain the effects of holding one's breath and feeling the sensation of shock, as reported by Kaempfer, because this experiment is alluded to in the 1773 published paper :

> Arthur holding his Breath, received Shocks several times, contrary to Kaemfer's [*sic*] Observations. (WE, 114 L.H.)

NEW REFLECTIONS ON THE ELECTRICITY OF THE TORPEDO

After recording the experiments of this long day of intensive work, Walsh wrote a series of «Reflections». This is the second time this happened in the space of two days, and some of the reflections recall previous ones. It is likely that at this point of his researches Walsh had become increasingly aware of having accumulated a rather abundant set of data. These now deserved to be analysed in order to draw the relevant conclusions, and furthermore, to work out what of importance remained to be done in the last period of experimental work. The «Reflections» of this particular day begin with some general considerations on the nature of electric fluid which are clearly inspired by a mechanical and pneumatic conception of electricity (probably derived from Franklin), a conception that would remain central to Walsh's further attempts to understand the most elusive characteristics of «torpedinal electricity»:

> The electric fluid is highly elastic, that is capable of great and sudden condensation and dilatation. It passes thro' or over certain substances but others refuse its passage. Of the fluid Elements, Water is a medium for it to pass but Air resists it because it is itself elastic tho' inferiourly so; a Vacuum or want of Air of course affords it a passage. Of the two other Elements, Earth so various in its combination and appearances, and Fire which tho' uniform in its appearance is so incomprehensible. I shall say nothing.

> The effect of the Torpedo is conveyed by the same mediums with the effect of charged Glasses [i.e. the electric capacitors]; that is by Metals, Water, and damp Substances, and by animal bodies, who feel it's passage thro' them.
> It is intercepted by Air, Glass, Silk, Sealing Wax.
> The Torpedo perhaps carries internally it's plus and minus dispositions with respect to Electricity; and at his will opens channels to them from without.
> To be affected by the Torpedo a person must therefore be in contact with those channels or apply to them better conductors than the surrounding mediums by which the equilibrium would otherwise be restored, for Electricity takes the Shortest and best conductors.
> No Spark therefore would be seen, nor of course any noise heard.
> Neither can the Pith Balls be affected, since the Condensation and Expansion from all we have observed are effected in the same instant.
> The force of the Shock appears to be in proportion to the size and vigour of the Animal. The large and strong give heavy Shocks; the small and active give them at first & and for some times smart, but as they grow feeble so do their Shocks they still give them frequently and sometimes incessantly. The large torpedo on the contrary are often lazy & backward in giving the Shock. (WE, 115-116 L.H.)

In these «Reflections», it seems that Walsh is trying to account for the impossibility of producing sparks and noise from the discharge of the torpedo by insisting on the difficulty that any electric fluid would have in passing through «Air». The fluid responsible for the torpedo's shock would pass through other conductors than the receiving person (perhaps the mucus and moisture covering the fish even when it is outside water) when this person does establish an intimate contact with the fish or does not use particularly good conductors for the contact. In subsequent phases of his analysis, Walsh would abandon this position, and, very probably under the influence of Cavendish, would further elaborate the pneumatic model of electricity in order to derive a different explanation for the lack of sparks and noise, an explanation that somewhat prefigures subsequent notions of physical electrology.

In the «Reflections» for this day, the explanation of the impossibility for the torpedo's shock to give signs to the Electrometer, appears to be more convincing, at least from our point of view. According to Walsh the phenomenon responsible for the Shock («Condensation and Expansion» in his pneumatic analogy) is so rapid that one cannot hope to reveal it by a gross mechanical effect as the movements of the «Pith Balls» of the common Electrometers of the age.

As for the passage where the electricity of the torpedo is intercepted by silk (as well as by air, glass and sealing wax), it might seem that Walsh is going a little too far, because, according to the notes in the journal, he did not test silk as an insulating material in his experiments on the transmission of the torpedo shock. However, for an experiment performed the same day, in which Arthur could feel by holding two wires that measured together 40 feet, Walsh made the following comment:

> NB. The Wires in all our Experiments were suspended on Silk Strings, which shewed the insulation of the Silk.(WE, 108-9 L.H.)

Probably Walsh was using this kind of argument (the silk does not shunt the shock of the torpedo to the ground) as evidence that it did not conduct it (and therefore that it would thus intercept the passage of the electricity of the torpedo).

One final comment: the idea of «plus and minus dispositions» of an electric fluid present inside the animal, who «at his will opens channels to them from without», is in line with the idea in classical medicine, revived by 17th century studies, which drew attention to the presence on the body surface of numerous microscopic openings (pores or channels), which could be restricted or relaxed, thus allowing for the penetration or expulsion of various humours or fluids, and consequently influencing the organism's the state of health. Various medicaments or treatments could modify the opening state of these pores. For instance, an excessive moistening of the skin might 'relax' these surface channels, and thus make the organism weaker and even exhausted. This was one of the reasons why the doctors of the time dissuaded their patients from an excess of baths and other hygienic procedures based on an abundant use of water. Walsh was probably influenced by these kinds of cultural assumptions when, on this day of experiments, he could not draw any shock from a torpedo after sprinkling various parts of his skin with water:

> could get no Shock from them any way whatever; quite faint and exhausted: Perhaps, more relaxed by the water. (WE, 112 L.H.)

TORPEDOES AGAINST LOBSTERS, AND THE NERVES OF THE ELECTRIC ORGANS

The experiments proceeded at the same pace the next day (July 19th, a Sunday). For the first torpedo a tinfoil was placed on the fish in different positions while the experimenter touched various parts of the animal or the wet cloth on which it had been placed. The general conclusion to these experiments, based on the occurrence and intensity of the shock felt in the various configurations, was consistent with previous observations «shewing for certainty that the greatest opposition is between the upper and the under side» (WE, 119 L.H.).

For a second torpedo, several experiments were performed in which Walsh manipulated a twisted «Brass wire of 28 feet long» by using a «Glass handle» in order to insulate himself from the discharge of the torpedo while one or more persons connected in a chain were affected. The people in the chain communicated with each other by dipping their hand «in Basins of Water», the method first introduced two days before. The use of insulating handles, and the connection through basins of water, made the experiment technically easier and more consistently successful, and these arrangements will be used most satisfactorily, in the public demonstration of the power of the torpedo that Walsh gave during the meeting of the La Rochelle Academy held three days later, on July 22nd, in Walsh's apartment at the *Hotel du Duc de Bourgogne*.

Among the remarkable things occurring during this working Sunday at the Isle de Ré, there is a dissection of a very large torpedo (14 inches in diameter) that Walsh carried out in collaboration with «M. Ranger, the Surgeon of La Flotte». The description of this dissection is preceded by the narration by Captain Bertrand of the fights between lobsters and torpedoes, that fishermen sometimes witnessed (and that sometime they provoked «for Diversion», when both types of animals happened to be captured):

> on presenting the Tremble to the Lobster, he seizes the Tremble with his Claw, but receiving the Shock, he immediately quits his hold and retires back. (WE, 123 L.H.)

Bernard's description is somewhat reminiscent of the scenes of fighting between marine animals that sometimes illustrate Greek or Roman decorations, as for instance the Pompeian mosaic of Fig. 15. Here the fight is actually between a Polyp and a Lobster, but a torpedo is also pictured nearby.

During the dissection of the large torpedo special attention was paid initially to the «four large Clusters of Nerves, which run branching into the middle of the Honeycomb, each Cluster dividing itself into two, then into more, and afterwards into many» (WE, 123-4 L.H.). In the accurate anatomical study of the electric organs that John Hunter will perform on the torpedoes brought by Walsh from France («in Brandy»), Hunter was particularly impressed by the abundance of the innervation of these organs. In his 1773 article, published in the 'Philosophical Transactions' immediately after Walsh's article, he wrote that this «must on reflection appear as extraordinary as the phenomena they afford». With reference to the electrical nature of the fish shock put forward by Walsh, Hunter supposed the nerves might be «subservient to the formation, collection, or management of the electric fluid», and concluded in a way that appears to anticipate the importance of electric fish studies for the subsequent development of neurophysiology:

> How far this may be connected with the power of the nerves in general, and how far it may lead to an explanation of their operations, times and future discoveries alone can fully determine.

Besides describing the nerves of electric organs, in their dissection of the large torpedo, Walsh and the French Surgeon made «an Horizontal cut through the Honeycomb [i.e. the electric organ]», and they could observe the upper surface of the cut part:

> with Hexagonal divisions in it for the most part; and some Pentagonal, and other irregular; according to the unequal compression of the Cylinders. (WE, 124 L.H.)

THE TORPEDO AND THE EFFECTS OF ARTIFICIAL ELECTRICITY

Monday 20th, the last day of experiments at the Isle de Ré, was again a day of very intensive work. Walsh did not continue the usual experiments of the immediately preceding days, but started investigating the effect of artificial electricity on a torpedo. The experiments on the first fish are described as follows:

> Sent a Shock through one Honeycomb of a fish perfectly dry, having been out of Water two hours: No apparent effect on him; No motion of his Eyes.
> Sent a Shock through his Back between the Gills, no apparent effect whatever: In both Trials the fish was suspended by the Tail.
> Laid him on a Table, he gave two Shocks, both weak, the first the weakest. (WE, 125 L.H.)

Although Walsh does not specify the rationale of these experiments, from the last statement it seems he was investigating the possibility that the external electricity could have been added to or joined with the internal electricity, and thus result in an increase of shock strength. This was evidently not the case, because the shocks produced by the fish were weak, and, as he remarks «the first the weakest».

This conclusion was confirmed by the experiments carried out on a second torpedo from which sparks were drawn after having been «Electrified», the procedure resulting in no «discernible emotion» in the fish, except when a spark was taken from the eye, because this «made him draw it in for a considerable time».

Walsh concludes by saying:

> Electrification had no perceptible Effect in increasing or diminishing his natural Electricity, by the different Shocks we took from him. (WE, 126 L.H.)

In another torpedo:

> held up by the Tail by one person: Two others touched each one Honeycomb, about the middle of it's different Surfaces, with opposed fingers; Each touching Person received a smart Shock in both hands; nothing the person who held him by the tail. (WE, 126 L.H.)

Moreover:

> One Person touching the upper and lower Surfaces of one Honeycomb as before, and another the upper Surface of the other Honeycomb, and the adjoining Fin: the first was Shocked smartly, the second felt only a fourmillement; (WE, 126-7 L.H.)

These experiments clearly belong to the line of investigation aimed at showing that the shock was being produced between the dorsal and ventral surface of the animal. Their particular interest lies in the fact that, in these specific circumstances, Walsh puts the test and control procedure together in the same trial.

In the first experiment, the control is the person touching the animal by the tail, who does not perceive the shock (because he does not establish a circle between the two surfaces of the animal generating the shock). The other two persons, who establish a connection between the two surfaces, are instead «Shocked smartly».

In the second trial, besides the control represented by the person touching the fish tail, there is an experimental comparison between the two people who touch the animal in different positions: the first one «touching the upper and lower Surfaces of one Honeycomb» is «Shocked smartly», while the second one touching «the upper Surface of the other Honeycomb, and the adjoining Fin», feels «only a fourmillement».

The tendency to combine different experiments together in a single trial, was typical of 18th century science and depended, at least in part, on the difficulty over controlling the stimuli accurately and recording the responses with precision. A special ability to combine several experiments in one was a characteristic of Alessandro Volta's scientific work. In order to show that the electricity generated by a bimetallic contact could produce a light sensation in the eye and a taste sensation in the tongue, he connected the tip of the tongue with the bulbar conjunctive through a silver spoon in contact with a tinfoil lamina. If the contact was of a very short duration only the light sensation was produced, whereas with prolonged stimuli the flash sensation produced at the onset of the contact was followed after a delay by a continued taste sensation. A frog preparation could be interposed in this chain, which jumped at the onset of the contact (and sometimes also at its offset), while the light and taste sensation followed their course. After the invention of the electric battery, Volta succeeded in producing an even greater variety of effects in a single experiment, due to the stronger electricity that the new device was able to generate, compared to that of a single bimetallic couple. In the concluding section of the March 20th 1800 letter announcing the invention of the battery, Volta described one of these experiments:

> But the most curious of all these experiments is, to hold the metallic plate between the lips and in contact with the tip of tongue; since, when you afterwards complete the circle in the proper manner, you excite at once, if the apparatus is sufficiently large and in good order, and the electric current sufficiently strong and in good course, a sensation of light in the eyes, a convulsion in the lips, and even in the tongue, and a painful prick at the tip of it, followed by a sensation of taste. [96]

To return to Walsh's experiments in the Isle de Ré, in a third trial on the same torpedo Walsh was able to show that sealing wax could intercept the shock produced by a torpedo, while another person touching the same torpedo received the shock directly.

> One Person with opposed fingers in one Comb; another Person with two fingers on the other Comb and fin, and steadying him with Sealing wax on the opposite site: the first Shocked smartly, the other felt nothing. – This twice. 127

Again in this experiment, test and control procedures were carried out at once on the same animal. The last animal to be studied was a torpedo «much fatigued with Experiments, both in Water and out of it». This fish:

> was observed to give, while laying on his back, perpetual Shocks without any provocation, as if it was in agonies: the Shocks were very feeble, but as frequent as a Pulse. (WE, 128 L.H.)
> .

A careful investigation of this torpedo showed that also with these spontaneous phenomena, the shocks were felt only if the experimenter touched the two opposite surfaces of the animal body, while no effect could be felt by touching two different point of the dorsal (or ventral) surface.

INVENTING A NEW PHRASE: 'ELECTRIC ORGANS'

At the end of this day (July 20th) Walsh wrote down a series of reflections, which, in the journal, represents his clearest and most highly developed thinking on the problem of the topography of the fish surface in

[96] «*Mais la plus curieuse de toutes ces expériences, est de tenir la lame métallique serrés entre les levres, et en contact du bout de la langue; puisque, lorsqu'on vient ensuite completer le cercle, de la manière convenable, on excite à la fois, si l'appareil est suffisamment grand, et en bon ordre, et le courant électrique assez fort et en bon train, une sensation de lumière dans les yeux, une convulsion dans les levres, et même dans la langue, une piqure douloureuse sur son bout, suivie enfin de la sensation de saveur*.» (VOLTA 1800, pp.24-25).

relation to the production of the shock, in an effort to establish a kind of semi-quantitative relation between the zones of the animal surface contacted and the strength of the shock. One aspect of special interest in these reflections is that they contain, perhaps for the first time, the expression «electric organs», which would designate the '*musculi falcati*' of Francesco Redi until modern times. In the journal of his experiments, Walsh generally used the descriptive term «Honeycomb» or more simply «Comb», derived from the aspect of the surface of the organs («candidus Favus. In French Raïon de Miel. Vulgarly named Paris Gateau» as he writes in a footnote to a page of the July 10th experiments), although in some circumstances he used expressions like «Electric part» or «Electric batteries»:

> A Circuit from one Surface of the Combs to the other, appears from hence absolutely necessary for the obtaining of the Shock. When the animal is fresh out of water; that is, we may suppose, still wet with Water, the upper Surface of each Comb will give Shocks with every part of the animal, excepting itself, or its follow Comb: the under Surface of each Comb observes the same Law: Between the opposite Surfaces of either Comb, singly or of the two jointly; and this varied in any manner; we obtain what to our feeling appears a full Shock. Between one Surface of the Combs, be it above or below, and every other part of the animal, we obtain a Shock about half as violent as the other. This is to be understood of the Animal when fresh from the water; that is wet, as we before supposed; But whether he be in this state or not, the half Shock is still obtainable between the Comb and lateral Fin, though not between the Comb and other parts. All this of the whole, half, and no Shock, was clearly constated in numberless Experiments. The agonized animal giving the regular spontaneous Shocks; Experiments to determine the parts between which the Electric fluid could or could not be made to pass, were tried most conveniently. The results of our Observations on his head, in the whole course of our experiments was; that the upper and lower Surfaces of the two Combs, or Electric Organs, as we may venture to call them, are capable of being thrown by the Animal at once into opposite States of Electricity. In this operation one or both Organs are concerned, according to the will or ability of the animal. The upper Surfaces of the two Organs never appear in opposite States of Electricity to each other; Nor do the lower with respect to each other; The lateral fin bounding the Organ outwards, seems to be in a neutral or semi-conducting State, capable of making an imperfect, probably a slow discharge with either Surfaces of the Organs: Perhaps the space between the two Organs below; perhaps indeed the whole Thoracic region, and all above the cross Cartilages which prop the Diaphragm, is the same.
> It is a very singular Effect that the touching of the electric organs of two fish separately (as in experiments n. 233, 239, 240, 241 and 256) shall communicate a shock, delivered by one of the fish, and sometimes by the other; The skin of these organs appeared in these instances, to conduct along its surface the shock of another Torpedo; while on other occasions it appeared incapable to conduct its own Shock along its surface, as in all the cases when the same surface of the two organs of a Torpedo was touched by an insulated person. (WE, 129-130 L.H., 131 L.H. and R.H:)

The last part of these reflections again shows that Walsh was unable to understand why the shock had been felt in the trials in which the experimenter established the contact between two homologous surfaces of two different fish, whereas no shock had been felt by connecting two points of the same surface of a single fish. This happened in spite of the fact that he clearly recognised that the «opposite States of Electricity« were generated between «the upper and lower Surfaces of the two Combs, or Electric Organs». His difficulty over accounting for these and related observations shows the mental block that natural philosophers of his age could have in interpreting electric phenomena in the absence of a clear notion of electric potential. Curiously, as repeatedly pointed out, it is from the attempt to interpret some of the phenomena of the torpedo that an early version of the concept of electric potential emerged, due mainly to the work of Henry Cavendish. It should also be remembered that the almost exclusive insistence on the conducting power of the skin of a fish for the electric fluid, generated either by the same fish or another one, betrays another kind of difficulty, hard for us to appreciate because of the differences of 18th century electric experiments from our own. In a letter to Franklin written in Paris on August 27th 1772 as he was about to return to London (and included in the 1773 paper), Walsh writes:

> The skin of the animal, bad conductor as it is, seems to be a better conductor of his electricity, than the thinnest plate of air.

Walsh is here alluding to the difficulty over obtaining a spark from the torpedo's shock by allowing it to flow along the minutest separation (obtained by using a tinfoil plate or in some other method). For us, of course, air is an insulating matter, a much worse conductor that any wet animal tissue, as for instance fish skin. For Walsh and the electricians of his time, things appeared to be different because it was generally very easy to discharge a charged electrical machine or a charged Leyden jar through a small air gap, because, with the very high voltages then usually involved in artificial electricity, air became easily ionised and thus conductive of electricity. Because of this, a small air gap might appear to be a rather good conductor, probably better than the skin of an animal.

On July 21st, Walsh and Arthur left the Isle de Ré «in Bertrand's barge, with fair but little wind and smooth water», and, after about one hour and a half, reached the port of La Rochelle. Mr Davies and Thomas arrived with another barge «towing the Acon [...] in which were Seven Torpedoes». In this way the intensive and successful period of researches at the Isle de Ré came to an end, but the torpedoes caught off the coast of this small island would be available for further experiments in La Rochelle. Together with other torpedoes found at their arrival, and with more live fish brought afterwards by fishermen, these animals would offer to Walsh and his collaborators the opportunity to continue their researches and, in addition, to give a series of public demonstrations of the electric nature of this singular fish.

THE GRAND FINALE: WALSH'S PUBLIC DEMONSTRATION AT LA ROCHELLE: THE *ACADÉMIE* FIRST

The first of these demonstrations was given on the day following the arrival at La Rochelle, on the occasion of the Meeting of the local *Académie*, which, in this special circumstance, was held not at the *Hotel de Ville*, but in Walsh's apartment ad the Hotel *du Duc de Bourgogne.* Besides many *Académiciens*, including the Director - M. Rault - and the Perpetual Secretary - M. Seignette - other gentlemen were present, some of whom, such as M. Saunier and the two MM. Weis, had already taken part in Walsh's previous experiments. The experiments, which were carried out on five of the eight live torpedoes available, are described in detail, with the comments of the attendants and part of what Walsh himself said on this occasion.

We have already commented on the custom and importance of giving public demonstrations of experimental achievements in 18th century science.

The demonstration was important - as Walsh would comment in his 1773 published paper - because of the need to have authoritative witnesses for «facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science», as were those concerning the shock of the torpedo. It also fitted in with the «intention of stirring up a spirit of inquiry, both as to its electricity and general oeconomy». A further reason of interest is that the discussion and comments of the learned society of La Rochelle during this public demonstration probably contributed to direct the last phase of Walsh's investigation in France.

Walsh planned the general experimental arrangements carefully in order to maximise the success and clarity of the demonstration, and show how he had taken advantage of the various technical improvements developed in the course of his previous experiments:

> Each fish being laid in it's natural position on a small separate Board, which was first spread with a wet Towel. Was in this state brought as wanted, and placed on a Glass legg'd Stool, which stood on a small Table; At some distance (about six feet) from the small Table was a large Table, on the margin of which stood several Basins [*cuvettes de Faïence* as specified in the footnote] filled with Water: two Brass wires, each of about 13 feet in length, were separately suspended by silk string to the Ceiling, so as to make distinct communication between the two Tables: One end of one Wire was folded in the wet Cloth on which the fish lay, and the other end reached the Water of one of the Basins, which for distinction sake we will call the first Basins: One End of the second Wire was inserted in another Basin of Water, we will in like manner call the last Basin; the other end for the present was disengaged. (WE, 137-8 L.H.)

In a first experiment «five Gentlemen insulated separately on Boards supported by Glass Tumblers» formed a chain by communicating with each other through the «Basins of Water». The first one communicated with the lower surface of the fish through the wire folded in the wet cloth, while the last member of the chain was connected to the upper surface of the fish through the other wire which was conveniently manipulated by Walsh (who was not in the chain):

> the five persons in the Circuit were immediately sensible of the Shock; while M^{r}. Walsh, who was close to the fish, and pressed the wire upon him, felt no effect whatever, manifestly by being out of the path of the Electric fluid. (WE, 140 L.H.)

The experiments were repeated several times and each member of the chain «was affected in every repetition» - as Walsh says - except for one of them «M. Arcere; who being advanced in years proved in all our trials to be hardly sensible of small Shocks»[97]. Walsh supposes that besides an individual low sensitivity, M. Arcere's slight reaction to the torpedo's shocks might be due not only to:

[97] Luis Etienne Arcère (1698-1785), Superior of the Congregation of the Oratorians, besides being member of the *Académie des Sciences et Belles Lettres* of La Rochelle and *Sécrétaire perpetuel* of the *Société d'agriculture* of the same town, was also correspondent member of the *Académie Royale des Inscriptions et Belles Lettres* of Paris. His main work is a '*Histoire de La Rochelle et du Pays de l'Aunis*' in 2 voll. published in 1756. This work was presented by Seignette to the Emperor of Austria Joseph II during his visit to La Rochelle (see footnote ++++). At the epoch of Walsh experiments Arcère was 74 years old.

> their being originally but weak, but as still considerably lessened by their passage through so many different mediums, and through such an Extent of Conductors. (WE, 140 L.H.)

On the other hand, he excludes that it might have been due to some form of experimental artefact:

> It must be observed that the escape of the fluid by Cuts, shorter than the main Circuit, was not very probable, not only from the precautions which had been taken, but from the dry state of the Air, which left both the table and the boarded floor of the Chamber, sufficiently insulated of themselves. (WE, 141 L.H.)

To verify the hypothesis that M. Arcere's slight reaction to the torpedo's shock might have been due to the «many different mediums», Walsh shortened the circuit to one in which only M. Arcere was present, and was connected to the torpedo through two basins and the metallic wires: «He now felt it very distinctly» - writes Walsh in the journal. Moreover, in a further trial in which some of the members of the human chain were changed but Arcere was still present, «the rough Minute says that the Shock was felt by all several times; without excepting on this occasion M. Arcere». (WE, 140-1 L.H.)

Experiments of this type were repeated many times by varying the people that composed the human chain, and their number. One can imagine that all the gentlemen present were eager to experience the shock, perhaps after seeing that it was not harmful (as might have been supposed on the basis of the legendary and literary accounts on 'the art of the torpedo'). On his side, Walsh was surely glad to be able to exhibit his experiments and make his results and conclusions tangible in some way by showing them «before numerous companies of the principal inhabitants of La Rochelle», as he writes in his 1773 article. He might also have been glad that in another public meeting, held the following day (July 23rd), he was able to register his highest score as to the longest human chain that could be shocked by a torpedo. The experiment was repeated using the same apparatus, and the elements of the chain were increased progressively from four to seven, «by all whom the Shock was repeatedly and distinctly perceived». In a further trial with the chain made up of eight persons the shock was also transmitted, «but it was never felt by all the eight, in any one Experiment». To these experiments Walsh alludes in his letter to Franklin sent from Paris on 27th August 1772, when, about «the torpedinal electricity» he writes:

> I was able, in the public exhibitions of my experiments at La Rochelle, to convey it through a circuit, formed by one surface of the animal to the other, by two long brass wires, and four persons, which number, at times was increased to eight.

This letter to Franklin was also included in the 1773 article published in the *Philosophical Transactions*. This article contained another account of these same experiments, an extract of a letter published on 30th October 1772 in the *Gazette de France*, written by M. Seignette, secretary of the *Académie* and Mayor of La Rochelle.[98] Among other things, Seignette mentioned that the experiment of the human chain «was repeated several times, even with eight persons; and always with the same success».

[98] The *Gazette* had already announced Walsh's experiments on August 14th.

In Walsh's report, and even more in Seignette's, there is no indication of the imperfect success of the experiment with eight people. This is rather an overstatement, since, in this experiment not all eight people generally succeeded in feeling the shock.

It is worth mentioning that in his 1773 article, in introducing the extract from Seignette's letter to the *Gazette*, Walsh says he did this to present a creditable «testimony of the facts that I have advanced». We again see in this justification the need that 18th century scientists felt for authoritative witnesses to their achievements.

In his first public demonstrations at La Rochelle, besides showing that the «torpedinal fluid» could pass through human chains of numerous people, Walsh also made experiments:

> shewing Sealing wax and Glass, to be incapable of transmitting the Shock of the Torpedo. (WE, 143 L.H.)

Moreover, in order to convince those attending of the close correspondence of the sensation produced by the torpedo and by artificial electricity, he applied:

> the Wires, and the whole Apparatus as it stood, to the Electric machine, regulating the Charge of the Jar with Mr. Lane's Electrometer.

Besides the similarities, also:

> The circumstances in which the Animal Electricity appeared to differ from the Artificial, were remarked by the Company. Particularly that it was attended with neither Sparks nor Sound; that it occasioned no appearances of Attraction and Repulsion in the Pith Balls and such light Substances; and that it was capable of affecting the Animal while the Torpedo was in Water that is surrounded by a conducting medium; a power which this Animal must necessarily be supposed to employ for the great purposes of Life Subsistence and Self Defence. (WE, 143-4 L.H.)

Walsh had already taken all these kinds of difficulties into account during his previous experiments on the torpedo, but probably the discussion held during the public demonstration contributed to revive his interest in these elusive aspects of the torpedo's electricity.

ANOTHER NEW PHRASE: 'ANIMAL ELECTRICITY'

Interestingly, for the first time in Walsh's journal of experiments the phrase «Animal electricity» appears in the passage quoted above to denote the electricity of the torpedo. In 1791 the same expression (in its Latin form '*animalis electricitas*') would be used by Luigi Galvani to designate the electricity present in the tissues of frogs and other common animals, an electricity supposed to be responsible for nerve conduction and muscle excitation. It is generally assumed that 'animal electricity' was introduced into science history by Pierre Bertholon, the French scientist, who used it in a Memoir published in 1780 on the role of electricity in normal and pathological conditions.

Probably the primacy does not pertain to Bertholon. Although Walsh does not use 'animal electricity' in his published papers, the expression appears in 1775 in the opening of John Hunter's article dealing with the anatomy of the electric eel. This article was based on a study of one of the eels that Walsh imported from Surinam and used in his physiological experiments (it was in these experiments that Walsh eventually succeeded for the first time in obtaining a spark from the discharge of an electric fish). Hunter wrote:

> To Mr. WALSH, the first discover of animal electricity, the learned will be indebted for whatever the following pages may contain, either curious or useful. The specimen of the animal which they describe was procured by that Gentleman, and at his request this dissection was performed, and an account of it is communicated.

Curiously, notwithstanding this evident proclamation of 'animal electricity', when Hunter's paper was read to the Royal Society the author seemed far from convinced of the genuinely electrical nature of the eel's shock. As Cameron Walker pointed out in a study of 'Animal electricity before Galvani', in the Minutes of the Society for March 18th 1775 the following passage appeared:

> An anatomical account of the Gymnotus by Mr. John Hunter was read. In the letter to Mr. Walsh prefixed to this account, Mr. Hunter says that [...] he avoided the word electrical, not thinking he was sufficiently warranted by any experiments that have been made to use it. [99]

This episode, which is confirmed by William Henly in a letter to William Canton dated 21st May 1775,[100] is probably to be referred to the debate then raging on the electrical nature of the fish shock, a debate ended by the final demonstration Walsh gave a short time afterwards, with his experiments on the eel. As a matter of fact, although the Minutes of the Royal Society report that «Mr. Hunter [...] avoided the word electrical»[101] in his communication on the Gymnotus, the paper published in the 'Philosophical Transactions' (entitled '*An Account of the* Gymnotus Electricus') begins with an apparent proclamation of faith in animal electricity. Probably this happened because, in the time between the communication of the paper to the Society and the publication of Hunter's paper on the *Philosophical Transactions*, Walsh had performed his experiments on the Eel, and these experiments, as Henly also attested, had succeeded in convincing even the most sceptical of the electrical nature of the fish shock.

A 'CAPITAL DESIDERATUM': THE OPERATION OF THE TORPEDO IN WATER

To return to Walsh's public demonstrations of torpedo experiments, there are other comments in the journal on the experiments at the Meeting of the *Académie* that were afterwards included, in a somewhat modified form, in the 1773 published paper.

> It was also observed that a knowledge of the operation of the Torpedo when in Water, was a capital Desideratum; as the Experiments wed have made with it were chiefly when it was out of water:

[99] See CAMERON WALKER 1937.
[100] Canton papers of the Royal Society, Vol. II, p. 105.
[101] Journal book of the Royal Society, vol. XXXVIII, p. 249.

> That what we had been able to do ------- had only opened the door to a curious and interesting enquire: That great points remained to be examined, both by the Electrician, and by the Anatomist.
> That Nature had denied the animal to our Country and that the pursuit rested in a particular manner on them whose Shores abounded with it; and that by consequence they might be well assured in their Researches that as Artificial Electricity had led to a discovery of some of the operations of the Animal, the Animal if well consider'd would lead to a discovery of some truths in artificial Electricity which were at present unknown and perhaps unsuspected. (WE, 144-5 L.H.)

There are various points of interest here. First, the initial consideration about the fundamental importance of ascertaining the electric power of the torpedo while the animal was in water (that emerges clearly during the discussions that followed the public demonstration) very likely contributed to directing the last phase of Walsh's research at La Rochelle, on that specific point. We may note here that, although in Walsh's case (as also in modern life science), the final purpose of the investigation was to understand the mechanisms underlying a process in physiological circumstances, very often the experimental scientist creates artificial conditions in order to gain better insight into the problem studied, and only later comes to consider conditions more similar to those prevailing in natural circumstances.

The other points of the passage quoted reveal some remarkable aspects of John Walsh's scientific disposition. The necessity of public demonstration was not only to have authoritative witnesses of the experiment's achievements, but also to arouse the interest of the people attending so that they might be induced to continue the research. This was particularly important in the case of torpedo studies because, as Walsh says, this fish, so abundant off the French Atlantic coast, could not be found off the English coast. Walsh will change his mind on this point because he will discover that torpedoes were also available in England, and in 1774 will publish in the 'Philosophical Transactions' a study 'Of Torpedoes found on the Coast of England'.

Walsh argued that investigation into the torpedo needed to be continued «both by the Electrician and the Anatomist». This interdisciplinary programme, an essential aspect of Walsh's efforts in France, was to be realised in a particularly splendid way upon his return to London, where he was able to get Henry Cavendish and John Hunter interested in the torpedo; they carried out important studies on this fish, from the perspective of physics and anatomy respectively. From his investigation into electric fish, beside clear evidence of the electric nature of these fish, important notions of physical electrology will also emerge. We can thus appreciate the validity of Walsh's final statement which brings out the productive interplay between physics and physiology that characterised his investigation, as well as the disposition of many natural philosophers of the 18th century. A somewhat analogous problem was at the centre of Alessandro Volta's rational proceedings in his invention of the battery.

A SPECIAL DEMONSTRATION FOR THE *COMMANDANT EN CHEF* AND FOR HIS OFFICERS

The success of the first two demonstrations of the torpedo's electricity held by Walsh at La Rochelle on 22nd and 23rd July caused an unforeseen continuation of both the public exhibitions and the experiments. On the evening of July 23rd Walsh was invited to a splendid dinner «by M. le Baron de Montmorency at the Gouvernment», where «the Company consisted of about fifty Gentlemen, mostly Military Officers», and the «Entertainment was well served in all respects, very superior to every thing I would have expected at La Rochelle».(WE, 148 L.H.)

After the dinner, there was a polite explanation of the unpleasant episode at the Isle de Ré (see above), with the conclusion «that the affair was a Mal-entendu». In a climate of reciprocal courtesies and compliments, and with Montmorency having heard of the previous public demonstrations of the torpedo's power, Walsh declared he would be happy to offer him and the other Gentlemen of the Company a special demonstration of his experiments the next day, July 24th (notwithstanding his previous plan to leave La Rochelle on that same day). And in the journal of experiments we read:

> At noon M. Le baron de Montmorency, with the Colonels of the Vivarais, Cambresis and Conti Regiments, and a Suite of about 40 Officers entered my Apartments. Mess. Seignette, Dumesnil, and some others of the Inhabitants accompanied them.
> They all received the Shock; first singly by dipping a finger of each hand into the basin of water in which the Wires came; and then by fours, communicating with each other by the Basins as formerly. (WE, 150-1 L.H.)

This rather solemn scene of the officers with their aristocratic *Commandant en Chef*, eager to experience the torpedo shock and verify its electric nature, is an expression of a science *d'autrefois*, difficult to imagine in modern times, when, despite its importance for the progress of mankind, science seems to be no longer capable of attracting the interest of learned people, as in the *ancient régime*.

JULY 1775, AN 'IMPERIAL' DEMONSTRATION

The scene would be repeated in an even more solemn way five years later, on 18th June 1777, when a similar demonstration was given at the *Académie* by Seignette and his colleagues upon the request of the Austrian Emperor Joseph II. Travelling in France *in incognito* as '*comte de Falkenstein*', the Emperor wished to take advantage of the occasion to personally witness the torpedo's experiments, news of which had spread throughout Europe. The details of this event are contained in a letter Seignette sent to Condorcet, the Secretary of the Paris *Académie des Sciences*, and are recorded in the Archives of the *Académie* of La Rochelle.[102] The method was basically the same Walsh

[102] Joseph II (1741-1790), son of Maria-Theresa of Austria and Francis I of Lorraine, was the first Emperor of the Hapsburg-Lorraine dynasty. Although he ascended to the Imperial throne in 1765, he reigned effectively only after the death of his mother in 1780. Before the journey to France, he travelled in different countries of the Empire and in Italy. He was one of the enlightened sovereigns of the 18th century and made many important reformations, and in particular he abolished torture and the slavery of peasants in his possessions. Apart from political and military organisation, he was interested in science and technology. As in the case of his journey to France, he travelled frequently *in incognito* in order to have direct contacts with the persons who he happened to meet (this leading to a series of significant episodes amply documented in his rather hagiographic biographies). Joseph II left Vienna on April 1st 1777, arriving in Paris on April 18 after passing through Stuttgart, Strasbourg, Nancy, Metz, the region of Picardy. Among the many places he visited during the period spent in the French capital, we may list several hospitals and public assistance institutions, in accordance with the philanthropic spirit of the *princes éclairés* of the time. He was particularly impressed by the poor hygienic and sanitary conditions at the *Hotel-Dieu*, being, however, reassured that a plan for the renovation of the hospital had been made by *M. Le Roy* (i.e. the same Jean-Baptiste Le Roy met by Walsh five years before in the same

used, with basins full of water (*cuvettes pleines d'eau*) employed to connect the members of a human chain, and metallic wires to establish a contact with the fish. When the contact was established through sealing wax (*cire d'Espagne*), no commotion was felt.

«The experiment – Seignette says – succeeded well and the Emperor was so nice as to signify his satisfaction» and, moreover, «he felt the commotion given directly by the torpedo». Apparently the Emperor mentioned this experiment on his return journey to Austria «being in Geneva in the physical cabinet of Professor de Saussure», as some contemporary journals reported.[103]

The letter to Condorcet is noteworthy because it contains an interesting comment on the methodological attitude Walsh showed during his experiments at La Rochelle.

> This Englishman made rapid discoveries. He had a system and followed it. This disposition, almost always contrary to the discovery of truth, conducted him from a knowledge to the other.[104]

From these words one could suppose that Walsh had appeared to the learned people of La Rochelle as if dominated by a single strong idea, and guided in his investigation by the principle of a 'rigid' system. An understandable impression, if one considers that the electric nature of the torpedo shock had become a kind of *idée fixe* for Walsh, and that he had been working, in an almost compulsive way, from morning to night, every day of the week, to obtain experimental evidence to support his hypothesis. In the implicit criticism of Walsh's attitude, one might perhaps also see the viewpoint of a French gentleman, who considered science mostly as a kind of literary entertainment, to be conducted with an *esprit libre* and with moderation, without any preconceptions. However, as Seignette recognised, in this particular case, the «*système*» that Walsh followed was eventually productive, leading the English scientist to acquire knowledge in a most effective and rapid way.

Coming back to 1772, there was a curious episode following the demonstration given by Walsh to Montmorency and his company:

> One Officer, who had been much wounded, was quite insensible of any effect the Torpedo could communicate. This Gentleman being desirous to know if he was proof against the Electrick shock in general returned after the Company had withdrawn and requested to experience the effect of the Leyden Phial; he

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town). Joseph II left Paris on May 31st, and on June 18th arrived in La Rochelle after visiting various towns of the regions on route (Rouen, Caen, Saint Malo, Brest, Saumur, Nantes, Tours). In La Rochelle he attended the meeting of the *Académie* at the *Hotel de Ville* which had been organised by Seignette upon receiving a letter from Condorcet. The description of the demonstration given by Seignette in his letter to Condorcet, closely corresponds to that presented in an account of the visit of Joseph II to France published anonymously in 1778, but probably due to Gauthier de Simpré (see the Bibliography). Among the things that we learn from this account is that the Officers accompanying Joseph II lodged in the same hotel (*Le Duc de Bourgogne*) where Walsh lived during the period spent at La Rochelle. (see footnote ++++).

[103] «*Cette expérience réussit très bien, et l'Empereur eut la bonté de témoigner sa satisfaction.* [...] *Il éprouva la commotion donnée immédiatement par la torpille*» Copy of a letter of *M. Seignette* to *M. le M.is de Condorcet, Archives départementales de la Charente-Maritime, 103, Archives de l'Académie des Belles-Lettres, Sciences et Arts de la Rochelle, reg. 3*, Second registre des délibérations de l'Académie de la Rochelle, 18 juin 1777.*

[104] «*Cet Anglois fit des découvertes rapides, il avoit un système et il le suivoit. Cette disposition, presque toujours contraire à la découverte de la vérité, le conduisit de connoissances en connoissances.* » Ibidem

> received a tolerable strong charge and went away satisfied of his sensibility. (WE, 151-2 L.H.)

WATER BASINS, WIRES AND GLASSES: THE 'MANY CIRCUITS' OF THE TORPEDO SHOCK

The experiments continued at an intense pace after the episode of the insensitive officer. Taking advantage of the water basins introduced in his experiments a few days before, to facilitate the contact between the different people in the human chain, and using metallic wires of various lengths, Walsh devised a great variety of spatial arrangements in the circuit of discharge of the torpedo's shock and, furthermore, of artificial electricity. With these circuits he was trying to understand some laws of the circulation of electric fluid, and of the conductivity of different bodies in a qualitative way, and also to obtain information on the individual's sensitivity to electricity.

In the initial experiments, one person first connected himself to the torpedo by holding with each of his hands a metallic wire, and, in a second trial, used the same wires but made the connection with them indirectly, via two water basins. The shock in the second «situation was surprisingly stronger than in the first». Walsh correctly noted that this result,

> Whatever may be the conducting Power of Water, […] can only be supposed to have arisen from the intimate embrace of Water by it's fluidity both with the Wires and the fingers immersed in it. (WE, 153 L.H.)

In a subsequent experiment a third basin of water was added, and this was:

> connected with the other two, sometimes by means of an additional insulated Person, and sometimes by ten feet of wire; the Person was found to weaken the Shock considerably more than the wire.(WE, 153 L.H.)

This experiment, which provides an evident indication of the superior conducting power of metals compared to human tissues, is interesting because the human body was used in this circumstance both as a measuring device (this is the case for the first person), and as the object to be measured (the body of the second person).

Afterwards, the arrangement was modified by replacing the third basin with «20 Glass tumblers» connected to each other «by small brass wires of six inches in length». Arthur was in the centre of the circuit, «that is between the 10^{th} and the 11^{th} glasses». In these circumstances the shock was strongly reduced, which - as Walsh remarks - showed that «an extension of the Circuit even where the Conductors are connected by Water diminishes the effect». (WE, 153-4 L.H.)

In these experiments, when the person in the circuit changed position with respect to the glasses or basins, the shock perceived did not change in intensity. To us today, this is an obvious result, but it would have been less so, if one refers to the pneumatic conception of the electric phenomena prevailing in Walsh's age: why should a compressed fluid that circulates through a chain of conductive elements, as a consequence of its sudden expansion, affect in a similar way the first or the middle element of the chain?

By using a modern words we could say that the experiments performed up to this point were basically a 'series combination' of various conductors. In a further experiment, Walsh tried what can be considered a 'parallel combination'. The glass tumblers were removed from the circuit and two persons, independently of each other, formed a double circuit by «dipping, each a finger of each hand into the two basins which held the long wires» connected to the torpedo. Both experienced the shock, but «weaker than that received in a single circuit: to their feel it appeared of half the strength».(WE, 154 L.H.) According to Walsh, this was an indication that «the weak electricity of the Torpedo is capable of taking in it's Circuit two distinct paths».(WE, 157 L.H.) From our point of view, this may be considered a nice instance of Kirchoff's law of the parallel combination of conductors, with the peculiarity already noted that, in this circumstance, the elements placed in parallel acted both as resistances and as instruments of measurement.

In commenting on these experiments, and particularly those with the glass tumblers, Walsh wrote that combinations of various elements «prove a good Electrometer in many respects». Moreover:

> by adding Glasses at pleasure and forming double Circuits with them the severest Shocks of the Torpedo may be melted down so as to be just perceptible to Persons of the most delicate Nerves.(WE, 156 L.H.)

Walsh's comment on this last observation is noteworthy:

> and perhaps the principal reason for the Torpedo not having been better examined was founded in the general dread which was entertained of the severity of it's shock.

By investigating natural phenomena in appropriate ways, we can dominate them and reduce them to conditions in which they can be approached without fear. In providing knowledge, science frees us from ancestral fears and increases our control over natural forces. From this latter perspective one can perhaps consider that the 'taming' of the torpedo, initiated by Walsh in 1772 with his experiments at La Rochelle, will be brought to final fruition in 1800 with Volta's invention of the electric battery: with the Voltaic battery, the *organe électrique artificiel* as Volta will call it (see later), an electricity similar to that of the fish would become available for ever for human progress. In a similar way in the mid-18th century the wild power of lightning («the formidable bolt of the atmosphere» as Walsh called it in his 1773 paper on the 'Philosophical Transactions') had been subjugated by Franklin's demonstration of its electric nature and by the invention of the lightning-rod.

To return to Walsh's experiments, he believed the combination of a variable number of glasses and wires in series could also serve to estimate the different sensitivity of various people «even in some degree».

In his shifting between physiology and physics, after investigating the circulation of the «torpedinal fluid» along conductors of various types and of various spatial dispositions, Walsh set up similar experiments using artificial electricity.

In a first experiment made by charging a Leyden jar from an electric machine via a Lane's electrometer, so as to regulate the strength of the discharge appropriately, the connection was made through two long metallic wires, directly or via the two basins of water, in a very similar way to the experiment previously carried out with the torpedo. The shock felt in the two situations was of similar strength («very little stronger in the water than when the wires were held out of the water.», WE, 158 L.H.)

The second experiment with artificial electricity seems to be an extraordinary demonstration of Kirchoff's laws (still to be discovered) of a complex (series and parallel) combination of conductors, in which again the people making up part of the different circuits acted both as experimenters and measurement devices, and as objects of the investigations. Three parallel circuits were established from the plus to the minus pole of the artificial electricity, each of which included a person. Initially the disposition consisted of:

> Circuit N°. 1. of two basins and One person
> N°. 2. of the two basins and five glasses, and One person.
> Circuit N°. 3. Of the two basins, and nine glasses, and One person. (WE, 158-9 L.H.)

In these circumstances all the three persons «were shocked strongly». This is an understandable result to us today, because due to the relatively high resistance of the three circuits, a relatively small total current is absorbed and thus the potential at the generator remains relatively constant; moreover, the voltage drop in each circuit is relatively small along the conductors in series with each person.

However, things changed when «Instead of the person in the 1.st Circuit used a wire between the two basins». In this case the «persons in the Circuits N°. 2 and 3 were little affected, the person in N°. 3 hardly feeling any thing». (WE, 159 L.H.) Again, this was a result easy to understand from the perspective of Kirchoff's laws because of the diminished resistance in the 1st circuit and thus of the greater shunt along this path.

In a continuation of the experiment, Walsh adopted a spatial disposition based on the use of the water basins that, in a rather evident way, betrayed his real purpose in making these experiments.

The two wires (coming from the artificial source of electricity) were put into one basin «six inches distant one from the other». A person put one finger of each hand on the basin, on one side with respect to the ideal diameter formed by the two wires one in front of the other, and another person did the same thing on the other side. The spatial arrangement of the wires and fingers in the water looked like «a regular hexagon whose sides were nearly three inches long». Both people felt the shock. As Walsh aptly remarks, in these conditions:

> Electricity therefore made three Circuits thro' the water, six inches to each wires thro' the two persons, and six inches water to each wire. (WE, 160 L.H.)

From the analysis of this and of several more experiments made by changing the spatial arrangement and relative distances of wires and fingers in the water of the basin, Walsh arrived at the following conclusion:

> The general result was that Electricity takes many circuits, and does not confine itself to the shortest nor even best Conductors.(WE, 161 L.H.)

The importance of this conclusion was that it allowed Walsh to conceive in a realistic manner the problem indicated as a «capital desideratum» by the company at La Rochelle, a problem difficult indeed to tackle within the electrological notions of the age, i.e. the problem of the «action [of the Torpedo] in water». How could the fish make use of an electric shock in his natural habitat as a defence or offence weapon against other fish or marine animals? Since water was considered to be a good conductor, were the shock genuinely electrical, all its electricity would very likely flow along the layer of water in close proximity to the fish's body and thus fail to affect animals situated at some distance from it.

In the context of the problem at the centre of Walsh's intellectual preoccupations after his public demonstrations of torpedo experiments, the statement that «Electricity takes many circuits, and does not confine itself to the shortest nor even best Conductors» implied, more or less clearly, that the electric fluid emitted at the moment of the shock by a torpedo in water need not necessarily circulate exclusively through the thin layer of water surrounding its body (i.e. the shortest circuit in the water medium). From Walsh's experiments it was also emerging that water was not a particularly good conductor, certainly much less so than metals.

AGAIN ON THE CONDUCTION OF THE SHOCK: THROUGH WATER, METALS, NETS AND CHORDS

The experiments carried out by Walsh and his collaborators the last two days he spent at La Rochelle (25th and 26th July) were entirely devoted to ascertaining «the action in water» of the torpedo, the «capital desideratum» of the learned Company at La Rochelle. To this purpose on Saturday 25th Walsh and his collaborators (Arthur and Mr Davies) boarded Captain Bernard's boat. In the «Rough Minutes verbatim of Mr Davies» the result of the first experiment carried out on a torpedo, «a 10 inch female Torpedo from the Isle de Ré» is described as follows:

> Expt. N°. 1. Torpedo under water about 3 inches in a flat Basket, finger underneath, thumb above honeycomb, a full Shock thro' finger, thumb above one honey comb, a full shock through finger and thumb (full, i.e. a fullness in the feel of it) - the same repeated again. (WE, 164 L.H.)

Notwithstanding the «fullness» of the shock, because of the immediate contact between fingers and torpedo, this initial experiment did not directly answer the main question concerning the «action in water», i.e. the transmission of the shock at a distance. In a second experiment, in which the fish was touched directly with one hand on its dorsal surface while a finger of the other hand was placed in water, the shock was felt only with the finger of the touching hand. Walsh (or Mr Davies) remarked that where they were sitting in the boat appeared dry, and thus the person feeling the shock was probably insulated, which suggests that the electric fluid emitted by the torpedo might have circulated also through the finger under water, and thus through the water itself. This experiment seemed thus to be only partly conclusive. However, in the course of the following experiments Walsh obtained clear evidence of the transmission of the torpedo's shock through the water in which the fish was plunged. This happened, for instance, in experiment N°. 5, carried out on a «12 inch torpedo»:

> Fish under water – Touched him with Iron in one hand, other hand in water; felt a strong shock in both.(WE, 166 L.H.)

This torpedo had two holes in its electric organs «like deep dimples, appearing as if the skin was broken all round looking red and excoriated within». In one of these dimples there was still a leech. The fishermen said that frequently there were leeches and dimples on the torpedoes captured. These notes were not recorded just for their descriptive value, or as curious observations for the benefit of natural history, but as evidence that the «The Leeches therefore are not affected by them», i.e. by the torpedoes when these fish discharge their shock.

If we could imagine what Walsh's ideas were in this case, we would surmise that he may have been thinking about the possibility of trying out the effect of artificial electricity on the leech found on the torpedo, in order to evaluate the similarity (or dissimilarity) of the sensitivity of this marine invertebrate to the torpedo and artificial shock. An experiment that normally required the availability of an electric machine and of a Leyden jar, devices that were very unlikely to be available in Captain Bernard's boat.

In this context it might be appropriate to say that twenty years after Walsh, the effect of artificial electricity on leeches (although not of the marine types) would be investigated by Alessandro Volta in the intense phase of experimental researches that he undertook after reading Luigi Galvani's *Commentary* on the involvement of electricity in muscle motion. In his '*Memoria seconda sull'Elettricità animale*', published in the spring of 1792, Volta reported a series of experiments in which he was trying to obtain muscle contraction by using metallic conductors, used to connect in various ways the nerves and muscles of frogs and other animals, in order to evaluate Galvani's supposition of the existence of an intrinsic 'animal electricity' inside living tissues. Among other animals, Volta eventually decided to investigate the effect of metallic conductors on less «perfect» species, such as «leeches, earthworms and others», in order to see if it was possible to find - as he says - «the same phenomena of animal electricity», in animals lacking a highly developed nervous system. The experiments showed that these animals did not move when metallic conductors were used to connect different regions of their bodies. On speaking of these animals, Volta says:

> it is well remarkable that not only they did not give me any sign of a intrinsic animal electricity, but they also reacted poorly to the discharges of the artificial one, that is incomparably less that all the other animals endowed with nerves.[105]

On the basis of these results, we could say that had Walsh tried to investigate the action of artificial electricity on the leech found on the torpedo at La Rochelle in 1772, he might have seen the similarity of the action of this electricity and that of the torpedo, and would have considered it as additional evidence to strengthen his belief that the «torpedo is electrical».

Let us, however, leave aside these considerations on what Walsh might have done (and did not do), and come back to what he actually did and recorded in his journal of experiments.

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[105] «*... ed è ben rimarcabile, che non solo non mi han dato segni in alcuna maniera di elettricità propria animale, ma poco si risentirono ancora per le scariche dell'artificiale, incomparabilmente meno cioè di tutti gli altri animali corredati di nervi*». (VEN, I, 81)

The experiment of the transmission of the shock produced by the torpedo affected by leeches was repeated in various ways, with or without the iron to establish the direct connection with one surface of the fish, and was generally successful. In one of the experiments (N° 10 of this day, Exp. 310 of the total series) the iron used to establish the direct contact with the upper side of the fish was dipped in water by keeping it with a hand through a «Wet cord, 4 inches, tied to the iron». When the other hand was put in the water, a shock was felt.

With these experiments Walsh gave support to the possibility of transmission of the fish shock through the fishing net as many fishermen had reported, including those of La Rochelle and the Isle de Ré (a possibility that was also consistent with the results of subsequent experiments). In the following experiments (N°. 11 and N°. 12), the shock was felt through the iron, even when the other hand was not plunged in water, but was wet and put «upon the gunwale of Boat». A note added by Walsh says that in this case:

> From wet hand there was a drip of water along the outside into the Sea.(WE, 167 L.H.)

This means that Walsh held that in these circumstances the electricity of the torpedo circulated from the fish to the sea, via the iron and the body of the experimenter and the water dripping from the wet hand.

THE INTENSITY OF THE SHOCK INSIDE AND OUTSIDE WATER: THE ‘SCALE OF FOUR’

Experiment N°. 15 performed in the afternoon of July 25th is interesting, and amply reported in the 1773 published article. This is the description given in the journal of the experiments:

> Walsh taking hold of 12 inch fish with both hands above and below both Honeycombs and handling him pretty roughly, raising him briskly up and down in the water and out of it, gave a great number of violent shocks – In water the Shocks is much weaker than in air; and the deeper he is in, the weaker the Shock seemed – Shock strongest in the action of raising him with quickness out of the water into air and plunging him again – Received in this way at least 100 Shocks in all ways, i.e. in Air, in Water, and in plunging him out of Air into Water &c.(WE, 168 L.H.)

In the published article the intensity of the shock perceived was quantified according to a kind of «scale of four»:

> The shocks in water appeared, as far as sensation could decide, not to have a fourth of the force of those at the surface of the water, nor much more than a fourth of those intirely in water.

In the journal of the experiments the ‘scale of four’ is absent from the description of the experiment itself, but appears in «The Observation verbatim of Mr. Walsh written by him the next morning», though there Walsh simply says that the shocks given by the torpedo under water are:

> greatly weaker than in air, perhaps not a quart part of the strength, at least to our sense of it. (WE, 170 L.H.)

In other words, there is no indication here that a difference between the shock felt with the torpedo fully plunged in water was a fourth of the shock felt with the torpedo at the surface of the water. Moreover, in experiment N°. 3 of July 26th (the last day in La Rochelle, a Sunday) we find the following annotation that seems to contradict the article:

> N.B. The Shock estimated a quarter part less in water than in air. No difference in Shock, whether fish is deep in water or no. (WE, 171 L.H.)

To return to July 25th, among the experiments made in Captain Bernard's boat, there is one in which the fishermen were invited to put their foot on the torpedo (very likely in the water):

> the animal weak, they felt small Shocks – Arthur also put his feet on him, but felt nothing.(WE, 169 L.H.)

This experiment is clearly aimed at testing, albeit imperfectly, what the fishermen had frequently said, that they felt the shock by placing their foot over a torpedo partially buried under the sand. In natural circumstances, according to their report, the shock could be so strong that they were overset by it.

Among the experiments of July 26th the most interesting are those in which the torpedo was put in a net, and the net dipped in the water:

> felt the Shock thro' net several times, net all wet. Arthur standing on wet grounds - Holding net with one hand or both indifferent.(WE, 172 .H.)

This again gave experimental support to the fishermen who had said that the shock was normally felt through the fishing net. The experiments were repeated with all the four available fish and were particularly successful with the freshly obtained torpedo.

In «The Rough Minutes verbatim of Mr. Walsh» recorded soon after the description of these experiments we find the following consideration:

> Confirmed the experiments of yesterday concerning the effect in Air and in Water in the completest and most satisfactory manner; the second Torpedo giving his Shocks most liberally, and smartly. Remarkable feel from the splashing of the water upon the arm in plunging. (WE, 172 L.H.)

In this «feel» of the shock «from the splashing of the water» Walsh probably arrived nearest to his main aim during these last experiments, the demonstration that the shock of the torpedo could be transmitted by water *tout-court* even in the absence of wet cords, nets or other solid supports.

FINAL CONSIDERATIONS

Soon afterwards we find a series of «Inferences from the Experiments in Water of Saturday & Sunday the 25th & 26th July», the final conclusions of the journal of the experiments at La Rochelle:

> 1st. That the Shock in water ceteris paribus is one fourth of the strength of that in air.

> 2d. That the torpedo can affect animal bodies in water by single contact.
>
> 3d. That the effect from the double contact in water is ceteris paribus nearly double to that from the single contact.
> This is not well proved, the circumstances being different.
>
> 4th. That the Torpedo in water can affect a person in air by single contact thro' a metalline medium, the person touching the water with the other arm.
>
> 5th. That in the same way it can affect thro' a metalline medium continued by a wet pachthread – Whether by the pachthread thro' water as well as thro' air remains doubtful.
>
> 6th. That it can affect thro' a metalline medium, the conduction being completed thro' the boat instead of immediate communication with the water. (WE, 173-4 L.H.)

These words end John Walsh's journal of the 'Experiments on the Torpedo or Electric Ray at La Rochelle and l'Isle de Ré – in June July 1772', but does not end Walsh's interest in electric fish, which had in La Rochelle and the neighbouring places its most intensive expression during this period. On July 28th Walsh left La Rochelle, and begins the long journey back home, through Angers, Le Mans, Alençon, Montaigne, Val Dieu, La Trappe, Paris. During his return journey Walsh did not make any reference to the torpedo or electrical experiments, except for the day of the departure of which he wrote:

> Left La Rochelle at 8 o' Clock in the morning having presented our electrical Machine, Acon and all the apparatus to M. Saunier. (WJ, 57 L.H.)

III PART

AFTER LA ROCHELLE: WALSH AND CAVENDISH

AUGUST 27TH: A SECOND LETTER TO FRANKLIN FROM PARIS

The last page of the Manchester manuscript was written on August 17th while Walsh was in La Ferté. We don't know if this is because Walsh decided to stop writing for some reason, or if the last pages of the manuscript have been lost. We do know that ten days later Walsh was in Paris, because he wrote a second letter to Franklin from there dated August 27th, reported, though only in part, in the 1773 published article.[106] In this letter Walsh makes a rapid survey of the second phase of his experiments. He mentions that, notwithstanding «The vigour of the fresh taken torpedos [*sic*] at the Isle de Ré», it had been impossible:

> to force the torpedinal fluid across the minutest tract of air; not from one link of a small chain, suspended freely, to another; not through an almost invisible separation, made by the edge of a penknife, in a flip of tinfoil past on sealing-wax. The spark therefore (of course the attendant snapping noise) was denied to all our attempts to discover it, not only in day-light, but in complete darkness.

As with the Reflections of July 18th, Walsh compares the electricity of the torpedo to that of a fluid that is «condensed in the instant of the explosion, by a sudden energy of the animal», and in this way he explains «why no signs of attraction or repulsion were perceived in the pith balls». He alludes to the public demonstration given at La Rochelle, in which it was possible to convey:

> torpedinal electricity […] through a circuit, formed from one surface of the animal to the other, by two long brass wires, and four persons, which number, at times, was increased even at eight.

Walsh's main conclusion is that:

> the effect of the Torpedo appears to arise from a compressed elastic fluid, restoring itself to the equilibrium in the same way, and by the same mediums, as the elastic fluid compressed in charged glass.

It would seem therefore that in this new letter to Franklin there is no new analysis, with respect to the content of the journal of the experiment, at least as far as the passages of the letter reported in the published article are concerned. The reference to the pneumatic model is, however, significant because it suggests that this type of model was now at the centre of Walsh's effort to account for the important difficulties that had emerged in La Rochelle, relating to the problem of the perfect identity of torpedinal and artificial electricity.

DENSE AND RARE ELECTRICITY AND THE ABSENCE OF SPARKS

In the rest of the published article Walsh will clearly recognise these difficulties, but will not consider them an insurmountable objections to the perfect identity of «the Torpedo and the charged Phial [i.e. the Leyden jar]».

[106] Walsh returned to England on September 17 as announced by *The Public Advertiser* of September 18 (as pointed out in "The papers of Benjamin Franklin", vol. 19, p. 295, footnote 6; see FRANKLIN 1960-).

After considering the similar properties of the shock of the torpedo and that of the Leyden jar, Walsh writes:

> But it may be objected, that the effects of the Torpedo, and of the charged Phial, are not similar in all their circumstances; that the charged Phial occasions attractive or repulsive dispositions in neighbouring bodies; and that its discharge is obtained through a portion of air, and is accompanied with light and sound; nothing which occurs with respect to the Torpedo.
> The inaction of the electricity of the animal in these particulars, whilst its elastic force is so great as to transmit the effect through an extensive circuit and in its course to communicate a shock, may be a new phænomenon, but is no ways repugnant to the laws of electricity; for here too, the operations of the animal may be imitated by art.

Immediately afterwards, Walsh suggests how, in this particular case, «the operations of the animal may be imitated by art» without contradicting «the laws of electricity». The relevant passages of Walsh's article are worth quoting *in extenso* because they seem to prefigure important notions of electricity.

> .
> The same quantity of elastic matter, according as it is used in a dense or rare state, will produce the different consequences. For example, a small Phial, whose coated surface measures only six square inches, will, on being highly charged, contain a dense electricity capable of forcing a passage through an inch of air, and afford the phænomena of light, sound, attraction, and repulsion. But if the quantity condensed in this Phial, be made rare by communicating it to three large connected jars, whose coated surfaces shall form together an area 400 times larger than that of the Phial (I instance these jars because they are such as I use); it will, thus dilated, yield all the negative phænomena, if I may so call them, of the Torpedo; it will not now pass the hundredth part of that inch of air, which in its condensed state it sprung through with ease; it will now refuse the minute intersection in the strip of tinfoil; the spark and its attendant found, even the attraction or repulsion of light bodies, wil1 now be wanting; nor will a point brought however near, if not in contact, be able to draw off the charge: and yet, with this diminished elasticity, the electric matter will, to effect its equilibrium, instantly run through a considerable circuit of different conductors, perfectly continuous, and make us sensible of an impulse in its passage.

The various effects of electricity therefore do not depend in an exclusive and univocal way on the quantity of electric matter involved in the specific phenomenon under consideration; they may depend also, and significantly, on the «dense or rare state» that the electric matter may assume, due to its «elastic» nature. A condensed state, such as in «a small Phial [...] highly charged», will easily result in a series of effects (sparks, sounds, attraction and repulsion) that are absent when the same quantity of matter is put in a «rare» or «dilated» state, by communicating it to a number of large Leyden jars. We know now that the effects that Walsh believed resulted from the «dense» state of the electric matter, are produced by electricity when it is at a high potential. Therefore, in Walsh's pneumatic analogy, «density» corresponds to the 'potential' of modern electrology.

Other effects, such as the passage «through a considerable circuit of different conductors, perfectly continuous» and the «sensation of impulse» (i.e. the shock) that it causes, may occur even in the «rare» state of the electricity, i.e. in a condition that corresponds, in modern terminology, to a low level of electric potential.

According to this pneumatic analogy, the shock of the torpedo is produced by a large quantity of electric matter present in a relatively rare state, compared to the conditions prevailing in the devices generally used in the *cabinets de physique* of the 18^{th} century, where, as we know, small quantities of electricity were normally present at very high potentials (usually more than 10.000 Volts) in electric machines or Leyden jars. The level of the potential involved in the common electric experiments of the epoch can be appreciated by remembering that one degree of a common electrometer of the age, William Henly's quadrant electrometer, corresponds approximately to 1335 modern units, i.e. Volts.

Although, as already noted, some reference to a pneumatic model of electricity is present in the journal of the experiments, and also in the last letter to Franklin (the one sent from Paris on August 27^{th}), nevertheless only in the printed article is the pneumatic analogy used to account for the lack of some characteristic manifestations of electricity in the torpedo's shock. This suggests that this type of conceptual elaboration emerged in a relatively late phase of Walsh's analysis of the mechanism of the torpedo's shock. Probably this happened during the preparation of the paper presented to the Royal Society, eventually published in 1773. In all likelihood it emerged from discussions that Walsh had with Henry Cavendish. A hint of Cavendish's importance for Walsh's attempt to account conceptually for the shock of the torpedo came from a reference to Cavendish that Walsh makes in the final part of his 1773 article:

> Let me here remark, that the sagacity of Mr. Cavendish in devising and in his address in executing electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air.

ELECTRIC ORGANS: BATTERIES OF MANY VESSELS

After alluding to Cavendish's primacy in showing that artificial electricity could be brought to a condition in which a shock is produced, even though it could not pass through a small gap and produce a spark, Walsh speculates on the morphological arrangements whereby electricity could exist in the torpedo in such a condition of low density:

> But after the discovery that a large area of rare electricity would imitate the effect of the Torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This, however, we know, that from an infinite division of parts infinite surface may arise, and even our gross optic tell us, that those singular organs, so often mentioned, consist like our electric batteries of many vessels, call them cylinders or hexagonal prisms, whose superficies taken together furnish a considerable area.

Although not explicitly, Walsh is anticipating here one of the important notions emerging from the anatomical study performed by John Hunter on the torpedoes of La Rochelle, the subject of a communication presented 1773 to the Royal Society in July, and of a subsequent paper published in the *Philosophycal Transactions*, immediately after Walsh's article. Hunter tried to be precise and quantitative in the description of the fish organs responsible for the shock, that he called «electrical». In the torpedoes examined by Hunter, each of these organs:

> consists wholly of perpendicular columns, reaching from the upper to the under surface of the body, and varying in their lengths, according to the thickness of the parts of the body where they are placed.
> The figures of the columns are very irregular, varying according to situation and other circumstances. The greatest number of them are either Hexagons, or irregular Pentagons;
> [.....]
> The number of columns in different Torpedos of the size of that now offered to the Society, appeared to be about 470 in each organ, but the number varies according to the size of the fish (in a very large Torpedo, the number of columns in one electric organ were 1182).
> [.....]
> Each column is divided in horizontal partitions, placed over each other, at very small distances, and forming numerous interstices, which appear to contain a fluid. These partitions consist of a very thin membrane, considerable transparent.
> [.....]
> The number of partitions contained in a column of one inch length, of a Torpedo which had been preserved in proof spirit, appeared upon a careful examination to be one hundred and fifty.

Hunter's morpho-quantitative data will be used by Cavendish in 1776 for his work on the mechanism of the torpedo's shock, based on the construction of an 'artificial torpedo', a device capable of delivering a shock similar to that of the fish when fed by a 'battery' of 49 large Leyden jars connected in parallel (see later). Cavendish's aim was to evaluate whether a sufficiently large area existed in the fish organ to accommodate the «extremely great» quantity of electric fluid involved in the shock, at a «degree of electrification» so low as to imitate perfectly the shock of the real torpedo. After considering the difficulty in envisioning a physiological mechanism corresponding to that of the physical model, he wrote:

> There seems, however, to be room in the fish for a battery of a sufficient size; for Mr. HUNTER has shewn, that each of the prismatical columns of which the electric organ is composed, is divided into a great number of partitions by fine membranes, the thickness of each partition being about the 150th part of an inch: but the thickness of the membranes which form them is, as he informs me, much less. The bulk of the. two organs together in a fish 10 ½ inches broad, that is of the fame size as the artificial torpedos, seems to be about 24 cubic inches; and therefore the sum of the areas of all the partitions is about 3700 square inches. Now 3700 square inches of coated glass 1/150 of an inch thick will receive as much electricity as 30500 square inches, 055 of an inch thick; that is, 305 times as much as the plate of crown glass mentioned in p. 206, or about 2 ¼ times as much as my battery, supposing both to be electrified by the same conductor; and if the glass is five times as thin, which perhaps is not thinner than the membranes which form the partitions, it will contain five times as much electricity, or near fourteen times as my battery.

In the course of the work leading to his paper on the artificial torpedo, Cavendish did not carry out any experiments on a real torpedo (due to the great difficulty over obtaining live specimens in London). How could he then evaluate the similarity of the shock produced by the artificial torpedo (built up using wood or leather) and the real one? He used Walsh's evidence. Describing the results of his experiments on one of the versions of his 'artificial torpedo' (the ‘leathern’ one), concerning the intensity of the shock produced in a particular circumstance, he wrote:

> which, I am inclined to think, from my conversation with Mr. WALSH, may be considered as about the medium strength of those of a real one of the same size as this.

He then compares the sensation produced by a strongly charged small jar or a «plate of glass» (i.e. a Franklin square capacitor), with that of a «large battery [of Leyden jars] electrified so weakly that the shock shall be of the same strength», and finding «the former being sharper and more disagreeable», comments:

> Mr. WALSH took notice of this difference; and said, that the artificial torpedo produced just the same sensation as the real one.

Apart from their specific interest, the above quoted passages from the articles of Walsh, Hunter and Cavendish, show that in the years that followed Walsh’s experiments at La Rochelle, there had been an active and reciprocal collaboration between Walsh and the colleagues that he had succeeded in involving in his electric fish researches. Besides the work on the anatomy of the torpedo, Hunter also carried out a study on the gymnotus, using one of the eels that Walsh had succeeded in importing from Surinam to England in 1775.

The work of Cavendish needs to be described in some detail, because it forms an integral part of the research program Walsh had begun with his journey to France in 1772. Moreover, it also helped to gain acceptance for the electrical nature of the fish shock, and promoted the subsequent developments of the science of electricity.

WALSH AND ELECTRIC FISH AFTER 1773: THE TRANSMISSION OF THE TORPEDO SHOCK THROUGH A HEMPEN CORD

Walsh’s interest in electric fish research continued after 1773, the year his first article on the torpedo was published. In 1774, he published his study on British torpedoes, based on the anatomical investigation of two specimens both caught off the Torbay coast, in August and in November 1773 respectively, one of which was the giant one already alluded to (see above). In 1775 Walsh carried out his famous (and unpublished) experiments on the production of a spark from the shock of the electric eel, and in 1778 he still kept some live gymnotus specimens in his London house and kept up his interest in them, although we have no evidence that he pursued his researches in this field any further.

However, before turning his attention to the electric eels, in the years that followed his return to England Walsh tried to continue his researches on the torpedo, though indirectly, following the plan he had outlined in his speech to the *Académie* of La Rochelle on July 22nd 1772:

> That Nature had denied the animal to our Country and that the pursuit rested in a particular manner on them whose Shores abounded with it.

Among the papers preserved in the *Académie* is the transcription of the extract of a letter Walsh sent to M. Seignette «*datée de Londres 5 avril 1774*», in which he asked his correspondent (the Perpetual Secretary of the *Académie* and Mayor of the town) to investigate a particular point concerning the transmission of the torpedo’s shock through some non-metallic bodies:

> The question is to know if the wood & the hemp which we know to be less perfect conductors of electricity than water, could, if plunged in it, transmit a commotion across this liquid to a person in the air; although in this occasion the water appears to us to be a shorter and, we might say, better conductor between the two sides of the Torpedo; although the thing does not appear to be likely, & nevertheless it has been reported to us and claimed in a positive way. A Captain of a war vessel has assured me that, having he struck a Torpedo with an harpoon at his feet under the water, he did feel a strong commotion through the harpoon; and all fishermen agree in saying that they experience the same sensation every time they happen to meet with a Torpedo in their nets, even when the fish and the nets are still in the water.[107]

The problem Walsh is dealing with in this letter is very closely connected to the object of the experimental studies carried out in the last days at La Rochelle, when various attempts were made to establish whether the electricity of a torpedo could take various routes when in the presence of various possible circuits, especially when the fish was in water. Walsh had tried to discover whether a shock could be felt by plunging a metallic rod or an arm into the water, either touching the animal or not touching it, and he eventually investigated whether the shock could be felt by plunging a «Wet cord, 4 inches, tied to the iron» into the water. These experiments were also reported by Walsh in his 1773 published article, where, among other things, we learn that the cord used was a «wet hempen cord». Although he derived some inferences from the results obtained, he nonetheless recognised that no firm evidence could be drawn concerning the transmissibility of the shock through «harpoons and nets, consisting of wood and hemp». He concluded:

> I mention the omission in the hope that some one may be induced to determine the point by express trial.

The request to Seignette in the letter of 1774 seems to be directed at this. He reminds his correspondent of the experiments in La Rochelle showing that metals, and also the arms of a person, were capable of transmitting the shock when plunged into the water, quite understandable - he says - because «the one and the other substance are better conductors than water». According to Walsh, the reports that harpoons or fishing nets can also transmit the torpedo’s shock might be verified:

> by touching the Torpedo under the water with a stick armed with metal at one extremity, and afterwards with a hempen chord; only paying attention that the chord or the stick would be, in part, plunged in the water.

He goes on to say that he had already carried out a similar experiment with the artificial electricity:

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[107] *Archives Départementales de La Charente Maritime* 103, J. *Archives de l’Académie de Sciences et Belles Lettres et Arts de La Rochelle, liasse 47, pièce 75..pp. 78-79.* This extract is transcribed just after a French translation Walsh’s 1773 article on the ‘Philosophical Translation’. Interestingly the person who made the translation (likely M. de Villamarais, see TORLAIS 1959) writes that he added the transcription of the extract of the letter «*pour remplir le blanc qui me reste*» («to fill the remaining blank pages».

> I have plunged an electrified bottle [i.e. a charged Leyden jar] in the water, & there, through a metallic conductor I have tried, in discharging it, to make one person in the air to feel the same sensation that the Torpedo is capable of producing; but this experiment is hard to do, & the electricity which in involved in it, being in a more dense state, is not at all similar to that of the Torpedo. Up to this day, I have only used arms or metals to transmit the commotion through the water, and with these conductors I have perfectly succeeded.

The letter to Seignette therefore offers another instance of him using a physical experiment to ascertain the mechanism underlying a biological process, matching the actual exchange between the physical and biological dispositions that, characterised his methodological approach to the study of the electric fish. It suggests, moreover, that the problems that had emerged in the course of the investigations at La Rochelle probably remained central to Walsh in the period of his active interest in electric fish, a period concluded by the experiments on the eel carried out in the summer of 1775. It is possible that when he wrote to Seignette, Walsh was also involved in discussions with Cavendish about the various difficulties emerging from the electric hypothesis of the torpedo's shock, and in particular, the problem of the transmission of the shock in water, one of the most important difficulties Cavendish was tackling in his study on the 'artificial torpedo'. This might have induced Walsh to write to his French colleague. It is possible, moreover, that the experiments in which Walsh plunged his «electrified bottle» into the water (in order to ascertain the transmission of the torpedo's shock «through the wood or the hen») helped Cavendish to envision the contrivance that eventually resulted in his 'artificial torpedo'.

Cavendish's results here were a successful development of Walsh's research on electric fish along the «walk of physics», that Walsh had indicated as one of the possible directions of the «large field of interesting inquiry» that might be opened up by the investigations on the torpedo.

Before dealing with Cavendish's studies connected to the investigation on electric fish, it is worth quoting the conclusion of Walsh's article on the 'Torpedoes found in the coasts of England. Here the exchange between physiology and physics which characterises Walsh's work is treated with typical British humour, with reference to the therapeutic virtues of the torpedo's shock for old medicine:

> I have thus shewn that Great Britain too claims the Torpedo, or Electric Ray; that ours is the *broad marine* sort, which Socrates, as Meno thought, resembled; and that it is the *black Torpedo*, whose influence subdues Head-aches, and the Gout itself. In announcing to our Naturalists and Electricians the presence of this wonderful guest, I would certainly felicitate our Invalids on their acquisitions, but that *the Leyden phial contains all his magic power.*

The first classical reference here to the theme of the torpedo is to Plato's 'Meno' where the protagonist compares Socrates to a torpedo, because of the 'benumbing' effects of his conversation:

> O Socrates, I used to be told, before I knew you, that you were always doubting yourself and making others doubt; and now you are casting your spells over me, and I am simply getting bewitched and enchanted, and am at my wits' end. And if I may venture to make a jest upon you, you seem to me both in your appearance and in your power over others to be very like the flat torpedo fish, who torpifies those who come near him and touch him, as you have now

torpified me, I think. For my soul and my tongue are really torpid, and I do not know how to answer you; and though I have been delivered of an infinite variety of speeches about virtue before now, and to many persons - and very good ones they were, as I thought - at this moment I cannot even say what virtue is. And I think that you are very wise in not voyaging and going away from home, for if you did in other places as you do in Athens, you would be cast into prison as a magician.[108]

The other reference is to Scribonius Largus, a physician of the Roman Empire who advised the use of the torpedo shock for the treatment of pain. This is Scribonius' prescription for alleviating the pain caused by gout:

For any type of gout a live black torpedo should, when the pain begins, be placed under the feet. The patient must stand on a moist shore washed by the sea, and he should stay like this until his whole foot and leg up to the knee is numb. This takes away present pain and prevents pain from coming on if it has not already arisen. In this way Anteros, a freedman of Tiberius, was cured.[109]

For headaches, Scribonius writes:

Headache even if it is chronic and unbearable is taken away and remedied forever by a live torpedo placed on the spot which is in pain, until the pain ceases. As soon as the numbness has been felt the remedy should be removed lest the ability to feel be taken from the part. Moreover, several torpedo's of the

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Ὦ Σώκρατες, ἤκουον μὲν ἔγωγε πρὶν καὶ συγγενέσθαι σοι ὅτι σὺ οὐδὲν ἄλλο ἢ αὐτός τε ἀπορεῖς καὶ τοὺς ἄλλους ποιεῖς ἀπορεῖν· καὶ νῦν, ὥς γέ μοι δοκεῖς, γοητεύεις με καὶ φαρμάττεις καὶ ἀτεχνῶς κατεπᾴδεις, ὥστε μεστὸν ἀπορίας γεγονέναι. καὶ δοκεῖς μοι παντελῶς, εἰ δεῖ τι καὶ σκῶψαι, ὁμοιότατος εἶναι τό τε εἶδος καὶ τἆλλα ταύτῃ τῇ πλατείᾳ νάρκῃ τῇ θαλαττίᾳ· καὶ γὰρ αὕτη τὸν ἀεὶ πλησιάζοντα καὶ ἁπτόμενον ναρκᾶν ποιεῖ, καὶ σὺ δοκεῖς μοι νῦν ἐμὲ τοιοῦτόν τι πεποιηκέναι, [ναρκᾶν]· ἀληθῶς γὰρ ἔγωγε καὶ τὴν ψυχὴν καὶ τὸ στόμα ναρκῶ, καὶ οὐκ ἔχω ὅτι ἀποκρίνωμαί σοι. καίτοι μυριάκις γε περὶ ἀρετῆς παμπόλλους λόγους εἴρηκα καὶ πρὸς πολλούς, καὶ πάνυ εὖ, ὥς γε ἐμαυτῷ ἐδόκουν· νῦν δὲ οὐδ' ὅτι ἐστὶν τὸ παράπαν ἔχω εἰπεῖν. καί μοι δοκεῖς εὖ βουλεύεσθαι οὐκ ἐκπλέων ἐνθένδε οὐδ' ἀποδημῶν· εἰ γὰρ ξένος ἐν ἄλλῃ πόλει τοιαῦτα ποιοῖς, τάχ' ἂν ὡς γόης ἀπαχθείης.

(PLATO, 'Meno' 80 - English translation by Jowett in 'The Dialogues of Plato' 1892, p. 39, Sect. 80.

109 «*Ad utramlibet podagram torpedinem nigram vivam, cum accesserit dolor, subiscere pedibus oportet stantibus in litore non sicco, sed quod alluit mare, donc sentiat torpere pedem et in futurum remediat. Hoc Anteros Tiberii Cesaris liberti supra heredidates remediatus est.*» (see SCRIBONIUS, 1983, Compositiones Medicae, CXLII). Besides this use of the shock of a live torpedo as a method for alleviating pain, likely effective as a kind of electrotherapy *ante litteram*, parts of the fish body, and notably the liver, were employed to prepare potions or oils with curative effects, often with indications that they would be effective only if prepared according a particular rituality, in a particular moment of the year, during a particular phase of moon, or if some magic words were pronounced etc. (see for instance TRALLIANUS 1557, pp. 71 and 115; RONDELET, 1575, p. 1221)

> same kind should be prepared because the cure, that is, the torpor which is a sign of betterment, is sometimes effective only after two or three.[110]

CAVENDISH AND THE 'ARTIFICIAL TORPEDO'

The themes central to Cavendish's study of the 'artificial torpedo' are related to some of the main difficulties emerging in Walsh's researches (and more in general in the science of the age), in the attempt to assign an electric nature to the shock of the torpedo (and of similar 'torporific' or 'benumbing' fish). In particular, the possibility that the fish, being in its natural habitat, could transmit its shock at a distance, to affect possible prey or predators, seemed in conflict with the common sense laws of the electrology of the age: the electricity responsible for the shock should have passed through the shortest circuit, i.e. the thin layer of water surrounding the fish's body, and thus any action at a distance would be prevented. The other difficulty came from the impossibility for the shock of the torpedo to produce sparks or sounds and cross «the minutest separation» in the circuit of discharge, an impossibility repeatedly verified by Walsh in his experiments, in spite of his numerous efforts to create the optimal experimental conditions for these events to occur.

For the first difficulty, the problem of the diffusion of the shock in water had been faced experimentally by Walsh, especially in the last days of his experiments in La Rochelle. The results of these experiments had led Walsh to the conclusion that electricity could flow through different circuits and need not necessarily pass through the shortest one (as he wrote in a late addition to the journal of the experiments «the electrick effect often makes more Circuits than one»). This is just Cavendish's departure point when he tackles his investigation into the torpedo's mechanism, clearly set out in the initial part of his 1776 article, where he discusses the problems raised by the electrical hypothesis of the torpedo's shock advocated by Walsh:

> One of the principal difficulties attending the supposition, that these phenomena are produced by electricity, is that a shock may be perceived when the fish is held under water; and in other circumstances, where the electric fluid hath a much readier passage than through the person's body. To explain this, it must be considered, than when a jar is electrified, and any number of different circuits are made between its positive and negative side, some electricity will necessarily pass along each; but a greater quantity will pass through those in which it meets less resistance, than in those in which it meets more. For instance, let a person take some yards of very thin wire, holding one end in each hand, and let him discharge the jar by touching the outside with one end of the wire, and the inside with the other hand; he will feel a shock, provided that the jar is charged high enough; but less than if he had discharged it without holding the wire in his hands; which shews, that part of the electricity passes through his body, and part through the wire.

Soon afterwards Cavendish discusses the assumption that electricity flows exclusively through the «shortest circuit»:

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[110] «*Capitis dolorem quamvis veterem et intolerabilem protinus tollit et in perpetuum remediat torpedo nigra imposit eo loco, qui in dolore est, donec desinat dolor at obtupescat ea pars. quod cum primum senscrit, removeatur remedium, ne sensus auferatur eius partis. plures autem parandae sunt torpedines eius generis, quia nonnunquam vix ad duas tresve respondet curatio, id est torpor, quod signum est remediationes.* (see SCRIBONIUS, 1983, Compositiones Medicae, XI)

> Some electricians indeed seem to have supposed that electric fluid passes only along the shortest and readiest circuit: but besides that such a supposition would be quite contrary to what is observed in all other fluids, it does not agree with experience. What seems to have led to this mistake is, that in discharging a jar by a wire held in both hands, as in the above mentioned experiment, the person will feel no shock, unless either the wire is very long and slender, or the jar is very large and highly charged. The reason of which is, that metals conduct surprizingly better than the human body, or any other substance I am acquainted with.

To support his contention Cavendish writes that he had estimated that rain or distilled water is about 400 million times less conductive than iron and 100 times less conductive than sea water. For Cavendish the extremely low conductive power of water, compared to metal, made it even more likely that the torpedo shock could diffuse at distance: due to the high resistance of the fish's liquid habitat, the electric fluid had difficulty in passing entirely through the water film immediately surrounding fish body, and a considerable part of it passed through distant objects.

As to the way Cavendish estimated the relative resistance of water and iron, although he writes that this «appears from experiments of which [he] propose[s] to lay an account before the Society», he will never carry out his proposal, leaving the acceptance of his estimates to be taken on trust. The laboratory journal of Cavendish's electric researches was discovered and published by Maxwell, so the method used for these measurements could be ascertained. As Maxwell put it in 1879 on presenting 'The Electrical Researches of the honourable Henry Cavendish':

> We learn from the manuscripts now first published, that Cavendish was his own galvanometer. In order to compare the intensity of currents he caused them to pass through his own body, and by comparing the intensity of the sensations he felt in his wrist and elbows, he estimated which of the two shocks was the more powerful.[111]

Maxwell remarks on the accuracy of the measurements that Cavendish had been able to obtain by this method. For instance, the value of 400,000,000 for the relative resistance of rain water to iron would agree with the value calculated by Faraday on the basis of modern experimental measurements by assuming that Cavendish made his experiments at 11°C . An interesting aspect emerging from the laboratory journal is that Cavendish made his first resistance measurements in November 1772 (the relative resistance of rain water and sea water) and in November 1773 (various measurements including a comparison of the resistance of iron and salt water). The times correspond to Walsh's return from France and the presentation of Walsh's 1773 paper to the Royal Society, suggesting that Cavendish's interest in these experiments was prompted by Walsh's studies at an early period, long before the publication of the 1776 paper on the artificial torpedo. It may be observed that, given the time of year, Maxwell's suggestion that Cavendish's experiments were done at 11° appears not improbable.

The passage where Cavendish outlines the diffusion of torpedo electricity in water is worth quoting *in extenso* because it makes reference to a famous figure in which, for the first time in science history, the spatial diffusion of a current is illustrated graphically:

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[111] MAXWELL 1879, p. lvii.

> if torpedo is immersed in water, the fluid will pass through the water in all directions, and that even to great distances from its body, as represented in Fig. 1. (our Fig.16) where the full lines represents the section of its body, and the dotted lines the direction of the electric fluid; but it must be observed, that the nearer any part of the water is to the fishes body, the greater quantity of fluid will pass through it.

As well as developing aspects of Walsh's investigation dealing with the action of the torpedo in water, Cavendish will also work in a masterly way on another of Walsh's suggestions: that the shock of the torpedo corresponds to the emission of a large quantity of electric matter, springing from the animal with a rather low force because of its relatively «rare» state compared to the condition of a highly charged small Leyden jar. This suggestion was put forward by Walsh in his 1773 article to explain why the shock of the torpedo was incapable of producing a spark or other evident marks of electricity, while at the same time it could produce a strong commotion. This problem underlay the «Remark of a future date» of the journal in one of the pages of July 16th:

> The Torpedo often gives severe Shocks, his Electricity therefore cannot be deemed weak. . (WE, 88 L.H.)

In Cavendish's development, the idea that the electric effects depend specifically on two different parameters, the quantity of electric fluid and the «degree of electrification», look forward to the notions of 'quantity of charge', electric 'tension' and the laws of capacitors that Alessandro Volta formulated in their full form in 1782.

The way in which Cavendish works on this aspect of the difficulties in assigning an electric nature to the shock of the torpedo, reveals an extraordinary sagacity in the English scientist, at both rational and experimental levels. His paper is a classic in the history of electricity, and has already been analysed by several scholars from the perspective of its contribution to the history and progress of physics. We will restrict ourselves to a rather short account specifically angled to point out how in this case a fundamental development of the physics of electricity emerged from work on a particular aspect of 'animal oeconomy'.

The first problem Cavendish approached in trying to explain why the torpedo could give a pretty strong shock even though it was unable to generate sparks or sounds (and other manifest marks of electricity), was to determine the maximum distance at which a spark could be generated by the discharge of artificial electricity, and, in particular, how this distance depended on the characteristics of this electricity. In order to do that, he took several Leyden jars and connected them (in parallel) to the prime conductor of an electrical machine and «electrified them in a given degree, as shewn by a very exact electrometer». Afterwards, he measured the maximum sparking distance of this assembly of jars by using a Lanes' type electrometer. He found that the sparking distance did not change if the four jars were charged to the same degree with the same arrangement, but the spark was drawn from only one, after separating it from the remaining three. Cavendish also noticed that the divergence of the electrometer did not change with this manoeuvre. The conclusion was that:

> the force with which the fluid endeavours to escape from the single jar is the same as from all the jars together.

And furthermore:

> that the distance to which the spark will fly is not sensibly affected by the number or size of the jars, but depends only on the force they are electrified; that is, on the force with which the fluid endeavours to escape from them: consequently, a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified to such a degree, that the spark shall fly to the same distance; for it is well known, that a large jar, or a great number, will give a greater shock than a small one, or a small number, electrified with the same force.

With this experiment, Cavendish showed that the sparking distance depended on a electric parameter, measurable by the electrometer as «force» or «degree of electrification», and did not depend on the quantity of the electric fluid involved, a parameter that, on the other hand, had a great influence on the intensity of the commotion felt by the experimenter.

The importance of the quantity of electric fluid in the production of commotion appears clearly, because, for a given degree of electrification, the intensity of the shock increases with the size and/or the number of the Leyden jars (in parallel) used to produce it. Moreover, Cavendish shows that a stronger shock is produced by fours jars charged to a lower degree than a single one, even if these are charged to a lower degree that results in a sparking distance which is half that produced by the single jar.

Having ascertained the different dependence of the two effects considered (the sparking distance and strength of shock) on the two parameters of the electricity (the degree of electrification measured by the electrometer, and the quantity of electric fluid involved varying according to the size and number of the jars), Cavendish tries to show that, if the quantity of electric fluid produced is very great (by using many large jars connected in parallel), a shock can be produced even when the degree of electrification is so low that the fluid cannot produce a spark with the shortest possible interruption of the discharging circuit. For these experiments Cavendish used a «battery [...] composed of 49 large jars, of extremely thin glass, disposed in rows, and so contrived that [he] could use any number of rows [he] chose».

The problem with this experiment lay in the difficulty in ascertaining the «degree of electrification» of the battery when it was charged so weakly as to produce an unreliably small divergence in the indicators of the electrometer. The method Cavendish used to solve this problem, partly derived from an approach already devised by Giovan Battista Beccaria, was very ingenious, and was based on the progressive loss of charge of a given assembly of Leyden jars when they were repeatedly discharged by connecting them to a square capacitor. Of this capacitor made of «crown glass» Cavendish had previously ascertained the quantity of electricity it could contain for a given degree of electrification. An important footnote is present in Cavendish's article with relation to the electric properties of «coated glass»:

> I find, by experiment, that the quantity of electricity which coated glass of different sizes and shapes will receive with the same degree of electrification, is directly as the area of the coating, and inversely as the thickness of the glass; whence the proportion with the quantity of electricity in this battery bears to that in a glass or jar of any other size, may easily be computed.

If we consider that, in Cavendish's language, «degree of electrification» corresponds to the modern concept of electric potential, then we can find here a rather accurate anticipation of the laws of the capacitor.

The battery of Leyden jars, appropriately charged to a very low degree, was used by Cavendish to power his 'artificial torpedo' in order to demonstrate that an electric shock can be received even under water. It was also used to show that a shock can be felt even with an electricity apparently incapable of passing «through any sensible space of air». With this last purpose in mind Cavendish performed various types of experiments apparently inspired by those Walsh had carried out in France. The first of these experiments he described as follows:

> I covered a piece of sealing wax on one side with a flip of tinfoil, and holding it in one hand, touched an electrical organ of the torpedo [the artificial one of course] with the end of it, while my other hand was applied to the opposite surface of the same organ. The shock passed freely, being conducted by the tinfoil; but if I made, with a penknife, as small a separation in the tinfoil as possible, so as to be sure that it was actually separated, the shock would not pass, conformably to what Mr. WALSH observed of the torpedo.

In a second experiment he used the adjustable separation of a Lane type electrometer, and found:

> that the shock would not pass, unless the knobs were brought so near together as to require the assistance of a magnifying glass to be sure that they did not touch.

The third type of experiment required a rather complex series of trials to create conditions to bring the similarity between the artificial and the natural torpedo as close as possible. He tried to obtain a shock from the artificial torpedo in conditions in which the discharge was not able to pass across «a chain of small brass wire». These experiments were evidently inspired by those of Walsh, alluded to in the letter to Franklin of 27th August 1772, where Walsh had written that he was unable to:

> force the torpedinal fluid across the minutest tract of air; not from one link of a small chain, suspended freely, to another.[112]

In Cavendish's initial attempts with a metallic chain, a shock was felt only with the battery charged to such a level that «the electricity was able to force its way through four or five intervals of the links». In these experiments, when «the electricity passed through the chain, a small light was visible, provided that the room was quite dark». The next step was to reduce the charge of the battery. However, when:

> the battery was charged only to a fourth or fifth part of its usual height, the shock would not pass through a single interval. But then it was very weak, even when received through a piece of brass wire, without any link in it.

In other words the shock in these circumstance was less strong than that of a real torpedo, as far as Cavendish could judge on the basis of a

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[112] WALSH 1773.

«conversation with Mr. WALSH». He supposed that this could be due to the fact that his largest battery was not large enough to imitate the mechanism of the real torpedo. To ascertain that, he built a special device allowing him to accurately investigate the dependence of the number of links that can be passed by electricity at a given degree of electrification, on the number of the jars. For a shock of a given intensity, the number of links decreases as the number of jars is increased, and this made it probable that, were the number of jars very large, the shock «would not have passed through a single interval of the links of a chain».

Cavendish showed that his artificial torpedo could give shocks when immersed in a basin of salt water imitating the composition of sea water, in a similar way to the real fish. A difficulty, however, arose in the initial experiments made with a wooden torpedo, because the intensity of the shock felt under water appeared to be much smaller than in air, compared to the situation of the real torpedo, according to the results of Walsh's experiments:

> the effects produced by the Torpedo, when in air, appeared, on many repeated experiments, to be about four times as strong as when in the water.

The difference between the intensity of the shock produced by the artificial torpedo in water and in air decreased when fresh water was used instead of salt water. This - Cavendish said - was an indication that, besides salt water, «the human body is also a much better conductor than fresh water».

In order to reduce the disproportion between the intensity of the shock in salt water and in air, Cavendish built a second artificial torpedo using leather. The idea was that, since leather, when «thoroughly soaked with salt water», conducts electricity «very freely», a greater proportion of electricity would pass through it, even when the device was in air, because of the humidity absorbed, and thus the difference of the intensity of the shock in air and in water would be decreased. The experiment was very successful, as Cavendish commented:

> The event answered my expectation; for it required about three times a great charge of the battery, to give the same shock in air, with this new torpedo as with the former; and the difference between its strength when received under water and out of it, was much less than before, and perhaps not greater than in the real torpedo.

The way Cavendish planned the experiments indicates that he had a clear idea of the division of currents between parallel circuits of different conductivity, and this about seventy years before Kirchoff formulated his classical laws.[113] Cavendish assumed that the intensity of the shock depended on the quantity of electricity passing through the experimenter's body, and in air this would represent a larger proportion of the total current. In water the flow of electricity would be divided between a first portion passing through the water, a second one passing through the artificial torpedo, and a third one through the experimenter's body, while in air there would only be the two latter components. It was possible to reduce the relative strength of the shock

[113] In his *Untersuchungen über Thierische Electricität* Du Bois-Reymond remarks that after Cavendish there had not been any real progress in the theory of current division for about seventy years up until Kirchhoff. As we shall see, Volta had a clear vision of the problem of current division among parallel and series circuits, even though he did not express the matter in mathematical terms.

between air and water. It could be done by increasing the proportion of electricity passing through the artificial torpedo by making it of wet leather, and thus more conductive of electricity. This reasoning Cavendish put in mathematical terms, in a way that may be considered the fullest development of the results of the experiments on the real torpedo at La Rochelle: experiments that had led Walsh to note:

> the electrick effect often makes more Circuits than one (WE, 78 R.H.)

The artificial torpedo could imitate the behaviour of the real one in many respects, as Walsh's observations at La Rochelle showed, as well as the accounts of fishermen. Among other things, with his artificial torpedo, Cavendish made the experiment of the shock felt by putting the foot over the torpedo covered with sand, similar to the one Arthur and the fishermen had performed in Bertrand's boat on 25th July 1772.

The general conclusion Cavendish reached from his experiments was:

> that there seems nothing in the phenomena of the torpedo at all incompatible with electricity.

In the final sentence of his paper, Cavendish put this conclusion in a slightly different form. In discussing the case of the impossibility of producing a shock from the torpedo, he wrote:

> the circumstance of their not having perceived any light is by no means repugnant to the supposition that the shock is produced by electricity.

This contains a kind of «signature», clearly linking Cavendish's work to Walsh, who, in his 1773 paper, discussing the impossibility of obtaining 'light and sound' from the shock of the torpedo, had written:

> The inaction of the electricity of the animal in these particulars, whilst its elastic force is so great as to transmit he effect through an extensive circuit and in its course to communicate a shock, may be a new phænomenon, but is *no ways repugnant to the laws of electricity*; for here too, the operations of the animal may be imitated by art. [the italics is mine]

In our commentary on Walsh's 1773 paper we have already discussed the calculations Cavendish made, on the basis of Hunter's anatomical work, concerning the total area of the membranes of the electric organs of a torpedo, which he considered as minute capacitors capable of accumulating a large quantity of electricity because of their large number and their extreme thinness. Cavendish estimates that a real torpedo might contain between 2 ¼ and 14 times more electricity of his large battery of Leyden jars, depending on the assumption made about the thickness of the membranes, and, according to him, this would explain the small discrepancy in the similarity between the behaviour of the real fish and the artificial torpedo.

The idea that the mechanism whereby the real torpedo accumulates electricity is similar to that of a capacitor, and consequently that the electric organs are made up by a large multitude of minute planar capacitors, an idea formulated by Walsh in 1773 and put into a quantitative form by Cavendish, will remain the reference hypothesis in electric fish researches for a long time. In 1797 this idea would be revived by William Nicholson, who assumed that the electric organs could be modelled by thin laminae of mica («Muscovy talc») or by some other analogous material. Like Cavendish, Nicholson developed a quantitative argument based on Hunter's anatomical data.

IV PART

ALESSANDRO VOLTA: FROM ELECTRIC FISH TO THE ‘ARTIFICIAL ELECTRIC ORGAN’

VOLTA AND ELECTRIC FISH: A LETTER FROM LONDON TO A FRENCH LADY

Although a line of development links an important part of the scientific endeavour of Alessandro Volta to Walsh's and Cavendish's work on electric fish, the idea of the electric organs made by capacitors was, on the other hand, strongly opposed by Volta.

The Italian scientist never made experiments with 'real' electric fish; nevertheless he showed great interest in these animals, and a reflection on their singular properties and on the structure of their organs played an crucial role in the path that led him to the invention of the electric battery. The studies that eventually culminated in this invention were started in the spring of 1792, when Volta became interested in Galvani's researches on the involvement of electricity in nerve function and muscle motion. However, a specific awareness of electric fish researches predated by more than ten years Volta's interest in Galvani's researches. Allusions to electric fish studies, and in particular to the experiments in which Walsh had produced a spark from the shock of an electric eel, are in Volta's manuscripts of 1777-1778, and very likely correspond to the content of lectures that Volta held at that time at the University of Pavia. Moreover, in 1782 Volta wrote a letter from London to a French friend and pupil, *Madame* Le Noir de Nanteuil, in which he dealt extensively with animal electricity and electric fish studies, and in particular with the researches of Walsh and Cavendish. At the time of this letter, Volta was visiting London on the occasion of the presentation to the Royal Society of his Memoir concerning the notions of 'tension' and 'electric charge', and of the laws of the capacitor. (VEN I, 8-12)

According to Volta, the expression «animal electricity» denotes «a kind of electricity essentially linked to life itself, and inherent in some function of animal economy». It was unsuited to those forms of electricity that could be produced «by rubbing the back of a cat, by currying a horse», or to the electricity «that has been observed arising spontaneously from the feathers of a living parrot». It should be used exclusively - he says - for the electricity recently discovered in the torpedo and in the «trembling Eel of Surinam ». On the basis of the inadequacy of the mechanical explanation of the fish's shock and of its similarity to the shock produced by the Leyden jar, it had been proposed by «some Physicists» that «the two phenomena might well be the same thing, and be produced by the same cause, i.e. electricity». However - Volta continues - «It was reserved to Mr. Walsh of the Royal Society of London the demonstration of their perfect identity on the basis of unquestionable experiments.» According to Volta, before Walsh's studies, there were various difficulties over assigning an electric nature to the fish's shock. It was difficult, for instance, to envision how an animal «could move at his will the electric fluid, make it more dense in one part of his body and more rare in the other». Moreover:

> It is even more [astonishing] that this charge and discharge could occur in the water, which is itself a conductor, and that it could precisely affect the arm of

man plunged to touch it, or another fish which swims near it (which is hit in a way such as he can no longer escape to the voracity of the electric animal.[114]

In the continuation of his letter, Volta says that it is now possible to account for some of these difficulties by considering that water is a less good conductor than metals and animal bodies, and consequently, a part of the large quantity of electric fluid emitted by the fish at the moment of the shock would hit these better conductors. Although expressed succinctly, this type of explanation was based on the experimental and logical development of the type of problems that Walsh was trying to define with the experiments carried out in the last period at La Rochelle, i.e. those concerning «the action [of the Torpedo] in water», and that, as we have just seen, were afterwards developed by Cavendish.

Volta is referring to the theory of Walsh (and of Cavendish) on the impossibility for the discharge of a torpedo to produce a spark and to cross the smallest interruption in the circuit of discharge, notwithstanding the strong commotion it produces, based on the observation that «all this occurs in the discharge of a very big electric Battery [i.e. a large number of Leyden jars connected in parallel], but very weakly charged». Volta continues:

> As a consequence [Walsh] estimates that the Torpedo discharges a very large quantity of electric fluid, similar to this large battery, but with a small energy, with a weak *tension* (according to my way to explain it), in such a way that none of these discharges, which on the other hand are totally similar in every aspect, can cross the smallest interruption.[115]

Volta also reports Walsh's successful experiment in producing a spark from the discharge of an electric eel:

> Mr. Walsh finding out that the trembling eel gives a shock much more violent than the Torpedo, estimated that that animal might not only discharge a larger quantity of electric fluid, but also dart it with more energy: he compared the eel to the same battery, charged to a higher degree of force (or of *tension* according to me): soon he conceived the hope to obtain a spark from it; which succeeded to him very well.[116]

He then describes the details of this experiment and gives other important information concerning Walsh's observations on the eel (see later). After referring to the fact that Walsh had published his studies on electric fish only in part («he has given just a report of the capital experiments»), he mentions the «excellent Memoir» of Mr. Cavendish on the artificial torpedo, concluding:

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[114] «*Il l'est encore plus* [*étonnant*] *que cette charge et décharge puisse s'opérer dans l'eau, qui est un conducteur elle-même, et que le courant électrique attrappe justement le bras de l'homme plongé pour le toucher, ou un autre poisson qui nage auprès de lui (qui est frappé de maniere à ne pouvoir plus se soustraire à la gueule dévorante de l'animal électrique).* (VEN I, 10)

[115] «[Mr. Walsh] *juge conséquemment que la Torpille décharge une très-grande quantité de fluide électrique, de même qu'une telle batterie, mais avec peu d'énergie, avec une faible tension (selon ma maniere d'expliquer) ; de sorte que ni l'une ni l'autre de ces décharges, qui d'ailleurs se ressemblent en tout point, ne peut franchir la moindre interruption.*» (VEN I, 10)

[116] «*Mr. Walsh trouvant que l'Anguille tremblante donne une secousse beaucoup plus violente que la Torpille, jugea que celle-là pouvoit non seulement décharger une très-grande quantité de fluide électrique, mais aussi le lancer avec plus d'énergie: il compara l'Anguille à la même batterie électrique chargée à un degré de force (ou de* tension *selon moi) plus perceptible: aussitôt il conçut l'espérance d'en obtenir une étincelle; ce qui lui réussit très-bien.*» (VEN I, 10)

> Our explanation based on the very large quantity of electric fluid, but endowed with a small *tension* or energy, is therefore in perfect accordance with the explanation and the experiments of Mr. *Cavendish*, who agrees himself with Mr. Walsh, from whom I have this avowal, together with a quantity of details that he communicated to me during a recent conversation with him.[117]

In the letter to Mme de Nanteuil Volta thus established a relation between his notions of the quantity of charge and «tension» (as the fundamental parameters involved in electric phenomena), and the conceptual developments emerging from the researches of Walsh and Cavendish on electric fish.

VOLTA'S EXPERIMENTAL STUDIES ON ANIMAL ELECTRICITY: FROM ENTHUSIASM TO INCREDULITY

Although Volta's letter was almost entirely devoted to the discussion of animal electricity, at the time he wrote it the Italian scientist was not yet involved personally in the experimental study of the problem. Only ten years later did Volta enter the field of experimental studies concerning animal electricity. These studies would be concerned with the possibility that, as well as in electric fish, a similar form of electricity might be present in more common animals. This possibility was central to the hypothesis Luigi Galvani put forward in 1791 after a long investigation into the involvement of electricity in nerve function and muscle contraction. According to Galvani, electricity was present in a condition of disequilibrium between the interior and the exterior of muscle fibre; the nerve fibre penetrating into the muscle fibre, like the conductor of a Leyden jar, would allow the discharge of this electricity when the nerve was connected to the external surface of the muscle through a metallic arc. In other words, in Galvani's conception, the muscle fibre with the thin nerve fibre penetrating its interior was like a minute «animated Leyden jar».[118]

In his first experiments on the involvement of electricity in muscle motion, in the spring of 1792, Volta confirmed Galvani's main experimental results, and in his first Memoir on this subject he praised his discovery of animal electricity:

> one of those great and luminous discoveries that deserves to make an epoch in the field of physical and medical sciences.[119]

Moreover, he established a relation between Galvani's achievements and Franklin's discovery of the identity of «the electric fluid and lightning». In this context, Volta alluded to electric fish by saying that the animal electricity of frogs and other ordinary laboratory animals was an expression «incomparably more tenuous and weak» than the same electricity that was «valid and shocking of the Torpedo and trembling Eel».

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[117] «*Notre explication fondée sur la très-grande quantité de fluide électrique, mais douée d'une très petite* tension *ou énergie s'accorde donc parfaitement avec l'explication et les expériences de Mr.* Cavendish*, qui est lui-même d'accord avec Mr. Walsh, de qui je tiens cet aveu, et une quantité de détails qu'il m'a communiqués dans une conversation que j'eus dernierement* [sic] *avec lui*». (VEN I, 11)

[118] GALVANI 1791; see BRESADOLA 1998; PICCOLINO 1998; PICCOLINO & BRESADOLA 2003.

[119] «*una di quelle grandi e luminose scoperte, che meritano di far epoca negli annali delle scienze fisiche e mediche*» (VEN I, 15)

In this Memoir Volta reiterated the themes of his letter to Mme de Nanteuil, making it clear that, although the phrase ‘animal electricity’ could not be used for the electricity that arises from animal bodies as a consequence of friction or of other mechanical actions, it was quite appropriate for the new electricity of Galvani, as well as for that already discovered in the torpedo and in the ‘trembling eel’ of Surinam. To the two fish alluded to in the letter of 1782, Volta adds the fish called *Trembleur* by Adanson and described by other scientists after him, as a member «of the genus of *Siluridi*, living in the rivers of Africa». He mention, moreover, another fish described in 1786 in the *Philosophical Transactions* which, he says, «belongs to the genus Tetrodon, and is found in the seas of India and of America». As to this last fish, described in 1786 by William Paterson, an English Officer, at ‘Santa Johanna’ in the Comore islands, and called for a while *Tetrodon electrics*, its electricity was not confirmed afterwards.[120]

In the course of his experimental investigations in this field, however, Volta changed his mind and eventually suggested that the electricity responsible for muscle contraction in Galvani’s experiments was not, as Galvani supposed, intrinsic to the animal, but was generated by the metals used to establish a connection between nerve and muscle tissue. One of the reasons that led Volta to envision such a possibility was that an arc made up of two different metals was more effective in producing the contraction than a mono-metallic arc. Another reason was that with bimetallic arcs the contractions could be obtained even by connecting two points of a nerve, without necessarily connecting a nerve and a muscle together. This last finding was inconsistent with Galvani’s hypothesis of the «animated Leyden jar», because in this model the nerve acted as the conductor of the jar, and a physical Leyden jar could in no way be discharged by connecting two points of its conductor.

The experimental and logical progress that led Volta, from his first insight into the special electric action arising by using dissimilar metals, to the experimental demonstration of the electromotive property of bimetallic contact, and eventually to the invention of the battery (along a path of discovery referred to usually as ‘Volta’s path to the battery’[121]), represents one of the most successful and extraordinary advances in the science of the 18th century, a real landmark opening the way to the subsequent progress of electrology and, more in general, to physics. It has been the subject of numerous thorough-going historical studies, so we will deal with it only in the context of the importance of electric fish research for Volta’s achievement. This particular aspect has been largely neglected by historians, despite its importance, and the fact that in his 1800 letter to the Royal Society in which he communicated his invention, Volta called his electric battery as ‘*organe électrique artficiel*’.

The main reason for this neglect is that Volta’s principal biographer, the Italian physicist Giovanni Polvani, interpreted Volta’s work almost exclusively from a physicist’s viewpoint, disregarding Volta’s interest in life sciences, and the importance of his thinking on the organisation of living beings. According to Polvani, the case of Volta, and in particular his debate with Galvani on animal electricity, is an instance of an irreducible conflict between a physical and a physiological disposition (Volta and Galvani’s respectively). Accordingly, the apparent success obtained by Volta over Galvani with the invention of his battery sanctioned the superiority of physics over physiology,

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[120] PATERSON 1796; see MOLLER 1996.
[121] HEILBRON 1977 A; PANCALDI 1990.

by proving *de facto* that metals were capable of generating an electromotive force as Volta asserted, and as a result, by showing that the electricity involved in Galvani's experiments on muscle contraction with metallic arcs derived from metals and was not intrinsic to the animal.[122]

This way the case of Volta and Galvani should be interpreted has been recently revived by certain trends in modern science philosophy that aim to exalt the importance of external influences, and of *a priori* conceptions, on the activity of scientists (in this particular circumstance the physical *Gestalt* of Volta, against the medico-physiological *Gestalt* of Galvani).[123] This attitude has been damaging for our understanding of this revolutionary phase of the 18th century science that led to fundamental advances in both electro-physics and electro-physiology. Not only has it contributed to our disregarding the importance of Galvani's conception of animal electricity, i.e. of the existence of an intrinsic electricity accumulated in a state of disequilibrium inside animal tissues, a conception that is central to modern electrophysiology. It has also contributed to our overlooking the importance of Alessandro Volta's interest in life sciences in his electrical researches, and in particular in the studies that led him to the discovery of the battery.

Like most 'natural philosophers' of his age, Volta showed a sincere interest in physiology and medicine, and indeed for him, as for other great scientists of the 18th century, there was no clear-cut boundary between the various sectors of science that emerged afterwards in the 19th and 20th centuries. In the course of the study that led him to the invention of battery, and also afterwards, Volta made a series of important discoveries in physiology. He anticipated, by about half a century, the fundamental idea of the functional organisation of the nervous system, Johannes Müller's doctrine of 'specific nervous energies'.[124] As is known, this doctrine stipulates that the physiological effects of nerve stimulation depends on the type of nerve stimulated and not on the type of stimulus used to achieve the stimulation. If different stimuli are used for stimulating, say, the eye or the optic nerve, such as mechanical or a chemical irritations, or light or electricity, the result will always be a luminous sensation. The same holds true for most other types of sensation, such as taste, hearing and somatic sensations. Moreover, Volta showed that the physiological effects of the electric stimulation of a nerve depends on its polarity. He demonstrated also that some nervous mechanisms have a tendency to generate transient responses to steady electrical stimulation, at the onset and, sometimes, at the offset of the stimulus application, (as for instance the visual system and that of motor nerves). Other mechanisms, on the other hand, generate tonic type responses (the gustatory system and the mechanism involved in a particular type of pain sensation).

Volta imagined that electricity might be used in medicine for both diagnostic and therapeutic purposes. He believed that the electric stimulation of the nervous part of the eye and of the optic nerve could allow us to differentiate between a blindness due to cataract and a blindness due to the «insensibility or paralysis of optic nerves» (VEN I, 222). Moreover, in 1802 Volta tried to improve the hearing of a 15 year-old girl, deaf from birth, by repeated applications of electric stimuli to the ears with his electric pile (and apparently with some success). (VEN II, 181).

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[122] POLVANI 1942.
[123] PERA 1986 (English translation 1992)
[124] MÜLLER 1840. See PICCOLINO 2000.

As already observed for Walsh, in the case of Volta too, the interplay between physics and physiology was bi-directional. Not only did physics help us to understand the mechanisms operating in living beings, but physiology became a fundamental tool in physics investigations.

By applying a bimetallic arc to the tip of the tongue, Volta was able to detect the weak electricity generated by the contact of two different metals in 1792, i.e. four years before he could measure this electricity with a physical instrument. Furthermore, by comparing the taste sensation produced by the bimetallic arc with that generated by a weakly charged Leyden jar, he could identify the polarity of the electricity induced by the metals. In addition, and importantly, it was by having recourse to sensory stimulation (taste once again) that Volta could show that the electricity excited by a bimetallic contact flowed in continuous way, unlike what happened in the discharge of electric machines and Leyden jars: a finding that opened up the way to the development of 'electro-dynamics', the characteristic electric science of the 19th century.

As already discussed, most of these aspects of Volta's work have been overlooked by a historical tradition that has preferred to see Volta as a physicist only slightly interested in life sciences, in contrast to a typical exponent of physiology and medicine, the Bologna doctor Luigi Galvani.

In our opinion Volta's interest in electric fish was genuine, and it was from his thinking on the organisation of the electric organs of the torpedo that Volta derived important inspiration for «the great step» that at the end of 1799 led him to the invention of the electric battery (Fig. 17) . Moreover, after the invention of the battery, interest in electric fish became an essential aspect of Volta's scientific work. This was mainly because the electricity of the Voltaic battery seemed to differ from ordinary artificial electricity, rather as the electricity of the torpedo differed from artificial electricity. Like the torpedo, the Voltaic battery could give strong shocks and produce other intense effects, although it was usually unable to produce sparks, sounds, or other typical marks of genuine artificial electricity. By reflecting on electric fish, and in particular by developing Cavendish's work on the 'artificial torpedo', Volta was able to account for most of the difficulties that had prevented many of the electricians of the epoch from accepting the perfectly electric nature of the fluid generated by his battery. This aspect of his scientific endeavour also implied a strong interplay between physics and physiology and led him to significant insights into important biological processes. As has occurred with so much of Volta's interest in physiology, it has also been largely overlooked by science historians.

In order to evaluate the importance of his interest in electric fish research for Volta's scientific work, we shall be examining the references to electric fish in his writings, both in the published works and in the manuscripts preserved in the 'Istituto Lombardo' in Milan (most of which are available in the monumental *Edizione Nazionale* of his writings).

RECURRENCES OF 'ELECTRIC FISH' IN VOLTA'S WRITINGS AND THE NICHOLSON MODEL OF AN ELECTRIC ORGAN

Besides the references to electric fish research which appears in the manuscripts of 1777-1778, Volta's interest in the torpedo and the electric eel comes out clearly in the 1782 letter to Mme de Nanteuil that we have already discussed above at some length. In this letter Volta agrees with Walsh and Cavendish’s interpretation of the reason why the shock of electric fish is not usually accompanied by sparks and sounds, and moreover he establishes a relation between his conception of the «tension» and quantity of electric fluid, and Walsh and Cavendish’s explanation of the two determining factors in the electric effects of the fish's shock.

After the letter to Mme de Nanteuil, a reference to electric fish reappears in Volta’s writings in 1792, in the *Memoria prima sull’Elettricita animale*, at the time he was praising Galvani’s hypothesis of the existence of an intrinsic electricity within the excitable tissues of common animals.(VEN I, 17-18) Afterwards, up until 1798, during the period of intense work aimed at demonstrating the metallic origin of the electricity responsible for muscle contraction in the experiments of Galvani, there is no mention of the torpedo or the electric eel in his writings.

During this period Volta demonstrated that bimetallic arcs are capable of producing movements and sensations similar to those induced by artificial electricity (of the friction type), and in 1796 he succeeded in measuring with physical instruments the electricity generated by the contact between two metals. Initially he used Nicholson’s ‘Doubler of electricity’, and afterwards his own ‘condensatore’, two kinds of device capable of revealing small voltages through the action of a variable capacity (eventually Volta also used a third more straightforward method for revealing ‘metallic’ electricity that was somewhat similar to the first two).[125]

However, the electric effects of a bimetallic couple were very weak and insufficient for any practical purpose. Volta tried to combine different metals, or to assemble many metallic couples in various ways, with the idea of combining the tiny electricity produced by each contact, but without success. He saw that when he assembled several metallic couples (of zinc and copper for instance), the effect was no greater than one produced by a single one. He soon realised why that happened: in the case of a chain of zinc-copper couples, a zinc element in one of the central parts of the chain would suffer the same electric action from the copper element that preceded it and from that which followed it, and the two actions thus cancelled each other out, for reasons of symmetry.

An assembly of «three or more different metals» connected one after the other also proved totally ineffective, because as Volta realised, in this case:

> the electric tension at the two extremities is the same, nothing more or less, one would have from an immediate contact between the first and the last one.[126]

During his vain attempts to multiply his metallic electricity, Volta’s attention was caught by electric fish through two publications, both appearing in 1797. One was a paper of William Nicholson, the inventor of the ‘Doubler’ of electricity bearing his name (an instrument that, Volta used in revealing the weak electricity of bimetallic couples). In this paper Nicholson had revived, in a somewhat modified form, the hypothesis of Cavendish (and Walsh) that electric organs could work as an assembly of minute planar capacitors. Instead of classical square capacitors (of the Franklin type), Nicholson was referring to

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[125] See HEILBRON 1977 A and PANCALDI 1990.

[126] «*la forza elettrica, che risulta, è sempre la medesima, né minore cioè, né maggiore di quella che si dispiega, ove vengano a contatto immediato il primo e l’ultimo*». (VEN II, 61)

an assembly of minute devices similar to ‘electrophores’, and assumed that an artificial model of the electric organ could be made up by using thin plates of mica. The electrophore was an instrument invented by Volta, consisting of a movable metallic plate and of another made of an insulating material (usually resin, and called ‘cake’). Manifest electric effects were produced by the electrophore as a consequence of a manipulation of the metallic plate that resulted in a change of its capacitance. Very likely Volta read Nicholson’s paper in 1799, a few months before the invention of the battery.

The other publication dealing with electric fish that came to Volta’s attention in the period that preceded the invention of the battery was a Memoir by Galvani concerning the experiments carried out in 1795 on torpedoes of the Adriatic coast of Italy. In this Memoir (that Volta probably read in 1798) Galvani reported a series of important observations, and, among other things, his discovery that torpedoes cannot generate a shock when the nerves of the electric organs are sectioned.[127]

Though dealing with electric fish from very different points of view, the works of Nicholson and of Galvani both served to re-orient Volta’s attention toward these singular animals and their particular organs in a period in which, Volta was trying to multiply his metallic electricity by assembling together a multitude of metallic couples. The electric organs were themselves made of an assembly of humid discs, one above the other, and they produced a strong electricity, a kind of ‘lightning’ of the animal world, resembling the lightning of the atmosphere, as Volta had put it in 1792. By their special arrangement they endowed these animals with the power of using electricity as a weapon for the purpose of defence and aggression.

Volta had already used humid bodies in his experiments, either solutions of a various nature or pieces of animal tissues, these last ones both as detectors of metallic electricity and as simple conductors. Moreover, after Galvani’s demonstration in 1794 that contractions could be produced in a frog preparation by simply connecting the nerve to the muscle, in the absence of any metal, Volta had tried to show that a contact between two conductors whatsoever, and in particular between two humid bodies, could generate an electromotive action.

At a time when his laboratory desk was occupied by the presence of metals of various types and by various humid bodies and solutions, and his creative mind was haunted by the project of generating a sizeable electricity from bimetallic contacts, the papers of Nicholson and Galvani on electric fish acted on Volta in a catalytic manner. Through paths it is difficult to determine in an unequivocal way, they probably led to Volta’s crucial decision to interpose humid disks between the discs of two different metals, stacked one above the other, the veritable breakthrough in the invention of the battery. This happened «toward the end of the year 1799».

In the letter to Banks of 20th March 1800 in which he communicated the invention of this extraordinary device, Volta called it *organe électrique artificiel* in order to acknowledge - he wrote - that it was «similar at bottom», as he constructed it, «in its form to the natural electric organ of the Torpedo or electric eel».[128] As it was made by an alternation of disks of two different metals (copper and tin, or silver and zinc) with interposed humid disks, the (Voltaic) battery bore an obvious visual resemblance to the natural electrical

[127] GALVANI 1797.
[128] VOLTA 1800, p. 405.

organ of the fish, which is also made up of stacked disks. In order to make the resemblance with the electrical eel more evident, Volta suggested that the stacked disks of the artificial organ «might be joined together by pliable metallic wires or screw-springs, and then covered with a skin terminated by a head and tail properly formed, etc.».[129]

From the constructional point of view there was, in his opinion, another more fundamental similarity. Unlike any other physical electric device, and similar only to the natural electric organ, his new apparatus was capable of producing and maintaining a flow of electric fluid without the need of insulating materials at its interior. In Volta's letter to Banks, this appears in a clear-cut way, from the very outset, in the title of the communication, 'On the electricity excited by the mere contact of conductive substances of different species'.[130]

The phrase '*organe électrique artificiel*' was obviously recalling Cavendish's 'artificial Torpedo'. As we shall see, after the invention of the electric pile, a significant part of Volta's scientific work seems to be a continuation of Cavendish's project on the artificial torpedo. This continuation, however, implied a fundamental conceptual break with the main hypothesis that had emerged in the electric fish research of Cavendish (and of Walsh and Nicholson, before and after him, respectively). Indeed Volta build up an 'artificial torpedo' of his own, that he powered with his electric pile. In his opinion, this artificial torpedo, was, from the mechanistic point of view, much more similar to the real torpedo than Cavendish's device. Some years after the invention of the pile, on speaking about the apparatus invented by the «celebrated English physicist [...] Lord Kavendish [*sic*]», he wrote that «this small machine (*macchinetta*), that he [Cavendish] liked to denote as *artificial Torpedo*», even though it was capable of imitating some of the effects produced by the natural torpedo «on the other hand, it did not imitate the natural one intrinsically, nor for any proper virtue or action with which the last one is endowed».

In his last scientific Memoir, written between 1801 and 1805 and published in 1814 by Pietro Configliachi, one of his pupils, in an impersonal way, with reference to «the experiments and observations of Kavendish [...] very beautiful and instructing», he wrote, that, on the other hand:

> they lack that much, that to their completion was added by our Italian [i.e. Volta himself], with the discovery of the Electromotor [i.e. the pile], and with the application of this extraordinary instrument to the true, or at least most probable, explanation of Torpedo's phenomena, and, moreover, to the most perfect and complete of the same effects.[131]

But why did Volta refer so clearly to this difference between his artificial torpedo and that of Cavendish? In order to understand this important aspect of Volta's attitude, we need to come back to Nicholson and to his 1797 paper dealing with the possibility of imitating the electric organs of fish by

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[129] *Ibid.* p. 419.
[130] The title was not Volta's but it reports what the Italian scientist wrote in the initial sentence of his letter to Banks: «*...l'électricité excitée par le simple contact mutuel des métaux de différente espèce*».
[131] «*ma esse mancavano ancora di quel molto, che a compimento vi aggiunse il nostro Italiano colla scoperta dell'Elettromotore, e coll'applicazione di questo maraviglioso stromento alla spiegazione vera, o almeno probabilissima de' fenomeni della Torpedine, non che all'imitazione più perfetta, e compitade de' medesimi*» (VEN II, 268).

assembling minute «electrophores» one above the other. Nicholson's paper revived, in quite a new way, Walsh and Cavendish's conception that the electric organ could accumulate a larger quantity of electric fluid by the assembly of a multitude of planar capacitors corresponding to the disks or membranes that constituted its basic structure.

In normal conditions, according to Nicholson, in the electric organ there would be an equilibrium between the electricity accumulated in two conductive liquid laminae separated by the insulating one. However, this equilibrium would be disturbed, as in the physical instrument, in consequence of some small movement leading to an increased separation between the laminae. In this way the electric force responsible for the shock would be generated. This hypothesis implied a parallel connection between all the upper conductive plates of the minute electrophores to each other (and a similar connection between the lower plates as well), and, according to Nicholson, this might be assured, by «the white reticular matter between the columns [which] may consist of conductors separately leading to the two opposite surfaces». These conductors «in all their subdivision, may be well kept asunder by a covering of non electric matter». A similar parallel connection among the constitutive elements of the electric was also implied in Cavendish's hypothesis, but Cavendish did not explicitly consider this point.

Although Nicholson's paper re-oriented Volta's interest toward electric fish, and probably had a decisive influence on him in the critical moments that preceded the invention of the battery, it acted on him, nevertheless, 'by contrast', i.e. by proposing an operational mechanism for the electric organ that Volta was not disposed to endorse.

As was the case with Cavendish, Nicholson's model electric organ was based on the interposition of a lamina of insulating substance between two conductive plates. As a matter of fact, an insulating material was a constitutive element of all instruments that were capable of maintaining an electric equilibrium (Leyden jars, Franklin's planar capacitors) and a proper insulation was also necessary in instruments capable of generating it (such as friction electric machines). For Volta, however, the idea that an insulating matter could be a constituent of the electric organ was in evident contrast to the knowledge of «animal physics». This he will state in a clear-cut way in his 1800 letter to Banks on the invention of the battery, where, with reference to Nicholson's hypothesis, he writes:

> such a hypothesis falls entirely, these pellicles of the organ of the torpedo are not, and cannot be, in any manner insulating or susceptible of a real electric charge, and much less capable of retaining it. Every animal substance, as long as it is fresh, surrounded with juices, and more or less succulent of itself, is a very good conductor. I say more, instead of being as *cohibent* [i.e. *insulator*] as resins or talc, to leaves of which Mr. Nicholson has compared the pellicles in question, there is not, as I have assured myself, any living or fresh animal substance which is not a better *deferent* [i.e. *conductor*] than water, except only grease and some oily humours. But neither these humours nor grease especially semi-fluid or entirely fluid, as it is found in living animals, can receive an electric charge in the manner of insulating plates, and retain it: besides, we do not find that the pellicles and humours of the organ of the torpedo are greasy or oily. This organ therefore, composed entirely of conducting substances, cannot be compared either to the electrophore or condenser or to the Leyden flask, or any machine excitable by friction or by any other means capable of electrifying insulating

> bodies, which before my discoveries were always believed to be the only ones originally electric.[132]

According to Volta, in order to account for the mechanism of the torpedo, it was necessary to refer to an entirely new instrument, capable of generating and maintaining an electric disequilibrium without containing any insulating matter in its interior. Such an instrument was the new device that he himself had built, a device capable of generating electricity «by the mere contact of conducting substances of different species», i. e. the device that he himself had invented, the 'artificial electric organ' (or «electromotor», or «electric pile» or «electric battery», as it was later called).

The final passage of this letter asserts Volta's conception in a particularly expressive way:

> To what electricity then, or to what instrument ought the organ of the torpedo or electric eel, &c. to be compared? To that which I have constructed according to the new principle of electricity discovered by me some years ago, and which my successive experiments, particularly those with which I am at present engaged, have so well confirmed, *viz*. that conductors are also, in certain cases, exciters of electricity in the case of the mutual contacts of those of different kinds, &c. in that apparatus which I have named the *artificial electric organ*, and which being at bottom the same as the natural organ of the torpedo, resembles it also in its form, as I have advanced.[133]

Much more than in the similarity of the shape, it was thus in the similarity of the operational mechanism that Volta's electric battery (or artificial electric organ) resembled the natural electric organ of the torpedo and electric eel. Because of the absence of any insulating material, the natural electric organ was entirely different from the model of the minute electrophores proposed by Nicholson.

It was also different from the artificial torpedo (the *macchinetta*) of Lord Kavendish, for as this was based on an assembly of capacitors, it did not imitate the natural organ «intrinsically, nor for any proper virtue or action» typical of the natural organ. On the contrary:

> Kavendish taught us nothing for what concerns the origin, or the cause of this prodigious electricity of the Torpedo, and he did not either teach us what role or

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[132] «*une telle hypothése tombe entièrement, vu que ces pellicules de l'organe de la torpille ne sont, et ne peu- vent être, aucunement isolantes, ou susceptibles d'une véritable charge électrique, et moins encore capables de la retenir. Toute substance animale, tant qu'elle est fraiche, entourée d'humeurs, et plus ou moins succulente elle-même, est un assez bon conducteur: je dis plus; bien loin d'être aussi* cohibente *que les résines, ou le talc, aux feuilles du quel Mr. NICHOLSON cherche à comparer les pellicules dont, il est question, il n'y a point, comme je me suis assuré, de substance animale vivante, ou fraiche, qui ne soit meilleur* déférente *que l'eau, excepté seulement la graisse, et quelques humeurs huileuses. Mais, ni ces humeurs, ni la graisse, surtout à demi fluide, ou fluide entièrement, comme elle se trouve dans les animaux vivants, peut recevoir une charge électrique, à la manière des lames isolantes, et la retenir; d'ailleurs, on ne trouve pas, que les pellicules et les humeurs de l'organe de la torpille soient graisseuses ou huileuses. Ainsi donc, cet organe, formé uniquement de substances conductrices, ne peut être rapporté, ni à l'électrophore ou condensateur, ni à la bouteille de Leyde, ni à une machine quelconque excitable, soit par frottement, soit par quelque autre moyen capable d'électriser des corps isolants, qu'on a toujours crus, avant mes découvertes, les seuls originairemeut électriques*». (VOLTA 1800, p. 430).

[133] «*A quelle électricité donc, à quel instrument, doit-il être comparé, cet organe de la torpille, de l'anguille tremblante, etc. ? à celui que je viens de construire, d'après le nouveau principe d'électricité que j'ai découvert il y a quelques années, et que mes expériences successives, surtout celles qui m'occupent maintenant, ont si bien confirmé, savoir, que les conducteurs sont aussi, dans certains cas, moteurs d'électricité, dans le cas du contact mutuel de ceux, de différente espèce, etc. à cet appareil, que j'ai nommé* Organe électrique artificiel, *et qui, étant dans le fond le même que l'organe naturel de la torpille, le ressemble encore pour la forme, comme j'ai déjà avancé*». (VOLTA 1800, p. 430).

function exert therein its organs denoted as electric; Nothing the other Physicists with so many, and various hypothesis, which on the contrary served more to confuse than to clarify the matter.[134]

Among the «other Physicists», he very likely meant Nicholson. Only the apparatus built by Volta, the electric battery or Voltaic pile (from the phrase *pile Voltaïque* the French used), could offer a valuable imitation, «the most perfect and complete» of the phenomena of the torpedo, because it was based on the same structure and functional principles.

Volta believed the apparent differences that existed between the natural electric organ (made exclusively of humid disks) and the artificial one (made of both metal and humid disks, as he had built it at first, in the most common «column» version of the battery), were not insurmountable. For instance, in order to make the similarity more perfect, it was possible to conceive an artificial electric organ made up exclusively of humid substances (conductors of the 2nd class in his classification) without any metal interposed (conductors of the 1st class). In 1801, even though he had not yet been able to produce any relevant mark of electricity by the exclusive combination of humid substances, he wrote:

> But if not art, nature has found the way to succeed with this in the electric organs of Torpedo and trembling eel (Gymnotus electricus), etc., which are built up exclusively of conductors of this second class, that is, humid ones, without anyone of the first class, without any metal. And perhaps we are not far from the possibility that also art could imitate them.[135]

Thus nature (in this particular case the animal kingdom) might open to the physicist unsuspected possibilities that he should try to imitate with his «art». In fact, Volta was eventually able, in 1804, to build a battery by using exclusively humid bodies, even though the electricity produced by this device was much weaker than that produced by his more classical apparatus made of two metals and one humid body. To explain the strong effects produced by the electric fish, he supposed that the membranes or pellicles of the natural electric organ might be made of humid matter of a special kind, possessing a stronger electrical efficacy than more common liquid substances.

With reference to the natural organ, he wrote:

> We are induced further to believe that it is at bottom, i. e. as to the essential construction the same, namely, that his virtue and activity comes from the general principle established by Volta, which is that, different conductors, put in mutual contact are also motors of electricity: keeping this in mind its is easy to suppose that the small laminae, or pellicles, stacked one above the other in great number in the many small columns, or tubes, of which these organs are made, differ one from the other in such a guise that two and three different substances, follow one after the other, in an alternate fashion, together with some humour, by which they would be interpolated two by two, in sum they would be in a convenient order, as indeed are the double metallic laminae, and interpolated by a third conductor in the [Voltaic] batteries: to which would be appropriate the

[134] «*Nulla dunque c'insegnò KAVENDISH riguardo l'origine, ossia causa di questa prodigiosa elettricità della Torpedine, e neppure qual giuoco e funzione vi esercitino gli organi di lei detti elettrici; nulla gli altri Fisici colle tante, e sì varie loro ipotesi, le quali anzi servirono più a confondere, che a rischiarare la cosa*» (VEN II, 269)

[135] «*Eppure, se non l'arte, la natura ha trovato il modo di riuscirvi negli organi elettrici della Torpedine, dell'Anguilla tremante (Gymnotus electricus), ec. costrutti di soli conduttori di questa seconda classe ossia umidi, senza alcuno della prima, senza alcun metallo; e forse non siam lontani che anche l'arte vi possa arrivare*». (VEN II, 62)

name of artificial electric organs proposed by Volta[136].

The structural and operational similarity between the natural and the artificial electricity implied also a ‘behavioural’ correspondence between the two devices. Unlike «the Leyden jars, or batteries [of them], which being inactive by themselves, need to be charged at every trial, and need to draw always the charge from the action more or less prolonged of an external agency» the artificial organ resembled the natural one because it «acted by a force of its own». Moreover, it was:

> capable finally of giving at any instant shocks more or less strong depending on the circumstances, shocks, that double at every touch, and that, when repeated with some frequency, by replicating with very short interruptions such touches, produce the same numbness of the arm and that species of fourmillement that produces, when tried in a likely way, the Torpedo.[137]

GALVANI’S MEMOIR ON THE TORPEDO: THE FLUID OF THE NERVES, THE ELECTRIC ORGANS AND THE DEAD FROGS JUMPING

The other scientific paper that drew Volta’s attention towards electric fish was Luigi Galvani’s Memoir dealing with an important series of experiments Galvani had carried out on torpedoes from the Adriatic coast of Italy. This Memoir was published in 1797, but was probably known to Volta in 1798. In a letter of that year to Jan Peter Franck, formerly Professor of Medicine at Pavia and thus a colleague of his, he praised the «beautiful experiments» of Dr Galvani, especially those on the ablation of the brain, as of a result of which the torpedoes:

> lose at once the virtue of giving the shock, in spite of the fact they remain alive and apparently in good conditions, i.e. they remain quite vivacious for long time, swim as usually, and make all the other movements. On the contrary, if they are deprived of the heart, by which made much more languid they lose the motion with the other functions, they maintain, nevertheless that of giving shocks more or less vigorous up to the complete cessation of any movement and with it of life.[138]

The importance of the nerves in the generation of the torpedo shock. emerging from Galvani’s study, seemed to confirm an earlier suggestion, put

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[136] «*Ciò, che c’induce vieppiù a credere, che sia anche in fondo, cioè quanto all’esenziale costruttura il medesimo, cioè, che la sua virtù ed attività venga dal principio generale stabilito da VOLTA, il quale è, che conduttori diversi posti a mutuo contatto sono anche motori di elettricità: il che ritenuto, è facile supporre, che le laminette, o pellicole sovrapposte le une alle altre in gran numero nelle molte colonnette, o tubi, di cui son composti tali organi elettrici, differiscano tra loro in guisa che due, e tre sostanze diverse si succedano alternativamente compresovi anche qualche umore, da cui sieno interpolate due a due, in somma trovinsi disposte nell’ordine conveniente, come appunto son disposte le doppie laminette metalliohe, e interpolate da un terzo conduttore diverso nelle pile: alle quali pile si comprende ora com’e bene converebbe il nome di* organi elettrici artificiali *proposto da VOLTA*». (VEN II, 268).

[137] «*capable enfin de donner à tout moment des commotions plus ou moins fortes, selon les circonstances, des commotions qui redoublent à chaque attouchement, et qui, repétées ainsi avec fréquence, ou continuées pour un certain temps, produisent ce même engourdissement des membres que fait eprouver la torpille, etc*». (VOLTA 1800, p. 405),

[138] «*che queste private del cervello, dell’organo cioè ove risiede la volontà, perdono a un tempo la virtù di dare scossa quantunque continuino a vivere apparentemente bene ossia mantengansi assai vivaci per molto tempo, nuotino al solito, e facciano tutti gli altri movimenti ecc. Che al contrario private del cuore, per cui rese molto più languenti vengono anche e molto più presto a perdere colle altre funzioni il moto, conservano pur quella di dare ancora delle scosse più o meno gagliarde fino alla cessazione totale di esso moto coll’estinzione della vita*». (VEP III, 416-417)

forward by Hunter on the basis of his anatomical investigations, on the relation between an electric fluid circulating along nerves and the electricity involved in the production of the shock from the electric organs. This relation fitted within the framework of the neuro-electric theory of the nervous signal. Hunter had also suggested that the special role of the nerves in the «formation, collection, or management of the electric fluid» for the electric organs might possibly «be connected with the power of the nerves in general».

Within his conception of 'animal electricity', Galvani was a strong advocate of the electrical nature of the nervous fluid 'in general'. His aim in studying the effects of nerve section on electric organs' discharge was to see whether electricity was involved in the nervous function. He particularly wished to determine whether the brain «would be the elaborator and the collector of such electricity, and nerves the conductors»[139], a passage that, apart from the distinction between the different role of the brain and nerves in the treatment of electric fluid, recalls the similar expression Hunter had used («formation, collection, or management of the electric fluid»). Galvani noticed that some collaterals of the nerves of the electric organs innervate the dorsal muscles of the torpedo, and from this derived support for the neuro-electric theory:

> The origin of the nerves that go to the mentioned [electric] organs it is the same of those that branch to the other parts, and also to muscles; it is also the same the fabric, the substance of the ones as well as of the others: it is therefore convenient to say, that it is also the same their use and their office, and by consequence the same also the fluid, that they receive from the brain, and conduct to the parts where they end. But as far as those that go to the electric organs, they have been shown to conduct an electric fluid; therefore the same should be as to those that go to other parts, and as to those that go to muscle.[140]

Galvani's Memoir on the torpedo contributed to direct Volta's attention toward electric fish in the critical period leading to the invention of the electric battery. However, Volta did not follow up the main issue of Galvani's study of the torpedo, i.e. the dependence of the shock on an electric fluid proper to the nerves, from which Galvani had derived his inference of the electric nature of all nerves. This was because, in contrast with Galvani, Volta was convinced that electricity did not play any special role in nerve function.

In his study of torpedoes, Galvani had made another interesting observation that Volta did exploit after the invention of the electric battery to account for the functioning of the natural electric organs of the torpedo. The Bologna doctor used his frog preparations to reveal the electricity of the torpedo. He placed several frogs on various parts of the torpedo's surface, and then described what happened:

> it was for me a joyful scene seeing all of them moving at once, and, I would say, almost jumping.[141]

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[139] GALVANI 1797, p. 65.

[140] «*E' dunque la stessa origine dei nervi, che vanno ai suddetti organi, che di quelli che si diramano alle altre parti, ed ai muscoli; è pure la stessa la fabbrica, la sostanza sì degli uni, che degli altri: dunque convien dire, che sia pur anche lo stesso l'uso, e l'offizio loro, e in conseguenza anche lo stesso il fluido, che ricevono dal cervello, e che conducono alle parti, nelle quali pongon termine. Ma relativamente a quelli, che vanno agl'organi elettrici, è dimostrato esser questi conduttori di un fluido elettrico; dunque lo stesso esser deve relativamente a quelli che si portano ad altri parti, ed a quelli che si diramano ai muscoli*». (GALVANI 1797, p. 70).

[141] «*mi riescì d'un giocondo spettacolo il vederle tutte ad un tempo muoversi, e direi quasi saltellare*» (GALVANI 1797, p. 75).

A similar thing also occurred if the frogs were placed at the same distance from the torpedo and communicated with it «through the layer of water sprinkled over the table in collocating the torpedo on it».

THE TORPEDO AND THE 'MINIMAL DEGREES OF ELECTRICITY'

During his experiments, Galvani was surprised to see that frog muscles contracted even when the torpedo did not produce any evident shock, and was not stimulated. He believed this was an indication of the existence, also in the fish that had not been stimulated, of a continuous circulation of electric fluid «albeit in a very small quantity» responsible for «minimal degrees of electricity», that could be revealed by the frog preparation («an electrometer of the most exquisite»), but which were unable to produce any sensation in the experimenter.

After the invention of the battery, Volta assumed that a similar operational mechanism underlay the functioning of both the 'natural' electric organ (i.e. the torpedo's) and the artificial one (the 'Voltaic' pile), both of which would generate electricity by the contact of conductive matter of different species. Within this framework, it was nevertheless difficult to see why, in contrast to the artificial organ (the pile), the natural organ was not continually active, and the torpedo needed to be stimulated in order to discharge its electricity. This occurred despite the fact that as with the artificial organ, in the torpedo too there was a constant communication between the different disks making up the electric organ in normal conditions: this ought to have produced a constant electric effect when the plus and minus pole were connected together.

To account for this difficulty, Volta suggested that in resting conditions the discs or membranes of the natural electric organs might not establish a perfect contact, and that there would be some kind of difficulty creating interference with the electromotive action of the disks and/or with the conduction of the electric fluid. The shock would be produced every time that:

> either as consequence of a voluntary effort of the animal, or by some other manner, would be brought to a congruous contact, or made completely communicating the one with the other, those parts of the organs, which in the natural state, being our fish free and quiet, happen to be disjoint or ill communicating.[142]

From this point of view, Volta attached importance to the mechanical movements sometimes observed in the torpedo at the moment of the shock, the movements on which Lorenzini and Réaumur had based their mechanical hypothesis for the torpedo's shock. In Volta's opinion, these movements were an expression of the animal's effort to bring the discs of the electric organ into close contact to produce a strong electric effect. If this were the case, it might be supposed that in resting conditions there would be a small flux of electricity, insufficient to produce a shock, but which might possibly be revealed by some sensitive device. In this context Volta recalled the «beautiful experiments of Galvani» showing

[142] «*o per uno sforzo volontario dell'animale, o per altra maniera addotte al congruo contatto, o rese compitamente comunicanti fra loro quelle parti di essi organi, che nello stato naturale del nostro pesce libero e quieto trovinsi per avventura disgiunte, o mal comunicanti.*» (VEN II, 198)

> the presence in the torpedo of «some continuous passage of electric fluid from the back to the belly, or vice-versa, a passage, quite scarce, however, such that it is enough to shock those trunks of frogs extremely excitable [i.e. Galvani's frog preparations], but it does not suffice to produce sensible shocks in our hands and arms». (VEN II, 202)

In the complex and sometimes harsh debate between Volta and Galvani on animal electricity, the case of the «minimal degrees« of torpedo electricity is one of the not unusual instances of one of the adversaries taking advantage of the results of his antagonist, to provide support for his own work.

PARADOX AND IRONY: IS VOLTA'S ELECTRIC BATTERY REALLY ELECTRIC?

After the invention of the battery, Volta's interest in electric fish became a dominant feature in his writings. There are two main, and somewhat related, reasons for this. One is that he wished to provide arguments in support of his initial contention about the profound similarity, both structural and operational, between the peculiar organs of electric fish and the new device, the «artificial electric organ», as he called it at the beginning.

The other reason was that a conspicuous number of the electricians of the age cast doubts on the nature of the fluid involved in the effects of his device. This was because the Voltaic pile, although powerful when producing some of the effects ascribed to genuine artificial electricity (i.e. the friction type), and in particular strong commotions, was only slightly effective in generating the effects that were considered the hallmarks of genuine electricity, such as sparks and sounds. These difficulties will have a familiar ring about them, as they were much the same as those that for a long while had prevented the acceptance of the electric nature of the shock of the torpedo and electric eel. As a matter of fact, against the perfect identity between the fluid produced by the Voltaic pile and genuine electricity, it was generally remarked that the electricity of the pile had some of the connotations of the animal type of electricity, i.e. the electricity involved in the phenomena of electric fish and also - as Galvani assumed - in muscle contraction. Paradoxically, and at the same time ironically, this led «Galvanic fluid» or «Galvanism» to be called the electricity of the 'Voltaic' pile.

To face this problem, Volta was thus obliged to reconsider the subject thoroughly, and in this context, he made frequent allusions to the electric fish. Moreover, and importantly, he did not limit himself to producing rhetorical arguments in support of his conviction of the perfect «identity of the electric fluid with the so-called Galvanic fluid», but made a series of experiments that are well worth considering.

FROM THE 'ARTIFICIAL ELECTRIC ORGAN' TO THE 'NATURAL ELECTROMOTOR': A MOBILE PERSPECTIVE BETWEEN PHYSICS AND PHYSIOLOGY IN VOLTA'S SCIENTIFIC ENDEAVOUR

Before dealing with this second aspect of Volta's interest in electric fish after the invention of the pile, let us first rapidly examine the first reason for his interest in these singular animals, i.e. the similarity he strongly urged between his pile and the peculiar organs of the electric fish. We have already discussed how his thinking on the 'natural' electric organs might have had an influence on the invention of his pile, and we will not come back to this point. We may add that this can be considered the culmination of the productive influence on his 'art' of his thinking on animated nature, and, more in general, of the effective interplay between physiology and physics that characterised his scientific work. If the natural electric organ had provided inspiration for building the battery, then a reflection on the battery might help him to interpret the functioning of the organ of the electric fish. Compared to the animal organ, the physical apparatus was easier to study, and it could be manipulated experimentally in an easier way. It might therefore help to provide clearer insight into the operational mechanisms that underlay the functioning of both.

The possibility that thinking about his 'art' might help to interpret biological phenomena is evident also in the linguistic transformations that characterised his writings after the invention of the battery. After an initial phase, Volta preferred to call his battery an «electromotor», rather than «artificial electric organ», in order to underline the mechanism at the basis of its functioning, i.e. the electromotive action of dissimilar conductors (rather than the source of inspiration leading to its invention). With this change, the natural organ of the torpedo became, in his terminology, the «natural electromotor». This was a 180° degree turn in his terminology, still, however, bringing out the fact that the functional principle of the electric fish organ was the same electromotive action as the one involved in the functioning of the battery.

The increasing confidence he had acquired in the electromotive action of dissimilar metals after the invention of the battery led him to remark that a physical principle, derived from the 'art' of the scientist, could account for the operation of living nature. We should notice, however, that he later often used the word «pile»,

In a letter dated 18th May 1801, after saying that his new device was capable of producing shocks that «resemble those produced by the Torpedo and other electric fish, that have been denoted as electric without doubt [...], shocks that to the imitation of these of the Torpedo, etc., even more constantly and unfailingly repeat themselves at any touch», he wrote:

> besides the above mentioned shocks, that as I have already observed announce themselves to be at least as electric as those of the torpedo, or trembling eel etc., this apparatus offer you electric signs so unequivocal, such as could not have been obtained from the Torpedo, and even the spark, and this not only by making recourse to a good Condensatore and by using it in the most convenient way (with the favour of this instrument also an invention of mine, so useful and instructive in the subtle researches of electricity, I am persuaded that I would obtain electroscopic signs from the Torpedo too), but also without such an assistance, provided only that the Apparatus [i.e. the Voltaic pile] is large enough, and well in order, and that the necessary cares are used in experimenting.[143]

[143] «*oltre alle accennate scosse, le quali come ho già fatto osservare si annunciano per elettriche almeno tanto quanto quelle della Torpedine, dell'Anguilla tremante ecc., vi offre esso apparato altri segni elettrici non equivoci, quali non si sono ancora potuti ottenere dalla Torpedine; de' segni sensibilissimi all'elettrometro, e fino la scintilla, e tutto ciò non solamente ricorrendo ad un buon Condensatore e adoperandolo nel modo il più conveniente (col favore del qual istromento parimenti di mia invenzione, cotanto utile ed istruttivo nelle sottili ricerche di elettricità, son persuaso, che otterrei simili segni elettroscopici pur anche dalla Torpedine); ma eziandio senza di un tal soccorso, sol che sia detto Apparato abbastanza grande, e ben in ordine e si osservino nello sperimentare le dovute attenzioni.*» (VEN II, 27)

This passage is interesting because it shows the general attitude he held about his comparison between his battery and the torpedo, an attitude we have said was based on a variety of viewpoints that altered with the progress of his researches and with their successful ‘interchange’: 1) the artificial organ is similar to the natural one, and, 2) it shows some characteristics that are the expression of the basic similarity of the functioning mechanism, and may give signs proper to the natural ones as well; therefore, 3) with adequate experiments one should be able to obtain these signs even in the natural electric organs, although they might be hard to get. As in the case of Walsh and Cavendish, the principle underlying this attitude was that the same basic mechanisms were involved in the functioning of the animate and inanimate world. In the progress of his researches, besides clarifying the similar properties of the natural and artificial organ, Volta will make use of these properties to underline the similarity of the mechanisms, and also to plan experiments on both the artificial and the natural device, to show properties not yet manifest more clearly.

Although 18th century culture shows an evident opposition between a ‘mechanistic’ and a ‘vitalistic’ disposition towards the interpretation of living phenomena, this opposition was completely extraneous to the attitude Volta held in the study of physiological processes.

DEMONSTRATING THE PERFECT IDENTITY BETWEEN ‘GALVANIC’ AND ‘ELECTRIC’ FLUID: THE TIME FACTOR

The main anomaly in the behaviour of the Voltaic pile that, in the opinion of many 18th century scientists, prevented the acceptance of the idea that the fluid moved by the new device was genuinely electric, was the difficulty over producing sparks and sounds (and other effects that were considered typical hallmarks of true electricity). A similar difficulty, as we have seen, had precluded also he acceptance of the electric nature of the shock of the torpedo and electric eel, a phenomenon that, after Walsh and Cavendish’s researches, no one any longer doubted were fully electrical.

In order to account for the peculiar characteristics of the fish’s shock, Volta fell back on his concepts of *tension* and quantity of charge, by developing an argument similar to that previously considered by Cavendish. The animal shock would be the effect of a great quantity of electricity present in the fish organ at a low tension. He followed a similar strategy also to account for the relative inaction of the pile in the production of sparks and sounds. However, in this case, he pushed the argument to a level of understanding so deep, in both logical elaboration and experimental demonstration, that he went far beyond Cavendish’s conclusions, and obtained results of great physical and physiological relevance. In this context, the main novelty of his work was the way he faced the ‘time factor’ in the effects of the discharge of electric devices.

Let us follow through this fascinating aspect of Volta’s work that, although poorly appreciated, was one of his main achievements in the post-battery period. It was dealt with in various scientific writings of both public and private character that he produced after the invention of his device, and it was particularly well developed in his last scientific Memoir, written between 1801 and 1805, but published anonymously in 1814.

One of the objections he wished to face in this Memoir was the observation that a given Voltaic pile usually produced a much stronger commotion than a Leyden jar charged at the same tension (or «electrometric degree») developed by the pile, an observation that appeared to be evidence of an essential difference between the fluid of the pile and true electricity.

To undermine this argument, Volta assumed that, since a pile produces a «continuous» or «indeficient» flow of electricity, it should be compared to a very large Leyden jar (or to a 'battery' of Leyden jars connected in parallel). This was because the discharge of a small jar lasted too short a time to fully affect the physiological mechanism responsible for the shock. In following the quotations of Volta's writings below, we need to keep in mind that he used the expression «battery» only to designate an assembly of Leyden jars connected in parallel, and never to designate his (Voltaic) battery or pile:

> It has been thought that one could explain how and why a battery or a Leyden jar of great capacitance, charged to a very low degree produce a strong shock, so strong as it could not be produced by a small jar charged to the same degree; it has been thought that one could explain that in a satisfying way by simply saying that this is so because the large capacitance jar or battery discharges in an instant a larger quantity of electric fluid that the small jar. But it has not been considered that this expression *in one instant* is not rigorously correct, and even it is completely wrong, if one means an indivisible moment. For a discharge whatsoever a finite time is always necessary: that is to say, that it should last a certain time, whatever this time might be, and although it is extremely short even for the big jars and batteries, and such that hardly we might measure it, such that it seems to be an instant.[144]

Volta goes on to say that, if two Leyden jars of different capacitance are charged at the same tension, necessarily «the speed with which the electric fluid is pushed, and must flow in discharging itself, is the same», and hence:

> It is therefore not true that the jars and batteries of a great capacitance would pour, in discharging themselves a *larger quantity* of electric fluid *in one instant*, that is in the same time as the ones small and of small capacitance, charged at the same degree of the electrometer. They will pour but the same quantity in the first instant (since the electric *tension*, and thus the speed of the current of that fluid, is the same for the large jars as for the small ones in our supposition); but the discharge of the large ones, that is of the of those of great capacitance,, last more instants than that of the small jars. No doubt: the discharge of the first ones takes more time; it extends itself, that's to say, to many subsequent discharges, even if it is completed in a very short time, such as it appears to us as an instant.[145]

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[144] «*Si è creduto di spiegare sufficientemente come e perché una batteria, od una boccia di Leyden di gran capacità, caricata a un grado debolissimo produce una forte scossa, quale non è prodotta da una picciola boccia caricata al medesimo grado, si è creduto di spiegar ciò in una maniera soddisfacente col dire semplicemente, che gli è perché la capace boccia o batteria scarica in un istante una più grande quantità di fluido elettrico che la boccetta, e precisamente una copia tanto più grande, quanto ella ha più di capacità. Ma non si è fatto attenzione che questo termine* in un istante *non è rigorosamente giusto, che egli è anzi assolutamente falso, se si intende un momento indivisibile. Egli vi abbisogna sempre di un tempo finito per una scarica qualunque: che è quanto dire, essa deve durare un certo tempo: quantunque questo tempo possa essere, e sia infatti per le scariche ancora delle più grandi boccie o batterie, brevissimo, e tale che difficilmente potremmo noi misurarlo, tale insomma, che ci sembra un istante.*» (VEN II, 74)

[145] «*Non è dunque vero, che le boccie e batterie di una grande capacita versino collo scaricarsi una* più grande quantità *di fluido elettrico* in un istante, *ossia nel medesimo tempo che le picciole e di ristretta capacità, cariche le une e le altre al medesimo grado dell'elettrometro. Elleno non ne versano che la medesima quantità pel primo istante (attesochè la*

To conceive that jars of different capacitance charged at the same degree take «the same time» to release their electric fluid - he wrote in another Memoir - is like saying that «a large quantity of electric fluid would flow from the large jar or large battery equally in the same instant, as a small one from the small jar, that is in an equal time, which is the same as saying with a greater speed». He decidedly rejects this conclusion:

> But this cannot absolutely be, because for equal electrometric degrees, for equal *tension* and push, also the speed of the electric fluid should be equal. Therefore only in a longer time can a larger quantity of fluid flow out, and thus discharge the large jar and the battery.[146]

Therefore the discharge of a Leyden jar of great capacitance extends itself to «many subsequent discharges». From the comparison of the effects of Leyden jars, of capacitances differing from each other by a factor of 100, he concluded that:

> it is possible, by comparing the discharge of the large jar or battery with the discharge of the small phial 100 times less capacious, it is possible, I say, to consider the first as the repetition of 100 discharges equal to that of the small phial, discharges that follow one the other 100 times in sequence; and since all these repeated blows succeed one to the other rapidly, that is in the space of 1/50, of 1/200 of a second or less, it is possible to envision them, because they are so near one to the other, as reunited and confused in only one blow, that makes thus it sensible 100 times as stronger.[147]

It was hence clear to him that the physiological effects of two or more stimuli summated each other in time, if they were separated by a sufficiently short interval, whose length was an expression of the capability of the physiological mechanisms to 'accumulate' the effects of subsequent impressions. It was his belief that, compared to a small jar, a large jar would be able to deliver a larger quantity of electric fluid during this interval, because the time for the effective discharge of common jars was much shorter than this physiological interval. As a consequence, the intensity of the resulting commotion would increase in proportion as the discharge time would be less that this effective summation time. This accounted for the stronger efficacy of large jars compared to small ones. It also accounted for the greater efficacy of the Voltaic pile, because this device, being able to produce a 'continuous' (or «indeficient» to use Volta's expression) flow of electricity, should be at least as effective as the largest jars or assemblies of large jars that could be realised in a laboratory.

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tensione *elettrica, e quindi la velocità della corrente di questo fluido, è la medesima per le grandi boccie come per le picciole nella nostra supposizione); ma la scarica delle grandi, ossia molto capaci, dura più istanti che quella delle picciole boccie. Non v'ha dubbio: la scarica delle prime impiega più tempo; ella si estende, per così dire, a molte scariche successive, quantunque si compia ancora in un tempo cortissimo, tale che a noi pare un istante*».(VEN II, 75).

[146] «*Ma ciò assolutamente non può essere, dovendo per gradi elettrometrici eguali, per eguale* tensione *e spinta essere anche le velocità del fluido elettrico eguali. Adunque non è che per un tempo tanto più lungo, che può tale maggior quantità di fluido scorrere fuori, e scaricare la boccia grande, o la batteria.*» (VEN II, 236)

[147] «*si può, paragonando la scarica della grande giara o batteria colla scarica della boccetta 100 volte meno capace, si può, dico, considerare la prima come la ripetizione di 100 scariche eguali a quella della boccetta, scariche le quali si succedono e colpiscono la persona 100 volte di seguito: e siccome tutti questi colpi replicati si succedono cotanto rapidamente, cioè nello spazio di 1/50, di 1/200 di secondo o meno, si può riguardargli, sendo così prossimi gli uni agli altri, come riuniti e confusi in un sol colpo, che si fa sentire per tal guisa 100 volte più forte.*» (VEN II, 78)

In comparing the effects of Leyden jars of different capacitances he showed that «the same commotion in the fingers» was produced by a small jar of «9 [square] inches of coating» and by «a jar 4 times more capacious, that is of approximately 36 [square] inches, charged four times less, that is at 2 degrees of the same electrometer», and also by the following jars:

> a jar of ½ square foot with the charge of 1 degree; one of 1. [square] foot with that of ½ degree; one of 2. [square] feet with the charge of ¼ degree; charges that are all made of the same quantity of electric fluid.[148]

He commented:

> Here therefore the greatness of the capacity in the jars makes up exactly, as it seems, or almost exactly for the smaller *tension*, that is the degree of charge; and since the product of this *tension* times the quantity of electric fluid results to be the same; also equal is the effect of the shock, equal at least for its value; nor it is yet perceived any difference in the quality, that is in the way it affects the organs.[149]

In putting forward the idea that the intensity of the sensation depended on the product of tension and quantity of charge, Volta thus anticipated a fundamental notion of sensory physiology, i.e. the dependence of the sensation on the total energy of the stimulus. This notion underlies Bloch's law, that the time and intensity of the stimulus are interchangeable parameters for evoking a given sensation. Bloch's law is valid only for short duration stimuli, and indeed Volta clearly realised that there is an upper limit to the time in which the sensory mechanism is able to summate the effects of the stimulus. He developed this type of argument with particular reference to the behaviour of the Voltaic pile (or «electromotor»), a device capable of producing a potentially unlimited flow of electricity:

> What, I say, should we expect, if we consider that such action of the pile continues unendingly, and should thus produce an electric current also unending, that is a current that lasts more than that produced by the most capacious battery [of Leyden jars]? Should we expect perhaps an unlimited shock? Certainly not, since the subsequent impressions, that accumulate in a certain way, and merge one into the other to form almost a single one, correspondingly stronger and more sensible, as we have already observed, these impressions are those only that occur in a very short time.[150]

On this basis, it could thus be clear why the efficacy of a discharge of a Leyden jar increased by increasing its size and capacitance, but only up to a given point; and, moreover, why the pile possessed an efficacy generally

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[148] «*una boccia di 1/2 piede quadrato colla carica di 1. grado ; una di 1. piede con quella di gr. 1/2; ed una di 2. piedi colla carica di gr. 1/4: le quali cariche tutte sono formate come si comprende del l'istessa quantità di fluido elettrico*». (VEN II, 249)

[149] «*Qui dunque la grandezza della capacità nelle boccie supplisce esattamente, come pare, o quasi esattamente, alla minor* tensione, *ossia grado di carica : e siccome il prodotto di questa* tensione *nella quantità di fluido elettrico risulta il medesimo, così pure eguale è l'effetto della scossa, eguale almeno il suo valore; nè si accorge ancora che differisca nella qualità, ossia modo di affettare gli organi.*». (VEN II, 249-250).

[150] «*Cosa, dico, aspettarci dovremo, avuto riguardo che cotale azione della pila continuando incessantemente, produce una corrente elettrica pur incessante, ossia che dura infinitamente più di quella prodotta da qualsiasi più capace batteria? Forse una scossa smisurata? Non già; essendochè le impressioni successive, che si accumulano in certo modo, e confondendosi insieme ne formano come una sola altrettanto più efficace e risentita, conforme abbiamo fatto osservare*». (VEN II, 236)

comparable to that of a very large jar, although it produced a continuous flow of current (of potentially infinite duration according to its inventor). Indeed, by measuring the discharge time of a large Leyden jar that produced a shock comparable to the one produced by the pile, Volta believed it should be possible to estimate the physiological time in which subsequent impressions could be summated together (the 'integration time', the time in which the product time-intensity rule is applicable, according to Bloch's law)

In the case of the mechanism responsible for the commotion, Volta estimated this «very short time […] to be about a minute third», that is 1/60 of a second (16.66 milliseconds). This value corresponds impressively to the integration time of the muscle response to the electric stimulation of the skin measured with modern techniques (about 17 milliseconds).[151] Volta's performance appears really extraordinary if one considers that in his era the measurement of electric parameters was very approximate, and there was no reliable method for estimating short times, like those involved in the discharge of common Leyden jars.

Although Volta does not provide any detailed information on the method he used to calculate such a short time, he nonetheless provides a key to the logical process used to achieve it. He assumes that the discharge time of a Leyden jar is proportional, *ceteribus paribus*, to the surface of the coating, i.e. to the capacitance. This assumption is not justified if we consider the discharge time of a capacitor as the time needed for the voltage to go to zero. As we now know, in order to be completely discharged, a capacitor requires an infinite time, independently of its capacitance, because of the exponential time course of the discharge process. However, if we define 'discharge time' as the time required by the discharge process to bring the tension of the capacitor to a given low value (a value where it no longer has any physiological effect), then Volta's assumption is justified because such a time is proportional (*ceteribus paribus*) to the capacitance.

In the case of very large Leyden jars (and in particular of assemblies - or batteries - made up of very many Leyden jars), the discharge time was long enough to be measured, at least in an approximate way. Volta estimates that the discharge time of the largest batteries of his laboratory «does not arrive surely to be 1/20 of a second, and perhaps not even 1/50 or 1/100»(VEN II, 76), but he does not say how he succeeded in measuring such short times. We are led to suppose that perhaps his estimates are more the effect of an 'educated' (and 'fortunate') guess, rather than of real experiments. Nonetheless, Volta writes that he had succeeded «with a certain artifice» in making contacts that «lasted 1/50 of a second, and even less, that is about a minute third». (VEN II, 169)

In his estimates of the very short discharge times of Leyden jars, Volta takes into account as a maximum reference time the value of 1/20 of a second calculated by Van Marum for the huge battery of Leyden jars existing at the Teyler Museum in Holland, a battery that was «eight or ten times more capacious» than his own largest. By applying his proportion between capacitance and discharge time, Volta therefore calculates «1 minute third» as the discharge time of his large battery of about «60 square feet». Since this battery produced effects comparable to those of the pile, he concludes that 1 minute third is a good estimate of the physiological summation time of the mechanism involved in the muscle shock produced by electric stimuli. Therefore, logical deduction and experimental measurement seem to converge

[151] see KIMURA 2000.

towards a similar value, «1 minute third», that, as we have seen, is a very good anticipation of modern measurements.(VEN II, 76)

MEASURING THE 'EXPLOSIVE' DISTANCE WITH A STRAW: WHY SPARKS FROM THE EEL AND NOT FROM THE TORPEDO

With reference to his extraordinary accuracy in his measurements of both physical and physiological nature, it might be appropriate to discuss here another problem the Italian scientist dealt with in his experimental studies concerning the nature of the fluid involved in the action of the (Voltaic) pile.

The problem concerned the estimation of the maximum 'explosive' distance of the discharge of the pile, and of other electric devices, i.e. the maximum separation in a circuit through which the discharge could pass, making its passage 'sensible' in the form of sparks, sounds and other perceptible phenomena. Also from that point of view Volta's electric researches after the invention of the pile were theoretically related to the electric fish studies of Walsh and Cavendish. In his paper on the 'artificial torpedo', Cavendish had been able to show experimentally that the maximum sparking distance depended exclusively on the «degree of electrification» (i.e. the «tension» according to Volta's terminology), and not on the quantity of the electric fluid involved in the discharge.

Volta followed the same path as Cavendish, building «a small machine or spincterometer» to measure accurately the variation of the sparking or «explosive» distance with the change of electrometric degree (or tension) at which a Leyden jar was charged, and, like Cavendish, he concluded that the distance depended only on the «electrometric degree». The distance did not change by using jars of different capacity, and was thus not affected by the quantity of electric fluid involved in the discharge. Moreover, Volta found that there was a reasonably good direct proportionality («a direct reason») between the tension and the sparking distance, as shown for instance by the fact that the explosive distance was ¼ of a line (i.e. 0.53 mm) when the tension measured with Henly's «quadrant electrometer» was 2 ½ electrometric degrees, and increased in proportion when the electrometric degrees became double, quadruple, and so on. Only with very high tensions was there an appreciable deviation from a linear relation and, in Volta's opinion, this might depend on the non-linearity of Henly's instrument for large deviations of its indicator. (VEN II, 240-246)

It has been calculated that one degree of Henly's electrometer corresponds, in modern units, to about 1335 Volts[152], a potential much higher than what could be obtained with an 'ordinary' Voltaic pile, one made by, say, 50 couples of silver and zinc (which generated a potential of about 40 Volts, because, as we now know, about 0.8 Volts are produced by a single silver-zinc contact).

To explain why it was usually difficult to get a visible spark from the discharge of his pile Volta tried to evaluate the explosive distance of Leyden jars charged to a tension much smaller than that measurable with Henly's instrument. He therefore decided to use his straw electrometer, a sensitive device that he had developed for the measurement of «very low charges», on the model of those invented by Cavallo and Bennett. Besides being much more

[152] see POLVANI 1942 and HEILBRON 1977 A.

sensitive than Henly's instrument (up to about 40 times more), Volta's device had the advantage of being linear. Having established that for 2 ½ degrees of Henly's electrometer, the explosive distance was ¼ of a line, Volta used his electrometer to measure experimentally that 20 degrees of this device were necessary in order to produce a spark through a distance of 1/8 of a line,

> and about 10. for a distance that he could judge to be 1/16 of a line, i.e. equal to the thickness of a paper sheet, 16 of which are necessary to make a line. By following such a relation, for 4. degrees of the same thin-straw electrometer, the discharge would occur only at distance of 1/40 of a line, for 2. degrees at 1/80, and for 1. degree at 1/160 lin. etc.[153]

If we convert Volta's measurements and extrapolations to modern units, and, moreover, if we take into account the modern estimates of the voltage associated to the shock of a torpedo or of an electric eel, we can figure out from his studies why a spark could easily be obtained from the discharge of the eel, but not from a torpedo.

Ten degrees of Volta's straw electrometer correspond to about 330 Volts, a value within the range of the discharge of the electric eel. Since at such potential, as we can derive from Volta's estimates, the discharge might be able to cross 1/16 of a line (0.1325 mm, «the thickness of a paper sheet»), it is easy to understand why in 1775 Walsh was able to see an «electric spark» by forcing the passage of the eel shock «through a small gap or separation made in a tin lamina pasted on a glass». On the other hand, the voltage of a torpedo discharge is 40-50 Volts and thus a little higher than that corresponding to 1 degree of the straw electrometer. For such potential Volta calculated an explosive distance of «1/160» of a line, something like 13 µm, a separation too short to result in the production of a visible spark (and also too short to create reliably in a metallic circuit with the ordinary methods available to the 18th century scientists (very probably in the eel experiments Walsh used a penknife to produce the separation in the tin lamina as he had done in 1772 in his unsuccessful attempt to produce the spark from the torpedo).

Although Volta did not use his measurements of the sparking distance of artificial devices to explain the effects of the discharge of electric fish, *a posteriori* here too we cannot but admire the precision of results obtained by using very simple instruments, in this case an electrometer whose sensitive indicator was simply nothing more than a thin straw.

THE INTERNAL RESISTANCE OF THE VOLTAIC PILE

During the comparison of the effects of batteries of Leyden jars and the Voltaic pile, an interesting and somewhat unexpected observation emerged, difficult to account for at first within the physical conceptions of the time. It appeared that very large batteries of Leyden jars might, in some cases, generate a shock even more effective than one induced by a Voltaic pile developing the same tension. This could occur even when the battery of Leyden jars had been charged by a pile with which it had been compared (as for instance had been noticed by Van Marum, who asked Volta for an explanation).

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[153] «*e 10. circa per una distanza, che potè giudicare essere appunto 1/16 di linea, eguale cioè alla grossezza di una carta, di cui ben 16. ve ne vogliono a far una linea. Seguendo un tale rapporto, per 4. gradi dello stesso elettrometro a paglie sottili la scarica non potrà farsi che ad 1/40 di linea, per 2. gradi a 1/80 lin. per 1. grado a 1/160 lin. ec.*»

This difficulty could provide arguments against the perfect electric nature of the fluid involved in the action of the Voltaic pile. It was thus important for Volta to explain it within the known electrological laws. He pointed out that in the case of a Leyden jar the current flows freely, its only obstacle being the resistance of the external circuit of the discharge (one that was negligible in the case of metallic conductors, but that could be substantial in the case of humid conductors, and in particular when the discharge occurred through the human body). On the other hand, in the case of the pile, the current needs to force its passage also through the internal circuit represented by the metallic and humid disks making up the pile, and this may be a further obstacle to its flow, especially for the presence of humid conductors. This additional obstacle could eventually limit the overall passage of the current, and also its flows through the external circuit. As a result, the discharge of a pile could be less effective that the discharge of a large battery of Leyden jars. This difference, normally small, he notices may become particularly evident when the internal resistance of a pile to the current flow increases for some reason, «especially if the disks or humid layers of the said pile are not humid enough, or are imbued of pure water», even though in these circumstances the tension measured by the electrometer at the poles of the pile may not decrease appreciably.

To support this interpretation Volta showed that, compared to a large battery of Leyden jars, the effectiveness of the pile increased if the humid disks were thoroughly imbued with a saline solution instead of ordinary water, and decreased when distilled water was used. These types of experiment betray Volta's conviction that the humid disks of the pile had exclusively a conductive function. Indeed he would never fully accept that the pile was an electrochemical device, and that it derived the energy for its electromotive action from the chemical energy of the solutions by which the humid disks were imbued. However, his experiments fully supported the argument for the existence in the pile of an internal obstacle to the circulation of the electric fluid represented in particular by the humid disks.

Volta carried out an experiment clearly demonstrating the importance of the resistance to the circulation of electric fluid due to the materials the pile is composed of. He built one of his apparatuses (both in the 'column' or 'pile' form, and in the 'crown of cup' version), with humid conductors intercalated between the metals, with the special precaution that the metallic disks were all made of the same metal, in such a way as to have «no electromotive action». Afterwards he took a large Leyden jar «weakly charged, up to the point e.g. that it might give a moderate commotion up to the elbows by discharging it through a metal and the two arms without the intervention of any humid substance», and noticed that «if one discharges this jar by inserting in the conductive arc one of these piles or cup apparatuses, a commotion will be felt much weaker, arriving at the best up to the wrist; and as much as weak as larger are these apparatuses [i.e. the inactive Voltaic piles], that is as more numerous will be the humid layers». (VEN, II, 88-89).

With this experiment he gave a clear demonstration that even in the active apparatuses there should be an obstacle, that is «a resistance and delay, that the humid layers intercalated to any metallic couple produce in the electric current moved by the mutual contact of the different metals, that compose these apparatuses [i.e. the active piles]». In other words, he visualised in some way the internal resistance of the pile, resistance that must be taken into consideration when the external circuit is closed, and particularly when it absorbs a large quantity of current.

Moreover, in his great experimental imagination, Volta modified the conditions of these experiments in a singular way. He intercalates one of his inactive piles in the circuit of discharge of a true [Voltaic] pile [i.e. one made of humid layers and different metals and well 'active']. The shock felt became much less «sensible» with this arrangement compared to the situation in which the active pile was connected to the experimenter only through a «metallic lamina».

THE POWER OF THE TORPEDO AND OF THE VOLTAIC BATTERY IN WATER: 'MORE CIRCUITS THAN ONE'

This experiment, with an active Voltaic pile discharged through an inactive one, at least visually reminds us rather of the experiments in which Walsh intercalated a torpedo in the discharge circuit of another, to see whether the electric organ of the first could conduct the electricity of the shock of the second, a possibility seemingly in apparent contrast with the electrological notions of the age.

Although Walsh's experiment had shown clearly that a torpedo was able to conduct the discharge of another torpedo (and of a Leyden jar as well), the English scientist concluded that this should happen «not interiorly we might suppose, but rather exteriorly», i.e. not through the intrinsic tissue of the electric organ, but through the liquids surrounding the organ, a conclusion probably inspired by the reluctance of 18th century scientists to believe that an electric disequilibrium might be generated and accumulated by a device made exclusively of conductive substances.

Towards the end of his researches in La Rochelle, Walsh was mostly involved in experiments to find out how the shock of the torpedo could be transmitted through water, and how it might flow through many parallel circuits, making «more circuits than one». As we have seen, this type of problem was also central to Cavendish's work on the 'artificial torpedo'. It will be remembered that Cavendish had a rather clear notion of the possibility that the current might be divided through many parallel circuits according to the resistance that any of these circuits offered to its flow, in a way rather anticipating Kirchoff's laws.

A similar kind of process was crucial to Volta, and here too there is an ideal link between Volta, Walsh and Cavendish. There are two main aspects of Volta's interest in this type of problem. One concerns the experiments in which he tried to show that his «electromotor» or pile was capable of imitating the effect of the natural torpedo in water. In these experiments he contrived arrangements recalling Cavendish's 'artificial torpedo'. As with Cavendish, he built this device mainly to show that the shock of the torpedo might diffuse at a distance through the water.

Volta's 'artificial torpedo' might even produce a «sensible» shock capable of affecting the experimenter simply by his plunging his hand into the water where the device was immersed, while in the case of Cavendish, the shock was generally felt only if the experimenter put one hand in immediate contact with the fish, and the other in the water. The difference was evidently related to the stronger efficacy of the Voltaic battery in generating a prolonged and powerful flow of electricity.

As with Cavendish, in Volta's experiments the problem of the diffusion of the shock at a distance through the water medium implied a consideration of the laws of diffusion of current through multiple circuits, and a comparison of the conductive power of the water and liquid media (2nd class conductors in Volta's terminology) compared to metals (1st class conductors).

Like Cavendish, and in contrast to the common opinion of the age that humid bodies were «good conductors» of the electric fluid, Volta showed «that the conductors of the 2nd class, i.e. the liquids and the bodies impregnated by them, and in particular simple water, are poorly permeable to electric fluid in comparison with metals, or conductors of the first class». Electricity can flow through liquid bodies in a relatively easy way only if these are very large. According to Volta:

> not only the poorly humid bodies happen to be unable in transmitting the shock, but also those well soaked, and also natural water itself, had they not considerable wideness; unable a thin small cord wet, a thread of water smaller than one line, etc. if they are just a little long.[154]

And although capable of discharging a Leyden jar:

> a large cylinder of water of one inch diameter, or even of several inches, does not offer to the current flood a passage totally free, but, on the contrary, somewhat difficult and impeded, and more so than that of a metallic wire of 1/10 or 1/20 of line [in diameter]; and that in sum the plenty of electric fluid, which flows without any difficulty and with the utmost rapidity through a very thin metallic wire, in order to pass with equal or almost equal readiness and easiness through water, needs to divide itself and flow for thousands and thousands of small threads of this [water].[155]

Volta acknowledged that Cavendish, before him, had shown how poor a conductor water may be (even though he does not agree with the estimate of the English scientist that «pure water might be 400,000,000 times less permeable to the electric fluid than metals»). He proposes, on the basis of his own experiments, that a more realistic estimate of the relative resistance of metals compared to water might be «somewhat more than 100,000 or 200,000»). (VEN II, 99)

Due to this difficulty, in the passage through «such imperfect conductors, namely water or humid bodies», the electric flow would not be limited «to an extremely narrow path, and would try to widen itself and flow along wide roads». This tendency would explain why the discharge of a Voltaic pile (or of a strongly charged Leyden jar), communicating with a basin full of water through two large metallic laminae, can diffuse up to a distance «of one foot or more» from the direct water route between the laminae «such as to shock in a sensible way the hands plunged there». He adds:

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[154] «*inabili a trasmettere la scossa riescono non solamente i corpi poco umidi, come già si è notato, ma ben anche i meglio inzuppati, e l'acqua stessa in natura, ove non abbiano una considerabile larghezza; inabili un sottile cordoncino bagnato, un filo di acqua men grosso di una linea ec ., per poco che sian lunghi*» (VEN II, 252)

[155] «*che neppure un cilindro d'acqua di un pollice, anzi di alcuni pollici di diametro, offre ad esso torrente un passaggio intieramente libero, ma bensì alquanto difficile e impedito, e più che non lo sia per un filo metallico del diametro di 1/10, o di 1/20 di linea: che insomma la copia di fluido elettrico, che passa non difficilmente e con somma rapidità per un sottilissimo filo metallico, ha bisogno per passare con eguale o quasi eguale prestezza, e facilità attraverso l'acqua di dividersi, e scorrere per migliaja e migliaja di filetti di questa*». (VEN II, 252)

> In the case the shock were insensible to the hands at such a long distance from the laminae, because the pile was not powerful enough, or the Leyden jar a little less charged, or less big of what needed; they did not fail, either the jar, or the pile, to shock, or make to jump a recently killed frog, or some pieces of it cut in quarters, plunged in the water instead of the hands, at any distance whatever from the metallic laminae.[156]

A similar phenomenon - he noticed - might happen also using a real torpedo as a source of electricity instead of artificial devices.

Volta's recourse to frog preparations to reveal the diffusion at a distance through humid bodies is clearly inspired by the experiments that Galvani published in 1797, experiments in which the Bologna doctor used several prepared frogs to reveal the shock of the torpedo (see above).

Galvani had vividly commented on the sudden and simultaneous movement of these preparations at the moment of the torpedo's shock:

> it was for me a joyful scene seeing all of them moving at once, and, I would say, almost jumping.[157]

Volta was particularly inspired by the experiment when Galvani had placed several frogs at some distance from the torpedo in a condition in which an electrical communication existed «through the layer of water sprinkled over the table in collocating the torpedo on it».

After describing the experiments of current diffusion through the water of a basin, Volta noticed that a convenient way to demonstrate the diffusion through humid conductors was to use «cloths or cardboard, or other similar material plentifully soaked in it, instead of basins or tubs full of water». In particular, if a moist napkin was placed on the table and connected to an electric source (either the pile or a Leyden jar) through two metallic plates, and then several frog preparations were placed on it, then the current flow could be easily revealed, in a way he comments upon as follows (rather reminiscent of Galvani):

> It will be a curious scene, several frogs being spread here and there on the wet cloth, and covering the entire table, some of which only killed, other cut in pieces, some with both thighs and legs, and others just with legs, etc., to see them contract and jump, some more, some less, depending on if they lie near or farther from the metallic plates applied to the wet cloth; to see such bizarre movements, and convulsions for each discharge of the jar or the electromotor brought to these plates is an evident proof, that in passing from the one to the other [of the metallic plates] the electric flood, must, in order to meet with less resistance, spread to all that humid layer and travel for each ways.[158]

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[156] «*Qualora poi la scossa riuscisse insensibile alle mani in tanta distanza dalle lamine, per essere o non abbastanza potente la pila, o la boccia di Leyden un poco men carica, o un poco men grande del dovere; non mancavano o la boccia, o la pila di scuotere, e far balzare qualche rana di fresco trucidata, o qualche pezzo della medesima tagliata in quarti, che si tenesse immerso invece delle mani in quell'acqua, a qualunque distanza da esse lamine metalliche*» (VEN II, 260)

[157] GALVANI 1797, see footnote 141.

[158] «*Curioso spettacolo sarà, sparse trovandosi quà e là sul panno bagnato, che copra un tavolo intiero, diverse rane, quali trucidate soltanto, e scorticate, quali fatte in brani, dove ambedue le coscie colle gambe, e dove una gamba sola, ec. vederle convellersi, e balzare, qual più, qual meno, secondo che giacciono vicine o lontane alle piastre metalliche applicate allo stesso panno bagnato; veder tali bizzarri movimenti, e convulsioni per ogni scarica elettrica della boccia o dell'elettromotore portata sopra ad esse lastre è un'evidente riprova, che passando dall'una all'altra di contro il*

In his experiments Volta showed that the electric fluid diffuses up to longer distances when using pure or distilled water, whereas the effective diffusion distance decreases when salts at increasing concentration are dissolved in it. In this context he noted:

> This is the case (it is appropriate to remark it here) of the torpedo in the salty water of the sea, which must be touched in some part of the body near the back, or at least a hand must be approached near to it, either naked or armed of a good conductor, if one wishes to feel the shock; which on the contrary will happen to be much stronger in a bath of non-salty water.[159]

The problem of the scarce conducting power of humid bodies allowed Volta to face another type of apparent difficulty emerging after the invention of the pile, a difficulty that also seemed to conspire to assign some peculiar character to the fluid moved by the new device, different from that of genuine electricity. This was that the pile was able to produce shocks even if there was a liquid layer on its external surface, as for instance happened when a pile was plunged into a solution, in order to re-humidify the paper disks that had become dried after prolonged use. In such conditions a shock could still be produced in spite «of the water that wraps to a large extent the column [of the pile] and drips from the sides». Were the pile really electric, this humid layer wrapping the outside of the column should have shunted the electricity that flowed between the top and the base of the column, and no effect should thus have been felt by establishing a connection between the two poles of the pile, as Volta clearly recognised:

> It will astonish somebody that some electric tension would still remain at the summit of our column, and the faculty to give the shock through the appropriate touchings (as if one brings a hand armed with metal to the said summit, while the other hand communicates in a similar way, or through some other good manner, with the basis of the column), it will astonish, I say, that the column will keep such a power or electric tension when there is so much humidity at the exterior of the same column that still maintains another communication not interrupted between the two extremities, when all the column is wrapped by a veil of water, by a wet paper or wet leather.[160]

As occurred in dealing with the problem of the diffusion of electricity in water, to face this difficulty Volta took into account the great resistance of water and of humid conductors in general, a resistance so high that:

> an electric current which does not suffer any impediment or delay in flowing along a metallic wire of the most thin ones, meets instead a notable resistance,

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torrente elettrico, dee, per incontrare minore resistenza, spandersi per tutto quello strato umido intorno e percorrerne ogni via». (VEN II, 261-262)

[159] «*Ecco il caso (cade qui in acconcio di far ci osservare) della Torpedine nell'acqua salsa marina, cui convien toccare in qualche parte del corpo vicino alla schiena, od approssimarvi almeno la mano o nuda, o armata d'un buon conduttore, se si ha a sentire la scossa: la quale pure riuscirebbe assai più forte in un bagno d'acqua non salsa*». (VEN II, 263)

[160] «*Farà stupore ad alcuno, che rimanga ancora e qualche tensione elettrica, alla sommità della nostra colonna, e facoltà, di dare la scossa mediante i convenienti toccamenti (come portando una mano armata di metallo al contatto di essa sommità, mentre l'altra mano comunica similmente, o in altra buona maniera colla base di essa colonna), farà, dico, stupore che la colonna ritenga un tal potere e tensione elettrica quando vi è tant'umido all'e- sterno della medesima che pure mantiene un'altra comunicazione non interrotta tralle sue estremità, quando tutta la colonna è involta da un velo d'acqua, da una carta, o pelle bagnata*». (VEN II, 99)

and is more delayed if it should flow along a small channel of water thousands of times bigger that that wire, along a stripe of wet cardboard, of cloth, of leather, etc, wide even several lines, not to say anything of bodies less humid and correspondingly less deferent.[161]

As a consequence, a pile that had been soaked in a solution, and thus retained a liquid layer on its external surface, would still be able to shock a person because only a small fraction of electric fluid would flow along this liquid layer, and there would still remain enough to affect the body of that person (provided that this person established a good contact with the top and bottom pole of the pile).

This behaviour is not exclusive to the pile - Volta remarked - and thus does not point to some peculiar characteristic of the fluid moved by this device. It can be observed also with an electrical machine, if its prime conductors is made to communicate with the ground through «a thread of water wide only a few lines, or a small cord or strip of leather or cardboard wet» and the machine provides «abundant and continuous quantity of electric fluid to the said prime Conductor». (VEN II, 100)

The scarce conductivity of humid bodies also explains why the Voltaic pile (and a Leyden jar and an electric fish as well) can affect by its shock two or more people simultaneously touching the two poles of the electric device (according to an arrangement ‘in parallel’). Volta remarks, however, that when many persons are connected simultaneously to the pile, the intensity of the shock progressively decreases. In his view, this is why «when the number of conductive arcs increases, the *tension* of the pile decreases in proportion». (VEN II, 255)

For him, what might be surprising in these experiments is that many humid conductors do not succeed in «giving an entire and free passage to the electric flood» whereas a thin metallic wire can drastically reduce the efficacy of the pile. He remarks that the progressive reduction of the efficacy of the pile by increasing the number of «conductive arcs» is accompanied by a decrease of the tension measured at its poles. This decrease is more evident when the paper disks intercalated to the metallic laminae of the pile are almost dried, and when they are made of pure water. It is evident from these and other passages in his writings, that he has a quite clear intuition of the laws underlying the combination of electric circuits in series and in parallel, although he does not express them in a mathematical way (as Kirchoff was to do in the second half of the 19th century).

Besides increasing the number of the people through which a pile (or a Leyden jar or an electric machine) is discharged, a progressive reduction of the power of the electric device can be obtained through a shunting action of various conductors. In addition to metallic wires, particularly effective as he often remarks, a «sensible» reduction of the power of a pile can be obtained by surrounding it with «a not thin layer of water», or even by plunging the whole pile in water. The attenuation will be less «sensible» if the column of the pile is wrapped «only by a tenuous lamina, or by a wet paper, cloth or leather» (VEN II, 104) and even less so if around the column there are only a few strips of these materials. However, according to Volta, although the attenuation is

161 «*una corrente elettrica, la quale non soffre nè impedimento nè ritardo scorrendo per un filo metallico de’ più sottili, incontra invece notabile resistenza, e vien molto rallentata ove debba scorrere per un Canaletto d’acqua migliaja di volte più grosso di quel filo, per una striscia di cartone bagnato, di panno, di pelle, ecc. larga anche parecchie linee, per nulla dire de’ corpi meno umidi e corrispondentemente meno deferenti*». (VEN II, 100)

scarcely perceptible when around the pile we find only the humidity that drips from the humid disks, in order to achieve the maximal efficacy with a given pile, it is necessary to completely dry its external surface.

VOLTA'S MEMORANDUM FOR EXPERIMENTS ON ELECTRIC FISH: 'A TORPEDO IN THE PRESS'

Volta certainly remembered these experiments when in 1805, he gave his pupil Pietro Configliachi his advice on the way to maximise the possibility of measuring the electricity of the torpedo with a physical device:

> The researches that more are at my heart have the purpose of rendering, if possible, that electricity of the torpedo sensible to the electrometer. Of no use would be to try the fish in the water. It will be therefore necessary to draw it out, and explore it with suitable methods, once exposed to the air and even to dry it a little, such as it will not be dripping wet.[162]

Volta thought it would perhaps be more convenient to try measurement with the electric organs separated from the animal, deprived of the humid tissues surrounding them, because these tissues might allow for a continuous passage of electricity and thus reduce the intensity of the shock.

The problem of the best way to attempt physical measurement of the torpedo's electricity (that he deals with in his letter to Configliachi) brings us to consider another interesting aspect of his relationship to this singular fish. We have seen how he believed the main experimental problem in measuring the electricity of the torpedo with a physical instrument to be the duration of the discharge, apparently too short to affect the electrometers of the epoch, and even a very sensitive 'Condensatore'. We may recall that Walsh had also invoked a similar explanation to account for the failure of the torpedo's shock to affect the «pith balls» of Lane's electrometer. Volta argued that if the production of electricity by the torpedo:

> every time it tries to give the shock, were transient, and did last but one extremely short instant, it is not difficult to understand that, being nevertheless sufficient in order to produce the shock, it might not be so in order to charge the Condensatore, it might not give enough time to that.[163]

It thus became crucial for him to try some artifice that could oblige the torpedo to produce electricity in a rather more continuous way, so as to bring to the measuring instrument sufficient charge for it to be revealed. We have already discussed his suggestion that a weak electricity might indeed be produced by the torpedo in normal conditions, because in the intervals between the shocks, there might be some kind of imperfect communication between the disks or pellicles of the electric organs. It was Volta's belief that the shock would be produced when, as a consequence of some mechanical effort, the

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[162] «*Le ricerche, che più mi stanno a cuore, hanno per oggetto di rendere, se si può, sensibile all'elettrometro codesta elettricità mossa dalla Torpedine. Inutile sarà il tentar ciò restando il pesce sommerso nell'acqua: converrà dunque trarnelo fuora, ed esplorarlo con mezzi acconci, esposto all'aria, ed anche asciugato un poco, tanto che non sia più grondante di acqua*». (VEN II, 194)

[163] «*ogni qualvolta tenta di dare la scossa, fosse passeggiero, e non durasse che un istante brevissimo, non è difficile comprendere, come essendo tuttavia bastante per produrre la scossa, potrebbe non esserlo per caricare sensibilmente il Condensatore, potrebbe non dar tempo a ciò; l'applicazione del qual Condensatore riuscirebbe altronde inutile, quando non venisse fatta al momento preciso, cosa assai difficile*». (VEN II, 199)

communication between these discs was made more perfect (see above). In developing this type of argument, Volta wrote to Configliachi that a good way to make torpedo electricity continuous would be as follows:

> by placing, on the back of the Torpedo laid down on with its belly on the basin, or directly on the wet cloth, a weight that might compress it enough, one might, I think, oblige those organs to act in a continuous way; then the Condensatore will be able to draw the sufficient charge of electricity, as it draws it from an ordinary pile put in a good order. It would be a good thing, that the weight placed on the back in the site correspondent to the place of the said organs were of metal, and that the Condensatore would be connected to this weight instead of the naked back. [164]

Poor torpedo, we are tempted to say, compressed under a metallic weight, or even cut into pieces in order to isolate the organ electric organs and make their electricity more evident to the instruments! Indeed, if such a reaction might be justified, especially given the increased sensibility towards animal pain and distress that characterises modern society, we should also see in Volta's attitude a further expression of his tendency to apply the same principles and laws to both the animate and inanimate world. For him the torpedo was rather like a physical instrument, a kind of electric machine, whose functioning can be studied using a similar approach to that used to understand the functioning of the 'physical' devices.

This attitude was not, however, simply an expression of the tendency to reduce physiology to physics, as has been claimed, since he was also convinced that the study of animate nature could be an important source of inspiration for understanding the mechanisms underlying the functioning of physical devices, and also for building new apparatuses, as had happened in the case of the invention of the (Voltaic) pile. The inspiration he drew from the torpedo's electric organ in the invention of his extraordinary device appears most vividly in a famous inscription that Configliachi wrote as an epigraph to a famous portrait of the Pavia scientist painted during his lifetime:

> Alexander Volta, in re electrica princeps, vim raiae torpedinis meditatus, naturae interpres et aemulus.[165]

Returning to the poor torpedo, that he suggested be placed under a weight in order to produce a continuous shock, and thus transfer enough charge to the measuring instrument, Volta told Configliachi to try a series of experiments that might confirm the similarity of the effects of the torpedo to those of the pile (and, more in general, to artificial electricity). He thought it should be possible to produce most of the physiological effects with the shocks of the torpedo that were usually obtained from the discharge of the pile. As well as the shock, it should be possible to obtain from the discharge of the fish «the known flash, or transient glimmer in the eyes« when the discharge passes

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[164] «*ponendo sopra la schiena della Torpedine giacente col ventre sul bacile, o immediatamente sul tavolo bagnato, un peso che la comprima sufficientemente, si ridurrebber, credo, quegli organi a dover agire continuamente; e allora il Condensatore ne ritrarrebbe in qualsisia momento la competente carica di elettricità, com'esso la ritrae sempre da una pila ordinaria messa in buon ordine, e in istato di agire incessantemente. Sarebbe bene, che il peso posto sulla schiena al luogo corrispondente a detti organi fosse di metallo, e questo poi si toccasse, anzichè la schiena nuda, dal filo annesso al Condensatore*». (VEN II, 195)

[165] «*Alexander Volta, prince in the field of electricity, having meditated the power of the electric ray, [was] interpreter and emulator of nature*».

across the experimenter's head, or also «that pricking pain, that lively burning sensation on the skin of the visage or of other delicate parts».

Interestingly, Volta believed the sensation of pain could be conveniently exploited to establish the polarity of the torpedo shock:

> since with the artificial electromotors the negative pole is that that excites a pain much more biting and burning; therefore, were to happen the same with the Torpedo, which we should consider as a natural electromotor, one might understand from such an experiment in which of the two parts would be located the electricity both *in plus* both *in minus*, if the discharge would happen from the back to the belly, or from the belly to the back; a thing that also would be important to ascertain.[166]

As to the taste sensation, this should also be, in principle, easily obtained from a torpedo reduced to providing continuous electricity because of the weight compressing its electric organs. However, to achieve this in the most appropriate way, one should try to decrease the intensity of the torpedo's electricity so as not to stimulate the shock and the burning pain simultaneously. We note here again *en passant* how Volta considers the biological device rather as an artificial electric instrument, whose power might be controlled more or less easily by some kind of manipulation. A similar attitude is evident when Volta suggests to Configliachi that, were the power of the torpedo decreased to such a low level that «it would not made itself sensible but to one or two articulations of the finger», one could try to restore it «by moistening the electric organs with water, either simple, or salty», as he usually does in re-empowering exhausted 'physical' piles. (VEN II, 201-202)

The last part of the letter to Configliachi vividly conveys the confidence he has acquired in the possibility of explaining the power of both the animal and the physical device, on the basis of the physical laws he has discovered, and in the belief that the torpedo and the (Voltaic) battery could produce the same effects, and were an imitation of each other (in a reciprocal sense):

> I have no doubt that more you will examine, and fathom the electric organs of the Torpedo, you cannot but discover a greater resemblance with my batteries, or even an essential conformity with those that I call of the third type [i.e. composed exclusively of humid conductors]. The construction altogether singular of these organs was long a mystery for both Physicists and Physiologist, and it is still so for many, but it ended being so for me since the moment that I succeeded in building my motor apparatuses, namely the above mentioned batteries of the third type [i.e. those made all by humid conductors], that are, I endeavour to say it, the same thing at bottom as those organs. The experiments and researches, that I proposed you, are aimed at verifying and confirming that in all manners, such as to eventually convince those that might still have doubts, or move objections. If such experiments succeed well, as I hope, they will show how could be obtained from Torpedoes outside water, or even from their electric organs alone, besides the shocks, already known, all the other phenomena that my batteries present. On the other hand, I have already shown how reciprocally the batteries imitate perfectly the Torpedoes, also in water, shocking and benumbing the hand plunged in the water itself: shocking it even before it could touch the fish body; also at a considerable distance: a thing that was not understood, and that I explain, and confirm with other experiments, in a way not to leave any difficulty. At imitation of the Torpedoes which throw shocks in

[166] «*Siccome cogli elettromotori artificiali il polo negativo è quello che eccita un dolore molto più rabbioso e cocente; così avvenendo lo stesso colla Torpedine, che dobbiam pur tenere per un elettromotore naturale, si capirà anche da questa sperienza in quale delle due parti risieda l'elettricità sua* per eccesso*, in quale l'elettricità* per difetto*, se la scarica facciasi dalla schiena al ventre, o dal ventre alla schiena: la qual cosa importa pure di sapere.*» (VEN II, 196)

their native element, I put in action these batteries of mine, and make them give similarly shocks even under water, and also to a plunged hand, which does not however come to touch them: in sum I reduce them to be true artificial Torpedoes.[167]

From this passage, we can also gain an insight into the vast scale of Volta's scientific project, a scale which is one of the reasons for the success of the extraordinary path he opened up with his research on animal electricity, and concluded with the invention of the (Voltaic) battery. By reflecting on 'nature', for both the forms and mechanisms it offers to attentive examination, 'art' succeeds in creating new instruments and new machines; by studying these machines, the scientist can discover the mechanisms underlying animal phenomena.

From an historical point of view, it is of secondary interest that the electric organ of the torpedo works in a way substantially different from what Volta had supposed more than two centuries ago, in particular the mechanism of the fish shock. It is perhaps appropriate to reflect on the fact that, from the development of the researches of Volta and Galvani, both ideally linked to John Walsh's scientific work, fields of science will emerge at both a conceptual and a practical level that will create the conditions for the birth of modern electrophysiology. Electrophysiology would eventually lead to our understanding of the mechanisms that underlie the extraordinary power of electric fish («the untamed art of the extraordinary Torpedo»). This development would occur through sometimes unpredictable ways, and often along routes very different from those hypothesised by the scientists that started the process more than two centuries ago.

As the research on the mechanism of the electric eel's shock begun fifty years ago by Richard Keynes and His Martins-Ferreira showed, the electric fish's shock depends on the summation of the discharge of the electrochemical potentials accumulated across the plasma membrane of the numerous 'electroplates' composing the electric organ ('electroplates' or 'electrocytes' are the modern terms for the disks of the organs).[168] Although an electrochemical potential arises from the contact of solutions having different ionic concentrations, this potential cannot be considered as a simple expression of the effect produced by the «mere contact of conductive substances of different species», as Volta supposed for both the artificial and the natural electromotor. It is based on a notion Volta never accepted, that of the chemical origin of the electric phenomena of the pile. Furthermore, it depends on a

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[167] «*non dubito, che quanto più esaminerete, e scandaglierete gli organi elettrici della Torpedine, non siate per ravvisarvi una più grande rassomiglianza colle mie pile, anzi una essenziale conformità, con quelle che chiamo di terzo genere. La costruzione affatto singolare di tali organi fu per gran tempo un arcano per i Fisici, e per i Fisiologi, e forse lo è ancora per molti; ma cessano d'esserlo per me dal momento che giunsi a costrurre i miei apparati elettro motori, e singolarmente le pile suddette di terzo genere, che sono, ardisco dirlo, la stessa cosa in fondo che quegli organi. Le sperienze e ricerche, che vi ho proposte, hanno per iscopo di verificare e confermar ciò in tutte le maniere, per finir di convincere chi ancor ne dubitasse, o movesse delle obiezioni. Riuscendo bene codeste esperienze, siccome spero, verranno a mostrare come si ottengano dalle Torpedini fuori dell'acqua, anzi dai soli loro organi elettrici, oltre le scosse, già note, tutti gli altri fenomeni che presentano le mie pile. Io altronde ho già mostrato come reciprocamente le pile imitino perfettamente le Torpedini eziandio sott'acqua, scuotendo e intorpidendo una mano tuffata in essa acqua: scuotendola anche prima che giunga a toccare il corpo del pesce; anche ad una considerabile distanza: ciò, che ben non si comprendeva, e ch'io spiego, comprovandolo con altre sperienze, in una maniera che non lascia più alcuna difficoltà. Ad imitazione delle Torpedini, che lanciano scosse entro al nativo loro elemento, faccio agire coteste mie pile, e dare le scosse parimenti sott'acqua, e sì anche ad una mano tuffata, che pur non giunge a toccarla: insomma le riduco ad essere vere Torpedini artificiali*». (VEN II, 202-203)

[168] KEYNES and MARTINS-FERREIRA 1953; see MOLLER 1996.

particular organisation of the cell membrane, with pumps that create asymmetric ionic distributions between the intracellular and extracellular compartments, and ionic channels that generate electric potentials from ionic gradients. On the other hand, the concept of electrochemical potential emerging in the 19th century, mainly due to the work of Walter Nernst, is undoubtedly a result of the development of the physico-chemical research triggered off by the invention of the electric battery or Voltaic pile at the turn of the 18th century.[169]

Another interesting aspect of modern research on the study of electric fish is that it shows that the production of a strong shock in electric fish is not based on the development, via evolution, of an entirely new mechanism of electricity production, proper to these unusual species of fish. On the contrary, it represents an adaptive organisation that exploits the mechanisms of electrical excitability present in the cells of all common organisms, mechanisms that are particularly developed in nerve and muscle tissues. From that point of view, one can safely say that two centuries of electrophysiological studies have fully confirmed the historical and logical relation that characterised, in the second half of the 18th century, the study of the torpedo and electric eel (carried out mainly by Walsh) and the study of the neuro-muscular physiology (mainly due to Luigi Galvani).[170] In some ways, modern studies (in particular the epoch making discoveries on the mechanism of the electric excitability of nerve cells published in 1952 by Alan Hodgkin and his collaborators), have confirmed the significance of the question Hunter raised in a rhetorical form in 1773, from the consideration of the extreme richness of the innervation of the torpedo’s electric organ:

> How far this may be connected with the power of the nerves in general, and how far it may lead to an explanation of their operations, times and future discoveries alone can fully determine.

Moreover, if one considers the importance of the research prompted by those studies of Walsh’s begun in 1772, as they have developed over more that two centuries, one sees the value of what he wrote on introducing his 1773 study of the torpedo’s shock:

> a subject not curious in itself, but opening a large field for interesting enquiry, both to the electrician in his walk of physics, and to all who consider, particularly or generally, the animal oeconomy.

AN ‘ELECTRIC SENSE’

Besides the inspiration that Volta derived from electric fish in general, and from Walsh’s work in particular, in inventing his «*organe électrique artificiel*» (or «electromotor» or «pile»), there is another significant aspect of Volta’s relationship with these fish and (with the English scientist).

In the already quoted letter to Mme Le Noir de Nanteuil that Volta wrote from London in 1782 (a letter, as we know, dealing with the subject of «animal electricity» and largely based on a personal conversation with Walsh), Volta referred to an interesting observation Walsh had made in his experiments on the electric eel:

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[169] NERNST 1888; BERNSTEIN 1912; see HILLE 1992.
[170] See PICCOLINO 1998.

> Mr Walsh [...] has discovered in the said eel what can rightly be called an *electric sense*. If one puts in the water tub where the eel is, one, two, or more good conductors, but separated, the animal does not seem to be affected at all; but, as soon as a communication is established between two of these plunged conductors so as to complete the circuit, and the parts of the conductors that are outside the tub are also reunited, the animal becomes agitated, and rushes to them, and brings the extremity of its head to one end of this *conductive arc* as if he would like to smell it, he provokes the electric discharge, which hits the intermediate person or persons, assuming that these create the chain linking the two conductors.[171] (see Fig. 18)

We have other reports concerning this same observation in other writings of the age, although only in Volta's does the phrase 'electric sense' appear to designate the surprising sensory mechanism allowing many species of fish to detect the presence in their habitat of objects on the basis of their electric characteristics. One of these reports was published in the same year as Volta's letter to Mme Le Noir de Nanteuil, by Jan Ingenhousz, who we have noted was in London in 1778, and who on that occasion was able to repeat Walsh's experiment of spark production from the shock of the electric eel, by using one of the fish used by the English scientist in his original experiments of the summer of 1775. The report pf Ingenhousz is also based on a conversation with Walsh. After speaking of his own experiments with Walsh's electric eel (see above), he describes Walsh's interesting observation on the peculiar property of the eel as follows:

> For example a group of 10 persons, hand by hand, situate themselves in such a way so that the fish can see only the two extreme ones. Of these, one puts the finger in the water near the head of the fish, while, at the same time, the other does the same near the tail. When the persons connect one to the other by joining hands, or, said with the words of the electrician, close the electric circuit, the fish will absolutely notice it and will discharge his electric shock. At the moment that the persons separate from each other, the fish will immediately understand that and will no longer shoot his dart, aware of its ineffectiveness. This is the peculiar characteristic that Mr. Walsh has observed. [172]

Volta's letter to Mme Le Noir de Nanteuil was not published at the time, and thus could not help to circulate this interesting result of Walsh's experiments with the eel. Probably also Ingenhousz' information had a relatively small circulation, since it was published in German by the Dutch scientist in his *Vermischte Schriften.* However, in 1795 Walsh's experiment was mentioned by Tiberius Cavallo in the last edition of his 'Complete Treatise on Electricity', a work widely circulated in the 'Republic of Letters'.

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[171] «*Mr. Walsh* [...] *a découvert dans la dite Anguille ce qu'on peut appeler proprement un* sens électrique. *Si on plonge dans le baquet d'eau où elle se trouve un, deux, ou plusieurs bons conducteurs, mais interrompus, l'animal n'en paroît aucunement affecté; mais si tôt qu'on vient à établir une comuni- cation entre deux de ces conducteurs plongés pour compléter le circuit, qu'on réunit même les parties qui restent hors du baquet, l'animal s'agite, accourt, et porte l'extremité de sa tête à un bout de cet arc conducteur comme pour le flairer, y fait la décharge électrique, qui frappe la personne ou les personnes intermédiaires, supposé que celles-ci fassent la chaîne de réunion des deux conducteurs.* (VEN I, 11)

[172] «*Zum Beispiel, eine Gesellschaft von zehn Personen Hand in Hand steht sich so, daß der Fisch nur die zwei letzteren sehen kann, davon die eine den Finger bei dem Kopfe des Fisches ins Wasser hält, mittlerweile es die andere bei dem Schwanze thut. Wenn diese zehn Personen mit den Händen einander fassen, oder als ein Elektriker zu sprechen, wenn sie die elektrische Kette ergänzen, so bemerkt es der Fisch vollkommen, und er wird einen Erschuetterungsfunken durch die ganze Gesellschaft durchschießen. Sobald aber diese Gesellschaft sich trennt, wird es der Fisch den Augenblick erkennen, und er läßt es wol bleiben, daß er seinen Blitz abschleuderte, überzeugt, daß er seine Wirkung haben wird. Herr Walsh hat diese sonderbare Eigenschaft wahrgenommen*». (INGENHOUSZ 1782 pp. 275-276).

After describing Walsh's observations on the torpedo and mentioning the experiment of spark production from the gymnotus (the eel of Surinam), Cavallo writes:

> Mr. Walsh made another remarkable discovery with the gymnotus, which he shewed at his house to various ingenious persons: it was a new sort of sense in the animal, by which he knew when the bodies which came near him, were such as could receive the shock (*viz.* Conductors) and when they were of the contrary nature; in the former of which cases the animal gave the shock, but not in the latter. In order to shew this wonderful property, divers experiments were made, but the most convincing one was the following: - the extremities of two wires were dipped into the water of the vessel wherein the animal was kept, then they were bent, and extended a great way, and lastly terminated in two separate glasses full of water. These wires being supported by non-conductors at a considerable distance from each other, it is plain that the circuit was not complete: but if a person put the fingers of both his hands into the glasses wherein the wires terminated, *viz.* those of one hand into one, and those of the other hand into the other glass, then the circuit became complete. Now it was constantly observed, that whilst the above-described circuit remained interrupted, the animal never went purposely near the extremities of the wires, as he used to do when willing to give the shock: but the moment that the circuit was completed, either by a person or any other Conductor, the animal immediately went toward the wires, and gave the shock; though the completion of the circuit was performed quite out of his sight.[173]

In contrast to what had happened with the experiment of spark production from the eel shock, Walsh's experiment of the 'electric sense' did not receive much attention at the time it was carried out, and it was also neglected for a long time afterwards, despite the recurrence of interest in electric mechanisms in animals that characterised the mid-19th century, particularly after the experiments of Carlo Matteucci in Italy and Emil du Bois-Reymond in Germany. As Bernd Fritzsch and Peter Moller have suggested, it is possible that this was because of the difficulty over recognising that animals might have forms of sensory perceptions that were totally lacking in people.[174]

With time, and rather like what had occurred with Walsh's previous achievement with the same fish, the memory of the ability of the eel to distinguish between conductive and non-conductive bodies was lost, and this probably contributed to delaying the discovery of electroreception, a sensory ability fully recognised only in relatively recent times, despite the presence of anatomical indications on the importance of electrical mechanisms in many fish.

There are various forms of electroreception, one important distinction being between an eminently passive one, whereby a fish can detect the electric signals emitted by other animals present in its habitat, and an active one, in which the sensory process is coupled with a mechanism of production of electric signals used to fathom the environment ('electrodetection' or 'electrolocation'). This second type of electroreception is present in many fish and depends on the action of organs similar to those of the torpedo, the electric eel and the Nile catfish, whose presence had been recognised in many 'ordinary' fish since the first half of the 19th century.[175]

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[173] CAVALLO 1795, IV Vol. p. 309-311.
[174] MOLLER and FRITZSCH 1993; MOLLER 1995.
[175] LISSMAN 1958; BULLOCK *et al* 1961; see MOLLER 1995.

Compared to those of classical ‘electric’ fish, the organs present in most of the fish with active electroreception systems are small and produce weak signals (only a few Volts), that are of no value as tools either of aggression or defence. The presence in many fish of small electric organs apparently devoid of the ability to produce strong shocks has intrigued physiologists and zoologists for a long time, and these fish have long been indicated as ‘pseudo-electric fish’.[176] Their organs, apparently destitute of any useful function, seemed a challenge to Darwin’s evolutionary theory. Notwithstanding the demonstration of the ability of these fish to produce weak electric signals (initially obtained through the use of frog preparations in experiments recalling those in which Galvani used frogs to demonstrate the torpedo’s electricity), the functional significance of these organs took a long time to be recognised. It emerged first from behavioural experiments showing that fish endowed with small electric organs were able to avoid obstacles in their swimming, detecting prey or avoiding predators in complete darkness.[177]

CONCLUDING REMARKS

The discovery of electroreception occurred at a time when the memory of Walsh’s experiment on the ‘electric sense’ of the electric eel had been largely lost. It is difficult to be certain that this discovery might have occurred in an earlier age, had Walsh’s experiment not disappeared into oblivion. What we can say is that, as in the case of the eel spark experiment, the author’s decision not to publish a detailed account of the experiment undoubtedly contributed to his achievement’s oblivion. If a scientist of the *Ancient régime* might be fully satisfied by a more or less private demonstration of his experiments, or by a discussion of the results obtained within the circle of a few colleagues, certainly a public record of the scientific achievements, in the form of a document written for wide circulation, offers a better chance that the findings of scientific investigation may help scientific progress in the course of human history.

From another perspective, a written document of a scientific character, even if it is conceived only for private use, and is thus not directly linked to the main course of history, may be of great interest for the reconstruction of important phases of scientific progress.

In the case of Volta, most of his manuscripts have survived through the many vicissitudes of history: during the Second World War, thanks to Francesco Massardi, they were hidden in iron chests, buried in the foundations of the bell tower of a church in the north of Italy, and thus preserved from destruction and dispersion. They are now kept at the ‘Istituto Lombardo’ in Milan. They were made available for historians through the magnificent *Edizione Nazionale*, published between 1918 and 1966, and are now available also in an electronic form. The manuscripts of Cavendish’s electric researches have survived thanks to the Dukes of Devonshire (the family Cavendish belonged to) and, as already mentioned, they were edited and published by Maxwell in 1879. In the case of Walsh, the manuscript of the ‘journal of the experiments’ was donated in 1965 to the Royal Society by John Arthur Walsh, last Baron Ormathwaite, an indirect descendant of John Walsh.

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[176] see SIHLEANU 1876.
[177] LISSMAN 1958; see MOLLER 1995.

We hope we have helped to rescue from oblivion the figure of John Walsh, an 18th century ‘natural philosopher’, largely unremembered, despite the importance of his studies on electric fish, and of the researches that he promoted, directly or indirectly.

Our work was prompted by our ‘discovery’ of the manuscript of Walsh’s ‘Journal of the experiments’ in the Library of the Royal Society, and of the ‘Diary of a visit to France’ now in the John Rylands Library of Manchester University. In studying these manuscripts, and other scientific documents of the age, we can realise how writing can transmit, across great distances of time and space, the record of past events, whose interest will not decrease with the passage of time, and whose reading might help us to relive exciting moments of human history from a privileged position.

Perhaps nobody has portrayed this extraordinary power of writing as well as Galileo Galilei in his *Dialogo sopra i due Massimi Sistemi del Mondo*, when Sagredo, one of the three protagonists, after enumerating some of the ‘many and so much wonderful inventions found by humans, concludes:

> But, above all the stupendous inventions, what highness of intelligence was that who envisioned to find a way for communicating his more recondite thoughts to any other person whomever, even if distant by an extremely long interval of space or time? To speak with those that are in the Indies, to speak to those who are still not borne or not will be for up to one or ten thousands years? And with which easiness? with the various medleys of twenty small characters over a paper. Let this be the seal of all the admirable human inventions. Le this be the end of our discourses of today.[178]

[178] «*Ma sopra tutte le invenzioni stupende, qual eminenza di mente fu quella di colui che s'immaginò di trovar modo di comunicare i suoi piú reconditi pensieri a qualsivoglia altra persona, benché distante per lunghissimo intervallo di luogo e di tempo? parlare con quelli che son nell'Indie, parlare a quelli che non sono ancora nati né saranno se non di qua a mille e dieci mila anni? e con qual facilità? con i vari accozzamenti di venti caratteruzzi sopra una carta. Sia questo il sigillo di tutte le ammirande invenzioni umane, e la chiusa de’ nostri ragionamenti di questo giorno*». (GALILEO 1632, p. 98).

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FIGURE LEGENDS

Fig. 1 The title page of journal on 'The experiments on the Torpedo' at the Library of the Royal Society, © The Royal Society.

Fig. 2 Two plates on Torpedo from Lorenzini 1678.

Fig. 3. A plate on Torpedo from Réaumur with the detail of the structure of the electric organs.

Fig. 4. A 19th century view of La Rochelle with the *Hotel de Ville* which was the usual place for the Meetings of the *Académie*, From a half of the 19th century view by A. Asselineau. By courtesy of the Mediathèque 'Michel Crepeau', La Rochelle ©.

Fig. 5. The initial page of the letter on the Torpedo written by Walsh to Franklin on 12 July 1772. By courtesy of the Royal Society of London © The Royal Society

Fig. 6. The half title page of the article of Walsh concerning the experiments on the torpedo performed at La Rochelle republished in 1775 by the Royal Society with on the left an autograph of the author. By courtesy of the Library of the Bakken Institute of Minneapolis ©.

Fig. 7. The passage of 'journal of the experiments' of 9 July 1772 where Walsh first announces «the Effects of the Torpedo to be Electrical»: «Je l'ai donté». By courtesy of the Royal Society of London, © The Royal Society.

Fig. 8. The half title page of the Italian translation of the 'Discourse on Torpedo' by John Pringle.

Fig. 9. The first page of the '*taccuino*' (notebook) of Galvani concerning his experiments on the torpedoes of the Adriatic coast of Italy.

Fig. 10. A plate illustrating the anatomical article on the electric eel published in 1775 by Hunter.

Fig. 11. The extract of the Le Roy article concerning Walsh's experiments with the electric eel published in Milan on 1777.

Fig. 12. The half title page of Pringle's 'Discourse on Torpedo' published in 1775 but read at the Royal Society in 1774.

Fig. 13. The plate with the torpedoes of La Rochelle illustrating the anatomical article published in 1773 by Hunter.

Fig. 14. An image of the 18th century with the transmission of torpedo shock through a human chain. From an original signed B. N./ J.L. Charmet.

Fig. 15. A mosaic from Pompei with Lobsters, Polyps, Torpedoes and other sea creatures. From Museo Archeologico Nazionale Napoli ©.

Fig. 16. The plate of the 'artificial torpedo' of Cavendish with the graphical representation of the diffusion of the electricity in water.

Fig. 17. A page of Volta's letter dated 20 March 1800 announcing the invention of the battery, the '*organe électrique artificiel*' By courtesy of the Royal Society, © The Royal Society.

Fig. 18 The passage of Volta' letter to Mme Le Noir de Nanteuil concerning the 'electric sense' of the Gymnotus. By courtesy of the '*Istituto Lombardo, Accademia di Scienze e Lettere*' of Milan ©.