

## BIOLOGY, ELECTROMAGNETISM, AND THE NERVOUS SYSTEM

### INTRODUCTION

In his 1982 *The Growth of Biological Thought* Emeritus Professor Ernst Mayr, *eminence grise* of biology at Harvard, wrote, "Philosophers of Science, most of them with a background in physics, have unfortunately based their treatment of the philosophy and methodology of science almost exclusively on the physical sciences."<sup>1</sup> Mayr insisted that the physical sciences were not 'an appropriate yardstick of science'; they condemned the biological sciences to vitalism and incompatibility with the 'hard sciences'. He stressed instead the plurality of science, suggesting that Darwin, Mendel, Bernard, and Freud had perhaps made a greater contribution to our world view than the physicists. But contributing to our world view was not one of the objectives of science as enumerated by Mayr in that same volume.<sup>2</sup> What Mayr had to say about the objectives of science and about scientific method did not characterize the work of the men he mentioned in any way. Of scientific method Mayr wrote:

The reason why the hypothetico-deductive method has been widely adopted is that it has two great advantages. First it fits right in with the growing conviction that there is no absolute truth and that our conclusions and theories should continually be tested. And second, connected with this new relativism, it encourages the continuous establishment of new theories and the search for new observations and new experiments that either confirm or refute the new hypotheses.

Although it may be argued that the men Mayr mentions perhaps endeavored in some way to discover patterns of relationships among phenomena and processes, because fields in which they worked were so new, any explanations they might offer were mostly statements of findings of the patterns of relationships discovered. It is important to point out that they were not trying to organize knowledge in a systematic way, nor were they concerned with generating testable, explanatory hypotheses. With regard to Mayr's own area of expertise, evolutionary theory, his insistence that genetic gradualism is necessary for the continuity of evolutionary change, and that life is unique and not favored by chemistry and the forces of entropy, will be portrayed as unscientific speculation. The case against Mayr's genetic gradualism will be made in the final section of this paper.

Mayr insists that:

Because evolution as explained by Darwin is by necessity gradual, it is quite incompatible with *essentialism*. However, the philosophy of essentialism fitted well with the thinking of the physical scientists, whose 'classes' consist of identical entities, be they sodium atoms, protons, or pions.<sup>3</sup>

Despite Mayr's characterizations of science as involving hypothesization and refutation, he still defends biology as scientific not by appeal to how it involves these things, but instead by appeal to the specialness of its subject matter. He also writes:

A fundamental difference between religion and science, then, is that religion usually consists of a set of dogmas, often 'revealed' dogmas, to which there is no alternative nor much leeway in interpretation. In science by contrast, there is virtually a premium on alternative explanations and a readiness to replace one theory by another. The discovery of an alternative explanatory scheme is often the source of great elation.<sup>4</sup>

Even aside from the historical naivete about the way alternative scientific explanations are greeted, Mayr's

claims about religion embodying dogmas to which there is no alternative suggest that academic biology, with its 'central dogma' from Francis Crick, is more like a religion than a science. Mayr's own hypothesis about the continuity of Darwinian evolution will be shown to be unnecessarily rigid by virtue of its failure to consider certain aspects of evolutionary, nervous system development which suggest that there are such things as systemic mutations after all. This will become apparent below. It is hoped that Professor Mayr would have felt great elation at these new explanatory schemes.

### Early Twentieth Century Biology and Medicine

Lewis Thomas, medical essayist, writing of his years in medical school in the 1930's observes:

"...the purpose of the curriculum was, if anything, even more conservative than thirty years earlier. It was to teach the recognition of disease entities, their classification, their signs, symptoms, and laboratory manifestations, and how to make accurate diagnoses. The treatment of disease was the most minor part of the curriculum, almost left out altogether...Nor do I remember much talk about treating diseases at any time in the four years of medical school except by the surgeons, and most of their discussions dealt with the management of injuries, the drainage or removal of infected organs and tissues, and, to a very limited extent, the excision of cancers...The medicine we were trained to practice was, essentially Osler's medicine..."<sup>5</sup>

"During the third and fourth years of school we also began to learn something that worried us all, although it was not much talked about...it gradually dawned on us that we didn't know much that was really useful, that we could do nothing to change the course of the great majority of the diseases we were so busy analyzing, that medicine, for all its facade as a learned profession, was in real life a profoundly ignorant profession.

Osler's medicine was the medicine of William Osler who, in the 1890's, created the school of 'therapeutic nihilism' at Johns Hopkins University. The school asserted that the first duty of the physician was not to cause any harm, after a two thousand year old dictum of Hippocrates who recognized that intervention by physicians usually resulted in greater problems, and that if left alone the patient frequently recovered anyway. Forty years later Lewis Thomas found that medicine was mostly observing and diagnosing. Of his neurological education in 1938 Lewis Thomas writes:

The making of a neurological diagnosis was itself a kind of game. All you needed to play were three implements, a rubber hammer for eliciting reflexes over tendons and muscles, a pin for testing pain receptors, and a wisp of cotton for testing light touch...

...Neurology had always been an entirely descriptive branch of medicine...there was nothing much to be done for therapy because of so little understanding of how the structures really worked. <sup>6</sup>

In his 1983 essay "Osler's Razor" Sir Peter Medawar, Nobel Laureate in medicine, wrote, "Neurology today is very like medicine in general fifty to a hundred years ago, in its preoccupation with interpretation and diagnosis and the relative backwardness of treatment."<sup>7</sup> Not surprisingly this neurological clinical poverty has not changed much since his essay was written in 1983. The reason for this, as will be seen below, will be attributed to the failure of biology and medical science to update, revise and test theories and explanations in keeping with progress in the hard sciences. For example, as will be discussed below, despite the long-held claim that the nervous system worked electrically, when the

phenomenon of electricity was finally understood well by the physical sciences the biological and neurophysiological sciences retained nineteenth century treatments of it grounded in a misreading of thermodynamic equations rather than electromagnetic ones. Biological and medical scientists did not believe their findings needed updating; they felt that dealing with observational 'facts' which could be supplemented, they did not have to concern themselves with theories which needed revising, despite what Mayr has to say about scientific method involving hypothetico-deductive testing and how this method characterised the biological sciences. It is a well accepted principle of the philosophy of science that there are no theory-independent facts, but the philosophy of science is not an adequate yardstick for the life sciences, according to Mayr. Theory and hypothesization were never a strong suit of biology.

### Early Understanding of Electricity and the Nervous System

William Gilbert coined the term 'electricity' around 1600 from the Greek word for amber, *elektron*. Amber was the substance one would rub with a cloth to generate a small static charge that would crackle when the amber was touched to a fellow party-goer. It was not until 1745 when Von Kleist, bishop of Pomerania, first devised a crude version of what was later known as the Leyden jar which allowed the electrical charge to be stored and accumulated for short periods of time. Just a few years later Benjamin Franklin noted that there appeared to be two types of electrical fluid, which he called positive and negative, and that they would cancel each other out. He also noticed that the charge in atmospheric electricity, lightning, was identical to that produced by rubbing amber. Franklin considered electricity a fluid because it was earlier associated with nervous functioning by John Walsh and John Hunter who dissected the electric organ of a torpedo or 'electric fish' and found that the electricity it produced was like that from a Leyden jar, and that the electricity was intimately associated with the nerve bundles that comprise the electric organ. Walsh and Hunter treated electricity as a fluid because of the still earlier speculations of Descartes who, in another appeal to metaphor, compared nervous functioning to the underground system of pipes used to water the lawns and move the statuary of certain palaces from a central place. It was only natural then that Franklin thought of electricity as a fluid too.

In 1756 Leopoldo Caldani noted that a Leyden jar discharged in the direction of a mounted and dissected frog's leg with the nerve attached caused the leg to twitch. This revelation and the increased number of reports of miraculous healings resulting from the application of electricity to an afflicted person prompted Franklin to investigate the medical efficacy of the use of electrical fluid. The nature of the sort of electricity from a Leyden jar is that, like all static electricity, it is of a high voltage, yet it was the only kind of electricity that Franklin had access to. Consequently his conclusions after some testing on volunteers were that application of electricity to a person for medical purposes produced more discomfort than any good it may have provided, if indeed it provided any relief at all.

In 1780 Luigi Galvani noticed that if the nerves running to the muscle of a frog's dissected leg were touched by a scalpel while a Leyden jar was discharged at a distance, the leg would contract vigorously, just as Caldani had noticed 25 years earlier. But he also noticed that if the leg was hung from an iron balustrade using a copper wire it would often contract without the intervention of a Leyden jar or atmospheric lightning. He surmised that some sort of 'animal electricity' was involved, and in 1791 published a paper on the phenomenon. He was convinced that those recently dead from drowning could possibly be revived using this new discovery, and sought for years, without consequence, to prove it, working in northern Italy where lakes abounded. His nephew, Giovanni Aldini, continued his work in the early nineteenth century, and it was word of Aldini's work which inspired Mary Shelley to write her novel, on a bet instituted by Lord Byron, about Dr. Victor Frankenstein piecing together from cadavers a creature who was then brought to life by lightning discharges.

In 1800 Alessandro Volta, in a paper given to the Royal Society in England, explained that what caused the muscle to twitch was not the intervention of 'animal electricity' but instead was the same electricity produced by chemical interactions involving the metals used for mounting and dissecting. It was in this paper that the use of what became known as 'voltaic piles', later called batteries, to produce electricity of a lower voltage than that available from a Leyden jar, was explained. But it would be another century before the nature of the electrical fluid started to be understood. Voltage was considered the fluid pressure it took to make the electronic fluid move. Volta subsequently combined the fields of chemistry and electricity, and chemical experimenters thereafter sought to isolate elements from solutions using electrolysis and noting what elements went to which pole of the battery. The same year that Volta

announced the voltaic pile Anthony Carlisle and William Nicholson used it to decompose water into hydrogen and oxygen. By so doing the two dramatically demonstrated the significance of electricity for chemistry and introduced the notion of polarity to chemistry.

In the next ten years Humphry Davy, using electrolysis, was able to isolate and identify six new metal elements. In 1812 Davy gave a lecture at the Royal Institution in London at which Michael Faraday was in attendance. Faraday was so impressed that he avidly pursued his own studies into the phenomena of electricity, light, chemistry, and magnetism. Faraday was guided in his work by a fundamental belief in the basic unity of nature, and later, in 1833, formulated the basic laws of electrolysis and created an electric motor using alternating or Faradic current. He had already created the first electrical motor using Galvanic current, the kind of current from a battery or Leyden jar, in 1821. But in 1833 he was applying theories of electromagnetic induction and speculation about 'lines of force' about a magnet he had detected in 1831 while building on the 1820 discovery of Hans Oersted who had noticed the magnetic effect of a Galvanic current passing near the needle of a compass.

Faraday's work with electromagnetism and 'lines of force' led later in the nineteenth century to the more mathematical field theory of Clerk Maxwell. Faraday was a poor mathematician, but his grasp of things that Newton's classical mechanics could not even begin to account for began to add a new field to the physical sciences. In the meantime, however, the chemists, using the emphasis on structure and Newtonian rigid bodies, began to envision atomic structures, even though the atom was still considered a hypothetical entity. John Dalton had already postulated that atoms differed from one another by mass in 1800 to account for how the same proportion of elements could always be found in certain chemical compounds. Building on this, in 1828, Jon Jakob Berzelius published a list of atomic masses, but his work went ignored for the most part.

In the mid 1860's Johannes Wislicenus, a German chemist, introduced a three dimensional concept of organic molecules on the basis of analysis of the isomers of lactic acid which behaved differently with regard to polarized light used spectroscopically. Wislicenus thereby began the combination of the fields of chemistry and mechanism, although at the time such things as atoms and molecules were still thought of by most chemists as too hypothetical for such a combination. Spectroscopy was the 1859 discovery of Gustav Kirchoff and Robert Bunsen, and was used to study elements from the light they emitted or absorbed upon heating. At about the time of Wislicenus's notion of three-dimensional organic molecules, 1865, another chemist, Friederich August Kekule, as the result of a dream, arrived at the structural formula of the benzene ring and established that molecules could form rings and other complex shapes. In 1868 Karl Graebe, another German chemist, discovered the molecular structure of alizarin, a dye, advancing the study of molecular structure based upon chemical and spectroscopic activity. The next year Richard Abegg, still another German chemist, began to espouse that the chemical properties of atoms were due to some quality of electrical charge in their surfaces. The same year two chemists, Dimitri Mendeleev and Julius Meyer, published versions of a periodic table of the elements based upon atomic masses. Because Mendeleev also predicted the existence of yet undiscovered elements on the basis of his version in 1871, he was credited with being the originator of the periodic table of the elements.

It was at this time that thinking about atomic and subatomic particles began to reify those entities, and this introduced controversy amongst the chemists. When Jacob H. van't Hoff published his revolutionary ideas on the asymmetrical carbon atom in 1874, he was ridiculed by the great German organic chemist Herman Kolbe, "...in part because he was only a member 'of the Veterinary School of Utrecht.'"

"...the opposition to van't Hoff's ideas arose also from the fact that he had written of atoms and molecules as if they had physical reality, in direct opposition to the views of most organic chemists, who were willing to use the concept of atom and molecule but were skeptical as to their actuality. Today van't Hoff's revolutionary ideas on the asymmetrical carbon atom are considered to be at the foundation of stereochemistry."<sup>8</sup>

During the 1870's Hermann von Helmholtz wondered about the possibility of atoms of electricity, and announced in 1881 that electric charges in atoms were divided into definite, integral portions, suggesting that there is a smallest unit of electricity. He wrote:

The most startling result of Faraday's law [of electrolysis] is perhaps this. If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also is divided into elementary portions which behave like atoms of electricity.

English physicist George Stoney named the hypothesized particle the electron from the term *elektron* which, as noted above, was the Greek word for amber, after having coined it in 1874 to stand for his estimate of the unit of charge of the particle that he and Hemholtz considered the atom of electricity. In 1875 Clerk Maxwell noted that atoms have a structure far more complex than rigid bodies, the rigid bodies of Newtonian classical mechanics. Almost twenty years later J.J.Thomson discovered the electron a year after Heinrich Lorentz calculated the theoretical mass/charge ratio of an electron by studying the deflection of cathode rays in a magnetic field. 9 This discovery took place at the same time that Emil Wiechert, a German physicist, predicted that there existed particles in atoms between 2000 and 4000 times lighter than the hydrogen atom, although these were not yet known to be the same as the particles found in cathode rays. Thomson did not, however, accurately measure the charge of the electron until 1899.

Until the very last years of the nineteenth century, most if not all physicists who believed in the reality of atoms shared Maxwell's view that these particles remain unbroken and unworn...It is true that many of these same physicists (Maxwell among them) were convinced that something had to rattle inside the atom in order to explain atomic spectra. Therefore, while there was a need for a picture of the atom as a body with structure, this did not mean (so it seemed) that one could take the atom apart. However, in 1899, two years after his discovery of the electron, Joseph John Thomson announced that the atom had been split: 'Electrification [that is, ionization] essentially involves the splitting of the atom, a part of the mass of the atom getting free and becoming detached from the original atom.10

In 1898 Thomson speculated that the position of the electron in the atom, the hypothesized source of electrons which were not yet associated with cathode rays, was like that of a negatively charged raisin in a positively charged pudding. In 1904 Japanese physicist Hantaro Nagaoka proffered the idea that electrons in an atom circled it like the rings around Saturn. This metaphor was expanded upon by Ernest Rutherford and Niels Bohr after Rutherford's experiments in 1911 which dealt with the scattering of alpha particle radiation when an atom was bombarded with x-rays. In their model of the electron Bohr and Rutherford spoke of them as more like the planets orbiting the sun. Amongst theoretical physicists it became more widely accepted that electrons were in fact parts of atoms although even as late as 1924 physical chemist Wilhelm Roentgen, the discoverer of the x-ray, was telling his students to dismiss speculation about the nature and existence of such particles. In 1914 Henry Moseley showed that elements had a definite number of protons and orbiting electrons, and that this atomic number and not atomic mass was the reason for the periodicity noticed by the chemists. Twenty years later Linus Pauling developed his theory of chemical bonding which explained the role of electrons in the formation of molecules - atoms could be bound together either by electrostatic forces (ionic bonding) or by sharing electrons (covalent bonding).

There was still conflict in the community of theoretical physicists with regard to the nature of the electron, its particle/wave duality, and quantum indeterminacy. These problems were to remain unresolved until the development of quantum electrodynamics beginning in the late 1940's for which Richard Feynman, Julian Schwinger, and Shin'ichiro Tomonaga were to receive a Nobel prize in 1965. Albert Einstein would go to his grave insisting that there was something incomplete about quantum mechanics because, in his classical *weltanschauung*, god did not play dice, and quantum mechanics stressed the role of probability instead of causality.

The metaphor of the electron as a particle orbiting a nucleus as a planet around a sun was adequate enough for the biologists and neurophysiologists who, in the first part of the 20th century, did not see the relevance of theoretical physics for biology. It took the physicists until the end of the first quarter of the 20th century to show its connection with the more empirical field of chemistry. Despite its role in chemical bonding, the electron is still regarded by biologists and neurophysiologists as having no pertinence to those fields of study except for the instrumentation made possible through the harnessing of the electrical influences. The planetary metaphor was adequate for the electrical engineering development of the transistor, the radio and television. Further refinements which spoke of quantum indeterminacy and 'quantum spookiness' were not considered pertinent to the understanding of matters biological since a whole different science was involved.

Speculation about the electrical nature of nerve impulses proceeded almost fully uninformed of the growing understanding of the nature of electricity and the electron. The 'facts' of nervous functioning had their origins in the 17th century with Rene Descartes, and, as will be seen below, the edifice of neurophysiological authority was pieced together without ever a theory being hypothesized and tested, but instead being propounded and institutionalized on the basis of authority figures like those cited above whom Ernst Mayr says contributed so much to our world view. A thermodynamic model of electricity was retained by the neurophysiologists who only used the measuring instrumentation developed over the next 50 years to buttress the model, while they postured as scientific because of the instrumentation they used to study nature and accumulate data.

### Electrophysiology and Electrotherapy

The idea of living things as machines dates from at least the time of Descartes in the seventeenth century, and is continued today both with respect to the functioning of the body and to the functioning of the cells of which it is comprised, with cells being spoken of as protein factories which embody mechanisms of functioning. Like a good Cartesian, Charles Bell, in 1811, published *New Anatomy of the Brain* which discussed the difference between motor and sensory nerves. Although the hypothesis that nature was so prodigal as to create separate nerves for sensory and motor functions seems a bit farfetched now, the idea, later known as the Bell-Magendie Law, was based upon vivisectionist experiments on dogs, and is still taught today as fact. It is seen in the claim that although nerves in the laboratory seem to conduct in both directions, in nature they are deemed to be unidirectional only. This is a graphic example of how preconceptions can influence interpretations of data, of observations. It is this dynamic which the philosophers of science seek to address when they insist that there are no such things as theory-independent facts. But, as we have seen from Mayr's *The Growth of Biological Thought*, the philosophers of science use an inappropriate yardstick to demand such rigor from the life sciences

In 1829 appeared James Mill's *Analysis of the Phenomena of the Human Mind* which tried to show that the mind was nothing more than a machine answering to the stimulus/response model of nerve functioning advocated by Descartes. The mechanism of Mill was further elaborated upon in 1834 in Johannes Peter Muller's *Handbuch der Physiologie des Menschen* which recapitulated a mechanistic explanation of thinking. It should be recalled that by this time, as discussed above, the nerves were thought to function electrically, that electricity was a fluid, and that therefore the dynamics of fluid flow were an important consideration in understanding nervous system functioning.

Using recently improved microscopy Robert Brown, in 1831, discovered the cell nucleus, and this was followed by the observation of Jan Purkinje in 1835 that animal tissues and plant tissues were made of cells. In 1836 Heinrich Wilhelm Gottfried von Waldeyer-Hartz noted that the nervous system too was made up of cells, and Theodor Schwann laid the foundations of cell biology soon thereafter. These observations and notations were not unified by any sort of hypothesis of organization and functioning because so little was known about such issues that a vague mechanism had to suffice. But in 1843 Emil Heinrich du Bois-Reymond demonstrated that electricity was used by the nervous system to communicate between different parts of the body by touching an electrode to a nerve and making a muscle jump. Although this was similar to what Galvani had done sixty years earlier, this time it was in a living creature, and once again sparked speculation about a vital force and animal electricity. This was followed in 1849 by Rudolph von Kolliker's finding that the nerve fibers electrified by Bois-Reymond were the extension of nerve cells.

In 1851 Claude Bernard noted that when the sympathetic nerve to the ear of a rabbit was cut it led to the warming of the ear and noticeable vasodilation. The next year another French physiologist, Charles

Edouard Brown-Sequard reported in an American medical journal:

...if galvanism is applied to the upper portion of the sympathetic nerve trunk after it has been cut in the neck (of a rabbit), the vessels of the face and the ear after a time begin to contract [after the initial dilation, noticed by Bernard a year earlier]; their contraction increases slowly, but at last it is evident that they resume their normal condition, if they are not even smaller. Then the temperature and the sensibility diminish in the face and the ear, and they become in the palsied side the same as in the sound side. When the galvanic current ceases to act, the vessels begin to dilate again, and all the phenomena discovered by Dr. Bernard reappear.<sup>11</sup>

What Brown-Sequard was announcing was that with a Bois-Reymond type of electrical stimulation of the nerve fiber, and without any attention paid to the polarity of the electrode touched to that fiber, vasoconstriction took place which reversed the vasodilation that Bernard noticed when the rabbit's sympathetic nerve was cut. This was taken as evidence that vasoconstriction was triggered by nervous functioning.<sup>12</sup> Neither Bernard or Brown-Sequard had any idea of the role of polarity in galvanic current even though it had been known amongst the physical chemists shortly after Volta's announcement of voltaic piles in 1800. In fact in 1865 in his *Introduction a l'etude de la medicine experimentale* Claude Bernard declared that living systems followed the same laws as inanimate systems, and denied that there was a 'vital force' involved. Bernard, Brown-Sequard, Bois-Reymond and the French physiologists seem to have known nothing about what had been found in the physical sciences in the previous years. Bernard's declarations about inanimate and animate systems following the same laws was just a philosophical prejudice, called 'naturalism', which was thought of as biologically scientific because it was Darwinian, and scientific because it encouraged further investigation of natural phenomena in a non-metaphysical sort of way.

Ten years before Bernard's naturalistic posturing, in 1855, his fellow countryman, Guillaume Duchenne, for whom Duchenne's muscular dystrophy is named, and the father of electrotherapy, pronounced Faradic or alternating current superior to Galvanic or direct current for electrotherapeutic purposes. Although the difference between the two types of current will be covered later in this paper in terms of electron behavior, Duchenne had no way of knowing it at the time he made his proclamation. The story propagated by Sydney Licht in his "History of Therapeutic Electricity" is that Duchenne preferred AC to DC because of the undesirable 'warming effect' of DC on the body.<sup>13</sup> The problem with this explanation is that it is too superficial, and in its superficiality it fails to account for a number of easily noticeable phenomena that Duchenne should have observed if he had been worthy of the name father of electrotherapy.

Like Bernard and Brown-Sequard, Duchenne too lacked awareness of the notion of polarity despite its importance to the physical chemists, and attached no significance to two important phenomena. First, with muscle contractions triggered by DC, the contractions would only come with the start of the current, after which it had to be pulsed for the muscle to keep contracting. Second, if allowed to run for more than a minute at a site and at a strength great enough to trigger muscle contraction, blistering occurred at the anode and pitting at the cathode. With AC neither of these 'problems' occurred. The current could be allowed to flow for hours, pumping out those all important muscle contractions, and making work easier for the electrotherapist while doing no harm to the patient's skin. Because the switching equipment was not available to pulse the DC, it would take a telegrapher's skill to repeatedly make a muscle contract using DC. So AC was preferred because of its ease for the therapist, not for any benefit it bestowed on the patient. Indeed, for a century and a half after Duchenne's pronouncement stimulation with AC never had any effect on the patient whatsoever except to cause discomfort during therapy sessions.

The ignorance of Duchenne and the other French physiologists was compounded by the contribution in 1870 of two German physiologists, Eduard Hitzig and Gustav Theodor Fritsch. Professor Patricia Churchland writes of this development, "The first well-designed and significant studies on the nervous system using electrical stimulation were undertaken by two German physiologists...and the method has been of enormous and enduring importance"<sup>14</sup> What these two did, not yet knowing about the

electronic nature of AD and DC, was codify, institutionalize, and give a scientific patina to the ignorance of the French physiologists, based upon laboratorial studies. This ignorance was indeed enduring. Over a century later Dr.'s Walthard and Tchicaloff, in the essay "Motorpoints" in *Electrodiagnosis and Electromyography*, say that AC and DC are interchangeable in locating what they define as motorpoints which are what is presently known as neuromuscular junctions or motor endplate regions. It turns out that not only do the points vary in location and number from one current to the other, but also such points cannot be found at all using AC on severely atrophic muscle. Because biological knowledge increases as a result of accumulation rather than revision, no one ever goes back to check these things once new knowledge becomes available. Instead a new veneer of data is added to the old. Even today there are simultaneous claims in medical literature that electrical stimulation can build muscle and that it can't. One thing is certain, however, no professional athlete or body builder uses it.

In 1991 Bertil Hille, in *his Ionic Channels of Excitable Membrane*, writes, "In much electrophysiological work, current is applied as a stimulus and the ensuing changes in potential are measured." Hille does not say whether the stimulating current is AC or DC, but it is almost always AC because 'changes in potential' addresses voltages not amperages, and AC is defined as a property of voltages, one in which the voltage changes. Medical electricity is almost exclusively concerned with voltage wave forms, and speaks of the AC/DC distinction only in terms of these forms, with the former being biphasic current while the latter is monophasic. Hille also writes, "Cells that can make action potentials can always be stimulated by an electrical shock," yet says nothing about what type of electricity is used to provide this shock. The misdirection from amperage to voltage is an outcome of the pre-twentieth century ignorance of electron behavior of men like Hitzig and Fritsch which was then perpetuated when the instrumentation was developed allowing cellular membrane voltages to be measured and subsequently interpreted to support nineteenth century thermodynamic explanations of electricity. This will be discussed below in particular with respect to a model of nerve impulse propagation awarded a Nobel Prize in 1963. If electricity is considered a fluid, then fluid pressure is voltage, while the rate of flow of the fluid itself is amperage. Just as the nature of water cannot be fathomed by studying the water pressure in plumbing, so the nature of the nerve message cannot be understood merely by studying the voltage changes as an impulse passes. Yet this is what modern neuroscience asks us to believe.

In 1909 Abraham Flexner, in the employ of the Rockefellers, visited the various schools of medicine around the United States and Canada, and in *Bulletin Number Four* issued a call for the standardization of medical education. This call was motivated not so much by a desire to advance scientific knowledge and teaching in the practice of medicine, but instead by tactical considerations for the appeal to state legislatures to pass certification requirements for the profession of medicine so that those who had not given their pound of flesh to certified teaching institutions would be prevented from calling themselves physicians in the lucrative health care market. Critics of organized medicine, such as Paul Starr, still accept myths that "Undoubtedly the most influential explanation for the structure of American medicine gives primary emphasis to scientific and technological change and specifically attributes the rise of medical authority to the improved competence of physicians."<sup>15</sup> The trouble was, as seen above in the case of Lewis Thomas and the other medical students at Columbia thirty years after Flexner's bulletin, medicine was still, in the tradition of Oslerian therapeutic nihilism, an ignorant profession that could do little for treatment. The rise of medical authority was not due to improved competence, it was due to political organization. This was not surprising given that the Oslerians would do nothing except that which was seen to 'work,' which would not do harm. And this was a recipe for stagnation, as Lewis Thomas found out at Columbia.

Flexner sought also to refine and advance medical education, combining clinic and laboratory; but from a reading of his autobiography, *I Remember*<sup>16</sup>, one finds that he is aware principally of the profitability of medical schools, and the benefits accruing to those who were certified. So that the push for certification laws could proceed with a show of unanimity amongst the 'educated' for a law establishing certification, Osler's Johns-Hopkins University and Harvard were to become the templates. Flexner, who later toured German, French and English medical schools and teaching hospitals, wrote, "The French had indeed something to teach, namely the importance of opening the hospital wards freely to medical students, but we had more to learn from the German method of building up medical education in an orderly fashion..."<sup>17</sup> Flexner was impressed with the enforcement of entrance requirements, the fact that teachers were professors and practicing physicians, and that 'research was held in high and proper esteem.' The

French, with greater emphasis on the clinic, and in the tradition of electrotherapy following from Duchenne, the father of electrotherapy, demonstrated the worthlessness of electrotherapy as a subject to be taught in American medical schools. In the meantime neurophysiological research in the laboratory done by those who followed the methods of Hitzig and Fritsch remained without clinical consequence because of a deep and enduring ignorance as to the nature of electricity.

It was already figured that if the nerve cells were able to operate by electricity, then a potential must be detectible across the membrane. *In Principles of Neural Science* John Koester writes:

In 1902 Julius Bernstein used the Nernst equation as the theoretical framework on which to develop the hypothesis that the resting potential of neurons is based on the selective permeability of the membrane to K<sup>+</sup> [potassium ions]. Bernstein's idea could not be tested quantitatively until the 1940's, when techniques for intracellular recording were developed, and voltage potentials were found across the membranes of cells.<sup>18</sup>

What Koester implies is that Bernstein was developing a theoretical framework so that an hypothesis could be generated that was or would some day be testable. Bernstein was not developing a theoretical framework as much as speculating as to what might be expected to be found, i.e., measurable voltages across a cell wall, and attributing these voltages to an ion concentration gradient. In so doing he was misinterpreting what the Nernst equation was about. It was not an electrical equation. It was a thermodynamic equation. The speculative explanation was considered verified *in toto* by the detection of cellular voltages in the 1940's by one school of thought that became dominant during the 1950's, and whose foundation figures received a Nobel Prize in 1963. But the discovery of these voltages did not verify that the reason for them was an ion concentration gradient. This will be discussed at length below in a section that deals more extensively with the philosophy of science, the difference between verification and refutation, and the hypothetico-deductive weakness of the reliance upon verification.

The life sciences, as has been said earlier, seldom if ever concerned themselves with the development of testable hypotheses, despite the insistence of Ernst Mayr that this is a big part of science and that biology is now and has been for some time scientific. In 1898 John J. Abel at Johns-Hopkins University isolated adrenalin or epinephrine from the extract of adrenal gland. The existence of some substance in adrenal extract with important properties for the bodies functioning was the discovery in 1895 of George Oliver and E.A.Schaefer. The growth of biological science was a cumulative growth, not one involving theoretical revision or the testing of hypotheses.<sup>19</sup> In 1901 J.N.Langley, a noted English physiologist, observed the results of the extract of adrenal gland on organs innervated by the (post-synaptic) sympathetic nerve. Langley noted the speeding up of the heart and vasoconstriction, so he figured the sympathetic nervous system functioned with epinephrine somehow since these effects were associated with the sympathetic nervous system. A student of his, T.R.Elliott, did the same thing with pure adrenalin and started to theorize about 'neurohumors' or neurotransmitters. "Langley, who disliked theories of any kind, discouraged further speculation by Elliott until more facts were available."<sup>20</sup>

Consequently, a quarter of a century later in 1926 when a Dr. Plank in his *Actinotherapy and Allied Physical Therapy*, in a brief section on direct current therapy, noted the effects of polarity on neurotransmitter secretion and associated them with sympathetic and parasympathetic functioning, there was no theoretical structure with which to apprehend the full impact of his revelations not just for electrotherapy but also the antagonistic, fight/flight model of nervous functioning. Instead the idea that the sympathetic nervous system was responsible for vasoconstriction was perpetuated despite even the attempt at the Mayo Clinic in 1925 to treat hypertension through what was then called a sympathectomy by surgeons Leonard Rowntree and Alfred Adson.<sup>21</sup> This attempt sought to show that by cutting nerve thought responsible for vasoconstriction such constriction could be countered. The results of this surgery were not supportive of the idea since, after a brief period measured in weeks, hypertension returned. The theory went untouched; there was no testing or refutation of any hypothesis involved. What was sought was verification. And not getting it did not threaten the theory since its truth was already widely accepted.<sup>22</sup>

In 1903 a device known as the string galvanometer was developed by a Dutchman named Wilhelm Einthoven. Einthoven was later to receive a Nobel for physiology in 1924 for the creation of

electrocardiography, a practice made possible by his device. His device detected minute 'currents' by detecting the movement of electrical charge through a magnetic field. At this time little was known about the electron and its position in the atom, or that cathode rays were streams of electrons, and Einstein's proof of the reality of atoms and molecules through the analysis of Brownian motion had not yet appeared. Einthoven and all the neurophysiologists after him until the present day, thought that these currents were due to the movement of ions, that there was such a thing as an ion current or molecular electricity that answered to fluid dynamics. The notion had astounding longevity and is still considered current in neuroscientific circles. Noted neuroscientist Bertil Hille writes in his 1991 *Ionic Channels of Excitable Membrane*:

Electrical phenomena arise whenever charges of opposite sign are separated or can move independently. Any net flow of charges is called a CURRENT, and ionic fluxes are electric currents...23

In an atmosphere of resistance to hypothetico-deductivism in 1906 Sir Charles Sherrington published his *The Integrative Action of the Nervous System*, and "...the study divided the study of the nervous system into three regions - the mechanical level, the level at which thought occurs, and the mind-body level; this influential work [had] its fifth edition in 1947." 24 Sir Charles Sherrington is considered the father of modern neurophysiology; at the time of his book theoretical physics was still in its early stages with regard to electromagnetism. Sherrington's work perpetuated the mechanistic approach to biology and electricity, institutionalizing it so thoroughly that its metaphors are still used, with cellular biologists looking for mechanisms and speaking of cells as protein factories.

Two years after the first appearance of Sherrington's book the term *ampere* was universally accepted by the scientific community in 1908. The neuroscientific community thought this had no relevance to them, this understanding of the nature of the electrical fluid. Ion currents are never measured in these terms, not because there is no instrumentation to measure them, but because ions are not electrons, and ion fluxes are not electricity. Below will be seen the claim by the neurophysiologists that not only are ion fluxes analogous to electricity, but so analogous that the equations of electricity can be applied to a fluid dynamics, the fluid being the 'waters of hydration' that carry the ions through 'voltage gated' ion channels.

In 1937 John Zachary Young noted that nerve impulses traveled faster on myelinated nerve than unmyelinated nerve, and sought an explanation for this. In keeping with prejudices from Julius Bernstein about ions and bioelectricity, this scheme involving electrical mechanisms was invoked which was then supplemented the next decade once the technology was developed to allow cellular membrane voltages to be recorded and nerve conduction velocities to be studied more closely. Lewis P. Rowland, in his "Diseases of the Motor Unit," writes that speed of nerve impulses is influenced by two things. "First, the axons of myelinated fibers tend to be larger in diameter, and there is a direct relationship between conduction velocity and axon diameter."25 But with electricity the increased conductor cross-sectional area does not result in speeded up movement of electrons, just decreased resistance to their movement, and so greater current flow at the same speed. As will be seen below, attempts to account for this increased speed of nerve impulses on myelinated nerves invoke the existence of RC time circuits and capacitive discharge, with increased axon cross-sectional area being said to be behind a smaller value for R. This resort to the equations of electricity, especially those concerned with resistance and capacitance, suggest that neurophysiologists really believe that ions can function capacitatively, a preposterous claim that suggests a profound misunderstanding of the nature of electricity. This will be discussed below.

Neuroscience never considered anything but voltages in its models of functioning, never amperages, because voltages were easily measurable and amperages were not supported by 19th century models of thermodynamic/fluid dynamic treatments of electricity based upon Bernstein's hypothesis about the Nernst equation. The detection of cell membrane voltages in the 1940s was taken as verification that these same voltages were due to ion concentration gradients since the man who joined the two, Julius Bernstein, hypothesized they were related. This hypothesized relation was never tested, but deemed verified by the discovery of cell voltages. In the next section will be presented a detailed discussion of this state of affairs with regard to the philosophy of science and hypothetico-deductivism, things which Mayr, as cited above, says are applicable to biological science in a limited and unrigorous way.

#### The Nernst Equation, and the History and Philosophy of Science

As seen above, the accepted explanation for hypothesized cellular membrane voltages was the Nernst equation. Soon after John Zachary Young noted the difference in speeds of nerve impulses on myelinated and unmyelinated nerve fibers the recording equipment for detecting any cellular voltages was developed. With Einthoven's equipment a nerve impulse was detected as movement of electrical charge, and this movement was attributed to the moving of an ion since electrons were still extremely theoretical. With the development in the 1940's of the equipment to actually measure cellular voltages, and the voltage change as the nerve impulse passed, problems immediately occurred. The Nernst equation did not deal with solutions having a concentration gradient and flow; the concentrations were at rest, or separated, or even non-existent and hypothetical. In fact the equation did not deal at all with electricity or measurement; it was strictly mathematical. When the equation was devised by Walter Nernst in 1888 the electron had not been discovered. What Nernst was trying to do was devise a way to calculate entropic pressure between two different solutions, the pressure which would cause two different solutions to mingle if brought together. Voltage was a measure of pressure; electricity was thought of as a fluid; and, following the work of Svante Arrhenius in 1884, it was believed that electrolytic salts, when placed in solution, generated electrical charges. But even Nernst would assert he was not concerned with electricity but instead with thermodynamics, and that what was being figured was not an electrical potential but instead an entropic pressure. Later it was found that the salts when placed in solution did not generate electrical charge after all, but instead the charge was something that was carried by the individual atoms that made up the salt. With the exception of the term 'voltage', there is not a single electrical term in the Nernst equation.

Nevertheless the Nernst equation was seized upon by the early neurophysiologists because this meant an entry into the world of the hard sciences with the use of mathematics and equations. Hille writes, "Nernst's (1888) work with electrical potentials arising from the diffusion of electrolytes in solution inspired numerous speculations of an ionic origin of bioelectric potentials." 26 The trouble was Nernst was not concerned with electrical potentials, but with entropic potentials. The diffusion of electrolytes in a solution into which the electrodes of a battery have been immersed takes place over a very narrow distance, less than twenty angstroms [one angstrom equals  $10^{-10}$  meters]. Nernst wasn't talking about batteries or electrochemistry; he was talking about the mingling of two concentrations of ionic or molecular solutions. Misreading of Nernst was an early twentieth century attempt to revive the idea of animal electricity from Galvani, only now appealing to equations transmogrified from the context in which they were developed and to which they applied. Almost a century later John Koester, in his essay "Membrane Potential", writes: "The electrostatic attraction between the excess cations on the outside of the membrane and the excess anions on the inner surface generates a thin cloud of positive charges on the exterior surface of the membrane and an equal density of negative charges on the interior surface." 27 Even as late as 1991 it is widely believed in the neurophysiological community that ions in solution generate a charge above and beyond that which they carry. This is more than amazing. This is scandalous.

It is important now to make a side trip into the world of the philosophy of science in order to understand the subtle nature of the deviation of the natural sciences from the life sciences and in particular the neurosciences that occurred in the first half of the 20th century. This understanding will allow the reader to grasp the dependence of neuroscience on the wrong-headed interpretation of voltage measurements to retain the grasp on an outmoded model of electricity in the attempt to legitimate claims to scientificity which are not in any way otherwise justifiable.

Niels Bohr, responsible more than anyone else for the move from atomic to nuclear physics and who created the foundations for the theoretical understanding of the periodic table of the elements, believed that quantum mechanics did not reflect a new, fundamental reality, but instead was a theoretical construct which allowed for certain statements to be made about nature. Through what he called the 'correspondence principal' he sought to establish links between the predictions of classical physics and the expectations of quantum mechanics. In the United States the positions of Niels Bohr and the logical positivists of the Vienna Circle were championed by Percy Bridgman. It was Bohr who insisted that science is not concerned so much with the determination of reality, but instead with what could be said about it. This meant hypothetico-deductive logic was very important since it dealt with the relations of statements to each other in a theory, and the meaning of the terms in the statements. Bohr redefined the term 'phenomenon' to necessarily include the observer and the instruments he was using as well as the theories of operation that these instruments embodied. "In the version developed by the physicist-philosopher Percy Bridgman (1927), these steps [to verify the existence of the events or relationships to

which the statements refer] were given the name 'operations', and the meaning of any term with an empirical referent was held to be identical with the operations by which the existence of the event could be established by independent observers." 28 "...Bridgman's widely read *Logic of Modern Physics* (Princeton, 1936) represents the theory of relativity as the transition to a new era where considerations of objective existence are replaced by considerations of measurability..."29 Bridgman relegated the quantum mechanical hypotheses to the world of what he called 'operationalism.' In this world the retention of old definitions of terms was justified after many years of 'corroboration' and 'verification' had established the factual status of the formerly hypothesized entities, and all future formulations had to be compatible with the observational data so far gathered, and the entities reified. "The very task of specifying the operational steps that should be taken to reach agreement about whether or not a complex structure exists cannot be carried out without relying on at least some terms whose meanings derive from natural language and whose validity is a matter of practical experience within a given scientific community."30

The neurophysiologists then, in order to adopt the logic of modern physics and be scientific in their handling of the newly measured cellular membrane voltages, were compelled to account for them in terms of the long accepted hypothesis of Bernstein for cellular voltages which fell back on a misinterpretation of what Walter Nernst was talking about, and a nineteenth century treatment of electricity in which, as Koester writes, "By convention, the direction of current flow is defined as the direction of net movement of positive charge."31 The volts of Nernst, entirely thermodynamic in nature, were taken to be the same as the volts of the electro-physicists. But electricity operates by the movement of electrons and 'holes' (as in semiconduction), not the movement of positive charge. That in an electrolytic/electrochemical cell the movement of cations from diffusion is toward the anode of the battery, and in the battery the electrons move from the cathode to the anode, does not mean that the movement of cations in an electrolytic cell is the same or equivalent to the movement of electrons in a battery going from cathode to anode. Yet this is what neuroscientific treatments of electricity envisioned, along with the idea that there is no battery causing the movement of the cations in the electrolytic cell in the first place, that their movement is the result of a Nernst entropic voltage acting on a concentration gradient. Nernst's use of the term 'voltage' was somehow taken to be an electrical voltage.

Bridgman's operationalism, with its stress on the retention of accepted definitions of terms whose entities named have been given factual status, played into the hands of the neurophysiologists who felt that 'volt' merited this status. Furthermore, by carefully selecting archaic, fluid dynamic definitions of electricity, neurophysiologists found it easy to imagine such things as ionic currents. This tendency, to retain the definitions of terms that were acceptable in early stages of a study or science, is called 'meaning invariance'. Paul Feyerabend writes:

...any form of meaning invariance is bound to lead to difficulties when the task arises either of giving a proper account of the growth of knowledge, and of discoveries contributing to this growth, or of establishing correlations between entities which are described with the help of what we will later call incommensurable concepts. ...it will usually turn out that a solution of these problems is deemed satisfactory only if it leaves unchanged the meanings of certain key terms and it is exactly this condition, the condition of meaning invariance, which makes them insoluble.32

F.S.C. Northrop comments on the phenomenon as follows: "One of the basic problems in the unification of scientific knowledge is that of clarifying the relation between those concepts which a given science uses in the early natural history stage of its development and those which enter into its final and more theoretical formulations as a verified deductive theory." 33 Through meaning invariance the neurophysiologists were misled into treating the volts of Nernst as those of the electro-physicists, and thereby into perpetuating the flawed notion that electricity, especially bioelectricity, could include the movement of positive charges borne by atoms and molecules. The analogy of molecular electricity of the neurophysiologists was taken by them to be so compelling that it was invoked by the ionic channel school of nerve impulse propagation of Eccles *et al.* in 1953 and said to involve a sodium ion 'pump.' This hypothesis was awarded a Nobel in 1963 to Eccles and his colleagues in physiology and medicine, and

again in 1978, only this time in chemistry, to Peter Mitchell for his study of the biological transfer and use of 'energy,' with that transfer involving the functioning of a 'proton pump.'

Operationalism became known as instrumentalism, though instrumentalism held too that with a change in the ontological view of what was being measured, there was needed an accompanying revision or reinterpretation of all earlier measurements. Paul Feyerabend writes: "Taken by themselves the indications of instruments do not mean anything unless we possess a theory which teaches us what situations we are to expect in the world, and which guarantees that there exists a reliable correlation between the indications of the instrument and such a particular situation." As has been seen, however, revision and reinterpretation were not characteristic of a science built on the accumulation of facts, as neurophysiology was.

Hille writes: "Much of what we know about ionic channels was deduced from electrical measurements...as this book is concerned with channels and not with techniques of measurements, the essential principles are few." 34 In other words much of what Hille and his apostles know about ionic channels was 'deduced' from measurements on an obsolescent model of electricity. As a result of this flawed basis techniques of measurement were arbitrarily changed so that the readings from which the deductions were made would be in agreement with the base hypothesis from Bernstein. Otherwise there would be a pronounced discrepancy between theory and experimental result. The mathematical sign of the voltage measured would be the opposite from that calculated from Nernst. The answer, as Hille admits, is to locate the voltmeter ground externally to the cell so the sign will be right. Yet when measuring the voltage across the membrane of an electrolytic cell, the ground should be placed inside the cell, not outside. This location of the ground extracellularly was an arbitrary deviation from the methodology of the physical sciences. Hille justifies it by arguing that although the cell is like a battery (in which case the ground should be located not only intracellularly, but within the nucleus), it is actually an electrolytic cell rather than a primary cell (a battery). This justification immediately suggests that Hille does not understand electrochemistry.

Furthermore, Hille writes:

...Julius Bernstein correctly (sic) proposed that excitable cells are surrounded by a membrane selectively permeable to  $K^+$  ions at rest and that during excitation the membrane permeability to other ions increases. His 'membrane hypothesis' explained the resting potential of nerve and muscle as a diffusion potential set up by the tendency of positively charged ions to diffuse from their high concentrations in cytoplasm to their low concentration in the extracellular solution. .35

That a diffusion potential is something which comes from a battery and acts within a few angstroms of the electrode in solution suggests that Hille is not talking about electrochemistry here when he speaks of diffusion. That a neuroscientific 'diffusion potential [is] set up by the tendency of positively charged ions to diffuse' from a high concentration to a low one does not render such 'diffusion' an electrical or electrochemical diffusion. For such diffusion characterizes all atoms and molecules in solutions of varying concentrations. There is nothing electrical here. And from this nothing neurophysiology has made deductions that have withstood the test of time, never once being applicable to the clinical world.

In an attempt to gain access to the equations of electricity Nernst had to be convertible to Ohm's law,  $V=IR$ . Hille writes, "Cole and Curtis (1939) correctly (sic) deduced that if conductance is 'a measure of the ion permeable aspect of the membrane' and capacitance, of the 'ion impermeable' aspect, then the change on excitation must be very 'delicate' if it occurs uniformly throughout the membrane, or, alternatively, if the change is drastic it 'must be confined to a very small membrane area' [the Ranvier node]." This was an attempt to justify resort to what is called the RC time constant where R is resistance and C is capacitance, in order to justify the idea that the difference in speed of nerve impulses on myelinated nerve was faster than that on unmyelinated nerve because the myelin wrapping resulted in a form of biological, capacitative discharge or, as Lewis P. Rowland in his "Diseases of the Motor Unit" prefers to call it, *discontinuous propagation*, the second reason Rowland says nerve impulses on myelinated fibers are faster than on unmyelinated. 36

Curtis and Cole write, "...we shall assume that the membrane resistance and E.M.F. [electromotive force] are so intimately related that they should be considered as series elements in the hypothetical equivalent membrane circuit," but in honesty they qualify their resort to this membrane circuitry and its arrogation of RC time factors by saying that this is an assumption, a hypothetical. 37 It was a hypothesis uncritically accepted. In the hypothetical equivalent membrane circuit, depending upon whose version, permeability is conductance, and impermeability is either resistance or, as Hille states above, capacitance. In the equivalent membrane circuit discussion below impermeability is treated as both, and a further resistance is conjured for the longitudinal movement of recorded voltage changes. The biological membrane is electrically intricate, it seems, if it is to fit the model.

In 1980 in a *Scientific American* article Pierre Morell and William T. Norton report in their essay "Myelin" in all seriousness that "...the mechanism by which Myelin facilitates conduction has no exact analogy in electrical circuitry," 38 this despite that fact that the equations of electrical circuitry are resorted to mathematically to 'prove' that myelin facilitates conduction in the very way for which there is no exact analogy. As usual for biology, the metaphor went untested, and was accepted as a fact for lack of an alternative explanation because this explanation agreed mathematically with the hypothesis that Schwann cell capacitative discharge accounted for the superiority of myelinated nerve conductance. How else to explain it given the limitations of credulity? Elaborating on this crude model Hille writes, "One can draw an analogy between Ohm's law for electrical flow and the rule for flow of liquids in narrow tubes." 39 Also Hille writes: "In this heroic time of what can be called classical biophysics (1935-1952) the membrane ionic theory of excitation was transformed from untested hypothesis to established fact...The story illustrates the tremendous power of purely electrical measurements in testing Bernstein's membrane hypothesis." 40 One might wonder at this point by what right neuroscientists think they deserve to be considered scientists given their open flouting of the lessons of the physical sciences. Bernstein's hypothesis was never tested, merely elaborated upon. The 'tremendous power of purely electrical measurements' was actually the tremendous power of insular self-delusion in the interpretation of those measurements fed by the institutional, collective lack of rigorousness in 'classical biophysics.'

Although the Nernst potential as defined by Nernst dealt with states in which there was no flow between the two concentrations, and so there was no change in Nernst voltage, in the case of changing membrane voltages Hille writes, "...[the] simplifying rule of equilibrium cannot be applied, and the derivation must make assumptions about the structure of the channel," i.e., about the values to be assigned to the permeability of the channels so that restrictions of ion flow could be treated as both R in Ohm's law  $[V=IR]$  where V is from Nernst, and R is assumed, and RC in a circuit subject to capacitative discharges necessary for 'discontinuous propagation'. Hille writes, "The physical chemist would say, 'Yes, you have a concentration gradient, so Ohm's law doesn't work.' But the biophysicist would then suggest that a gradient is like a battery." This was discussed above, the idea that the movement of cations is the same as the electrical current needed from a battery to make them move. Here we see the gulf between biophysics and the kind we all learned in high school. Hille's authoritative account clashes with that of John Koester in his "Membrane Potential" (*Principles of Neural Science*) in which he speaks of this gradient as not like a battery, but a chemical force (when in fact it is a thermodynamic pressure; there is nothing chemical about it) that interacts with a further electrostatic force which exists as a result of the very presence of these ion concentrations. Furthermore, this electrostatic force is separate and distinct from the charge carried by those ions even though it is dependent upon their presence for its existence and the mutual interaction of the two. This tortured model of interacting thermodynamic pressures and electrostatic pressures or 'forces' as Dr. Koester calls them, is the stuff of dreams which 'has no exact analogy in electrical circuitry'.

Neuroscience is tangled in an elaborate model like a Ptolemaic universe full of epicycles to account for the observed movement of the planets; its clinical inconsequence is understandable. John Koester in his "Membrane Potential" in *Principles of Neural Science* shows a graph of cell membrane potentials versus  $K^+$  concentration gradients, and it is seen that nerve cell membrane potentials deviate quite significantly from the potentials predicted by Nernst, with the two slopes intersecting once and otherwise diverging, especially at low concentration gradients. Rather than question the viability of a feeble theory, Koester makes the claim that this divergence is due to the presence of still other species of ions. He then goes on to describe how the Goldman equation helps to account for this, and to make theory and fact more close he points out how the values for permeability are easily assumed, not measured or calculated, so that the mathematics work and the pretense of scientificity dependent upon mathematics, measurement, and quantification can be perpetuated!

The emphasis on measurement and mathematics obscured the bankruptcy of the ionic channel model as anything more than thermodynamic. Hille writes, "The biophysical method fosters sensitive and extensive electrical measurements and leads to detailed kinetic descriptions," and "...cares less about the chemistry of the structures involved than about the dynamic and equilibrium properties they exhibit." 41 Cares less about the chemistry of the structures? This rules out electrochemistry as one of the possible explanations for the nature of the nerve impulse. Hille also writes in his 1991, "A variety of formal, quantitative descriptions of excitation prevailed long before there was knowledge of the molecular constituents of biological membranes." And when that knowledge became available these quantitative descriptions were not rethought. Instead, since the authors of some of these descriptions had won large cash and status prizes for them, these quantitative descriptions became fundamentals taught to eager and gullible neuroscience students. Because these descriptions had no consequence, their adequacy could not be tested in the clinic. In a world of Oslerian therapeutic nihilism these descriptions could still be considered scientific because, lacking clinical consequence, they could hurt no one. That they prevented effective, electrochemical intervention in the treatment of a variety of neurological and degenerative disorders was never a matter for consideration. Amazingly in his narrative, at this point, Hille is bragging of the pioneering work done by early neurophysiologists; clinical relevance is not a concern. Instead he goes on, "This tradition culminated in the Hodgkin-Huxley model for action potentials of the squid giant axon." In 1963 this model was awarded a Nobel Prize.

In his inaugural address for the Prussian Academy in 1914 Albert Einstein "...spoke in praise of Planck, whose 'quantum hypothesis' overthrew classical mechanics for the case of sufficiently small masses moving with sufficiently small velocities and large accelerations." 42 A quantum account of nerve activity based upon the hypothesis that the acceleration of electrons down a fiber was at least in keeping with the new understanding of electricity as an electronic phenomenon. Yet such an hypothesis was not available to a neurophysiology with both feet in nineteenth century thermo and fluid dynamics; one that then, and now, sees electrons as cathode rays and beta radiation, not chemical energy; a biology whose practitioners still fail to see the relevance of quantum mechanics for biology.

The movement of ions laterally across the membrane of an axon is not precluded by this explanation at all, and so the ion channel findings of neuroscience and molecular biologists are not threatened at all by an electrochemical model. But in any electrochemical model, the movement of ions is  $R$ , not  $I$  in Ohm's law,  $V=IR$ . Permeability influences the movement of these ions, and so affects the value for  $R$ , but the movement of these ions is also dependent upon  $I$ , the movement of electrons in an oxidation/reduction reaction.

### The Two Schools of Thought, Ionic Channel and Epiphenomenalist, for the Nature of Nerve Impulses

Two schools of thought contested the issue of the nature of the nerve impulse as revealed by the measurements made possible during the 1940's. Bertil Hille in his 1991 calls these two schools the ionic channel school and the epiphenomenalist school. The beliefs of the ionic channel school have been presented. Of the epiphenomenalist school Hille writes, "...propagation of the nervous impulse was a chemical reaction confined to axoplasm and the action potential was only an epiphenomenon - the membrane reporting secondarily on interesting disturbances propagating chemically within the cell." For the epiphenomenalists the cell and axon were black boxes about which not enough was known. Some fundamentals of the epiphenomenalist school are presented in "The Electro-Dynamic Theory of Life" by H.S.Burr and F.S.C.Northrop in which the authors argued that 'living systems are physical systems in the sense prescribed by field physics'43. This description is a bit more detailed than that of Claude Bernard in 1865 who spoke only of natural laws. The two later in 1939 published "Evidence for the Existence of Electro-Dynamic Fields in Living Organisms" in *Proceedings of the National Academy of Sciences*. Northrop and Burr sought to provide a principled account for the organization of cells that comprised an organism, especially for those organisms with nervous systems. Northrop preached that 'the central difficulty of biology' was the problem of organization, and this is still considered the central difficulty by today's molecular biologists. In their essay "Causality in Field Physics in its Bearing upon Biological Causation" they warn against the "...danger of falling back upon the rejected mechanical models by surreptitiously using the particle and wave of quantum mechanics in the sense of the particle and wave of Newtonian mechanics and hydro-dynamics." 44 In a 1936 *Yale Journal of Biology and Medicine*

article entitled "The History of Modern Physics in Its Bearing Upon Biology and Medicine" Northrop writes:

The insufficiency of the thermo-dynamical theory as a complete account of biological organization centers in the fact that there is nothing in the theory to prescribe the particular relatedness into which energy organizes the moving chemical materials. This can be put in more technical language by saying that there is nothing in thermo-dynamics itself which prescribes at what point precisely in the tendency toward a state of maximum entropy the energy from outside the system compensates that tendency, to produce a steady state, or the state of mean compensated entropy, which is a living organism. 45

p.155 Life depends for its very existence upon energy radiated to it upon the earth from the sun. This radiation is an electromagnetic phenomenon. Consequently, living organisms depend for their very existence upon electro-magnetics.

...Thus the emphasis in chemistry is more on the constancy of the entities than on the constancy of the relatedness. This would lead us to expect that the chemical theory of life would be quite effective in accounting for the constituents of living organisms, but somewhat ambiguous in providing an adequate account for the persistence of biological organization.

p.160 This means, however, that we cannot expect to find the scientific basis of biological growth and organization solely within the chemical constituents of the animal's body. Growth and organization involve chemical factors within and without the animal's body on the earth's surface and in its atmosphere, and these in turn depend upon energy relations with our solar system.

So the epiphenomenalists were concerned with electromagnetics and energy, in particular, chemical energy, which is galvanic or direct current, electrons. The necessity and organization of cells is directly related to thermodynamics and the dynamics of oxidation/reduction reactions. A cell wall is required to create a thermodynamic, open system, one which may capture energy from its surroundings to combat entropy. This capture of energy involves photosynthesis and oxidation/reduction reactions, reactions which involve the movement of electrons. In order for there to be such reactions there must be a barrier across which a voltage can be measured. This barrier is provided by the cell wall or membrane, and within the cell by the intricate folding of the internal walls of mitochondria. The first life, archaebacteria and prokaryotes, were purely electrolytic cells, cells which did not embody a catabolic, oxidative process but which got their energy from photosynthesis and from oxidation and corrosion that took place externally. This can be seen in the case of algae in acid filled volcanic caldera, or subterranean oxidation of iron deposits made possible by the release of oxygen liberated by electrolytic bacteria that derived energy from heat photosynthesis. The light involved in these cases was the infrared light from geothermal sources. Only later did cells begin to embody these catabolic processes, to be like batteries, and these cells were the eukaryotes, nucleated cells. The organization of cells, biological organization in general, revolves around energy capture. The electrochemical, oxidation/reduction dynamic of cells is seen again in multicellular organisms, especially those that embody gastrulation, the digestive breakdown of organic molecules by acids. In these cases the volcanic caldera are carried around by the creature in the form of a gut or stomach.

The thermodynamics favored by the heroic neurophysiologists in their attempts to model nervous functioning is grossly inadequate for any analysis of an organism involving energy transactions. It does not address such things as principles of organization or the quantum nature of chemical energy. During the 1950's the epiphenomenalist school started to die out along with the study of the nature of nervous system

trophism, but not because of the superior theories and results of the ionic channel school, not for any scientific reason at all, as a matter of fact. When Dr. Albert Szent-Gyorgi, who had worked for years with x-ray diffraction to study the molecular structure of protein molecules, ventured that the protein molecule was crystalline enough in its lattice to support semiconduction at certain temperatures which were the temperature of the body, he was greeted with indifference if not hostility from those who had a vested interest in maintaining the thermodynamic model upon which their own status as experts was based.

The attitude seen above, the description of Hille of the ionic channel school caring less about the chemistry of the structure, just the measurements, also carried over to the structure of the nerve itself. And this breathtaking insouciance was awarded a Nobel in 1963 with the prize going to John Eccles, Alan Hodgkin, and Andrew Huxley for a model that stressed the primacy of measurements over structure. In particular, John Eccles, in his 1973 *The Understanding of the Brain* implies that the structure of the nerve fiber is not at all important to the way it works naturally, that the contents of the axon could be considered as a resistor as well as a capacitor, and could be dispensed with entirely in the attempt to show that the hypothesized measurements of the model would still result. He writes:

The content of the axon has the consistency of jelly, and for most purposes you can substitute an appropriate salt solution without deteriorating impulse conduction by the fiber. For example Baker and Shaw were able to squeeze out the contents of the giant squid axon with an open end by a kind of microroller, leaving a collapsed, flattened axon that appeared destroyed. Yet when they reinflated it by an appropriate salt solution, a potassium salt, the fiber was restored and conducted well for hours.

This is a simply amazing statement in its boldness, and ignorance, even for the year 1973. The reader is asked to believe that all that is important for nerve conduction simulation is the axon membrane and electricity provided by an electrode, not the axon itself. But this statement is in keeping with Hille's comments noted above that "A variety of formal, quantitative descriptions of excitation prevailed long before there was knowledge of the molecular constituents of biological membranes." These quantitative descriptions were wrong before this knowledge came along, and were not corrected or questioned even after. The trouble was that their incorrectness could have been detected before detailed knowledge of the structure of membrane and nerve fiber was available if only the pioneers of classical biophysics had understood physics. The quantitative description of Eccles and the ionic channel school rested upon a metaphysics in which a sodium pump, was figured to drive things. Five years after Eccles's book, in 1978, Peter Mitchell received a Nobel in chemistry for an ionic account of biological energy which replaced the sodium pump with a 'proton pump.' More than a quarter of a century later clinical neurology still can do little more than it could a century earlier.

The inability to truly simulate nerve impulses prevented medical science from understanding or doing anything about trophoneurotic conditions like muscle atrophy or heart disease, or any of the degenerative diseases of aging. The encouraging results of experimentation done during WWII by people like Guttman, Melville, Wehrmacher, and Hines using what was called *galvanic exercise* on the atrophic hands of those who had recently undergone surgical repair of an ulnar nerve lesion, could not be understood according to the classical model of electrotherapy and nerve impulse propagation, and were allowed to lie fallow. The results of this research were noted, and the next decade Dr. Guttman even denied that electrotherapy of any sort would have any effect on stopping and reversing atrophy.

In his essay "Control of Movement" Claude Ghez claims that "Muscle weakness may result from disturbances in descending motor pathways or in the spinal motor neurons themselves."<sup>46</sup> Ghez does not say in his essay which disturbances, upper or lower motor neuron lesions, are involved in the motor weakness of those suffering from disuse atrophy like that in astronauts returning from weightless conditions or those consigned to long periods of bed rest. Yet these people too suffer motor weakness. Presumably the wasting of muscle through atrophy is not a neurogenic condition, but a myopathic one. In his "Diseases of the Motor Unit" Lewis P. Rowland writes, "When the sole manifestation of a disease is limb weakness, as often happens, clinical criteria alone rarely suffice to distinguish between neurogenic and myopathic diseases."<sup>47</sup> In the list of examples of myopathic diseases Rowland does not list muscle atrophy. Claude Ghez defines muscle atrophy as 'loss of muscle volume', but he is not clear as to whether

this loss of volume is due to loss of capacity or loss of contents. Neither Dr. Ghez nor any other physician in *Principles of Neural Science* gives any indication just what it is that might be lost to result in this loss of volume. Rowland does make the claim, however, that in upper motor neuron lesions atrophy is rare. This is an odd statement since in the case of stroke and brain injury the atrophy of muscle on the affected side is commonplace and quite dramatic.

It is important to distinguish between muscle volume and muscle density, especially in view of the inadequacy of electromyography (a recording technique handed down from the heroic neurophysiologists of the 1940's to the clinicians along with their oscilloscopic misinterpretations) for distinguishing healthy from atrophic muscles. A healthy muscle is a dense muscle that will sink in water regardless of its volume or size, while a voluminous, puffy muscle is a weak muscle. Both may display the same electromyographic signs and even have the same volume in the sense of same size. When Ghez speaks of muscle volume he refers then to its contents and not its size or capacity. What then are these contents? And why should their loss not trigger something on the oscilloscope more akin to the *reaction of denervation* which appears when nerve supply to a muscle has been terminated?

In his 1976 "Hemiplegic Amyotrophy" Sudhansu Chokroverty notes that in the case of muscle wasting he and others found a noticeable diminution in the cross sectional area of type II muscle fibers, that this wasting was found usually in cases of prolonged bed rest, and suggests that disuse atrophy could be the gross appearance of this degeneration. 48 In *Principles of Neural Science* there is no mention of type II muscle fibers, and the pathology of disuse atrophy, as mentioned above, is dismissed without comment other than that it is a loss of muscle volume which, we are asked to believe, the brain injured do not suffer. *Principles of Neural Science* runs to over a thousand pages, and is a standard text of neuroscience even today. One of its editors, Dr. Eric Kandel, received a Nobel with two other neuroscientists in the year 2000 for decades of work which, not surprisingly, has little or no clinical consequence.

For Dr. Chokroverty the type II fiber was similar to the transverse tubule shown in the diagram on page 549 in Claude Ghez's "Muscles: Effectors of the Motor Systems," with this exception. Whereas each and every muscle cell has a transverse tubule, there are certain cells which lie directly post-synaptically and from which arborizes a fiber. This fiber, the type II fiber, informs the muscle and provides most of its bulk. This fiber forms what are called *gap junctions* with all of the cells in a muscle. At these gap junctions are found the electrical synapses which, in essence, maintain cytoplasmic continuity between all of the cells of a muscle and those which are immediately post-synaptic so that all of the muscles of a cell respond in concert to an impulse arriving at the neuromuscular junction. Although this structural arrangement is to be found in the liver and all smooth muscle, it is not taught that striated muscle has the same structure. The truth of the matter is that all post-synaptic, somatic structures have this structure, from striated muscle to liver to kidneys and lungs and heart. The question is, why isn't this seen as the way striated muscle is organized? And the answer to that is found in the way the action potential arriving at the synapse is seen to propagate on the muscle. The account that is favored is so blinding in its error that the presence of electrical synapses on striated muscle is seen as unnecessary. The role of the type II fiber is seen then as the provider of muscle volume, nothing more.

Let's examine then how neuroscience sees the propagation of the nerve impulse to the millions of muscle cells in a muscle. The favored explanation is fully in keeping with the idea that there are such things as ionic currents that are electrical. This farfetched and archaic treatment of electricity characterizes almost all of the thinking of modern neuroscience with regard to the nature of biological energy. The question becomes how do these ions penetrate the muscle. Ghez writes:

Contraction is set off by the depolarization of the muscle fiber [depolarization is only possible with DC]. When an action potential in a motor axon reaches the neuromuscular junction it generates an end plate potential, which in turn triggers an action potential in the muscle fiber. This action potential is propagated rapidly **over the surface** of the fiber and **conducted into the muscle fiber** by means of the system of T-tubules [emphasis added]. The T-tubule system insures that the contraction that follows a single action potential, termed a twitch, spreads throughout the entire fiber. 49

This explanation resulted from the observations of the early electromyographers who noted that when a muscle contracted a great deal of electrical activity could be detected at the muscle surface. At the time,

the 1940s, the role of the electrical synapse on post-synaptic, somatic structures like muscles and organs was not understood. Consequently the electrical activity detected at the muscle surface was thought to be the result of the movement of ions at the muscle surface penetrating the muscle rather than coming from within the muscle to the surface as electrons, which then drew calcium ions into the muscle to initiate the splitting of ATP there. It is the splitting of ATP which energizes the contraction of the type I muscle fiber. As noted above, when the fine details of biological structures like muscle became known, there was no attempt to revise the by then well accepted quantitative descriptions based upon the study of electrical recordings. Neuroscience is founded upon traditions. Revision is threatening to traditions.

Dr. Ghez writes, "A key aspect of the electromechanical mechanism by which the action potential triggers mechanical contraction, a process termed *excitation-contraction coupling*, is a sudden increase in intracellular  $Ca^{2+}$ ." Many of the functions of cells, from neurons to immune cells, are dependent upon the availability of calcium ions as catalysts. It is the movement of positively charged calcium ions which is thought to be the primary role of the proton pump. As noted above, biological accounts of electricity stress the role of positively charged ions as functionally equivalent to electricity. This ionic treatment of chemical energy can be found at all levels of biology. So when a fungal cell's apical hypha extends toward the cell's membrane in the growth of the cell, to be bombarded there by calcium ions coming from without [just like with the striated muscle], the bombardment of calcium ions is seen as the result of a proton pump acting from outside the cell rather than an electromagnetic response to the cell's internal release of chemical energy resulting from the splitting of ATP by the mitochondria internally. The alleged existence of a proton pump is a key part of what became known as the chemiosmotic hypothesis, for which Peter Mitchell received a Nobel in 1978. The award lent further authority to the idea that electromechanical mechanisms like the process of excitation-contraction coupling could adequately model electrochemical functioning. The use of terms from electrochemistry and electromagnetics, like 'volt' in the Nernst equation, became standard practice for neurophysiology. This standard practice did not extend to such things as the placement of the ground electrode when measuring cell membrane voltages.

Bertil Hille writes:

Pulse-like messages called ACTION POTENTIALS are sent down the motor nerve from the central nervous system. When they reach the nerve terminal, action potentials evoke the release of a chemical signal, the neurotransmitter acetylcholine, which in turn **diffuses** to the nearby **muscle surface** [emphasis added] and causes acetylcholine-sensitive channels to open there. 50

The use here of the term 'diffusion' cannot be based upon any electrochemical treatment. In electrochemistry diffusion is the movement of ions within a few angstroms of the surface of the battery's electrodes when they are immersed in the solution of an electrolytic cell. It is not clear what is meant by the term, though a better word may have been dissipates. Yet by using terms from electrochemistry a certain intellectual dishonesty can be brought to the lesson that is adequate enough to turn the head of the student of neuroscience who will never return to this subject matter. It is strictly academic in nature and is without pertinence to reality, just as the model in which it occurs. For all muscle physiologists propagation and diffusion take place at the muscle surface because this is where the proton pump functions is said to exert its magical force. The opening of acetylcholine sensitive channels seems to be the same as the opening of 'voltage sensitive channels.' According to Ghez:

The depolarization of the T-tubule system acts on the specialized voltage sensitive channels in the terminal cisterns located in the apposing regions of the sarcoplasmic reticulum membrane. By mechanisms that are not fully understood, these local voltage-sensitive channels cause the release of  $Ca^{2+}$  throughout the membrane of the sarcoplasmic reticulum.

Just how these action potentials 'evoke' the release of acetylcholine or the release of calcium ions is 'not fully understood'. Otherwise such non-specific language like 'evoke' and non-electrochemical 'diffusion' would not be used to describe an eminently electrochemical event. This account of the functioning of the muscle cell and the lower motor neuron is the same as that for apical hypha activity in

the growth of certain types of cells like neurons and fungal cells. This is discussed by Emeritus Professor in Molecular Biology Franklin Harold in his 2001 *The Way of the Cell*. Dr. Harold says that the most widely accepted view is that this hypha works by hydrostatic pressure from within the cell and a proton pump acting from outside the cell membrane. Fluid dynamics and thermodynamics are a very inadequate way to model an electromagnetic/ electrochemical phenomenon. And this inadequacy manifests itself quite graphically in the clinic.

Returning now to the question of muscle weakness and atrophy, one can see that given the electrically synaptic structure of the type II fiber, its extensive arborization as it grows from the motor end plate region and ramifies extensively before it reaches the gap junctions at the individual muscle cells, the long noticed amplification of the power of the arriving nerve impulse at the neuromuscular junction is easily accounted for in the equations of electricity. Dr. Ghez says the strength of muscle contraction is due to the initial length of the muscle or the rates of movement of the thick and thin filaments of the sarcomere, the type I fiber. These rates of movement are influenced by the concentration of calcium ions that catalyze the splitting of ATP at the individual muscle cell's sarcomere. And that concentration directly reflects the amplitude of the arriving, depolarizing charge carried by the muscle cell's T-tubule to the invaginations at the muscle cell's surface. Furthermore, the amplitude of the depolarizing charge carried by the individual muscle cell's t-tubule to the muscle cell's surface, is directly dependent upon the amplitude of the charge arriving at the individual muscle cell to begin with. This charge is carried there by the type II fiber arborizing from the neuromuscular junction. This fiber, by maintaining cytoplasmic continuity between all the cells of the muscle, is the determinant of the power of the charge conducted from the gap junction by the t-tubule to the muscle cell's surface. The amplitude of this charge is inversely proportional to the square of the cross-sectional area of its conductor, the type II fiber. If the cross sectional area of the type II fiber is reduced [as it is in disuse atrophy] by one half, the charge arriving at the gap junction and carried from there by the t-tubule to the muscle cell's surface, is diminished by seventy five percent. Excitation-contraction coupling is thereby reduced in strength accordingly since the number of calcium ions drawn from the extracellular space to initiate the splitting of ATP at the sarcomere is reduced. And with this reduction is seen a less energetic contraction, a slower rate of movement of the thick and thin filaments of the sarcomere.

The loss of the cross sectional area of the type II muscle fiber, or disuse atrophy is one way in which muscle or motor weakness may be brought about. If the type II fiber is allowed to deteriorate to the point where it no longer passes to the individual cell's t-tubule enough charge to pull calcium ions sufficient for contraction, then it is possible for a muscle to be unusable not because of upper or lower motor neuron damage, but because of muscle deterioration. In other words, if allowed to advance far enough, disuse atrophy may result in paralysis. Given this, it follows that it is extremely likely that there are great numbers of people in wheelchairs from chronic paralysis following non-destructive, nervous system trauma because of advanced atrophy which takes place during the acute phase of injury during which the muscles remain unused and so are allowed to degenerate. The idea that the spasticity of the spinal injured, and the palsy and trembling, as of the aged or those with Parkinsons, is or might be due to nervous activity, is dependent upon the notion that the muscles are intact, that the type II fiber is passing enough charge to the individual muscle cells.

That a muscle does not display the reaction of denervation, a sign on the oscilloscope that occurs when the nerve supply to the muscle has been interrupted, when the needle probe is inserted, has been long thought to mean the muscle is entirely intact and functional. Yet the reaction of denervation is lacking when atrophic muscles are examined; the signs are similar to that of healthy muscle despite the tremendous muscle wasting or loss of volume. Electromyographers and clinicians conclude that atrophic muscles are still functional and that continuing paralysis is due to lower motor neuron damage rather than muscle deterioration. Consequently the enfeeblement of those whose spinal cord injuries and head injuries and strokes are followed by motor weakness and paralysis is said, in all seriousness, to be due to 'learned non-use.' Paralysis researchers resort to therapies involving treadmills and what is called 'constraint-induced therapy' in which a hemiplegic's usable muscles are restricted so that he will be forced to use muscles he can't move in order to regain some function through re-education of the nerves. The wrong-headedness of this approach to the problem of paralysis escapes the orthodox clinical neurologist, but has no detrimental affects upon his income.

Unfortunately for the victims of ionic-channel-school-perpetuated clinical ineptitude, chronic paralysis from disuse is irreversible. Exercise is limited to traditional resistance exercises necessitating

muscle use, and accepted modes of muscle strengthening using electrotherapy [FES – functional electrical stimulation] stress muscle contractions not triggered in any fashion even close to how the body works. That is, the muscle is made to contract without the use of electrochemistry and without engaging the type II fiber/transverse tubule interface at the gap junction. Consequently such ‘exercise’ does not cause the type II fiber to grow or increase in cross-sectional area. Only stimulation at the motor end plate region with the anode of direct current can provide the anabolism necessary for the growth of the type II muscle fiber. And in doing this muscles long-paralyzed may be restored to functionality again without having to be used.

### Chronic and Degenerative Diseases of Aging, and the Complexity of the Nervous System

In his essay "Development, Critical Periods, and the Emergence of Behavior" Thomas M. Jessell shows a diagram of the neuromuscular junction or motor endplate region in which numerous presynaptic terminal boutons are present. 51 Each of these boutons marks a neurotubule terminal, and each of these neurotubules is made up of three kinds of fibrillar element - microtubules, neurofilaments, and microfilaments, with microtubules being the thickest. 52 Dr. Schwartz writes of these "proteins that constitute the cytoskeleton [that they] mediate the movement of organelles from one region of the cell to another" amongst other things. "They also determine the shape of the neuron," and "In the axon they are oriented longitudinally with polarity always in the same direction. This arrangement is presumably important for the directional specificities of the two forms of fast axonal transport" of organelles like mitochondria. 53

The role of the numerous nerve terminals marked by the pre-synaptic boutons in a motor endplate region is not, in every case, to initiate a secondary action potential charge that rides out on the type II fiber to the individual muscle cell. The origins of each of these terminals is not necessarily the spinal cord; some come directly from the brain via the sympathetic chain or vagus nerve, and join with the spinal nerve roots, for example, where they synapse at the dorsal root ganglion to form the spinal nerves as they wrap the spinal nerve roots coming from the cord in Schwann cell myelin. From each bouton, postsynaptically, proteins like those composing the type II muscle fiber, are expressed by the cells found in the endplate region. Some of the boutons located in each motor endplate region are in fact associated with muscle, whether smooth muscle or striated muscle. Some of the proteins expressed by the cells that lie postsynaptically make up organs. The type II fibers that inform the muscles like the multifidus attached to the vertebrae grow from the dorsal root ganglion. Traditionally the presence of the dorsal root ganglion is taken to signal that the role of cells located in the dorsal part of the spinal cord serve an afferent rather than an efferent function. This discrimination is behind the Bell-Magendie law mentioned above on page 6. As noted it has no basis in modern experiment and obscures the more likely functional division of the spinal nerves into flexion and extension. Although it has been noticed that in a laboratory a nerve fiber is found to conduct in both directions, it is still believed, in tribute to the Bell-Magendie law, that in nature the fibers can only conduct in one direction. Nature is seen as very extravagant in this regard, requiring different nerves for afferent and efferent functioning. This requirement defies the presently understood, paleobiological origins of the neuron's axon as a kinesisome in the cell, a thing devoted to motor activity. There is no such thing as a sensosome. But then again, revision and hypothetico-deduction have never been the strong point of a science dedicated to the accumulation of facts and the maintenance of traditions.

An important diagnostic point, and diagnostics has always been the strength of a neurology which could do nothing in the clinic, is that an electromyographer with his needle electrode cannot assume that each of these synaptic boutons in a cluster of them located in each motor endplate region and ganglion has as its provenance an alpha motor neuron in the anterior portion of the spinal cord. Yet this conclusion is routinely made by electromyographers in the clinic. In addition they pronounce a severely atrophic muscle healthy but lacking in volume, and attribute continuing paralysis to problems in the brain or spinal cord since a severely atrophic muscle shows the same signs as a healthy muscle. Although the clinical neurologist and electromyographer does no harm with his Oslerian medicine which emphasizes diagnosis, he does no good either. And this fits the definition of Oslerian scientific medicine.

There is a condition called trophoneurosis; it is the deterioration of the muscle or organ that lies post-synaptically from the above mentioned boutons at the nerve terminals, deterioration resulting from failure of the lower and upper motor neuron to adequately energize these degenerate muscles and organs with acetylcholine and beta radiation. In the muscle this shows up as disuse atrophy, but this sort of

deterioration can strike any post-synaptic structure. With regard to these nerve terminals Dr. Chokroverty writes:

The number and distribution of synaptic vesicles per unit nerve terminal area also differs from end-plate to end-plate and from one region to another. Similar variations were noted in the nerve terminal area. Some nerve terminals were large, whereas others were small. In some regions the nerve terminals appeared distended, with an excess of neurofilaments; others were filled with abundant mitochondria, while some were devoid of mitochondria. The postsynaptic region appeared denuded of the nerve terminal in occasional sections. All these conformational changes were noted in both control and prednisolone-treated animals. 54

Dr. Chokroverty's findings are totally beyond anything the electromyographer is capable of detecting, dealing with structure rather than wave forms on a cathode ray tube, yet the pronouncements of the electromyographer about the neurological integrity of the muscle, given the lack of the reaction of denervation on the oscilloscope, are still taken to be the authoritative, clinical judgment about the health of those muscles. Dr. Schwartz writes, "Rapid growth or disappearance of microtubules plays a crucial role in many cellular processes, for example in the movement of chromosomes during cell division and in the nervous system during the growth and extension of axons and dendrites."<sup>55</sup> Tubules make up the centrosome, the kinetosome, the apical hypha, the undulipodia, the neuron's microtubule, the muscle's transverse tubule, the type II muscle fiber. All function electrochemically even in their actions upon each other. The multi-cellular organism is a concert of cells that can be divided between pre- and post-synaptic. The synapse is a gap into which beta radiation or electrons may be introduced to energize the organism through the triggering of protein synthesis and the stimulation of nervous trophism.

Francis Crick, the author of what is known as the central dogma of biology, writes in his 1994 *The Astonishing Hypothesis* that "It is sobering to realize that after almost thirty years of research we still do not know for certain how either simple cells or complex cells are wired to produce their observed behavior," where behavior is that of cells and not that of organisms. The behavior of organisms, however, is nothing more than the behavior of cells acting in concert, cells with a nervous system which provides for perceptual feedback. The behavior of these organisms cannot be considered separately from the nature of their nervous systems. Crick also notes that "...the main function [of a neuron] is to receive signals and send them out - that is, to handle information." The question immediately arises, how is this information encoded in signal form and transmitted, then re-translated to something meaningful to other neurons or post-synaptic cells? Especially on a model of nerve impulse propagation (the ionic channel model) that functions via a proton pump which reverses entropy by pumping ions of sodium laterally across the axon's membrane rather than longitudinally down the fiber?

Bert Sakmann won the Nobel Prize in physiology in 1991 for his development of patch clamping. Patch clamping allowed for the extremely fine study of cellular electrical functioning through the divination of the meaning of electrical measurements. When a nerve impulse passes there is a change in cell membrane voltage that heralds its passing. A patch clamp allows for the introduction of electrical charge carried by electrons so that the wave of changing voltage of a nerve impulse is stopped, with the voltage remaining constant. Those who work with patch clamping assert that they are determining the amperage of the ionic current this way, even though amperage is a term of measurement of the flow rate of electrons, not protons or ions of hydrogen. In Ohms law the movement of ions is R, not I. I is measured in amperes. Patch clampers believe they are measuring R in amperes, under the mistaken notion that R, the movement of ions, is I, and that R is membrane permeability while C is membrane impermeability. Sakmann stated in an interview in 1994 that "It's very difficult to explain how the current is carried by ions and not by electrons. The way the cell signals and how messages are sent by action potentials or impulses that travel down the axon is hard for people to grasp." (Bass, 1994) Sakmann (1994) states: "Membrane potentials are the means by which cells communicate with each other...the requirements for signaling in the nervous system." The motor neurons, types of cell which send signals to the muscle to contract, "...generate action potentials that travel along the nerve...it's all done electrically." Given that, it would seem that the neuroscientific understanding of electricity is badly in need of emendation. Sakmann states, "Now that I think I understand the neuromuscular synapse, I am curious to know more whether similar principles

govern synapses in the central nervous system. When I began working again in this area, one of the most surprising things was how little had changed. People are asking the same questions I was confronting twenty years ago." Sakmann (1994) responds, when asked what the ion channels look like, "A funnel and a gate large enough to pass one ion at a time...at least this is one theory. The actual mechanism remains a mystery." Finally, Sakmann (1994) confesses, "Information is not contained in one action potential, but in the different rates or frequencies at which they transmit. This is called frequency encoding, but how it actually works is a complete mystery." This is still neuroscience's understanding of information processing by the nervous system.

An electrochemical model, on the other hand, would hold that a nerve message's information and meaning is identical with the chemical reaction triggered by its arrival. Frequency encoding is no problem, no mystery. It has been used in radio for almost a century, and involves, in the case of FM, the varying of the frequency of pulses of beta radiation, electrons, cathode rays.

Francis Crick's book, subtitled *The Scientific Search for the Soul*, contains the statement that "When these [optic nerve] signals arrive at the brain stem they have to be transformed into a different set of signals to control the muscles of the eye. Exactly how this is done has yet to be discovered." So the nature of these signals, the way they carry information and how this information is translated and communicated, like the concept of energy that appears in most biology texts, remains unarticulated despite the claims of the ionic channel school of having a working model of the nature of nerve impulse propagation. Professor Patricia Churchland writes, "...a singularly surprising thing to learn in neurology rounds is how often distinct neurological disorders include motor impairments of some description. Even in cases where cognitive deficits are particularly prominent, there are typically accompanying motor deficits." This conjunction of neurological disorders and motor deficits also remains unexplicated, as if the brain did not have as its primary role the motor activity of the organism, as if the motor neuron's axon and the cell's kinetosome didn't have the same origin.

Changing directions now for a moment and addressing once again the phenomenon of electricity and its importance for biology and neurophysiology, it is seen that

...for biologists 1953 was a vintage year the likes of which had not been seen since the publication of Darwin's classic in 1859. In this single twelve month span, not only did Watson and Crick unravel the double helix geometry of DNA and Frederick Sanger work out the chemical structure of proteins, but also the modern era of scientific investigation of the origins of life on earth was ushered in with Stanley Miller's experiment showing that the chemical building blocks of life on earth could be formed by natural physical processes taking place in the primordial environment. While the work of Watson, Crick and Sanger is crucial for understanding how living forms function, it was Miller's experiment that set the stage for what has become the dominant scientific paradigm for how life as we know it today got its start here on earth. 57

What Miller did was introduce an electric discharge, a lightning bolt, i.e., direct current, into an atmosphere figured to be like that on earth billions of years ago, an atmosphere rich in methane, ammonia, hydrogen, and water vapor. The result was a rain of amino acids which varied in constituents depending upon the proportion of gases present. The largest amounts of amino acids present were glycine and alanine. A form of alanine, phenylalanine, is produced by the chromaffin cells which lie post-synaptically throughout the sympathetic or para-vertebral chain. As the protein fiber that contains phenylalanine is pushed from the cell that phenylalanine changes to dopamine and eventually norepinephrine, a precursor to one of the two major neurotransmitters of the body, epinephrine, the other being acetylcholine. The chemistry of the body is not so mysterious; self-assembly can be seen throughout the cell's functioning if the energy is provided. Clearly one of the natural forces invoked by Claude Bernard, and Charles Darwin responsible for life was electricity, and this electricity was galvanic, not faradic. Biologists and neurophysiologists are still not aware of the difference, nor do they see its relevance to a body of knowledge which has 'worked' at least in the sense that it has not exacerbated the suffering of those

afflicted with neuromuscular disorders while it has provided employment for generations of neuroscientists and neurologists.

Lynn Margulis and Dorion Sagan in their 1986 *What is Life*, observe that the microtubule, constructed from RNA, can act either as a centrosome pulling apart the chromosomes as a cell divides, or as a kinetosome, a cellular structure which became the cell whip, flagella, or undulipodia that developed to propel the cell toward food sources. With the development of multicellular organisms with nervous systems, this cell whip was to become the axon of a neuron. And the way it functioned was through rapid movement of electrical charge, that is, amperes, through waves of polarization and de-polarization. Lynn Margulis and Dorion Sagan write in their 1986 *Microcosmos: Four Billion Years of Microbial Evolution*:

The axons and dendrites of the brain are a differently organized mass of microtubules containing all the microtubular proteins...mammal brain cells - the richest source of tubulin protein anywhere - do not waste their rich microtubular heritage. Rather the sole function of mature brain cells, once reproduced or deployed, is to send signals and receive them, as if the microtubules once used for cell whip and chromosomal movement had been usurped for the function of thought.

Roger Penrose, in his 1994 *Shadows of the Mind*, writes that the protofilaments that make up the microtubule are capable of two conformational states. "There is evidence that these two conformations correspond to two different states of the dimer's electric polarization, where these come about because an electron, centrally placed at the a-tubulin/b-tubulin juncture, can shift from one position to another." 58 In other words the microtubule's functioning, passing of information for nervous system information processing, depends upon the movement of electrons and polarization/-depolarization/repolarization. This view immediately clashes with the widely accepted biological belief that there are no free electrons in the body. But polarization of electrical charge is found only with direct or galvanic current. How can polarization and depolarization take place in the body without electrons changing places, without electron-driven conformational changes? Biology has nothing to say about this. In addition Penrose, who is not a biologist but a mathematician, writes, "The role of neurons, in this picture, is perhaps more like a magnifying device in which the smaller-scale cytoskeletal action is transferred to something which can influence other organs of the body - such as muscles." 59 And it does this through the amplification of the power of the nerve impulse by electrochemical activity across the chemical synapse, as discussed above. The behavior of an organism as well as the feedback it receives from its surroundings is reduced to the binary state of negative electrical charge, or a hole, just as in semiconduction.

Margulis and Sagan, in their 1996 *What is Life?*, discuss the two key factors to qualify a thing as living. These things are the ability to replicate and, more importantly, autopoiesis, or self-building and maintenance. And in either case, the underlying consideration is one of energy capture, production and use. For the single, nucleated cell energy considerations are important for the production of proteins/enzymes, e.g., polymerase, which are crucial to accurate genetic replication, or detoxification of the corrosive bi-products of mitochondrial energy production which, as free radicals, can cause genetic mutations. These mutations may lead frequently to cancers, but more commonly are seen as changes in the proteins expressed by a cell, or failure to express proteins at all. Cellular energy is, for the most part, provided by mitochondria. Margulis and Sagan write:

Inhabiting nearly every nucleated cell, these dark, membrane-bound bodies provided the cell surrounding them with abundant energy derived from the oxygen in the air. Because of mitochondria, all earthly beings made of nucleated cells - which, of course, includes us and all organisms except bacteria - have remarkably similar metabolisms...In eukaryotic cells wastes from fermented food molecules in the cytoplasm such as alcohol and lactic acid enter the mitochondria and pass through a cycle of reactions involving oxygen and the same sorts of electron chains that are found in aerobic bacteria. These reactions inside the mitochondria produce most of the ATP for both the mitochondria and the rest of the cell.

Jaime Miquel of the University of Alicante in Spain has sought to account for cellular deterioration in terms of energy production since the cytoplasm in nucleated cells is quite motile, unlike in most non-nucleated cells, and this motility is a result of electromagnetic forces since it appears directed rather than being the result of random mechanical collisions. This is just what F.S.C. Northrop hypothesized over sixty years ago, as discussed above, with regard to the principle of biological organization being electromagnetic. If intracellular activity were merely the result of molecular collisions entropic pressure would disperse chemical energy throughout the cell. "Miquel has suggested that molecular wear and tear may directly affect the 'mitochondria', the 'power plants' that provide energy for all the cell's activities," writes Leonard Hayflick in his 1994 *How and Why We Age*. In addition he writes:

Furthermore, unlike the nucleus, the mitochondria do not seem to have a repair system for damaged DNA. In many animal species the mitochondria in old cells show a decrease in numbers, an increase in size and various structural abnormalities. Even cultured normal cells reveal these mitochondrial changes at the end of their lifetime.

...Until experiments are devised to test directly whether mitochondrial damage causes age changes we can only admire the ingenuity of the biogerontologists who have developed such an interesting theory.

Three years after the publishing of Dr. Hayflick's book appeared an article in the August, 1997 edition of *Scientific American* by Dr. Douglas C. Wallace, head of the molecular biology department of Emory University. This article was an update on a similar article in that same publication which appeared at the time of the publication of Dr. Hayflick's book. It discussed how the genetic deterioration of mitochondria in a cell lead to that cell's being unable to properly take care of itself and perform its functions, and suggested that this might be a cause of the degenerative diseases of aging. Dr. Wallace detailed what he called the 'amplification of mutated mitochondrial DNA', and that this was the tendency of mutated mitochondria less able to produce energy to replicate more readily than non-mutated mitochondria when the cell 'sensed' an energy deficit. The result was that the cell would eventually have more underpowering mitochondria. Dr. Wallace goes on to say that so far the only thing found clinically to help slow the aging process was physical exercise.

The introduction of pulsed negative electrical charge by transcutaneous electrode at the site of the plexuses, ganglia, and motor endplate regions simulates nerve activity like that which triggers voluntary muscle contraction. With each pulse the muscle will be seen to twitch. At the rate of 2000 twitches per second the muscle can be overloaded. In response the type II fiber grows and the muscle strengthens just as if it were being subjected to weight training. Because of the ionizing effects of the beta radiation on the skin, stimulation durations at 2000 hertz per motor endplate region can be limited to one second every other day. This form of electrochemical exercise has the same effects as regular resistance exercises, but the muscle does not have to be used to overcome any resistance. In addition all somatic proteins, whether gland or organ, to the extent that they are influenced by nervous system trophism, will be similarly restored. In this way longevity can be promoted and fitness be attained without having to do any physical exercise. Since what is involved is electrochemistry, oxidation-reduction reactions and the movement of electrons, every bit of anabolic reduction must be balanced by catabolic oxidation and corrosion. Energy capture and production in the body is balanced with the production of free radicals, oxygen ions looking to bind with and share the electrons of other atoms and molecules. This means that every bit of muscle building triggered by the introduction of electrons must be balanced with corrosion somewhere. By using a cathode of steel that corrodes, what is called a *sacrificial metal*, the number of electrons introduced by the anode to the body is far greater than the number of electrons removed, the balance coming from the corroding metal plate. In this way the body can be energized by electrochemical intervention with little creation of free radicals, and the degradation of the somatic proteins through entropy can be reversed.

Epilogue

The organization of chemistry that lies at the foundation of life is along lines of oxidation-reduction reactions taking place on opposite sides of a wall or membrane across which a voltage may be measured. Cell walls provide the fundamental structure of this electrochemical dynamic; cells are thermodynamic open systems to which energy may be introduced from the ambient stream of energy. In addition to oxidation-reduction reactions energy may be introduced to the open system through photosynthesis and still qualify as electrochemical. In the case of photosynthesis the biological cell may still be regarded as an electrolytic cell. The organization of cells is most clearly seen in terms of electrochemistry because that is how the cell captures energy. Archaeobacteria and the early prokaryotes were electrolytic cells dependent upon photosynthesis, fermentation, and surrounding corrosion due to the presence of acids, as in volcanic caldera. Paleobiologically the move from prokaryote or simple, non-nucleated cell to the nucleated eukaryote containing organelles like mitochondria might be seen statistically unlikely. What is actually involved is just the elaboration upon a simple electrochemical dynamic, the move from electrolytic to primary cell. This move involved the embodiment of an oxidative, respirational process, that of the mitochondria.

...one group of bacteria, known as desulfovibrios...generate ATP, the energy molecule, during conversion of sulfate to sulfide. As they breath sulfate, the desulfovibrios synthesized a kind of molecule called a porphyrin ring which passed electrons and generated ATP along the way...Related porphyrin molecules, bright red in color, circulate in our blood today, where they carry oxygen to our cells.

The evolution of photosynthesis is undoubtedly the most single metabolic innovation in the history of life on the planet. It occurred not in plants, but in bacteria. Early photosynthesis was different from that found in plants today. The first photosynthetic organisms were bacteria that used hydrogen gas or hydrogen sulfide for the process and never used oxygen. 60

The highly reactive, unused oxygen given off as waste when water was broken down so that chemical reduction, the combination of hydrogen with carbon and phosphorous, could be triggered photosynthetically, had the effect of oxygenating the planet. It was this oxygenation coupled with electrochemical pressures which resulted in the appearance of the eukaryotes which then used this oxygen in mitochondrial respiration.

Aerobic respiration, the breathing of oxygenation, is an ingeniously efficient way of channeling and exploiting the reactivity of oxygen. It is essentially controlled combustion that breaks down organic molecules and yields carbon dioxide, water, and a great deal of energy into the bargain. Whereas fermentation typically produces two molecules of ATP from every sugar molecule broken down, the respiration of the same sugar molecule utilizing oxygen can produce as many as thirty-six. 61

Metabolism can be divided into catabolism and anabolism, just as a battery has a cathode and an anode. The former involves oxidation or the breakdown of organic molecules by acids in the liberation of energy as electrons. The latter involves the construction of organic molecules through a reductive chemical process. The former can be seen in the functioning and respiration of a cell's mitochondria. The latter in its genetic replication and production of proteins and enzymes.

The electrochemical pressure that drove cells to move from prokaryote to eukaryote, also pressured the eukaryotes to evolve into multicellular organisms as *fauna*, which preceded the development of flora. Within the cell the nucleus had a wall which separated reductive processes from the oxidative mitochondrial processes in the cytoplasm. The mitochondria themselves acted as batteries. These batteries powered the expression of the proteins that would make up the digestive organs such *fauna* relied upon for the capture and creation of energy. The intricate folding of the mitochondrion's internal walls created a number of 'voltaic piles' like those Alessandro Volta worked with in 1800 when he first announced the battery. In multicellular organisms which were collections of neurons, it was the cell membrane which

separated the oxidative/reductive processes therein from the catabolic breakdown of organic molecules in the gut known as gastrulation, and the oxidation of carbon known as breathing. This breathing and gastrulation/digestion, by increasing the extracellular catabolism, provided increased cellular energy by increasing the voltage difference measurable across the cell membrane to the extracellular space, that voltage being necessary for the difference between reduction and oxidation in terms of electrical potential. With this the drive of Darwinian evolution was set off, with the increasingly complex *fauna* merely elaboration upon a fundamental, binary system of energy production and use to power replication, behavior, and autopoiesis.

In his 1982 *The Growth of Biological Thought* Ernst Mayr describes the failure to understand the functional organization of the nervous system as a bottleneck of biological thought that is related to the failure to understand the organization of chemistry that makes life possible. Mayr attributes this failure to the continuing lack of 'certain basic facts'. As shown, the understanding of the functional organization of the nervous system as well as the cell itself is readily understandable in electrochemical terms, something that could have been understood long ago if biology had been more concerned with revision of theory rather than accumulation of data, and had been more attentive to the possible cross-pollination from the physical sciences.

In the essay "The Autonomic Nervous System" is shown a diagram of the nervous system which purports to discriminate between the sympathetic and parasympathetic nervous branches. 62 The autonomic system is called the visceral motor system and is described as 'largely involuntary', an effector system, in contrast to the somatic motor system, usually thought of as the voluntary motor system. Yet the synapses in both systems rely upon the same neurotransmitter, acetylcholine. It is stated that 'certain autonomic adjustments...can be brought under voluntary control with practice'. The line between voluntary and involuntary begins to be blurred. The autonomic system is thought of as the primitive system, being more associated with reptilian portions of the brain rather than the cortex. The autonomic system, evolutionarily, preceded the existence of vertebrates, and even skulls, and can be seen as clusters of neurons, as in worms, and these clusters are always associated with motor activity and the functioning of the viscera or digestive organs. In the embryo the division of that cell mass into three segments puts the same cells in the endoderm (the brain and neural tube) that line the stomach.

The understanding of the functional organization of the nervous system is no longer something which awaits, as Mayr would have it, more facts. Instead revision is called for. But this is unlikely in a quasi-scientific milieu in which there is a struggle for academic influence, status and prizes that are not based upon hypothetico-deductivism and refutation but upon tenure and publication. Lorenz Oken, whom Stephen J. Gould discusses in his "The Rule of Five", is characterized as a 'fine descriptive anatomist,' though Mayr dismisses him as an essentialist. 63 Oken lived in the first part of the nineteenth century and defined each new step in the complexity of living things as due to the addition of an organ or muscle. Of course this entails increased numbers of neural groups, microtubules, nerve terminals, presynaptic peripheral boutons, etc. Ernst Mayr made his reputation in the 1940's overturning the ideas of an evolutionist named Richard Goldschmidt who claimed that there were such things as *systemic mutations*, and this accounted for the appearance of new organs and muscles, what is called macroevolution or changes in phenotype. For Goldschmidt there were such things as 'hopeful monsters' which manifested systemic changes which distinguished them from others of their species in some drastic way. Mayr countered that the pace of genetic change was slow and continuous, and the idea of systemic changes was anathema to the gradualism which he preached was part of Darwinism. Stephen J. Gould writes:

The conflicts between adherents of rapid and gradual change had been particularly intense in geological circles during the years of Darwin's apprenticeship in science. I do not know why Darwin chose to follow Lyell and the gradualists so strictly, but I am certain of one thing: preference for one view or the other had nothing to do with superior perception of empirical information. On this question, nature spoke (and continues to speak) in multifarious and muffled voices. Cultural and methodological preferences had as much influence upon any decision as the constraints of data. 64

The extreme rarity of transitional forms in the fossil record persists as

the secret of paleontology. The evolutionary trees that adorn our textbooks have data only at the tips and nodes of their branches; the rest is inference, however reasonable, not the evidence of fossils. Yet Darwin was so wedded to gradualism that he wagered his entire theory on a denial of the literal record. 65

This gradualism, coupled with the idea that relative brain size and weight was an indication of cerebral superiority, has been used by bigots to 'scientifically' support racism. The argument is made that certain members of the species *homo sapiens* could be closer to chimpanzees or other members of the primate family on the continuum than other, more advanced models of the same species. An excellent discussion of this sort of pseudo-science made possible by the idea of genetic gradualism can be read in Stephen J. Gould's *The Mismeasure of Man*. The quantification of the functional organization of the nervous system is more in keeping with the ideas of Goldschmidt and the 'punctuated equilibrium' of Gould and Niles Eldredge even though its applicability cannot be easily studied with regard to fossils since it concerns itself with soft tissue. The level of nervous system complexity is indicated more clearly in whole numbers by the number of axons which emerge from the nervous tissue to innervate muscle and organ and gland rather than cranial volume "In one respect Gould and Eldredge differ fundamentally from Mayr. They maintain that punctuated equilibria are produced by discontinuities of such size that they correspond to Goldschmidt's hopeful monsters: Macroevolution proceeds by the rare success of these hopeful monsters, not by continuous small changes within populations". 66

Continuous small changes of a genetic nature do not quite account for the larger changes in the system that are manifested by the appearance of a new neurotubule/ microtubule and pre-synaptic bouton, and a correspondingly new muscle or organ. It is increasing numbers of emergent fibers from the nervous matter and not relative brain size and weight that is a truer indication of nervous system complexity. And this most definitely entails what can only be termed a systemic mutation. That is one way punctuated equilibria may show up, especially as a form of vertebrate speciation. The emerging tool-making and linguistic capacities amongst hominids is inescapably caught up in considerations of refinements in the muscles that it takes to do such things as make tools, throw overhand, and communicate linguistically.

The story goes that 98% of the genes of humans and chimpanzees are shared, but this cannot be taken as an argument that chimpanzees have 98% of the nerve terminals that human beings have. Chimpanzees lack chins, opposing thumbs, and shoulders that are rotatable. What it takes to provide these things is an increasingly large brain. Murray Gell-Mann writes, "Those comparatively few genetic changes that permit an apelike creature to develop language, advanced thinking, and elaborate culture, all manifesting great effective complexity, have greater significance than most comparable sets of alterations in the genetic material." 67 The question becomes: what is the nature of the genetic shuffling that results in the appearance of a new neural net and the microtubule that flows from it to a presynaptic bouton peripherally? Anyone wishing to understand the influence of genotype on phenotype must address this. The equality of the human species, as Gould observes, is a contingent fact of nature, and it is most assuredly dependent upon the similarity in number of emergent microtubule/axons distributed throughout the 1,152 motor endplate regions, ganglia, and plexuses of the human body.

In his 1992 *Bright Air, Brilliant Fire: on the Matter of Mind*, Gerald Edelman, Nobel Laureate, director of the Neuroscience Institute, and chairman of the Department of Neurobiology at the Scripps Research Institute, insisted that biology and physiology must now turn to the study of how the brain is organized. He writes:

What we need to understand are the rules connecting the ways in which genes are sorted and expressed with the ways in which genes lead to changes in the phenotype. p.44.

The nature of this relation is the outstanding central riddle of modern biology - that of morphologic evolution. p.46

What we want to know is how alterations in form, either in the whole animal or at microscopic levels of brain, muscle, or bone affect behavior, and how behavior alters form. This is the part of Darwin's program that

remains largely incomplete. p.48

To see why this problem is so important, one need think about the extraordinary evidence from fossil data on hominids indicating the large increase in hominid cranial capacity and brain size that has occurred over less than a million years of evolution. p.48

What is the connection between overall morphology and behavior and the microscopic morphology of the brain? How do these evolutionary developments connect with the behavior of hominids in groups and with the development of language? p.49

It is hoped that this paper adequately addresses these issues in such a way as to suggest not only the direction of further research with regard to understanding the relation between genotype and phenotype, but also suggests a mode of treatment involving the transcutaneous delivery of negative electrical charge to the numerous motor endplate regions, ganglia, and plexuses that will provide a remedy for chronic paralysis, all manner of trophoneurotic conditions, and the degenerative diseases of aging.

1 Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution and Inheritance*, Belknap Press, 1982, p.33.

2 Mayr presented three objectives of science: (1) "...to organize knowledge in a systematic way, endeavoring to discover patterns of relationship among phenomena and processes," (2) "to provide explanations for the occurrence of events," and, (3) to propose "explanatory hypotheses that must be testable, that is, accessible to the possibility of rejection."

3 Essentialism was the foundation of the philosophy of Plato which emphasized that there was a fixed number of unchanging forms or essences that exist independently of the objects which possess them. Variation was the result of flawed manifestation of the underlying essences.

4 Mayr, 1982, p.22

5 Lewis Thomas, *The Youngest Science - Notes of a Medicine Watcher*, Viking Press, 1983, p. 28-29.

6 Lewis Thomas, p.69-70.

7 Medawar, *The Threat and the Glory: Reflections on Science and Scientists*, 1991.

8 .I. Bernard Cohen, *Revolution in Science*, 1985, p.38

9 Sir William Crookes, in 1855, began using the recently developed vacuum tubes to produce what he called, in 1876, cathode rays which would later lead to the discovery of the electron since what these rays were was a stream of electrons. His apparatus became known as a Crookes tube.

10 Abraham Pais, *'Subtle is the Lord...': The Science and the Life of Albert Einstein*, Oxford University Press, 1982, p.85.

11 Julius Comroe, *Exploring the Heart*, 1982, p.232.

12 This led, as will be seen, to the 'fight/flight' model of nervous functioning which, the one time it was tested, was found wanting, but which is still taught today.

13 *Therapeutic Electricity and Ultraviolet Radiation*, ed. Sydney Licht, 1969.

14 .Patricia Churchland, *Neurophilosophy*, 1986, p.165.

15 Starr, *The Social Transformation of American Medicine: the rise of a sovereign profession and the making of a vast industry*, Basic Books Include., 1982

16 Simon and Schuster, 1940

17 *I Remember*, p.174

18 "Membrane Potential", John Koester, p.84-85, in *Principles of Neural Science*, Kandel, Schwartz, Jessel, 3rd edition, Appleton and Lange, 1991

19 .Earlier in 1887 Walter Gaskell at Cambridge mapped out the sympathetic and parasympathetic nervous systems and re-enforced the idea that the two systems functioned antagonistically because he found that the two systems innervated the same muscles and organs. Yet he attached no significance to the fact that the sympathetic nerve fibers were post-synaptic while parasympathetic fibers terminated in synapses. The reason he attached no significance to this was probably because he was not aware of the function synapses served, though they had been deduced to exist in 1879 by Sir Charles Sherrington who

could not observe a spark when the nerve impulse reached the contracting frog muscle. Sherrington later observed the synapse with microscopy, but no attempt was ever made subsequently to map out these synapses over the body, nor was their occurrence in the nervous system thought to have any bearing on the idea that the two systems were antagonistic. Instead the neurophysiological collection of facts and artifacts of research only increased, and speculation about how the whole thing functioned remained unrevised or reexamined. And so, as cited above, Lewis Thomas writes of the 1930's that still at this time no one had any idea how the system worked. Even to this day the role of the chemical synapse in amplification of the power of the nerve impulse is poorly understood.

20 "Neurotransmitters", Julius Axelrod, *Scientific American*, June, 1974

21 Comroe, p.250-52

22 There is an observed acetylcholine-mediated secretion of epinephrine triggered by the parasympathetic nervous system acting as a result of sensory inputs.

23 Hille, 1991, p.2.

24 Alexander Hellemans and Bryan Bunch, *The Timetables of Science*, Touchstone, 1988, p.409.

Many of the historical references not cited in this paper are taken from this book.

25 *Principles of Neural Science*, 1991, p.84

26 Hille, 1991, p.2.

27 *Principles of Neural Science*, 1991, p.84.

28 Marvin Harris, *Cultural Materialism*, Vintage Books, 1980, p. 14.

29 Marvin Harris, *Cultural Materialism*, Vintage Books, 1980, p. 14.

30 Harris, p.14-15.

31 Koester, *Principles of Neural Science*, "Membrane Potential", p.82.

32 "Realism and Instrumentalism," Paul Feyerabend, *Realism, Rationalism, and Scientific Method*, Cambridge University Press, 1981, p. 185.

33 "The Two Kinds of Deductively Formulated Theory", in *The Logic of the Sciences and the Humanities*, Meridian Books, 1959, p.102.

34 Hille, p.6.

35 Hille, p.2.

36 *Principles of Neural Science*, 1991. p.251.

37 quoted in Hille, p.29.

38 *Scientific American*, May, 1980.

39 Hille, p.8.

40 Hille, p.24.

41 Hille, 1991

42 Pais, p.241.

43 *The Quarterly Review of Biology*, 1935, no.10, p.322.

44 Northrop, 1959, p.222.

45 Northrop, 1959, p. 162.

46 *Principles of Neural Science*, p.545.

47 *ibid.*, p.246.

48 *Archives of Neurology*, Feb.

49 *Principles of Neural Science*

50 Hille, 1991.

51 *Principles of Neural Science*, p.935.

52 "Cell and Molecular Biology of the Neuron", James H. Schwartz, in *Principles of Neural Science*, p. 60-1.

53 Schwartz, p 60-61.

54 "Effect of Prednisolone on Motor End-Plate Fine Structure: A Morphometric Study in Hamsters", Sudhansu Chokroverty, *Annals of Neurology*, April, 1978.

55 Schwartz, p.63.

56 Bass, Thomas A, *Reinventing the Future*, Addison-Wesley Publishing Co., 1994

57 *Paradigms Lost*, John L. Casti, 1989, p.68-9.

58 Penrose, Oxford University Press, p.359. In Dr. James Schwartz's "Synthesis and Trafficking of Neuronal Proteins" (*Principles of Neural Science*, p.63) the microtubule is described thusly:

Microtubules...are helical cylinders composed of 13 protofilaments each 5 nm. in width. Protofilaments are linearly arranged pairs of alternating a- and b-tubulin subunits...A tubulin molecule is a heterodimer consisting of one a- and b-tubulin subunit.59

The amplification of power of the nerve impulse across the synapse for striated muscle is traditionally accounted for by appeal to the size of the ACh channels of the immediately post-synaptic cells, those cells purportedly having larger channels to accomodate a greater flow of ions per pulse, and, consequently, a greater flow of ionic current. Biopsies have never been done comparing ion channel size from these cells to any other. Such amplification of the power of the nerve impulse is surely a result of electrochemistry for two reasons. First, the arrival of negative charge draws the calcium ions which break the vesicles of acetylcholine. Secondly, if  $p=VI$  where p is power, V is voltage, and I is current flow, then the neuron's power is amplified by increasing I, not V, and this is done by diminishing resistance to movement of electrons by increased cross-sectional area of the type II fiber over the nerve's microtubule. The comparative size of the two tubules can be judged to verify this explanation.

Penrose writes (p.81), "...when systems more complicated than dilute gases or of large collections of gravitating bodies are considered, it is not likely that one can steer entirely clear of the issues raised by the quantum-mechanical nature of the materials concerned." This has incredible implications for the archaic, Cartesian idea that there is a mind or consciousness at the controls of the body, to the point where this idea is no longer tenable if the epiphenomenalist model of nervous functioning is to be accepted. If quantum events are not predictable as individual events but can only be statistically understood for a large number of such events, and if human behavior or muscle functioning is a result of large numbers of these events (electron flow) activating firings at the neuromuscular junction, than human behavior is also unpredictable and understandable only in terms of statistical likelihoods or occurrences on a bell shaped curve. In this model, free will is only the idea that this behavior is unpredictable, lies on a bell shaped curve; and consciousness is defined not in terms of individuals but the parameters of this bell curve where this curve is the result of population studies and not a quality had by an individual 'mind' or 'consciousness'.

60 *Microcosmos*, p.77-79.

61 *ibid.*, p.109.

62 Jane Dodd, Lorna W. Role, *Principles of Neural Science*, p.763.

63 *The Flamingo's Smile*, 1985.

64 Gould, *The Panda's Thumb*, "Episodic Evolutionary Change", p.180, 1980 The influence of 'cultural and methodological preferences' independent of the constraints of the data or developments in other areas of the sciences and their pertinence for understanding the functioning of neurons has already been addressed.

65 *ibid.*, p.181.

66 Mayr, 1982, p.617.

67 *The Quark and the Jaguar*, Gell-Mann, 1991, p.70