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L'Autre Côté du Miroir (The Other Side of the Mirror) : French Neurophysiology and English Interpretations

A discussion of neurophysiology in France in the early decades of the twentieth century invokes the image of Louis Lapicque, the neurophysiologist at the Sorbonne who rose and fell on his conception of nerve-muscle action which he defined as "chronaxie", (sometimes termed the "strength duration curve"). Lapicque's theory was more than a simple time duration measurement, it was a grand theory of the nervous system which guided a great deal of research in France and throughout Europe. His own investigations extended from 1901 until his death in 1953. He taught and worked with students and collaborators first at the Muséum d'Histoire Naturelle and then at the Sorbonne.[1]

My title "L'autre côté du miroir" reflects (if you will excuse the pun) my original intention to look at the description by Anglo-American researchers of the image of Louis Lapicque. I was struck by the manner in which both recent and contemporary descriptions depict the events in which Lapicque's theory of "chronaxie" was ruled out of international neuroscience by the experiments of W.H. Rushton. This story has been told over and over again with varying degrees of delight by the English nobelist Rushton himself, by the Belgian Z.M.Bacq, by both Americans and English in the 1970s. Very recently, Whitteridge in his review of a hundred years of International Congresses of Neuroscience tells the story once again.[2] The question remains to what degree the same perspective of events existed among both the French and the Anglo-Americans. Was it primarily a retrospective construction? What was the function of this presumed defeat for the neurophysiology community?

As late as the sixties according to one medical student of that period, chronaxie was still taught in the Ecole de Médecine as the central theory of neurophysiology. Recently, the French have adopted the Anglo-American perspective, privately describing the period of chronaxie as "a shameful period of French science." The recent autobiography by Paillard, a psycophysiologist trained immediately after the second world war by Alfred Fessard hails him in the postwar period as "searching to create a modern neurophysiology revitalized by electronic progress in reaction against a French neurophysiology still dominated by the dogmaticism of theories of chronaxie."[3] For the historian a whiff of gunpowder comes across the old battlefield.

The problem for a historian is aggravated by the fact that Lapicque's term chronaxie has not been ruled out of science completely, as the current historians of neurophysiology in England seem to believe.[4] It has been retained in a very limited sense by American, and French text books as a useful method of calculating (and

predicting) the change of the strength duration curves over time. (And I will return to this at the end of the paper.) Nor did Lapicque fail to adopt the technology of his day, as some of the recent comments have implied.

This term,"chronaxie", coined by Louis Lapicque, was initially defined a strength duration curve over time. It provided a cohesive way of describing excitability of many different types of tissues through examination of the duration of these curves. By developing a measure which took a threshold (rheobase) sufficient to fire the nerve. the "chronaxie" (which Lapicque explained as "time-value") by definition was an intensity double the rheobase. The time axis of this curve might be calculated in seconds, tenths of seconds, hundreths of seconds or milliseconds, depending on the type of tissue and the organism, but the curves were identical in shape. Using small electrodes of silver to stimulate, there was what Lapicque termed an "invariable chronaxie". Searching a reason for this, some preliminary work of Lapicque in 1913 with his student Legendre seemed to indicate that in myelinated nerves the diameter of fibers accounted for the size of the chronaxie. "The diameter [of the fiber] was greatest when the chronaxie was smallest, that is to say when the speed was greatest."[5] Other parameters can be varied, or the chronaxie of abnormal tissues can be examined to study disrupted functions. For example, it was possible to study the effect of a variety of nerve and muscle toxins (like curare, strychine etc) on the chronaxie of those tissues.

A useful tool? Yes, but Lapicque was not content with chronaxie as a tool: he believed that this regularity hid a basic theory of nerve function. In a period in the first decades of the twentieth century when constants in physics were proving so fruitful, it is no wonder that the discovery of a constant in physiology would seemed so suggestive. Lapicque was interested in physics, his work in medicine had been in a laboratory of medical physics. He warmly greeted Nernst's ideas of polarization in 1907 and 1909 when Nernst suggested their relevance for nerve action.[6]

Let me give a little of the background of Lapicque and his theory and then sketch only in the most superficial manner what seems to have been at stake in the international community. It is important to point out that Lapicque was a man of the nineteenth century. He was born in 1866 and trained at the end of the last century in medicine and science, obtaining a doctorate in both fields. His medical degree was in Paris, his natural science degree at Oxford. As a young man he was clinical chief in one of the laboratories of the Faculty of Medicine, a prestigious position. Interested in physics of medicine, he also was an associate professor at the Sorbonnne in 1899. For a while he worked as a biological anthropologist with Léonce Manouvrier head of the Société d'Anthropologie, making physical measurements on peoples of Oceania in the course of a long voyage. . Later, from 1911-1919 he directed a laboratory of general physiology at the Muséum d'Histoire Naturelle. and following that, he was Professor at the Sorbonne in a chair of general physiology where he also had a laboratory .His interest in nerve function seems to have been stimulated at the turn of the century by his professor, George Weiss, chairholder in medical physics at the Faculté de Médecine. Later dean of the Faculty at Strasbourg, Weiss in 1901 began to work with Lapicque and another young medical clinician Bourgignon on the interesting curve which could be produced by nerve stimulation. Lapicque made this his life work. Joined by his wife Marcelle de Herrida from 1901, he began to define this more carefully, naming the strength duration function "chronaxie"in 1907. He reached the height of his fame in 1926, when as professor of physiologie générale at the Sorbonne, he published his widely read book, *L'Excitabilité en Function de Temps*. Highly influential in France, especially in Paris where he came to epitomize the science of neurophysiology, his influence began to wane in Anglo-American circles with the systematic attack by A.V. Hill, Henry Dale and their student

W. Rushton in England in the 1930s. At the time of Rushton's attack Lapicque was 67 years old. It is this attack and the response to it, which is the center piece of my paper.

From the beginning of the century, Louis Lapicque worked closely with his wife, who had received a doctorate in sciences for her thesis on excitation times of tissues in a variety of different organisms in 1905. He credited her as an initiator of many of his ideas and techniques. More than half of Lapicque's papers were published jointly with her, but she also published independently with Lapicque's colleagues and students.[7] Lapicque made a definite attempt to underscore her importance in the introduction to his book *La machine nerveuse*. Here he noted "[M}a femme Marcelle de Heredia m'a donné depuis l'origine une collaboration riche en initiatives heureuses."[8] Curiously, her name has dropped out of many of the Anglo-American necrologies. Even Monnier's tribute to his teacher in the *Dictionary of Scientific Biography* fails to mention her. Yet there was already a tradition in studies of the nervous system for husband and wife teams. Jules Déjerine, the leading neuropathologist of the previous generation worked closely with his wife, the first woman interne in France, of American origin.[9] Although I have no time to develop the interesting sidelight, I might add that the field of neurophysiology seems unusually full of husband and wife teams, in America and Germany as well as a number more recent examples in France.[10]

From 1903 to 1938, Lapicque had an enormous influence, and his theory was at first seen as stimulating to research. Scientists from America, Belgian, Japan, Ireland and South America came to work in his laboratory in the 1920s and 1930s and to learn his techniques. Among the best known to Americans was Herbert Gasser, then a student, later a collegue of Joseph Erlanger of Washington University, St. Louis, with whom he won the Nobel prize. With a grant from the Rockefeller Foundation (through the National Research Council) Gasser went to study in Europe in 1924, and spent some time in the laboratory of A.V. Hill as well. Gasser worked in Lapicque's lab for part of 1924, where he examined nerve fibers "to see", as he wrote back to Erlanger, "if there is anything in the relations between size and function" [11].

A.V. Hill encouraged Gasser and Erlanger to develop the oscillograph for studying the nerve impulse " action potentials" which they would demonstrate so dramatically in 1929 at the International congress of Physiology in Boston and which received heavy funding from the Rockefeller. Rockefeller money also allowed a Lapicque student, A.M. Monnier to go to St. Louis to learn these new oscillograph techniques in 1929-1930. Monnier, returning to Lapicque's laboratory, received significant annual funding from the Rockefeller Foundation until 1938 first to purchase a Gasser-Erlanger oscillograph and later to develop his own electronic equipment in Lapicque's laboratory.[12] It is therefore incorrect to say, as a recent scientist has claimed, that Fessard in the post-war period was the first French scientist to use the new electronic instruments.[13]

H.H.Jaspar, came from America to work with Monnier and Fessard, in Lapcique's laboratory. Jaspar obtained a French degree during this period. Later he made his name for his work on electronic recording of Penfield's experiments on the brain in Montreal. A Belgian neurophysiologist Z.M. Bacq, who had developed the first hints of chemical release of noradrenaline at the synapse with Walter B. Cannon in America, also worked with Monnier in Lapicque's laboratory. According to Bacq, Lapicque was more welcoming to the ideas of foreigners in his laboratory than to those of his collegues and students. Bacq explained the new ideas of chemical transmission at the synapse to Lapicque with some trepidation in 1934, and to his surprise found Lapicque publicly conceding to his students and collegues that he was convinced by Bacq that this occured at least at the autonomic nervous system. He was assured by Lapicque's collegues that this sort of concession would never have made had Bacq not been a foreigner, since

Lapcique was not willing to listen to challenges from his own students.[14]

Why did the English particularly see Lapacque as a source of irritation? Why was it so important to eliminate his theory? Chronaxie would appear to have been a useful theory for research since in 1933 and 1934 hundreds of papers were generated on this subject. One reason seems to have been Lapicque's incorporation of the totality of current experimental evidence under the rubrique of his grand theory. He had criticized Sherrington's great work on the reflex arc as simplistic, suggesting that the concept of coordination at the spinal level should be rethought in terms of interactions within the spinal cord. He also denied any real importance in the role of the synapse in an era when chemical vs. electrical transmission at the synapse and the role of the sodium pump in effecting polarization and depolarization were seen to be the "hot topics."

The electrical analogy of the nervous system had been an important one in physiology since the work of Hermann Helmholtz on the measurement of the nerve impulse in the mid 19th century, refined and defined by DuBois-Reymond and others. DuBois-Reymond, friend and colleague of Helmholtz had in 1877 also hinted at the possibility of a chemical mode of neural transmission. That alternative possibility led to much heated debate over the next 70 years. Nerve and muscle measurements were expanded by many new electrodes and stimulation devices and recorded first by the traditional smoked drum and then by Marey's stimulation and recording devices, and serial photographs. By the 1920s new oscilloscope techniques (using the cathode ray tube) permitted one to visualize the action potential of the nerve.[15]

There had been a parallel development of interest in time-constants and strength duration curves in England, with the work of A.V. Hill and Keith Lucas. Lucas had developed a strength duration curve, and as early as 1910, Hill had developed a time constant tro which he gave a mathematical expression. Lapicque, while he gave extensive credit to British investigations in his courses at the Sorbonne, discussed the British in his articles and books primarily in connection with his own ruling idea of chronaxie.[16]

In 1930, a major challenge to Lapicque's work came from England. A young neurophysiologist, W. H. Rushton began to publish a series of articles in the *Journal of Physiology* [of London] challenging chronaxie as a useful theoretical concept. Lapicque began to answer the criticisms in the same journal, convinced he had done so successfully.

The young Rushton then challenged Lapicque in person at the Fourteenth International Congress of Physiology in 1932. This event was later reported by a large number of observers as the highlight of the meeting. Lapicque "in his morning coat" spoke on "metachronosis", his new term for changes in chronaxie following drugs or from one cell affecting another. According to Witteridge's account: "By drugs, Lapicque meant curare which causes failure of conduction from nerve to muscle and which he thought to lengthen the chronaxie of muscle. According to Lapicque whenever the chronaxie's of two tissues differ by a ratio of more than 1: 2 condition between them fails. Rushton then pointed out that in Lapicque's earlier work he had not measured the normal chronaxie of muscle, since he had used the frog's gastrcnemius and had made no attempt to use an area free of intra-muscular nerve branches. What he thought was the chronaxie of muscle was in fact that of intra-muscular nerve branches. Following Keith Lucas, Rushton had used sartorius which has a considerable nerve free area. The normal chronaxie of muscle, or as Keith Lucas had it its excitation time is much longer than that of nerve and is unchanged by curare [!] There was an unprofitable discussion of electrode sizes, as Lapicque maintained that the name chronaxie should be limited to measurements made with small electrodes and that values obtained with Rushton's large

electrodes should be called "excitation times" It was clear that Lapicque's theory of "isochronism" of equality of chronaxie of nerve and muscle was under grave suspicion. The sequel should not be unexpected as scientists are not always open minded and an immense amount of work had been done on chronaxies of all excitable tissues by Lapicque and his many pupils. Rushton was booed. Fredericq and others poured scorn upon him, but he was stoutly defended by J. F. Fulton."[17] So runs Whiteridge's description, but the story does not end there.

Rushton followed up this attack with a long paper in the *Journal of Physiology* in which he systematically repeated his attack on Lapicque's studies of curare using the concept of chronaxie, concluding: "If then chronaxie does not play the important physiological role that has been attributed to it, if it is not the pass sign of nerve-muscle condiction, what finally is the value of this measure?".[18] He added that since Lapicque had not wanted the term used for large fluid electrodes. "If we mean [by chronaxie] a measure satisfying all Lapicque's recent restrictions...then clearly the utility of chronaxie will be limited".[19] Rushton substituted the term 'excitation time" instead (which one might note is not a constant in the same sense as Lapicque;s term. Although Rushton's paper explicitly attacked only Lapicque's study of curare using chronaxie, it was greeted as a dismissal of all of Lapicque's work.

Reading Rushton's paper today, it seems like a model of experimental argument and proof. Yet there are some questions which are raised by it. The fluid electrodes of the size which Rushton (and Lucas) used do seem to give a very different value for the "chronaixe" or strength duraiton curve. Rushton's dismissal of Lapicque's interpretation of the action of curare and by extension all of Lapcique's work includes the doubtful insistance by Rushton that curare acts "on the nerve not the muscle."

Perhaps the story has been oversimplified in retrospect. The importance of the concept of chronaxie in international science in the mid to late nineteen thirties is shown by the enormous numbers of references under that term, and related terms in the *United States Surgeon General's Catalogue* for 1938, in German, Italian, Spanish, Roumanian as well as English and French, most of them dating from the period between 1930 to 1936.[20]

Had Lapicque gone down to defeat with the finality of the retrospective accounts? It was only some years later that Lapicque understood how absolutely the English had slammed the door on his theory. At the urging of Z.M. Bacq and A. Fessard who were in Plymouth England in 1936 at the time,. the English physiologist A.V. Hill invited Lapicque to cross the channel on his yacht, "The Axon" to visit the Marine Biology laboratory there. According to Bacq, Hill was surprised find that he, his wife and a single sailor would have navigated the Channel in that way. to which Lapicque replied "My dear colleague, you never believe anything I say." the result, however was some serious discussion between Hill and Lapicque.[21]

Lapicque reviewed his discussions with Hill in a note published in the *C.R. Académie des Sciences* in 1937. He reported the discussions as friendly although the "eminent English physiologist had a prejudice against my conceptions". He had declared to Lapicque that "he would have no more theoretical objections to make if they were presented in a different form the principle of which he indicated to me. In the correspondence which followed our accord appeared more difficult and I have not succeeded in finding the exact form acceptable to M. Hill." (s there more than a touch of sarcasm here. Or a real wish to mend fences?. He continued "It appeared worthwhile to me to try, once more to clarify our differences and for that reason each to explain his point of view publicly."[22] Lapicque explained his objections to Rushton's experiments once again, and rehearsed his own point by point dismissal of them. In a follow-up note

on isochronism, he more fully explained Hill's suggestions that chronaxie should, to be truly useful, include not only time but amplitude of impulses. But Lapicque argued that this was implicit in his concept of para-resonance which his collegue Monnier had examined. He argued that the elevation of the rheobase of the muscle could very adequately explain curarization.[23]

Lapicque also had some correspondence with Henry Dale, the strong advocate of chemical transmission in England to whom he insisted that it was necessary to combine the two theories in order to explain nerve transmission. But for Lapicque, who had questioned the concept of the synapse repeatedly, this concession was accompanied by a comment that "the theory of a chemical intermediary has grave problems." In this he was not alone since John Eccles was hotly contesting Henry Dale's chemical transmission at the same time.[24]

It was hard for Lapicque to accept the challenge to his ideas because for him neither chronaxie nor isochronism were theories. They were "quantitative facts independant of all hypotheses".[25] Chronaxie, he explained once more was in a limited sense only a particular measure of physiological time, but Chronaxie in the larger sense included by definition "the nervous impulse and the process what ever it may be by which this impulse exites the following element" [26]. Hill had jokingly called this Lapicque's "credo"in their discussion, but to Lapicque his theory was only a "generalization from known facts"[27] Lapicque insisted that apart from the explanatory power of chronaxie and isochronism ("large although not unlimited") his ideas had "the merit of opening up new directions of research, the fertility of which is not yet at an end".[28]

Lapicque was wrong. Chronaxie as a theory was more or less dead in the water. The new concepts of polarization, of measurement of action potentials and chemical transmission made the older theory too restrictive. Lapicque's little book, *La Machine Nerveuse* written in part when he was imprisoned by the Gestapo during the Second World War recounts the earlier history of chronaxie, but refers only in a footnote to the challenge "by the English school of physiology".

After the war, when French science was revived, Alfred Fessard was made the head of neurophysiological research at the College de France, taking a chair specially created for him. He, not Monnier now received heavy funding from the Rockefeller Foundation. Lapicque's old laboratory at the Sorbonne, headed by Monnier had become something of a backwater. Lapicque in the late 1940s, still took up the question of curare, still argued for the validity of the research done ten years before. He made major concessions on the subject of the synapse, but too late. The community was no longer listening. [29]

This story like all stories may have a moral, but I am suspicious of morals. One might say for example that when a theory explains too much, it also fails to focus on new questions in a useful manner, as occured in Lapicque's effort to absorb both chemical and electrical transmission under "heterochronism" The theory seems to weaken as it becomes a dogma as well as a credo. But how is the researcher who has devoted his life to these ideas to recognize that his fruitful hypotheses have become too rigid and are choking the young crop of ideas?

Yet one wonders if Lapcique had made concessions to Hill, if he had agreed to the more limited application of his terms, if he had incorporated the new research into his theory, expanding it and making it flexible, whether the events of the Rushton- Lapicque encounter would have been recalled with in the same manner. We must not forget that Lapicque was now seventy, the events of the Second World War obliterated many of the final arguments, not all of them faulty, on Lapicque's side.

In contrast, another scientist in danger of finding himself marginalized in neuroscience by the English community, publicly underwent a spectacular public conversion. The future nobelist John C. Eccles, who had been in Sherrington's lab in the 1930s found himself very much in the provinces, teaching during and after the Second World War in New Zealand where he met Karl Popper. He used his friend Popper's philosophy of scientific "falsification" to facilitate an abandoning of his ardent defense of electrical transmission at the synapse, and adopted chemical transmission, to public acclaim.[30]

Perhaps the story of chronaxie would not be as interesting if there had not been a recent resurgence of the term with multiple recent citations to Lapicque in America. L.A. Geddes and his co-workers at Purdue have found it a useful predictive measurement in the field of electroencephalography, and published a number of papers between 1985 and 1989 using Lapicque's term. Admittedly, it is no longer the center piece in a systematic theory of nervous activity.[31] In England, however, even the term seems to have dropped out of the literature. Something suspiciously like Lapicque's chronaxie curve is illustrated in a very recent edition of English book on *Nerve and Muscle* by R. D. Keynes and D.J. Aidley, but here the citation is to the very article in which Rushton attacked and demolished Lapicque and the theory of chronaxie.[32]

The study of failed theories in biology, may illustrate much more clearly than any "success" the role of theoretical structures in science, their usefulness as a heuristic, and the dangers of retaining a theory too long after its usefulness has been shown to be limited. The complication of this story is the nature of that so-called 'defeat' and the surprising nature of the triumphant delight as the community of neuroscientists have retold the story of chronaxie. Like Fraser's accounts of the death of the fertility god every spring, it has the aura of myth. It may recount not simply the defeat of a theory but the triumph of youth over age, of new science over old. The other side of the mirror may not reflect the Anglo-Saxon world looking at French science, or French science looking backwards at itself, but the old scientist looking in the mirror and refelcting on his past, seeing himself young again vanquishing the old king.

Notes

[1] A.M Monnier

[2] See especially the autobiographical recollections of Rushton, Gerard, J.Z. Young, Jaspar and others in F. O. Worden, Judith Swazey, George Abrams Eds. Neurosciences, Paths of Discovery. \M.I.T. Press 1974. and Ralph Gerard , "Neurophysiology Diary and Summary Report of a Survey of European Science in Relation to Neuropsychiatry for the Rockefeller Foundation May 14, l935" (444 pages plus appendices and index) Rockefeller Foundation Archives RG 6.1 Series 1.1.(700a), Box 18, File 130, 131. I have looked at the role of the Rockefeller Foundation in funding a certain kind of neurophysiological research in a talk for the History of Science Society, "The Rockefeller Foundation's Program in Neurophysiology: Politicis and Instrumentation in the Medical Sciences", Seattle, Washington, October 1990. See D. Whittbridge's account below for a very recent retelling of the story

[3] "Autobiographie de Jacques Paillard" in Parot et Richelle pp 177-206 [p. 178] which says of Fessard and his colleagues "cherchait a imposer une neurophysiologie moderne, renouvelée par les progres de l'electronique, en reaction contre une physiologie franiçaise encore dominee par le dogmatisme des theories chronaxiques."

[4] Tillie Tansey for example in England, working on Adrian's papers at the Wellcome Institute for the History of Medicine, London has has expressed this belief as does D. Whitteridge (former prof Physiology Edinburgh and Oxford) in One hundred years of Congresses of Physiology. (Published by the International Union of physiological sciences Finland 1989.

[5] Louis Lapicque "Chronaxie" Chapter XI 220-239 in L'Excitabilité en Fonction du Temps Presses Universitaires de France, Paris 1926 (The book carries the sub-title "La chronaxie, sa signification et sa mesure").

[6] See for example Louis Lapicque, "Recherches quantitatives sur l'excitation electrique des nerfs traité comme une polarisation" J de Physiol et Path gen 9 (1907) 620-635. "Conditions physiques de l'excitation electrique etudiees sur un modele hydralique de la polarisation" J de Physiol et de Path gen 11(1909) 1009 and 1035 He cites in his 1926 book a series of studies of Nernst including Zur theorie der galvanischen Polarisation... Sitz ber der K Preuss Akad der Wiss I^{e} sem. p. 1

[7]Her thesis was on electrical excitablity of muscle in vertebrates and invertebrates (1905). Her publications with J. Weill, C. Veil and M. Nattan-Larrier are also cited.

[8]Louis Lapicque, La machine nerveuse Flammarion 1943 p.7

[9]Augusta Klumpke of American origin succesfully won the right with Blanche Edwards (grandaughter of Henri Milne-Edwards) to compete for the internat. Dejerine, like Lapicque, credited her work in his prefaces and on his title pages and continued to work closely with her until his death.Augusta Klumpke became a member of the Societe de Biologie only after his death in the 1930s.

[10]Lapicque's student Monnier worked closely with his wife Andree Monnier. Henri Piéron worked with his wife Mathilde in his laboratory. Even more recently, Alfred Fessard and his wife Denise Albe-Fessard have been important models of cooperative work.

[11]Nov 27 1924 H. Gasser to J.Erlanger in Joseph Erlanger Papers Washington University, St Louis, Missouri. Later the two would demonstrate this successfully with their new electronic equipment. Erlanger, Joseph and H.S. Gasser The Action Potential in Fibers of Slow Conduction in Spinal Roots and Somatic nerves. Am J Phys 92 (1930) 43-81Lapicque says that the fundamental basis for work Gasser completed with Erlanger on relationship between fiber diameter and speed of impulse begun in his lab which Gasser then began to question. Erlanger seemed to reluctant to credit Lapicque for his first suggestion of this in 1913 with Legrande but Gasser answered him in 1934 Dec 11 " I know on all accounts that Lapicque and Legendre's evidence for association of chronaxie and fiber size is wrong. They made the association of properties and size on a fallacy but nevertheless they made the association and it takes paragraphs to explain the history since." (Erlanger papers)

[12] "The Physiology laboratory of the Sorbonne had been since the end of the war [WWI] mainly devoted to neuromuscular physiology. Research had been essentially based upon the methods of electrical excitation. The director of this laboratory, Professor Lapicque felt the need of using another line of approach to the problem i.e. recording of electrical activity of tissue. It is (sic) at that time (1929) that he advised me to study the latest developments in this technique. A Rockefeller Fellowship in America gave me the opportunity to work with Dr. Gasser and Dr. Erlanger and to become acquainted with this new field open to research... For four years this equipment revealed itself very satisfactory. In 1930 a complete Cathode ray recording outfit was installed in our office". Ali Monnier is listed in 1936 as Maître de Recherches, Laboratoire de Physiologie generale, Sorbonne, University of Paris. Note his request for funds for making his own oscillograph and related equipment 11,000 francs." . He added at the end of his report: "Au fur et à mesure de ces recherches, les conditions de leur réalisation apparaissaient plus clairement : le succès dans la recherche en électrophysiologie ne depend pas de l'existence d'appareils achetés à grands frais mais de celle d'un equipement realisé avec l'aide de chercheurs specialisés. Cette intervention est absolument decisive dans la réalisation d'expériences qui requiérent la mise en service d'instruments de plus en plus sophistiques." See Ali Monnier, Report to Rockefeller Foundation(1937) in Rockefeller Foundation Archives RF/ 6.1 series 1.1. box 3 folder 37. By 1938 Monnier received a final 'terminal ' grant of 45,000 francs, after receiving grants each year for specified equipment.

[13] See Jacques Paillard in Françoise Parot et Marc Richelle (directeurs) Psychologues de Langue Francaise Paris: PUP 1992

"[Fessard] m'expliqua le principe de ces nouvelles methodes d'exploration des activites electriques des muscles et le fonctionement de ce merveilleux appareil qui constituait l'oscilloscope a faisceau cathodique dont il fut, je crois, le premier utilisateur dans le recherche biologique en France." p.182-3] Monnier, however adapted the instruments for research on chronaxie measurements. Fessard became a very important figure in international neuroscience. Among other works, Fessard edited Handbook of Sensory Physiology III/3 and wrote "Physiology of electroreceptors".I Introduction pp 60-64 and II "Peripheral mechanisms of electroceptors in teleosts 64-95.

[14] Z.M. Bacq Chemical Transmission of Nerve Impulses : A historical sketch Pergamon Press Oxford, New York, Toronto 1975 (French edition 1974)

[15]Gasser and Erlanger who developed this instrumentation most completely received a Nobel prize, although the apparatus seems to have been envisioned first by Hill. They demonstrated their apparatus publicly at the Boston 1929 meeting of the International Congress of Physiology.

[16] Note the limited referencing Lapicque made to the work of the multiple groups in England actively engaged in neurophysiological work, after Charles Sherrington.(It may be significant, for example that only the first of A.V. Hill's studies is cited here and that one was from 1910 : A.V. Hill "A new mathematical treatment of changes of ionic concentration in Muscle and Nerve under the action of electrical currents with a theory as to their mode of excitation J of Phys v.20 (1910) 190-224. In 1943, in his book La Machine Nerveuse, Lapicque mentions the English "ecole physiologie" as though it were a single group, and hints at Rushton's objections without naming him.

[17] D. Whitteridge [former professor of Physiology, Edinburgh and Oxford) One hundred years of Congresses of Physiology. (Published by the International Union of physiological sciences Finland 1989
p. 23 He goes on to compare this reception of a "newcomer" with the much pleasanter reaction by Alan Hodgkin when challenged. Whitteridge's account written in 1989, was given to him by H.M. Sinclair.

[18].W.A. H. Rushton "Lapicque's theory of curarization" J Physiology Lond 44 (1933) 337-364.

[19] Ibid

[20]"Chronaxia" U.S. Surgeon General's Catalogue Index Medicus Vol "C" Washington D.C.1938

[21] Z.M. Bacq Chemical Transmission of Nerve Impulses : A historical sketch Pergamon Press Oxford, New York, Toronto 1975? (French edition 1974).

[22] L.Lapicque and M. Lapicque, CR Acad des Sciences 205 (1937 p. 502-[503] His note on isochronism is CR Acad Sciences 205 (1937 530-541.

[23]Ibid.

[24]See Bacq's account above. The correspondence of Lapicque with Henri Piéron may illuminate this period of L's thought see Papiers de Henri Piéron, Archives Nationales AP 520 /7 and the papers of A.V. Hill in Churchill College, Cambridge University.

[25] Louis Lapicque CR Acad Sciences 205 (1937 530-541 see p. 533
[26] Ibid. " l'influx nerveux et le processus quel qu'il soit, par lequel cet influx excite l'element suivant."

[27] Ibid p. 533.

[28] Ibid. p.534. The quotation is the last sentence of the paper.

[29] See for example the papers by Lapicque which cites further theoretical work with A.M. Monnier in 1947, 1948. Louis Lapicque "Sur la curarisation preventative dans la convulsotherapie psychiatrique." Bulletin Academie de Médecine 132(1948) 136-142. He avoids the reference to chronaxie here, however. Monnier's later work, after the death of his "spiritual father" in 1951 seems to have broken some new ground, and he became the editor of Actuelles neurophysiologiques from 1959 and lectured in England, America and South America in the mid 1950s.

[30] John C Eccles, "The Synapse from Electrical to Chemical transmission", AnnRev Neurosc 5(1982) 325-339. Bacq's account of this carries with it a touch of sarcasm about the usefulness of Popper's philosophy in the conversion, making a virtue out of a change of heart.

[31] Konrad, L.A. Geddes, W.A. Tacker jr., D. Reuter, D. Schooler and S. Dull,"Existence of a strength-duration curve for spinal cord motor evoked potentials in cats" EEG and clinical Neurophysiology 74 (1989) 463-468; L.A. Geddes and J.D. Bourland The Strength Duration Curve. IEEE Trans Biomedical Engineering 32 (6) 458- 1985; G.A. Mouchawar. L.A. Geddes. J.D. Bourland and J.A. Pearce "Ability of the Lapicque and Blair Strength-Duration Curves to fit Experimentally Obtained Data from the Dog Heart."IEEE 36 (9) 1989 971-974. The three authors are with Hillenbrand Biomedical Engineering Center, Purdue (Indiana)] See an article by a group from Strasbourg, Pierre Schmitt, Guy Sandber, Pierre Kali "Escape and Approach induced by Brain stimulation :a parametric analysis Behavioral Brain Research 2 (1981) 49-79. This is from Lab de Neurophys Centre du Neurochimie CNRS Strasbourg.

[32] R. D. Keynes and D.J. Aidley, Nerve and Muscle, Cambridge University Press, 1991, (2nd edition). They cite to W.A. H. Rushton "Lapicque's theory of curarization", J Physiology Lond 44 (1933) 337-364.