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C. R. Biologies 329 (2006) 450–459
COMPTES RENDUS BIOLOGIES
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Neurosciences

Between local cultures and national styles: Units of analysis in the history of electroencephalography

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Received 15 November 2005; accepted after revision 15 February 2006

Available online 15 May 2006

Abstract

The history of the discovery of the human electroencephalogram (EEG) and the ensuing implementation of electroencephalography is characterized by striking national differences. The first publication on the EEG in 1929 by the German psychiatrist Hans Berger was met with skepticism. Substantial work in this area did not start before the public demonstration of the EEG by the British neurophysiologist Edgar Douglas Adrian in 1934. Soon afterwards, many groups specialized in the new method, particularly in the US, whereas interest remained more limited in France and Britain. A comparative analysis of the rise of electroencephalography has certainly to account for such national differences, but the trajectory of the implementation of this technology calls for an investigation of local research cultures in order to identify units of productivity and to understand the dynamics along this trajectory. ***To cite this article: C. Borck, C. R. Biologies 329 (2006).***

Résumé

Entre les cultures locales et les styles nationaux : unités d'analyse dans l'histoire de l'électroencéphalographie. L'histoire de la découverte de l'électroencéphalogramme humain (EEG) et de sa mise en œuvre est caractérisée par des différences nationales remarquables. La première publication d'un EEG en 1929 par le psychiatre allemand Hans Berger fut reçue avec scepticisme. Des travaux significatifs dans le domaine n'apparurent qu'après la démonstration publique d'un EEG en 1934 par le neurophysiologiste Edgar Douglas Adrian. Peu après, plusieurs groupes se spécialisèrent dans cette nouvelle méthode, spécialement aux États-Unis, tandis qu'un intérêt plus limité se manifesta en France et au Royaume-Uni. Une étude comparée des développements de l'électroencéphalographie doit rendre compte de ces différences nationales. Mais l'étude de la mise en application de cette technologie requiert de prendre en compte les cultures de recherche locales, afin d'identifier les unités de production et de comprendre la dynamique le long de ce parcours. ***Pour citer cet article : C. Borck, C. R. Biologies 329 (2006).***

Keywords: Electroencephalography; EEG; Research cultures; Research schools; National styles; Historiography of the neurosciences

Mots-clés : Électroencéphalographie ; EEG ; Cultures de recherche ; Écoles de recherche ; Styles nationaux ; Historiographie des neurosciences

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1631-0691/$ – see front matter
doi:10.1016/j.crvi.2006.03.015

1. Introduction

Electroencephalography, the recording of electric activity from the human scalp, undoubtedly counts among the most important and productive research tools in the neurosciences during the twentieth century. Originally published in 1929 by the German psychiatrist Hans Berger [1], the human electroencephalogram (EEG) figured prominently in international meetings and conferences since the late 1930s [2], was discussed widely across the many scientific and clinical disciplines involved in the study of mind and brain, revolutionized the conceptualization and treatment of the epilepsies [3], was of diagnostic significance for a variety of other clinical conditions, including brain tumour localization [4] and brain death confirmation [5], and still is an important object of scientific scrutiny and research [6]. The history of the discovery of the human EEG and of electroencephalography as an important avenue for both research and clinical practice is well known and forms part of the specialty's memory [7–10].

Berger's original publication kindled only limited scientific interest in Germany as elsewhere. A group of neurophysiologists at the Kaiser-Wilhelm-Institute for Brain Research in Berlin-Buch, for example, compared his recordings from the human scalp with their results from animal corticograms and presented their data at important national as well as international meetings, but without any recognizable response [11,12]. This widely shared ignorance regarding the EEG changed only when the British neurophysiologist and Nobel-prize laureate Edgar Douglas Adrian demonstrated the scientific validity of electroencephalography to an astonished international audience at the Cambridge meeting of the Physiological Society in May 1934 [13,14]. Within a few years, electroencephalography was well established across the continent [15–20] and beyond. Particularly many groups formed in the US and started to explore brain and mind by means of electroencephalography [21–26].

The greater availability of electronic amplification technology in the US and the focus of neurophysiology on recording electric potentials appear to have paved the way for the ready acceptance of the EEG there [27], whereas, for example, resistances among British clinical scientists towards diagnostic technology may account for the comparatively small number of EEG units there [28,29]. A comparative analysis of the rise of electroencephalography has certainly to account for such national differences. However, if the purpose is less to present once again the well known story of the various groups engaged in early EEG studies but more to take electroencephalography as a case study for identifying the units of activity in the neurosciences during the twentieth century, the investigation has to zoom in on more local dynamics [30]. In order to arrive at a better understanding of the productivity in the neurosciences during the last century, the investigation has to focus on the series and cycles of shifting centres, topics, and frameworks that shuffled productivity across time and space.

Distinctive local styles tend to disappear with increasing standardization and international instutionalization. The process of implementing electroencephalography demonstrates, however, that this was not an automatic process linking dissipation with universalization. Quite the contrary, at every centre involved, the EEG had first to be adapted to pre-existing research questions, local strategies and practices before it started to yield new data. As will be shown here, the technology was appropriated in many different ways at each local centre of development.

2. Paving the way by technological determinism

> "Looking back on my own scientific work I should say that it shows no great originality but a certain amount of business instinct which leads to the selection of a profitable line." [31]

Describing himself along such modest lines, the late Edgar Douglas Adrian demonstrated, above all, perfect British understatement. However, like many other neuroscientists Adrian was highly aware of how his own success throughout his career had essentially depended on the availability of new and sophisticated technologies which had typically been developed in a different arena and for different purposes. He used to explain his achievements as a consequence of technological advances:

> "Fortunately the detection of very small and very rapid electric changes has recently become a problem not confined to physiology, and our difficulties can be solved by the use of methods devised for wireless communication. When the academic scientist is forced to justify his existence to the man in the street he is inclined to do so by pointing out the essential part played by academic research in the development of our modern comforts. It is only fair, therefore, to point out that in this case the boot is on the other leg and the academic research has depended on the very modern comfort of broadcasting. As everyone knows, wireless telephony became pos-

> sible only with the introduction of the three-electrode valve which was developed on a large scale in the war." [32]

Electronic amplification is an obvious example of how industrial technologies shaped the path of discovery in the neurosciences. The vacuum-tube, developed during World War I for military broadcasting, enabled Adrian to confirm experimentally the all-or-none principal his teacher Keith Lucas had postulated, after his American friend Alexander Forbes had familiarized him with the new technology [32,33]. Later, his engineer Bryan Matthews had just constructed a mobile electrocardiograph with such a set of vacuum tubes, arranged to record the comparatively slow potential changes of the electrocardiogram, which put Adrian in the position to quickly confirm Berger's observation of the electroencephalogram, once they had finally become aware of Berger's work:

> "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 three 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." [9]

Along similar lines and following an explanatory framework that regards research tools and scientific instruments as determining the path of scientific achievements, the development of neurophysiology in the United States, for example, can be traced to local research clusters which coalesced around a particular technology, as Louise Marshall has shown [34]. The oscilloscope served Joseph Erlanger and Herbert Gasser in recording nerve fibre activity and stimulus conductivity; the microelectrode singled out individual neurones as objects of investigation in Ralph Gerard's lab; or the stereotaxic instrument framed a particular type of brain lesion studies in Stephen Ransons's team. By analogy, one could extent and apply her argument to, for example, Herbert Jasper's intraoperative EEG recording and Penfield's cortical stimulations in order to identify the Montreal School of clinical experimental neuroscience [35]; or one could point to Cécile Vogt's skills in handling of the gigantic microtome at Berlin-Buch for Oskar Vogt's micro-localization studies [36].

As much as all these examples demonstrate the general dependency of research in the neurosciences on advanced technologies and on the construction of sophisticated instruments, the underlying framework of a technological determinism, implicit in such a seamless alignment of labs and instruments, should be evaluated carefully. The EEG illustrates how complex the application of so comparatively simple a technology can be, as the example of Adrian reveals. Certainly, new research technologies opened up new fields of investigation, but typically, this resulted in a variety of new opportunities rather than a predetermined path. It is generally questionable whether there are instances in which a specific new technology opened up a single line of investigation and steered research automatically towards a major advancement. Much more likely is a scenario in which new technologies opened up entire networks of interconnected research paths in different, and at times, opposing directions, each leading to further possibilities once taken. Adrian, for example, did not look into slow brain waves and did not record an EEG before 1934 despite the availability of the technology and even though Rachel Matthews had observed slow, regularly oscillating brain potentials as early as 1928 in Adrian's lab [37]. Only as a consequence of a specific path of pursued experiments and of the results yielded thereby – in this case, the consistency in recording surprisingly slow and regular potential oscillations in central neurones in Rachel Matthew's studies with the conger eel and their recurrence in a new set of experiments by Frederik Buytendijk with different animals some three years later [38] – did Adrian start to look into these at first strange potentials that, eventually, solidified into a research problem to be addressed experimentally.

Similarly, the trajectory of the work in Adrian's lab after the arrival of the EEG did also not follow a clear path paved and determined by the new technology. After the confirmation of the "Berger rhythm" [14], Adrian did not follow this line of investigation very far once he got his laboratory adapted to recording EEGs. After a few further papers rounding up his initial observations of the occipital localization of the 10 Hz waves, Adrian moved on and diverted his attention to a new topic. His biggest contribution to the history of the EEG thus remains its effective publication – first with a public demonstration of his own EEG to the meeting of the Physiological Society in Cambridge on May 12, 1934, where he took the audience by a surprise similar to the one he had faced just a few weeks earlier, and then by a tour through some major cities in the US delivering the message about the new method and the rhythmic electric activity of the human brain to the new continent.

The EEG presents itself as an example of how the availability of a technology does not automatically decide about scientific practices and future research. There was no determinism guiding scientists towards the EEG but a rather vast field of heterogeneous opportunities. Electronic amplification, although necessary for EEG recording did not lead by itself to brain wave recording but rather steered the professional neurophysiologists away from it. On a more systematic level of analysis, the case of how Adrian arrived at and implemented electroencephalography points to several shortcomings of a technological determinism. As explanatory framework, it does not put enough emphasis on the specific role of technology, and thus, misrepresents the very productivity inherent in new technologies [39]. A technological determinism does not sufficiently account for the openness in the productivity of instruments and technologies as long as it limits their role to that of discovery tools guiding research along a path of unfolding scientific reality.

Again, Adrian can be quoted as avid observer of how a technology and its very productivity steered the community of scientists away from observing brain waves rather than guiding them towards recognizing brain waves:

> "If we want yet another excuse for our lack of curiosity about work on the brain, it might be added that most electrophysiologists then were engaged in work on the peripheral nervous system and not on the central. We were reaping the harvest due to the new techniques of electronic amplification. This was giving important results in the problems of transmitting information by nerve fibres 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." [9]

3. A question of style

If neither an intrinsic logic of scientific discovery nor determinism by the research instruments guided the path the EEG has taken during these years, an alternative framework that suggests itself is the concept of competing styles, of different ways to experiment in neurophysiology. Heterogeneity of scientific practices has been suggested by historians of science in the notion of research schools or national styles, and these have been applied to various scientific disciplines in order to account for geographical specificities changing over time [40–45]. Schools and styles address differences at the level of scientific practices that become identifiable primarily by comparison among different groups of scientists, e.g., they imply a concept of intrinsic heterogeneity of scientific practice even in highly specialized and internationalized fields such as neurophysiology. However, much of the debate about the concept of scientific styles has been centred on the question of whether these differences map the geographical boundaries of nations and whether the concept of national styles has explanatory power [46,47]. It seems obvious that any observation of national boundaries in scientific practice should be taken rather as the starting point of an investigation than as an answer in itself: What are the differences in the organization, institutionalization, and regulation of scientific practices and training in the respective domains that may account for the observed national differences? Introducing the concept of scientific style should not imply an indulgence in idiosyncrasies or commonsensical stereotypes, but explore diversity as possible source of productivity [48].

The EEG gives an especially rich case for a cultural analysis of neuroscientific research strategies. It took Berger almost 30 years to convince himself sufficiently about the soundness and robustness of his method and to dare a first publication on the EEG [1]. Between 1901 and 1929, apparently no physiologist pursued a similar line of investigation although probably the majority of physiological laboratories in the Western world were better equipped to record an EEG than Berger's makeshift laboratory in the basement of the Psychiatric Clinic of Jena, particularly since electrophysiology became of major focus of investigation during the 1920s [49]. A state-of-the-art electrophysiology laboratory of the mid-1930s required, in all likelihood, not more than 30 min instead of 30 yr for finding the Berger rhythm, the characteristic 10-s^{-1} undulation of electric brain activity over the occipital cortex.

The rapid dissemination of electroencephalography after Adrian's public demonstration, seems to follow, corroborate, and extend many details of Jonathan Harwood's analysis of national styles in genetics [43]. For its coming into being, the EEG required a 'Kulturträger' from Germany, pursuing a holistic and speculative research program against the consensus among the international community of neurophysiologists. But once the open-minded and pragmatic British Nobel-prize winner had confirmed the at first rather dubious reports from the German psychiatrist, the American scientists, eager to explore and exploit every new profitable research opportunity, quickly jumped upon the new method and applied it rigorously – with many consequences, most of them unforeseen. During World War II, for example, the number of EEG machines in service for the American forces overseas outnumbered by far those run in Germany. On basis of such a brief outline, it seems as if the EEG lends itself to the introduction of the concept of national styles into the history of neurophysiology. Berger's interest in a more speculative form of electropsychological research appears to be a kind of German 'Sonderweg', somewhat similar to what Anne Harrington has argued vis-à-vis Kurt Goldstein and holism in interwar psychobiology [50,51]. And the greater availability of electronic amplification technology in the US and the focus of American neurophysiology on recording electric potentials suggest economic and technological factors to have paved the way for the ready acceptance of the EEG there [27,34]. If the explanatory aim is to make the peculiar trajectory of the EEG fit with national boundaries, this account appears to be suggestive. The initially somewhat obscure German observation was rescued by British pragmatism but was converted into a clinical diagnostic routine only in consequence of the typical American reliance on technology and because of the advanced American standard of industrial production in electronics.

A scenario such as this one provokes the immediate question of what exactly it would explain as long as it simply aligns oddities with stereotypes. Certainly, factors such as national differences should be carefully evaluated for any explanation of why electroencephalography had first been developed by a German psychiatrist and was then taken up most intensively by a cohort of young American scientists sharing an interest and training in both physiology and psychology. In this way, the concept of national styles functions more as a kind of heuristic than as explanatory in itself, thereby avoiding some of its more problematic implications. The most obvious of these is the engagement in national stereotypes that are hardly explanatory but rehearse prejudices [41,47]. In order to avoid the shortcomings of such an arguing on the basis of national characters, the many differences among the scientific styles, of which only some may match national boundaries, should be taken not as causes but as the consequences of different ways of doing science and of different forms in the organization of science.

From various kinds of training patterns and different forms of institutionalization to more general codes of conduct in laboratories and to national regulations regarding animal experimentation, for example, many factors may contribute to differences that match national boundaries. In such an extended conceptualization however, the very idea of a national style becomes blurred and looses some of its conceptual grip. At least, it would become difficult to explain why an analysis on the level of national styles should be superior to more detailed and nuanced forms of analysis, looking at the many other factors influencing science on a more regional and local level. The choice of a particular experimental system, for example, usually reflects an individual training trajectory, local availabilities and technical facilities, networks of cooperation, etc. and may be decisive regarding the style of work performed in it.

Thus the debate on styles in the historiography of science becomes a question on how to specify and situate the concept of style. It should be remembered in this context that already in 1935 Ludwik Fleck introduced the notion of style into his sociology of science, in the last chapter of his *Genesis and Development of a Scientific Fact* [52]. According to his analysis, the sciences do not simply follow a path guided by objective scientific inquiry, observation, and reasoning but a contingent path depending on influencing factors on all levels of analysis. Science is something that is being formed constantly and at every level.

4. Local richness in research culture

The history of the emergence and dissipation of electroencephalography across the Western world presents the case where local factors appear to be of particularly strong importance and to outweigh the differences discussed so far on the national level. Even the universalization of the EEG, the process through which electroencephalography was established as a standardized method, did not follow a straight line but occurred in a series of appropriations, in which every laboratory engaging in the new endeavour recording aimed at bringing brain wave recording in resonance with locally existing practices and research trajectories.

The developments in Adrian's laboratory that eventually led him to start brain wave recording after he could no longer ignore slow, regular oscillations of cortical potentials have already been described above. Details that initially impressed as disturbances in a well implemented and smoothly running experimental system, gradually turned into a continuously reproduced artefact and later into a new scientific object. Thus, the course of events followed here fairly closely the pattern that Ludwik Fleck has described as the coming-into-being of new scientific objects under the notion of the 'Widerstandsaviso': "This is how a fact arises: At first there is a signal of resistance in the chaotic initial thinking, then a definite thought constraint, and finally a form to be directly perceived." [52 (p. 95)]. The situation of the Harvard-based group around Hallowell Davis, one of the major groups engaging in EEG studies early on, was somewhat similar. They had also already encountered slow, rhythmic potential oscillations during their investigations of the auditory pathway [53]. And again, the group converted their experimental system to brain wave recording only after they had learned of Adrian's confirmation of the EEG, regardless of the group's later claim to having confirmed Berger's brain rhythms independently of Adrian [54 (p. 316)]. On its own and by itself, the observation of rhythmic potential changes recorded from central neurons made little sense; it required a stabilizing interpretative context.

This context materialized only slowly, even after Adrian's public demonstration that had almost instantaneously established general agreement about the existence of brain waves in 1934. The EEG remained a phenomenon in search of its significance for at least a couple of years. Local specificities made all the difference during this period. Whereas Adrian who aimed at integrating the observed alpha rhythm in his electric theory of nervous activity did not pursue EEG studies much further, his countryman W. Grey Walter established the method in a clinical setting where he incidentally recorded particularly slow waves in patients suffering from brain tumours [4]. Following up on this observation by serendipity, Walter turned electroencephalography first into a method to localize brain tumours in suspected cases, before he eventually devoted his entire career to ever more sophisticated forms of electroencephalography, an endeavour that was accompanied by a specialization in the construction of new methods of data acquisition and analysis [55,56].

In the United States, the psychologist and physiologist Herbert H. Jasper was the first to publish an EEG [21] (though there was some debate as to whether he was also the first to record an EEG in the US [54,57]) but his achievements were quickly overrun by the spectacular results from another group. William Lennox and Frederick and Erna Gibbs had studied the pathophysiology of epileptic diseases along various investigative pathways and for more than a decade without many conclusive results [58]. It may well be the case that a certain desperation resulted from this situation and that this made them particularly sensitive to jump on the new method when it became available at the end of the year 1934. However, as soon as their colleagues in Harvard's physiology department got the equipment ready for the new recording, Lennox and his co-workers joined the experiments by Davis' group and contributed clinical expertise that the basic scientists lacked. For some more obvious and some more contingent reasons, the collective group decided to experiment first on Lennox' secretary, a diligently working epilepsy patient with a striking variant of this disease, a so-called petit-mal epilepsy. She suffered from frequent brief episodes of unconsciousness intersecting with her daily routines but not impairing her intellectual and organizational abilities. These contingencies, the interest in a particular disease and the availability of a particularly suited person willing to participate as experimental subject, made all the difference. The group hit upon a goldmine when they observed the characteristic three-per-second spike-wave pattern that since has become the diagnostic hallmark of this condition [22,59]. An anecdote about this episode that Frederick Gibbs loved to tell illustrates inadvertently how the regularity took all the attending scientists completely by chance. Apparently, the secretary became herself aware of how intimately the actions of the machine recording her brain waves coincided with her seizure episodes; in the words of the secretary: "Don't do that, it gives me a seizure every time you do it." [60]

In November 1934, no obvious connection between brain waves and epilepsy did yet exist. Suggesting such a link – and be it only for the purpose of starting an experiment – was plausible only in Harvard where these two fields of expertise, clinical experience plus basic research into epileptology, on the one hand, and neurophysiology of the central nervous system, on the other, coexisted. Here, an entirely new perspective upon electroencephalography, as well as upon epilepsy, emerged out of the contingent fusion of two fields that only locally made sense and only at a particular moment in time. Eventually, the new recording turned into the most important diagnostic method for the condition. The group of epileptic diseases was reconceptualized as neurological conditions affecting the electrical signalling in the brain, whereas before, they had been lumped together with many others psychiatric conditions affecting personality, intelligence, and social behaviour [30, Chapter 5]. This may be a particularly striking example of how serendipity is more than simply chance. It may include the coming together of heterogeneous research cultures. Serendipity is one possible product of the 'multiculturalism' in the sciences that Gerald Geison reflected about in his discussion of styles [48 (p. 238)]: "Perhaps, however, in the post-Enlightenment world we are ready to acknowledge some degree of multiculturalism even in the case of science itself."

5. An exemplary lack of local scientific culture, despite all expertise

The importance of such local appropriations for the shaping and moulding of the electroencephalogram as an epistemic thing [61] may be illustrated even more drastically with the series of events occurring in Jasper's laboratory at the Bradley Home, New Providence, Rhode Island. Here, the specific circumstances appeared to be perfect. The individual training was excellent, the local context particularly rich and valuable, the funding and availability of materials far above average. In short, every contributing factor seemed to have added positively to an ideal setting of support, and yet, the experimental system quickly ran into serious difficulties.

Jasper's early success in publishing the first EEG in the US [21] reflected how exceptionally well he was positioned to start EEG research. He had a double training in psychology (with Lee Travis at the University of Iowa) and physiology (with Louis Lapique at the Sorbonne in Paris), was just about to finish his second Ph.D. in electrophysiology (after his first in psychology), when he learned about brain waves [62]. He enjoyed a network of excellent scientific contacts among the international community already during the period of his extended training. At his first academic position, bridging the clinical setting of a home for socially and mentally handicapped children with the well-known psychology department at Brown University, Jasper disposed of a state-of-the-art physiological laboratory with equipment purpose-built to his own specifications due to generous funding by the Rockefeller Foundation [63].

As an obvious consequence of these extraordinary circumstances, Jasper had no difficulties in repeating Adrian's success in recording and observing human brain waves. Within a month, he published his first paper on brain waves in *Science* – an event widely publicized when it was also reported in *Science News Letter* [64]. On the basis of this initial success, Jasper decided to investigate brain waves systematically and along strictly rational principles. He received enormous help in doing so. The Rockefeller Foundation, for example, financed an extended trip to Europe that enabled him to visit the major groups in Europe working on the EEG. When Warren Weaver from the natural sciences branch of the Rockefeller Foundation heard of Jasper's work, he got so excited that he moved Jasper's funding from the medical sciences branch to his direct supervision [65]. Weaver mediated contacts with mathematicians and natural scientists offering, in Weaver's view, important and indispensable expertise. In this way, Jasper gathered massive amounts of information regarding the new phenomenon of the EEG, the technologies involved in recording it, and about the underlying physical and electrical principles.

However, when Jasper tried to consolidate this information to a coherent understanding of brain waves, the masses of readily available data turned into an obstacle difficult to overcome. One problem was conflicting evidence due to differences in the practices implemented for recording an EEG among the emerging groups of electroencephalographers. Certainly, Jasper was not the only person who was acutely aware of these striking differences. But he was one of the very few who had first-hand knowledge about almost every laboratory involved – and he lacked a similar degree of embeddedness in a particular research program. His openmindedness, the simple fact that he did not yet share a particular point of view from a pre-existing research program, caused a peculiar epistemic problem when he tried to mediate between the different strategies. As a newcomer, he did not lack sufficient training in the sense of inadequate expertise with specific methods or experimental systems but he lacked the socialization into a particular research trajectory. Jasper had not yet formed a particular "thought style", in the words of Ludwik Fleck – the kind of blinding specialization that went hand in hand with a scientific socialization [52].

In addition, and in contrast to Lennox or Walter, Jasper did not hit upon a spectacular new observation that would eventually push him to put his more fundamental questions aside. International exchange, rational universalization, sophisticated experimentation, generous funding, they all did not make up for serendipity. Instead of being of only little help to Jasper, these supportive factors effectively resulted in a severe burden for the development of his own research program. His rational and universal approach prevented Jasper from tuning his experimental system to local resonances. In his search for the basic constituents of cortical potentials and of the general significance of brain waves, he went so far to implant artificial sources of electrical currents into geometrical head models or the heads of dead animals [66]. In another series of experiments, he trained himself – or better: his brain – to generate a pattern of brain waves somewhat similar to the one just described by Lennox and Gibbs as pathognomic for petit-mal epilepsy [67]. His style of experimentation did certainly not lack scientific rigor or critical examination. But without a locally established anchor that could function as nucleus of crystallization for further scientific evidence, Jasper's experimental system generated inconsistent data because he aimed at too high a level of consistency.

Jasper's abstract rationalism was doomed to failure. With his scrupulous questioning of all available evidence in search of the internationally true significance of the EEG, he did not advance much further than publishing some of the best early reviews in this field [68]. It must have come as a God sent when Penfield visited his laboratory and offered him to open a clinical EEG laboratory at the Montreal Neurological Institute where Jasper would eventually establish the world's centre of reference for intra-operative EEG recordings [69].

6. Conclusion

Different styles of recording an EEG, of observing electric brain activity, and of exploring the significance of brain waves were a striking feature of early work in this domain, between the years 1930 and 1950. Some of the early differences matched national boundaries whereas others can be linked to site-specific techniques, materials, or practices. These site-specific factors point to local dynamics that made all the difference in the successful implementation of electroencephalography as a research instrument and as diagnostic tool. Many details of doing neurophysiological research were involved here, many more than can be accounted for in the concept of different national styles. Furthermore, the intrinsic tendency of the concept of national styles to presuppose what has emerged in large and socially configured bodies to be personality-like types obfuscates rather than highlights the forces at work here. Different forms in the institutionalization of scientific work and the professionalization of the researchers involved are probably more useful concepts in order to investigate the dynamics at work even at the national level.

Local styles, in contrast, appear to provide ample material for analyzing and explaining the many obstacles along the march of electroencephalography towards truly international standards and universal significance. Rather than mirroring site-specific, disciplinary, or national oddities, styles provide a framework of analysis with a sufficient intrinsic complexity or granularity for identifying the factors that turn a local experimental system into a source of productivity and scientific achievement. Observing the electric activity of the brain started to answer scientific questions once it could be addressed in specific ways, e.g., after brain waves had been brought in resonance with existing, distinctive cultures of experimentation.

A corroborated understanding of the EEG, finally, emerged only as a consequence of Jasper's and many more actors' reviews. Only in consequence of the many negotiations that these reviews reflected in their evaluations of conflicting data, an internationally shared standard for the procedure was established that guided the recording, reading, and interpretation of an EEG. The standard eventually agreed upon was not more abstract or less rational than Jasper's earlier attempts in achieving this. But a standard could only be established after so many publications and as a consequence of the various negotiations that went hand-in-hand with them. This new standard – that did not emerge before the end of World War Two – functioned as such because it reflected an already collectively shared practice in doing EEG research.

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[1] I want to thank the Rockefeller Archive Center for making this material accessible to me.