HANDBOOK OF ELECTROENCEPHALOGRAPHY AND CLINICAL NEUROPHYSIOLOGY

EDITOR-IN-CHIEF A. RÉMOND

VOLUME 1

Appraisal and Perspective of the Functional Exploration of the Nervous System

EDITOR: W.A. COBB
The National Hospital, London (Great Britain)

PART A

Evolution of Clinical Neurophysiology since Hans Berger

PART B

Frontiers of Neurophysiology

PART C

Value of Clinical Neurophysiology

ELSEVIER

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Preface

This Handbook, presented by the International Federation of Societies for Electroencephalography and Clinical Neurophysiology, offers a comprehensive, integrated account of the past and present-day knowledge of the electrical activity of the nervous system. Conceived primarily to provide a reference guide for the various specialists in the neurological sciences, particularly clinical electroencephalographers and electromyographers, neuropsychiatrists, and scientists in fundamental and applied neurophysiology, it should also serve as a teaching tool for students the world over, especially for young specialists in those countries where no systematic instruction on this material exists.

The origin of this long-awaited publication is worth describing briefly. Certainly, the idea of organizing and compiling the significant information available on clinical neurophysiology is not new but, as little as thirty years ago, the various aspects of the field could be expertly covered in a single book, such as the one by Y. Bertrand, J. Delay and J. Guillain of France (1939). In 1950, D. Hill and G. Parr of England also successfully synthesized the knowledge to that date in a book that has remained a classic up to the present.

Until recently, the three or four review works produced annually provided sufficient coverage, but because of rapid discoveries in the field and a parallel expansion of the literature, this kind of compendium has become increasingly difficult to accomplish. Authors now are obliged to limit themselves either to incomplete and superficial surveys or to reports on highly specialized areas, dealing in depth only with the newest developments.

Since 1950, most original work done in clinical neurophysiology has been presented in various scientific journals, such as *Brain* in England, *La Revue Neurologique* in France, *Neurology* in the United States and the most highly specialized of them, the EEG Journal *Electroencephalography and Clinical Neurophysiology*, transferred in 1949 to the Federation by H. Jasper who had initiated it 2 years before with a small group of devoted colleagues. However, because of the continual increase of diversified information on the electrical activity of the nervous system published in more than 300 journals, it became obvious that an easier method of examining the literature had to be devised. Thus, Dr. Mary Brazier pioneered the first inventory of EEG literature in 1950 with her *Bibliography of Electroencephalography: 1875–1948*, which was supplemented by the useful indexing done periodically by C. E. Henry for the EEG Journal. These achievements were successfully followed in 1965 by the compilation of three *KWIC Indexes of EEG Literature* which covered work from the earliest years of EEG through 1969. Recently the International Federation of EEG Societies, with the invaluable assistance of Pat Walter of the Brain Information Service in Los Angeles, assumed responsibility for continuing publication of the *Index to Current Literature* on a permanent basis.

But the problem of a comprehensive presentation of the material remained. In 1961,

a group of scientists, among whom were H. Jasper, W. G. Walter, H. Gastaut, A. Rémond, O. Magnus, W. Storm van Leeuwen, R. Naquet and W. Cobb, met with this concern in mind to discuss the possibility of producing a Handbook. They envisaged a finished work of great scope and depth, resulting from a highly concerted group effort.

However, none of those involved in this preliminary planning session were able to devote as much time as he had hoped to such an important task and no one in the group was free enough to assume the difficult role of leader. Thus, unfortunately, at the end of the year the project was implicitly abandoned and was relegated to the long list of wished-for but unmet possibilities.

This was regrettable in more ways than one since the need for a significant compendium of data was certainly tied to an equally pressing need to reinforce the value and prestige of EEG and Clinical Neurophysiology teaching. Official courses exist only in certain countries; even there the material is often presented merely as part of a course in neurology, neuropsychiatry, neurosurgery or psychology, and the oft-referred to "Diploma" of Electroencephalography hardly exists as a serious, widely-accepted and well-recognized credential of learning.

Fortunately, however, after Dr. O. Magnus of the Netherlands had successfully organized and implemented two international courses of EEG, in conjunction with the 5th and 6th International EEG Congresses, held in Rome and Vienna respectively, the organizers of the 1969 EEG Congress incorporated an idea which led to a reactivation of the Handbook project. Round Tables, consisting of high quality presentations whose goals were to synthesize available scientific knowledge and to teach, made up a large part of the Congress scientific sessions. The proposal to generate one volume of the Handbook from each Round Table topic was formally presented to both the Council and the General Assembly of the Federation where it received a vote of warm approval and was subsequently accepted by the publisher of the EEG Journal.

Thus, the Handbook evolved to its present form. Each volume contains several parts, each prepared by a team of co-writers. The organization of the Handbook has been kept flexible enough to allow for additional co-writers and the modification of part content, such that the original 36 parts have become 40, and the original 14 volumes have become 16, with the inclusion of the Congress Common Session lectures. Over 180 of the most renowned specialists from 20 countries finally participated in this project and the results of their efforts are impressive.

The Handbook begins with an appraisal of forty years of achievements of the pioneers and a critical evaluation of the current use and future perspectives of EEG and EMG. This introductory material constitutes the first volume of the collection and represents the reflections of some of the best minds in our field who were invited to present at the San Diego congress the synthesis so necessary at the beginning of such an achievement. The Handbook subsequently progresses through a wide variety of topics: the analysis of the basic principles of the electrogenesis of the nervous system; a critical review of techniques and methods, from the most traditional to the most modern using data processing; the description of the normal EEG from

birth to death with special consideration of the effect of physiological and metabolic variables and of the changes relative to brain functions and the individual's behaviour in his environment; the recent acquisitions in the understanding of the somaesthetic mechanisms. Finally, the large and rich section covering the electrical abnormalities in the various diseases is introduced by a study of the electrographic semiology and of the rules of diagnostic interpretation.

And so, a long-standing dream of many people has at last come to fruition. We shall now have a knowledgeable companion at hand to guide, to inform, and to supply rapid answers to the multitude of questions which daily challenge the various specialists in the neurological sciences as they pursue their professional activities.

The Federation of EEG Societies and the Editorial Committee of the Handbook would like to acknowledge with unreserved thanks the considerable role played by the very competent and efficient editorial assistants Miss Gillian Dunkley and Miss Cristie McGuire in designing and planning the Handbook and later in the harder task of its day to day realization.

Paris, September 1971 ANTOINE RÉMOND

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PART A

EVOLUTION OF CLINICAL NEUROPHYSIOLOGY SINCE HANS BERGER

Chairman of the First Common Session
at the VIIth International Congress
of EEG and Clinical Neurophysiology:

H. JASPER

Introduction

H. JASPER

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The origin and development of electroencephalography from the first publication of Hans Berger in 1929 will be reviewed in this section by Lord Adrian, Pierre Gloor and William Cobb. The first two chapters will be devoted to the life and work of Hans Berger, Lord Adrian speaking on "The Discovery of Berger" and Pierre Gloor on the "Work of Hans Berger". William Cobb will then trace the development of EEG from the early days in Britain, France and the United States to the present under the title of "The Past 40 Years of EEG". Speculations regarding the future of EEG in the broad context of contributions to the brain sciences and society will then be presented by Grey Walter.

We are particularly fortunate in being able to begin this series with an account of the discovery of Berger by Lord Adrian as he is nearing his eightieth birthday since Adrian was largely responsible for the original confirmation of Berger's work and its introduction to the English speaking world in the early 1930s. His own contributions then played a major role in the revolutionary development in neurophysiology which followed. Important as the potential and actual clinical applications of electroencephalography were at the time, it was of even greater importance that leading scientists throughout the world be convinced that, in spite of its enormous complexity, the electrical activity of the living brain, human and animal, asleep or waking, while perceiving, learning, thinking and acting, was a legitimate and promising field of study with rigorous objective methods of exploration. The importance of this stimulus to all the brain sciences, neurophysiology, neurochemistry and pharmacology, neuroanatomy, psychology and the behavioural sciences, as well as to the more recent developments in computer and communications sciences and engineering, can hardly be overestimated. Adrian's contribution to this development was of special significance not only in the early days, but throughout the years that followed. He brought with him great distinction, with prizes and honours from all parts of the world, including the Nobel Prize in Physiology and Medicine in 1932, the Order of Merit in 1942, President of the Royal Society 1950–1955, President of the British Association for the Advancement of Science 1954, Master of Trinity College and Chancellor of Cambridge University and, of course, Honorary Fellow of the International Federation of Societies for Electroencephalography and Clinical Neurophysiology and the International Brain Research Organization. To those who have had the privilege of knowing him and Lady Adrian personally his influence has surpassed science, an example of strong character and a way of life. We are particularly fortunate, therefore,

in being able to have Adrian's personal recollections and reflections on the "Discovery of Berger" as the introduction to this session.

Pierre Gloor will give us an account of some of the highlights of his intensive studies of the life and work of Hans Berger, presented more completely in his commemorative volume (*Electroenceph. clin. Neurophysiol.*, 1969, *Suppl. 28*), in which will be found a brief biography as well as an excellent English translation of all of Berger's works. With German as one of his native tongues in Switzerland, and with scholarly proficiency in English, together with outstanding work in the field of neurophysiology and clinical electroencephalography at the Montreal Neurological Institute since 1952, Pierre Gloor is particularly well qualified to speak on "The Work of Hans Berger".

Electroencephalography became thoroughly established in Britain, the United States, France and, to some extent, in Belgium and Italy during the 1930s but it was not until after the war that the most rapid development occurred. At the first international meeting in London in 1947, under the presidency of Lord Adrian, the International Federation was discussed and the EEG Journal was established. The second International Congress was held in Paris in 1949 under the presidency of Professor Baudouin. Our Congresses have since followed at 4 year intervals: 1953 in Boston with Professor Alexander Forbes as president, 1957 in Brussels with Professor Frederick Bremer, 1961 in Rome with Professor Mario Gozzano, 1965 in Vienna with Professor Hans Hoff and 1969 in San Diego with Professor Herbert Jasper. Presidents of the International Federation have included H. Jasper, Grey Walter, H. Gastaut, M. Brazier and A. Rémond, soon to be followed by C. Ajmone Marsan. The story of this development will be told by William Cobb, a leader in this field in Britain since the war, Head of the Department at the National Hospital in Queen Square, London, active in the work of the Federation and Managing Editor of the EEG Journal.

Among the great pioneers since the very first days of electroencephalography was Grey Walter. Having first described and named the "delta waves" of brain tumours, he then proceeded to develop practical techniques of frequency analysis and toposcopy, and to provide us with many highly imaginative ideas concerning the interpretation and significance of the EEG in relation to the mind and behaviour. For Grey Walter, electroencephalography was always more than a technique; it stood for the science of the brain in its broadest context (electroencephalology as he would call it). This will be demonstrated in his chapter on "The future of clinical neurophysiology". His provocative approach to this subject will engender much thought and discussion during the years to come. It would seem to me that the future may be bright only insofar as we continue to develop and refine EEG techniques in relation to all the brain sciences, and may be of rather limited interest if confined to its routine use in clinical diagnosis, as Grey Walter will emphasize.

The Discovery of Berger

E. D. ADRIAN

Trinity College, Cambridge (Great Britain)

Our meeting celebrates the fiftieth anniversary of the publication of Hans Berger's first paper on the "Human electroencephalogram", published in 1929, and I have the task of trying to explain why there was a gap of 4 or 5 years before we discovered what he was doing.

If his evidence could be trusted, here was a new and unexpected development of great interest to the physiology of the brain as well as to clinical neurology, but until 1934 scarcely anyone outside Germany and few inside it paid any attention to Berger's papers.

The people best qualified to judge the importance of what he described were the electrophysiologists. Why were the electrophysiologists of my generation so slow to take any interest in what he had done? Many of them were my friends; some, I am glad to say, are still active and I am as much to blame as they were. It is true that I had not begun any work on the mammalian brain until 1933, but I had been lecturing on it for 10 years and thought I was keeping abreast of current research.

I am sure that the principal reason for our neglect of Berger's earlier papers was simply that none of them was published in the journals normally read by electrophysiologists, the journals which would have been available in the libraries of physiological departments. He did publish one (in 1930) in the *Journal für Psychologie und Neurologie*, but the psychologists and clinical neurologists would have made little of it; all the rest of his work appeared in the *Archiv für Psychiatrie und Nervenkrankheiten*. To add to this limitation of their appeal there is the fact that his papers were long and were written in a way which made it difficult for those who were not good linguists to follow the argument.

There were few of us then who had worked in German laboratories and knew the language, but I do not believe that the difficulties of translation would have been enough to prevent us from taking any interest in Berger's work if we had come across it in the physiological journals. The title of all his papers "Concerning the human electroencephalogram" was surely arresting enough to have made the reader take some trouble to understand their contents. It was because the electrophysiologists outside Germany were unaware of Berger's papers until 1933 that we made no attempt to verify his conclusions.

There are other reasons for our neglect which are sometimes put forward. One is that his papers were published at a time when most of the countries of the Western world were beginning to feel the great economic depression which was at its worst in

the early thirties. This affected the machinery for exchanging scientific information, the abstracting journals, the travel grants and the rest. I have vivid memories of my first visit to the U.S.A., as the guest of Dr. Bronk. It was in September 1931, when Great Britain had just gone off the gold standard and many of the banks on this side of the Atlantic were closing their doors. It was a time when funds for scientific research and publication were jealously guarded.

I do not in fact recall any sign of a slackening in the amount or quality of the research in neurophysiology on either side of the Atlantic, yet guessing the financial position of German laboratories, we might have thought it extremely unlikely that important work in our own particular field could be going on in a mental hospital there. This may have been an additional reason why we did not look at the journals in which Berger published his papers.

If we want yet another excuse for our lack of curiosity about work on the brain, it might be added that most of us then were engaged in work on the peripheral nervous system and not on the central. We were reaping the harvest due to the new technique of electronic amplification. This was giving important results in the problems of nervous communication and most of us probably thought we were better employed in following up this line of advance than in paying attention to the much more complex field of the cerebral cortex.

I ought to explain perhaps that before 1914 our information on the nature of nervous activity at the unit or cellular level was almost entirely based on records of the muscular twitch following electrical stimulation of a motor nerve and on records of the potential change which travelled down the nerve trunk. The recording instruments then were not sensitive enough to show the potential changes in individual nerve fibres and so we were forced to rely on records of the total potential change when all the fibres in the nerve trunk were excited simultaneously.

But after the war, in 1919, the triode amplifier developed for radio-signalling had made it possible to use less delicate instruments and yet to record much smaller electric changes. Gasser and Newcomer were the first to construct a practical system using triode amplification and this was the start of a new chapter in neurophysiology.

Throughout the twenties Erlanger and Gasser and their colleagues were engaged on an accurate survey of the electric responses from nerve fibres of different function and diameter in various nerve trunks.

In England by 1925 we were using amplifiers modelled on theirs to record the impulses in individual sensory nerve fibres when the sense organs were excited by the appropriate stimulus. Records of this kind gave direct evidence for what in the past had been not much more than a conjecture, namely that in the signals transmitted by nerve fibres the information was conveyed by the frequency of the impulses and not by any variation in their character.

Yet before such results could be taken as proof of some general principle it was necessary to examine all kinds of nerve fibres, large and small, medullated and non-medullated, engaged in their normal activity. Thus in the period after 1919 the new recording techniques were used to examine the discharges in all kinds of nerve fibres, from insects and crayfish and caterpillars, as well as from frogs and rabbits. This was

the field where the results would be of immediate value.

But in the period after the war research on the cerebral cortex had not been greatly aided by the better electronic recording. The network of cells and dendrites and axons in the cerebral cortex was bound to be harder to analyse than the bundles of parallel fibres in the peripheral nerves and there was always the difficult problem of anaesthesia, of abolishing pain without too much reduction or disorganization of cortical activity.

It has now become possible to study the electrical activity of the brain in the same detail as that in which nerve fibres were studied in 1920–1930, and this has been due to improvements in technique which came in the following decade or later: microelectrodes, new anaesthetics like the barbiturates and new preparations of the cerebrum, like the *cerveau isolé* of Bremer, which dispensed with the need for anaesthesia. Before these improvements, between 1920 and 1930, scarcely anything was published in British or American journals on the electrical activity of the cerebral cortex. Research on the brain was carried out by destruction or stimulation of particular regions but not by electrical recording.

My own acquaintance with the electrophysiology of the central nervous system did not reach the level of the mammalian brain until 1933. But in 1931 Buytendijk and I had found some slower potential changes in the isolated brain of the goldfish and after that I had picked up various rhythms in the optic ganglion of the water beetle, *Dytiscus*. Some of these were admittedly due to the abnormal state of the preparation, perhaps aided by surges of grid current from the direct coupled amplifier we used, but whatever their cause they arose in masses of nerve cells and dendrites and led on to an investigation of the rabbit's cerebral cortex in 1933. In this I was joined by Matthews, who understood amplifiers much better than I did.

In our work on the rabbit's cortex we were mainly concerned with the electrical activity produced by injury or stimulation of the anaesthetized brain; we hoped to learn something about the reactions of a sheet of nerve cells and dendrites and we were not concerned with the problems of cerebral localization. But the only literature we found relating to electrical events in the brain was chiefly concerned with that particular problem and in one of the papers we found a reference to Berger's work.

The paper was written by M. H. Fischer, from the Berlin-Buch Institute, and was published in *Pflügers Archiv* in 1932. Fischer had made records from the exposed brains of cats and rabbits, curarized and under artificial respiration, and he described spontaneous current oscillations, variable in size and frequency, at much of the cortical surface, as well as a characteristic response from the striate area when the eyes were exposed to light. However, his paper began with a good historical section, starting from Caton in Liverpool in 1875 and mentioning all the important work on the electrical activity of the cortex. It was in this section that we learnt for the first time of the work that Berger had done. Fischer described the earlier observations that Berger had made on the cerebral cortex of the dog and added that he had then turned to the study of the "Electroencephalogram" in man (Berger always wrote "Elektrenkephalogram"). Fischer said no more about it, for this, he said, was a subject outside the scope of his paper, though he added that "Berger's findings are of con-

siderable interest".

Fischer gave references to the paper which Berger had published in the *Journal für Psychologie und Neurologie* in 1930 and also to one by Kornmüller in 1932. We found the volume with Kornmüller's paper, which described experiments like Fischer's, recording electrical oscillations from the exposed brain of the rabbit or cat and finding differences in the spontaneous or evoked rhythms in different parts of the cortex. But Kornmüller paid rather more attention to what he described as "Berger's interesting researches on the EEG". He says, however, that they have only an indirect relation to his own work, for his was concerned with the different types of electrical oscillation recorded from parts of the cortex with particular functions and cell structure "whereas Berger's work deals with the brain as a whole".

When we turned to Berger's paper in the same journal (in 1930—the only one he published there) we were bound to agree that his findings were of considerable, indeed of exceptional, interest, and we were greatly surprised that no one, apparently, had tried to repeat them. We were also impressed by the general tone of Berger's paper which seemed to be written almost as an appeal to be taken seriously, though he had no quarrel with any definite person.

It was certainly surprising to find such differences between Berger's records from man and Fischer's and Kornmüller's from animals, even allowing for the many differences in technique. On general grounds it seemed likely that the electrical activity of the cerebral cortex in man would show a division into projection and association areas like those found in the cat and the rabbit. It was true that in man the association areas would take up most of the cortical surface, but it was difficult to think of the human brain displaying such a simple and uniform activity when the subject was conscious; even though his attention was not fully engaged it was difficult to suppose that most of the cortical neurones would be free to join in such uniform and regular pulsation.

Like all electrophysiologists of that period we were keenly aware of the danger of artefacts in our records and of the difficulty of localizing the source of an electrical oscillation picked up by electrodes applied to a large organ or mass of tissue. But it seemed that Berger was also aware of this and had made the appropriate controls, though because of our difficulty in translating his papers we were not always sure of his meaning; and clearly there were some of his colleagues in Germany who were not convinced.

I do not remember whether we found the rest of the papers which Berger had published before we decided to look for the human alpha rhythm ourselves, but I think we decided at once, early in 1934. It happened that Matthews had recently designed a portable electrocardiograph, using an amplifier and writing the tracing in ink on a roll of paper. This had only one channel but that was all that we needed for a preliminary survey and it was more convenient than the 3-channel system with optical recording which we had used for the rabbit's cortex. We worked in the basement of the Physiological Laboratory, which was reasonably free from electrical disturbances as it could take all its electrical current from a 100 V storage battery.

We found Berger's alpha rhythm almost at once, but only in one of us. When

Matthews was the subject with electrodes on vertex and occiput the record showed no regular waves, but when I was the subject the waves at 10/sec appeared whenever I closed my eyes. We were surprised at first to find such great differences in the records from two human subjects but it did not take long to examine other inhabitants of the Physiological Laboratory and we found that a few were like Matthews in showing no sign of the alpha rhythm with eyes open or closed but that the majority showed some sign of it: often a prominent alpha rhythm on closing the eyes, becoming intermittent but returning from time to time till the eyes were opened again.

There were various controls to be done before we were satisfied that the waves in the record from scalp electrodes were derived from the cerebral cortex and not from eye movements or scalp muscles or some other extracranial source. Most of the controls we made had in fact been made already by Berger, but it was not until we came to study his papers in detail that we realized all he had done.

There was some lack of agreement between our conclusions and Berger's, which may have been due to the wide range of variation between one subject and another. Although we had taken records from many in the laboratory besides ourselves, we found we were always in danger of generalizing from too few examples.

We certainly went too far in trying to locate the alpha waves exclusively in the occipital lobes, for in later work we came on many examples of a frontal as well as an occipital alpha rhythm. On the other hand the occipital rhythm produced in some of us by flickering light and the much greater effect of visual than of auditory attention were arguments in favour of some localization of cerebral activity, at least in some individuals.

But whatever our reservations about Berger's explanation of his results—or what we took it to be—we were very soon quite convinced that he had made an important discovery which deserved immediate recognition instead of the neglect it had suffered. We had found it so simple to record the EEG that we decided to arrange a demonstration at the meeting of the Physiological Society which was to be held in Cambridge on May 12th, 1934. It was a meeting which was always well attended by visitors from Europe as well as by most of the physiologists working in British laboratories.

It was easy to fix our roles in the demonstration. I could be trusted to produce a regular alpha rhythm and Matthews to record it on the ink-writing electrocardiograph which he had designed. So I was the subject and Matthews was the recorder. We were fortunate in having an epidiascope in the lecture room, large enough to accommodate the moving parts of the ink-writer, so that the magnified image of the pen as it wrote the tracing could be thrown on the lecture room screen.

As I was the subject, I was unable to see the alpha waves from my head being written out on the screen when I closed my eyes, and ceasing when I was required to solve problems in mental arithmetic, but I could tell from the general hush in the audience and the scratching noise of the pen that the demonstration was going well.

I cannot remember now whether we showed the rhythms produced by flickering light as well as the alpha waves. I believe we did but I have not been able to ask Matthews, as he is now on his yacht. I do recall that several members of the audience asked for short lengths of the ink-writer record which had covered the floor of the

theatre as the demonstration progressed.

In fact it had gone so well that I began to wonder whether I had not unconsciously trained myself to produce the result by some kind of trick movement, though by that time we had quite enough evidence to rule this out.

A fortnight later my wife and I had been invited to Montreal and we went from there to New Haven and New York and to a meeting of the American Neurological Association at Atlantic City. I was glad to have Berger's discovery as a theme for several lectures, though we had come to the U.S.A. on Dr. Bronk's invitation and we were soon enjoying a splendid tour which took us as far as the Teton range in the Rocky Mountains.

The paper which Matthews and I had written about what we then called the Berger Rhythm came out in the autumn, but that is by no means the end of the story.

Sir Gowland Hopkins, the Professor of Biochemistry at Cambridge, had been at the demonstration we gave. He was the President of the Royal Society and had to deliver an annual address on the progress of science at the meeting in London on November 30th. In this address he referred to the demonstration of Berger's discovery and next day this was reported in the London papers with varying degrees of accuracy. The temptation to write about brain waves was too great to be resisted but, although some of the papers mentioned the name of Berger, there were several that referred only to what had been shown at Cambridge. I began to feel that our demonstration had been a failure for it looked as though Berger was in danger of being passed over again. So I sent him a letter explaining what had happened and saying that we proposed to write to the medical press in London, the *Lancet* and the *British Medical Journal*, explaining that all we had done in Cambridge was to demonstrate his discovery. I said we should like to send a similar letter to some of the German medical papers if we had his permission. He replied in a brief but friendly letter agreeing and adding his thanks. And to the best of my recollection he wrote again after these letters had appeared in the medical journals in Germany, and this time he wrote very warmly and it was clear that he was pleased to have had his discovery recognized, even though we had not accepted the whole of his interpretation of the results.

I kept the correspondence for many years and it survived several moves from one house to another, but I am sorry to say that I could not find it again after the war. At all events I do remember being greatly pleased with his letter and afterwards by meeting him in person at the Psychological Congress in Paris in 1937. We could not discuss the differences which still seemed to exist between us in our interpretation of the waves, for we had no common language, but by then there was no doubt at all that Berger's discovery had given a valuable aid to clinical diagnosis and was bound before long to help in the deeper understanding of cerebral activity.

In the past 30 years since that meeting in Paris the chief advances seem to have been in the clinical field and I do not feel sure that we have much clearer ideas of the neuronal activity which gives us the waves in the EEG. But the subject has grown so much that I have probably missed the more important papers in recent years, as we all missed Berger's in 1929. At least it is certain that the EEG has given new facts about brain activity which cannot be neglected. If it has added some difficult puzzles which have still to be solved, that in itself shows the great importance of Berger's discovery.