From Reflex to Rhythm:
sleep, dreaming, and the discovery of rapid eye movement
1870-1960

by

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Abstract

Rapid eye movement (REM) is a phenomenon of sleep easily visible to the naked eye of a careful observer. Yet it was not discovered until 1953. Why did it take so long for this phenomenon to come under scientific scrutiny? From 1870 to 1960, disciplinary, institutional and instrumental factors transformed the cognitive basis of sleep research. Once a passive and reflexive response to fatigue, sleep emerged as a self-regulating biological rhythm. As an objective sign of dreaming, REM encapsulated this concept of "activated" sleep.

Modern sleep research emerged from a number of nineteenth-century clinical and physiological problems, including insomnia, hypnotism and fatigue. Around 1900, Sigmund Freud, Henri Bergson, and Edouard Claparède introduced a biological perspective, describing sleep and dreaming as functions, not effects. Henri Piéron used the method of "enforced wakefulness" to develop a concept of sleep as both fatigue and rhythm. During the 1920s, Nathaniel Kleitman adopted a similar method in his research at the University of Chicago, where

REM was later discovered.

Medical and technological developments during the 1920s and 1930s had a great impact on the study of sleep. Epidemics of encephalitis lethargica, or "sleeping sickness," turned sleep into a highly visible object of neurophysiological research. Constantin von Economo linked its symptoms to a damaged "sleep centre" in the brain, reinforcing the idea that sleep was an active function. Vacuum-tube amplification created the electroencephalogram, which inscribed sleep as brain activity.

This period also witnessed the creation of departments of "neuropsychiatry" across the U.S. Such interdisciplinary and holistic approaches to biomedical research bound together psychological and physiological concepts with clinical practice and instrumental performances. The discovery of "sleep stages" at the Tuxedo Park laboratories of Alfred Lee Loomis in 1937–a crucial event in the story of REM–was made possible only through such interdisciplinary, technologically-driven efforts.

Cognitive goals of American neurophysiologists and neuropsychologists were realigned from the 1930s into the immediate postwar period. Freudian psychoanalysis turned dreaming into a central problem for sleep researchers, who hoped to unify body and mind through an analysis of the rhythms of REM.

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rather less than poetry.

In the end, my ability to see this thing through relied upon the devotion of just three people. My parents, George and Nell Kroker, put their confidence in me from my earliest memory. I can only hope to follow their example. Leanne, my wife, shared with me her very soul. The present work is but a pale reflection of our friendship.

Whatever is right about this dissertation, I owe to these people. Whatever is wrong I happily claim as my own.

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Introduction

This dissertation is an examination of how sleep research, an interdisciplinary field that combines psychological and physiological styles of investigation, emerged in the first half of the twentieth century. Nineteenth century investigators approached sleep as a generalized reaction to a stimulus—that is, as a passive reflex. This began to change during the Interwar period (1919-1939), when researchers began to depict sleep as a form of active, organic self-regulation. The most important outcome of this new image of sleep was certainly the discovery of rapid eye movements (REM) in 1953. Its discoverer, a physiology student at the University of Chicago, quickly interpreted REM as a sign of dreaming. Within a decade, REM had come to signify the new field of sleep physiology as well.

Rapid eye movements can be observed with the naked eye. Why did they go unnoticed for so long? My explanation is historical: the interplay of technological innovations, shifting experimental practises, disciplinary competition and theoretical outlook over almost a century of sleep research transformed the way investigators perceived sleep in the laboratory. Once the inevitable fallout of fatigue, sleep became an expression of biological time. And the rapid eye movements that had once slipped invisibly through the night began to chart the temporal course of dreams.

Introduction to the question

As a scientific field with its own distinctive identity, sleep research belongs exclusively to the twentieth century. Much of the credit for establishing this field, which has attracted investigators from virtually every discipline in the human sciences, has traditionally gone to the discovery of rapid eye movement (REM) in 1953. Over the next two decades, REM came to define the parameters of sleep research. Psychiatrists, neurologists, physiologists, psychologists and philosophers struggled to come to terms with the significance of this new phenomenon.

The perennial questions that have surrounded REM since its discovery have concerned its status as an objective sign of dreaming. Do dreams take place only during REM periods? Are

REM periods related to dreaming at all? Can REM tell us something about the function of dreaming? Are we really only investigating memory, when we investigate REM? Such questions have pervaded the literature, and have been asked by representatives of every field with an interest the sciences of mind and brain, from philosophers to physiologists (Malcolm 1959; Foulkes 1966; Hall 1967; Hacking 1975; Flanagan 1995).

In contrast, I propose to leave these questions about the epistemological status of rapid eye movement, and focus instead on the historical forces that led to its emergence as a scientific fact. While the status of REM as a sign of dreaming is contentious, it nevertheless remains part of the bedrock upon which modern sleep research has been built. In humans, a normal night of eight hours' sleep will feature three to five periods of distinctive physiological behaviour, characterized by bursts of rapid eye movement, an activated brain wave pattern, and a loss of muscle tension in the throat. The first episode appears around 90 minutes after the onset of sleep, and usually lasts between 5 and 20 minutes. Subsequent periods tend to be longer. Deprived of its relationship to dreams, REM would still be a fact of sleep research.

Following Ludwik Fleck's remarkable investigation of how the Wassermann reaction came to be accepted as a diagnostic sign of syphilis, we might well ask: how did REM come to appear as an object of scientific cognition in the first place? The answer to such a question, which Fleck articulated in terms of *Denkkollecktiv*, or "thought collectives," provides the basis for the present study. I take Fleck's call for recovering the phenomenology of scientific research seriously:

> ...we have even lost any critical insight we may once have had into the organic basis of perception, taking for granted the basic fact that a normal person has two eyes. We have nearly ceased to consider this as even knowledge at all and are no longer conscious of our own participation in perception. Instead, we feel a complete passivity in the face of a power that is independent of us; a power we call "existence" or "reality." In this respect we behave like someone who daily performs ritual or habitual actions mechanically. These are no longer voluntary activities, but ones which we feel compelled to perform to the exclusion of others (Fleck 1979: xxvii-xxviii).

There have been innumerable changes in how the sciences are studied since Fleck wrote these lines in the summer of 1934. But his desire to understand how perception can be transformed by historical forces remains with us, nearly seven decades later.

Even a minimum of historical research into the nature of rapid eye movement will reveal the particular importance of Fleck's ideas to my study. Rapid eye movement is quite unlike most other discoveries in the history of twentieth-century laboratory science in one crucial respect: at the very outset, its discoverers insisted that this phenomenon was visible to the naked eye. The same cannot be said, for example, quarks, antibodies, or deoxyribonucleic acid (DNA), the nature of which can only be revealed with a particular assemblage of instruments, techniques, and visual displays. The discovery of REM, on the other hand, was less a matter of finding out about the material structure of something than it was about determining the nature of physiological time. To assure their readers that their observations were not instrumental artifacts, Aserinsky and Kleitman declared that they had "visually inspected" their subjects to make sure their eyes were indeed moving beneath their lids, a rhetorical flourish that persisted for at least a decade after REM's discovery. Despite the miles of paper devoured in the detection, comprehension, and published representations of rapid eye movement, it retained at least this one aspect of its clinical heritage: REM is a sign that, when correlated with other signs, serves to diagnose an on-going state. It is not a snapshot of some real entity that lurks beneath the temporal or spatial thresholds of perception, nor is it a static, portable model of events taking place within the body. Rather, it is the endpoint of an orchestration of inscriptions—inscriptions that do not *extend* the senses so much as they calibrate and synchronize events in time.

The history of sleep research is filled with the efforts of those who wanted to correlate dreaming to some physiological event. Some investigators gazed at their subjects all night, in the hope of discovering some relationship between facial expressions and dream content. Others offered casual observations along these lines, but failed to elaborate on their discoveries. Rapid eye movement was either invisible or insignificant to these people. Why?

This question has been rather casually addressed by some of the historical actors that I am examining. One early REM investigator, for instance, offers the following anecdote about an encounter with one of REM's discoverers in 1957:

> ...I told him [Nathaniel Kleitman] that I was interested in setting up a dream research laboratory and was, of course, particularly interested in the phenomenon of rapid eye movements (REM), which had been described as accompanying dreaming. He immediately replied, "Then let me show you them." He leaned forward in his chair, closed his eyes, and I could clearly see his eyeballs moving back and forth under the closed lids. He opened his eyes again and said, "You saw that, didn't you? It is unmistakable. Yet no observer had made that observation for many, many centuries!" I realized that this phenomenon, despite all the electronic complications of its measurement, was basically simple and observable without complex instrumentation (Whitman 1974:217-8).

Kleitman's legacy as the first physiologist to dedicate his career to the study of sleep will provide one of several guiding threads in my narrative. His work in sleep research, which began in the early 1920s, spans most of the period under consideration here. Were it not for the appearance of rapid eye movement, the apogee of his career would almost certainly have been in 1939, with his publication of the first English-language textbook on sleep physiology (Kleitman 1939). His approach to sleep, coming as it did, from a combination of technological innovation and a desire to explain sleep in functional, rather than mechanical, terms, was a melding of Old World ideas and New World technology that tended to characterize science in North America during the Interwar period. His portrayal of REM as something that existed independently of the instruments he first used to detect it indicates his tenacious hold on both cultures.

For Kleitman, a physiologist representing his work to Whitman, a psychoanalyst, the most immediate way to demonstrate REM was by mimicking the phenomenon with his own body. Not so for Roger Broughton, a neurologist who established the first North American sleep clinic in 1968 at the University of Ottawa. When I interviewed Broughton in February of 1999, he offered me, a historian with no background whatsoever in sleep physiology, a rather different account of REM. When I suggested that Edmund Jacobson, a colleague of Kleitman's at Chicago during the 1920s and '30s, had a legitimate claim to having described REM in 1938, Broughton responded with an armchair demonstration of his own:

Roger Broughton: ...if you want to say that someone like Jacobson described eye movements first, during sleep, well you can check that against all kinds of Ancient texts and you'll find descriptions of this...you know, eyes flickering, twitching. It's very obvious, particularly in infants, for instance. They're asleep, and people are awake, looking at them, and they have very intense twitching, very intense activated sleep. The issue, it seems to me, historically and scientifically, is who defined a new biological state with rapid eye movement.

Kenton Kroker: *Right. And with criteria that we continue to use today.*

RB: Yeah. Even the criteria might be wrong. But with the basic concept that, that this is sleep, and it's different [starts to scribble lines that look like EEG tracings on a piece of paper]. Look! You know, there's a...[scribbling]...low-voltage EEG, with all of these...[scribbling]...eye movements, and other features, which we don't see in all the other patterns of sleep we know, which then was stages A, B, C, D and E. And that's Aserinsky! (Broughton* 1999)

Broughton's career began forty years after Kleitman's, and a full decade after the discovery of rapid eye movement. Yet his reconstruction of the history of REM is somewhat counterintuitive. I had expected that the further one got from the original discovery, the more this phenomenon would became "black boxed" in the "inscription device" that produced it (Latour & Woolgar 1979; Latour 1987). That is to say, they would treat the phenomena their instruments created as unproblematic indexes of reality. But Broughton, who, as a clinical neurologist at a research hospital, spends a good deal more time staring at electroencephalographic traces than Kleitman ever did, seemed quite comfortable with accepting the overwhelming role of graphic traces and instruments in creating and safeguarding the status of REM as a phenomenon of sleep. In fact, he insisted on it without hesitation, the moment he reached for his pencil to *show* me what REM is.[1]

One neuroscientist with a somewhat more marginal interest in rapid eye movement, Peggy Mason, encouraged me to reconsider the status of these graphical traces yet again. After she described her study of how the brain regulates pain, she took me to her lab at the Surgical Brain Research Institute in the University of Chicago Hospitals, to show me the latest improvement to her experimental assemblage—a video camera focussed on a Norwegian White Rat in a plexiglass cage. Several of her students had set up a time code on the video display, thereby allowing the video recording to be calibrated to the polygraphic recordings taken from a

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[1]Broughton later suggested to me that the EEG had been "over-emphasized" until the late 1970s, as it was virtually the "only sleep tool" available to researchers (Broughton* 1999).

six-pronged plug surgically attached to the top of the animal's skull. Her description of the benefit of this new addition led her directly into the divide that has separated behavioural psychologists and physiologists for nearly a century:

Peggy Mason: That [pointing to the camera], that piece of it...I mean, it took them [two of her undergrads] two or three days [laughs], and you know, twenty bucks! But it's a huge help!...I think the bottom line is, what's the animal doing?

Kenton Kroker: *And, and, visible behaviour still has something to do with it, despite the fact that...what gets reproduced in journal articles is all the lines...*

PM: Well, I mean, the...one of the place where we found it to be true is if, if you just looked, just looked at the traces...*yes*....you could think that an animal is asleep when he's walking around...

KK: *So the visual inspection still counts for an awful lot?*

PM: To me it does. And, you know, Al [Rechtshaffen, a student of Kleitman's, who took over his lab in the 1960s] sort of thinks I'm, I'm off on this, and I probably am. But...for us, the first thing is the visual inspection.

KK: *Oh, really?*

PM: First thing that we do is go, is we actually go through [the videotape] and say, "oh, he's walking around for these times." *Right. So he's clearly awake*...he's awake...*right, right. So, um, I guess you'd call the behavioural manifestations...you'd put on the same level as the traces*...the EEG, absolutely, and we're trying to explain behaviour!

KK: *Right, right....and that's something that somehow controversial?* [pause] *I mean, to me, it just seems like common sense, but of course, I don't work in a laboratory.*

PM: Well, see...I think...I think it's sort of more sociology...I think it's because these people...uh, people like Al...record for twenty four hours a day. *uh-huh.* They can't go through and score. We score, you know, a ninety minute video tape, and it takes me, it takes an undergraduate, uh, I don't know, fifteen minutes, twenty minutes. They couldn't do it, and produce the data that they produce....it's too time consuming...

KK:...*right, because someone would have to go through twenty-four hours' worth of recording...*

PM: ...not twenty-four hours, um, I mean, when, when Al does his, his uh, deprivations, we're talking months! *Right.* But...with these guys [gestures to rat], we only record for, you know, four or five hours. And, those are very precious hours, into those hours, I dump, you know, hundreds of hours into an...analysing! I'm willing to give up an undergraduate's hours, to look at the tapes. So I think that's sort of...I think that's the bottom line why most sleep researchers don't do that. They have an automated system. And they...they probably have better recordings than me, and they're probably right most of the time. But...we're not...our recordings are...they're not...as good as Al's, for instance (Mason* 1998).

This tension between different forms of surveillance, both in scientists's accounts of their own work, as well as in the history of their respective disciplines, will help to frame the historical question that I am asking about rapid eye movement in terms of *visualization*. How do laboratory researchers come to see phenomena? This is a particularly pressing question for physiology, which, by the very nature of its subject matter, has always struggled to contain the motions of the body by picturing them. The seventeenth-century English anatomist William Harvey did not, after all, *see* the circulation of the blood in the same way that his contemporary, Thomas Willis, saw the fine network of blood vessels in the brain now known as the "Circle of Willis." Harvey witnessed what he thought were the circulation's effects, and proceeded to combine examples of these effects into a logically coherent and powerful argument to convince his contemporaries of the reality of the circulation.

The unique difficulty that sleep has historically presented for physiology is that its effects are, on a mundane level, generally below the threshold of the visible. Sleep tends to be described in terms of the *absence* of phenomena (of consciousness, of movement, of sensation), rather than the *presence* of anything at all. This image of sleep as pure passivity is the very same image that motivated the philosopher Georges Canguilhem to make the following comment in *The Normal and the Pathological* in 1942:

> It is the abnormal which arouses theoretical interest in the normal. Norms are recognized as such only when they are broken. Functions are revealed only when they fail. Life rises to the consciousness and science of itself only through maladaptation, failure and pain. A. Schwartz, following Ernest Naville, points out the glaring disproportion between the place occupied by sleep in men's lives and the place accorded it in the works of physiology...This is because the essence of sleep...is to let life go without calling it to account (Canguilhem 1989: 209).

As a passive state that temporarily suspended subjectivity, sleep could never be part of Canguilhem's demonstration of how the concept of health always implicated subjectivity. Thus, in reading Alfred Schwartz's 1939 review, Canguilhem merely recycled Schwartz's comment (a well-worn trope among sleep researchers by that time) about why sleep attracted so little medical or scientific interest. He entirely missed Schwartz's point, which was that some of the effects that new drugs had on sleep suggested that the passive theory of sleep should be abandoned.[2]

It was primarily through the application of the "graphical method"—the conversion of physiological events into graphical traces—that the phenomenology of sleep was transformed. The value of the graphical method was in its ability to depict time as a function of space, thereby allowing an investigator to take in the organic activity of a lengthy period of time in a single glance. Through the application of this method, the enormous amount of time consumed by sleep, emerged as a fragmented, but strictly rhythmical physiological performance by the middle of the 20th century. This prepared the way for investigators to study dreaming in terms of the regularity of its appearance, rather than through the analysis of its content.

Narrative structure and organization of chapters

My narrative thus describes how sleep came to be visualized as an active process. I focus on human, rather than animal, research, because a physiological correlate of dreaming could hardly appear in the absence of a speaking subject. I also emphasize the North American story, as REM first emerged in the very local cultural and scientific climate of Chicago.

Chapter one will introduce some nineteenth-century aspects of the scientific study of sleep. There were two dominant approaches to the problem of sleep in this period: one from the clinical point of view, and one from the perspective of physiological psychology. The most prominent clinical problem concerning sleep during this period was insomnia. In the hands of neurologists like William Alexander Hammond, insomnia became part of a continuum of sleep "derangements" that ranged from an occasional inability to fall asleep all the way to a full-blown

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[2]Schwartz 1939. Schwartz was a professor of medicine, and the Director of the Institute of Pharmacology and Experimental Medicine at Strasbourg.

neurosis. In popular books and in the margins of physiology texts, clinicians described the different ways that one could fall asleep, but they gave hardly a thought to what natural sleep actually was.

This question was eventually formulated during the last two decades of the century, in the midst of the French debates over hypnosis. At first, many investigators were enthusiastic about using hypnosis as a tool that could probe the unconscious mind. The hypnotic state was thought to be equivalent to sleep, and hallucinations experienced while under hypnosis were analogous to dreams. By the late 1890s, this analogy had begun to unravel, as hypnosis became tantamount to "suggestion"—the ability of the hypnotiser to influence the behaviour of his subject. By the time war broke out in Europe in 1914, hypnotism had fallen from grace, and its status as a form of "artificial sleep" was thoroughly rejected.

The study of fatigue, on the other hand, was just getting started. Its origins lay in the application of Étienne-Jules Marey's graphical method to the scientific study of labour. But one of Marey's students, Angelo Mosso, soon turned fatigue into an independent object of study. Sleep, as the product of intellectual and physical fatigue, was granted a minor but significant status in this program. Marey's "graphical method" offered a means of describing physiological phenomena without disturbing the organism's natural state, regardless of whether it was an animal, a patient, or a labourer. This was tremendously important for sleep research, as it provided a way of obtaining sleep's "natural signatures" (blood pressure, pulse, respiration and the like) without awakening the sleeper. The graphical method allowed the construction of an image of sleep from the perspective of the body, rather than from the mind.

But if sleep was becoming more and more grounded in the body in the early 20th century, psychoanalysis, which took dreaming as a model of mental activity, seemed headed in the opposite direction. My second chapter will deal with two distinctive approaches to dreaming in the early 20th century: those of Sigmund Freud, and Henri Bergson. Both produced important contributions to the study of dreaming around 1900, although only Freud has attracted much attention from historians. Freud developed a clinical technique based on the interpretation of dream content as an elaboration of repressed desire. For my purposes, his original significance lies in the fact that he turned dreaming into a vital function, linking it to health, rather than pathology. Bergson, on the other hand, took dreams to be a primitive experience of time as duration, which he contrasted to the mechanized, scientific vision of time as a succession of instants that could be represented by space. Bergson had a tremendous impact on French philosophy, which in turn affected the growth of psychological knowledge there, because most workers in that field were first trained as philosophers. His metaphysics encouraged psychologists to rigorously separate objective experiment and subjective experience.

Bergson's influence is charted in chapter three through the early career of Henri Piéron, a psychologist who had studied under Pierre Janet and Théodule Ribot. He turned to physiology, and produced a dissertation on sleep in 1912, which he published the following year. Piéron's study of sleep revolved around his method of "experimental insomnia," a technique that involved depriving his animal subjects of sleep until they slipped into a coma and died. Such a method was admirably suited for demonstrating a physiological need for sleep that operated outside of the parameters of fatigue laid down by Mosso in the 1890s. It also enabled Piéron to examine sleep from a physiological, rather than psychological, perspective. In this respect, Piéron's work rode on the earlier success of the "biological" theory of sleep proposed in 1904 by the Swiss psychologist, Edouard Claparède, which suggested that sleep was an "active defense" against fatigue. Such claims reinforced the belief that sleep should be studied through the observation of *comportement*, or "behaviour," rather than through the methods of introspective psychology.

At this point, our story shifts to the New World. In the U.S., sleep was little more than an appendage of clinically-oriented physiology until well after the First World War. Chapter four discusses the epidemic of "sleeping sickness" that spread through the U.S. and Canada in the wake of the influenza pandemic of 1918-19. Although influenza initially received the bulk of medical attention, by the early 1920s, American neurologists (who had little to say about influenza) began to focus on this mysterious disease that often left its victims in varying states of dementia. Their interest in sleep brought the topic out of the doldrums of hygiene and into the world of organic brain disease. To add to the high degree of visibility of sleep during this period, Constantin von Economo, the Viennese clinician who first described "encephalitis lethargica" in 1917, visited the U. S. in 1929. Drawing on Pavlov's description of sleep as inhibition, Economo argued that natural sleep was the product of a regulatory "sleep centre" in the brain, which was somehow damaged over the course of the disease. His ideas resonated with neurologists, who relied on the concept of brain localization for their own diagnostic authority.

Physiological interest in sleep had already been piqued by Ivan Pavlov's visit to the United States in the summer of 1923, also discussed in chapter four. Pavlov, depicted in the American press as "the last free man in Russia," was probably the most famous living physiologist at the time. The application of his method of conditioned reflexes to the study of sleep was at the top of Pavlov's agenda, and at every opportunity, he spoke about his theory of sleep as "generalized inhibition." Pavlovian conditioning, which dove-tailed with Watson's behaviourism, was already well-known to American physiologists and psychologists by this time. But for my purposes, Pavlov's significance lay in the currency his interest gave to the physiological study of sleep.

Chapter five examines the very local conditions of early sleep research at the University of Chicago during the Interwar period. When Pavlov visited the A.J. Carlson's Physiology Department in 1923, a young student named Nathaniel Kleitman was just publishing the first of a series of papers on the physiology of sleep. Kleitman, who would eventually establish himself as the first physiologist to dedicate his entire career to sleep, had adapted Piéron's method of "experimental insomnia" to the unique conditions of physiological research at Chicago. Kleitman used human subjects for many of his experiments, preferring to rely on innovative physiological recording techniques rather than post-mortems to frame his questions about sleep. His non-interventionist methods were more faithful to Marey and Mosso than they were to Piéron or Pavlov. They also reflected the growing interest, sponsored by the Rockefeller Foundation, in establishing a Department of Neuropsychiatry at Chicago. Alan Gregg, the head of the Medical

Sciences Division of the Rockefeller, hoped that Kleitman's research would lend neuropsychiatry some scientific credibility. To this end, he funded Kleitman's study of sleep for several years in the mid-1930s. The neuropsychiatry project collapsed, along with Kleitman's funding, in 1939, just as Kleitman was publishing his famous textbook on sleep.

The professional conflicts that destroyed the Rockefeller project played themselves out on a smaller scale in Carlson's physiology department. Edmund Jacobson, a clinician with an active interest in sleep, left the department in the wake of this disaster. He took with him a remarkable skill in electrophysiological recording, a talent that would be resurrected by one of Kleitman's graduate students more than a decade later.

While no amount of Rockefeller money seemed to be able to make the neuropsychiatry project work, the electroencephalograph was bringing together neurology, psychiatry, physiology and psychiatry like never before. The electroencephalogram (EEG), the topic of chapter six, was first discovered by a German psychiatrist named Hans Berger in 1925. But it did not receive much attention in the U.S. until 1934, when the Nobel laureate Edgar Adrian began experimenting with "Berger waves" in his laboratory at Cambridge. Adrian's interest in the EEG was short-lived. But there was also a distinctive American tradition of research, led by the millionaire financier Alfred Lee Loomis from his private laboratory in Tuxedo Park, New York. Unlike Adrian, the Loomis group used the EEG as a research tool, rather than attempting to explain its origins. Before their work ended in 1939, sleep had become one of the major focal points at Tuxedo Park. Loomis, who had a longstanding passion for precision instruments and timekeeping, used his extensive financial resources to construct an enormous kymograph that was able to record eight hours' worth of sleep at a time. By late 1935, his work had established a standardized set of five "sleep stages" based on distinctive EEG tracings, though which the brain "cycled" throughout the night. This image of sleep as a phenomenon of brain-regulated timekeeping was to dominate sleep research for the remainder of the century. In 1936, he was joined by Hallowell Davis, a neurophysiologist from Harvard who had just begun to dabble in psychoanalysis. His hope was to apply the EEG as the basis for a study of individual differences, modelled on ego psychology. His neo-phrenological aspirations illustrate the vast potential this instrument was thought to have in the years before the Second World War.

My final chapter will examine the conditions under which REM finally appeared to scientific perception in the year that led up to Aserinsky and Kleitman's publication of their discovery in 1953. Aserinsky, on the advice of Kleitman, had originally set out on a project to study eye movements as an index of wakefulness in infants. After observing regular periods of eye quiescence in these infants, he turned to observing adults. Here, he found periods of eye movement instead, and he quickly associated these with dreaming. After soliciting the advice of Edmund Jacobson, Aserinsky reconfigured his experimental assemblage to include EEG and Jacobson's technique of measuring eye movements. With months, he had managed to convince most people around him, including Kleitman, that dreaming was closely related to these periods of eye movement.

Although he himself was no Freudian, Aserinsky's discovery came in the midst of a revival of scientific interest in the dream in the United States. Once the province of an elite group of neurologists and ambitious psychiatrists, psychoanalysis had assumed a dominant role in the sciences of mind and brain by the early 1950s, and the systematic study of dreams outside of the clinic was high on the agenda. Aserinsky, anxious to leave Kleitman's laboratory, abandoned his research to William Dement, a young medical student with a keen interest in psychiatry. Dement recognized the potential of REM to bridge the gaps between the physiological laboratory and the psychiatric clinic. By the mid-1960s, his experiments with psychiatric patients and his theories about "dream deprivation" had brought sleep research out of the backwaters of physiology, and onto the centre stage of the neurosciences.

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In its outlines, the structure of this narrative is by no means original. The general trajectory of the history of sleep research as a laboratory science, beginning with Piéron, moving through the work of Pavlov and von Economo, and reaching an apogee in the detailed neurophysiology of the post-war period has been laid out in previous studies (Lemaine *et al.* 1977; Schiller 1982). What has been missing in such studies, however, is an examination of how instruments and laboratory practises have shaped sleep research. One group of sociologists, for instance, has analysed the direction of research in sleep physiology according to the notions of "risk" and "strategy" (Lemaine *et al.* 1977). This study plays down the idea that Kuhnian "paradigms" determine the shape of scientific research, and emphasizes instead the many different tactics consciously adopted by investigators in this field. The changing phenomenology of the sleep laboratory brought on by the EEG, however, is relegated to an appendix.

Historical studies of sleep research typically treat sleep and its associated phenomena as a semantic container that is filled up with scientific knowledge over time. The reader is introduced to notions of sleep in Antiquity, only to be immediately thrown into the Modern period, replete with its vast array of facts about sleep with little or no relationship to each other (Schiller 1982). A focus on the history of particular aspects of sleep, such as narcolepsy or encephalitis lethargica, tends to generate a more coherent narrative within a limited scope (Dement 1993; Peng 1993). Unfortunately, such studies leave disciplinary context and laboratory practise behind, in favour of merely recounting the steps that led towards the acceptance of a theory or diagnosis. The reader is obliged to believe that such research took place in virtual isolation, despite the rather striking fact that the individual scientists involved are rarely, if ever, committed to sleep or its disorders as their sole research interest.

The present study is a history of sleep research that concentrates on how sleep has made visible in an investigative context. Our central interest has been in charting the progress of the various techniques and technologies that have helped turned sleep, the epitome of inactivity, into an active problem for experimental biology. With the introduction of electroencephalography, sleep (and later, dreaming) entered the field of physiognomy (Dagognet 1982). It achieved a kind of morphology that it had never had before. This shape was dynamic, and as such, the electroencephalography of the 20th-century sleep laboratory becomes part of the history of time, and time-keeping (Attali 1982; Webb 1994). The discovery of rapid eye movement belongs to the larger history of post-war brain research that has struggled to elucidate the regulatory nature of sub-cortical structures, supplanting the earlier experimental tradition of mapping motor and sensory responses onto the superficial cerebral cortex. Investigating sleep turned out to be one of the earliest and most profound ways to demonstrate that the brain kept time.

* * * * *

Was rapid eye movement socially constructed? As Ian Hacking points out, the meaning of the term "social construction" depends upon the nature of the classificatory work that is being done (Hacking 1999). A "natural kind" of classification, like quark or electron, cannot react to the description that is imposed on it. A "human kind" of classification, like child abuse or internet shopping, often solicits strong reactions from those people who are thus described. This interactivity of human kinds raises radically different sorts of questions that are absent in the indifference of natural kinds. Moral and ethical criteria, for example, can be self-imposed in case of human kinds ("is it good or bad to shop on-line?"), and such activity can change the meaning of the classification. This phenomenon is absent in the case of natural kinds.

Rapid eye movement is a way of classifying sleep. It was built upon the "sleep stages" that were first revealed by the application of electroencephalography to sleep in the 1930s. In this sense, REM is a natural kind, and it is amenable to an analysis of how political and disciplinary interests, research communities, industrial technologies, and experimental assemblages converged in a certain way, at a specific time, to generate the phenomenon of REM. But REM has also been treated as a way of classifying human experience. Had it not been considered to be an objective sign of dreaming, it is unlikely that REM would ever have held any great significance outside of the narrow field of sleep physiology. Although REM periods have been detected in most mammals and in many birds, only human beings can offer a dream report. If dreaming, and thus REM, is a human kind what sort of "interactivity" should we be looking for?

It seems to me that this question is not the appropriate one for understanding the history of rapid eye movement, or of the sleep physiology that created it. Sleep research has long aimed at naturalizing dreams by delimiting them to sleep periods and brain activity. The individuals who developed modern sleep physiology were all dedicated to adapting the language of physiology and biology to the experience of dreaming. In particular, they relied on the concept of function. Once a self-described experience (of fear, of fatigue, of having a dream) could be expressed in terms of its supposed biological or physiological purpose, then it became a "real question," in Nicholas Jardine's sense, for sleep researchers (Jardine 1991).

The history of sleep research revolves around this question of function. Owen Flanagan, a philosopher of science, has recently argued that dreaming and REM periods cannot be analysed in the same way, because they serve very different purposes (Flanagan 1995). Sleep is a biological phenomenon, and its stages must have some sort of organic function. Flanagan compares dreams, on the other hand, to "spandrels." Like the decorative carvings around an arch, dreams serve cultural, but not organic, purposes. But when and why did anyone start thinking that dreams served any useful purpose in the first place? Flanagan ignores this historical question, and instead supports a variant of ontological dualism that carves the world up into natural (sleep) and cultural (dreams) entities. Bruno Latour has argued that such a system is an effect of scientific work, rather than a cause of scientific knowledge (Latour 1989). Latour encourages us to abandon this sort of puritanism, and think in terms of "hybrids"—entities that are always in the process of becoming purely natural or purely cultural. But this process is endlessly deferred to some future, more "modern" era.

Latour and Hacking offer a more appropriate historical mode of inquiry into REM than Flanagan. The discovery of REM, as I will demonstrate, came out of a desire to eliminate just those boundaries between nature and culture that Flanagan's arguments seem to preserve. The characters of my story are, for the most part, physiologists who always wanted to bring the nature of society within the boundaries of their own discipline. If historians adopt Flanagan's ontology, these actors appear to simply stumble over facts, like tripping over chairs in a darkened room. My point is that these physiologists (and neuropsychiatrists, and psychologists) were always *looking* for a particular place to sit. Flanagan's philosophy would erase this aspect of REM's history.

The tradition of sleep research that generated REM's discovery was rooted in a long series of attempts to turn mind into brain. To accomplish this, the peculiar difficulty of human kinds—often understood by sleep and dream researchers in terms of "suggestion"—had to be meticulously avoided. This was accomplished largely through the epistemological power of the graphical method that dominated physiological research by the end of the nineteenth century. The standardized, reproducible traces generated by recording instruments, when calibrated to subjective testimony, supplanted introspective descriptions of experience. Like their psychological colleagues, sleep researchers distanced themselves from introspective psychology by constructing a subject that reflected their experimental practises (Danziger 1990). But where American psychologists relied on psychological tests to create an "aggregate subject" that supplanted introspection, sleep researchers turned instead to a biological subject. They struggled to find a way to make the brain speak for itself, and believed they had succeeded with the EEG. Electroencephalography proposed a new way of studying the brain. Instead of applying the standard reflex model of stimulus and reaction, the EEG allowed physiologists to chart the rhythms of brain activity. The brain's internal electrical organization appeared to simply write itself, without interference. The discovery of periods of rapid eye movement was built upon this new interest in rhythm. The American neuropsychiatry of the 1950s, with its heady atmosphere of a biologically-charged psychoanalysis, provided a perfect forum in which rapid eye movement seemed to allow the brain to narrate its own dream report.

Chapter I

Sleep at the margins of knowledge: insomnia, fatigue, hypnotism

Nineteenth-century investigations of sleep relied primarily upon analogous states, both of the mind and of the body, to illuminate the nature of sleep. The problem of sleep was framed in terms of three pathological states: insomnia, fatigue and hypnotism. The reflex arc was at the core of every explanation of sleep. Whatever normal sleep might be, any explanation had to account for its appearance as a product of decreased stimulus.

By the end of the century, sleep had begun to coalesce around the concept of fatigue. Hypnotism had lost its status as a form of "artificial sleep," and insomnia was no longer part of a great continuum of nervous disorders. The graphical technologies that sustained the study of fatigue were poised to be applied to the study of sleep.

Blood in the brain

The nineteenth-century origins of psycho-physiology—the study of how physiological process are related to psychological phenomena—can be traced to the examination of the circulation of the blood in the brain. If the brain was indeed the organ of mind, then the dynamic processes within must somehow reflect mental events. Anatomy could, at best, merely illustrate the material framework of such processes. Trading in solids, rather than fluids, anatomy could not hope to capture the physical basis of the dynamic affairs of the mind.

The cerebral circulation, on the other hand, offered other possibilities. Blood was the nutriment of the brain, and the study of its movement was inspired by the hope that it could be linked to the brain's role in sensation—the basis of all nineteenth-century empirical theories about mind.

The mundane pathology of sleep: Georges Cabanis

The study of sleep in the nineteenth-century was closely linked to this "alimentary" model of psycho-physiology. Decades before the so-called "materialist revolt" of German physiologists in 1848, interest in sleep seems to have consisted largely in demonstrating that dreams did not have a supernatural origin, but were instead caused by the suspension of the activity of the external senses (Ripa 1988: 133-154). The origins of this position can be traced back to the "bible" of French physiology, George Cabanis's *On the relations between the physical and moral aspects of man* (1802), an obligatory reference point for French physiologists throughout the greater part of the nineteenth century. Cabanis (1757-1808) considered dreams to be the essential activity of the mind in sleep, which he assumed could be explained in terms of cerebral circulation.

For Cabanis, the study of the body was the means by which the formation of ideas could best be understood (Staum 1980). Dreams, then, were a sign of the brain's activity in sleep, rather than the result of some form of divine intervention. The hallucinatory nature of dreaming, however, implied that it was more akin to madness than to the normal processes of thought. Dreams, like delirium, were the product of an imbalance of fluids in the brain. Citing the Edinburgh physician, William Cullen (1710-1790), as the first to recognize "the constant and definite relations between dreams and delirium," Cabanis extended Cullen's original "hint" that sleep came to the various organs of the body unequally. The "irregular and confused images that have no basis in the reality of objects" that characterized both dreams and delirium were the result of "the partial stimulation of the points of the brain that correspond" to the organs (Cabanis 1981: 602). This "partial stimulation" was itself due to the contraction of nervous powers towards the brain that was, for Cabanis, as for Cullen, the very definition of sleep. As the centre of stimulus was relocated from the periphery to the interior, the flow of blood naturally followed: "for the circulatory movements always tend especially toward the points of the animal economy at which the stimulating causes gather" (Cabanis 1981: 615). Cabanis seems to have had some difficultly convincing his colleagues that there was some sort of increased stimulation of the brain going on during sleep—he was obliged to include two lengthy footnotes at this point (different in the first and second editions) that attempted to make clear why there would be more blood in the brain, even while it acted less in sleep. His idea that the "causes of excitation concentrate to produce it [sleep] within the brain" ran against the established principles of his time. Cabanis himself could only defend his claim that the same causes that stimulated the brain into dreaming actually dulled all other aspects of the body by deferring his reasoning to "physiological laws on which it would be inappropriate to dwell at this point" (Cabanis 1981: 615-616).[1]

Part of this activity consisted of an increased flow of blood to the brain that created a pathological alteration of thinking (dreaming) in sleep, as it fulfilled its physiological function of restoration. Cabanis never seems to have never attempted to measure the blood flow to the brain during sleep. Even if he did, the fact that he never described any procedure of this nature in his most important physiological text reveals that such experiments—if they indeed existed at all—held little rhetorical value in the early years of the French republic. Instead, he relied on an analogy from pathological anatomy, the dominant means of creating evidence for physiological arguments at the time (Temkin 1951; Foucault 1973). The confused images of dreams were identical to those of delirium, which, Cabanis argued, depended on an alteration of the normal state of the brain:

> Anatomical sections have shown, in a considerable number of subjects who died in a state of dementia, different alterations in the color, in the consistency, and in all the perceptible appearances of the brain...

Even if structural defects were not visible, Cabanis insisted that the damaging effects of excessive and unequal blood flow left a visible trace:

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[1]By the time he published a second edition (1805), Cabanis had apparently had enough of his colleagues' criticisms, and added another footnote to this effect: "Some people appear to have misunderstood the meaning of this passage. I have not said that there is more action in the brain during sleep than during wakefulness, but that sleep is not merely a passive function...[the brain] rests from wakefulness by sleep, and from sleep by wakefulness; but it is never in that inert state imagined by men who carry on the study of life notions only for a rough mechanism."

> The vessels of the ventricles have been found to be sometimes stuffed with blackish, pitchlike, and deleterious matters. As in lesser degrees these organic disorders have been accompanied several times by corresponding and proportional disorders of the mental faculties, it is difficult not to attribute it to them when they are found in individuals affected by the maniac and furious madness (Cabanis 1981: 609).

Dreaming was on a continuum with other "disorders of the mental faculties," and thus it was to be expected that it too was caused by a transient imbalance in cerebral blood flow. This was, of course, the dominant medical view of madness in republican France, and it was shared by most of Cabanis's colleagues; most notably, François-Joseph-Victor Broussais. Excessive blood flow to various organs of the body accounted for a huge range of illness, and also supplied the rationale for that most notorious therapy, blood-letting.

Cabanis's efforts to depict sleep as an active process were difficult to understand in the context of the clinico-anatomical methods that dominated medical thinking at the beginning of the nineteenth century. How could sleep be represented as an activity, when it consisted in the slackening of all motions? Here, Cabanis turned to observing the movement of sleepers, noting that sleep-walking, which seemed to involve both judgement and the will, indicated that sleep was not entirely uniform. That is, at any given time, not all organs slept equally. "Asleep" could describe both the active, but pathological, state of the entire body, as well as the passive, restful and normal condition of the organs. But the anatomical evidence for this position was not forthcoming. Pathological anatomy could not represent process. It could only present the reality of the organism's state at the moment of death, hence the epistemological value of dissecting subjects who had expired in a state of dementia. Dreaming seemed to present a set of phenomena similar to madness or dementia, and thus Cabanis attributed it, along with sleep, to a similar cause.[2] The internal dynamics of sleep, if they even existed, remained hidden from medical view.

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[2]The immense popularity of phrenology in the early nineteenth century also fed into theories of sleep. Phrenology assigned psychological faculties to distinctive regions of the brain, thereby supporting the study of personality based on measuring the bumps on a person's skull. Robert Macnish (1802-1837), a Glasgow physician, relied on such theories to explain sleep and dreaming (Macnish 1977). This tendency was much more pronounced in the revised second

The relationship between sleep and madness persisted well into the second half of the nineteenth century. The historian Yannick Ripa has argued that, in the French case, this was accomplished largely through the association between dreaming, pathological anatomy and the illusions and hallucinations of insanity that were forged by writers like Cabanis in the early years of the Republic (Ripa 1988: 133-154). But outside of France, medical interest in sleep was by no means limited to the explanation of its mental phenomena. Dreaming was an obvious point of access for investigators like Cabanis, who wanted to plumb the depths of the mind by studying its anatomical and physiological correlates. But for practising clinicians, its was the transformation of the sleep regimen and its association with mental disease that provided a focus for their theories about the nature of sleep.

The insomniac as neurotic: William Alexander Hammond

If the consistent number of papers produced on the subject is any indication, insomnia has been a widespread and longstanding medical concern since the late nineteenth century.[3] But the creation of insomnia as a symptom of an underlying mental disorder can be traced back, at least in the American context, to the work of William Alexander Hammond (1828-1900). Hammond, who was appointed surgeon general of the U. S. Army in 1862 (only to be dismissed sixteen

edition (1834) of his book than in the first (1830), which might explain why this popular book, which was translated and reprinted numerous times, was almost entirely forgotten when phrenology fell into disrepute. Macnish's name was purged entirely of sleep and dreaming, and became associated with multiple personality, through his description of the case of Mary Reynolds (the "lady of Macnish"), which was reintroduced to the medical public by Silas Weir Mitchell in 1889. See Henri F. Ellenberger, *The Discovery of the Unconscious: The history and evolution of dynamic psychiatry* (Basic Books: New York, 1970), pp. 128-129.

[3]An examination of the *Index Medicus* (1879-1954) reveals that insomnia and (after 1880) narcolepsy were the two most frequently discussed sleep pathologies for almost every year. This would probably not be the case today. To the surprise of many clinicians, excessive daytime sleepiness (often as a consequence of sleep apnea), rather than insomnia, seems to have become the most common reason for referral to a sleep clinic since the mid-1980s (Broughton* 1999).

months later), had an extraordinarily successful career as one of the first neurological specialists in America (Blustein 1986 & 1991). His theory of sleep, which he began to formulate as early as 1854, proved to be a crucial element of his lucrative New York practise. His diagnosis of thousands of his patients with a condition he named "cerebral hyperaemia"—the excess circulation of blood in the brain—was the leading edge of a series of indeterminate, but unifying, diagnoses (which would include George Beard's "neurasthenia" by 1880) that characterized the nosology of mental illness in America after the Civil War (Grob 1983; Lutz 1991; Shorter 1992).

Cerebral hyperaemia described a combination of symptoms that could include headaches, dizziness, forgetfulness, hallucinations, ringing in the ears, and back pain. But for Hammond, the distinctive sign of this disease was insomnia. His clinical observations were based on a theory of sleep in complete opposition to Cabanis, whose ideas were still widely held in mid-century. In 1854, while he was serving as a physician to the U. S. Army in Kansas, Hammond saw a patient who had suffered a cranial fissure in a railroad accident (a popular source of neurological research material, and perhaps second only to warfare). The man's scalp over the fissure appeared to rise when he was in a coma. After he recovered, the skin was level with the rest of his scalp, except when he was asleep, at which point the skin was slightly depressed. In 1860, Hammond, on medical leave from the Army and awaiting a position at the University of Maryland at Baltimore, began to experiment on animals. He trephined the skulls of dogs and rabbits to observe the movement of the *dura mater* that lay just above the cerebral cortex. Hammond peered through this tiny window (covered by a watch glass) on the brain while submitting his animal subjects to narcosis by ether, chloroform and opium, and comparing the results to those gleaned from natural sleep. Based on the colour and size of the brain, sleep was *not* on a continuum with any of these states. This was contrary to most theories about sleep that originated in the late eighteenth and early nineteenth centuries, but it fit well with Hammond's emerging ideas about creating a "positive mental science" based on the "facts of physiology" rather than the "metaphysics" of psychology (Hammond 1865 & 1866).

In his review of Henry Maudsley's *The Physiology and Pathology of Mind*, Hammond suggested that "it is now necessary that the unholy barrier set up between psychical and physical nature be broken down" (Hammond, as cited in Blustein 1991: 165). He rejected the doctrine of substance dualism, and the psychophysical parallelism that went along with it. The brain was not the organ of some other substance called "mind." Mind was nothing more than the performance of the brain. Inspired by the evolutionary writings of Darwin and Spencer, Hammond attempted to interpret all neurological phenomena in materialist terms. As Lorraine Daston has pointed out in her study of British psycho-physiology, sleep became a testing ground for physiologists wanting to turn mind into body (Daston 1978). Because volition was suspended it sleep, it seemed a reasonable prospect that if the natural ebb and flow of the will in sleep could be understood, other, more complicated mental phenomena would follow.[4] Sleep offered a possible field of inquiry where the conflicts between free will as a moral imperative and mental science as a law-governed enterprise might be resolved (Daston 1978).

Following the British researcher, Arthur E. Durham, who presented his investigations of sleep at the Oxford meeting of the British Association for the Advancement of Science in June of 1860, Hammond took as a first principle that sleep could be defined in terms of its physiological function, which Durham had described as "that period of cerebral inactivity during which nutrition of the brain substance takes place" (as cited in Blustein 1986: 31). This meeting was, of course, became notorious for the confrontation between Thomas Henry Huxley and Bishop Samuel Wilburforce over the evolution of man. But Durham's arguments were also part and parcel of the growing strength of Victorian materialism. His concept of "cerebral inactivity" was crucial for Hammond's understanding of sleep. The brain's activity was dominated by thought and the exercise of the will, both of which were lacking in sleep, when the brain was at rest. Thus

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[4]An interesting exception to the idea that the will was suspended in sleep was the Marquis Hervey de Saint-Denys, who argued, in 1867, that one could control the direction of one's own dreams through the cultivation of certain habits, which included keeping a meticulous record of one's dreams (Hervey de Saint-Denys 1867). The Marquis was no medical man, however. He was a sinologist and historian at the *Collège de France*.

the circulation of the blood to the brain was analogous to that of any other organ, and Hammond expressed his dismay that so many investigators had been deceived on this point in the past:

> It is well established as regards other viscera, that during a condition of activity there is more blood in their tissues than while they are at rest. It is strange, therefore, that, relative to the brain, the contrary doctrine should have prevailed so long, and that even now, after the subject has been so well elucidated by exact observation, it should be the generally received opinion that during sleep the cerebral tissues are in a state approaching congestion (Hammond 1892: 17).[5]

The source of this confusion, Hammond argued, was the ontological dualism that dominated most of the physiological thinking of the day. Mind and body were considered to be distinctive and separable entities, and this contributed to the belief that thought, the activity of the mind, consisted primarily in *picturing*. Hammond's vision of thought, on the other hand, was tied inescapably to the will. "Thinking," he insisted, "is an *action* which requires cerebral effort, and which is undertaken with a determinate purpose. We will to think, and we think what we please." "But," he continued, "it is very different with our dreams, which come and go without any power on our part to regulate or direct them" (Hammond 1892: 87. Italics original). Citing John Locke's *Essay Concerning Human Understanding*, Hammond argued that dreaming had no relationship whatsoever to thought, as the latter was always accompanied by the conscious awareness of oneself, which the images of dreams inevitably lacked. Dreaming was not, as Cartesian doctrine would have it, the mode of the soul's activity in sleep. Sleep involved the annihilation of the will, and, as a consequence, the temporary abasement of mind.

Dreams were not an imbalanced or disordered form of thought, as Cabanis had argued. They were not thought at all. And if dreams were not thought, then the brain was inactive in sleep, and cerebral circulation diminished. So what *were* dreams? Here Hammond relied on a combination of association theory and a Victorian doctrine of the will as self-restraint to dismiss dreams as anything but the random activity of the imagination run riot. Dreams were "either impressions made upon the mind at some previous period, or produced during sleep by bodily

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[5]This book was a revised edition of his *Sleep and Its Derangements* (Lippincott: Philadelphia, 1869).

sensations. These impressions, however they may be formed, are subjected to the unrestrained influence of the imagination" (Hammond 1892: 90). Memory, which was to serve such an enormous explanatory role in Freud's theory of dreams thirty years later, was entirely absent from Hammond's work. But his physiological evidence readily confirmed his theory about the circulation in sleep. In an appendix to his book, Hammond described one of his experiments, probably conducted in the 1860s. He devised a primitive manometer to detect changes in cerebral pressure in trephined animals. Although his "cephalohemometer" offered only the fluctuations of coloured water against an index, they nevertheless allowed him to measure, rather than simply observe, the degree to which the size of the brain was reduced in sleep. "Nothing can exceed the conclusiveness of experiments of this character," Hammond argued. "No mere theorizing can avail against them" (Hammond 1892: 245).

Cerebral hyperaemia, which Blustein has described as "precisely equivalent to insomnia," formed the basis of Hammond's success as a neurologist during the 1870s and early 1880s. By the end of the '80s, however, it had began to unravel as a diagnosis. It has hardly recovered since, even in the historical imagination. The historian of psychiatry, Edward Shorter, has been particularly harsh in his assessment of Hammond, whom he dubs "the dean of American reflex theorists" (Shorter 1992: 239). "Few documents," Shorter says, "could testify more eloquently to the retarded state of American medical practise around the turn of the century than the work of William Alexander Hammond," a view that was widely propagated by Hammond's contemporaries in the 1890s (Shorter 1992: 34). Blustein has rehabilitated Hammond by emphasizing his work on sleep and its empirical grounding, rather than focussing on his clinical descriptions of cerebral hyperaemia. Although his clinical work did not rely on physiological instrumentation, she credits him with introducing a number of items into American medical practise, including the ophthalmoscope for neurological diagnosis, Duchênne's trocar for muscle biopsy, and the dynamometer, which measured muscle strength (Blustein 1991: 118-133). His work on poisons, which he conducted with Silas Weir Mitchell (1829-1914) in the late 1850s, led Charles-Édouard Brown-Séquard (Claude Bernard's successor at the *Collège de France*), to describe Hammond as the "first original Physiologist in the United States" (as quoted in Blustein 1991: 52).

Nonetheless, Blustein is obliged to concede that "by 1890 'cerebral hyperaemia' and the theory of sleep on which it rested had been seriously undermined."[6] American neurologists were beginning to strive for a higher degree of precision in their diagnostic criteria, this corresponded to a professional skepticism about the ability to explain all aspects of mind by reference to physiological function. This trend was parallelled by the arrival on the scene of the "psychological paradigm," which suggested that the cause of certain mental illnesses could be traced back to the activity of the mind, and, in particular, the emotions (Shorter 1992: 233-266). Hammond, whose work was steeped in the reductionist reflex theories pioneered by Marshall Hall in the 1830s, could have no truck with such ideas, and his work was subsequently discarded. Blustein concludes that "cerebral hyperaemia, along with the Hammond-Durham sleep theory, would disappear, not only from the medical textbooks, but also, for close to a century, from the histories of medicine as well" (Blustein 1986: 51).

This may be an accurate assessment of Hammond from the perspective of the history of neurology and psychiatry. But the fate of Hammond's research in physiology was quite different. Although the names of both Hammond and Durham disappear from the theory of sleep based upon a decreased cerebral circulation, their observations retained the status of empirical facts well into the twentieth century, and were staunchly defended by some of the leading physiologists of the day. In France, Charles Richet described the disappearance of excitability in

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[6]Blustein 1986: 48. In an ironic turn, Blustein cites an 1889 paper by Brown-Séquard that contradicted Hammond's ideas about sleep, declaring it to be the result of "inhibition of mental activity" rather than a decreased circulation of the blood. The relationship between inhibition and sleep was brought to physiologists largely through the work of Ivan Pavlov, and will be discussed in chapter four.

a dog's cortex upon compressing the carotid arteries, inferring a similar process in sleep.[7] In Italy, Angelo Mosso, whose 1891 book on fatigue was translated into several languages and went through numerous editions, produced an important book on the circulation in 1894. In it, he argued that in sleep, the blood pressure in the brain declines, while that of the periphery increases (Mosso 1879). In the United States, the growth of physiology as a field independent of medical education meant that all physiological facts did not have be harnessed to theories of disease (Fye 1987). Here, where Hammond's reputation as a scientist seemed to have suffered the most among neurologists and psychiatrists, we find a leading American physiologist, William Henry Howell (1860-1945) confirming a decreased circulation in sleep (Howell 1897). One of his students at Johns Hopkins University even extended this idea into the realm of hypnotic sleep (Walden 1901). This is not to say that the evidence for a decreased circulation in sleep went unchallenged. When Howell first published his *American Text-Book of Physiology* in 1896, a mere three pages was dedicated to the question of sleep, and Hammond was not mentioned at all. It was not until subsequent editions that Howell devoted a separate chapter to sleep, largely based on his own research.[8] In later editions, Howell noted that his results had been challenged, and that theories of sleep based on a diminution of blood flow in the brain had been "brought into question." Nonetheless, he simply cited these critical references and continued on, as though no evidence had been brought against his work whatsoever (Howell 1921).[9] Nathaniel Kleitman, who became known as the "dean of American sleep research," complained of a similar situation as late as 1939. Physiologists refused to let go of the idea that the cerebral circulation diminished

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[7]Charles Richet and Broca, *Comptes rendus de la Société de Biologie*, Feb. 12, 1897. Hammond had conducted a similar experiment on himself, cutting off his brain's blood supply until he fainted.

[8]On Howell's textbook and its significance, see Fye 1987: 203-204.

[9]Howell cites J. F. Shepard's 1909 article in the *American Journal of Physiology*, p. 23, and Brodmann's 1902 article in *Journal für Psychologie und Neurologie* 1, p. 10 (Shepard published a book entitled *The Circulation and Sleep* in 1914) as his critics. Howell's chapter on sleep was not abandoned until the entire textbook was reconstructed by John F. Fulton in the 1940s.

in sleep.[10] Neurologists may very well have trashed Hammond's nosology by the end of the century, as Blustein argues. They may also have rejected his "reflex theory" paradigm, as Shorter suggests. But his ideas about the circulation decreasing in sleep persisted just where the locus of scientific medicine was supposed to be—in the physiological laboratory.

How was this possible? The fact of the matter is that, unlike neurologists, physiologists had *not* abandoned the concept of reflex in their investigations. The focus of their interests simply shifted from away from disease aetiology and towards the field of psychology, which was undergoing a radical expansion in the United States during the first decade of the twentieth century (Danziger 1990; Smith 1997). The entry of psychology into industry, education and the military meant that physiological knowledge now had a new conduit out of the laboratory. And while the reflex paradigm was rejected by clinicians, it continued to be an indispensable part of how psychologists constructed the normal human mind (O'Donnell 1985).

By the end of the 1830s, reflex action formed the basis of all materialistic physiology, largely due to the work of Marshall Hall (1790-1857) and Johannes Müller (1801-1858). Hall proposed three different reflex systems: the "spinal system," which was unconscious and depended only on the presence of the spinal cord; the "excito-motory" system, which involved conscious sensation; and the "sensori-volitional" system, which involved the will (Boring 1957: 35-39; Smith 1992: 66-79).[11] Müller took a more integrated view, arguing that while the brain and spinal cord were anatomically distinct, their functions were not so easily separated. But in spite of their differences, both agreed that it was the "reflex arc" that translated sensation into movement.

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[10]"...the idea of cerebral anemia as a cause or result of sleep refuses to die, and reviewers like Allen and Mott seem to accept Mosso's results as if nothing had been done on the subject since 1880" (Kleitman 1939: 74).

[11]On the early background of the reflex concept, see Fearing 1930, and Canguilhem 1955. Fearing, a physiologist at Northwestern University in Chicago, offers no hint that he is writing the history of a "dead" concept in 1930.

Hammond's theory of sleep was thoroughly grounded in this concept of reflex. The lack of movement that characterized sleep was, he thought, the result of decreased stimulation, due to the onset of darkness, diminishing noise, and the decrease in intellectual and physical activity that accompanied sleep. Increased stimulation demanded a greater blood flow to the brain (described as increasing the affinity of the brain tissues for oxygen). When the stimulation diminished, so did blood flow, and sleep ensued. Such an explanation could not stand on its own, as lack of sensory stimulation was neither a necessary nor sufficient condition of sleep. It was clear that normal sleep, which was regularly recurrent, was based on a cycle. And to explain this cycle, physiologists had to have recourse to some regulatory principle that incorporated the instantaneous nature of the reflex into a longer-term explanation of periodicity. Hammond, and his contemporary, Durham, described this periodicity in terms of a build up of "waste products" in the brain due to its increased activity during wakefulness. By the end of the century, this idea began to be described in terms of *fatigue*.

The physiology of fatigue

In a provocative analysis of how the metaphor of the "human motor" originated in materialist physiology and ended as an organizing principle for human culture by the beginning of the twentieth century, Anson Rabinbach has argued that physiological interest in the problem of fatigue can be traced back to the work of Herman von Helmholtz, a student at Johannes Müller's laboratory in Berlin in the 1840s (Rabinbach 1990). Helmholtz provided the most consistent and clear statement of the law of the conservation of energy, which, when applied to physiology, suggested that all organic phenomena could be understood in terms of work. This included mental processes. Thought was not, as Cabanis had suggested, something that was secreted by the brain, as bile was secreted by the liver. It was, like all the brain's activities, the product of a transformation of energy, something which was, in principle, amenable to detection and measurement by traditional physical methods.

Fatigue was certainly one of the most palpable experiences of the exhaustion of energy. Likewise, sleep was depicted as the most immediate experience of its restoration. Curiously enough, Rabinbach never mentioned sleep at all, most likely because a central theme of his narrative was the rise of the scientific study of work, from both a physiological and from a Taylorist/managerial perspective. In such a history of muscle and mind, there is little room for the physiology of a seemingly passive activity like sleep.

Neither sleep nor fatigue were the topic of sustained physiological research until the late 1870s. Hammond's work, which we have discussed above, was oriented towards the clinic. What little research he did on sleep was left unpublished until he could incorporate it into a comprehensive theory of nervous illness, which goes some way to explaining why his theories on sleep were forgotten once his diagnosis of "cerebral hyperaemia" was abandoned. Between 1845 and 1875, mechanistic physiology was dedicated to the examination of the micro-phenomena of animal nerve-muscle preparations. This experimental assemblage could physiologists quite a lot about fatigue in terms of the rate and frequency of induced electrical shocks were required to exhaust a muscle. But these models said little about how this translated into the subjective experience of fatigue, nor could it offer more than the most rudimentary analogies regarding the phenomenon of sleep, which pervaded the entire body. This latter approach to the physiology of fatigue appeared with the work of the French physiologist, Étienne-Jules Marey (1830-1904). I consider Marey's efforts in this regard to be an important precursor to the biomedical holism that provided such a striking counterpoint to the mechanistic medical and physiological practise of the interwar period (Lawrence & Weisz 1998). As a name for a scientific aesthetic that claims to examine wholes, rather than parts, "holism" only appeared well after Marey's death, in the musings of the South African statesman, Jan Christiaan Smuts (Smuts 1926). But Marey's attempts, along with those of his student, Angelo Mosso (1846-1910), to use graphical methods to capture and measure organic activity in its natural setting mark him as a methodological holist, if such a term is permitted. For my purposes, Marey is important because he created an alternative to vivisection, which had long been the mainstay of physiological research. This opened the door to a study of organic performances that placed a premium on the ability to record such events without disturbing their trajectory. Fatigue was one of Marey's first attempts in this direction, but through Mosso, sleep was eventually brought under the purview of the graphical method.

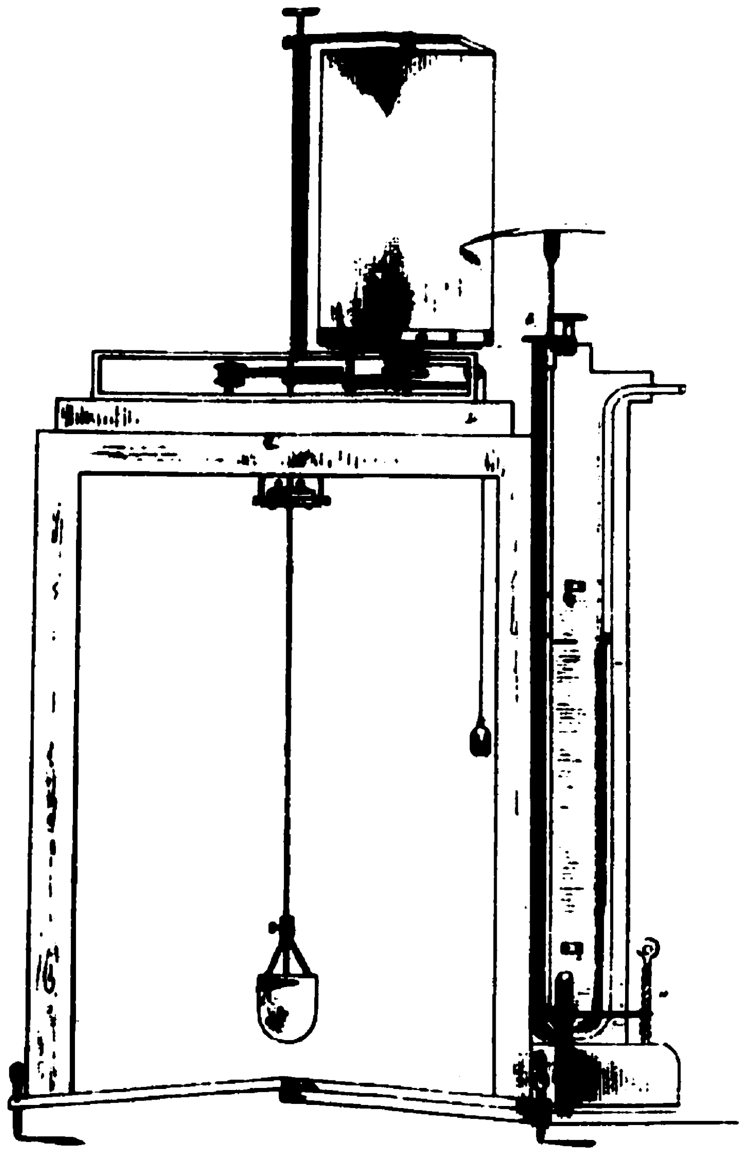
Synchronization: the kymograph

The graphical method can be traced back to the work of one of Johannes Müller's students in Berlin, Carl Ludwig. In 1847, Ludwig described a new device in the pages of *Müller's Archiv* that would transform physiological practise [Figure I]. No longer limited to the rational reconstruction of motion through physical, chemical or morphological analysis, physiologists would become actively engaged in the visual representation of biological time (Borell 1987; Braun 1992; Chadarevian 1993). Ludwig's invention, the kymograph, or "wave writer," was the first self-inscribing physiological instrument, the origins of which have been traced to similar instruments used in ballistic studies already in the 1830s (Hoff & Geddes 1959 & 1960). Its invention offers a poignant instance of how the mechanical *analogy*, an integral part of physiological thinking since Descartes, became a mechanical *identity*, through the intervention of representational instruments.

Ludwig wanted to understand the influence of one vital system (respiration) on another (the circulation of the blood). To accomplish this, he needed to compare changes in the air pressure in the thoracic cavity to changes in blood pressure. The measurement of either phenomenon posed no difficulty—instruments were readily available for both applications. It was the *synchronization* of these two rapidly-fluctuating measurements that framed Ludwig's investigation, as the precise recording of these two simultaneous readings was beyond the perceptual skills of any experimenter.[12] Ludwig's solution, which we will see recreated in the

[12]Consider, for example, William Harvey's quandary, experienced while studying the anatomy of the heart during the 1620s. He was unable to determine whether the systole of the aorta occurred before, after, or simultaneous with that of the ventricle, because the vivisected animal's heart beat so rapidly. His solution was to rationally reconstruct the phenomenon

Figure I
Ludwig's self-recording instrument (1847)



(Chadarevian 1993)

discovery of the electroencephalogram and of rapid eye movement, was to historicize the phenomena in question. That is to say, he converted a chronicle (or table) of singular events (manometer readings) into a narrative (an unbroken trace). By placing a rod-shaped float on top of the mercury column, and attaching it to a stylus that traced a line on a rotating drum, driven at a constant rate by a clockwork mechanism, Ludwig effectively flattened the presence of these two qualities, thus making possible their comparison in time (Frank 1988).[13]

The physiological investigator's labour now went into obtaining a proper record that could, when carefully edited and properly reproduced in scientific journals and monographs, allow the facts to speak for themselves. The rhetorical potential of this method was enormous, as it gathered experimenter and reader together around the same phenomenon. The entire field of physiology came to congeal around this one experimental practise: "what was presented as a technical solution to a particular problem was soon perceived as nothing less than 'a symbol' of the new disciplinary growth of physiology" (Chadarevian 1993: 269-270). The kymograph generated an entirely new mode of representation within the life sciences. The work (that is, the motion) of the heart, lungs, and muscles would soon be converted into a kind of universal sign—a trace—that presented "vital force" as a question, not of morphological space, but of eloquently synchronized time.

Nicholas Jardine has suggested that scientific development proceeds through a process of "calibration" (Jardine 1991). New instruments bring real questions into being by expressing data in a manner similar, but not identical, to the expression of older instruments. Ludwig's invention offers us an example of this technologically-driven construction of difference. Ludwig's invention depended upon its ability to frame what Jardine calls "real questions" ("what is the

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through an analysis based on anatomical structure (the valves of the heart) and comparative anatomy (observing the slower heartbeat of cold-blooded animals).

[13]On the importance of visual representation to the sciences in general, see Latour 1988. For a rather more meticulous study of the control of time in a neuroscience laboratory through methods of visualization, see Lynch 1985.

relationship between the work of the heart and that of the lungs?") by manipulating already existing representational forms. The graphical trace that once represented changes in air pressure was adopted to express physiological changes in the body. This new relationship did not remain on a merely epistemological level. Ludwig also pioneered the calibration of two disciplines—physiology and physics—whose time-centred practises would later flourish as the "biophysics" of the interwar period. And as we will see in the discovery and elaboration of the EEG, the production and development of such instruments also brought science and the industry of the assembly line closer together.[14] As Chadarevian points out, Ludwig's kymograph achieved its greatest epistemological impact once its industrial production was provided by the Leipzig instrument manufacturer, Baltzar.[15] But perhaps the most persuasive feature of the kymograph was precisely its ability to visually represent calibration within the body itself. Its greatest strength, it seems, was found in its extraordinary flexibility to render virtually any (inherently transitory) phenomenon of movement into a stable image, always calibrated to an already-accepted aspect of the phenomenology of time, be it a heartbeat or the oscillations of a tuning fork.

[14]Brain and Wise (1994) have argued that Helmholtz's enthusiasm for graphical methods was influenced by his participation in the Berlin Physical Society at least as much as it was by the performance of Ludwig's kymograph. Ludwig himself "ascribed both his and Helmholtz's use of graphic methods to the principles laid out by James Watt in his indicator diagram, a device invented to measure the work performed in the cylinder of a steam engine." The Physical Society was run by the German instrument-maker, Werner von Siemens and the electrophysiologist, Émil duBois-Reymond. These graphical methods have not only transformed medical knowledge, but have changed the doctor-patient relationship by replacing patient testimony with visual data. They are also used to resolve moral questions, such as determining the nature of death through electroencephalography (Reiser 1978; Frank 1988).

[15]Chadarevian 1993. On Baltzar, Chadarevian cites W. Gerabek, "Der Leipziger Physiologe Carl Ludwig und die medizinische Instrumentation," *Sudhoffs Archiv* **75** (1991):171-179.

The graphical method: Étienne-Jules Marey

This new tool for achieving a practical objectivity in visualizing vital phenomena is probably best exemplified by the career of the French physiologist, Étienne-Jules Marey (Braun 1992; Dagognet 1992). In 1857, Marey (1830-1904), who was studying at the Faculty of Medicine in Paris, completed his doctoral dissertation on the circulation of the blood. By studying the traces generated by arterial pulsation, and particularly the left and right radial arteries, Marey was able to diagnose various abnormalities in the heart's rhythm, effectively bringing Ludwig's kymograph out of the laboratory, and into the clinic (Toulouse 1904). From this point on, Marey was dedicated to transforming the observational platform of experimental physiology. He founded the first laboratory for experimental physiology in France in his private residence on the rue Cuvier. His "physiological station" soon received state sponsorship, and by 1878, he had developed a complete methodology based around his recording instruments. In place of the artificial pathologies created by vivisection, Marey constructed an epistemology of physiology, which he dubbed "la méthode graphique." In the words of one colleague (in this case, the editor of *Revue Scientifique*), "The graphical method, which in reality was and is only a means of study, became, through Marey's scientific research, an end in itself" (Toulouse 1904: 674). In 1868, his analyses began to breach the walls of his physiological station. This was the year that Marey succeeded Pierre Flourens in the chair for "the natural history of organized bodies" at the *Collège de France*. Their difference in investigative style was striking. Flourens (1794-1867), who had been Cuvier's protégé in Paris in the 1820s, based his entire career upon his skill in removing parts of the brain, and observing the functional failures that resulted. Marey's reputation lay almost entirely on his ability to construct devices that could detect and measure aspects of the organism without disturbing it.

Given the cultural impact of Marey's efforts to reconstruct the passage of time outside of the physiological laboratory, the most notable of which was certainly cinematography, it is perhaps worth investigating his long-standing rationale for the general deployment of such devices. In the first volume of his journal, which detailed the research conducted at his physiological station, which had not yet moved to the Parc-des-Princes in the Bois de Boulogne, Marey offered his readers a preface that summarized his vision of how the graphic method would transform the science of life. The article that followed took the problem of fatigue to illustrate how the graphic method allowed physiology to be applied to concrete problems of human existence.

Physiology, Marey complained, had been isolated from the physical sciences for too long (Marey 1875a). The study of organic motion had hardly changed since Albrecht von Haller had dubbed it *Anatome animata* over a hundred years earlier. It remained subservient to anatomical techniques and concepts. This was largely due to the lack of precision in physiological methods. The exact sciences had little place in a field dominated by Claude Bernard's cautious legislation between mechanism and vitalism, which championed vivisection as the only appropriate means of studying the science of life (Bernard 1961; Coleman 1985). Although Bernard (1813-1878) had insisted that all life was governed by the same physical and chemical laws that ruled over the inanimate world, Marey argued that his reliance on vivisection isolated physiology from the sciences that discovered those laws. Shortly after he became Flourens's assistant in 1867, Marey began depicting vivisection as a crude and destructive practise, incapable of overcoming the experimenter's limited observational capacities. It "can do no more," he argued, "than lay bare the phenomenon simultaneously with the organ which is the seat of it; it reveals to our senses only what they are capable of perceiving" (Marey 1868: 287; Braun 1992: 37-41). In its place, Marey wanted to ground physiology in a new observational practise—one that would "renounce" vivisection, replacing it with apparatus capable of examining living organisms in their undisturbed conditions.

It was only through the application of "the great law of the conservation of force," Marey argued, that the last vestiges of vitalism could be abolished from physiology (Marey 1875a: ii). But this victory would be won through the transformation of practise, not theory. Inscribing instruments would be the common ground of the physical and the life sciences. One of their greatest advantages for physiologists was the fact that they were recent inventions, and their dissemination had been rapid and thorough. "Inscribing instruments are found everywhere," cheered Marey, "in the observatories of astronomers and meteorologists, in physics laboratories, and in those of physiology" (Marey 1875a: ii-iii). They rendered all phenomena into a similar form. The institutionalization of this new medium became Marey's personal campaign. This transformation of the lifeworld of the physiologist would, Marey thought, finally enable the mechanical analogy, thus unifying the sciences in a shared phenomenology.

These ideas were by no means commonplace in French physiology, even in 1875. It was not merely Marey's physiological precepts that were situated outside of the mainstream of laboratory-clinic relations, exemplified by the work of Pasteur and Bernard. Marey's physiological station was physically set apart from clinical medicine. Although Marey's laboratory was located in the Sorbonne (until it was relocated to the Bois de Boulogne in 1881), his journal, *Physiologie Expérimentale*, was a not a product of the Faculty of Medicine. It was instead the result of a new creature in the French academic system that appeared in the early 1870s: the *École Pratique des Hautes Études*. In the wake of France's ignoble defeat by Germany, this new school was designed to be a distinctly French response to the perceived superiority of the German educational system. While its emphasis was on improving science education, its mission was to orchestrate all scholarly research in France, overseeing the work of the faculties of law, medicine, theology, letters, and sciences, as well as that of the *Collège de France*, the Museum of Natural History (where Bernard's laboratory had been relocated), the School of Pharmacy, the Paris Observatory, and various other institutions. Its architect, Victor Duruy (1811-1894), whose career as minister of public instruction had begun under the shadow of the "Renan affair," considered this new system to be his greatest achievement.[16] It addressed

[16]In 1863, Ernest Renan had been suspended from his teaching post at the *Collège de France* for denying the divinity of Christ in one of his lectures. This put Duruy in a difficult position, as he supported academic freedom, but opposed any public instruction that might disturb the religious or political peace (the *cours libres* were supposed to be a solution to this issue, as they operated relatively independently from their sponsoring institutions). He attempted to resolve the issue by relocating Renan to his old post at the *Bibliothèque Impérial*. Renan, however, continued to protest, and the liberal press took up his cause against Duruy. His

the complaints, frequently made by high-profile researchers such as Louis Pasteur and the chemist Jean-Baptiste Dumas, regarding the chronic poverty of French science, while it freed research from the traditions of the entrenched strictures of French higher education. The school was open to anyone, regardless of nationality or former education, and it did not place particular emphasis on terminal degrees or examinations, although students could petition for either a *diplôme de l'École* or a *doctorat-ès-sciences* if they wished. The new faculty was generally quite young, and many had been trained in Germany. Thus, there was good reason for Duruy to describe his new institution as "a germ which I am depositing in the cracked walls of the old Sorbonne," which, he hoped, "will crumble it" (Horvath-Peterson 1984:195).

So it was from the vantage point of a freshly-minted research institution that Marey began to publish his studies on animal locomotion and the applications of the graphical method, caring somewhat less for medical therapeutics than for technological innovation in the pursuit of nature. After his 1863 monograph on the circulation of the blood, Marey began a series of studies of animal movement, which included the flight of birds, before publishing his definitive methodological statement in 1878. Duruy's enthusiasm for Marey's work reflected the latter's obvious skills as an engineer, rather than his abilities as a medical practitioner. Duruy first met Marey in the mid-1860s, while touring the physics laboratory at the Sorbonne. Like his successor, Jules Ferry, Duruy was spell-bound by Marey's arsenal of recording devices, artificial organs, and mechanical birds, and immediately began to sponsor his research (Braun 1992: 38-39).

This is not to imply, however, that Marey was uninterested in medical practise. Quite the contrary, the very title of one of his books—*La méthode graphique dans les sciences expérimentales et principalement en physiologie et en médicine*—suggests otherwise (Marey 1878). But his focus was always on technological development. When cholera broke out in

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response was to cite a decree from 1852, which gave the minister final say over all appointments and revocations at the *Collège*, thus seriously comprimising his reputation as a liberal reformer (Horvath-Peterson 1984:180-181).

France during the summer of 1884, he was on a committee from the *Académie de Médecine* to investigate the matter (Braun 1992: 93-99). Like Pasteur, who was also on the committee, he subscribed to the germ theory of disease, and took a keen interest in the role of the water supply in the spread of cholera. But to answer this question, he adopted the approach of the contagionists, sending questionnaires out to doctors, asking them about the effects of weather changes, the disposal of faecal matter in the district, and local hygienic conditions. He also studied the flow of the water supply.[17] In short, he treated the cholera epidemic as a problem of civil and social engineering that could be solved by the visual analysis of motion, rather than the study of bacteriology.

Such an approach was well in keeping with Marey's passion for tracing out the economy of motion in organic life. His journal, which was abandoned after issuing its fourth volume in 1879, was primarily concerned with the extension of the graphic method into all realms of organic movement, including those of the heart, the brain, and the iris, as well measuring muscular force, and investigating the mechanics of flight in birds. Significantly enough, the very first article to appear concerned the question of work and its economization; that is to say, it was about fatigue.

Marey's efforts in the domain of work physiology extended throughout the greater part of his career. The introduction to his paper, which he had presented to the *Académie des sciences* in 1874, reiterated his rejection of vitalism, and his fidelity to the fruits of German reductionism (Marey 1875b). He replaced all talk of the unique properties of life with the language of work and efficiency:

> To demonstrate that the elasticity of the organs exists not only for the regularisation of the movements of which it is the seat, but that it increases the useful work that can be

[17]Marey began the project of mapping out the flow of water sources in Paris, but soon reverted to the study of his birthplace, Beaune, apparently because as he was more familiar with its topography. The other aspect of his investigation, the questionnaire, did not receive enthusiastic support from the *Académie* (Braun 1992: 93-99).

> accomplished—this has been a longstanding preoccupation of mine; and I believe that I have demonstrated that in the circulation of the blood, just as in the actions of the voluntary muscles, elasticity plays an indispensable role (Marey 1875b: 1).

The perfection of the organs was tied to the body's ability to render change rhythmical and regulated. "In the circulation," he observed, "the elasticity of the aorta and of the arteries does not merely transform the saccadic and intermittent movement of the heart into a continuous flow. To this well-known influence, one must add another, which is more important yet, but which has escaped the attention of physiologists to this point: *the elasticity of the arteries economizes the work of the heart*" (Marey 1875b: 2. Italics original). Starting from the principles of classical mechanics, Marey noted that all problems of dynamics (of which the circulation was one) could be approached from two perspectives: that of the "motor work" performed, and that of the "work of resistance," which are equal. But this latter could be further analysed into "useful" (*utile*) and "useless" (*inutile* or *nuisible*) work. The elasticity of the arteries minimized the useless work done, thus improving the efficiency of the heart. When this elasticity was compromised, as in old age, diseases such as hypertrophy of the heart would appear.

The motif of such efficiency was the phenomenon of continuous, smooth motion. Elasticity allowed the rhythmic pumping of the heart to be communicated to the periphery, creating the relative stability of blood pressure. Marey used this image, grounded in the work of the organs, to depict the social work of human labour.[18] To accomplish this transition, he shifted his analysis to the physiology of muscle, and the study of locomotion. It was the elasticity of muscle that allowed the successive shocks (*secousses*) to be transformed into the smooth motions of our limbs. From this analogy, Marey argued that "it is not much further to conclude that from the point of view of the utilization of work, muscular elasticity presents the same utility as that of the [blood] vessels" (Marey 1875b: 3). Marey took the same approach to the problem of work as he had with the circulation: he set about building a mechanism that could generate some sort of graphical trace.

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[18]For a survey of the "reflective" relationship between natural and social order, see Barnes and Shapin 1979.

There was an important difference between these two problems, however. In his earlier work on circulation, Marey had built an artificial heart that generated the same traces as the organ itself, in the hope that he could understand the heart's natural efficiency. But when it came to muscular work, the examples that surrounded Marey—human and animal labour—were examples of *in*efficiency that needed improvement. So Marey constructed devices that could not only measure the work done, but could improve the efficiency of the performance as well [Figure II]. Through such devices, he was able to represent the performance of the machine and that of the body through the medium of the graphical trace. Marey had forged a mechanical identity out of a seventeenth-century mechanical analogy.[19]

It was elasticity that was the crucial aspect of performance, be it human or mechanical, for Marey: "To place elasticity between our muscular efforts and the masses they must move is to imitate the processes of nature for the better utilization of the intermittent action of our muscles" (Marey 1875b: 7). Elasticity had beneficial psychological effects as well. An elastic harness helped "calm" a workhorse by reducing the intensity of pulling a heavy load over a rough road.[20] It would also be a more efficient use of its "motor work." Thus an offshoot of

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[19]This shift in interest from the mechanism to the motor, which Rabinbach has illustrated so well, was readily apparent to Marey: "No doubt, the physiologists of old discerned levers, pulleys, cordage, pumps, and valves in the animal organism, as in the machine...But these passive organs have need of a motor; it is life, it was said, which set all these mechanisms going, and it was believed that thus there was authoritatively established an inviolable barrier between inanimate and animate machines. In our time it is at least necessary to seek another basis for such distinctions, because modern engineers have created machines which are much more legitimately to be compared to animated motors..." (Marey 1884: 1).

[20]This claim was not Marey's own, but that of a Berlin engineer—Fehrmann—whose *Pferdeschoner* ("horse-manager," from *shonen* "to indulge") had been tested in 1874 at Halle. Marey thought Fehrmann's paper to be so important that he translated it and republished it along with his own (Marey 1875b). Fehrmann's *Pferdeschoner* and Marey's labour-saving device were practically identical, but Marey was careful to point out that Fehrmann's work was not as precise as his own, as the former did not include any description of how speed or effective work was measured. It is worth noting that Fehrmann's results were presented in tabular, rather than graphical, form

Figure II
Marey's dynamograph (Marey 1875b)

Fig. 2. — Dynamographe ou dynamomètre inscripteur transmettant à distance les indications des efforts de traction.

Marey's application of the dynamometer was the reduction of the subjective experience of hauling a load to a quantitative problem capable of graphic illustration. Rather than offering tables of mathematical analysis (there were none), he simply reproduced the traces taken in two experiments—one generated by a dynamometer attached to a harness *with* an elastic connection, and one without [Figure III]. A comparison of the areas between the abscissa and the trace in both cases revealed that former was twenty-six percent smaller than that of the latter. Thus that same percentage of work had been "economized." Marey concluded that "The economy of work and the diminution of fatigue that one obtained through this method of traction constitutes an important application of physiology to the amelioration of the fate of animals and of man" (Marey 1875b: 13).

This conclusion was in keeping with Marey's thoughts about the improvement of the human race in general. In *Animal Mechanism* (1873), Marey indicated that the fate of humankind was actually a question about the relationship between the musculature and the will. Like many physiologists in France during the last quarter of the nineteenth century, Marey carved up "development theories" into two major divisions: the "old school," whose members believed that species were inalterable forms that persisted, relatively unchanged, through time; and the proponents of the "new school," who insisted that species are modified by degrees (Marey 1884: 78-101). He admitted that the modification of species seemed incapable of experimental verification, but he nevertheless insisted that Lamarck's original insight of the inheritance of acquired characteristics was probably correct, as was Darwin's more recent "addition" of the phenomenon of natural selection (the difference between these two was, Marey felt, more rhetorical than substantial). He then presented his own position on development by describing the relative variability of the skeletal and the muscular systems. The structure of the skeletal system, was, it turned out, entirely dependent upon the muscles. "[I]n the form of the bony structure," Marey argued, "everything bears the trace of some external influence, and particularly the function of the muscles. There is not a single depression or projection in the skeleton, the cause of which cannot be found in an external force, which has acted on the bony matter, either to indent it, or draw it forward" (Marey 1884: 93). In Marey's internal economy of the body,

Figure III

Dynamograph tracings with (above) & without (below) an elastic medium
(Marey 1875b)

Fig. 3. — Tracé du dynamographe pour une voiture tirée avec un intermédiaire élastique (Surface au planimètre de Amsler, 53).

Fig. 4. — Tracés du dynamographe pour une voiture à bras traînée avec un trait rigide (Surface au planimètre de Amsler, 72).

tendons hollowed out bones, the radius of the curvature of the tarsus increased as the mobility of the bone decreased, and the shape of a bird's wing depended upon how its muscles propelled it in flight. Muscular work was the key to the evolutionary process, and the muscles themselves were dependent on the will:

> It is understood that the skeleton, as it is modified, plays a passive part; that it is subject to the form imposed upon it by the muscle. But what gives to the muscle itself, an organ eminently active, and the true generator of the mechanical force by which the skeleton is in some degree modified, the particular form which is revealed to us by anatomy? We hope to demonstrate that the power to which the muscular system is subjected belongs to the nervous system. The nature of the acts which the will commands the muscles to perform, modifies the muscles themselves, in their volume and their form, so as to render them capable of performing these acts in the best possible manner. And, as this *necessity* which determines all the actions of animal life, governs the will, it is this, which, according to the external conditions under which every living being is placed, influences its form, and regulates it according to the laws which we must know endeavour to make known (Marey 1884:94-95. Italics original).

The campaign of human improvement that followed such a claim was thus integrated with the question of making human labour more efficient. Taking a cue from Darwin, Marey argued that the demonstration of the viability of such a project would come out of work done on domesticated animals. "It would be necessary," Marey contended, "to do violence to the habits of animals, and to constrain them gradually to perform acts to which their organism is but slightly adapted" (Marey 1884: 100). Human beings, on the other hand, were capable of transforming such habits in themselves, and were thus placed at the forefront of evolutionary progress. The remaining two sections of the book, which dealt with terrestrial and aerial locomotion, attempted to uncover the laws of efficiency that ran through the natural world of movement, in order that the cultural world of human labour could likewise be rendered more efficient, ultimately leading to the improvement of the species itself.

The psychophysiology of fatigue: Angelo Mosso

The application of Marey's time-and-motion studies to practical problems in the workplace, however, were still largely in the future at his death in 1904.[21] It was the investigations of his student, Angelo Mosso (1846-1910), that brought work physiology into the field of human productivity in the twentieth century. He also brought a deeper psychological dimension to the study of labour. Unlike Marey's instruments, which were aimed at uncovering and recording hidden physiological processes, Mosso's greatest invention, the ergograph, attempted to measure fatigue as a sensation. The device helped to unite physiologists and psychologists in the common goal of improving the performance of the human motor.

Mosso's ergograph [Figure IV] was spread across Europe through the pages of his most popular work, *Fatigue*, which went through numerous German, French and English editions in the years following initial 1891 publication in Italian. Mosso claimed that his book—an eclectic amalgam of anecdote, image and experiment—was the product of ten years' worth of investigations. But in fact, his interest in fatigue can be traced back to the early 1870s. Just as Duruy was centralizing the direction of scientific research in the Third Republic, and in the same year (1874) that Marey was presenting his dynamometer to an audiences across France, Mosso appeared at the door of Marey's laboratory on the *rue de l'Ancienne Comédie*. Mosso, who had recently completed a dissertation at the University of Turin on the growth of bones, had gone on

[21]Shortly before he died, Marey published his final paper on elasticity as the natural model of work efficiency: "L'économie de travail et l'élasticité," *La Revue des Idées* 1 (1904): 161-176. The topic was then passed to Marey's longstanding collaborator, Auguste Chauveau (1827-1917). Chauveau was the "chef de service" of Physiology and Anatomy at the Veterinary Institute of Lyons, and took over the directorship of the *Institut Marey* after Marey's death. His later work used Marey's devices—in particular the spirometer—to measure respiratory exchanges, arguing on this evidence that carbohydrates, rather than fats or proteins, provided the most efficient use of oxygen combustion in the human subject. By concentrating on the metabolic changes involved in work, Chauveau identified a work régime that came closer to isolating the internal sensation of fatigue as the limiting factor in efficient performance (Braun 1992: 18-20, 324-326 & 390; Rabinbach 1992: 127-128).

Figure IV

Mosso's ergograph (Mosso 1906)

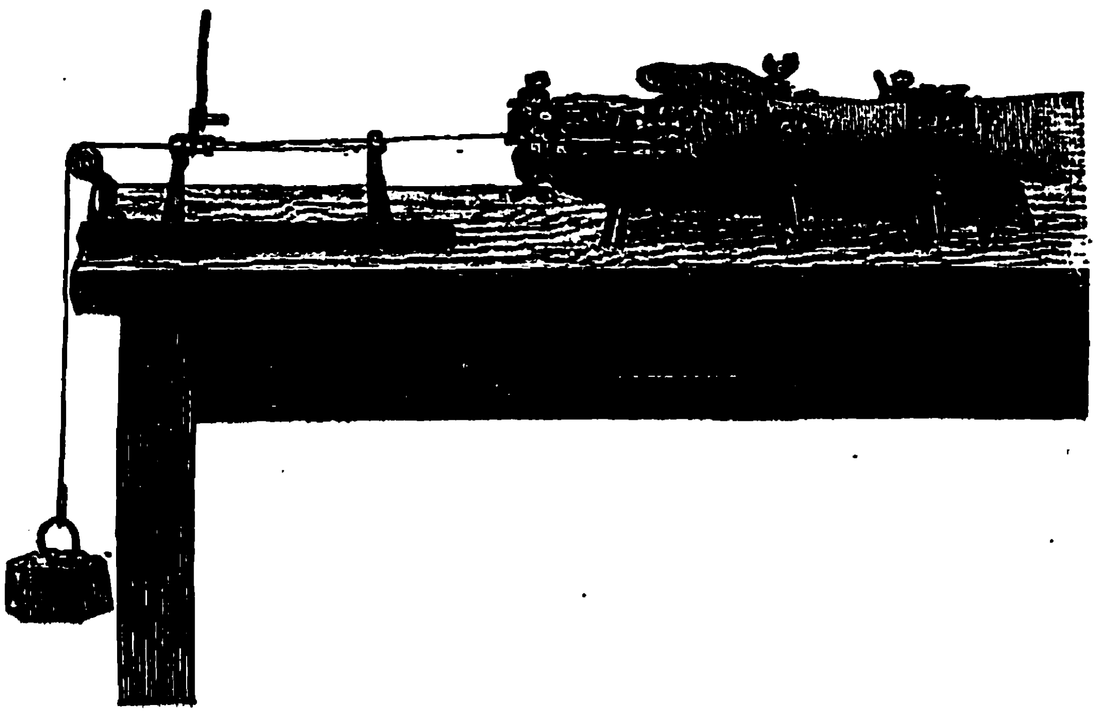

FIG. 6.—Arrangement of the ergograph to take a fatigue tracing.

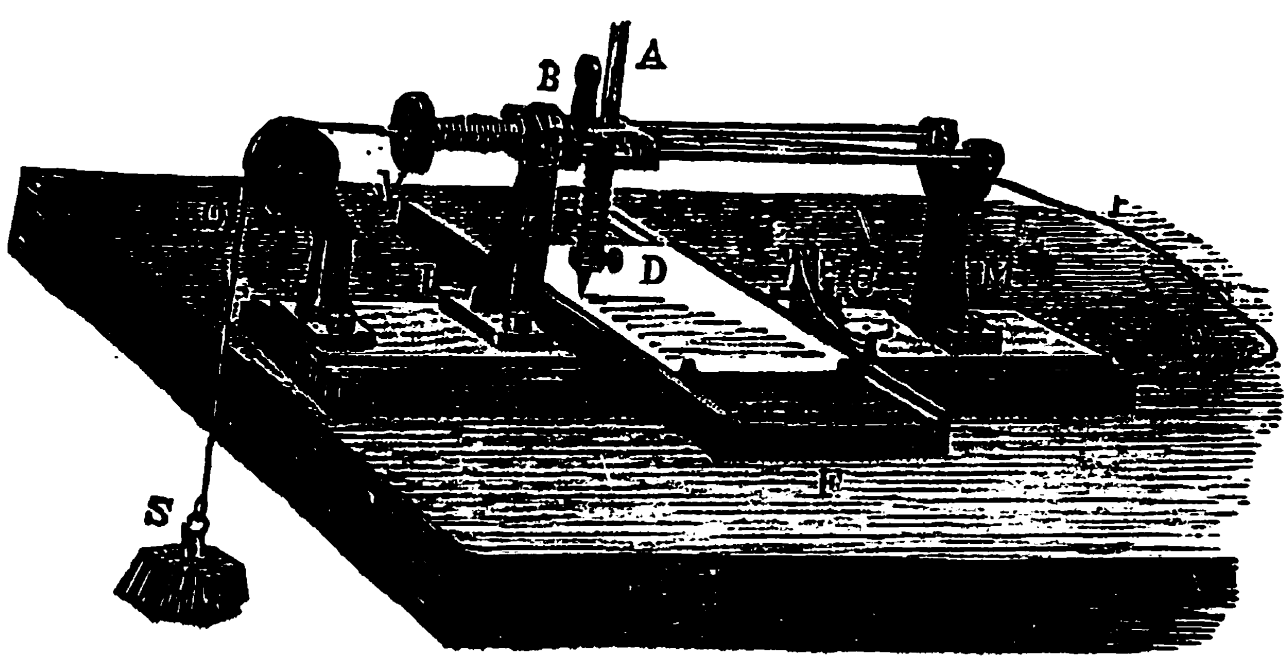

FIG. 5.—The registering runner of the ergograph.

to work under Moritz Schiff in Florence for two years. He then travelled to Leipzig, where he studied with Carl Ludwig (1873-4). It was not, as Mosso later recollected, the particular subject matter under investigation in Ludwig' laboratory that appealed to his young mind. It was the experience of witnessing the well-orchestrated, industrial precision of physiological experiment. Arriving in Leipzig in 1873, Mosso recalled that he was

> in time to be present at the latest experiments made by Professor Kronecker in completing his researches upon the fatigue and the restoration of the striped muscles of the frog. It is a duty—and more than a duty, a pleasure—for me to avow that it was these experiments which first fired me with the desire of applying myself to the study of fatigue. The exactitude of the method, the elegance of the apparatus, the precision of the results, could not but charm a novice...[22]

Kronecker, who had also worked with Marey, was determined to understand the law-like properties of fatigue.[23] To this end, he removed the leg muscles from frogs, electrically stimulated them at regular intervals, and recorded their subsequent contractions on a kymograph. The resulting image, which made such a deep impression on Mosso, must have looked something like the curves obtained earlier by Marey, which Mosso faithfully reproduced in his book [Figure V]. Marey's figure illustrated the phenomenon of latent excitation (the increasing length of time the muscle remained in contraction as fatigue increased). Kronecker, however, wanted to demonstrate how the height of the contraction decreased over time. After exposing the same muscle to up to 1500 contractions, he concluded that the fatigue curve was always a straight line. That is to say, fatigue was a perfectly regular phenomenon, directly proportional to the time interval between equally strong induction shocks: the longer the interval, the slower the onset of fatigue.

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[22]Mosso 1906: 81. Mosso claimed that Wilhelm Wundt was the first to use the myograph to study fatigue in muscle, around 1858, although Rabinbach notes that he was unable to trace the origin of this remark (Mosso 1906: 77; Rabinbach 1992: 330). Wundt would have had considerable experience with this new instrument. In 1858, he was a *Dozent* in Physiology at Heidelberg, where he was obliged to demonstrate muscle twitch experiments for students as part of a revised medical curriculum that emphasized the importance of scientific experiments.

[23]On this point, Mosso cites Hugo Kronecker, "Über die Ermüdung und Erholung der quergestreiften Muskeln," *Berichte der Verhandlungen der säsischen Gesellschaft der Wissen schaft zu Leipzig* (Leipzig, 1871), p. 718.

Figure V

Kronecker's fagtigue curves

(Mosso 1906)

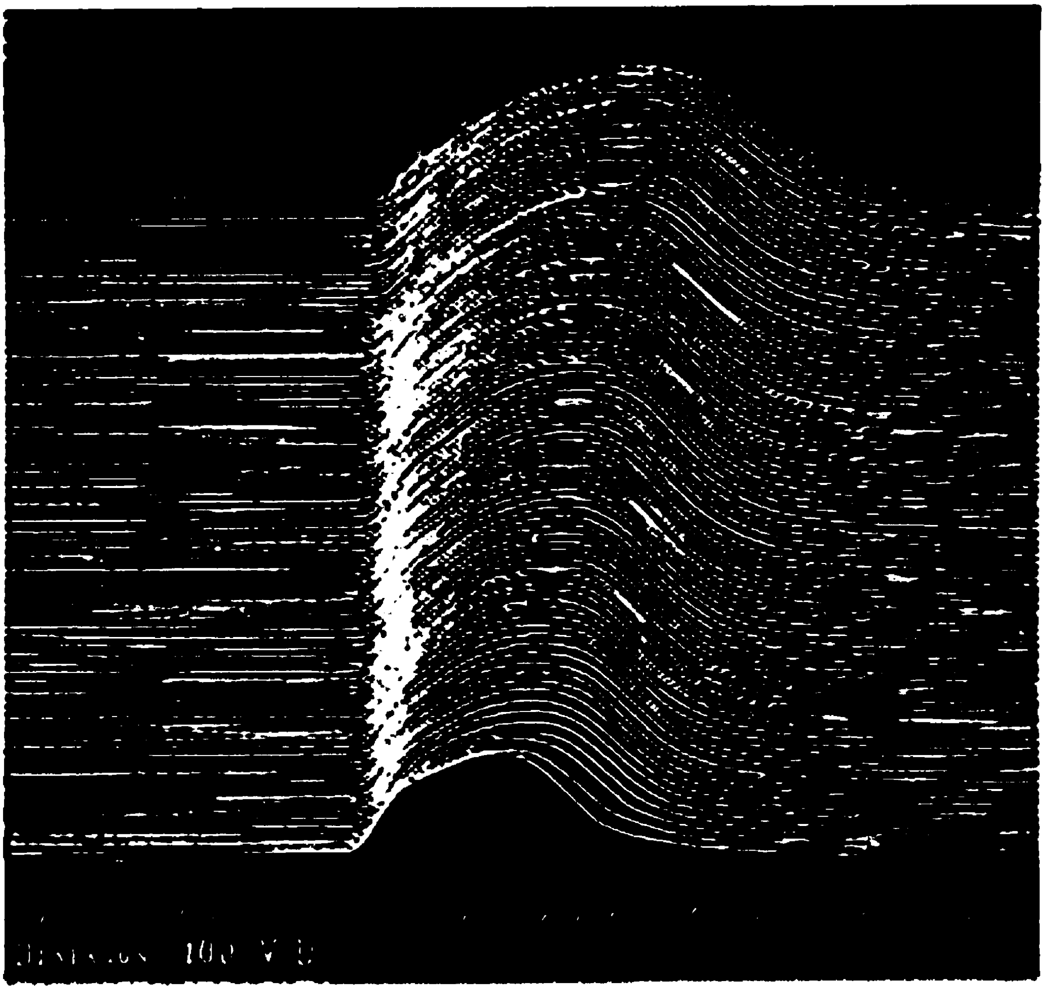

FIG. 1.—Tracing of muscular shocks written by the leg of a frog. The first contractions are in the lower, the last in the upper part of the figure: and in the latter is seen the effect of fatigue. A chronograph which makes 100 vibrations per second writes the undulating line at the foot of the tracing. Each undulation corresponds to $\frac{1}{100}$ of a second, and thus the absolute duration of the various phases of a shock can be measured. (Marey.)

It was Kronecker who recommended to Mosso that he travel to Paris to meet Marey. Although his stay in Paris was short, Mosso seems to have absorbed one crucial element of Marey's experimental rhetoric: the need for physiologists to study things in their places, rather than submitting the vivisected remains of lower organisms to the artifice of induced shock. In *Fatigue*, Mosso was highly critical of Kronecker's research, arguing that "with frogs it is impossible to reproduce the normal function of muscles and to imitate the action of a man who is doing mechanical work" (Mosso 1906: 83). While Mosso's ergograph aimed at producing similar visual phenomena to Kronecker's tracings, its experimental context was quite different. His ergograph could only be operated by human subjects who were both capable and willing to follow instructions.

Such an approach was in keeping with the general tone of *Fatigue*. The book began with a chapter on the migration of birds to illustrate the interplay of emotion, education, and muscular performance. Mosso offered a number of experiments that demonstrated that only adult pigeons possessed the instinct of direction, and that the migratory instinct was the product of education. Emotion called the instinct into play: "When they [carrier pigeons] are taken a long distance, the fatigue and toil which they endure in order to find their home once more is incredible. One might think they had become blind and had ceased to recognise danger; they care no more for their lives, they are infatuated with love" (Mosso 1906: 7-13). For Mosso, music, movement, and emotion were united in Marey's graphic studies of insect flight:

> Bees, which have been more minutely studied, furnish us with a very conclusive demonstration that emotion affects their flight, just as it does the gait of man. When startled or excited they emit a shriller sound. When in tranquil flight they seek honey from the flowers, they emit a *la*, and when in the evening they arrive wearied at their hive, the hum is on a lower note, namely, *sol*, just as we ourselves slacken our pace after a long walk (Mosso 1906: 17).

Mosso's attempt to encompass emotion within the purview of the graphical method through fatigue came through in his choice of experimental subjects. In place of Kronecker's twitching frog muscles, the reader found ergograph tracings made by Mosso's fellow professors at the

University of Turin, whose records were often identified by name.[24] This emphasis on the individual subject spilled over to Mosso's criticism of Kronecker's law. Fatigue, which Mosso understood to be as much a property of the brain as of the musculature, was subject to individual variation. Each subject had their own different, but rigorously consistent, style of fatigue that would produce a unique tracing [Figure VI]. Some would slowly lose their ability to lift the three-kilogram weight, thus producing a long series of declining curves, while fatigue would descend suddenly upon others, causing the trace to immediately drop off. "The ergograph," Mosso mused, "thus gives us a record of one of the most intimate and most characteristic features of our individuality—the manner in which we fatigue, and this feature remains constant" (Mosso 1906: 92). Where Kronecker's frog muscles were animal and anonymous, Mosso's human subjects were unique and historical.

The basis of this individuality was located in the body. Mosso demonstrated this by a remarkable experiment. He isolated a single motor unit (a group of muscles controlled by a single nerve) in the middle finger, and then conducted two experiments with his ergograph. For the first, his subject simply attempted to raise the weight as high as was possible, to the beat of a metronome. The second tracing was taken from the same subject, but with an important difference: "To eliminate the mental element which might alter the fatigue curve of the muscle, I thought of stimulating the nerve of the arm, or rather the flexor muscles of the fingers...In this way one can make the muscles perform work without the aid of the will...in this case fatigue of both brain and nerves was excluded, the muscles having been stimulated directly by the electric current" (Mosso 1906: 98-99). Mosso hoped his device would enable him to compare fatigue in the muscular and nervous systems, which would eventually lead to an analysis of thought, creativity and genius in the measurable terms of work. The distance Mosso had moved from Marey was evident in his emphasis on the role that the nerves played in fatigue. Fatigue in the

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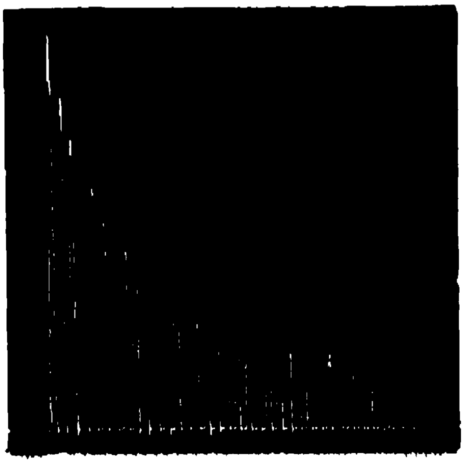
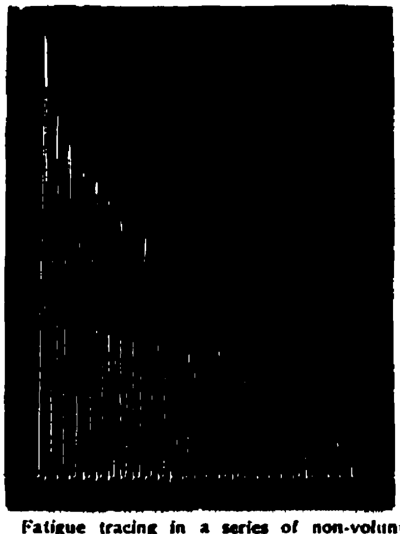
[24]Kurt Danziger (1990) has fastidiously documented the decline of this practise of identifying subjects in the history experimental psychology, tying it to the appearance and dominion of an "aggregate subject," first in the laboratory, then in the applied psychology of industry, education and the military.

Figure VI

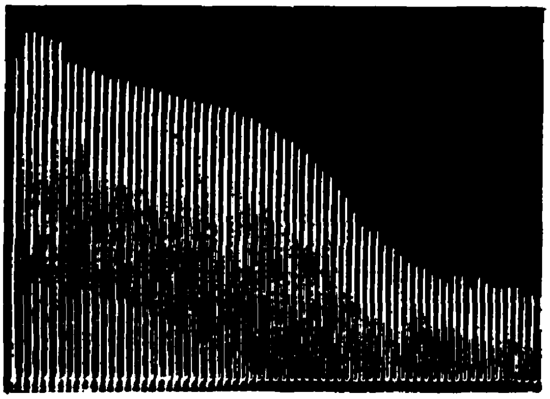
Fatigue curves taken from Mosso's ergograph (Mosso 1906)



Fatigue tracing written by Doctor Maggiora in 1884



Fatigue tracing in a series of non-voluntary contractions. The flexor muscles of Dr. Maggiora's fingers were stimulated directly by an electric current, and raised a weight of one kilogramme.



Fatigue tracing in a series of non-voluntary contractions, obtained by stimulating the median nerve in Dr. Maggiora's arm. The flexor muscles of the middle finger raised a weight of three kilogrammes.

musculature was only an expression of the exhaustion of the nervous system, something that Marey, who always analysed work in terms of muscle power, had missed. "The nervous system," Mosso concluded, "is the sole source of energy...there exists only one kind of fatigue, namely, nervous fatigue; this is the preponderating phenomenon, and muscular fatigue also is at bottom an exhaustion of the nervous system" (Mosso 1906: 241-242).

The ergograph was probably the most popular representation of Mosso's passion for transforming psychological experience into the graphic images of physiology (Rabinbach 1992). But even before the publication of *Fatigue*, Mosso was well known by physiological psychologists for another instrument—the plethysmograph [see Figure VII]. Mosso had been working on the device, which measured the change in volume in body parts, ever since he left Marey's laboratory in 1874. After presenting a few papers on the subject, he eventually produced a book-length study that featured the instrument (Mosso 1879). The book was dedicated to Kronecker and Marey, and it dealt primarily with the respective roles of the cardiac and arterial pulse in the blood's circulation. What little psycho-physiology there was in it amounted to a brief foray into the physiology of *sleep*, not fatigue. Happening upon two young subjects with small openings in their skulls, Mosso created a simple device, built out of a Marey tambour, to record changes in cerebral blood pressure during sleep [Figure VIII]. At the same time, he used his plethysmograph to record blood pressure in the periphery. These few pages in *Die Diagnostik des Pulses* offered the first graphical evidence of a decrease in cerebral pressure. But unlike Hammond, who argued that decreased circulation *caused* sleep, Mosso framed his observations in terms of the overall physiological economy (Mosso 1879: 12-14). In his comparison of the two curves, Mosso noted that brain volume decreased in sleep while that of the arm increased, noting that a mild stimulus produced a slight rise in brain volume and a corresponding decline in arm volume. Even though his subjects remained asleep, their brains reacted to the outside world.

Mosso extended this application of graphical methods to psychological phenomena in his study of fear, first published in 1884 (Mosso 1896). He used Marey's sphygmograph to demonstrate that arterial tension increased during intellectual activity, as did blood supply. In

Figure VII
Mosso's plethysmograph
(Mosso 1879)

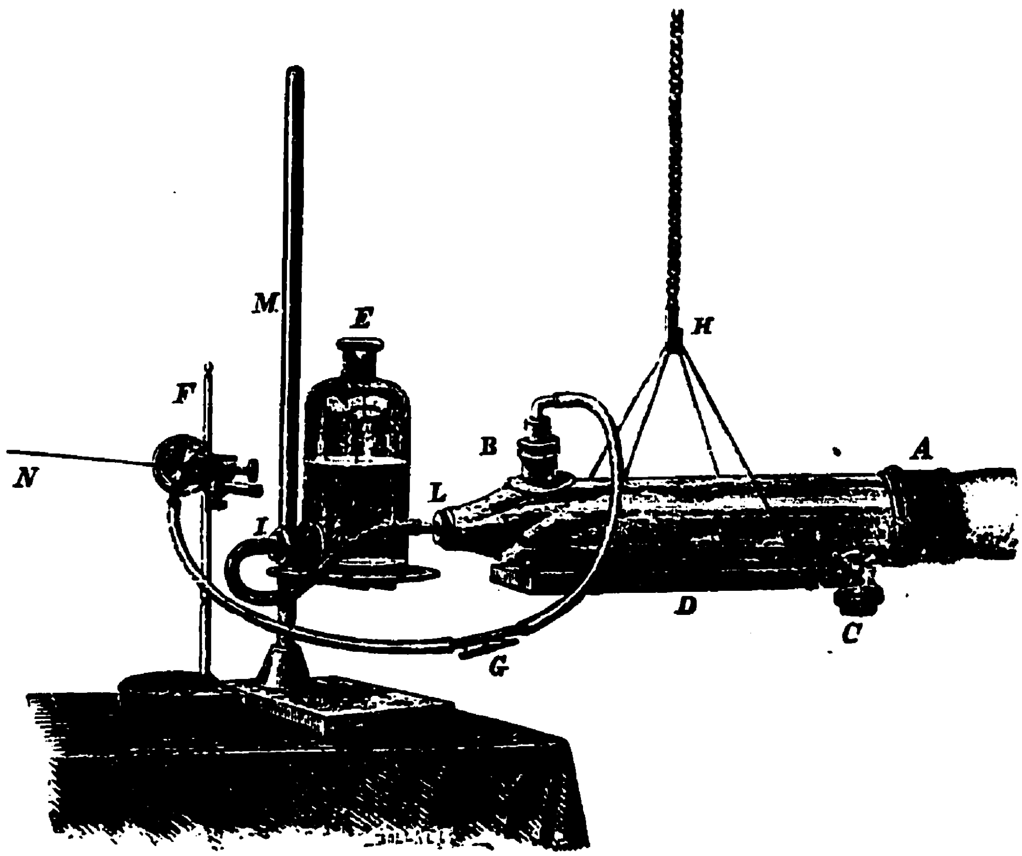
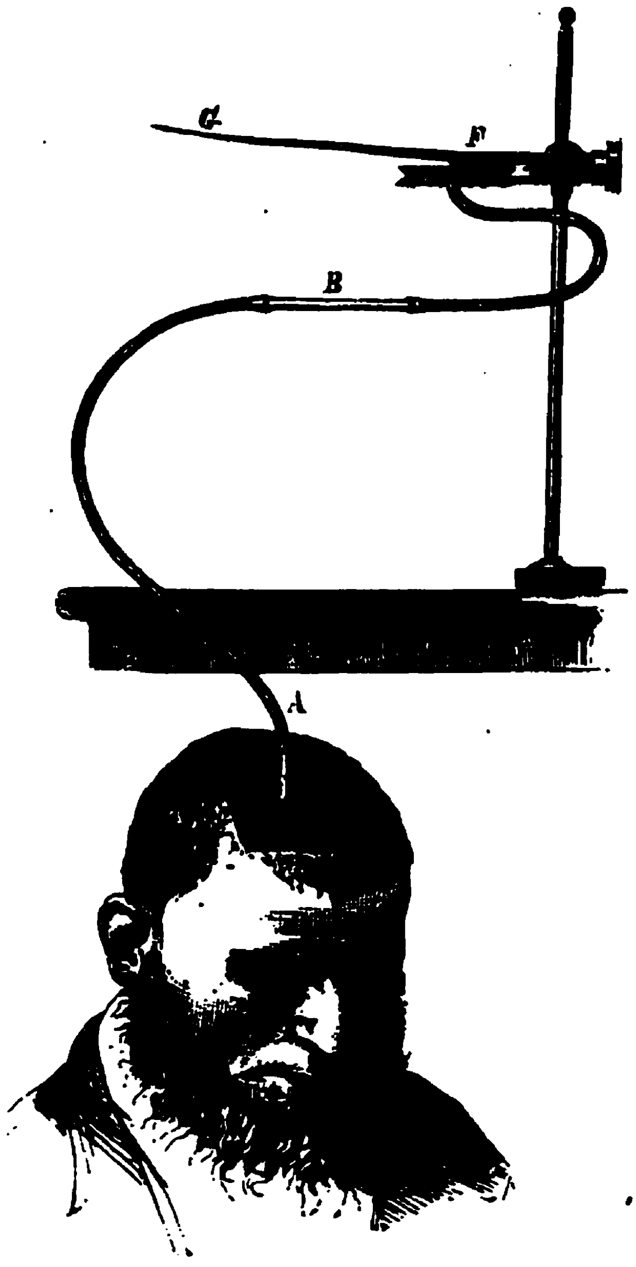


Figure VIII
Mosso's device for recording the pulse in the brain
(Mosso 1906)



contrast, fear was accompanied by a decrease in blood pressure.[25] The exquisite balance of the body's blood supply, which we have already encountered in Mosso's plethysmographic experiments on sleep, took on a new laboratory aesthetic in *Fear*. Mosso had his subjects lie on a carefully balanced table that would tip at the head or foot end, depending upon where blood began to collect [Figure IX]. He found that with the onset of an emotion or any intellectual activity, the head of the table would begin to descend.[26] And as was the case in his other studies, sleep haunted the margins of his research in *Fear*. In a few pages on night terrors (*pavor nocturnis*) in children, Mosso described his study of the changes in blood pressure that accompany their frightened awakenings.

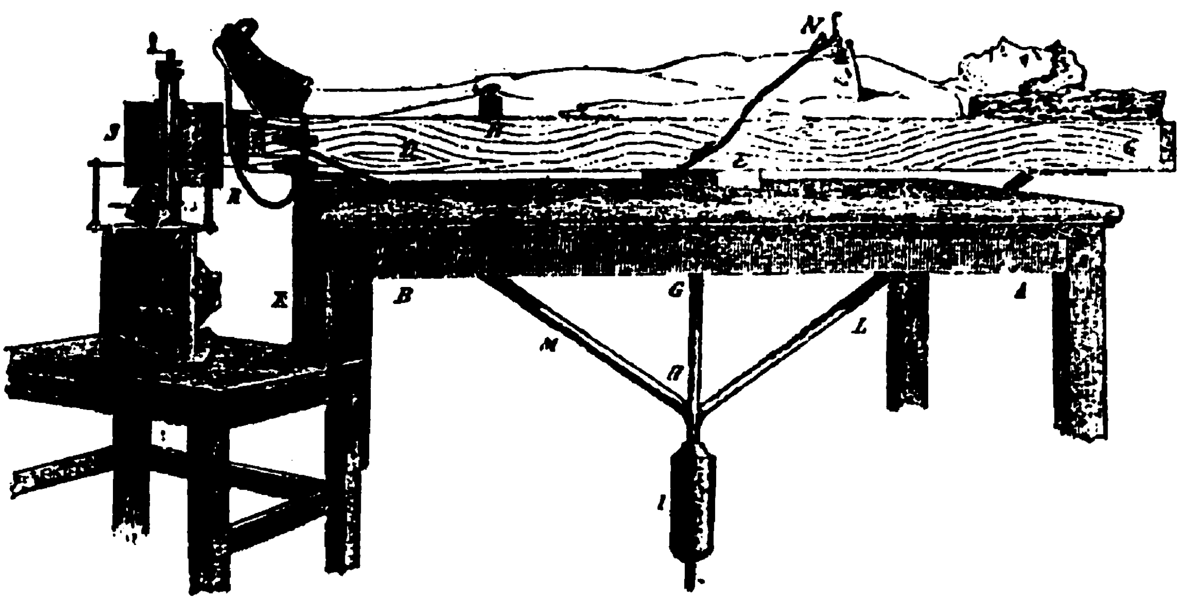
The function of emotion: William James

The invention, production and dissemination of self-registering instruments such as the dynamometer, the ergograph and the plethysmograph were a boon for psycho-physiologists at the end of the nineteenth century, many of whom were beginning to take an interest in applying their psychological expertise to fields such as education and labour. This practical approach to psychological science has been characterized by historians as quintessentially Anglo-American, in contrast to the European tradition of conceiving psychology as a way of doing philosophy (Smith 1997: 492-529). In the 1880s, however, the social utility of psychological knowledge was largely restricted to moral questions about the nature and limits of human freedom. To what extent did the body govern the mind? How did emotional states affect thought? Psychologists began to turn to evolutionary theory to respond to such questions, and one of the most influential representatives of such a turn was the American psychologist and philosopher, William James

[25]One of his examples was, once again, taken from academic life. In this instance, he detected a substantial drop in the blood pressure of his subject (a student) when Carl Ludwig walked into the room.

[26]These experiments are cited in William James, *Principles of Psychology* (Henry Holt and Company: New York, 1890), pp. 97-99.

Figure IX
Mosso's balance bed, for studying circulation in the body.
Note the plethysmograph on the foot, calibrated to a device measuring respiration
(Mosso 1896)



(1842-1910). James, like Herbert Spencer before him, made liberal use of evolutionary concepts in his claim that in order to be understood, consciousness must be interpreted in terms of its function. James was also one of the first to integrate Mosso's into his own arguments.

James's essay, "What is an Emotion?" appeared in 1884, the same year as *Fear*. James argued that the emotions were primarily affairs of the body, not of the mind. They did not come as a direct result of perceiving something that held some emotional significance in one's thoughts. Emotions arose because the body reacted unconsciously to some object, and then the mind interpreted this reaction as the emotion (this theory was proposed by a Danish physiologist, Carl Georg Lange, around the same time, and was henceforth known as the James-Lange theory of emotion). Emotions were a kind of internal sensation:

> Our natural way of thinking about these standard emotions is that the mental perception of some fact excites the mental affection called the emotion, and that this latter state of mind gives rise to the bodily expression. My thesis on the contrary is that *the bodily changes follow directly the* PERCEPTION *of the exciting fact, and that our feeling of the same changes as they occur* IS *the emotion*. Common sense says, we lose our fortune, are sorry and weep; we meet a bear, are frightened and run; we are insulted by a rival, are angry and strike. The hypothesis here to be defended says that this order of sequence is incorrect, that the one mental state is not immediately induced by the other, that the bodily manifestations must first be interposed between...we feel sorry because we cry, angry because we strike, afraid because we tremble...(James 1884: 189-190. Italics original).

This reversal of the fortunes of emotion fit in with James's interest in function. Emotions appeared in consciousness as a sensation of the body's reflexive performance in any situation that was significant for survival. This reflex was, like all reflexes, a movement. And this movement must be detectable. The physiologist's task was to visibly enumerate the subtle movements of the body that the mind understood internally as emotion. The rude, largely morphological work had already been done (James cited Charles Bell's *Anatomy of Expression* as an example). It was easy to see fear, surprise, and love in the countenance of another. But what remained was the study of the imperceptible changes that sensation provoked in our bodies, as James anticipated that this would furnish the proof that when we felt an emotion, what we were feeling was our body reacting. "The researches of Mosso with the plethysmograph," James opined, "have shown that not only the heart, but the entire circulatory system, forms a sort of sounding-board, which every change of our consciousness, however slight, may make reverberate. Hardly a sensation comes to us without sending waves of alternate constriction and dilation down the arteries of our arms" (James 1884: 191-192). For James, the experience of emotion testified to the functional nature of consciousness. Mind was not an epiphenomenon, an incidental string of states that accompanied brain processes. Nor could it be explained by associationist psychology, which stipulated that every perception evoked a train of associated ideas. The body intervened. And Mosso's work, thought James, could reveal that intervention, and demonstrate that the work of consciousness had real and important consequences for human fitness and survival.

Mosso was clearly thinking in terms similar to those expressed by James. His understanding of fatigue as a sensation could not be separated from his firm belief in analysing fatigue in teleological terms. Fatigue had an evolutionary purpose: "...what at first sight might appear an imperfection of our body, is on the contrary one of its most marvellous perfections. The fatigue increasing more rapidly than the amount of work done saves us from the injury which lesser sensibility would involve for the organism" (Mosso 1906: 156). A similar analysis was later offered by another fatigue researcher, Josefa Ioteyko. In an address given at the IV International Congress of Psychology in Paris in 1900, she compared fatigue to pain in terms of their common roles as "psychical defenses"(Ioteyko 1913). Polish-born and Paris-trained, Ioteyko was an important figure in the physiology of fatigue, when she took over the leadership of the *Laboratoire d'Énergétique Solvay* (renamed the *Institut de Physiologie de Bruxelles*) in 1902. Solvay had been a student of Marey's, but Ioteyko was a devout disciple of Mosso, naming the science of fatigue "*ergographie*" after Mosso's instrument in 1904 (Rabinbach 1992: 136-142).

Psychology, physiology, and even historical analysis were converging around designs for the amelioration of human existence through scientific research, aspirations that were shared by the educational psychologists who translated *Fatigue* into English.[27] Social progress, interpreted as the gradual elimination of class difference, was a historical fact for Mosso. But it remained incomplete without an appropriate analysis of the fatigue that perpetuated such divisions. Calling for "fresh investigations...made by independent men, by physiologists free from all preconceptions whether political, humanitarian, or social," Mosso described the racial degeneration among Sicilian sulphur workers brought on by their endless fatigue. The development of industrial machinery had diminished the price of commodities, but had done nothing to relieve the misery of human labour. But although he agreed with Marx's history, Mosso had no truck with the politics of revolution. The moral education of the masses through the dissemination of scientific knowledge was his surrogate for political upheaval. This would serve to increase the *social* sensitivity to the impending degeneration in a fatigued civilization, just as the sensation of fatigue in the individual prevented him from persisting in his destructive labour:

> It was Christianity which proclaimed the equality of men, and for the first time gave us a glimpse of the community of goods. As civil progress has been accomplished, men have advanced steadily towards equality till a privileged nobility has disappeared. But the onward march of humanity is not arrested, and today we are tormented by the grave and fearful problem of a more radical equality. This is the great difficulty, on which all are engaged who have at heart the liberty and the dignity of man. It is no longer a party question, no longer a cry raised to overthrow governments; it is a profound conviction, a sacred moral sentiment, which spurs us on to seek out means by which property may be divided without violence, without bloodshed; by which he who gives employment may give it in virtue of humane laws, and he who receives it not become a slave, nor the human race degenerate under the usury of fatigue (Mosso 1906: 176).

The physiological study of fatigue in the late-nineteenth century was the result of the intersection of methods, concepts, and professional and social interests. Graphical technologies were fused with evolutionary hypotheses by physiologists who wanted to expand their domain beyond the medical realm to the classroom and the factory floor. Physiologists interested in

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[27]Mosso's English translators—Margaret Drummond and W. B. Drummond—both wrote on developmental psychology. Mosso himself was keenly interested in the influence of physical exercise on mental development, publishing several books on the subject between 1881 and 1900, all of which were translated into German, English and French.

fatigue were fixated on the activity of the muscles, and relegated the muscular passivity in sleep to the status of an insignificant diversion.

Hypnotism

Hypnotism offers a third example of a phenomenon that was akin to disease, and which drew numerous comparisons to sleep. Sleep and hypnosis were thought to be virtually identical when A. A. Liébeault revived the clinical use of hypnotism in 1864. But sleep was little more than a word that described a state in which the will languished. Hypnosis was a state of sleep in which the hypnotiser's will supplanted that of the subject through suggestion. Forty years later, Hyppolyte Bernheim, a devout follower of Liébeault, insisted that sleep and hypnosis were entirely unrelated: where true sleep was, hypnosis could not be, and where hypnosis was, there was nothing but suggestion. What remained of sleep after the debates over hypnotism subsided was dreaming, which will be the subject of chapter two.

Hypnotism in the clinic: Liébeault, Bernheim & Charcot

The idea that sleep can intrude into the course of normal wakefulness is undoubtedly very old. In the late twelfth and thirteenth centuries, the behaviour of "sleepers" became an important problem for many theologians (Borreau 1991, 1993; Hacking 1995:147). Sleepers would fall into a trance-like state and act differently than they would when awake. They would perform violent or unusual acts, or suddenly became capable of doing things they could not ordinarily do. They also had little recollection of what they had when they returned to their normal state.

Somnambulism, or sleep-walking, took on considerable importance when an artificial variety was introduced as a cure for nervous attacks by A.M.J. Chastenet de Puységur (1751-1825). Puységur was a disciple of Franz Anton Mesmer, whose work launched the debates over the moral and therapeutic merits of "animal magnetism" shortly before the French Revolution (Ellenberger 1970; Chertok & Stengers1992; Gauld 1992). Mesmer himself seems to have made few references to sleep in his description of his therapeutic "magnetic passes" over his patients. His work invoked a discourse of forces—it was a description of how magnetic fluids accumulated in animate and inanimate bodies. As one commentator has noted, "Mesmer regarded his discovery of animal magnetism as a major contribution not just to physiology but to physics" (Gauld 1992:11). Puységur, on the other hand, emphasized the relationship between magnetic "sleep" and sleep-walking (Ellenberger 1970:71). In 1812, he argued that invoking a state of "artificial somnambulism," in which the magnetizer would instruct his patients to diagnose diseases and prescribe treatments, cured a host of nervous ailments (Puységur 1999). By the 1820s, sleep and somnambulism begin to appear in numerous other treatises on magnetism, particularly those by the Abbé Faria and others who had seen the miraculous cures of this Portuguese priest.[28]

Physiological explanations, rather than those based on occult causes, were not forthcoming until James Braid (*ca.* 1795-1860), a Manchester surgeon, definitively abandoned magnetic passes in favour of having his subjects stare at bright objects. He rejected explanations based on magnetic fluids and forces, and argued instead that "hypnotism" was a kind of sleep, caused by a paralysis of the eye muscles (Braid 1843). Braid at first argued that mesmerism was entirely imaginary, while hypnotism was the result of physical stimulation. But he later admitted that hypnosis could also be achieved through verbal suggestion. It was this latter path that Auguste Liébeault would follow in the 1860s.

Scientific, although not popular, interest in hypnotism languished between the 1850s and the late 1870s. So although his work was not particularly well-known until Bernheim visited his

[28]Ellenberger (1970) lists, for example: Abbé de Faria, *De la cause du sommeil lucide, ou Étude de la nature de l'homme.* Tome 1[er] (chez. Mme. Horiac: Paris, 1819); Alexandre Bertrand, *Traité du somnambulisme et des différentes modifications qu'il présente* (Dentu: Paris, 1823); and F.J. Noizet, *Mémoire sur le somnambulisme et le magnétisme animal* (Plon: Paris, 1854).

clinic in Nancy in 1882, Liébeault (1823-1904) is frequently considered to be the father of the practise of hypnotic therapy (Chertok 1966). Having taken his doctorate of medicine at Stasbourg, he began to practise at Pont Saint-Vincent, just outside of Nancy. As a student, he had cultivated an active interest in animal magnetism, but his patients, who were mostly peasants, did not seem to respond to this practise. On the 27th of February, 1860, he heard a lecture on "Braidism" delivered to the *Académie des sciences* by Velpeau, a well-known surgeon.[29] Inspired, Liébeault began to experiment with the technique. Velpeau, who drew upon the work of Etienne Eugène Azam and Paul Broca, wanted to promote the use of hypnotism as a surgical anaesthesia. Liébeault used it to cure disease. Just around the same time that William Hammond was developing a psychiatric nosology based on his observations of the circulation in sleep in the United States, Liébeault was also establishing a therapy based on the idea that sleep somehow instigated a process of healing that could be harnessed by clinicians. In 1866, he published a monograph on the subject, which was reported to be so unpopular as to sell a single copy in ten years.[30]

Despite their common interest in developing a theory of sleep in conjunction with medical practise, there are substantial differences between Hammond and Liébeault. Hammond offered a theory of sleep that relied on accepted physiological principles—in particular, the reflex

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[29]Chertok 1966. Eugène Azam, a Bordeaux surgeon who had a substantial presence at the university there, had already introduced Braidism to the Surgical Society, and the Academy of Medicine of Bordeaux, about a month earlier. Like Velpeau, Azam was interested in hypnotism for its anaesthetic purposes, and he and Paul Broca successfully used it on a patient. Pierre Janet later claimed that Azam had introduced the concept of multiple personality at this time, although it is clear that Azam said nothing of this topic then (Hacking 1995: 159-161).

[30]Auguste Liébeault, *Du sommeil et des états analogues, considérés surtout au point de vue de l'action du moral sur le physique* (Masson: Paris, 1866). Ellenberger (1970: 107) dismisses this story as apocryphal, noting, that Liébeault's theory of sleep was known even by Russian authors. Chertok (1966: 2946), on the other hand, cites a critical review in *Annales médico-psychologiques* (1867) as evidence that Liébeault's ideas were everywhere rejected: "Physiology," it reads, "as it is demonstrated by M. Liébeault, strays in all respects from that which guides medical practise down the road of progress today...We know not to place our trust in the mode of treatment that he advocates."

doctrine. Sleep appeared when brain activity, stimulus, and blood pressure all declined in concert. It was a passive, but necessary, withdrawal from the world. Disease would appear when sleep was neglected, due to the nervous excitability that was accompanied by excessive cerebral blood pressure. Liébeault, on the other hand, introduced the idea of "attention" into his description of sleep, which he identified with "nervous force."[31] Attention was not diminished in sleep, but merely redirected, usually towards the *idea* of sleep. But the direction of this force had physiological consequences. When too much of this force was directed to one or another brain centres, various pathologies could develop, as this disruptive force was conducted along the "grand sympathetic nerve" to other organs. Liébeault presented hypnotic therapy as a way to take advantage of the state of sleep (provoked by a command from the hypnotiser), and correct the pathological directions of the patient's attention. In either instance, both doctors built a successful practise in which discipline played a crucial role. Liébeault enforced his from above, telling his patients, who were mostly peasants, how to think when their will was apparently suspended in hypnotic sleep. Hammond relied on his patients' ability to discipline themselves, providing his well-to-do New York clients with regimens of electrotherapy and drugs, combined with recommendations on diet and hygiene.[32]

Liébeault, however, would probably have been forgotten to medical history, had he not received a visit from Hippolyte Bernheim (1840-1919) in 1882. Bernheim was a physician from Strasbourg, who, along with most of the university faculty, relocated to Nancy after the Germans annexed Alsace in 1871. Bernheim was convinced that hypnotic effects were not caused by any unknown force, or through normal physiological mechanisms. They were produced through

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[31]See Gauld, *A History of Hypnotism*, pp. 322-324.

[32]Shorter sets Hammond against George M. Beard, who, in 1876, recommended the use of mental cures in medical practise (*From Paralysis to Fatigue*, p. 239). But, as Blustein points out, Hammond's own M. D. thesis (now lost) was entitled "The Etiological and Therapeutical Influence of the Imagination." It is not the case that Hammond rejected outright the prospect of the mind creating disease; he simply refused to look at the mind as an entity distinct from the body (*Preserve Your Love For Science*, p. 159-161).

suggestion, which relied on the transference of an idea from the experimenter to the hypnotic subject (Chertok & Stengers 1992: 28). Thus, Bernheim encouraged Liébeault to try his cures with ordinary water that he had told his patients had been magnetized. It worked, and the Nancy School of hypnotism was born.

Bernheim set his claims up against those of Jean-Martin Charcot, the famous Parisian neurologist, who had been using hypnotism to illustrate the nature of hysteria. Charcot argued that hypnotism was actually a symptom of a (often hidden) hysterical condition. If you could be hypnotised, you must be an hysteric. Bernheim thought that anyone could be hypnotised, all it depended upon was the nature of the psychological relationship between the hypnotiser and the subject. Charcot's interest in hysteria was first and foremost diagnostic, which is perhaps not so surprising, when one considers the fact that spent almost all.of his time working in the Saltpêtrière, the largest asylum in France. Bernheim and Liébeault, on the other hand, were interested in the therapeutic potential of hypnotism.

Charcot seems to have said very little about the relationship between hypnotism and sleep. Alan Gauld, who has dealt with the relationship between sleep and hypnotism at some length, does not say anything about Charcot on this point. This is not particularly surprising—Charcot felt that hypnotism was a symptom of a disease, so why would he think it had any relationship with sleep? Bernheim, on the other hand, had been a student of Liébeault, who looked upon sleep as a part of a curative process that hypnotism could emulate.

The trajectory of Bernheim's ideas about hypnotism and sleep after 1890 presage, if they do not illustrate, the transformation of sleep from a question about the psychology of sensation to one of evolutionary development. By the end of the 1880s, Bernheim was becoming more and more successful in generating the phenomena of hypnosis in wakeful subjects. His suggestions to them seemed to produce the same effects—hallucinations, movements, anaesthesias—but without hypnosis. At first, he remained relatively faithful to Liébeault's interpretation, arguing that hypnosis was "a peculiar psychological condition" that increased the suggestibility of the subject. It could also produce sleep, which Bernheim continued to view as a phenomenon of auto-suggestion (Gauld 1992: 544). By 1897, Bernheim had completely rejected the notion that hypnosis was a distinctive state at all. At the twelfth International Congress of Medicine held at Moscow in August of that year, he declared that hypnotism simply did not exist—there was only suggestion.

Bernheim's argument was based on his observation that not all hypnotised subjects presented the phenomena of sleep. Some would fall into a deep state of sleep, forget they had been hypnotized, or even report that they had been dreaming. Others would not even become drowsy, and retained a full memory of what had taken place. The only thing these patients had in common was that they were amenable to suggestion, something that the onset of sleep tended to eliminate. Bernheim was convinced, as virtually everyone was before Freud, that dreams could be manipulated by external stimulus, the classic motor phenomena of hypnotism (catalepsy, paralysis, anaesthesia) could be created only when the subject was awake, and capable of collaborating with the experimenter.

In Bernheim's scheme, suggestion did not simply supplant hypnotism as a unique state of consciousness. It was part of everyday existence. In a paper that he presented to the 1911 meeting of the International Society for Medical Psychology and Psychotherapy, Bernheim insisted that suggestion in the wakeful state did not somehow generate a unique state, analogous to hypnosis, in which it could function. "It would be an abuse of words," he charged, "to call it hypnotism, a word which involves the idea of sleep and the idea of a special state which would not be our ordinary state" (as cited in Gauld 1992: 546). To underscore this idea that such influences took place all the time, Bernheim turned to the graphical method:

> Without saying anything, I record [a waking subject's] pulse on a cardiograph...While the trace is forming I count the pulse loudly, at first correctly. After a while I count more beats than there really are; and then less. Studying the trace later, I discover that the pulse rate increased during the accelerated counting, and diminished during the slowed counting...without the knowledge of the subject...(as cited in Gauld 1992: 546).

Two points need to be made here. First of all, Bernheim's position was not particularly well-received. He seemed to many, like Auguste Forel, to be overstating his case about the effects of waking suggestion. Others, like Edouard Claparède of Geneva, felt that hypnotism could still be distinguished from any other suggestive state by virtue of post-hypnotic amnesia. We will speak of Claparède later, in chapter three. The second point is that, by 1911, it did not matter a great deal what anyone said about hypnosis. As a research programme, it was practically dead. Although Gauld does a good job of demonstrating that interest in hypnosis lingered on after 1911, for all intents and purposes, hypnotism was in decline as early as 1896. More importantly, its relationship with sleep was dissolving. Sleep was becoming an object of physiological interest, just as hypnotism was becoming part of psychology's past.[33]

Sleep & memory: Delboeuf

Sleep was, however, being fit somewhat uncomfortably into the discourse of memory through the study of dreams. As the nineteenth century wore on, memory was becoming an increasingly popular field for both physiological and psychological research (Otis 1994; Hacking 1995). The concept of "organic memory" explained physiological effects from inheritance to habitual behaviours, while psychological memory was beginning to dominate the debates over hypnotism, hysteria, and the nature of mental illness.

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[33]A popular review of sleep research illustrates once again the close correspondence between fatigue and sleep, particularly for social reformers who drew their arguments from physiology. Marie de Manacéïne, a Russian educational reformer from St. Petersburg, produced a book on fatigue and its relationship to degeneration entitled *Le surmenage mental dans la Civilisation moderne* in 1890. Two years later, she published a book on sleep, which, despite its title—*Sleep: Its Physiology, Pathology, Hygiene, and Psychology*—was concerned more with the importance of sleep hygiene than anything else. The preface to her book on fatigue had been written by none other than Charles Richet, a physiologist, and one of the most outspoken eugenicists in France.

Joseph Delboeuf (1831-1896), a professor at the University of Liège, seems to have been sucked into the vortex of hypnotism a few years after the "sciences of memory" made their appearance along with the revival of hypnotism in France. He visited Charcot at the Saltpêtrière, and Bernheim at Nancy in 1886.[34] But a year before he made the trip to discover the truth about hypnotism, Delboeuf had published a book on sleep and dreams that featured an extensive review of some recent books on the physiology of sleep (Delboeuf 1885). The book's curious title—*Le sommeil et les rêves, considérés principlalement dans leur rapports avec les Théories de la Certitude et de la Mémoire*—indicated Delboeuf's interest in the old and the new. Descartes had brought the question of doubt to philosophical debates over dreaming in his *Meditations*. But the relationship between dreams and memory was only just beginning to assume a major part of the study of dreams. On this basis, one historian has suggested that Delboeuf's book had a substantial impact on Freud's thinking (Duyckaerts 1989).

Delboeuf had begun his project several years earlier, publishing an analytic bibliography on theories of dreaming in the *Revue scientifique* in 1879. It would seem, then, that Delboeuf was brought to his interest in hypnotism through his study of sleep. Regardless of whether or not this is the case, hypnotism killed his interest in sleep. After his visit, he published nothing further on the subject, instead becoming mired in the debates over the moral dimensions of the use of hypnotic therapy in medical practise.

Delboeuf thought that physiologists were making a grave error by interpreting sleep in terms of fatigue. Sleep was not a state in which some unknown force appeared to "repair" the body during the night. It was simply a passive state that followed the exhaustion of the senses:

> The function of nutrition in relation to sensation points us towards the cause of sleep and its periodicity. The nutriments that accumulate in the body serve or served, among other functions, to form the foundation of peripheral sensitivity. The latter loses its sensibility through its exercise; at last there comes a point where it can no longer shut out the sensations and becomes, by

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[34]Ellenberger 1970: 172. Gauld (1992: 321) has Delboeuf visiting Liébeault's clinic in 1888. I thank André LeBlanc for encouraging me to pay attention to Delboeuf's curious work.

> consequence, incapable of reacting. Sleep seizes hold of us—sleep, the sign that there is a barrier between us and the external world. This period of torpor is used to reconstitute sensitivity, and, as this work advances, sleep disappears, giving way insensibly to wakefulness. Sleep is not a function; it is a concomitant effect. It does not repair any force. The truth is that it appears when sensitivity is enfeebled, and it disappears when it is revived (Delboeuf 1885: 166).

The most interest aspect of sleeping, thought Delboeuf, was that it denied any power to doubt. The sleeper was absolutely confident that her dream-images corresponded to an external reality. He was also startled, as Freud would be, by the ability of dreams to evoke images taken from the past that had been completely abolished from waking memory. Physiology, Delboeuf argued, could explain neither observation. His account was based on the notion that the function of sensation involved a "fixation of force." Sensitivity was nothing more than the organism's ability to transform external forces that impressed themselves upon the internal senses. Memory-traces, which could never be erased, were the result of forces becoming "fixed" in the organism. And, like all physical forces, the force that created memory tended towards equilibrium. The exhaustion of the organism's ability to fix sensation as memory coincided perfectly with a state of disequilibrium between the internal force of the organism and that of its environment. This process ended in sleep, which featured a withdrawal of external sensation, causing the hallucinatory and memory-laden phenomena of dreaming.

For Delboeuf, sleep had little to do with the physiology of the musculature. It was the phenomena of sensation that provided the key to understanding not only sleep, but also its artificial analogue, hypnotism. "Natural or artificial," Delboeuf said, hinting at the direction of his future research, "sleep is always accompanied by an insensibility more or less extended, more or less profound. The cause of the one is the cause of the other" (Delboeuf 1885: 166). In his conclusion, Delboeuf expanded this argument into the realms of epistemology. The function of dreaming, he suggested, was to provide a native instance of "speculative doubt." This doubt was insincere and superficial—people only seemed to believe in the reality of their dreams while they were dreaming. But the doubt was a nagging and persistent one. It encouraged people to challenge accepted doctrine, thus promoting the health and well-being of the sciences.

Sleep, hypnotism & memory: Albert Moll

Delboeuf's arguments indicate the extent to which dreams were gaining in philosophical and epistemological importance through their association with memory by the end of the 1880s. But was hypnotism—a word Braid had borrowed from *Hypnos*, the Greek god that brought sleep to humans—a kind of sleep or not? Yes and no, went the chorus. Bernheim's final concept of "waking suggestion," which eliminated the hypnotic state altogether, smacked too much of irrationalism for most people. Albert Moll (1862-1939), a Berlin psychiatrist who made pilgrimages to the Saltpêtrière and Nancy the same year as Delboeuf, took a more tempered view of the relationship between sleep and hypnotism. But it turned on the question of memory, just the same.

In 1889, Moll published a general survey of hypnotism—*Die Hypnotismus*—that was quickly translated into English, going through five editions by 1901. Moll is better known for his later work on sexual psychopathology, but he first made his mark by helping, along with Auguste Forel in Zürich, to disseminate hypnotic therapy among German clinicians.

Moll sided with the Nancy school on most issues. Hypnotism was not, as Charcot argued, a pathology. It was invoked by the hypnotist's suggestion to concentrate on the idea of sleep. This began the state of "rapport" that characterized the relationship between hypnotiser and hypnotic subject. Moll disagreed, however, with Liébeault's argument that the hypnotic state and normal sleep only differed insofar as one depended upon suggestion and the other did not.[35] This was misleading, said Moll, because there are actually two stages of hypnosis: a light stage, which

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[35]Moll 1891: 192. "Rapport" is an elusive term, to say the least. For Ellenberger (1970: 152-155), it refers to a crypto-transference—the patient's desire for the love of the psychoanalyst. Gauld (1992), who discusses Moll at some length, approaches the question from the other side—"rapport" simply describes the ability of the hypnotiser to manipulate the patient. Moll uses the term as Gauld does, saying nothing of desire or sexuality in regards to hypnotism in 1889. On Liébeault's understanding of the relationship between hypnotism and sleep, see Ellenberger 1970, p. 86.

was entirely unlike sleep; and a deep stage, which was closely related to sleep. The early stage of hypnosis was marked by a loss of voluntary muscular movement. The onset of sleep, on the other hand, featured only the feeling of fatigue. There was also a decrease in mental activity in sleep that did not appear in light hypnosis. Characteristic of this was the persistence of self-consciousness in hypnosis, which was signified by the fact that lightly hypnotised subjects could remember their experiences. The sleeper's awareness of his present situation, on the other hand, was obliterated.

Deep hypnosis, however, bore a closer resemblance to true sleep, and Moll characterized this relationship by referring to dreams. Following the influential work of Alfred Maury (1817-1892), a Professor of History and Philosophy at the Collège de France, Moll argued that dreams originated in some external stimulus, from which the brain followed a train of idiosyncratic, but logical, associations.[36] The sense delusions induced in deep hypnosis were analogous to the dreams that could be induced in sleep. The only difference was a quantitative one: suggestion in hypnotism was stronger than physical stimulus in sleep (Moll 1891: 201).

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[36]Alfred Maury provided the prototypical example of such a dream for the second half of the nineteenth century. He dreamed of being a victim of the Terror that followed the 1789 revolution in France. His dream seemed to take days: he was chased, caught, tried, led to the guillotine, and executed. When he awoke, he discovered that a curtain rod had fallen on his neck. The dream—a beautiful work of associationist psychology—demonstrated the speed at which the mind composed narratives that were triggered by a single stimulus. The third edition of Maury's book contained a lengthy appendix on David Hartley's associationist theory of mind (Maury 1865). Maury, part of a larger group of mid-century French materialists, also published on magic, astrology, mythology and medieval religious legends (Ripa 1988: 138-147). J. Allan Hobson, a neuropsychiatrist with an interest in history, is attracted to Maury's meticulous practise of keeping dream-journals, calling him "a French scientist." Freud, a neurologist with training in zoology, physiology and anatomy, is denied a similar scientific status. It is not at all clear that Hobson is aware that Maury was primarily engaged in historical, rather than the biological, research (Hobson 1988: 32-34).

Waking suggestion, however, was ruled out of court. If hallucinations could be induced, the pathological state of hypnosis must be present. Bernheim was wrong, and hypnotism and sleep must not be confused. They were analogical, not identical:

> Authors often confuse hypnosis with sleep in speaking of suggestions in the waking state. We have seen that the light hypnotic stages do not much resemble sleep; consequently we must not conclude that a state of contracture, &c., is, or is not, a hypnosis because it resembles sleep or not...they [also] think that hypnosis is excluded in these cases of waking suggestion, because none of the usual methods of inducing hypnosis have been used. But the methods are not absolutely necessary for the induction of hypnosis...If, then, we can do the same thing without any previous appearance of hypnosis, *we must call the state a hypnosis all the same, particularly if there is subsequent loss of memory*, which is generally the case in delusions of the senses. There has been a kind of hypnosis in both cases (Moll 1891: 210. My italics).

Loss of memory had become the distinctive sign of the hypnotic state by the end of the nineteenth century. Its importance to the debates over hypnotism was so marked that people like Moll could even argue that if amnesia was present, hypnotism must have taken place, even if no one had intentionally induced it! Sleep, on the other hand, was retained as a sort of firewall against the artifice of hypnotism. Sleep's very presence as a natural, but entirely unexplained, phenomenon, provided the backdrop upon which a discourse of hypnotism and memory could be manufactured. Moll and Bernheim both invoked sleep as a counterpoint to their ideas about hypnotism, as they elaborated them through the phenomena of hypnotic amnesia and waking suggestion. But neither Moll nor Bernheim attempted in any way to explain what sleep was, from a psychological or a physiological point of view. When the use of hypnotism as an experimental technique fell from grace, the question of sleep became more attractive to those investigators who wanted to study the psychology of the unconscious from a physiological perspective.

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By the end of the nineteenth century, sleep was poised to become an important subject for physiological investigation. Even while sleep's clinical significance was in decline with the eclipse of Hammond's nosology of "cerebral anaemia," it was brought into the physiological laboratory through the growing epistemological power of the graphical method. Mosso's study of fatigue had brought psycho-physiological research to the borders of eugenics and social reform. Although sleep was only a marginal aspect of this research, it held a highly symbolic meaning for psycho-physiology precisely because it offered a natural, mundane instance in which the body appropriated the mind with a law-like regularity. The late-century revival of hypnotism and the corresponding interest in memory served to reinforce sleep's symbolic status by analogy. Even when hypnotism's own significance dissolved, sleep's importance was retained through the alliance forged between dreams and memory. Within a space of five years (1899-1904), the importance of both sleep and dreaming would be articulated in terms of function.

Chapter II

The function of dreaming: Freud & Bergson

Freud was the first to argue that dreams served a function. Although he defended this argument by offering a peculiar form of dream interpretation, the force of his claim was in its ability to tie the images of dreams to biological concepts. The confused, hallucinatory nature of dreams were previously thought to be the product of a consciousness that had somehow been compromised during the night. Dreams were akin to the delusions of madness. Freud turned the tables on this analogy, and claimed that dreams were actually an integral part of normal mental life. They were hidden memories that the mind revised in an attempt to protect sleep. An examination of the regressive and repressive aspects of this process would reveal how the mind operated in wakefulness. Such an analysis would, Freud thought, bring together the study of culture and nature under the rubric of the unconscious.

While Freud was constructing his unique vision of dream interpretation, Bergson was also bringing the subject of dreams to bear on his analysis of normal mental activity. But where Freud developed an elaborate system to untangle the true dream that lay beneath its clouded surface, Bergson adopted a phenomenological approach. He left the confused images of dreams as they were, and constructed a theory of mental representation that incorporated their disordered nature into normal mental activity. Like Freud, he argued that the secret to dreams was their relationship to memory. But it was waking reality that wore a disguise, argued Bergson, not dreams. The pragmatic demands of waking life forced a re-orientation of memory towards the question of organismic survival—a question that was entirely absent in the "disinterested" state of sleep.

Both Freud and Bergson held that dreaming shared a special relationship to the past, and were thus useful tools in the study of normal consciousness. Both were inspired by recent scientific developments, first in neurophysiology, and later, in evolutionary theory. But where Freud saw dreams as serving an important purpose, Bergson denied that they served any useful function at all. In this way, Freud and Bergson framed the debate over dreams for the twentieth century as a question of function.

The unity of dreaming: Sigmund Freud

Anyone interested in the history of the modern sciences of mind faces the daunting task of wading through the secondary literature on Sigmund Freud (1856-1939), the Viennese neurologist and central architect of psychoanalysis. The commentary on Freud is enormous in volume, vast in scope, vehemently partisan, and frequently opaque. Freud himself was a prolific writer—the standard edition of his works runs to two dozen volumes in the English translation, and this does not include the mountains of correspondence (much of it published) that he maintained with his fellow analysts. For a century, disciples and detractors alike have followed suit, creating their mass of literature that shadows Freud's own.

Much of this literature can be traced back to one of the most common forms of story-telling, that of narrating a dream. Freud published his most famous book, *The Interpretation of Dreams* in 1899. Although initially greeted with a rather ambivalent reaction, the book was established as an indispensable text for the study of dreaming within a decade. It was the only book that Freud chose to revise throughout his career, and with good reason—it was his most popular work. Books on dream interpretation had become a best-selling genre of the book trade by the middle of the nineteenth century (Ripa 1988). Freud took advantage of this aspect of popular culture by daring to suggest that all dreams could be understood through biological principles, rather than through the application of a lexicon or "dream key." In this sense, his role was very much like that of Charles Darwin more than forty years earlier. Each took a popular epistemological practise (collecting as part of natural history, or interpreting dreams as part of self-knowledge or divination), and tried to render it scientific by appealing to principles of biological order (the branching of natural selection, or the flow of psychical energy).

My interest in Freud is only insofar as he is relevant to the history of sleep research, so I will draw on a number of studies that attempt to fit him into his historical context—even though many of them say little or nothing about his dream theory. Freud's context was primarily within clinical medical practice and the life sciences. He was, after all, a practicing neurologist with medical training. In the last quarter of the nineteenth century, he studied anatomy, zoology, and physiology, all of which were framed by the variations of Darwin's evolutionary theory that prevailed when Freud was a student during the 1870s and 1880s. As a medical student, Freud was expected to take a number of courses in the laboratory and life sciences that would have had little to do with the everyday practise of neurology. But Freud had a keen interest in those sciences, and exploited his knowledge of them, using evolutionary and physiological theory to arrive at a method of dream interpretation that treated dreams as diagnostic signs revealing an underlying, but pervasive, pathology.

But while there is an enormous scholarly interest in Freud's impact on the concepts and practises of the human sciences, very little of this research is dedicated to his work on dreams. Harvie Ferguson has recently encouraged his fellow sociologists, who find Freud's reincarnation in fields ranging from literary theory to social anthropology, to re-evaluate Freud's relevance for sociologists by examining Freud's work on dreams, which Ferguson places at the origins of modernity. "The important question," Ferguson claims, "is not whether sociologists should make judgements on *Civilization and its Discontents*, but how they should respond to *The Interpretation of Dreams*" (Ferguson 1996:viii). Social scientists, complains Ferguson, have been blind to all but Freud's later works. Ferguson suggests that an examination of the context in which Freud arrived at his dream-theory provides the key to the rest of the Freudian corpus. Freud's enormous influence on modern social thought can be traced back to his discovery that dreams have meaning.

Patricia Kitcher, a philosopher of science with an interest in the interdisciplinary dynamics of contemporary neuroscience, seems to agree with Ferguson. Her analysis of Freud as "the first interdisciplinary cognitive scientist" turns on an examination of Freud's dream-book as well (Kitcher 1992:5). Both Ferguson and Kitcher offer a strongly contextual account of *Interpretation of Dreams* that is drawn from historical research into the biological background of Freud's ideas. Some of this work has been done by professional historians, some of it by natural scientists (Pribram & Gill 1976; Sulloway 1979; Ritvo 1990; Bilder & LeFever 1998). All of it, however, is motivated by a similar concern, best expressed through Kitcher's emphasis on interdisciplinarity: the neurosciences, an orphan child with many fathers, are searching for a history of their field that extends beyond the end of the Second World War. And they have found it in a "hidden" Freudian text—his "Project for a Scientific Psychology," which Freud abandoned in 1895, only to have part of it reappear as the seventh chapter of later editions of *Interpretation of Dreams*. The "Project" itself, curiously enough, was only published in 1950, just as the neurosciences were starting to take shape.

Kitcher and Ferguson emphasize the importance of biological styles of thinking to Freud's work on dreams. Freud was the first to provide a comprehensive account of dreaming based on the concept of function, a dominant theme of the evolutionary biology of Freud's day (Bowler 1984; Persell 1999). But I am less interested in formulating an origins myth for the neurosciences (or for sociobiology), nor am I convinced that Freud, who created a highly distinctive set of practises and theories while also maintaining a quite rigorous and orthodox social network, can be best understood in terms of interdisciplinarity. But when viewed from the perspective of the rise of sleep research as an independent field, the Freudian moment was crucial. It introduced a functional account to sleep's most mysterious mental phenomenon, and thus established the problem of dreaming as a key problem for *any* physiological theory of sleep. After Freud, sleep researchers had to incorporate dreaming into their theories, rather than simply explain it away. Dreaming became an essential, rather than an accidental, aspect of sleep.

Freud's dream theory

What did Freud think dreams were? Kitcher simply states that "dreams occur," and that "only the most skeptical or cautious would question the existence of the phenomenon of dreaming and assume only dream reports" (Kitcher 1992:115). She assumes that Freud, like contemporary neuroscientists, held dreams to be mental events taking place over the course of a night, and not merely stories told upon awakening.[1] Aiming to depict Freud as a theoretician above all else, Kitcher neglects the fact that Freud's ideas about dreaming were first formulated through listening to hysterics describe their symptoms—the "talking cure" supposedly developed by Freud's colleague, Josef Breuer, and first presented in their joint 1895 project, *Studies in Hysteria*.[2] Freud's interest in the content of dreams must be set within his context as a clinician, listening to patients narrate their experiences.

A better guide to the affairs of the day is the historian of psychiatry, Mikkel Borch-Jacobsen. In his enlightening study of the very first psychoanalytic cure, Borch-Jacobsen reconstructs the famous case of "Anna O.," whose real name was Bertha Pappenheim. He argues that Pappenheim was never cured of her hysteria through psychoanalysis. In fact, she returned to the asylum several times after seeing Breuer. She successfully rehabilitated herself, eventually becoming a prominent philanthropist and social worker in Frankfurt on the Main. Borch-Jacobson argues that Breuer cooked up many of the details of the case (some written thirteen years after the fact), on the insistence of Freud (Borch-Jacobsen 1996). The "talking cure," which at first featured Pappenheim narrating "childish stories" to Breuer on a highly regular basis (usually one each day, two if she missed a session), culminated with Pappenheim, placed in a trance-like state by Breuer, narrating every event that had produced each of her hysterical symptoms, thus curing her. Her later stories, unlike her earlier fantasies, were all verified by a

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[1]One sleep researcher has suggested that the idea that dreams, like perceptions, are discrete mental events tied to sensations began with Alfred Maury's research of the early 1860s (Jouvet 1999:3 & 31; Maury 1865). Maury's efforts were part of a larger movement by moderate liberal reforms to emphasize the mundane physiological origins of dreams, in contrast to the conservative Catholics and radical republicans who looked to dreaming as a fount of inspiration (Ripa 1988; Dowbiggin 1990).

[2]It is surprising that Kitcher misses this genealogy, given her remarkably concise contrast of the diachronic emphasis in the evolutionary-based explanation of Freud's era, and the synchronic, computational emphasis of post-W.W. II cognitive science (Kitcher 1992: 215). But Kitcher's study clearly aims at preserving the transcendental nature of empirical facts. Thus Freud, in order to be wrong (which Kitcher admits he is), must be a misguided theoretician in her story.

meticulous diary that Pappenheim's mother kept of her daughter's behaviour. The cure, buttressed by the evidence of the diary, suggested that the cathartic action of remembering could cure hysteria. And psychoanalysis was born.

Borch-Jacobsen presents a strong case for his claim that the origins of psychoanalysis were completely misrepresented by Freud. But my concerns here are rather different. Although Freud clearly used his strategy of dream interpretation as part of his therapeutic procedure, he also applied it to non-pathological cases; most notably, himself. Dreams were the segue through which the "talking cure" entered into popular culture, where it traded its therapeutic value for something more akin to "self-knowledge." Borch-Jacobsen says nothing about Freud's dream theory, because its epistemological arena was not the clinic. The wish-fulfilment theory of dreams was not demonstrated to be false if it did not effect a cure.

How did Freud come to understand the problem of dreaming in functional terms in the first place? It is certainly true that Freud tied the origins of his dream-theory together with the original psychoanalytic myth. He seems to have willfully misrepresented not only the fact that Pappenheim was not cured, but that her cure evolved through her remembering real events, not dreams. In his historical survey of "the psycho-analytic movement," composed long after *Interpretation of Dreams* had gone through several editions, Freud lashed his theory of dreams firmly to the mast of Anna O.:

> LADIES AND GENTLEMEN,—It was discovered one day that the pathological symptoms of certain neurotic patients have a sense. On this discovery the psycho-analytic method of treatment was founded. It happened in the course of this treatment that patients, instead of bringing forward their symptoms, brought forward dreams. A suspicion thus arose that the dreams too had a sense (Freud, *Introductory Lectures on Psycho-Analysis* 1950 XV: 83).[3]

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[3]All references made to *The Standard Edition of the Complete Works of Sigmund Freud* will be as follows: (Freud [year] [volume]:[page number]). References to other translations of Freud's work will not include a volume number; for example (Freud 1999:180).

But in fact, neither Freud nor Breuer said very much about dreams in *Studies on Hysteria* (1895). Breuer did state that Pappenheim's "altered state" in which she told her childish stories "may well be likened to a dream in view of its wealth of imaginative products and hallucinations, its large gaps of memory and the lack of inhibition and control in its associations," but this was simply an analogy (Freud 1953 II: 45). Breuer said nothing about what Pappenheim herself actually dreamed.

Yet 1895 was important for Freud's dream theory in another way. In this year, Freud had a disturbing dream about one of his former patients—the famous "dream of Irma's injection." "Irma," who Freud describes as a composite character, was a patient whom Freud had been unable to cure through psychoanalytic methods. In his dream, she appeared as a guest at a party held by the Freuds. He chastised Irma for not accepting his proposed cure, and she responds that her complaints (pain in her throat, stomach, and abdomen) are more serious than he thinks. Freud fears he has overlooked some organic affliction, and proceeds to inspect her mouth. He finds a white patch, along with greyish scabs formed in the shape of the turbinal bones of the nose. Other physicians begin to crowd around, and agree that there is certainly an infection. Its source turns out to be an injection given Irma by Freud's friend "Otto," and that "probably, too, the syringe was not clean" (Freud, *Interpretation of Dreams* 1953 V). On its surface, the dream supported Freud's theory that dreams were always the fulfilment of a wish. Freud wanted to absolve himself of responsibility for Irma's continued illness. After a lengthy self-analysis, Freud was convinced that *every* aspect of the dream was the manifestation of a secret wish. Freud felt this dream to be so important—he returns to it over and over again in *Interpretation of Dreams*—that, in 1900, he was prompted to make the following comment in a letter to his mentor (with whom he would soon break), Wilhelm Fleiss: "Do you suppose that someday one will read on a marble tablet on this house:

Here, on July 24, 1895
the secret of the dream
revealed itself to Dr. Sigm. Freud[4]

Regardless of which of Freud's 1895 origin stories we follow, it is clear that Freud drew from his clinical context to formulate his theory. As a neurologist treating hysterics, he listened to them describe their symptoms, and talk about their memories. As an aspiring scientist, he felt an acute anxiety about his professional reputation. In both situations, Freud was concerned about self-representation.

Dreams, like hysterical symptoms, were narratives based on memory. Whether this memory corresponded to some real event was actually much less important to Freud than Kitcher would have us believe. In fact, Freud took an attitude somewhat similar to that later adopted by his fellow countryman, the philosopher Ludwig Wittgenstein. Wittgenstein suggested that dreams were nothing more than a name people are taught to affix to a behaviour—that of telling a story upon awakening (Wittgenstein 1958). Later commentators, writing in the immediate post-REM era, attached the concept of meaning to that of verification (Malcolm 1959; *cf.* Hacking 1975). The question "am I dreaming now?" is impossible to verify, as verification would entail exercising judgement, which requires a lucid state of wakefulness, thus eliminating the dream state that was to be verified.

Freud certainly did not state his anxiety about the epistemological value of dreams in terms of analytic philosophy. Instead, he compared them, as we should expect, to his patient's delusional symptoms:

> In investigating dreams one is not even certain about the object of one's research. A delusion, for instance, meets one squarely and with definite outlines. 'I am the Emperor of China', says the patient straight out. But dreams? As a rule no account at all can be given of them. If anyone gives an account of a dream, has he any guarantee that his account has been correct, or that he may not,

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[4]Such a plaque was erected in front of Freud's house on May 6, 1977 (Masson 1985: 417-418).

> on the contrary, have altered his account in the course of giving it and have been obliged to invent some addition to it to make up for the indistinctness of his recollection? Most dreams cannot be remembered at all and are forgotten except for small fragments. And is the interpretation of material of this kind to serve as the basis of a scientific psychology or as a method for treating patients?...*We can help to overcome the defect of the uncertainty in remembering dreams if we decide that whatever the dreamer tells us must count as his dream, without regard to what he may have forgotten or altered in recalling it* (Freud 1953 XV:84-85. My italics).

This was a methodological, rather than an epistemological, pronouncement. There was no way to verify that a dream report actually represented a dream. Breuer claimed to have verified Pappenheim's stories as accurate descriptions of past events. But this issue did not particularly matter to Freud, although it certainly does to the postwar neuroscientists Kitcher addresses in her book. Freud thought that dreams were the active product of normal mental processes that went on all the time, not just during the night. Their function was to hide illicit desires from the sleeper's mind by transforming these disturbing images—the latent dream-thoughts—into the more palatable scenes of the manifest dream. Dreams demanded negotiation and interpretation. And, in this process of negotiation through free association, Freud argued that the dream process inevitably revealed itself, regardless of whether or not the story was truly a report of a dream. It was the narration of the dream as a process of self-representation that Freud felt was important.

Freud's understanding of dreams hinged on the notion of interpretation. But, as he was at pains to point out in *Interpretation of Dreams*, his version of interpretation was quite unlike that of the "dream key" books claiming to have discovered a code whereby each image of a dream held a distinctive meaning. Such books, following on the ancient practise of dream divination, usually took dreams to be portents of the future. But Freud's target was the past. Why did dreams hide the truth about the past? And how did they accomplish this? Freud's theory turned on the idea that every dream was the fulfilment of a wish. He had chosen the dream of Irma's injection well—it graphically illustrated Freud's wish to be considered a conscientious and careful physician. Dreams of falling, of failing an exam, or of close friends dying, however, presented a different situation. How could they be interpreted as wishes?

All dreams, argued Freud, had a manifest and a latent content. The superficial appearance of dreams—the dream report—was not to be trusted. It did not represent the thoughts that gave rise to the dream, but instead was the product of the "dream work" that served to repress those very thoughts which made up the latent content of the dream. The thoughts had to be hidden from consciousness because of their disturbing nature. They were typically (but not exclusively) expressions of illicit and immoral sexual desire that had their origins in the impulses of childhood. These impulses persisted in memory throughout adult life. They were, however, largely inaccessible to waking consciousness, as they had been completely repressed by an internal pattern of censorship that developed upon maturity. In sleep, this internal censorship was relaxed, and the pressure built up by repressed desire was allowed to escape. The only way it could escape, however, was in disguise. This was because there were actually *two* aspects of desire running through *Interpretation of Dreams*. The first was the sleeper's wish to continue sleeping, uninterrupted. The second wish was that of the unconscious, which wanted its urges to receive some sort of conscious representation. An expression of illicit desire might disturb the sleeper. Thus, the dream took unconscionable desire and cloaked it in the images of memory.

Dreams thus served two closely related functions. On the one hand, they were the "guardians of sleep," in that they often took stimuli that might awaken the sleeper—an noise in the bedroom, or the feeling of hunger or thirst—and converted it into a story in which these sensations were either satisfied, or portrayed as something that was part of the normal course of affairs, and the dreamer continued sleeping. But they also protected sleep from a more threatening source of stimulus—the memory of repressed desire. By disguising objectionable desires in the neutral images taken from recent memory—the unnoticed events of the immediately preceding day, for example—sleep remained undisturbed even as the unconscious mind achieved a limited form of expression.

Freud held fast to his functionalist account of dreaming, demonstrating how dedicated he was to adapting biological thought-styles to this question of mental representation (Sulloway

1979). Even if dreams were ridiculous, bizarre, or frightening, Freud argued that sleep could persist—in such cases, the sleeper actually knew he was sleeping:

> In some cases, when a dream carries things too far, the *Pcs.* [preconscious system, which was involved in censorship] says to consciousness: 'Never mind! go on sleeping! after all it's only a dream! But this describes in general the attitude of our dominant mental activity towards dreams, though it may not be openly expressed. I am driven to conclude that *throughout our whole sleeping state we know just as certainly that we are dreaming as we know that we are sleeping.* We must not pay too much attention to the counterargument that our consciousness is never brought to bear on the latter piece of knowledge and that it is only brought to bear on the former on particular occasions when the censorship feels that it has, as it were, been taken off its guard (Freud, *Interpretation of Dreams* 1953 V:571. Italics original).

Freud's explanation of anxiety dreams (of falling, of the death of a loved one) followed a similar pattern. He asked his readers to reject the idea that such dreams were part of the normal process of dreaming, precisely because they ended in wakefulness. Anxiety dreams were actually a mark of pathology, because they failed to serve their physiological purpose of protecting sleep:

> The dream-process is allowed to begin as a fulfilment of an unconscious wish; but if this attempted wish-fulfilment jars upon the preconscious so violently that it is unable to continue sleeping, then the dream has made a breach in the compromise and has failed to carry out the second half of its task. In that case the dream is immediately broken off and replaced by a state of complete waking. Here again it is not really the fault of the dream if it has now to appear in the role of a *disturber* of sleep instead of in its normal one of a *guardian* of sleep; and this fact need not prejudice us against its having a useful purpose...Even where psychical health is perfect, the subjugation of the *Ucs.* by the *Pcs.* is not complete; the measure of suppression indicates the degree of our psychical normality...The theory of anxiety-dreams, as I have already repeatedly declared, forms part of the psychology of the neuroses (Freud 1953 V:580-582. Italics original).

All aspects of dreaming fell under Freud's functional account. If they did not, as in the case of anxiety dreams, they were simply ruled out of the physiological court, and condemned to mental pathology.

This was quite a shift from earlier accounts of dreaming, in which dreams were explained on the basis of how they related to perception. They were, as in the case of Maury's famous guillotine dream (discussed in chapter one), the result of the power of the mind being compromised in sleep. But they served no particular purpose—such teleology was probably far too reminiscent of the mystical and religious rituals that Maury studied while he wasn't keeping his dream journal.

Freud's biology

This is not to say, however, that Freud's arguments were without precedent. On the contrary, other biologically-minded researchers were getting into the game of dreams at around the same time. The biological context of Freud's ideas has been discussed at length elsewhere, and, as I have already suggested, much of the evidence for the claims that he was a "biologist of the mind" comes from his "Project for a Scientific Psychology" (Gould 1977; Sulloway 1979; Ritvo 1990; Kitcher 1992). Although Freud abandoned the "Project" in 1895, his study of dreaming gave him the opportunity to revise some of the ideas first set out in the "Project," and include them as the seventh chapter in later editions of *Interpretation of Dreams*.

Repression, one of the most important concepts for Freud's dream theory, had already appeared in the "Project." Repression drew upon the idea of energy.[5] The flow of psychical energy, described as *Q* in the "Project" and "cathexis" in *Interpretation of Dreams*, provided Freud with a neurophysiological style of explanation, grounded in the reflex concept. Every mental activity was accomplished through the transformation of energy that originated from outside the brain. Freud felt that ideas became "invested" or "charged" with energy, and that memory, too, could be understood as an accumulation of energy.[6] But memories differed from

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[5]It is surprising to find no reference to Freud's ideas about psychical energy in a book about "the human motor" (Rabinbach 1990). Alfred Maury also relied on the energy concept to describe the operation of the mind, describing it thus: "...there is thus a motor that is only the vibrations of the nervous substance. This mysterious motor, in man, as in nature, unveils itself before experiment" (Maury 1865: 481).

[6]A more recent English translation of the first edition of *Die Traumdeutung* translates *Besetzung* as a "charge" or "investment" rather than a "cathexis" (for examples, see Freud 1999:364 & 408).

one another according to the amount of affect, or emotion, that was associated with them. Perceptions evoking very little attention or interest became memories with virtually no cathexis. Experiences or perceptions laden with emotion, on the other hand, held vast amounts of psychical energy. When these latter were repressed in memory, they represented a huge store of energy in the unconscious. But there was a regulatory principle involved, one that was "built upon the plan of a reflex apparatus" (Freud 1953 V:598). This principle stated that too great an accumulation of psychic energy was experienced as pain. So it needed to be released. Dreams served exactly this purpose: they provided a "safety valve" whereby the energy accumulated in repressed memories was released to relieve this built-up pressure.[7] At the same time, dream-images did not cause the sleeper any further pain, because they masked the true nature of unconscious desire.

Variations of Freud's concept of psychical energy abound in many nineteenth-century discussions of reflex. Freud had, after all, studied under Theodor Meynert, one of the most important reflex theorists of the day, at his Institute of Cerebral Anatomy at the Vienna Medical School (Ritvo 1990; Shorter 1992). Charles-Édouard Brown-Séquard, who succeeded Claude Bernard in the Chair of Experimental Medicine at the *Collège de France* in 1878, was also spreading the concept of reflex theory among neurologists during the 1880s. We have already seen one application of the energy concept to the study of dreams in Delboeuf's "fixation of force." Another can be found in a paper on dreams by Yves Delage, whom Freud cited approvingly in *Interpretation of Dreams*. Delage (1854-1920) had started out as a zoologist, completing a thesis on the circulatory system of marine crustaceans in 1881 (Persell 1999: 59-100). He soon obtained a post at the Sorbonne, and became Professor of Zoology there, while continuing to conduct research at the Roscoff biological station in Brittany, where he also

[7]As early as 1899, Freud argued that the question of repression was particular acute in the case of memories: "We learn that repression, which originally served a purpose but nevertheless ends in a harmful loss of inhibition and inner control [in the case of the neuroses], is more easily brought to bear on memories than on perceptions, because they cannot receive any additional energy-charge from the excitation of the psychical energy system" (Freud 1999:408).

dabbled in charity medical practise. Like many French scientists of his day, including Alfred Binet and Charles Richet, Delage longed to make his work relevant beyond the strictures of the laboratory sciences. He published several novels under a pseudonym, and tried his hand at poetry and philosophy as well.

Delage experienced a crisis in his thinking around 1886. A historian of French biology, Stuart Persell, argues that Delage was "torn between the ideology of a vigorous and detailed experimentalism and the need to expand into the realm of general theorizing" (Persell 1999: 63). Delage began to use evolutionary concepts to expand the relevance of his biological research. In 1895, he founded a journal—*L'Année biologique*—to deal with just such questions of "general biology."

Persell's marvellous description of the development of evolutionary biology in late-century France overlooks one of Delage's earlier attempts to bring biological ideas to bear on more general questions. In 1891, Delage published a paper on dreams in Richet's journal, the *Revue scientifique* (Delage 1891). In the piece, Delage proposed that dreams were the nocturnal release of a build-up of sensory impressions that were received, but largely ignored, during the day. Recurring nightmares or anxiety dreams could be eliminated, thought Delage, by paying close attention to every last horrific detail of the dream, which would fix them in conscious memory, and make them impervious to the dream process. Delage's idea that unattended perceptions made up the bulk of the dream was welcomed by Freud, who argued that the dream mechanism took most of its images from the same storehouse. He also approved of Delage's idea that the mind maintained its equilibrium by discharging energy through dreaming, a theme to which Freud returned over and over again:

> It begins to dawn on us that it actually *is* more expedient and economical to allow the unconscious wish to take its course, to leave the path to regression open to it so that it can construct a dream, and then to bind the dream and dispose of it with a small expenditure of preconscious work—rather than to continue keeping a tight rein on the unconscious throughout the whole period of sleep. It was indeed to be expected that dreaming, even though it may originally have been a process without a useful purpose, would have procured itself some function in the interplay

> of mental forces. And we can now see what that function is. Dreaming has taken on the task of bringing back under control of the preconscious the excitation in the *Ucs.* which has been left free; in so doing, it discharges the *Ucs.* excitation, serves it as a safety valve and at the same time preserves the sleep of the preconscious in return for a small expenditure of waking activity (Freud 1953 V:578-579. Italics original).

But Delage did not account for the phenomena of free association that Freud had observed in his study of hysterics. The "talking cure" brought them back to past events that were filled with emotion—not the unattended perceptions of waking life. To explain this, Freud turned to another biological concept, the notion of recapitulation, which he described in terms of regression.

Freud's application of Ernest Haeckel's "biogenetic" law, which dictated that "ontogeny recapitulates phylogeny," has been discussed in considerable detail by others (Gould 1977; Ritvo 1990; Kitcher 1992). In the wake of the defection of his star pupil, Carl Jung, from his close circle of disciples around 1912, Freud was obliged to begin to articulate a metapsychology in opposition to Jung's theories (Ferguson 1996:134-137). He used the notion of recapitulation (as did Jung) to lend biological authority to claims that the normal psychological development of the individual was nothing less than a progression, on a microcosmic scale, through the entire history of human civilization. But where Jung offered an interpretation of recapitulation in the tradition of German Romanticism, interpreting the symbols of dreams as archetypes "lodged in the soul," Freud tried to fit his ideas within a generalized biology. Freud went so far as to declare that the individual was nothing more than "an appendage to his germ-plasm" (as quoted in Ferguson 1996:137).

Like many neurologists, Freud's first serious introduction to evolutionary theory came through the work of the British neurologist, Hughlings Jackson. Jackson had divided the brain into "integrative levels" that represented evolutionary stages in the human nervous system, from the most recent—the cerebral cortex, which controlled language and thought—to the most ancient cerebral formation—the brain stem, which regulated vegetative activities. Neurological disease was described as a process of "disintegration," in which damage to a higher, more complex system allowed the more primitive phenomena of behaviour to emerge, uninhibited.

Freud was enthusiastic about this convergence of evolutionary theory and neurological diagnosis from the start. In 1891, for instance, he used the concept of disintegration to explain the word-deafness suffered by aphasics (Fullinwider 1983).

That same year saw the publication of Sigmund Exner's *Outline to a Physiological Explanation of Psychical Phenomena*. Exner and Freud had worked together in Ernst Brücke's laboratory, and Exner had also been Freud's instructor. Exner used evolutionary theory to shape social and cultural questions out of neurophysiological investigations (Fullinwider 1991). Like Angelo Mosso, Exner took fear as his paradigmatic example of a mental phenomenon that could be explained in physiological terms. He argued that the fear of wild animals was an example of how emotional reactions, which had once been learned, could be inherited. But Exner went beyond an analysis of fear. He attempted to explain how the function of many emotions was not to protect the individual, but the group. The following year he published an essay entitled "Morality as a Weapon in the Struggle for Existence," in which he argued that moral sentiment had a physiological basis that acts always in favour of the community. The feeling of duty, for example, was nothing more than the representation (of heroic action, for example) associated with a feeling (in this case, pride). The inheritance of such an association demonstrated that evolution functioned to protect the group, often at the expense of the individual, who might be encouraged to sacrifice his own life in the name of duty.[8]

Freud did not offer much in the way of teleological explanations in his book on aphasia, nor were evolutionary hypothesis strongly represented in the first edition of *Interpretation of Dreams*. But when Freud began to embark on his metapsychology in the seventh chapter of the second edition (1909) of *Interpretation of Dreams*, he added the idea that the sleep of the

[8]Fullinwider 1991: 37-38. Oedipus's fate in *Oedipus Rex* was one of Exner's examples of how the Ancients, who punished Oedipus despite the fact that he didn't know what he was doing was wrong, were more in line with nature than the Moderns, who judged morality on the basis of the individual's motives. Freud did not *invert* this argument so much as he *extended* it, by describing the "disastrous" consequences of modern morality (especially concerning sexuality) as a natural progression of the work of civilization (Ferguson 1996).

preconscious, which has exhausted itself by performing its various functions of generating motor action, movement, attention and thought during the day, needed to be protected. "Protection" naturally invokes the idea of "defense"—a term that Freud had been struggling with since 1894, when he began to approach the psychoneuroses from the perspective of *Abwehr* (defense).[9] The biological need to protect sleep fit perfectly into his scheme of how dreaming helped to maintain the equilibrium of psychical energy. The concept of organismic defense, championed by evolutionary theorists across Europe, allowed Freud to offer a functional account of dreaming in neurophysiological, psychological, and physiological terms.

The proposal that dreams represented the fulfilment of a wish was by no means new. Freud acknowledged that at least six other authors had suggested such an interpretation (Sulloway 1979: 330). But the idea that *every* dream could be understood in such terms was entirely novel. And, as I have tried to demonstrate, Freud grounded his generalization by framing dreaming in functional terms. In neurophysiological terms, dreams defended the continuity of sleep that was necessary for the proper functioning of the nervous system. In evolutionary terms, dreams represented the residuum of earlier developmental periods, both of the individual, and of the species. They were fragments of hidden and illicit desire that harnessed the energy of memory-traces to break through to consciousness. Even anxiety dreams, the great counterargument to Freud's theory, could be incorporated. The disturbance of sleep by such a dream, claimed Freud, simply announced the body's attempt to continue to function normally in the wake of disease:

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[9]Ellenberger 1970: 486. Sulloway (1976: 123-130) speculates that the missing third notebook of the *Project* that Freud withheld from Fleiss, entitled "The Psychopathology of Repression," must have held been an analysis of repression and defense in evolutionary terms. At any rate, Freud was in no way alone in directing his attention to the analysis of neurophysiological phenomena in terms of defense around 1894. The most famous physiologist in France, Charles Richet, was lecturing at the *Collège de France* on organismic defense as well (Richet 1893).

> This [the disturbance of sleep by an anxiety dream] is not the only instance in the organism of a contrivance which is normally useful becoming useless and disturbing as soon as the conditions that give rise to it are somewhat modified; and the disturbance at least serves the new purpose of drawing attention to the modification and of setting the organism's regulative machinery in motion against it (Freud 1953 V:580).

What other "instances" Freud had in mind is unclear, as he does not cite any examples. But he would have had one very close to hand. In 1905, Charles Richet's 1902 discovery of anaphylactic shock had been converted into a clinical problem by two Viennese pediatricians, Clemens von Pirquet (1874-1929) and Béla Schick (b. 1877). Anaphylaxis, a condition of acute and even fatal sensitivity to injected proteins, represented the dark side of the growing program of employing vaccination and serum therapy against infectious diseases. Certain cases of it—particularly those that ended in the death of the patient—were well-publicized in European and American newspapers. In their study of the negative reactions many children had to anti-diphtheria serum, Pirquet and Schick coined the term "allergy" (from the Greek *allos*, other + *energia*, work, activity, energy), to describe an organism's state of "altered reactivity." "Allergy," like "anaphylaxis," was a term that tried to account for the confusing fact that the body's immune defenses could, under certain conditions, create pathological symptoms, rather than protection against disease.[10] Evolutionary explanations seemed to provide a solution, at least for some biological theorists. Charles Richet eventually settled on an argument that had the immune system working some of the time to preserve the individual, but, in the case of anaphylactic shock, it killed the individual in the interests of "preserving the chemical integrity of the species" from the kind of permanent alteration that was included under the rubric of "allergy" (Richet 1910).

Freud's approach was also innovative in his refusal to grant external stimulus an important role in the formation of dreams. He insisted that dreams were primarily, if not exclusively, *memories*:

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[10]See Clemens F. von Pirquet & Béla Schick, *Serum Sickness*, translation by B. Schick (Williams & Wilkins: Baltimore, 1951). On allergy and anaphylactic shock, see Canguilhem (1989), Silverstein (1989), Moulin (1991), and Kroker (1999).

> ...in my opinion, somatic sources of stimulation during sleep (that is to say, sensations during sleep), unless they are of unusual intensity, play a similar part in the formation of dreams to that played by recent but indifferent impressions left over from the previous day. I believe, that is, that they are brought in to help in the formation of a dream if they fit in appropriately with the ideational content derived from the dream's psychical sources, but otherwise not. They are treated like some cheap material always ready to hand, which is employed whenever it is needed, in contrast to a precious material which itself prescribes the way in which it shall be employed (Freud, *Interpretation of Dreams* IV: 237).

Freud was not, as we have seen, the only biological theorist to conceive of dreams in terms of memory. But his program was unique. In his quest to trace all dream elements back to associated memories, Freud constructed an entire theoretical and therapeutic structure out of interpreting individual dream reports. This was in sharp contrast to the approach of Freud's contemporary, Henri Bergson, who took the form of the dream, rather than its content, as its most instructive element.

Henri Bergson

Marie Cariou, a philosopher, has offered a detailed comparison of the ideas of Sigmund Freud and Henri Bergson (Cariou 1990). In the wake of the dominant position psychoanalysis has assumed in French intellectual life, Cariou has attempted to rehabilitate Bergson's reputation as a philosopher of mind. She has argued that Bergson had more than a passing familiarity with Freud's work, and that, in fact, they shared many of the same interests: the phenomenon of aphasia, the psychological role of forgetfulness, and the analysis of humour, to take but three examples. But Bergson demonstrated a remarkable and even willful ignorance of Freud's work in his 1901 essay on dreaming. The first version of this paper made only a passing reference to Freud, which is perhaps not so surprising, as Freud's book had not yet been translated, and there was not a great deal of widespread interest in his work at this point. But when Bergson revised the entire paper in preparation for the publication of *L'Energie spirituelle* in 1919, he merely added a footnote stating that the tendency to dream about insignificant events of the preceding day was equivalent to Freud's arguments about "repression."[11] This is hardly the work of someone who had read and admired Freud. Instead of making Bergson out to be a crypto-psychoanalyst, I will use Bergson's work to set Freud's ideas in a particular historical context that revolved around the question of whether or not dreaming served a biological purpose.

When Carl Jung introduced Freud's theory of dreams to a French audience through the pages of Binet's *L'Année psychologique* in 1909, Henri Bergson was undoubtedly the most famous living philosopher in France (Jung 1909). In the wake of his *Creative Evolution* (1907), Bergson was arguably the most influential intellectual figure in Europe. In France, Britain, and the United States, "Bergsonism" was celebrated in political, religious, artistic, literary and philosophic circles as having liberated subjectivity from the strictures of nineteenth-century scientific materialism (Gunter 1982; Grogin 1988; Antliff 1993). Among biological and psychological scientists, his work was typically received with caution, and even suspicion. But he could not be ignored, and numerous scholars tried to either refute his arguments or demonstrate that they were beyond the pale of reason (Le Dantec 1907; Russell 1971).

The prominence of psychoanalytic thought throughout much of the twentieth century makes it all too easy to overlook Bergson's importance in discussions of modernity, particularly when it comes to examining theories of dreaming. Ferguson, for example, mentions him only in passing, despite his expressed interest in setting Freud within a modern "culture of crisis" in the years leading up to the Great War (Ferguson 1996). An important work on the evolutionary controversy in France ignores Bergson altogether, despite the fact that it was *Creative Evolution* that set the tone of this debate in the public arena (Persell 1999). Other studies have been more circumspect, arguing for Bergson's influential role in the political, religious, and scientific debates of his age (Grogin 1988; Rabinbach 1990; Braun 1992; Burwick & Douglass 1992; Antliff 1993). The significance of Bergson for a history of sleep and dreaming is that he used dreaming as a phenomenological basis for his attack on the science of psychology. Like Freud,

[11]The extent of Bergson's revisions can be seen in Bergson (1972), pp. 443-463.

Bergson reified the dream as the crucible in which any scientific explanation of sleep would pass or fail. But where Freud pursued a method of dream interpretation, Bergson merely wanted to grasp the dream's form. And while Freud used dreams as a platform upon which he could develop a scientifically-sound metapsychology, Bergson took dreams as an instantiation of the very limits of scientific knowledge.

Dreams, according to Bergson

Henri Bergson (1859-1941) started to incorporate the problem of dreams into his philosophy after graduating from the *École normale* in 1881. R.C. Grogin, a historian of philosophy, has argued that Bergson took an active interest in hypnotism around 1883, while he was teaching at the *École normale*. In 1886, Bergson conducted a rather famous experiment that involved two boys who had been credited with impressive telepathic powers. Bergson demonstrated that the boys had actually experienced an increased sensitivity under hypnotic suggestion (Grogin 1988:22). Neither a neurologist nor an experimental psychologist, Bergson was nonetheless entranced by those same problems of mind exhibited by the patients and subjects of Joseph Delboeuf, Théodule Ribot, Jean-Martin Charcot, and Pierre Janet (Ellenberger 1970:354-355). While his penchant for, and his skill in, metaphysics made him stand out in such a crowd, his curiosity about these margins of human experience were perfectly run-of-the-mill in the 1880s.

What made *Time and Free Will*, his first major publication, unusual was Bergson's devotion to classical introspectionist philosophy. The book, which first appeared in 1889, was an extended polemic against experimental psychology in general, and psychophysics in particular. Psychophysics, which was based on the pioneering work of Gustav Theodor Fechner (1801-1887), tried to obtain law-like relationships between stimulus and sensation using the method of "thresholds" (Boring 1957). Experimenters varied the intensity of a stimulus (a light or colour, the distance between two points of a compass applied to the skin, or a weight held in the hand)

and then asked their subjects to judge the minimum amount of change they could perceive in the stimulus. Results were taken from a number of subjects, and the average threshold was used to calculate laws of sensation as a function of stimulus intensity.

Bergson claimed that this entire program was in error, because it confused magnitude, a measurable quantity, with intensity, an experienced quality (Bergson 1916; Lacey 1989). Bergson's critique involved a detailed analysis of what it meant to "feel" (he includes, among others, joy, sorrow, fear, pain, and muscular effort in his analysis). He then asked whether such feelings were capable of having a measurable minimum difference. They were not, he argued, because, in order to have such a property, "some identical residuum" had to remain "after the elimination of their qualitative difference" (Bergson 1916:64). Fechner, noting that sensation increased by sudden jumps while stimulus increased continuously, argued that this "residuum" was the minimum difference itself—the *minima*. But, Bergson argued, it was impossible to determine that any two of these *minima*, which were themselves sensations, were equal, even though this premise was built into every psychophysical law.

This error of psychophysics was merely reflective of a pragmatic and scientific way of representing experience, which Bergson characterized as "representing time as space" (Bergson 1916:181ff). Time, he argued, was a quality perceived directly by the soul as "real duration." It mediated the intensity of sensations. Space, on the other hand, was pure quantity. Its essence was objectification. It enabled subjects to arrange the world as a collection of things that stood in relationship to each other. Associationist psychology taught that these "things" included ideas that followed each other in succession. According to Bergson, if I rise to open the window, and suddenly forget what I wanted to do after standing up, the associationist would say that of the two ideas (an end to be attained and a movement to achieve this end), only the movement remained (Bergson 1916:160ff). But Bergson argued that the feeling of intention remained as well. The idea of the end has "tinged with a certain colouring the mental image of the intended movement." The two ideas were not separable, as associationism suggested. They were bound up together as a purposeful act.

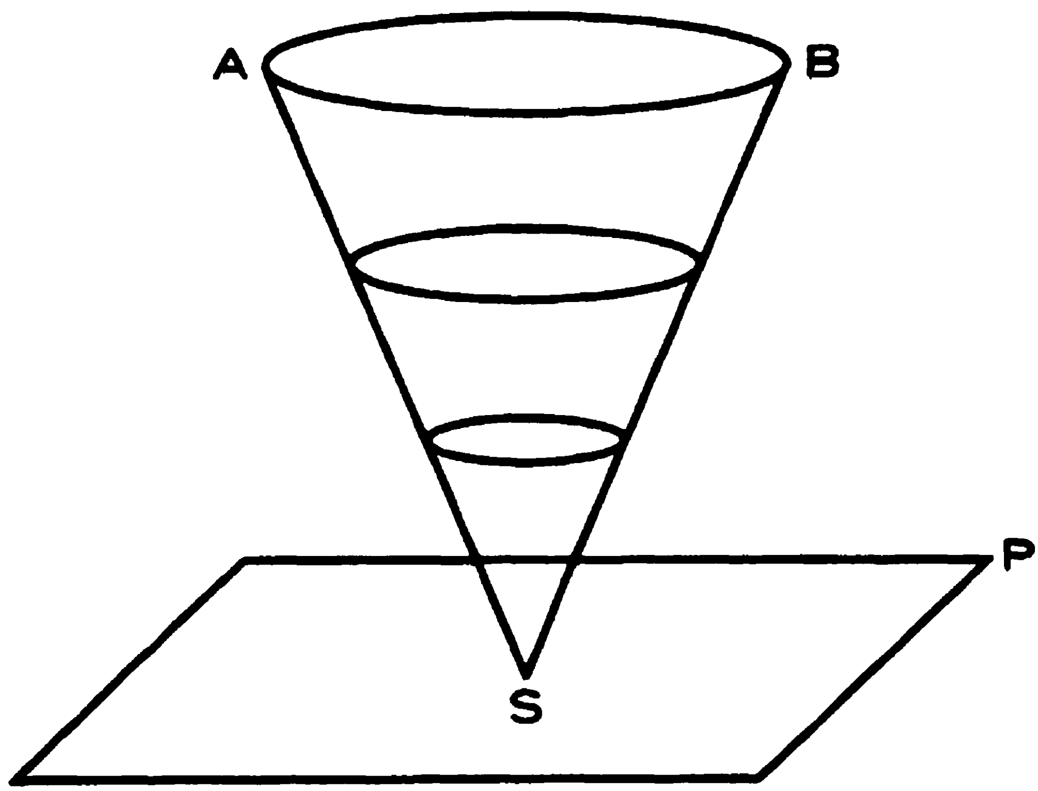
In *Time and Free Will*, dreaming was one of the introspective examples Bergson used to demonstrate that sensations were experienced in terms of intensity, not magnitude. Evocative dreams were like passionate feelings, in that they "coloured" all aspects of memory (Bergson 1916: 8-9). They were closely related to the physiological state of sleep, which "relaxes the play of the organic function" (Bergson 1916: 126-127). This relaxation engendered confusing and overlapping dream images, which were a primitive experience of real duration, in contrast to the rigid, homogenous and successive instants of time experienced during wakefulness. The sleeping mind, cut off from external sensation, was forced rely on memory and "parodied" its normal activity by superimposing nonsensical images (Bergson 1916: 136-137).

None of this amounted to an analysis of dreaming comparable to Freud's *Interpretation of Dreams*, or even Delboeuf's *Le sommeil et les rêves*, which appeared four years before *Time and Free Will*. But many of Bergson's arguments about dreaming were beginning to take shape already in 1889: dreams refuted associationism; dreaming illustrated real duration; and dream-images came primarily from memory.

These ideas were substantially expanded in *Matter and Memory*, Bergson's 1896 critique of neurophysiology and the doctrine of brain localization. Did memories reside in a particular place in the brain? Once again, Bergson tried to undermine a scientific project by arguing that the question it was based upon was absurd. The phenomena of memory and the sensori-motor responses that neurophysiologists and neurologists relied upon in their experiments were actually two radically different, but related, poles of human existence. Their relationship was mediated by the necessity of engaging the world through activity. Bergson visualized the structure of memory as "an inverted pyramid," an image he would use throughout much of his later work [Figure I]. At the top of the figure (the base of the pyramid), mental states were made up of an "expansive memory" filled with images, and completely removed from the world of sensation and activity (Bergson 1988: 133-177). This was the "plane of the dream." At the bottom of the figure, the pyramid of memory was focussed to a point, representing how memory became enrolled in the service of perception, sensation, and muscular response. This was the "plane of action."

Figure I
Bergson's "pyramid of memory"

AB represents memory in the "plane of the dream"
S represents memory focussed on the "plane of action" (P)
(Bergson 1988)



Bergson thus contrasted dreaming to what he called "attention to life." Memory played a dramatically different role in each plane. In the dream, memory supplied contemplative images divorced from all movement. These images were thus divorced from measurable time. "A human being," declared Bergson, "who should *dream* his life instead of living it would no doubt thus keep before his eyes at each moment the infinite multitude of the details of his past history" (Bergson 1988:155). The plane of action, on the other hand, featured the body dictating the use of memory in turning sensation into perception. Contiguity (the ability to perceive the continuity of phenomena over time) and similarity (the ability to recognize an object as being "like" another) relied upon memory, but without it being recognized as such. Choice and action were inconceivable without the body. "Our body," Bergson wrote, "with the sensations which it receives on the one hand and the movements which it is capable of executing on the other hand, is then, that which fixes our mind, and gives it ballast and poise" (Bergson 1988:173).

Attention was the result of the sensori-motor system focussing memory. Sleep represented a relaxing of this tension, thus engendering the random memory-images of dreams. Bergson appealed to the theory of sleep advanced by the Spanish neuroanatomist Santiago Ramón y Cajal. This theory, also known as the "amoeboid theory of sleep," argued that the synapse—the tiny distance between one neuron's axon and another's dendrite—grew larger during sleep, which accounted for the diminished sensori-motor activity of this state (Hobson 1988:90-99). Bergson recognized that this theory was controversial, but he was nevertheless fascinated by the idea that the nervous system might consist of "everywhere conducting lines, nowhere any centers." It suggested that sleep could be defined as "a functional break in the relation established in the nervous system between stimulation and motor reaction." He then combined this theory with two well-worn ideas: that sleep was caused by fatigue; and that dreams were akin to insanity. The result, he thought, confirmed his opposition between dreams and action. I quote Bergson at length only to demonstrate how tightly wound these associations were:

> ...it appears more and more probable that this relaxing of tension in the nervous system [sleep] is due to the poisoning of its elements by products of their normal activity accumulated in the waking state. Now, in every way, dreams imitate insanity. Not only are all the psychological symptoms of madness found in dreams—to such a degree that the comparison of the two states has become a commonplace—but insanity appears also to have its origin in an exhaustion of the brain, which is caused, like normal fatigue, by the accumulation of certain specific poisons in the elements of the nervous system. We know that insanity is often a sequel to infectious diseases, and that, moreover, it is possible to reproduce experimentally, by toxic drugs, all the phenomena of madness. Is it not likely, therefore, that the loss of mental equilibrium in the insane is simply the result of a disturbance of the sensori-motor relations established in the organism?...If our analyses are correct, the concrete feeling that we have of present reality consists, in fact, of our consciousness of the actual movements whereby our organism is naturally responding to stimulation; so that where the connecting links between sensations and movements are slackened or tangled, the sense of the real grows weaker, or disappears (Bergson 1988:174-175).

Cariou has attempted to defend Bergson against the charge that he ignored psychoanalysis. But given Bergson's interest in dreaming during the late 1890s, as well as his enormous popularity in France and abroad, we might just as well ask why it is that Freud says nothing about Bergson. Kitcher has argued that Freud's theories about the mind were not "pseudoscientific," because they rested upon a number of theories, drawn from several disciplines, which were acceptable science in Freud's day. From psychology, Freud borrowed ideas about "associationism, developmental psychology, reflexology, evolutionary and functionally distinct faculties, and the project of tracing human mentality and civilization to its prehistoric origins" (Kitcher 1992:186). Yet Bergson was engaged in extensive critiques of many of these ideas between 1895 and 1907. Bergson's work also seems to have been well-known to Freud's former disciple, Carl Jung (Gunter 1982). It is beyond the scope of the present study to examine the reasons why Freud might have ignored Bergson. My point is simply that around the turn of the century, Freud's was not the only, or even the most prominent, theory to use an analysis of dreaming as the basis of a critique of modern culture.

Dreaming and metapsychics

To this point (1896), Bergson had made substantial use of dreaming as an introspective vehicle that could conveniently convey his ideas to his reader. Regardless of whether his readers had any experience or knowledge of madness, hypnotism, or neurophysiology, they doubtlessly knew something of dreaming. But, although he had incorporated it into his philosophy, Bergson had yet to make a thorough analysis of dreaming.

His analysis came in the wake of a growing interest in occult phenomena, not just on Bergson's part, but within much of Western Europe. Bergson made his first formal and public commitment to psychical research by joining the *Institut psychologique international* and the *Institut générale psychologique* in 1900 (Grogin 1988: 51). Of course, the institutionalized study of ectoplasmic apparitions, table-turning, telepathy, mediums and mind-reading considerably predates Bergson's entry into the field. In Great Britain, the Society for Psychical Research (SPR) had been formed in 1882 by a group of scientists and philosophers connected with Trinity College, Cambridge (Oppenheim 1985). Their membership included several who had been part of the many spiritualist societies that had sprouted up across Britain since the 1850s. The question of survival—not that of the species, but of the soul after death—had loomed large in the SPR's research agenda since its inception (Cerullo 1982; Haynes 1982). One historian has suggested that it was only during the 1920s that "psychic researchers began to turn their attention to the more respectable pursuits—from the point of view of materialist science—of laboratory measurement of phenomena that did not have so direct a bearing on man's conception of his status in the universe" (Webb 1971: 19-20).

In France, the systematic study of occult phenomenon arrived much later. Charles Richet, the weigh-station of physiological knowledge in France, helped create the *Institut psychologique international* in 1900 (Wolf 1993). Aside from Bergson and Richet, its membership also included Pierre Janet, Cesare Lombroso, and William James (Grogin 1988: 51). Richet had considerable experience in the study of parapsychological phenomenon, and was already beginning to develop ideas about how to create a discipline which he later dubbed *métapsychique* (Richet 1922). Richet's ready willingness to believe in the reality of most parapsychological phenomena was pronounced. But, as an atheist and a positivist, his interest was in devising experimental methods to demonstrate that such phenomena could be explained without recourse to spiritualism. His study of telepathy, for example, was one of the first to employ randomization in experiment (Hacking 1988). Richet was a founding or active member of several parapsychological organizations, in and outside of France (Wolf 1993). He wrote widely on the subject, and his outspoken anti-spiritualism, coupled with his profound belief in the reality of psychical phenomena, obliged anyone in France with an interest in such questions to take a position for or against Richet.

Bergson, who was a close friend of Richet, participated in Richet's enthusiasm for the experimental demonstration of occult phenomena. Bergson and Marey were both members of a special group of the *Institut général psychologique* commissioned to study psychical phenomena.[12] In fact, at one séance that Bergson attended, an examination of Eusapia Palladino, a Neapolitan medium famous for her telekinetic powers, a recording device was apparently put to use. The description of the séance is brief, but its results—the destruction of the recording instrument in the middle of the experimental session—seems to strangely reflect Bergson's own ambivalence about Marey's research:

> *Séance* 1905-V-10-11. Rupture of a rubber tube connecting the Marey balance to a recording tambour, just as Eusapia broke a red pencil. (Controllers: on the left, M. Chapentier; on the right, M. Bergson).[13]

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[12]The *Institut général psychologique* featured Liébeault and Madame Curie as members, as well as the physiologists Arsene D'Arsonval and Émile Duclaux (Grogin 1988: 51; Bergson 1972: 509-510).

[13]Bergson, *Mélanges*, p. 673. Eusapia was famous for her ability to move objects at a distance (telekinesis). She was also a self-confessed cheat, taking every opportunity to hoodwink her interlocutors. Hence the "controllers" who held her on each side. See Harry Price, *Fifty Years of Psychical Research* (Arno Press: New York, 1975), pp. 74-76.

Bergson, however, was not an experimentalist by trade. He was a philosopher and thus relied more on logical and linguistic analysis than empirical demonstration. Dreams were a natural point of entry for Bergson, who was curious about the occult, but was also convinced of the value of introspection.

Bergson's interest in dreams was brought to a focus at the *Institut*. He was asked to compose an address on this topic, and he presented it before a crowd of more than five hundred, on March 26th, 1901.[14] He began by adopting a rather different perspective on the origins of dreams. Citing Hervé de Saint-Denis, Alfred Maury, Philippe Tissié, and the American psychologist George Trumbull Ladd, Bergson contradicted what he had said in *Matter and Memory*, and argued that all dreams originated in sensation. Bergson had hesitated on the question of associationism before, and his description of dreaming was no exception (Lacey 1989:1-16). Dream images were "fabricated" out of vague sensations, and given distinct form by the images of memory (Bergson 1920: 113). This process was, in fact, not substantially different from that of normal perception, which invoked the clarity of memory to fill in the vagueness of sensation.

Bergson entirely abandoned the close-knit relationship between dreaming and madness that he had expressed five years earlier in *Matter and Memory*. The psychology of sleep took on a new significance in Bergson's analysis: "I am not concerned, of course, with its physiological conditions. This is the business of physiologists; it is far from being settled" (Bergson 1920: 122). To understand the nature of the mind in sleep, Bergson argued that one must reconstruct its activities just upon awakening, precisely the method that Ladd had taken earlier (Ladd 1892). He described a dream—so self-referential it seemed contrived—of standing at a podium, addressing

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[14]Bergson's essay first appeared in the *Bulletin de l'Institut génerale psychologique* in May of 1901, and then received a wider audience in Richet's popular *Revue Scientifique* (Bergson 1901). English translations of the essay eliminate the introductory sentence, in which Bergson indicates that he was asked to speak on this particular topic (Bergson 1914, 1920). A synopsis of the lecture also appeared in *Revue de Philosophie* 1 (1901), pp. 486-488.

an assembly. In his dream, a noise arises from the back of the room. It grows louder and louder until the auditorium fills with the rhythmical cry of "Out! Out!" He awakens to find a dog barking in his neighbour's garden (Bergson 1920:123-124).[15]

Now Bergson launched into his analysis, which took the form of a waking consciousness interrogating a dream-consciousness. The difference between the two was the amount of energy the former expended in making perceptions—the "perfect fit" between sensation and memory. This energy, which Bergson characterized as the attention that held back the floodgates of memory, began to lag over the course of the day. Bergson's dream-consciousness was supposed to have been addressing his waking consciousness, but he might as well have been speaking to his colleague at the *Institut*, the "common sense" philosopher, William James:

> Your life in the waking state is, then, a life of toil, even when you suppose you are doing nothing, for at every moment you must choose and at every moment you have to exclude...You choose among your memories, since you reject every recollection which does not mould itself on your present state. This choice which you are continually accomplishing, this adaptation ceaselessly renewed, is the essential condition of what you call common sense...But it fatigues you in the long run. Common sense is very fatiguing (Bergson 1920:125).

The dream-consciousness, on the other hand, "did nothing," because it was completely detached from waking life. *Dormir c'est se désintéresser* (to sleep is to be disinterested) quipped Bergson—a phrase, he noted in later editions of this essay, that had been picked up by other theorists, including Edouard Claparède, who will be discussed in chapter three (Bergson 1920: 126).

The idea that sleep was identical to a withdrawal from life explained a number of features of the dream. The instability of its images indicated that the strength of association was exactly

[15]Naturally, the dog outside Bergson's window spoke French. Its yelps of "Ouâ, ouâ" appeared in Bergson's dream as "A la porte! à la porte!" (Bergson 1901:711). Fortunately, this example made for a felicitous translation into English, where "arf! arf!" became "out! out!" (Bergson 1914). Incidentally, Bergson seems to have preferred the company of his two cats over that of any dog (Slosson 1968).

corelated to that of the will. Without interest, associations between ideas were little more than random. The rapidity with which the dream unfurled—Bergson turned to Maury as evidence of this—also indicated a radically diminished engagement with the "homogenous" time of the world. Finally, Bergson adopted Delage's theory that the dream consciousness had a marked preference for the insignificant, unnoticed details of the past day. If dreams were about events of grave concern, this sleep tended to be very fatiguing. Normal sleep, on the other hand, typically featured images that had passed through waking consciousness and aroused little or no attention.[16]

Like many of his contemporaries during the first years of the twentieth century Bergson began to take a deep interest in applying evolutionary questions to questions about the mind. But unlike Freud, Bergson never seriously applied questions of development to dreaming. Where Freud continually invoked ideas of recapitulation and regression in his later writings on dreams, Bergson made only incidental references to dreams in *Creative Evolution* (1907), and offered no new analysis of the topic (Bergson 1911). Why this great absence in the midst of a growing psychoanalytic frenzy? For Bergson, dreams could play no role in evolutionary theories of any stripe, simply because *dreams served no function*. His critique of associationism—a position which, along with his theory of sleep, was quickly picked up by Claparède and Piéron—also made it impossible for him to accept the psychoanalytic method of free association. Dream images were random selections taken from memory. They were useful insofar as they could help the introspective philosopher illustrate the nature of time and memory, but they served no physiological function. As a consequence, dreams could have no place in evolution or any significance for functionalist psychology. They were the marginalia of human existence, valuable only for their ability to fix the modern and scientific vision of time against a backdrop of real duration. For Bergson, dreams represented the limit of scientific explanation.

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[16]Bergson 1901, p. 713. His 1919 revision and its 1920 translation eliminated the claim that dreaming of the day's concerns caused fatigue.

Curiously enough, it was Bergson's ideas, and not Freud's, that made a difference to the emerging physiology of sleep, as we shall see in the next chapter. Bergson's lecture on dreams also received international attention long after it was first published in 1901. When he visited New York to lecture at Columbia University in February of 1911, hundreds were turned away from the two-thousand seat lecture hall because it was already overflowing with people.[17] His visit was reputed to have caused what was perhaps "the first traffic jam of the brand-new automotive age" (Jonçich 1968: 334). The *New York Times* carried no fewer than thirteen feature articles and editorials on Bergson, particularly after the Pope denounced his philosophy in August, 1913.[18] Edwin Emery Slosson, a chemistry professor and science popularizer, translated and published Bergson's essay on dreams in his daily, *The Independent*, two months later.[19] The *Times* took a rather sceptical position towards Bergson, fearing American readers might be taken in by Bergson's continental charm. But Slosson argued that the importance of Bergson's work far exceeded that of Freud.[20] The question of dreams was in vogue again, argued Slosson, who had toured Europe and interviewed Bergson in 1910, and "the cause of this revival of interest is the new point of view brought forward by Professor Bergson."[21]

Slosson set Bergson beside Maurice Maeterlinck, Henri Poincaré, Élie Metchnikoff, Wilhelm Ostwald and Ernst Haeckel as one of the most important "living prophets."[22] Slosson, a devout Congregationalist, was at pains to show that Bergson did not in any way reject the

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[17]*New York Times* (February 5, 1913).

[18]"Pope Denounces Bergson," *New York Times* (August 28, 1913).

[19]*The Independent* (Oct. 23 & 24, 1913). Slosson, who dubbed Bergson "a modern prophet," published the lecture as a monograph the following year (Bergson 1914).

[20]On Bergson as a know-nothing about science or politics, see "The Philosophy of Henri Bergson and syndicalism" (*New York Times*, January 26, 1911, p. 4), and editorial, *New York Times*, August 29, 1913, p. 8.

[21]Slosson, introduction to Bergson 1914, p. 6.

[22]Slosson 1968. This book originally appeared in 1914.

validity and importance of scientific knowledge. On the contrary, he portrayed Bergson as a spiritualist for a scientific age. Bergson's approach to dreaming avoided the "excesses" of psychoanalysis, and also appealed to the "pragmatic genius" of the American people.[23] At the same time, Slosson noted that there was room in Bergson's philosophy for the survival of the soul after death. Bergson himself had indicated this in his Presidential address to the SPR in London in 1913 (Bergson 1920).

After the English translation of *Creative Evolution* appeared in 1911, Bergson was reputedly selling more books in Britain than he was in his native France.[24] Whether his ideas were accepted or not was another matter. Charles Sherrington (1857-1952), the most important neurophysiologist in Britain at the time, offered the following assessment in a letter written to his former student, Alexander Forbes:

> As to that book on Bergson I thought the introduction by Ray Lankester so good that you might enjoy looking at it. But the actual text itself was much inferior to the introduction. What I have read of Bergson I find rather nebulous—the problem of the mind & matter nexus seems so insoluble that it seems for our epoch unattractive. One thing appears clear I think, viz. mind is not an epiphenomenon & no science—least of all physiology of nervous system—should treat it as such.[25]

Fifteen years later, Bergson's theory of dreams would surface again in Britain, this time in the form of John W. Dunne's famous analysis of dreams as prophecy, *An Experiment with Time* (1927). Regardless of whether they were accepted as inspired truth, or rejected as anti-scientific

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[23]Slosson, introduction to Bergson 1914, p. 7.

[24]Slosson, introduction to Bergson 1914.

[25]Letter, Sherrington to Forbes, September 14th, 1912 (AFA 15.2.726). Sherrington is undoubtedly referring to E. Ray Lankester's introduction to Elliot (1912), which was a point-blank rejection of the ideas expressed in *Creative Evolution*. Elliot, a former military man, wrote popular tracts on evolutionary theory, producing a biography of Herbert Spencer and a translation of Lamarck's *Philosophie Zoologique*. Lankester, on the other hand, was a serious morphologist and evolutionary theorist, who has only recently surfaced to historical attention (Lester 1995).

rubbish, Bergson's ideas persisted in the popular and professional imaginations as an attempt to limit the boundaries of science through metaphysics.

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Shorn of its affinities with madness, Bergson's vision of dreaming illustrated the need for a psychology of sleep. Indeed, it was Henri Piéron, a young psychologist, who pursued the *physiological* research necessary to assess the value of Bergson's depiction of sleep as a disinterested state, as I will discuss in chapter three. Freud's influence on the physiological study of sleep actually arrived much later, once psychoanalysis began to gain influence in the United States during the late 1920s and 1930s. This irony—that the "anti-scientific" Bergson should shape the direction of scientific research, while Freud, the "biologist of the mind," failed to make an immediate impact—has been lost by historians who have failed to grasp the context in which these figures were writing. Bergson's theory appealed to the crypto-religious spirit of his age, which wanted to preserve a sense of spirituality and freedom, while at the same time pursuing a pervasive analysis of the mind as a pragmatic phenomenon oriented towards organismic survival. For Bergson, dreams were the soul's last stand in the midst of *La belle époque*. He drew upon the distinctive flow of their images as an exemplary form of subjectivity, an existential state that could not be grasped by a psychology that took the function of consciousness as its object.

Chapter III

The natural history of Sleep
1904-1913

It was only a few years after Freud and Bergson had framed dreaming in terms of function that sleep itself emerged in a similar light. In 1904, Edouard Claparède, a Swiss psychologist, proposed a "biological" theory of sleep. He argued that sleep was not the consequence of fatigue, but an instinctive reaction that served to protect the organism from fatigue's harmful effects. The feeling of sleepiness was an adaptive response that represented an evolutionary advantage. It was not merely a mechanical response to the build-up of "fatigue toxins."

The idea that sleep was an active function proved controversial in France, where Alfred Binet immediately associated Claparède's theory with Bergson's description of sleep as "disinterest." Binet feared that Bergson's popularity represented the return of metaphysics to a philosophy curriculum only just purged of its religious affiliations by the Separation Act of 1905. Binet's former student, Nicholas Vaschide, took up the charge against Claparède. Like Binet, Vaschide argued that there was no evidence to support Claparède's theory. It could have no place in the program of experimental psychology that Vaschide was trying to build in France.

Vaschide's collaborator, Henri Piéron, took a more moderate stance towards Claparède's ideas. Piéron, a psychologist and a dedicated social reformer, was intrigued by Bergson's juxtaposition of matter and memory. Piéron rejected this dualism, and argued instead that memory had evolved through all forms of matter, and had reached its highest stage as the scientific knowledge sustained by social order. Piéron's ideas fit well with the social, political and educational reforms that swept through France in the decade before the Great War.

Sleep, too, was a form of memory. Its periodicity, Piéron argued, was akin to that displayed by organisms that instinctively "anticipated" changes in their environment, and reacted accordingly. To separate the effects of these "anticipations" of consciousness from the fatigue toxins that mechanically triggered sleep, Piéron developed an ingenious experimental method—that of "enforced wakefulness." In his conclusion, he attempted to rewrite Claparède's theory without invoking the teleological notion of function.

The concept of function in psychology

Throughout the nineteenth century, the question of sleep had been left mainly to physiologists and neurologists. Neurologists studied sleep as a crypto-pathology; physiologists depicted sleep as the mechanical result of an exhausted body. Neither took sleep to be anything other than the passive consequence of fatigue. Sleep was not a biological object in the sense that it represented an active, organic process. It was merely the absence or diminution of such a process. Delboeuf said as much in 1885, when he rejected the idea that sleep could even have a function:"Sleep is not a function," he argued, "it is a concomitant effect...The truth is that it [sleep] shows itself when sensibility is dulled, and disappears when sensibility returns...Natural or artificial, sleep is always accompanied by an insensibility more or less extended, more or less profound. The cause of one is the cause of the other" (Delboeuf 1885:166).

Such a definition of sleep left only dreams for psychological research. Wilhelm Wundt, for example, devoted a special section of his *Principles of Physiological Psychology* (1873-4) to dreams (Wundt 1904). But dreams were simply treated as a species of hallucination. Paul Radestock, one of Wundt's disciples, published a monograph on the subject five years later (Radestock 1879). But despite its title—*Schlaf und Traum*—only one chapter out of ten offered any information about sleep. Like the Belgian philosopher Delboeuf, these early experimental psychologists took sleep to be nothing more than the physiological backdrop for dreaming. Outside of dreaming, "psychology of sleep" had no meaning. The Anglo-American heir of the Wundtian tradition of experimental psychology, Edward Bradford Titchener, narrowed the psychological study of sleep and dreams even further. Titchener's numerous textbooks hardly mentioned dreaming at all, other than to include it, along with hypnotism and insanity, as part of abnormal psychology (Titchener 1898). Dreaming and sleep were subsequently excluded from his study of the psychological structure of the normal adult mind through the technique of introspection.

This situation began to change as psychologists began to incorporate evolutionary concepts into their research. The tradition of describing psychological phenomena, such as emotion and sensation, as functions that evolved and contributed to the survival of the species can be traced back to the second edition of Herbert Spencer's *Principles of Psychology* (1870-2). Spencer argued that associations, if made frequently enough, could be inherited. But this had little to do with sleep, as Spencer thought sleep the result of nervous fatigue, not the product of an association of ideas (Spencer 1888:§37). Dreaming was also treated in terms of human evolution by early anthropologists. In *Primitive Culture* (1871), Edward Burnett Tylor identified dreaming as one of the origins of the human belief in the soul, which he dubbed "animism" (Tylor 1958:I.429). But animism, reincarnated in Tylor's time as "spiritualism," was an example of a "survival," a relic of archaic and primitive forms of thought. For Tylor, dreams furnished no evidence of an afterlife, nor did they demonstrate the existence of "spirits" whose operations could not be explained by materialistic science. Instead, Tylor treated dreams as anthropological objects. Their persistent status as manifestations of the spirit world in modern culture was nothing more than evolutionary residue.

A commitment to functional explanation—the notion that psychological phenomena should be explained in terms of their role in human survival and evolution—has traditionally been recognized as a distinctively American contribution to psychology. Edwin Boring, in his classic *History of Experimental Psychology*, argued that the United States was naturally receptive to functionalism, because it was still a new country in the late nineteenth century, filled with the "pioneers' spirit" and "the belief that usefulness is the chief good" (Boring 1957:242-244). The rapid development of psychology as an independent discipline in the U.S. is probably a more important factor than national character, however (O'Donnell 1985; Smith 1997). Psychologists defended their disciplinary identity on the basis of its practical utility in industry, education, and the military (Danziger 1990). Many psychologists, even those who were not actively engaged in applied research, would describe themselves as "functionalists." Consciousness was not an epiphenomenon, but a practical invention of evolutionary progress. The utility of consciousness reflected American psychologists' own belief in the practical value of their professional knowledge.

I will argue that functionalist thinking about *sleep* did not begin in the United States, where psychology had reached an astonishing level of professional and academic development by the first years of the twentieth century. Nor did it begin in Germany, where the ancestral home of experimental psychology—Wundt's laboratory in Leipzig—churned out a many influential investigators whose names fill the histories of psychology. It began, instead, in France, where psychological research was cut off from the laboratory and dominated by clinical experiments in hypnotism and psycho-pathology. France was undergoing enormous political and social change in the first years of the twentieth century. The Dreyfus Affair had split the nation into two camps—those who supported the idea of France as a modern, liberal republic, and those who wanted to return France to her glorious position before her defeat at the hands of the Germans in 1871. The formal separation, in 1905, of Church and State brought a series of radical reforms to the French educational system. It also sparked a conservative Catholic revival. Internally, the nation was plagued by massive strikes and widespread labour unrest. On the international scene, France appeared, after 1905, to be at risk of losing her colonial influence in North Africa to Germany.

The pathologies of the nation went hand in hand with those of the body. During the last quarter of the nineteenth century, many scientific, medical and literary observers used the biomedical concept of "degeneration" to weld together a host of social problems, including sexual perversion, criminality, suicide, alcoholism and drug addiction, and (particularly in France) a chronically low birth rate (Nye 1984). These pathologies, argued Théodule Ribot, who held the first chair in Experimental and Comparative Psychology at the *Collège de France*, could be traced back to a diseased will (Ribot 1884; Carroy & Plas 1996; Faber 1997). The ability to consciously direct the course of mental and physical events had been the last in a series of evolutionary advances that distinguished the civilized man from the savage. So it was also the most susceptible to degeneration. Ribot's fascination with the pathological, a tradition perpetuated by his successor, Pierre Janet, set the tone for psychology in France. It was in this context of that Henri Piéron began his study of sleep just after the turn of the century.

Edouard Claparède

The argument that sleep could even be part of the study of normal psychology did not, however, originate in France. It was a psychologist from Geneva—Edouard Claparède—who first suggested that sleep was more than the defeat of consciousness by fatigue.

Edouard Claparède (1873-1940) is known today for his work in child psychology, or "experimental pedagogy," as it was then known [Figure I]. In 1912, he founded the *Institute Jean-Jacques Rousseau* in Geneva, an independent research centre devoted to the study of childhood development and its relationship to education. But the Institute's fame came less from Claparède's work than from that of Jean Piaget (1896-1980). Claparède made Piaget, who had just returned from two years studying Parisian schoolchildren in Binet's old laboratory, Director of the Institute in 1921 (Chapman 1988:31-35). It was here that Piaget began to focus his research on "genetic epistemology," which would eventually make him the most famous developmental psychologist of the twentieth century.

Claparède, however, did not feel that his work in child psychology was his greatest scientific contribution. He reserved that honour for what he called his "biological" theory of sleep, which he first proposed at a meeting of the Society of Physics and Natural History of Geneva in 1904 (Claparède 1961:78). Its aphoristic formulation became a commonplace among psychologists over the next three decades: "We do not sleep because we are intoxicated; we sleep so as not to become intoxicated" (Pillsbury 1941).

Claparède's theory brought together biological finalism with the psychology of sensation in much the same way that James had married the two twenty years earlier in his theory of the emotions. Both attempted to depict the work of consciousness as purposeful. This similarity between the two comes as little surprise, as Claparède clearly adopted James as his mentor in psychology. Like James, Claparède came to psychological research through medicine, after hearing his cousin, Theodore Flournoy, lecture on "The Soul and the Future of Psychology" in

Figure I
Edouard Claparède
(Pillsbury 1941)

1891 (Claparède 1961:68ff). Flournoy (1854-1920) taught experimental psychology at Geneva, and was heavily influenced by James, whose work he propagated in reviews and a book, *Métaphysique et psychologie* (1890). James himself had studied natural history under Flournoy's father (Claparède's uncle) in Geneva in 1859, and, around 1890, began to cultivate a close relationship with the son, whom he visited in 1892.[1] Claparède, who was just beginning a study of the coloured images associated by some people with sounds (*audition colorée*), met James in Flournoy's laboratory that same year. This initial meeting was to have a great impact on Claparède's career. In 1904—the same year Claparède presented his theory of sleep—he was recommended by James to take over as director of Flournoy's laboratory (Pillsbury 1941:272). In 1915, Claparède became Professor of Psychology at Geneva, replacing Flournoy, who had been made Professor of the Philosophy of the Sciences.

The description of sleep as an active, adaptive instinct fit nicely with the functionalism of James, John Dewey, and James Angell that dominated American psychology before the First World War (O'Donnell 1985). Claparède argued that sleep was the product of the sensation of fatigue, a sensation that, like the emotions that James had already described, carried with it an evolutionary advantage. The feeling of fatigue prevented the physiological damage that would otherwise result from the build-up of fatigue toxins in the body (Claparède 1905). Sleep was merely one part of the overall economy of sensation, which Claparède later described as obeying the "Law of Momentary Interest." As the cutting edge of adaptive response, consciousness tended to follow whatever was advantageous at the time. A feeling of fatigue could thus appear

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[1]See James's letter to Flournoy of May 31, 1891, in which he thanks Flournoy for his "unrestricted and unqualified praise," and reciprocates in kind, saying of the latter's book, "this is to be really 'scientific' without being a bar[b]arian, as so many of our 'scientists' are!...It behoves all of us who on the whole agree in aims and methods to close up our ranks and give each other a helping hand, and perhaps our 'School' will prevail!" (James 1999:VII.164-165). In a letter written in July of the following year, just after he visited Flournoy, James asked Flournoy to tell Claparède of "a charming young American lady, an admirable musician, who has a most elaborate system of chromatic symbols accompanying sounds, letters of the alphabet, and names, also a number diagram, of all of which she can probably give an intelligible account" (James 1999:VII.304).

even in the absence of muscular or intellectual exhaustion, so long as there was nothing more important engaging the organism's attention. Fatigue and sleep were related, but not in a mechanical way. Their relationship was governed by the organism's immediate situation, which was always viewed from the perspective of adaptation and self-interest.

Experimental psychology in France: Alfred Binet

The French reaction to Claparède's theory was rather different than the American response. Teleological thinking was warmly embraced by the American proponents of functionalist psychology. Functionalists set themselves up in contrast to their "structuralist" counterparts, led by E.B. Titchener, a student of Wundt's who founded the laboratory at Cornell in 1892. Titchener argued that psychology's goal was akin to that of chemistry: reduce the object to its elements, and then study the way these elements combined with each other (O'Donnell 1985:9-11). Likewise, psychology was more than simply an applied science. It could not, he argued, be reduced to its technical applications any more than physics or chemistry could be. Psychology's experimental methodology and its agnostic approach to metaphysical systems also served to separate it from philosophy. The idea that mind could be understood in terms of purpose simply had no place in Titchener's scheme.

Titchener's arguments were a minority view among the practically-oriented psychologists in the U.S. In France, psychology was a much different affair. There, psychologists were almost always trained first as philosophers. Their authority came not from their ability to experiment, but to reason. Their end was not to transform society, but to reveal the nature of mind. The scale of their enterprise was also dramatically different: by 1903, there were at least forty psychological laboratories across the United States, and they awarded more doctorates than all other fields except chemistry, physics and zoology (Smith 1997:493). In France there were but three laboratories, and all of them were in Paris: Janet's, at the Salpêtrière; Binet's, at the Sorbonne; and Edouard Toulouse's, at the Villejuif asylum.

Psychology was a minor concern in France, and it was distinguished only by the clinical observation of psycho-pathological phenomena, rather than an ethos of "experimentalism" (Carroy & Plas 1996). Philosophy and physiology, on the other hand, were inundated with metaphysical arguments and teleological premises. Bergsonism was on the rise, and Richet had insisted that all physiological analysis had to be conducted from the perspective of organismic defense (Richet 1893). French psychology could not distinguish itself as a discipline by adopting a functionalist approach to consciousness. Bergson was on the verge of doing just this, and his status as a scientific authority was controversial at best. So French psychologists emphasized the experimental nature of their studies. Hypnotism, as I argued in chapter one, was on the wane. So they chose either the clinical route of psychiatric observation (Janet), or the psychophysiological methods (Toulouse and Binet). The latter route, which two commentators have recently suggested finds its origins in "mimicry and fetishism," involved the rejection of Bergsonism as a threat to establishing psychology as an experimental science (Carroy & Plas 1996:83).

When Claparède's theory first appeared in France, reaction to it was ambivalent and guarded. Binet set the tone in the pages of his journal, *L'Année psychologique*. Binet agreed that fatigue theories could not explain the periodicity of sleep. But Claparède's "interest" theory of sleep was no better, as he offered no physiological demonstration of how this attention mechanism worked. He concluded that Claparède's argument could be reduced to that simple Bergsonian phrase, *dormir c'est se désintéresser*—to sleep is to be disinterested (Binet 1906).[2]

In 1906, this last comment was tantamount to a charge of metaphysics—a charge that carried a great deal more anti-scientific weight in France than it did in the United States at the time. To most French social reformers, religion appeared as a threat to the safety of the republic

[2]This is an awkward rendering of Bergson's phrase, but the alternative "to lose interest" suggests a passive account of sleep that Claparède is obviously trying to avoid. I have chosen the current formulation on the basis of Claparède's own English publications. For example: "Sleep is then, a reaction of defense. This reaction consists in an inhibition of the attention to the present situation. A being who sleeps is a being who is disinterested. But this inhibition, this distraction, is not passive, it is active..." (Claparède 1906:85).

in the wake of the Dreyfus Affair (Grogin 1988; Agulhon 1993). The arrest, in 1894, of Albert Dreyfus, who was charged with selling military secrets to the Germans, polarized French society.[3] Dreyfus had been convicted only because his handwriting resembled that found on documents associated with the espionage. But many younger republicans were convinced of Dreyfus's innocence, and argued that he had been the victim of the anti-Semitic sentiments of those who supported the glory of Old France, with its deep traditions of military and clerical order. The anti-Dreyfussards, on the other hand, supported the conviction, even arguing that the question of Dreyfus's innocence was irrelevant: the psychiatrist Dejerine, for example, reputedly stated that if Dreyfus were really a good patriot, he would spare France the turmoil, and stop protesting the charge (Piéron 1961). When, in 1899, the case was re-tried, and Dreyfus was pardoned, the anti-Dreyfussards became enraged. This galvanized the leftists, who feared a rightist coup, under the rubric of "republican defense." They swept the elections of 1902, and inaugurated a new period of reform that was to make France safe for republican values (Grogin 1988; Agulhon 1993). This translated into an almost institutionalized anti-clericalism, as anti-Dreyfussards were, more often than not, Catholic conservatives. In 1905, Napoleon's Concordat of 1801 was revoked, effectively ending the role of organized religion in any state affairs. Clergymen no longer received a state salary, and the Church's role in educational affairs was severely curtailed. The Inventory followed, in which sacred objects in cathedrals and monasteries across France were handled and counted by taxmen in the service of the state. Riots broke out in many provincial areas, and a Catholic revival emerged in response.

It was in this context that Binet described Claparède's theory in terms of Bergsonian metaphysics, which Binet felt were both conservative and dangerous. In 1906, Alfred Binet (1857-1910) had been closely implicated with republican reforms for a little less than a decade. Binet had started out studying law, but abandoned it for medicine (Wolf 1973:5). He failed to

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[3]The Dreyfus Affair spanned nearly fifteen years, from his arrest in 1894, through his official pardon in 1899, to the transfer of Zola's ashes to the Panthéon in 1908, where Dreyfus was nearly assassinated. A helpful chronology of events can be found in Agulhon 1993, pp. 474-480.

complete his medical studies, and began, around 1880, to read psychology. He received no formal instruction in the field, but began to publish papers on the subject in Ribot's journal, *Revue philosophique*. Around 1882, he met up with a former classmate, Joseph Babinski, and was drawn into Charcot's circle at the Salpêtrière. He passionately defended Charcot's vision of hypnotism as a disease, but his claims were demolished by Delboeuf (Wolf 1973:8). After abandoning hypnotism as an experimental method, Binet published his last book on the subject in 1892.

In its place, Binet went searching for other ways to reveal the "laws of character" (Carson 1999:362). Without a laboratory or a teaching appointment, it could just as well be said that Binet went searching for subjects. After he left the Salpêtrière in 1890, and before he joined Henri Beaunis at his laboratory at the Sorbonne in 1891, Binet conducted research on his two young daughters at home. Inspired by the work of Marey and Mosso, Binet studied the developmental changes in the bilaterality of hand movements and reaction times with a Marey tambour and a kymograph (Wolf 1973:79-98). He combined these with cognitive tests, in which his children's ability to recognize small differences in length between lines drawn on different cards turned out to be almost as good as that of the adults he studied. Once Beaunis appointed him director of the laboratory at the Sorbonne, Binet had access to a larger group of subjects, which included schoolchildren. It was, of course, his work with this population of human subjects that would eventually generate his greatest legacy to psychology: the IQ test.

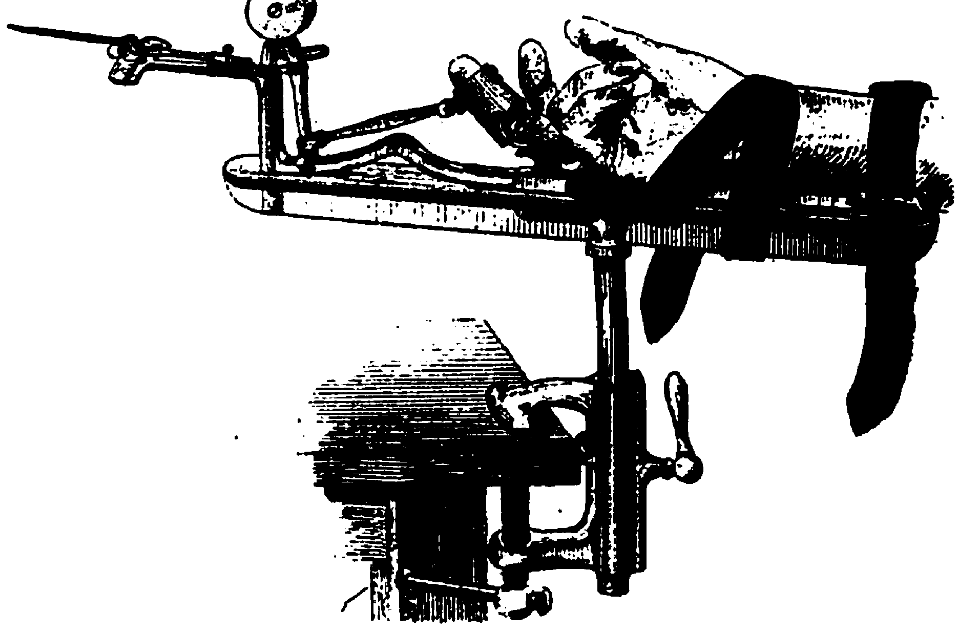
In the spring of 1895, Binet travelled to Bucharest to lecture at the university there. He still lacked a teaching position in France, but was greeted in the newspapers as "a representative of modern science," nonetheless, and he addressed enthusiastic crowds in filled auditoriums (Wolf 1973:16-17). He also picked up a student, Nicholas Vaschide, who followed him to Paris to study in his laboratory at the Sorbonne.

By this time, Binet had become consumed by the idea that psychology should model itself around the study of individual differences. Knowledge of the psychological variations that existed within a population would, he argued, "have practical importance for the pedagogue, the physician, the anthropologist and even the judge" (Binet & Henri 1895:411). To this end, Binet and his collaborator, Victor Henri, outlined a number of categories in which individuals could be tested: memory; mental imagery; imagination; attention; comprehension; suggestibility; aesthetic sensibility; moral sensibility; muscular force and willpower; and motor habits. To this end, Binet embarked on a lengthy study of these properties with his primary collaborator, Vaschide, in 1896.

Given the current importance of standardized tests in educational systems, it is tempting to read history backwards and depict Binet, who pioneered the use of such tests, as having nursed a deep interest in educational psychology after abandoning hypnotism in 1890. Binet scholars have been unable to agree as to when Binet began to turn to "scientific pedagogy" (Avanzini 1999:11). Some say it was as early as 1886. Other suggest that it was more like 1895, just around the time that he went to Rumania. The more interesting question, however, is *why* Binet decided to pursue this particular avenue of research. As Binet himself portrayed it, he and Vaschide came to study large groups of schoolchildren simply because no other avenues of research were open to them.

Conducting such tests took an enormous amount of time, for both the experimenter, and for the subjects. On the instrumental end of things, Binet and Vaschide praised Mosso for having invented a quick, simple, and portable method of measuring fatigue. "The ergograph," Binet cheered, "is not one of those devices destined for use in only one kind of experiment; it is a veritable tool that can be used in a wide range of studies" (Binet 1895:452). Binet and Vaschide improved upon Mosso's original design several years later, creating a "spring ergograph" that could be used by even the weakest children [Figure II]. In their outline of the history of the device, they noted that "Mosso's ergograph, which today serves so many functions in the laboratory, realized a new principle" (Binet & Vaschide 1898:253). This "new principle" was the instrument's ability to isolate and graphically depict the work done by a single group of muscles. This isolation prevented another muscle group from picking up any of the work that the fatigued

Figure II
The spring ergograph
(Binet & Vaschide 1898d)



Binet (r.) and Vaschide (c.) conducting a psychometric experiment muscles could no longer accomplish. The ergograph enabled the careful production of fatigue in the laboratory, and one small part of the body to speak for the state of the whole organism. It also enabled psychologists to study "intellectual fatigue" rather than just physical exhaustion. Binet was so enthusiastic about this project that he published a book on the topic in 1898 (Binet & Henri 1898).

Isolating a cooperative group of subjects was another matter entirely. The difficulty was partly structural. Binet and Vaschide agreed that Wundtian laboratories formed the basis of psychology's success as an academic discipline. But these laboratories had also deformed the science by their reliance on experimental introspection. Students would participate uncritically in such experiments "to sanction a good grade in their exams" and thereby earn their doctorate (Binet & Vaschide 1898a:3). They would then go on to found laboratories elsewhere, and perpetuate the same meaningless exercise in professional legitimation. The other type of psychological laboratory, which took individual differences as its object, had been developed by the British eugenicist, Francis Galton. But, Binet and Vaschide observed, such laboratories did not offer degrees, and were therefore shunned by students, thus denying the study of individual differences access to a large pool of subjects.

Binet and Vaschide first turned to amateur sporting societies and private gyms. But these subjects were undisciplined, and often turned the experiments into sporting contests. Binet had some experience with studying public figures, who were motivated to participate out of sheer self-love. But they were an atypical lot—Binet had, for example, studied a famous calculator (Carson 1999). Such people could not possibly represent the wide spectrum of differences Binet hoped to discover. So they turned to the schools. But even here, there were problems. Administrators of colleges and lyceums would not allow psychological studies, because "the students of these institutions have parents who know about the journalists and the disputes; one fears that the parents, ignorant of the nature of the research to be conducted on their children, suspect some form of *hypnotism* or some study of *materialism*, and raise complaints that resound in the newspapers or in the courtrooms..." (Binet & Vaschide 1898a:5-6. Italics original). In desperation, they settled on a study of the children at the primary schools, where the administration was much more open to pedagogical studies.

Binet's interest in pedagogy increased dramatically after 1898, when he published the studies he and Vaschide had conducted, filling filled hundreds of pages of *L'Année psychologique* (Binet & Vaschide 1898a-d). Théodore Simon, an intern at a colony for retarded children at Perray-Vaucluse, was so impressed by Binet's work that, in 1899, he arranged for Binet to supervise his M.D thesis (Wolf 1973:18-20). In the same year, Binet was asked to join the *Société libre pour l'étude de l'enfant*, which had just been founded, and would later bear his name. Under his direction, the Society petitioned the Ministry of Public Education to do something about the education of retarded children. To this end, he and Simon developed a series of tests that differentiated children who learned normally, and those who did not. Binet, who still lacked an official teaching position, now had access to a group of subjects and a cause.

Bergson's philosophy represented a threat to the system Binet was helping to reform. Bergson had already established himself against Binet in 1902, when Binet made a bid to replace Ribot, who had just retired from his Chair in Experimental Psychology at the *Collège de France*. Bergson supported Pierre Janet, and—it should come as no surprise—Marey supported Binet. Janet won out, and Binet was disappointed again when he then tried to replace Janet at the Sorbonne. He lost out to George Dumas. It was around this time that his work on intelligence testing started to dominate his concerns (Wolf 1973:160-175).

Just a year after he reduced Claparède's theory to Bergsonian metaphysics, Binet published his study of the teaching of philosophy in France—the famous "Binet report" (Binet 1908; Grogin 1988). The philosophy curriculum in the lyceums and colleges had been revised as part of a series of educational reforms passed in 1902. These changes reflected the officially-sanctioned anti-clericalism of "republican defense." Religion and metaphysics were no longer mandatory parts of the curriculum. Binet wanted to know what had replaced it, and for good reason—practically all French psychologists during this period had trained as philosophers (Brooks 1998). Binet proceeded to send out an 18-question form to 300 professors across France.

When he presented his report to the *Société de philosophie* in 1907, he summed up his findings in a single phrase: "The State philosophy, like the State religion, is on the run because the number of believers has diminished" (Binet 1908:227). But, Binet complained, despite the fact that many students were demanding to be taught the new psychology, most instructors lacked the competence (or the desire) to demonstrate even the simplest experiments. Bergsonism, on the other hand, proved to be remarkably popular. Many instructors, Binet observed, had adopted his system of philosophy completely and uncritically. He fell just short of calling Bergsonism a fashionable surrogate for religious metaphysics.

Metaphysics and sleep: Nicholas Vaschide

Binet lost his Rumanian student, Nicholas Vaschide (1873-1907), just as he was redirecting his interests away from psycho-pathology and towards pedagogical reform. It is not known exactly why Vaschide left Binet's laboratory. One historian has suggested that, in the wake of some unkind press their collaborative efforts received, Binet broke with Vaschide, who "seems not to have shared Binet's rigor as an experimentalist" (Wolf 1973:18).[4] It seems equally likely, given Binet's hopes for a teaching appointment, that the master blamed the student. Regardless, Vaschide left Binet's laboratory in 1898.

But if Vaschide did not share Binet's rigour, he certainly approved of his rhetoric. "Experiment" would become his watchword at his new post as *Chef de travaux* in the

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[4]The negative reaction in question was an article by Shepherd I. Franz, in the *Psychological Review*. Franz pointed out numerous errors in their calculations, and noted that they had calculated mean variations for "ranks" of their subjects, rather than raw scores (Wolf 1973:97-98).

psychological laboratory at the asylum at Villejuif. From 1900 until his untimely death in 1907, Vaschide collaborated with Henri Piéron and Edouard Toulouse, creating a text-book which would eventually define the nature of experiment among French psychologists.

Vaschide has remained almost invisible in histories of French psychology (Reuchlin 1965; Ellenberger 1970; Wolf 1973; Paicheler 1992; Carroy & Plas 1996). Carroy and Plas, for example, argue that his colleague, Henri Piéron, is the key figure in directing French psychology away from psycho-pathology and towards reaction-time experiments. Likewise, Paicheler claims that he is recognized as "the true founder of scientific psychology" in France (Paicheler 1992:15). Certainly Piéron played a pivotal role in establishing psychology as an academic discipline in France. But his relationship with Vaschide should not be overlooked, precisely because it was Vaschide who seems to have brought Piéron to think about the question of sleep. And it was Piéron's response to this question, through his 1911 doctoral dissertation, that solidified his reputation as an experimentalist in the first place. An examination of Vaschide's work, then, will provide the context in which Piéron came to define sleep in physiological, rather than psychological, terms.

In 1900, Vaschide found himself in the newest laboratory for experimental psychology in France. It was the creation of Edouard Toulouse (1865-1947), a medical doctor who first gained prominence in 1896 with his psychological study of Émile Zola entitled *Les Rapports de la supériorité intellectuelle avec la nevropathie* (Schneider 1991). In 1898, he was appointed *médecin-chef* at the asylum at Villejuif, on the southern outskirts of Paris. Unlike the Salpêtrière, which traced its history back to seventeenth century, the Villejuif asylum was a thoroughly modern creation, having been completed only in 1889.

In 1900, Toulouse created a laboratory at Villejuif under the auspices of the *École Practique des Hautes Études*. Despite the fact that his new "*Laboratoire de psychologie expérimentale*" was located within an asylum, Toulouse had no intention of restricting his experimental inquiries to the problem of madness. This is not to say that Toulouse was not interested in mental illness. On the contrary, his eugenics was keenly devoted to the *prevention* of mental illness, largely through the early identification of pathological individuals through psychopsychological tests; the likes of which we have already seen in Binet and Vaschide's 1898 study (Ribeill 1980; Braun 1992). Toulouse wanted such testing to pervade every aspect of the young republic, where he eventually hoped to find "a psychophysiological laboratory acting as an organ of selection and classification" that could direct citizens to their appropriate places in education and industry (Schneider 1991:416). The wave of strikes that shook France around 1905 were, Toulouse thought, partly the result of workers not performing tasks that suited their personality. Toulouse argued that his testing laboratories could set standards for these "human machines" exactly as laboratories established standards for industrial products. Toulouse, who later took over as editor of the popular *Revue scientifique* from Charles Richet, wrote so widely on his rationalized approach to psychiatry and psychology that "Eh, va donc chez Toulouse!" became a popular exchange among irate Parisian cab drivers (Schneider 1992:426).

One of the first publications to emerge from Toulouse's laboratory, however, emphasized this practical orientation in a rather different way. In 1902, Vaschide and a young *préparateur,* Henri Piéron, who had defected from Binet's laboratory after spending only a few months there in 1899, produced a curious monograph entitled *La Psychologie du Réve au point de vue médical.*[5] The book's premise—that dreams could be used as an effective diagnostic tool—was a strange Antiquarian throwback in a self-consciously Modern age, invoking the Aesculapian cults as a precursor to their study. Yet it fit well with Toulouse's mandate to create a form of psychological knowledge that was based in psycho-pathology, but had practical applications beyond this field. The book, which was little more than a review of the literature, concluded that dreams did indeed come from organic sensations and thus should be exploited as such in medical

[5]There is a curious conflict between Wolf's (1973:20-21) description of Piéron, and that of Carroy and Plas (1996). While the latter emphasize Piéron's role in making reaction time experiments the paradigm of experimental psychology in France, the former suggests that Piéron found these same tests "extremely frustrating," and were perhaps his reason for abandoning Binet's laboratory. Wolf personally interviewed Piéron in 1960.

fields outside of psycho-pathology (Vaschide & Piéron 1902:94). Equally important was the fact that such investigations could "project the light of science on these obscure regions, making radical superstitions vanish" (Vaschide & Piéron 1902:95).

In a posthumously-published work, *Le sommeil et les rêves*, Vaschide continued this theme of using science to combat the superstition that surrounded sleep and dreaming.[6] Vaschide took careful note of Freud's study of dreaming, and devoted an entire chapter to the wish-fulfilment theory (Vaschide 1911:175-196). But he disagreed with Freud's interpretation of anxiety dreams, and he rejected the distinction between the manifest and latent content of the dream. Instead of interpretation, Vaschide opted for observation (Hobson 1988:69-73). He made recordings of the pulse and respiration, and coupled these with his observation of his subjects's motor activity in sleep. He even watched the faces of his sleeping subjects, hoping to observe an emotional expression that could then be correlated to a dream.

Vaschide put together his physiological observations and his subjects's dream reports, and concluded that all dreams had one element in common: emotion. Dreams were always coloured by an "intense emotivity able to cloak a sense of spirituality unknown to waking life" (Vaschide 1911:285). This emotivity was sometimes hidden from the dreamer, but it was obvious to physiological surveillance. It "augmented" the intensity of the dream-image, making it appear real to the dreamer. Vaschide called this process "spiritualization," and it accounted for the belief that dreams had been sent by God (Vaschide 1911:286-294). Dreams, he argued, were filled with the same banal images as waking life. The difference between the two was nothing more than a physiological state that encouraged credulity by increasing emotional sensitivity.

Vaschide felt he had located a physiological basis for the belief that abstractions—a word he used to describe dream-images—reflected an underlying reality (Vaschide 1911:294-297). He

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[6]Vaschide died of pneumonia in 1907, at the age of thirty-three. Eugène Osty, a devout follower of Richet's *métapsychique*, reported that Vaschide's death had been foretold by Mme. Fraya, a palm-reader Vaschide had examined for his study of cheiromancy (Osty 1923:60).

was taking the first steps towards a physiology of metaphysics. It is no surprise, then, that he rejected Claparède's theory of sleep, which, Vaschide claimed, simply "transported our systematised ignorance into another domain" (Vaschide 1911:23). While he agreed that a concept of inhibition might be derived from the phenomena of sleep, this added nothing at all to what was known about sleep itself:

> The hypothesis of sleep as a biological instinct reminds me of those classroom demonstrations that are meant to strike at the passive intellects of students and docile readers. They appear to explain certain phenomena, but only on the condition that certain initial conditions are met. The why of sleep remains as mysterious as ever... (Vaschide 1911:22-23).

Claparède's theory had postulated the existence of a "sleep centre" in the brain. If sleep was an active instinct after all, it would have to be physically located somewhere. This, charged Vaschide, was evidence that Claparède had "allowed himself to be influenced by the mania of the neurologists for searching for centres at every turn...Why this imperial demand for a sleep centre?" Vaschide cried. "It is true," he reminded his reader, "that modern psychologists [that is to say, phrenologists] localized even the emotions by cranial inspection and percussion!" (Vaschide 1911:20-21).

In place of Claparède's theoretical musings, Vaschide called for facts. "Hypotheses," he complained, "have had bizarre and diverse fates in the history of the sciences...The belief in words invented to explain what is yet inexplicable makes me appreciate all the more the facts we must endeavour to multiply in profusion" (Vaschide 1911:24). J. Allan Hobson, a neuropsychiatrist who brought Vaschide's work to the attention of historians, has perhaps tried a little too hard to rehabilitate this cantankerous character. Hobson, who never mentions Claparède, claimed that Vaschide "thought sleep had a positive function," and that "he believed sleep to be an instinctual function of the brain." Worse, Hobson concludes that "these ideas of Vaschide's reflect the first impact of Darwin's thinking upon theories of sleep" (Hobson 1988:73). As Vaschide's vituperative reaction to Claparède demonstrates, nothing could be further from the truth. Like his former master Binet, Vaschide decried all functional explanation as useless metaphysics.

Claparède responded in kind, albeit five years after his adversary had died (Claparède 1912). It was not, he argued, a question of whether or not his biological theory rested on facts, but of *which* facts one chose to accommodate. Vaschide's description of sleep had relied on the fatigue theory, which could not explain many facts about sleep:why it could appear in the absence of fatigue; why it came in one long period, rather than in several short ones; or why it could be brought on by hypnotism or association. The disagreement, Claparède insisted, was not over the facts themselves, but their origins. Which facts should be explained: those derived from lived experience and introspection or those generated in the laboratory? Vaschide refused to recognize the need to explain experience, which Claparède felt was a curious kind of pathology in itself:

> [Vaschide's refusal] is certainly interesting for the psychology of the professional deformation of certain individuals given up to experimentation: these persons have lost the habit of seeing what goes on around them in real, concrete life to such an extent that, for them, in order to have the right to the title of "fact," a phenomenon must have occurred within the four walls of a laboratory or a hospital room, or have been recorded on a registering cylinder or by means of some other apparatus! The facts, as one can observe them in everyday life, count for nothing! My theory of sleep rests precisely on these mundane facts, which have been completely neglected up until now, counting for nothing among people like Vaschide and others who, without a doubt, think like him (Claparède 1912:424).

"We must remember," Claparède continued, "that if we prevent sleep, the individual will soon be in a state of complete exhaustion. But, assuredly, it is always the physiologist, who, *interpreting* the result of the experiment, draws from it the notion of defense or protection. This notion, of course, will not inscribe itself on a registering cylinder!" (Claparède 1912:426. Italics original).

Sleep as a physiological problem: Henri Piéron (1881-1964)

Claparède's rebuttal appeared just before one of the most important studies of sleep in the twentieth century. In 1913, Henri Piéron's doctoral dissertation, *Le problème physiologique du sommeil*, was published (Piéron 1913). The impact of this work came less in France than it did in the United States, where Nathaniel Kleitman would use it to shape his own inquiries a decade later. In his introduction to his very first paper on sleep, Kleitman offered the following comments about Piéron's work:

> One of the best books ever written on the subject of sleep is Piéron's *Le Problème Physiologique du Sommeil*, which besides embodying the results of original research on sleep from the histological, biochemical and physiological standpoints (this by a psychologist!) contains a valuable and exhaustive bibliography brought up to 1912...(Kleitman, 1923:68).

Kleitman's somewhat muted surprise that a psychologist would have approached sleep from a *physiological* perspective indicates his ignorance of the context in which Piéron was working. Sleep became a somewhat controversial topic in the wake of Claparède's biological theory, and Piéron's dissertation was an attempt to straighten out the differences between Claparède's approach and that of his former collaborator at Villejuif, Vaschide. Like Vaschide, Piéron railed against what he felt to be the speculative excesses of functionalist thinking. Teleology needed to be defeated. What was needed were more facts. But Piéron was equally intrigued by the question of memory. He disagreed with Bergson's divorce of memory and matter, and for good reason: Piéron was a zealous reformer who felt that the immense popularity of Bergson's introspective philosophy was another example of France's failure to keep pace with modern life. Introspection was, like the state religion and metaphysical belief in general, on its way out. Piéron thus endeavoured to invent a way to study sleep that separated it from consciousness, but retained the idea of memory. He found it in his technique of "enforced wakefulness."

Ribot, Janet, and the neurophysiologist Louis Lapique must have seen the writing on the wall when they recommended that a young philosophy student, Henri Piéron, serve as *préparateur* in Toulouse's freshly-minted laboratory in 1901.[7] Here, Piéron could satisfy his desire to study the nature of mind in a laboratory, rather than a clinic. Piéron, who was only twenty at the time, was in the process of completing his *licence* and *agrégation* in philosophy. His father, who had taught mathematics for seventeen years at the Lycée Saint-Louis and became *inspecteur-en-chef* of public education in 1894, insisted on a scientific education for his youngest son. By opting for experimental psychology, Piéron tried to satisfy his father's demands as well as his own curiosity about the relationship between mind and body.

Piéron immersed himself in all the battles being waged over the soul in France at the turn of the century. He studied under Charcot's successor at the Salpêtrière, Fulgence Raymond, who advocated a strict psychological determinism (Ellenberger 1970:779-780). Janet also held a clinic there, and Piéron served as his secretary. He also visited Binet's laboratory at the Sorbonne, which served as his introduction to psychological testing.

But Piéron's greatest source of inspiration came from outside of neurology and psychopathology. Like Charles Richet, Piéron wanted to develop a theory of mind capable of integrating the phenomena of consciousness into a theory of life. So Piéron, like so many French biologists of his day, turned to evolutionary theories of consciousness as a way of making psychology scientific. He was particularly smitten with the work of one of his father's former students, a "most precocious mathematician side-tracked to biology" named Félix le Dantec (Piéron 1961:263; Mengal 1994). Le Dantec (1869-1917) was the most important populariser of the debates between neo-Lamarckians and neo-Darwinians that raged in France from the 1890s until the Great War. He held fast to "an evolution explicable in the mechanistic terminology of the physical sciences and shorn of the Malthusian implications of selectionist theory" (Persell 1999:101-102). For French neo-Lamarckians, the question of inheritance was framed in the

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[7]Schneider (1991:419-423) reports that Ribot, Janet and Lapique suggested Piéron for the position at Toulouse's laboratory, but Piéron himself does not mention this in his autobiographical paper (1961). Fessard (1951) and Reuchlin (1965) also offer biographical detail on Piéron.

materialist terms of physico-chemical determinism (Roger 1979). Acquired traits had to be inherited through some sort of bio-chemical or bio-physical mechanism, and the job of the biologist was to discover this mechanism. Neo-Darwinian claims about selection were dismissed as metaphysics. August Weismann's claim regarding the continuity of the "germ plasm" was, for example, rejected by Le Dantec as "a revival of the spiritualist error," because it effectively made the germ plasm immortal (Diara 1979). Piéron's approach to the problem of sleep adopted a similar reliance on demonstrating the existence of physical and chemical mechanisms. He also shared Le Dantec's hostility to non-empirical biological theories.

Le Dantec introduced Piéron to the father of evolutionary theory in the Third Republic, Alfred Giard. Giard (1846-1908) had been a classmate of Piéron's father, and exerted a great influence on the young Piéron, who repeatedly referred to Giard as his "master" (Piéron 1910, 1913; Mengal 1994). Giard held the first chair for the study of the "Evolution of Organised Bodies," which was created at the Sorbonne in 1888. The very existence of this position was the subject of violent debate in the Parisian municipal council, as its creation was expressly dedicated to the overthrow of the eclectic "spritualist" thought that had dominated French philosophy for much of the nineteenth century. Those who supported the creation of the chair pointed out that in Great Britain and in Germany, evolutionary theory had long provided the epistemological foundations of the moral sciences. France was still, they feared, "a century behind" its main competitors (Viré 1979).

Piéron was thus introduced to psychology at a time when Binet and Toulouse were attempting to render it more practical, and to biology at a time when le Dantec and Giard hoped it would ground the moral sciences of the nation. Piéron adopted the posture of a materialist reformer early in his career. He passed his *agrégation* in philosophy in 1903 only through the intervention of Lucien Lévy-Bruhl, a sociologist. Lévy-Bruhl became a member of the jury only to oppose Darlu, whom Piéron described as "a narrow-minded moralist" who was "violently opposed to the scientific method in psychology." Darlu had apparently announced to Piéron, who had already failed twice, that he had no hope of passing (Piéron 1961:263). Having completed his degree in philosophy, Piéron then decided to pursue his *doctorat ès sciences*. He chose sleep as his research topic, having already been introduced to its possibilities through his work with Vaschide on dreams.

Like Vaschide and Toulouse, Piéron was dedicated to developing an experimental method in psychology that was psychophysiological, analytic, and aimed towards an application in education and industry. But Piéron was interest in the psychology of skills and performance, and put rather less emphasis on the naturalization of individual differences than Binet did (Reuchlin 1965). Toulouse, Vaschide and Piéron had consistently worked together at hammering out a manifesto of experimental psychology from the earliest collaboration. In 1901, they presented their first version at the Physiological Congress in Vienna (Toulouse, Vaschide & Piéron 1902): they published it as a book in 1904, and Toulouse and Piéron reworked it for a second edition after Vaschide died (Toulouse, Vaschide & Piéron 1904, 1911). The book soon became a founding text for French psychology, and its status rose along Piéron's stature as a professional psychologist (Carroy & Plas 1986). After Vaschide's death in 1907, Piéron became *chef-de-travaux* of the laboratory at Villejuif. When Binet died in 1911, Piéron beat out Janet to head the laboratory at the Sorbonne, effectively amalgamating the two competing sites under his charge. Janet had already been thrown out of his laboratory at the Salpêtrière by Dejerine. Aside from objecting to Janet's use of psychiatric patients as experimental subjects, Dejerine was a rival of Raymond, who had sponsored Janet's position at the Salpêtrière laboratory (Brooks 1998:174 & 284). When the opportunity appeared to get rid of Janet, Dejerine jumped at it, effectively making room for Piéron.

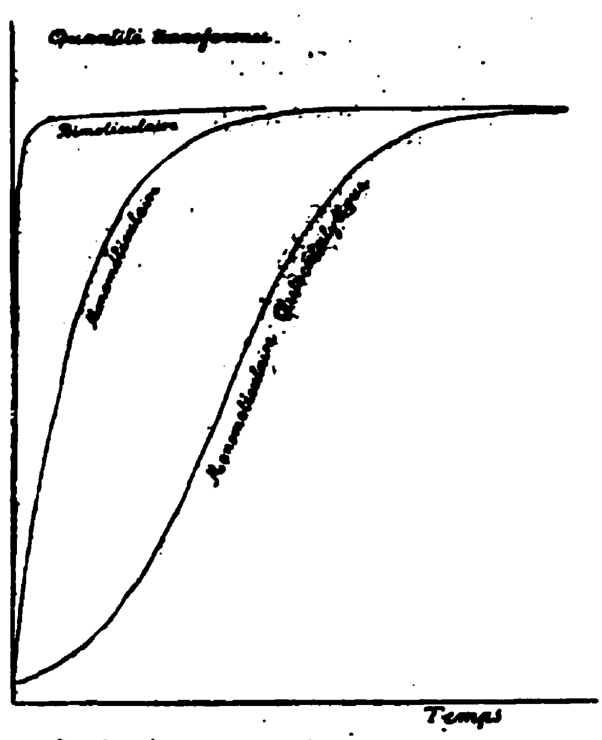
When Piéron published his dissertation in 1913, his status in the psychological field in France was unsurpassed. He now controlled not only the work of the two laboratories, but the main organ of psychology in France: Binet's old journal, *L'Année psychologique*. At thirty-two years of age, he was in a position of substantial influence and power, and had few competitors. His study of sleep, the importance of which was acknowledged by psychologists and biologists alike, was designed to weld these two disparate fields together.

Organic memory

Piéron, however, was equally interested in another issue, one that had a rather more pressing social impact than sleep. For his first major study, published while he was still completing his doctorate in the natural science, Piéron chose a topic dear to the heart of his masters—organic memory (Piéron 1910). The concept of organic memory suggested that heredity and psychological memory were essentially the same. The site of memory was the body, and it could be investigated through the study of history and development. Drawing upon scientific and literary sources, the historian Laura Otis has argued that the idea of organic memory arose with the nationalist fervour that swept through Europe in the last quarter of the nineteenth century (Otis 1994). Her comparative survey also reveals that organic memory was an expression of European anxieties about race relations and degeneration that were taken to be the core of numerous social problems (Otis 1994:4). This was particularly the case in France, where the idea of degeneration exposed the immense insecurity the nation felt at losing its role as the cultural and scientific head of Europe after its fateful loss to the Germans in 1871. Organic memory had long been a favourite theme of Ribot's. Richet also picked up on it, and used it to defend his racist eugenics (Schneider 1990; Kroker 1999). At the height of his influence, between 1900 and 1912, Richet completed a manuscript entitled *La sélection humaine*, which outlined a radical program of human breeding that included the elimination of mental defectives and the prohibition of inter-racial marriage.

Piéron's contrasted his vision of memory to that of Richet and Bergson. Memory, Piéron argued, progressed *through* different stages of matter. It was not separable from matter, as Bergson had argued, nor was it the unique property of human beings, as Richet maintained. Piéron was no vitalist, and happily claimed that there were forms of memory in all matter, easily demonstrated, he thought, by noting the similarity of the curves of autocatalytic monomolecular reactions to the learning curves delineated by Hermann Ebbinghaus [Figure III]. Memory, for Piéron, was nothing more than "a persistent influence of the past on a present state," or the "consecutive effect of events, which have since disappeared, on current phenomena" (Piéron

Figure III
Piéron identified monomolecular autocatalytic
chemical reactions with Ebbinghaus' "learning curve"
(Piéron 1910)



Courbes des réactions chimiques de trois tubes,
(le troisième représenté par le type en escalier).

1910:10). One of the most basic manifestations of memory in the organic world, thought Piéron, was *rhythm*. So he began with a discussion of the various rhythms that appeared in the lowest forms of organized matter, such as the influence of light on the rhythmic movements of plants, and that of the tides on sea creatures. Piéron described such phenomena as "anticipation," and argued that what distinguished the anticipatory reflex from that of the rhythm was that the latter could appear spontaneously, in the absence of any external excitation. Such rhythms were adaptive, and were included under the rubric of memory, because they indicated "a use of the past to determine a future that was advantageous to the organism" (Piéron 1907a:340). Piéron had no qualms about extending this observation. Anticipation, he thought, could be found "in every degree of differentiation and evolution, from the nervous cell of the Actina [sea anemone] up to the cerebral centres of man, from the reaction to the tides up to our own scientific forecasting" (Piéron 1907a:340).

From here, he built up a claim that became the lynchpin of his argument about the relevance of organic memory to social progress. Against Richet, Piéron held that memory was not an acquired form of intelligence that had evolved only in humans and was transmitted through neo-Lamarckian inheritance (Piéron 1910:2-3). Memory was not the ability represent ideas to oneself—this was a holdover of unscientific introspective psychology. Piéron argued instead that memory was nothing more than a form of repeated activity, which could be observed objectively.

As an investigative model, Piéron preferred the work done by comparative psychologists in the United States to Wundtian introspection. There was, Piéron thought, a perfectly good sociological reason for the American lead over France in scientific psychology. American psychological laboratories were dispersed among a number of widely diffused centres, and this forced them to find a way of cooperating by coordinating the nature of their knowledge. They accomplished this by using animal subjects instead of introspective observers—a method that was championed by, among others, Robert Yerkes at Harvard. In France, on the other hand, all scientific work centred on Paris. This promoted an intensive bickering among psychologists and prevented new ideas from being accepted quickly (Piéron 1910:3-4).

Yet France did have an intellectual precursor to this style of research, which focussed on what people did, rather than on what they thought. Piéron identified French Catholicism's greatest apologist of the seventeenth century, Blaise Pascal, as the true forerunner of the revolt against introspection. Pascal, argued Piéron, had prepared the way for a study of Man based on the observation of behaviour alone. Piéron cited a letter Pascal had written to Périer, which suggested that "in order to determine whether or not it is God who makes us act, it would be better for us to examine our external *behaviour* [*comportements*], rather than study our internal motives" (Piéron 1910:23. Italics original). Piéron used Pascal to nationalize what then emerging in the U.S. as the study of behaviour, which culminated in J. B. Watson's famous manifesto against experimental introspection (Watson 1913).

Piéron thought that Richet and Bergson shared a common problem. They both attempted to explain consciousness (and in particular, human consciousness), and thus they consistently went beyond facts that could be observed objectively. A comparative methodology that studied behaviour, on the other hand, seemed to Piéron to eliminate this problem entirely.

But why write a book about memory in the first place, if not to uncover the secret nature of mind? Piéron's analysis of behaviour was consumed by more practical concerns. Clearly the progress—the evolution—of memory was relevant to the "general improvement of the conditions of the life of species" (Piéron 1910:346). On this note, Piéron returned to Richet, but this time he addressed the question of eugenics. Richet identified memory with intelligence, and felt that the exercise of mental powers, coupled with the practise of artificial selection, would improve the lot of mankind by developing superior human beings. The human mind was, after all, the most precious instrument in the pursuit of scientific knowledge. And, for Richet, science was the only road to progress:

> One cannot have scientific conquests without new instruments. If, in 1980, our grand-children have nothing but our microscopes, telescopes, galvanometers, micrometers, and balances from 1911, they will know nothing more than we do. The condition of scientific progress is the creation of newer, more perfect instruments. And the instrument of all science, one more necessary and more powerful than any telescope or galvanometer, is the human spirit. Thus the first condition, indispensable to all progress, is the progress of the human mental machine (Richet 1917:14).

This, argued Piéron, was incorrect. "The biological evolution of memory," he mused, "appears to have been terminated." It was unlikely that human selection would do anything to change this. But progress was nevertheless possible: "Mental progress is incontestable, but it concerns knowledge; this knowledge, however, is no longer individual. It is not at all hereditary—it is social" (Piéron 1910:350).

The social dimensions of memory invoked exactly those structures that Piéron and Toulouse had set out to improve: institutionalized vocational guidance, a rationalized educational system, and the abolishment of metaphysics and speculative philosophy. It also included more mundane examples of development, such as the invention of the alphabet and printing. It was progress in these areas, rather than the improvement of the human body itself, that would secure happiness and social stability in the future. The greatest obstacle to such progress was in overcoming tradition. History, which had provided eugenicists like Richet with arguments about the biological superiority of the white race, had to be overturned:

> ...there remains the danger of a willful preoccupation with things of another time...history, this recapitulationary form of social memory, can occasionally raise legitimate concerns...but it is not good to always look behind, to absorb or hypnotize oneself in contemplating the road that has been taken. Happy, in a sense, are the people who are without a history, who can look only to the present and to the future. All their efforts are fecund, and the great heights currently achieved by science and industry in America are, to a great extent, possible only in the absence of the burden of history. In France, on the other hand, the predominance of historical study certainly appears to be one of the principal causes of our relative decadence. It is through science that social progress is created, and it is sterile to give oneself up to the vain knowledge of the past. To better see what has been done, one forgets to do something. Greece, which lives in its memory, believes itself today to be a great nation (Piéron 1910:352).

Sleep as rhythm and reflex

It is within this context of the debate over organic memory and the nature of social progress that Piéron attacked the problem of sleep. Claparède had suggested that sleep was an instinct of defense, invoking the same teleological principles that Richet relied upon in his physiology. Piéron, on the other hand, rejected the idea that this defensive purpose somehow caused sleep. Instead, he described sleep as a form of memory. Its rhythmical form certainly indicated that sleep was an expression of adaptation (Piéron 1907a). But sleep's periodicity did not arise out of any need for defense. It was merely an example of the force of history acting on the present. To distinguish between the chemical mechanisms that caused sleep, and the organic memory that dictated its periodicity, Piéron devised a new method of investigation—"enforced wakefulness."

Piéron's method, which he first proposed in 1907, followed a well-established formula in French physiological and psychological research: induce a pathological state, and observe the results. Ribot, echoing Claude Bernard, had proposed just this method of research around 1870 (Carroy & Plas 1996). Illnesses were "natural experiments," and could be used to illustrate the normal condition. In this scheme, the study of hysteria, somnambulism, and other psychological diseases were paired with the induction of hypnotism as appropriate investigative tools. Enforced wakefulness (which Kleitman later renamed "experimental insomnia") was a pathological mirror of sleep, which could help its normal conditions (Piéron 1907b).

But Piéron was equally impressed by the American technique of comparative psychology. So he combined the two, and offered a phylogenetic as well as a physiological description of sleep (Piéron 1913). Did plants sleep? This was obviously not a question that he could have asked if he had taken consciousness, rather than behaviour, as his starting point. His answer relied upon an application of his method. It certainly seemed as though some sensitive plants, such as *Mimosa pudica* or *Hedysarum gyrans*, slept, because they periodically entered a state of rest. But this periodicity was not in itself enough to determine the presence of sleep. Sleep also had to indispensable to the organism's survival:

> We consider sleep to be a necessary periodic state, this periodicity being relatively independent of exterior circumstances, and characterised by the suspension of the complex sensori-motor relations that unite the organism to its milieu and enable it to continue its life, and in particular, its means of nutrition. It remains for us to see if the states considered as states of sleep do or do not possess these diverse characteristics, and whether the common name of sleep is justified or not (Piéron 1913:3).

Plants did not sleep, because if they were placed in continuous illumination, their rhythms of motion would persist, but only for a few days. Then they would continue to thrive. Their sleep was thus almost entirely dependent upon external, rather than internal, circumstances, and was not worthy of the name. It was an "anticipatory reflex," rather than a rhythm. The same was true for hibernation, which was not a case of "true sleep," but merely an adaptation to external conditions of cold and lack of food. If the animal was placed in a situation in which these conditions were met, it would no longer hibernate.

Piéron's main experimental subjects were mammals, as their physiological behaviour displayed the greatest amount of variation in the concomitants of sleep, which included changes in circulatory activity, respiration, digestion and secretion. Unlike Mosso or Hammond, Piéron did not feel that such concomitants were causally related to sleep. These rhythms were, like organic memory, simply expressions of the remarkable plasticity, or the adaptive ability, of the organism. In one of his earliest investigations of sleep, Piéron tied together rhythmic behaviour and memory by demonstrating that many rhythms persisted in the absence of the phenomena with which they were associated. In 1907, he and Toulouse studied the inversion of the diurnal temperature rhythm (*l'oscillation nycthémérale*) among night-shift nurses at Villejuif (Toulouse & Piéron 1907). It was already known that body temperature was slightly higher during the day than at night. But the availability of a group of subjects with inverted sleep/wake routines allowed Toulouse and Piéron to see if this rhythm could be changed. They discovered that their subjects actually maintained higher temperatures during sleep when they first changed shifts. The rhythm's normal relationship to sleep and wakefulness was only fully restored five to six weeks later.[8]

It was the study of experimental insomnia in dogs, however, that Piéron felt to be his most important contribution to the problem of sleep (Piéron 1913:v-vii). The technique itself was simple. Piéron attached a dog's collar to a leash that was hung at a point on a wall above the dog. The leash was just long enough to allow the dog to sit or move around a bit. But if the animal tried to lie down, the collar would tighten around its neck, and begin to strangle it [Figure IV]. Thus the dog was forced to maintain consciousness for as long as possible, but it could hardly move, and was subjected to very little stimulus. Like Mosso, Piéron felt he had discovered a way of isolating fatigue. Piéron's methodology, however, eliminated problems which Binet and Vaschide had faced in their own study of fatigue. His use of animals meant that he could be unconcerned about his subjects' emotional state, or their willingness to cooperate in the experiment. There was no question that each animal would perform to the best of its ability, because its very survival was at stake. The necessity of sleep, rather than the desire for sleep or the feeling of sleepiness, became the object of Piéron's investigations. More importantly, all sources of fatigue were internal, and purely physiological. The dog was not exhausting itself by motor activity or by responding to external stimulus. Its only expenditure of energy was in its attempt to stay awake as long as possible. Piéron had discovered a way to make the minimum physiological requirements of consciousness visible. Where functionalist psychology had *defined* consciousness as something that provided an evolutionary advantage, Piéron was *demonstrating* the physiological effects consciousness had on the body.

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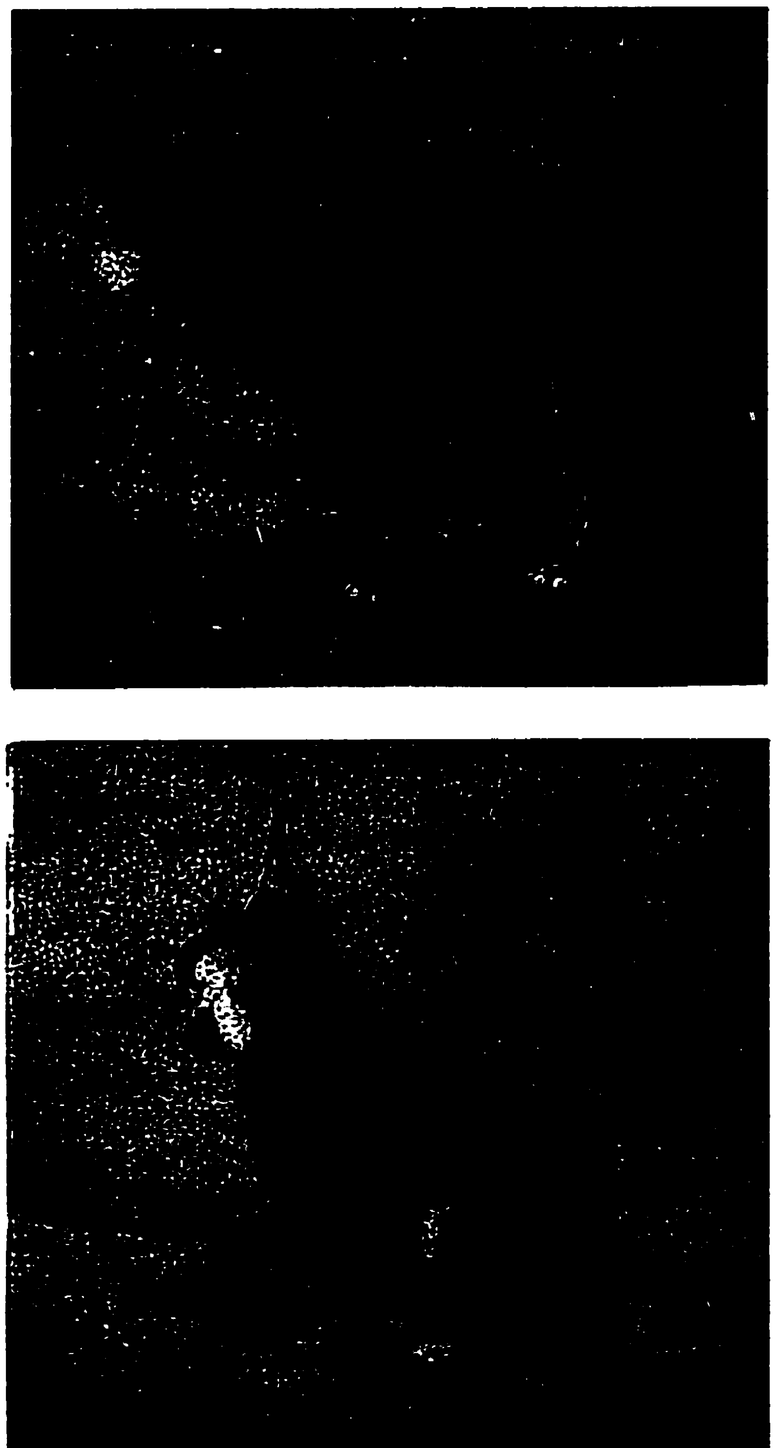
[8]This was only one of many studies of the physiology of work that was conducted at Villejuif. Some of the more notable efforts were made by Jean-Marie Lahy, one of the founders of the physiological study of work in France. His study of typists (1905) was used to demonstrate that women tended to be more efficient at this skill than men (but were paid far less), and his study of Parisian tram drivers (1908) led to a uniform training system in this field (Schneider 1991).

Figure IV
One of Piéron's experimental dogs ("Tunis")
After 248 hours of wakefulness, it can still repond to its name by standing, but soon collapses from exhaustion (Piéron 1913)



Some of Piéron's animals were able to stay awake for up to five hundred hours.[9] When they eventually strangled, or slipped into a coma, Piéron immediately set to work studying the physiological transformations they had undergone. Just as his neo-Lamarckian masters struggled to find identify a chemical mechanism for heredity, so too did Piéron hope to discover the chemical basis of sleep. His methodology fit his research into two important aspects of early twentieth-century physiology—immunology and fatigue research. He attempted to transfer the pathological state of the exhausted animal to a normal, or "fresh" animal, just as Richet himself had transferred immunity from one animal to another three decades earlier. After examining the animal's brain for aberrations (he found some minor changes in the pyramidal cells of the frontal cortex, but Piéron was no anatomist, and this played little role in his argument), he took various parts and fluids from his exhausted dogs, and transferred them to "fresh" animals. Mosso had already conducted similar transfers years earlier, but his experiments had studied animals fatigued by exercise—not by the need to stay awake.[10]

After injecting whole blood, serum, and cerebral pulp into the peritoneal cavities, veins, and brains of normal dogs, Piéron finally discovered his "hypnotoxin" in the cerebro-spinal

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[9]Piéron's efforts did not escape the notice of the anti-vivisection movements that were active in France, England and America at the time (Lalouette 1990; Rupke 1990). Piéron reports that a press campaign was started against his work by a journalist named Gustave Téry. For his part, Piéron stated that "I have always loved animals and only with great reluctance have I performed vivisections" (Piéron 1961:264).

[10]Mosso 1906, p. 119. The immunological paradigm reached its zenith in fatigue research with Wilhelm Weichardt, a physiologist at the University of Erlangen. In 1904, Weichardt announced his experimental attempts to isolate "kenotoxins"—the poisonous substances responsible for fatigue. He withdrew this substance from the blood of fatigued rats and produced an "antikenotoxin," the injection of which enabled immunized rats to outperform normal rats. In 1909, he began spraying the air in a Berlin classroom with the substance, and discovered that the time required for these students to conduct rote calculations was cut in half. Interest in Weichardt's antikenotoxin only subsided around 1914, when several experiments conducted by the Austro-Hungarian army demonstrated that its effects did not seem to differ from those of a placebo (Rabinbach:142-145).

fluid.[11] When he injected it into the fourth ventricle of a normal dog's brain, the animal presented "a more or less irresistible need to sleep" (Piéron 1913:302). This effect could not be due to increased pressure within the brain, he argued, because he had first withdrawn a like amount of fluid from the normal animal. He then followed textbook immunological practise, in order to determine the properties of his "hypnotoxin." Its effects were diminished when the fluid that contained it was heated to around 65°C, or when it was exposed to oxygen. It was precipitated by alcohol, and could be extracted from this precipitate by water, although this extract was somewhat less powerful than the original substance.

What was the relationship of hypnotoxin to fatigue toxins produced by muscular exertion? Piéron argued that the two were entirely independent. Muscular fatigue produced a marked hypothermia, while enforced wakefulness had no such effects. Fatigue toxins were produced in the blood and destroyed there, leaving no trace of damage to the nervous system, which Piéron had demonstrated earlier in experiments he had conducted with Réné Legendre (Legendre 1912). Hypnotoxin, on the other hand, was part of the economy of the nervous system. Its effects could only be demonstrated when it was injected into the brain.

Piéron continued to speak of sleep in terms of fatigue, but described it as a nervous fatigue. It was engendered not by the labours of thought (he refused to use this word to describe animals), but by that of sensori-motor activity. It was this activity that provided the physiological basis of all behaviour—the phenomenon that Piéron, the psychologist, wanted to investigate. In a long and enthusiastic review of *Le problème physiologique du sommeil*, Yves Delage argued that Piéron had demonstrated how sleep provided the physiological basis for reflex action. Piéron had shown how "the work of the nervous system, and in particular the sensori-motor work necessary for the maintenance of equilibrium and the motor reactions appropriate to all the diverse excitations that can appear" was impossible without sleep (Delage 1912:582).

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[11]Schiller mistakenly suggests that Piéron's hypnotoxin was in the blood serum (Schiller 1982).

For Piéron, sleep was at least partly due to mechanical causes. It was the price paid for the responsiveness of wakefulness, which drained the organism of its vitality by producing waste products. This physiological definition of sleep eliminated hypnotism as a kind of sleep. The hypnotic state was in no sense necessary to the organism's survival, consequently, Piéron paid very little attention to it (Piéron 1913:228-235).

But the effects of hypnotoxin could not explain sleep on its own. Sleep could appear in the absence of the endless days of wakefulness necessary to generate suitable quantities of hypnotoxin to put a fresh animal to sleep. Clearly, hypnotoxin needed to work in concert with something else in the organism. It was merely a mechanical trigger. Although Piéron expressed reservations about the value of theoretical speculation, he indulged in it nonetheless. In a lengthy chapter entitled "theories of sleep," Piéron paid tribute to Claparède's ideas. While he avoided the idea of organismic defense so dear to Richet, Piéron acknowledged that it was "Claparède who, in his theory of the sleep-instinct, has found a seductive formula, and diffused a 'biological' conception of sleep, the success of which has not yet been determined" (Piéron 1913:367). Claparède's depiction of sleep as a state of disinterest had, of course, been proposed by many others, including Leibniz, Liébeault, and Bergson. But it was incomplete, because it postulated no mechanism. But, Piéron argued, if it was described in physiological terms as the manifestation of an "inhibitory mechanism," in the brain conditioned to be triggered by an accumulation of hypnotoxin, Claparède's biological theory was indeed the most "complete" theory of sleep available (Piéron 1913:385-386). It explained sleep's rhythm as well as its mechanism.

Piéron remained hesitant about the explicit teleology of Claparède's theory. Sensori-motor fatigue did seem to cause irreparable neurological damage, but did this necessarily imply that the purpose of sleep was to *protect* the organism against such damage? Piéron felt this formulation was meaningless: by the same token, one could say that organisms breathed to prevent asphyxiation, or ate to prevent starvation. Talk of final causes was sterile and misleading. In place of Claparède's finalism, Piéron proposed an explanation of sleep in terms of "anticipation." Hypnotoxin was only an internal contribution to the appearance of sleep—a physiological trigger. Sleep's periodicity, on the other hand, was an example of the persistence of anticipatory rhythms—rhythms that had been established in the organism through the repeated of sleep with relaxation, darkness, and muscular fatigue (Piéron 1913:442-444). It was frivolous to attempt to trace these associations back to some concept of defense, and to then postulate defense as a cause of sleep.

* * * * *

Piéron offered a description of sleep that was more synchronic than diachronic. Following the style of his neo-Lamarckian masters, he devised a method that separated the humoral (physico-chemical) aspects of sleep from its habitual rhythms. But while the latter offered Piéron the potential of aligning psychology with biology under the rubric of organic memory, it was the former problem that was the focus of his research. Where Claparède had formulated the question of sleep as a question about how sleep had evolved, Piéron was content to add yet another factor, hypnotoxin, to the growing list of concomitants of sleep. Sleep was not an *active* phenomenon for Piéron as it was for Claparède: the inhibition of interest did not serve any function. In keeping with his neo-Lamarckian training, Piéron insisted that sleep always had a mechanical cause, be through the accumulation of hypnotoxin, or through an association with relaxation or darkness. Admittedly, these mechanical causes might not always be present—they could act at a distance through the phenomena of anticipatory rhythms, a concept which bore a substantial similarity to Ivan Pavlov's idea of "conditioned reflexes," discussed in the next chapter.

Piéron's arguments cannot be separated from the anti-clerical and anti-metaphysical context of republican France in the early years of the twentieth century. While he could recognize the value of Claparède's arguments, he could not accept them. As Binet had observed in his terse review, Claparède's "biological" theory invoked a teleological premise. And it was exactly this sort of reasoning that social reformers like Piéron wanted to eradicate from French science.

The onset of hostilities in the summer of 1914 reduced sleep to insignificance once again. Piéron hardly returned to the topic for the rest of his career, although he did train Nathaniel Kleitman, whose impact on the field of sleep research will be discussed in chapter five. It was not until the outbreak of a strange "sleeping sickness," which appeared throughout Europe and America just as the war was drawing to a close, that sleep began once again to receive substantial scientific attention.

Chapter IV

Sleep as inhibition & disease
1910-1929

Ivan Pavlov, a world-renowned physiologist, began to study sleep around the same time as Henri Piéron. But where Piéron intentionally set out to separate the humoral phenomena of sleep from the habitual, Pavlov accidentally stumbled over sleep as an obstacle to his study of conditioned reflexes. Yet he soon incorporated sleep into his theory of nervous function, which depended on the interaction of the two equal and balanced forces of excitation and inhibition. Inhibition was a normal and mechanical feature of all nervous activity, and sleep was merely the spread of inhibition across the cerebral cortex. Like Claparède, Pavlov believed sleep protected cortical cells from exhaustion. But his experiments relied on a theory concerning the interaction of stimuli in the cerebral cortex, so he was uninterested in the idea that sleep might be localized in a sub-cortical regulatory centre. His interest in sleep began in the laboratory, but it soon spread to clinical medicine. Sleep cures became an important part of Soviet psychiatric practise for decades after Pavlov's death in 1936.

In 1923, Pavlov brought sleep to the attention of physiologists outside of the Soviet Union in a lecture tour that took him across Europe and the United States. At the same time, sleep was gaining prominence as the most outstanding symptom of a new neurological disease, encephalitis lethargica. First described by Constantin von Economo in 1917, encephalitis lethargica spread across Europe and the U.S. during the 1920s, killing thousands. As an infectious brain disease with pronounced psychological symptoms, "epidemic encephalitis" helped neurologists expand their authority into the realm of public health and developmental psychology. The disease also offered a radical demonstration of what happened when the normal functioning of the "sleep centre" identified by Economo was damaged.

Encephalitis lethargica directed attention towards the periodicity of sleep and away from questions about its relationship to fatigue, or to sensation. Like Piéron's creation of an experimental method unique to the study of sleep, Economo's use of encephalitis lethargica helped turn sleep into an object of biomedical investigation.

Sleep as physiological behaviour

In his 1911 dissertation, Henri Piéron had suggested that the physiological problem of sleep could only be resolved by turning to animal experimentation. Sleep was necessary to life—it was a vital function. The study of vital functions, as Bernard had shown fifty years earlier in his demonstration of the liver's role in the production of sugar, demanded the controlled creation of an artificial pathology. Such experiments could not use human beings. Piéron's decision to starve some twenty dogs of sleep came out of these considerations.

Yet it must be remembered that Piéron was a psychologist by profession, not a physiologist. His use of animal subjects was just as much a reaction against psychology's reliance upon introspection as it was a continuation of the grand French way of studying the body. This reaction against introspection was particularly pronounced in the United States, where it began as functionalism and culminated in behaviourism. Both methodologies proved intrinsically important to the accelerated professional expansion of psychology in the United States, a success story that Piéron, a great proponent of secular and institutional reform, hoped to duplicate in his native France.

Just as Piéron, a young psychologist, was moving towards physiology, Ivan Pavlov, a physiologist well into middle age, was making gestures towards psychology. Pavlov (1849-1936) began his investigations of "higher nervous activity" around 1901, and presented his first paper on conditioned reflexes in 1903, at the XIV International Congress of Physiologists in Madrid (Todes 1997a). According to Pavlov, the concept of "conditioned reflexes" was an outgrowth of his work in the physiology of digestion, for which he won a Nobel Prize in 1904. Pavlov, whose research was then based at the Military Medical Academy at St. Petersburg, divided the digestive process into two phases. The first involved salivation, which in turn triggered the second phase of nervous/chemical secretion that helped break down the food once it entered the stomach. Pavlov and his collaborators dubbed the first phase "psychic secretion," because they noticed that their experimental dogs began to salivate even before the food was placed in their mouth. The sight or smell of meat, or simply placing the dog in its harness, would cause it to salivate.

According to Pavlov, one of his students, A.T. Snarskii, who had been greatly influenced by Wundtian psychology, attempted to explain this phenomenon in terms of the dog's subjective sensations and desires. Pavlov, who constantly harped on the need for psychological science to remain objective, rejected Snarskii's account, and insisted that the dog's behaviour be described in the classical physiological language of reflex. This story of the "birth of the method of conditioned reflexes" has often been repeated by historians (Joravsky 1989; Windholz 1990; Smith 1992). Daniel Todes, who seems to have been one of the first historians to actually examine Snarskii's 1901 dissertation to see if it corresponded to Pavlov's criticisms, has recently suggested that Pavlov completely misrepresented his student's work (Todes 1997a). Snarskii was indeed influenced by contemporary psychologists, but he cited these authorities to argue that psychic secretion was nothing more than a low-level, habitual association. It had nothing to do with the will, choice or judgement that was the object of introspective psychology. Todes suggests that this negative thesis was ultimately unhelpful to Pavlov, who wanted to expand his research into the psychological domain. Thus, he turned to the work of another student, I.F. Tolochinov. Tolochinov's 1902 thesis argued that psychic secretion had to be understood as a reflex that, unlike those reflexes studied by physiologists, had been formed by experience. Pavlov appropriated Tolochinov's thesis, and linked it to I.M. Sechenov's famous 1863 essay on the "reflexes of the brain," an old touchstone for liberal progressives who set their materialism against the Orthodox tsarist regime that ruled Russia. Pavlov's method of conditioned reflexes, which he announced in 1903, gave an experimental context to Sechenov's ideas.

Initially, the notion of a conditioned reflex was rather simple. Pavlov divided all stimulus into two groups: unconditioned and conditioned stimuli. The former were innate, and would elicit a response without any training. Chewing food, for example, would always provoke salivation. Conditioned stimuli only provoked a response when repeatedly associated with unconditioned stimuli. Pavlov demonstrated, for example, that the mere sound of a metronome could make a dog salivate, provided it had been presented with food numerous times in the past.

From this straightforward paradigm, Pavlov and his many collaborators developed a complex and often unwieldy series of experiments, purporting to explain psychological phenomena while using the nomenclature of physiology. Abandoning the mental language of introspective psychology, Pavlov argued that all behaviour could be explained in terms of the dynamics of the conditioned reflex. Within a few years, Pavlov had ceased to describe conditioned reflexes in terms of excitation only. He began to describe his experimental results in terms of inhibition.

Inhibition & sleep

This was a felicitous choice of terms on Pavlov's part. The word "inhibition" had a rich history over the course of the nineteenth century, and, as Roger Smith's unique account of this one word demonstrates, "inhibition" played a crucial role in tying together arguments about moral conduct, self-control, mental order and physiological function (Smith 1992). The word (*Hemmung* in German) first gained popularity as a term describing mental order and controlled conduct in the work of J.F. Herbart, a Professor of Philosophy at Königsberg, during the 1820s and 1830s. Herbart rejected Kant's assertion that psychology could never become a science, because the objects of introspection were only related in time, not in space, as was the case with the objects of the mechanical sciences. Herbart argued that the nature of mind could be rationally deduced from general principles, and to this end created a system of mental dynamics involving the relative strengths of mental elements: a stronger "presentation" would fill consciousness by "inhibiting" weaker ones, which would then become unconscious (Smith 1992:59-65). Thus Herbart married British associationist psychology with the language of dynamics. Inhibition entered psychiatry through the work of the German psychiatrist Wilhelm Griesinger during the 1830s. It became part of reflex theory through the 1845 discovery, by E.H. and E.F.W. Weber, that the stimulation of a frog's vagus nerves slowed, or "inhibited" its heartbeat.

By the end of the century, inhibition (from the Latin root *in* + *habere*, "to hold in") crossed several disciplinary boundaries, as it came to signify any regulatory force that opposed the free reign of excitation, and thus forged order out of chaos. Psychologists, neurologists, psychiatrists and physiologists alike contributed inhibition's expansive meaning. One late-nineteenth century student of American psychologist G. Stanley Hall, for instance, organized his study of restlessness in children around the concept of inhibition. The author of the study, Henry S. Curtis, introduced his topic by expressing a certain misgiving about his own use of the term "inhibition:"

> It is feared by the writer that the title under which this article appears may prove deceptive; so that those who would not be interested may be led to examine it, while those who might find in it something of interest will pass it by unheeded...(Curtis 1898:65).

Curtis worried that his audience would think his paper dealt with the finer points of physiology, when it was in fact based on his observations of children's behaviour in school. He argued that educators should not try to prevent children's nervous and seemingly purposeless movements in the classroom by discipline and book-learning. Instead, they should harness the natural power of inhibition, which Curtis took to being "nearly equivalent to natural selection," by attracting the children's attention (Curtis 1898:65). He left this analogy between inhibition and natural selection undeveloped, except to insist that there was some sort of purposeful struggle going on between the parts of the body during development (Curtis 1898:76). The brain continually exercised a restraining influence over reflex activity, thus creating the physiological and mental order that made for a normal, healthy child.[1]

Inhibition signified order in whatever context it was used. Pavlov first mentioned inhibition in 1909, and he interpreted it primarily in motor terms. The English neurophysiologist, Charles Sherrington, was working along similar lines at the same time, describing inhibition as a

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[1]Curtis was a great advocate of play in public education, and published numerous books as part of the "play movement" that discussed playground construction and the role of play in learning. His work on restlessness and attention was part of the first wave of interest in aspects of children's behaviour currently described as part of Attention Deficit and Hyperactivity Disorder (ADHD). Late nineteenth and early twentieth century psychologists, medical practitioners and educators argued that attention was an innate force, the result of "moral control" produced by the inhibitory power of the will. Defects of the will, however, could be inherited, and could exist apart from any defects of the intellect (Still 1902). Curtis was uninterested in demonstrating that restlessness was a sign of a degenerate will. Instead, he emphasized "attracting" and "harnessing" children's attention through the stimulus of play. I thank David Pantalony for the reference, and for letting me read his valuable study on the history of ADHD.

necessary feature of the integrative action of the nervous system (Smith 1992:179-190). Sherrington, who would share the Nobel Prize in Physiology or Medicine with E.D. Adrian in 1934, eventually came to define the current idea of inhibition as a force that modulates the transmission of nervous signals at the level of the synapse—the chemical junction between the ends of two different neurons. But in 1909, the meaning of inhibition was much less restrictive. Pavlov's use of the term, his treatment of what he called the "cursed problem" of inhibition, was quite different from Sherrington's. As Jeffrey Gray has observed in a rather generous critique of Pavlov's ideas, Pavlov's concept of inhibition was dynamic, not morphological (Gray 1979:90-103). Inhibition was a force that was in every way the mirror of excitation. The two forces opposed and balanced each other; where excitation provoked movement and response, inhibition prevented it. So it is hardly surprising that Pavlov found the most dramatic manifestation of inhibitory force in sleep and hypnotism, where movement and responsiveness appeared to be severely restricted.

Pavlov was by no means the first to describe sleep in terms of inhibition. Shortly after he replaced Claude Bernard as Chair of Experimental Medicine at the *Collège de France* in 1878, Charles-Édouard Brown-Séquard began to propagate his theory of *dynamogénie*, which depicted all diseases in terms of an imbalance of excitatory and inhibitory forces within different organs of the body. His doctrines were well-received by neurologists, who proceeded to construct a therapeutic system based on the idea that disordered reflex activity could be carried by the nervous system and spread to other organs (Shorter 1992:41-42). Brown-Séquard's proposal was a natural outcome of his 1856 discovery that removing the adrenal glands would quickly kill an experimental animal. The function of these glands was unknown at the time, and Brown-Séquard suggested that their purpose was to control and regulate, rather than excite. His observation was one of the first that led to the study of endocrinology, which uncovered numerous connections between various systems in the body.

In 1889, Brown-Séquard attempted to incorporate sleep and hypnotism within his scheme of *dynamogénie*. He had already made his reputation as a critic of Charcot's program of brain localization, so it is not surprising that he tried to interpret sleep and, more particularly, hypnotism, in terms of a balance of nervous forces, rather than the unique work of a morphologically distinct brain centre (Gasser 1995:82ff). He suggested that the various phenomena of sleep and hypnotism, including the closure of the eyelids and the changed position of the eyes, indicated the work of an inhibitory force preventing normal mental activity from taking place (Brown-Séquard 1889).

Likewise, Pavlov may very well have taken his initial inspiration for applying the thesis of inhibition to sleep from his earlier exposure to hypnotism. George Windholz has commented upon the virtual identity between Pavlov's theory and that of Rudolf Heidenhain (1834-1897), who taught Pavlov physiology at Breslau in 1877 and in 1884 (Windholz 1996). In 1880, Heidenhain announced that hypnosis was merely the result of a cortical inhibition that arose from the over stimulation of the visual, tactile, or acoustical senses. Pavlov, who held a deep respect for Heidenhain, offered a similar theory in 1910, just a year after he began describing conditioned reflexes in terms of a balance between inhibition and excitation. Pavlov does not cite Heidenhain on this point, however, and Windholz's suggestion remains at the level of speculation.

Curiously enough, Windholz seems completely uninterested in thinking about the reasons why Pavlov might have started to discuss hypnotism in the first place. In chapter one, I argued that hypnotism was already in decline as an experimental methodology by the early years of the twentieth century. Why would Pavlov raise the question anew? One possible answer might be his age. He was only a year older, for example, than his French colleague Charles Richet, who has been credited with introducing Charcot to the phenomenon of hypnotism in 1875 (Ellenberger 1970). Hypnotism had been one of the most provocative and widespread ways of investigating the nature of the will in France during the last quarter of the nineteenth century. Not surprisingly, members of Richet's generation wanted to offer a definitive solution to this problem. But, as Thomas Kuhn observed many years ago, such solutions are often not forthcoming in scientific research (Kuhn 1962). Instead, the exemplary experimental practises of one generation are frequently supplanted by different practises of the next generation offering greater technical precision, but little definitive resolution of earlier questions.

Windholz, who always wants to set Pavlov in the best possible light, seems to have a Kuhnian reading of history in mind, suggesting that Pavlov simply incorporated hypnotism into his theory of "higher nervous activity" in 1910 and then moved on (Windholz 1996). Similarly, other commentaries, if they mention sleep at all, use it to illustrate Pavlov's idea of the "irradiation of inhibition" (Sherwood 1970; Gray 1979). The question of how hypnotism appeared as a problem to Pavlov remains invisible. Perhaps this is because much of the secondary material on Pavlov has taken an apologetic tone, excusing the Nobel laureate for conceptualizing brain function in terms of a cumbersome and lame theory. Focussing on the role hypnotism played in his work would perhaps put him out of step with his younger physiological colleagues, who, like Piéron, wanted to leave the great debates of the 1880s in the past, where they belonged.

But there is plenty of evidence to support the idea that hypnotism and sleep played an important role in Pavlov's experimental practise, and that this importance persisted throughout the remainder of his career. First, there is the question of why he even bothered to invoke hypnotism in the first place, a remarkable turn for someone who denied introspection any objective value, and who worked exclusively with animals. Pavlov himself offers a strikingly simple explanation for his interest in hypnotism and sleep. It prevented his collaborators and himself from getting their work done. In a lecture entitled "Some Fundamental Laws of the Work of the Cerebral Hemispheres," read before a meeting of the Society of Russian Physicians in 1910, Pavlov remarked that "for many years we noticed that our dogs became sleepy; this interrupted our work, for the conditioned reflexes weakened and disappeared" (Pavlov 1941:I.158). He repeated the story in 1915, at a meeting of the St. Petersburg Biological Society (Pavlov 1941:I.250).

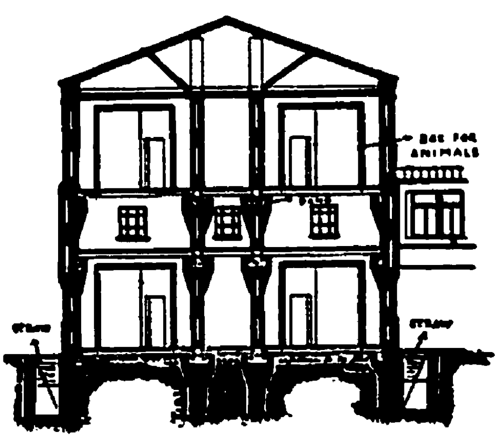
Sleep made the study of conditioned reflexes impossible. As Daniel Todes has pointed out, Pavlov's laboratory was itself a veritable factory of knowledge-production by this time (Todes 1997a, 1997b). Pavlov's students, most of whom were physicians, had little knowledge or appreciation of physiology, and were merely trying to earn a doctorate to advance their career. They were expected to choose a research topic, investigate it, write it up and defend it in a mere two years. Methods were absolutely streamlined—around one hundred of these workers passed through the laboratory between 1891 and 1904, with about three-quarters of them defending their theses successfully. Any obstacle to this progress needed to be dealt with quickly and efficiently. Sleep and hypnotism (which, Pavlov argued, was nothing more than a localized sleep) represented nothing less than a work stoppage on the part of the experimental animals, so Pavlov dealt with it by simply building his theory of inhibition around it.

Pavlov's study of hypnotism and sleep did not come out of a native interest in these topics. Rather, he treated them as problems encountered in the process of knowledge-production. This interpretation of events is strengthened by the fact that, around 1910, Pavlov was engaged in another attempt to refine his experimental assemblage. He hoped to redesign his laboratory in order to eliminate all possible sources of uncontrolled stimulus (Sherwood 1970:206-210). Street noise had to be muted, vibrations dampened, illumination made uniform, and climate controlled in order to minimize any interference with the process of forming conditioned reflexes [Figure I]. The experimenter also had to be isolated from the animal to avoid giving it behavioural cues [Figure II]. Pavlov's new laboratory was only constructed when he began enjoying state support under the "market communism" of Lenin's New Economic Plan during the mid-1920s. But his description of such a laboratory in 1910 indicates that sleep was just one of many factors Pavlov was attempting to refine in his experimental program.

But if sleep appeared to Pavlov almost by accident, it remained an integral part of his method of conditioned reflexes. W. Horsley Gantt, Pavlov's most dedicated American disciple, wrote the following in a letter to the Harvard physiologist Walter B. Cannon, shortly before Pavlov left for a lecture that took him to France, the U.S., and Great Britain:

Figure I
Pavlov's laboratory
note the sand used to minimize unwanted vibrations
(Pavlov 1960)

FIG. 3.—The special laboratory built for the study of conditioned reflexes, Institute of Experimental Medicine, Petrograd.



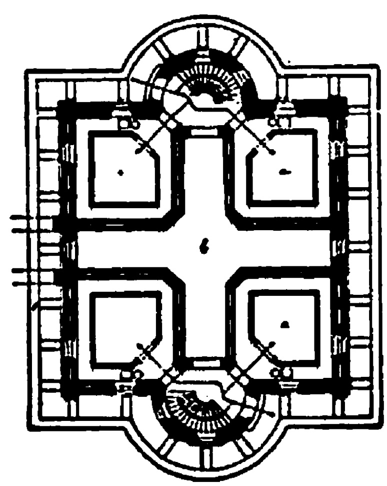


Figure II
Pavlov's laboratory

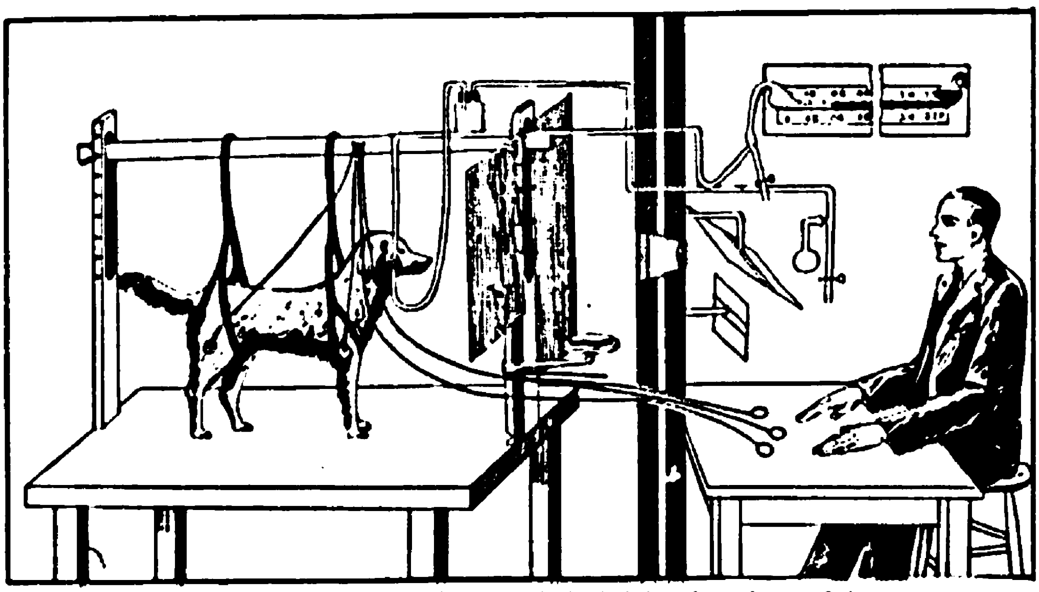


Diagram illustrating experimental method & isolation of experimenter & dog

The animal's section of the double chamber

Experimenter's section of the double chamber

> Dr. Pavlov is very enthusiastic about going to America. He is still very active and alert, and has not missed a single day from work on account of the Revolution, although his chief asst. Dr. Volborth tells me that bullets were coming in thru the windows sometimes. He is still the spirit and soul of physiological work in Petrograd and keeps up a wonderful spirit among his workers. He told me that he considered his most important work that on sleep and the conditioned reflexes; he is also doing some showing [*sic*] that acquired characteristics can be inherited and still a great amount on digestion, although he does not feel so much interest in this.[2]

The importance Pavlov accorded the problem of sleep in his research is indicated by the fact that it is featured in both of the published papers that appeared after his 1923 lecture tour of the U.S. (Pavlov 1923a, b). This interest in sleep endured until the very end. One of his very last lectures, presented in 1935, a year before he died, was entitled "The Problem of Sleep" (Pavlov 1957). Curiously enough, the role of sleep in Pavlov's experimental practise and theory has been almost entirely overlooked. One recent study, which sets Pavlov's work within a context of state-directed, totalitarian science, completely ignores the role sleep played in Pavlov's 1923 lecture tour, and instead focusses exclusively on Pavlov's brief comments regarding some experiments suggesting that conditioned reflexes could be inherited (Krementsov 1997:264). The Soviet attempt to turn Pavlov into a mouthpiece for the neo-Lamarckian beliefs of Lysenko are thus unfortunately perpetuated in this otherwise excellent history.

So what role *did* sleep play in Pavlov's system? His western contemporaries took his ideas seriously. His views received considerable attention in Henri Piéron's 1932 contribution to a massive psychological encyclopaedia edited by George Dumas (Dumas 1932:II.19-39). They were also featured in Claparède's discussion of sleep and wakefulness from the same series (Dumas1934:IV.455-522). As Smith has demonstrated, Pavlov's ideas about sleep are inseparable from his ideas about inhibition (Smith 1992:201). Pavlov claimed to have isolated three kinds of inhibition describing different relationships between the conditioned and unconditioned stimuli encountered in the dog's environment, all of which served to eliminate or reduce the strength of the conditioned response, measured as drops of saliva (Dumas 1932:II;

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[2]Letter, Gantt to Cannon, May 31, 1923 (WBCA 37.462)

Smith 1992). "External" inhibition referred to the effect a powerful new stimulus had over another weaker one. A dog, for example, could be conditioned to reject food if the food was repeated accompanied by an electrical shock. "Internal" inhibition described the fate of conditioned reflexes over time. If the unconditioned stimulus did not consistently accompany the conditioned stimulus, the conditioned response would eventually disappear. The same effect could be had if a new stimulus was presented at the same time as a conditioned stimulus, but without the unconditioned stimulus. This "extinction" of responses was never complete, however. Pavlov noted that conditioned reflexes could often be revived, provided the conditioned stimulus was once again accompanied by an unconditioned stimulus.

Sleep, Pavlov argued, was a third kind of inhibition (Pavlov 1923a, b, 1928:250-254). It appeared because of the repeated presentation of a single stimulus. Pavlov thought that sleep and hypnotism illustrated a particular principle of internal inhibition—the law of irradiation, which Pavlov's contemporary, Vladimir Mikhalovich Bekhterev, described as the most important law of the activity of the nervous system (Dumas 1932:II.31). Excitation and inhibition, Pavlov argued, irradiated across the surface of the cerebral cortex, forming a "mosaic" of points that guided the behaviour of the animal at every turn (Pavlov 1960:152-233). Every stimulus the animal encountered was focussed on a single point on the cortex, and in this sense Pavlov was a staunch localist. Many of his experiments assumed that the surface of the skin had an analogous "cortical projection," with each point on the skin corresponding to a unique point on the cortex. The time it took the "wave" of excitation to spread across the cortex could be calculated simply by creating an inhibited point on the animal's skin, and then measuring how long it took for this inhibition to spread out on the skin itself.

But stimulation was not simply a matter of excitation, and, in his explanation of the reciprocal influence of excitation and inhibition, Pavlov left the arena of strict localization and entered the fray of holism. "I shall not commit a great error," Pavlov said in 1910, "if I liken these two phenomena [exitation and inhibition] to positive and negative electricity" (Pavlov 1928:I.156). Excitation could not exist without a corresponding inhibition, just as electricity could not flow without a positive and negative charge. Thus, every excitation involved a wave of inhibition that irradiated across the cortex. With the repeated presentation of the same stimulus, this inhibition became more and more generalized, finally ending in a state of sleep, which protected the cortical cells from damage otherwise resulting from extreme fatigue. These stimuli, as Pavlov discovered in 1910, were everywhere in his laboratory. Even the harness that kept the dog from moving around became incorporated as a conditioned reflex, one that could turn an active, responsive dog into an experimental liability.

The irradiation of sleep in psychiatry

Pavlov's theory of sleep was largely rejected by positivist psychologists like Piéron, who dismissed Pavlovian terminology as unnecessary and even useless (Dumas 1932:II.39). Likewise, Claparède suggested that Pavlov's theory of inhibition could not really explain anything at all about normal sleep, which was not induced by prolonged excitation, but was largely a periodical phenomenon (Dumas 1934:IV.509). Psychiatrists and neurologists, however, seemed to be working with ideas very similar to those of Pavlov. In the early 1920s, Jakob Kläsi, a psychiatrist working under Eugen Bleuler at Burghölzi in Zurich, attempted to treat schizophrenia with a new hypnotic drug called Somnifen (Windholz & Witherspoon 1993:83-93). The use of hypnotics to arrest the course of mental illnesses dated back much earlier, to the use of sodium bromides to treat "hysterical epilepsy" during the 1850s (Shorter 1997:200-207). But the interwar period saw a renewed effort to treat schizophrenia and psychoses with barbituates, a new group of compounds which had been mass-marketed as a treatment for insomnia around 1905. The popularity of "sleep therapy" roughly coincided with a number of other physical cures, including the induction of malarial fever in neurosyphilitic patients by Julius Wagner-Jauregg in Vienna (1917), insulin coma (1933), Metrazol convulsion (1934), and electroshock (1938) therapy for schizophrenics by Manfred Sakel in Vienna, Ladislas von Meduna in Budapest, and Ugo Cerletti in Rome, respectively. These therapies, along with the psycho-surgical procedure of lobotomy, pioneered by the Lisbon neurologist Egas Moniz, the

American neurologist Walter Freeman, and his neurosurgeon sidekick, James Watts, represented a reaction to the growing number of incurable schizophrenics being "warehoused" in asylums (Shorter 1997). They were simultaneously a cure, an exploitation of human subjects whose consent was largely inconsequential, and the solution to the administrative difficulties of managing a growing asylum population with insufficient resources (Grob 1983, 1994; Valenstein 1986; Shorter 1997).

Windholz and Witherspoon do not report that Kläsi ever invoked Pavlovian theory to justify his practise of putting patients to sleep for as much as eleven days at a stretch (Windholz & Witherspoon 1993). Pavlov, however, began recommending sleep therapy in the Soviet Union in the 1930s, a period during which Soviet asylums were being filled in the wake of the often brutal implementation of Stalin's first two five-year plans. Pavlov himself argued that hypnotic symptoms were a natural reaction to the neuroses, so the induction of the same symptoms must be healthy. He promoted the use of the older bromides in psychiatric therapy (Gray 1979:121). Sodium amytal, a barbituate, was introduced to Soviet psychiatry by Yagoda and Ilinskii in 1937 (Wortis 1950:162). In postwar Soviet asylums, patients were often narcotized for a period of one-and-a-half to two days, which was followed by an equal period of stimulus by induced convulsions or fever. After the unprecedented slaughter of the Great Patriotic War, one of Pavlov's students, Ezras Asratian, used sleep therapy to treat phantom pains and traumatic shock among the wounded. Such therapies were part of the enduring Pavlovian legacy, which included not only the spread of the method of "classical" conditioning among American psychologists, but also the use of behaviour therapy in American psychiatry (Gray 1979:130ff).

Pavlov's method of conditioned reflexes offered little to physiologists or psychologists interested in sleep. Pavlovian experimentation relied on the induction of sleep, which was virtually equivalent to a revival of hypnotism as an investigative tool. From the perspective of sleep, the greatest difference between the method of conditioned reflexes and the hypnotic studies of the 1880s was in Pavlov's use of animal, rather than human, subjects. Naturally, this invoked a radical shift in the language used to describe the phenomena. But an almost complete disregard for the periodic nature of sleep remained, and this was considerably out of step with the developments that had produced Piéron's 1911 dissertation. Piéron had approached sleep as a problem demanding the creation of a unique methodology. Pavlov, on the other hand, ran into sleep as an obstacle that interrupted his experimental labours. He accounted for it by simply assimilating it into his theory of inhibition. This difference is further illustrated by examining the legacy of the graphical method. Piéron's work at Villejuif had relied on the devices that Marey and Mosso had contrived to observe the temporal course of phenomena without disturbing them. This certainly had an impact on his study of sleep, which depended upon just this sort of "non-interventionist" thinking. For Pavlov, the graphical method was little more than an afterthought. Drops of saliva measured the relative strength of a reflex, with practically no regard for the nature of this strength as a function of time. In a lecture delivered at the Military Medical Academy in Petrograd in 1924, Pavlov indicated his changing attitude towards the graphical method:

> I believe the aggregate of facts given in the present lecture can be taken as sufficient proof of the view that sleep and internal inhibition are fundamentally one and the same process. I personally do not know, up to the present, of a single fact in all our researches which contradicts this conception. It is to be deplored, however, that we have as yet no reliable graphic method of registration of sleep. On some occasions we tried to apply for this purpose a graphic registration of the position of the head of the animal. A perfection of some such method for the graphic registration of sleep is greatly to be desired, so that the whole evidence regarding sleep can be expressed in an exact quantitative manner (Pavlov 1960:263-264).

But Pavlov continued to take the induction of sleep through conditioning as the paradigm of what sleep was. Just two years before Pavlov delivered this lecture, however, a young physiologist in Chicago was taking up the graphical method in his own study of sleep. Nathaniel Kleitman, whose work will be discussed in chapter five, had already been in communication with Pavlov by this time, and was busy positioning his own research against that of the master of the conditioned reflex.

Pavlov's influence was certainly felt in the dissemination of behavioural studies that burst onto the American scene in the period just before the First World War. Certainly behaviorism would likely have blossomed in the U.S. with or without the Pavlovian contribution to the field. But Pavlov, a Nobel laureate, lent a substantial amount of scientific credibility to this enterprise, just at the time when American psychologists were reconstructing their field towards a professional orientation. J.B. Watson advocated the method of conditioned reflexes as a replacement for introspection in 1915. Shortly after, he announced that behaviour, rather than consciousness, was the proper object of a scientific psychology (Watson 1913, 1916). Robert Yerkes, who was doing comparative psychology in the Department of Philosophy at Harvard, may well have introduced Pavlov's method of conditioned reflexes to an American audience in 1909, because of his conflicts with his introspectionist overlord, Hugo Münsterberg, who was reluctant to promote him (O'Donnell 1985; Wight 1993). Pavlov's name offered scientific credibility to those engaged in a disciplinary struggle to determine the nature of the psychological field.

Behaviourism had little to offer the study of sleep, however. While functionalist psychology, which analysed the biological significance of consciousness, at least provoked some controversy over the question of sleep, behaviourism simply ruled consciousness out of court. Sleep had very little to offer in the way of behaviour, as it had almost always been framed in terms of a loss or diminution of consciousness (Peter 1996). The question of sleep as an experimental problem was thus left primarily to physiologists interested in understanding the biological foundations of conscious experience.

Sleep as disease

The appearance, however, of a series of epidemics of sleeping sickness, dubbed "encephalitis lethargica" by the Viennese neurologist Constantin von Economo in 1917, offered an exceptional opportunity for medicine to take up the question of sleep anew. The irony that sleep appeared as an epidemic disease just as it was being proffered as a psychiatric cure should not be lost here. It aptly illustrates the ambivalent nature of sleep: a sweet restoration of life's energies on the one hand, and a frighteningly close relative of death on the other. And like sleep therapy, the outbreaks of encephalitis lethargica offered a renewed opportunity for the application of Pavlov's theory of sleep, as neurologists struggled to frame this new disease in the language of inhibition. These epidemics brought sleep to the attention of neurologists, bacteriologists, physiologists, psychologists and psychiatrists across the world. Massive amounts of funding and biomedical resources were turned towards the investigation of sleeping sickness and the devastating effects it often had on its victims. Encephalitis lethargica earned sleep a place in the biomedical science of the twentieth century. It gave sleep a physical location in the sub-cortical brain through the neuroanatomical studies of Economo.

Curiously enough, historians have generally failed to investigate the role of encephalitis lethargica in the growth of neuropsychiatry, preferring instead to focus on the worsening institutional conditions in asylums, the spread of psychoanalytic theory, and the emergence of a new wave of organic therapies for mental illness in the years between the wars (McHenry 1969; Castel, Castel & Lovell 1982; DeJong 1982; Grob 1983, 1994; Valenstein 1986; Aird 1994; Shorter 1997). A possible reason for this exclusion might be the relative lack of rhetorical value of encephalitis lethargica for historical debate. It cannot be depicted as a glorious triumph of modern medicine, nor can it be represented as a poignant moment of insight for psychoanalysis. No sensational therapies were ever devised to cure the disease, leaving little for historians to condemn or defend. This non-reaction to the epidemics of encephalitis lethargica by historians is almost an absurd mirror, parodying the stupefied countenances of the chronic victims of the disease.

The few exceptions to this rule are a handful of historical articles written by neurologists with a clinical interest in encephalitis lethargica (Ward 1986; Biéder *et al.* 1989; Sacks 1990). These papers offer several useful references, but provide little in the way of analysis. Sacks's clinical studies of the effects of levodopa (L-DOPA), a chemical analogue of dopamine, on a group of patients suffering from post-encephalitic parkinsonism during the late 1960s are well-known. They provided him with material for a book and a popular film (Sacks 1973). Biéder *et al* have provided a review of all papers on encephalitis lethargica published by the *Société clinique de médecine mentale* between 1920 and 1929, but leave this material unanalysed, and without a historical context. Ward, like Sacks, emphasizes the enormous difficulty that neurologists faced in trying to diagnose this new epidemic disease. Both recount the history of the disease in a tragic mode: for Sacks, it was a tragedy for the patients, locked away and rotting for decades in asylums; for Ward, the disease was a tragedy for scientific medicine, which failed to identify viral origin for the disease, or even to come up with any useful therapeutic measures. Ward's narrative is centred exclusively in the present. The neurology and psychiatry of the 1920s, he argues, suffered from a series of "deficiencies" that prevented the advance of knowledge:

> Clinical description was hampered by the still relatively early stage that clinical psychiatry and neurology had by then reached. Important explanatory concepts were lacking, especially those relating the ascending reticular formation to sleep, limbic structures to emotion, and the hypothalamus to endocrine and autonomic function. The development of the electroencephalogram came just too late for its application to the acute disease. Virology and immunology were in their infancy, and clinical neuropharmacology was about to be born. These deficiencies go some way toward explaining the somewhat meager references to encephalitis lethargica in current textbooks of psychiatry and neurology. A disease that was of momentous importance two or three generations ago has been relegated to merely antiquarian significance (Ward 1986:223).

This is a striking failure of the historical imagination. Granted, the biomedical investigators of the 1920s keenly felt their inability to solve the problem of encephalitis lethargica. But their efforts can only be considered "deficient" if we assume the story of the disease must be written from the perspective of late twentieth-century diagnosis and therapeutics. Other narratives are certainly possible. If the history of the disease is written with the disciplinary development of neuropsychiatry and sleep research in mind, it is clear the actors themselves agreed that encephalitis lethargica represented a great opportunity. More importantly, assuming the *presence* of a context rather than the *absence* of scientific knowledge allows this interesting and largely unwritten episode in the history of disease to emerge in full relief.

An analogue: narcolepsy

The pivotal role encephalitis lethargica played in turning sleep into a concrete biomedical problem—a "real question" in Nicholas Jardine's terms—can be illustrated by briefly examining the fate of an earlier sleep-related disease, narcolepsy. Pathological somnolence, the major symptom of narcolepsy, was treated by neurologists just as sleep itself had been treated by physiologists and psychologists. Sleepiness was merely a symptom that represented a pathological modification of the normal state of the body. It did not exist as a biomedical entity in its own right. Narcolepsy did not explain anything at all about sleep. Rather, it was encephalitis lethargica that reified sleep as a phenomenon of self-regulation in Economo's discovery of a "sleep centre."

In 1880, a new kind of disordered sleep—narcolepsy—emerged from the spate of nervous diseases that seemed to plague late-century France. It was first identified as a disease by Jean Baptiste Edouard Gélineau (1828-1906), a former navy physician and general practitioner from Rochefort, an important naval base on the Atlantic coast that was established at the mouth of the Charente river by Louis XIV in 1667 (Gélineau 1880; Passouant 1981a, b; Schiller 1982). Gélineau was not an academic physician, and never held a post at a university or medical school. He had studied surgery at the Rochefort Navy Medical School, and collected enough data during his trips around the Indian Ocean to earn him a Doctoral degree in Medicine from Montpellier in 1858. He left the navy two years later, and set himself up as a private practitioner in Aigrefeuille d'Aunis, a small town near Rochefort. It was here that Gélineau made his reputation by peddling a pharmacological concoction called *Dragées Gélineau*, which he claimed was a remedy for epilepsy and other nervous disorders. The pills, which he introduced in 1871, after serving in the army during the Franco-Prussian war, contained a combination of bromide, antimony and picrotoxin—essentially a combination of "uppers" and "downers." The tablets were well-known in French medical circles, and quite possibly made Gélineau a small fortune.

He left for Paris in 1878, where he set up shop in a private neurological clinic. Gélineau, relatively unknown in Paris and unaffiliated with Charcot's circle, was not long in finding a star patient that would launch his diagnosis of narcolepsy. The patient, "G.," was a male wine-barrel retailer, 38 years old, who had a rather unremarkable medical history up until the two years before he came to Gélineau. He began to suffer from a series of "sleep attacks," which were preceded by a feeling of "deep heaviness," and of "a heavy load on the forehead and deep in the eyes." He fell asleep seconds later. The attacks could occur at any time: in the middle of a meal, at the theatre—even half-way through a sentence (Passouant 1981a).

Gélineau soon concluded that this was not epilepsy. For one thing, his bromides had no effect! Also, he could be awakened from his attacks as readily as he could be from normal sleep. But G. also described an unusual symptom: his attacks often followed any strong emotional expression. He would burst into laughter after concluding a good professional deal, and his legs would suddenly buckle beneath him, and he would fall asleep. Embarrassment would also provoke this cataplexy, which Gélineau dubbed *astasie* (the inability to stand up). Once, as he stood "around the monkey's cage, rendezvous of the curious, the maids, the soldiers," he had suffered from an attack (as quoted in Passouant 1981a:241). Everyone stood around him, laughing, which only made the situation worse.

This was clearly not an example of normal sleep, but neither did G.'s symptoms map onto any other diagnoses, such as syncope or agoraphobia. Borrowing from a claim, made by E.F.A. Vulpian, who was then Professor of Pathology at the Salpêtrière, that there was a centre for emotional associations in the mid-brain, Gélineau concluded that he had discovered a new disease which he described in terms of a shock delivered to this same region, where a "sleep centre" was also located. He dubbed the disease "narcolepsy."

Gélineau's disease was almost immediately brought down to the level of a symptom by the elite neurologists of Paris. Even after Gélineau published fourteen new cases of the illness a year later, narcolepsy was thoroughly rejected as a disease entity (Schiller 1982). Gilbert Ballet, who had replaced Charcot as *chef de clinique* at the Salpêtrière, set the tone in 1882: "Affirming that narcolepsy is a neurosis simply perpetuates the annoying tendency to willingly content oneself with a label [*une étiquette*] without ever trying to uncover what it is that this label hides" (as cited in Passouant 1981b:133). Ten years after Gélineau first announced the disease, it remained practically invisible. Indeed, in an 1890 address to the Association of American Physicians, Silas Weir-Mitchell, the father of the "rest cure," did not breathe a word about narcolepsy or Gélineau, despite the fact that his talk ran the gamut of sleep disorders recognized at the time (Weir-Mitchell 1890). Things did not fare any better for narcolepsy after Gélineau's death in 1906. Even Piéron, whom I have depicted as a pivotal figure in the creation of sleep as a scientific entity, rejected narcolepsy as anything more than a symptom (Piéron 1913:196).

Schiller argues that narcolepsy became accepted as a disease once the epidemics of encephalitis lethargica "triggered a rising interest in the medical rather than the psychological problems of sleep" (Schiller 1982:387). But it is not clear what, exactly, Schiller means by "medical" in this instance. As I demonstrated in chapter one, medicine and psychology were never far apart in their study of sleep through the induction of hypnotism and the pathology of insomnia. And as Piéron's work admirably demonstrated, sleep could be considered to be a *physiological* problem, even by psychologists, in the years before the Great War. Schiller's claim does not explain why Weir-Mitchell, a physician, did not discuss narcolepsy in 1890, nor does it account for the interest in narcolepsy expressed by R. Henneberg, a Berlin neurologist, who complained that "Gélineau's description of the disease of narcolepsy in 1880 has hardly received any general recognition" (Henneberg 1916:282). Henneberg's comment came from a paper read before the Berlin Society for Psychiatry and Nervous Disease in 1916, a year *before* Economo identified encephalitis lethargica.

Schiller is mistaken to suggest that there was a shift from the "psychological" to the "medical" problems of sleep with the onset of encephalitis lethargica, a disease which was embraced by neurologists, virologists, psychologists, and public health officials alike. On the contrary, these investigators were attempting to incorporate psychological phenomena (the role of the will in maintaining wakefulness, the relationship between juvenile delinquency and brain damage) in their biomedical investigations. Sleep itself was undergoing a reassessment, inaugurated by the accounts of sleep as a protective and active function variously described by Claparède, Piéron and Pavlov, all of whom attempted to frame the problem of sleep in *positive*, but nonetheless *psychological*, terms. Doubtless, encephalitis lethargica focussed scientific and popular attention on sleep, just as it brought new resources and experimental tactics to the field. But it cannot by itself account for this shift in how sleep was perceived.

Sleeping sickness, 1917-1929

A curious form of sleeping sickness appeared among some of the patients in the neurological clinic of Julius Wagner-Jauregg early in 1917, shortly before he began experimenting with fever therapy. The hospital was filled with soldiers, whose brain injuries or neurosyphilis represented enormous potential for clinical research. These patients proved to be an excellent source of material for investigating neurological function, both for the program of brain localization (as shrapnel injuries left a visible trace on the skull and brain), and for the treatment of insanity by organic methods (as soldiers, then as now, are pliable and accessible sites of experimental inquiry).

The seven patients that appeared in January of 1917, however, were rather different from the rest of the population of Wagner-Jauregg's clinic. First of all, they were civilians, with no traumatic brain injury. Secondly, they all displayed a series of symptoms, including fever, delirium, vision disorders, and convulsive movements, that were not amenable to any diagnosis. At that time, Wagner-Jauregg had at his disposal a talented young neuroanatomist, Constantin von Economo, who had recently returned to Vienna from serving in the air force in the south Tyrol. Before the war interrupted his research, Economo (1876-1931) had worked under Wagner-

Jauregg since 1906 (Economo & Wagner-Jauregg 1937; Bogaert & Théodoridès 1979).[3] Economo, who was probably familiar with Gélineau's narcolepsy, if only as a symptom, soon recognized a common element to all these patients's behaviour: they all slept excessively. They could drop off while sitting up, or even while standing. Some remained asleep for weeks at a time. This was not a case of coma, however, as the patients could be roused with little difficulty. Their thoughts did not seem particularly irrational or disordered to Economo, and after the non-specific, "flu-like" symptoms wore off, his patients were left with a variety of motor symptoms, including rigidity and akinesia, and ophthalmoplegia (involuntary movements of the eyes). But excessive sleep was the most pronounced symptom, and it was on this basis that Economo had christened the mysterious disease "encephalitis lethargica," and wrote up seven cases for the *Wiener klinische Wochenshrift*.[4]

Sleeping sickness was not in any way new. In fact, it was well-known around the world not only to medical practitioners, but to the general public. A variety of the disease had plagued Central Africa since the late 1880s, claiming hundreds of thousands, if not millions, of victims. It was popularly known as "sleeping sickness," but its medical name was trypanosomiasis, named after the organism, *Trypanosoma gambiense*, which had been identified in 1905 (Lyons 1992). It was well-publicized in Europe, because the disease, which caused a morbid sleepiness in its victims, was an on-going concern for the ruling colonial powers in Africa. Environmental measures to eliminate the tsetse fly, which was the main disease vector, had been taken since

[3]These are the only two biographies of Economo in English. Both are adoring depictions of Economo, who died at a relatively young age (55) of angina, and offer little in the way of historical context. One was written by his widow and Wagner-Jauregg himself, and was in part the transcript of a celebratory radio address given by Wagner-Jauregg shortly after his student's death (Economo & Wagner-Jauregg 1937). The second relies mainly on the first (Bogaert & Théodoridès 1979). Their greatest value is certainly as translated collections of several of Economo's papers.

[4]Constantin von Economo, "Encephalitis lethargica," *Wiener klinische Wochenshrift* **30** (1917): 581-585. The paper is translated, by Robert H. Wilkins and A. Brody, minus six of the case descriptions, in Bogaert & Théodoridès 1979, pp. 79-84.

around 1900. But a treatment was not forthcoming until the development of Bayer 205 in 1922, at which point the German Colonial Society made headlines by suggesting that the rest of the world could benefit from the new drug, provided that Germany could have some of her African colonies returned.[5] As Maryinez Lyons has demonstrated in her engaging social history of the disease, trypanosomiasis was an important geopolitical, economic, epidemiological and bacteriological concern (Lyons 1992). In neurological terms, however, the disease seems to have counted for nothing. This is not simply because there were probably very few (if any) neurologists in Central Africa at the time. They could have been sent to investigate the disease along with the bacteriologists who came from as far afield as Canada. But this did not happen, probably because the colonial rulers did not see the African victims of trypanosomiasis in terms of brains or minds whose agency was being extinguished, but simply as bodies and populations that needed to be governed and regulated. This lack of concern for psychological questions made death, rather than sleep, the intriguing feature of African sleeping sickness for Euro-American biomedicine.

Encephalitis lethargica changed all that. Mysterious epidemics of sleeping sickness had been reported in Europe before: strange cases of *Schlafkrankheit* had struck Tübingen in 1712; the curiously-named *nona* that struck Northern Italy in 1890 had been widely reported in the medical and popular press (Bogaert & Théodoridès 1979: 39-41, 85-96; Schiller 1982). During this epidemic, Mauthner, a Viennese ophthalmologist, had noticed that the illness featured opthalmoplegia. He argued that the it was almost certainly a polioencephalitis, similar to epidemic meningitis, and that it was also a pressing public health concern (Mauthner 1890; Schiller 1982). Economo, who presented his clinical observations in conjunction with his neuropathological examinations, agreed with Mauthner that *nona* and encephalitis lethargica

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[5]The *New York Times* carried a number of articles on this topic, all with the sufficient quota of outrage and indignation against what the Germans were doing to the sanctity of the scientific enterprise. See the stories and editorials of January 10th, 1923 (II, p. 14), and January 27th (VIII, p. 4), 30th (p. 1), 31st (p. 14), and February 1st (p. 16) of 1924.

were probably one and the same disease, a feature of which was damage to a specific area in the mid-brain.

Without getting into the intricacies of the problems associated with such retrospective diagnosis, however, one thing is abundantly clear: Mauthner's arguments had little or no bearing on the fortunes of sleep physiology, while Economo's certainly did. If encephalitis lethargica (or *nona*) had not changed between 1890 and 1917, sleep itself certainly had. In 1891, Mosso, one of the most prominent physiologists of the day, had used the graphical method to describe sleep in terms of the physiology of sensation. His investigation, which relied on human subjects, was little more than an outgrowth of his study of yet another sensation, fatigue. By 1917, sleep had assumed a much more robust distance from questions of the power of the mind. With the widespread use of animal models by psychologists, sleep had been transformed into a question about brain function. Piéron had forged a chemical basis for fatigue theories of sleep. Claparède had propagated the claim that sleep was not the annihilation of consciousness, but its active defense of the brain. Hypnotism had been scrapped as an experimental tool. Pavlov had defined sleep in terms of a cortical inhibition unrelated to the introspective question of the nature of the will. When Economo framed encephalitis lethargica in terms of sleep, sleep itself had changed. Periodicity was replacing fatigue as the feature of sleep that needed to be explained. The great value of this new disease to the growth of sleep physiology was that it provided, in epidemic proportions, a human analogue to the animal model of sleep that had only recently been established. There were no "deficiencies" here, but rather a plethora of new ideas and experimental assemblages.

Encephalitis lethargica & sleep as inhibition

Economo did not immediately make the connection between encephalitis lethargica, inhibition, and the existence of a sleep centre in the brain. His early studies of encephalitis lethargica were first and foremost works of clinical pathology. Economo was a skilled neuroanatomist and he used his talents to defend the ontological status of encephalitis lethargica, by distinguishing it from the encephalitis caused by influenza (which was then raging across most of the world), food poisoning, tuberculosis, and meningitis. Thus his first paper was filled with descriptions of symptoms, coupled with post-mortem anatomical observations. In particular, he observed "a tremendous infiltration by small cells of the vessels in the grey matter of the third ventricle, the area of the ocular nuclei, around the aqueduct of Sylvius and the floor of the fourth ventricle," in the brains of his patients (as cited in Bogaert & Théodoridès 1979:83). He said nothing about the nature of normal sleep.

It was only in his second article that he took a position regarding the existence of a sleep centre in the sub-cortical brain (Bogaert & Théodoridès 1979:85-96). "The explanation of the symptoms of the strange desire to sleep is difficult," he began. It could be due to a specific toxin produced by the virus, or perhaps it was a result of increased brain pressure, as is seen in hydrocephalus and in meningitis. But the nature of the sleep in encephalitis lethargica was too similar to normal sleep to warrant either of these explanations. The toxic theory of sleep was no more able to explain the ability of encephalitis patients to wake up (they would usually just fall back asleep again) any more than it could explain this phenomenon in normal sleep. And the somnolence of increased brain pressure was like a coma. Economo's patients could usually wake up and respond clearly when aroused—they just simply fell back asleep when given the least opportunity. Mauthner had suggested that sleep was caused by a functional break between the cerebral cortex and the lower regions of the nervous system. His evidence was strictly analogical: the droopy eyelids and occasional paralysis of the eyes experienced by normal sleepers were chronic symptoms of *nona*, and ocular movements were known to be controlled by certain sub-cortical brain centres. The pathological eye movements and constant sleep must be the result of damage to the same region of the brain.

This was nothing more than an aside in Economo's second paper, which was published in 1917. It was not until 1926 that he began to articulate a formal theory of sleep based on his work in encephalitis lethargica. The gap between 1917 and 1926 was an important one. In the interim, encephalitis had spread throughout Europe and North America, striking tens or even hundreds of thousands of victims.[6] Pavlov's theory of sleep had also become well-known to every physiologist through his Euro-American lecture tour of 1923, which took place shortly after a series of major outbreaks in North America. Equally important, Economo's own reputation as a talented brain anatomist had been solidified by the publication of his book, *The cytoarchitectonics of the human cerebral cortex* in 1925 (Bogaert & Théodoridès 1979).

In a lecture delivered to the College of Physicians and Surgeons at Columbia University in 1929, Economo described his new theory of sleep in detail. He began by noting that "the extinction of consciousness, this most striking symptom of sleep of man and of higher animals," had "appeared until recently as the essential characteristic of sleep and as the only one which demanded explanation" (Economo 1930:249-250). This had generated a theory of sleep that was based upon "lack of stimuli," the textbook evidence for which had been provided by Adolf Strümpell, a Professor of Psychiatry at Leipzig, in 1898. Strümpell's patient suffered from an almost complete cutaneous and sensory anaesthesia. Only his left ear and right eye remained responsive to stimulus, and as soon as Strümpell closed the eye and plugged the ear with cotton, the patient immediately fell asleep.

Economo argued that Strümpell's patient was merely acting on suggestion. Besides, he argued, "lack of stimuli" theories of sleep were unsatisfactory in other ways: they could not explain why sensation was undisturbed in pathological sleep; they described sleep as a change of consciousness, which could not explain the sleep of certain plants; they could not account for the alternating sleep and wakefulness of animals without a cerebrum or anencephalic babies; nor could they explain the qualitative changes that went on in sleep (the change in sugar and calcium content of the blood, the narrowing of the pupils, and the like).

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[6]The first report of the Matheson Commission (1929) claimed that by 1928, nearly 85,000 cases of encephalitis lethargica had been reported world-wide. The authors suggested that this represented only a fraction of the actual number of cases, because the disease was likely to be incorrectly diagnosed, as it was new and unfamiliar to many clinicians.

Most importantly, however, these theories could not account for the periodicity of sleep. The regular diurnal rhythm—the one aspect of sleep that was often inverted, or completely destroyed, in encephalitis lethargica, regardless of whether the dominant symptoms were pathological sopor or chronic insomnia—was left unexplained, as were the many variations in organic functions (respiration, heartbeat, bodily secretions). The "toxic fatigue" theories of Piéron were no better, as they could not explain the initiation of sleep without fatigue, or sleep's "reversibility" (the possibility of being aroused at any time).

Economo was considerably more enthusiastic about the concept of hormonal regulation, first proposed by two Italian neuropathologists, Mingazzini and Barbara. Economo approved of their idea that one type of hormone dominated during sleep and another dominated during wakefulness, because it redefined sleep as an active state of the body, rather than the elimination of such activity (Economo 1930:253). But the idea of a sleep centre deep in the brain was still anathema to neurologists and psychiatrists alike, who preferred to describe sleep in terms of cognitive function, and therefore looked to a modification in the activity of the cerebral cortex as its origin.

The advent of encephalitis lethargica changed all that, Economo thought, because its major symptom—the destruction of a normal sleep/wake rhythm—was always accompanied by brain damage.[7] Economo's proposal was that there was a regulatory centre for sleep deep in the brain, lying close to other centres that controlled vegetative functions such as respiration [Figure III]. This regulating centre, which Economo, who was both an aviator and ardent motorist, described as a *Schlafsteuerungszentrum*, or "sleep steering centre," coordinated the various physiological and psychical changes brought on in the sleep state. How did this centre effect these changes? Through inhibition, a concept which Economo borrowed directly from Pavlov

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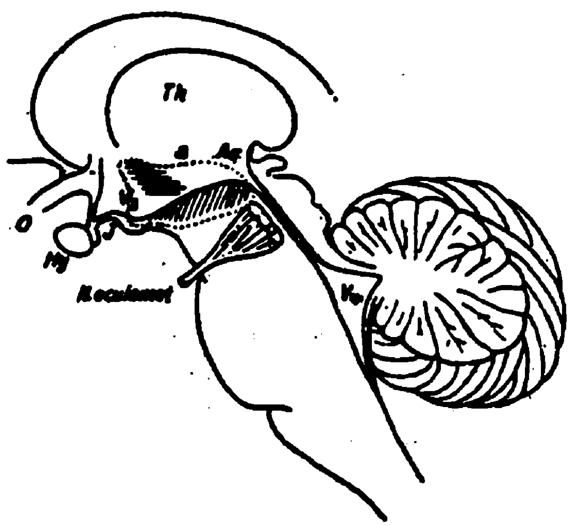
[7]Few of Economo's critics or supporters used trypanosomiasis to refute or support Economo's theory. For an exception, see Haberman's argument that there was no sub-cortical damage in African sleeping sickness, as well as his claim that there were many cases of encephalitis lethargica with no damage to the superior brain stem area (Haberman 1922).

Figure III
Economo's sleep centre



Schema of the median section of the interbrain; the dotted line is the boundary of the field, in which the center for brain regulation is lying.

(Economo 1930:256). Economo, and others who agreed with him, completely ignored the fact that Pavlov had focussed exclusively on the activity of the cerebral cortex in his work on conditioned reflexes (*e.g.* Trömner 1928).[8] Instead of a wave of inhibition spreading across the surface of the cortex, Economo argued that inhibition originated in the sleep centre, and then spread outwards to the thalamus and the cerebrum. Equally important was the activity of the "waking centre," whose task was to maintain the cortex in a tonic state.

Economo admitted that his theory was "somewhat similar to the hormonal explanation," but he also incorporated elements of the fatigue theory into his argument. Taking his cue from Claparède, Economo suggested that it was small quantities of fatigue toxins triggering this inhibitory centre, thus preventing the brain from becoming more intoxicated.

Economo had successfully shifted the focus of the problem of sleep. "Inhibition" was no less vague a term than "fatigue." But it seemed to explain the rhythms of sleep in a way that had been ignored in older fatigue theories. The brain was not simply a transfer point, where one form of energy could be converted into another. It was a self-regulating and automatic device that incorporated fatigue toxins into its overall performance. As far as sleep was concerned, Economo argued that the brain's work was administrative and organizational, rather than interpretational. His centres for sleep and wakefulness governed the *states* of consciousness, instead of determining its contents.

Sleep comes to America

Economo concluded his 1929 lecture by pointing towards the potential therapeutic value of locating a sleep centre in the brain. Once such a centre had been clearly identified, he argued,

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[8]Pavlov himself definitively rejected the idea of a sleep centre only in 1935, in a lecture entitled "The Problem of Sleep" (Pavlov 1957).

therapeutic efforts could be directed towards this area, making it possible "to treat insomnia and other sleep disturbances in a better and more active way than by drugs or by the roundabout way of hydrotherapy and psychotherapy" (Economo 1930:259). In New York, where Economo had delivered his lecture, psychotherapy had actually merged with neurology in the analysis and even treatment of encephalitis symptoms. In fact, Economo's lecture was published in Smith Ely Jelliffe's *Journal of Nervous and Mental Disease*, an organ for a field that had only recently been described as "psychosomatic medicine" (Krasner 1984; Levenson 1994).

Psychosomatic medicine was a curious mix of organic and psychoanalytic theories of disease, and Jelliffe (1866-1945) was one of its main advocates. Jelliffe had studied under Kraepelin in Munich and Déjérine in Paris, and he was also, along with A.A. Brill and J.J. Putnam, an early champion of psychoanalysis. He had also written a great deal on encephalitis lethargica (Jelliffe 1927, 1932). Proponents of psychosomatic medicine did not deny the organic origins of mental illness. Rather, they argued that *every* disease had a psychological component, and by using psychotherapeutic techniques to analyse and treat symptoms, health could be restored. The growth of psychosomatic medicine during the 1920s, which has been documented by Dorothy Levenson, suggests that Schiller's claim that the "medical" was being separated from the "psychological" is quite mistaken (Schiller 1982; Levenson 1994).

On the contrary, medicine and psychology readily mingled in the interwar period, particularly when it came to the problem of encephalitis lethargica, which featured movement disturbances and tics that were typically associated with catatonic schizophrenia. Following the lead of Adolf Meyer, one of the most influential figures in American neuropsychiatry in the early twentieth century, many psychiatrists and neurologists were beginning to depict the psychoses in psychological terms (Shorter 1997:109-112). The epidemic nature of encephalitis lethargica made it impossible to deny its biological etiology. But this did not prevent neurologists and psychiatrists from depicting the disease in psychological terms. Jelliffe and several other neurologists thought that the symptoms of encephalitis lethargica betrayed a deep interaction between organic and psychical components of all illness (Abrahamson 1920, 1935; Jelliffe 1927;

Bromberg 1982). In his classic 1945 textbook, the psychoanalyst Otto Fenichel described Jelliffe's work in terms of a shift in psychiatry, from debates over the etiology of mental diseases to what were then held to be more pragmatic therapeutic considerations:

> In other organic brain diseases, too, the reactions of the mental personality to the disease—the struggle between attempts to adapt oneself to or even to make use of the organically determined symptoms and attempts to deny them—comprises part of the clinical picture. The conflicts are very illustrative, even contributing to the understanding of the adaptive functions of the normal ego during its development. Likewise Jelliffe's attempts to "psychoanalyze" encephalitic symptoms should be understood as a study of the ways in which the personality reacts to or makes use of the symptoms rather than as a belief in the "psychogenesis" of encephalitis (Fenichel 1945:258-259).

One might expect that the repeated outbreaks of an epidemic disease sharing a number of symptoms with psychotic illness would have encouraged neuropsychiatrists to reject psychodynamic theories of mental illness as irrelevant. But this was not the case. Instead, the disease provided an opportunity for psychoanalysts and sleep theorists alike to advance their theories.

More importantly, encephalitis lethargica drew attention to the question of sleep. When Economo delivered his lecture in 1929, his American audience was almost certainly better prepared to speak of encephalitis lethargica than any in Europe. The disease arrived on American shores in early 1918, shortly after it had first appeared in Europe (Abrahamson 1920). It struck down thousands and killed hundreds of people in New York City between 1918-1923. What was perhaps the most remarkable about this epidemic was that its mode of transmission was completely unknown, and it affected the wealthy as well as the poor. Although the epidemics subsided considerably by the middle of the 1920s, it was still mysterious and prominent enough to garner substantial philanthropic support. Simon Flexner, the first Director of the Rockefeller Institute for Medical Research, began to take an active interest in the disease around 1921.[9] In

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[9]In a 1921 address at the New York Infirmary for Women & Children, Flexner argued that the name of the disease should be changed to "epidemic encephalitis," as its symptoms could include chronic wakefulness as well as pathological sleep. See the *New York Times* (February

May of that year, the *New York Times* reported that Mt. Sinai Hospital had finally been completed, at a cost of four million dollars, much of it coming from the Guggenheim sons. One of its main features was the consolidation of the neurological and medical departments, which meant that diagnosis and therapy in such cases could be conducted by the same doctor. Epidemic encephalitis was the *only* disease discussed in the article.[10]

The long duration of the disease sometimes kept its more famous victims in the news for months. In June of 1925, Mrs. J.P. Morgan, wife of John Pierpont Morgan, Jr., the American banker who had helped to finance $500 millions worth of loans to France and Britain during the war, fell ill with epidemic encephalitis. She died the following August. In 1927, Morgan donated $200,000 to equip a floor of the Neurological Institute of New York, then associated with Columbia University, for the study of the disease.[11] A commission had already been set up in 1925, through William J. Matheson, who had made a fortune in the dye industry, to investigate the causes of epidemic encephalitis and infantile paralysis. The first product of this funding appeared in 1929. It was an 850 page report reviewing all the work that had been done on the disease to date (Matheson Commission 1929). When Matheson died the following year, an endowment of $400,000 was established at the Neurological Institute.

Like Economo, writers for the *New York Times* were fascinated by the transformation of the sleep regime that befell the victims of epidemic encephalitis. A favourite editorial focus was

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25th, 1921, p. 15). Flexner and the Rockefeller Institute had recently commissioned research into outbreaks of trypanosomiasis in the Congo, and he may well have wanted to distinguish the two illnesses. The Matheson Commission later adopted Flexner's term, but most other researchers continued to employ Economo's terminology.

[10] *New York Times* (May 1st, 1921, II, p. 5).

[11] One of Morgan's personal physicians, Dr. Frederick Tilney, from the Neurological Institute, was also on the editorial committee of Jelliffe's *Journal of Nervous and Mental Disease*. He later became a central figure in the Matheson Commission. On Mrs. J.P. Morgan's illness and death, see the stories in the *New York Times* of 1925 (June 19th, p. 1; August 20th, p.1). On Morgan's donation, see March 22nd, 1927, p. 29.

the enormous length of time that encephalitis victims slept. One child in New York slept for a month. An man in Arkansas slept for three years, awoke for a moment at eight-thirty in the morning, yawned, and fall back asleep.[12] Another newsworthy feature was how the patients emerged from sleep, if they did so at all. One woman, a Mrs. Fred Tracy of Oxford, Chenango County, held the prestigious position of "the record sleep of the year." She was eventually awakened by a "talking machine," brought in by her neighbour as a last resort. But she fell back asleep almost immediately.[13] Another woman in Wisconsin had been asleep for nearly two years before the "spell" was broken by the appearance of her 6-year-old son at her bedside, which had by then been moved to the county asylum. Upon awakening, she recalled being perfectly aware of all that was going on around her the entire time she was asleep, but she was unable to open her eyes, move, or even speak. Her memory, however, appeared to be perfectly intact. She was able to recall events of the Great War, and could remember several family members who had fallen on the field of battle.[14]

Just as epidemic encephalitis seemed to treat the rich and poor equally, it failed to discriminate between the young and the old. Children were victims of the disease just as adults were. But where the most striking symptom in adults was their persistent somnolence, children were seen to suffer from a rather different condition. When it struck the young, epidemic encephalitis was seen to somehow interfere with the child's ability to develop properly. Encephalitis—even if it had not been diagnosed as such—was blamed for what were taken to be behavioural problems in children ranging from the failure to pay attention in the classroom, to juvenile delinquency. As early as 1921, reports began to emerge about restless and hyperactive children, who had suffered from encephalitis lethargica, and now were unable to behave at

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[12] *The New York Times*, April 16th, 1919, p. 8; March 29th, 1921, p. 5.

[13] *The New York Times*, December 24th, 1919, p. 8.

[14] *The New York Times*, August 21st, 1920, p. 18.

school or at home (Leahy & Sands 1921; Auden 1922; Edbaugh 1923).[15] The diagnosis of encephalitis in children was frequently retrospective. If a child suddenly began to display abnormal behaviour, brain damage, due to a mild encephalitis, must be the cause, even if the initial "flu-like" symptoms had been missed. Such a procedure was not unique to pediatrics, however. Under-diagnosis was a mainstay of public discussions of epidemic encephalitis. The decision to make the disease reportable was made on a municipal basis for each year, so most statistics were not considered to be reliable. Typically, neurologists and public health officers deduced the incidence of the disease according to the number of deaths reported. A fatality rate of about 10% was agreed upon, and thus the 211 deaths due to encephalitis in New York City in 1920 generated a projection of over 2000 actual cases of the disease, even though only 654 were actually reported.[16]

Children themselves appear to have been well aware of the cultural significance of this new disease. In the summer of 1925, a 10-year old boy tried to steal a woman's purse near Luna Park, in New York City. Upon feeling a tug at her handbag, the woman whirled around and captured her tiny assailant. At the Coney Island Police Station, the boy confessed to being a purse-snatcher—he had stolen twenty-six of them before finally ending up in police custody. His frustrated mother had returned each purse to its rightful owner. The police naturally inquired as to why the boy would behave this way. "Sleeping sickness" was his reply. It had apparently left him with a mania for taking purses.[17]

Neurologists, emboldened by the availability of considerable financial support, as well as public exposure, soon entered the fray. In 1924, Earl D. Bond, a neurologist at the Pennsylvania Hospital at Philadelphia, helped set up a small boarding school for the "re-education" of

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[15] Thanks to Dave Pantalony for these references, which come from his historical study of Attention Deficit Disorder.

[16] *The New York Times*, January 28th, 1921, p. 24; February 16th, 1921, p. 3.

[17] *The New York Times*, July 13th, 1925, p. 19.

delinquent children. He emphasized the use of educational techniques that, he claimed, should work with normal children as well: individual attention; positive reinforcement; and the importance of teaching the parents how to support their child at home. He published a book on the subject in 1931, and, in a newspaper article, he argued that epidemic encephalitis was responsible for "the flood of juvenile delinquency" that had been described by recent committee.[18]

* * * * *

The physiology of sleep was obviously not *the* central concern for the neurologists, psychiatrists, child psychologists, medical practitioners and public health officials that took an active interest in encephalitis lethargica. They were naturally much more interested in the nature of the disease itself—its viral origins, its means of transmission, and possible therapeutic interventions. The disease itself, however, disappeared before any of these questions could be answered definitively. Mosquitoes were identified as a disease vector in some of the outbreaks of the disease in the 1930s, but the symptoms of encephalitis lethargica were so protean that it was difficult to assess whether or not investigators were even dealing with a unified disease entity (Matheson Commission 1939). This mattered very little, however, because the disease virtually disappeared by the end of the 1930s.

Still, the emergence of this disease marked an important episode in the development of sleep physiology. Economo's study of encephalitis lethargica provided anatomical evidence of what Claparède and Piéron had tentatively suggested ten years earlier—sleep was the product of a regulatory centre located deep within the brain. Unlike Gélineau's narcolepsy, which was

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[18] *The New York Times*, "'Badness' Responds to Mild Methods," November 15th, 1931, III, p. 7.

accepted as a symptom but not as a disease, the epidemic nature of encephalitis lethargica could hardly be denied. Economo's theory of sleep spread across the Western world, right alongside the disease that had spawned it. Encephalitis lethargica also helped to bring sleep to the forefront of neurological research, and move it out of the field of fatigue physiology, which had, by the late 1920s, receded to the backwaters of medical science. It severed sleep from the physiology of sensation, and united it with the study of neurophysiological organization. The circulatory images of the fatigue theories and the reflexive images of stimuli theories were giving way to a picture of sleep as a self-regulatory phenomenon.

Chapter V

Sleep as performance: physiology at the University of Chicago 1923-1939

The encephalitis lethargica epidemics of the early 1920s brought the concept of a "sleep centre" to the forefront of neurological research. The idea that sleep could have a physical location in the brain implied that it was something much more than the annihilation of consciousness. It now had a structure, as well as a function, and was thus becoming an important object of biomedical research. The problem of sleep was transcending the psychological description of consciousness.

Such a perspective readily took hold in the United States during the interwar period. In the U.S., functionalist and behaviorist approaches were battling for supremacy in the psychological field. Behaviorism, which argued for an elimination of introspective consciousness as a psychological object, was the dominant approach to empirical questions about the mind. But such an approach had little impact on the study of sleep, which continued to be depicted as a state rather than a behaviour. Functionalism, on the other hand, retained consciousness as an object of inquiry. Physiologists with an interest in consciousness assumed this functionalist perspective, and used Pavlov's method of "conditioned reflexes" as a bridge between their field and physiology. In his 1923 lecture tour, Pavlov had assigned sleep a central place in his research program, thus putting it on the agenda for physiologists with an interest in understanding the biological origins of consciousness.

The method of conditioned reflexes, which used physiological concepts to explain psychological phenomena, fit perfectly into the holism that transformed American biomedicine between the wars. Holists insisted that the whole was somehow greater than the parts. Any complete medical treatment or meaningful physiological experiment needed to consider its subject in both dimensions. Sleep research at the University of Chicago was formulated on precisely these premises during the 1930s.

The University of Chicago hoped to create a new field in psychiatric medicine that would combine somatic and psychological cures. Two affiliates of this "neuropsychiatry" project, Nathaniel Kleitman and Edmund Jacobson, played important roles in the discovery of REM. While Kleitman developed an evolutionary theory of sleep based on his physiological research, Jacobson developed a relaxation therapy that spilled over into the problem of sleep.

Nathaniel Kleitman (1895-1999) is a pivotal figure in the history of twentieth-century sleep research in North America. Before Kleitman, sleep was an interesting, but incidental topic of research for physiologists and psychologists alike. Claparède, who first proposed a biological theory of sleep, actually conducted very little research in this field. Likewise, Piéron more or less abandoned the problem of sleep for the physiology of sensation shortly after publishing his dissertation in 1913. Pavlov came to the study of sleep only late in his career, and Vaschide's premature death cut short his well-developed interest in the subject. Economo also died at a relatively young age, just as he began seriously pursuing the question of a sleep centre.

Kleitman, on the other hand, clung to the problem of sleep with an unparalleled tenacity. He had an extraordinarily long career, with the problem of sleep always at its centre. His first publication on the subject appeared in 1923. His last, a review of his theory of a "Basic Rest-Activity Cycle," came out almost sixty years later, in new journal entitled, appropriately enough, *Sleep* (Kleitman 1982).

The very existence of such a specialized journal owed a great deal to Kleitman's efforts. His laboratory at the University of Chicago—the first to be organized around the study of sleep—was at the centre of American sleep research for decades. Many of the most prominent sleep researchers in the U.S., including William Dement, David Foulkes, Allan Rechtshaffen, and Eugene Aserinsky, studied there and went on to establish sleep laboratories in other universities across the country during the 1960s and 70s. Even if not actually studying with Kleitman, psychiatrists and psychologists with an interest in sleep (and there were many in the post-REM era) would often consult with him about how one established a sleep laboratory.

A comment on biography & research schools

Thus the story of sleep research is in many ways caught up in Nathaniel Kleitman's own story. This is not to suggest, however, that a biographical study of Kleitman is the proper route to a better understanding of the history of sleep. This task would be both futile (Kleitman was a somewhat reclusive person who rarely updated even the minimal biographical information in the standard directories, and left no archive of his work), and chimerical, as it would give sleep a far greater degree of internal coherence than it in fact possessed. Sleep was little more than a baroque collection of facts when Kleitman arrived on the scene, and it remained this way until a research community consolidated around a single phenomenon—rapid eye movement.

Postwar sleep research has already been taken up as a case study in the sociology of scientific fields (Lemaine *et al.* 1977). Lemaine *et al* use the field of sleep research to examine how scientists balance risk against possible rewards. Their emphasis on "choice" assumes that their subjects are always acting rationally, and are able to determine the success of a certain avenue of research in advance. The force of institutions, experimental practices and technologies in shaping scientific cognition is relegated to insignificance. It is not what researchers perceive that counts for this "sociology of scientists," but how they choose to handle the evidence that they uncover in the process of investigation. In many ways, this sociological study reflects the method of data-gathering, which relied primarily on interviews with the sleep researchers themselves, who gave retrospective accounts of their own activities. History is reduced to little more than a chronicle of discoveries in the annals of sleep research. It serves only to frame their sociological question: what are the relationships of power among social networks that can explain why some laboratories and individuals outperform others? How these discoveries emerged in the first place remains unanalyzed.

The historical approach to the sociology of research schools has offered some interesting results (Geison 1987; Geison & Holmes 1993). Gerald Geison has observed that this program of study is an historical extension of Robert K. Merton's functionalist analysis of scientific productivity (Geison 1987). Rather than describing the history of American physiological research as a progress in ideas, Geison offers a comparative analysis of schools, pedagogical structures and laboratory practices. In another attempt to combine sociological theory with historical research, Marin Kusch has offered a remarkable study of two psychological laboratories engaged in the "imageless thought" controversy (Kusch 1995). Kusch argues that a focus on research schools can be used to demonstrate how the structure of power relationships within laboratories (authoritarian or egalitarian, in this case) can itself reflect the knowledge that is generated in of psychological experimentation.

The "research schools" tradition enables the historian, like the sociologists they have followed, to move from the case study of a laboratory to generalizations about science writ large. Without the ability to conduct personal interviews, historians must turn to texts. As a result, their attention becomes focussed on particular controversies in the history of whatever science they choose to explore. They then reinterpret these old debates through the lens of research schools. The examination of the process of discovery is abandoned for the study of how conflict and competition between individuals and institutions engenders scientific knowledge. This emphasis on conflict and debate often means that the stable, uncontested elements of the scientific enterprise—its tools and techniques, for example—are simply taken for granted.

This is a pressing question for the history of sleep research, because the graphical method had an enormous impact on how sleep was visualized in the laboratory. Yet the method itself was rarely a source of conflict. Without an analysis of how technologies, methods and theories combined in different ways to create a stable investigative platform for sleep researchers, it would be impossible to understand the curious fact that there were many opportunities for investigators to observe rapid eye movement before it became a discovery. Vaschide, for example, watched the faces of his sleeping subjects throughout the night, but offered no observation of eye movements that matches REM. In the present chapter, I will offer another example, that of Edmund Jacobson, who suggested that eye movements and dreams were related, but failed to elaborate on this relationship. What sort of cognitive, instrumental, and institutional elements needed to be in place before this observation could mean anything at all? I have started to answer this question by focussing on the graphic inscriptions and evolutionary explanations that dominated laboratory physiology in the late nineteenth and early twentieth centuries. My inclination for generalization lies at the opposite end of those who write about research schools: I have begun with far-reaching narratives about the structure of physiological and psychological experiment, and am now working my way down to a particular case—sleep. This method has the advantage of bringing to light formerly hidden or ignored stories about the scientific research of the past; in contrast, the continuing relevance of studying research schools depends upon the high-visibility of the case studies selected for investigation.[1]

Thus, I have selected a discovery, rather than an institution or an individual, and tried to chart its pre-history, taking advantage of the work done on research schools along the way, as well as gesturing towards the work of biography. My interest in Kleitman will extend only insofar a study of his career path illuminates the social structures, instrumental apparatus and laboratory practices that helped shape the cognitive basis of American sleep research. While this field was certainly shaped by Kleitman's work, its context was not so local that it depended upon him. Encephalitis lethargica and Pavlov's program of conditioned reflexes had turned sleep into a problem of some consequence during the 1920s. It was left to Kleitman—or someone like him—to attempt to consolidate this diverse spectrum of facts and theories into a coherent conception of sleep.

Nathaniel Kleitman: the first American sleep researcher

Four weeks before Ivan Pavlov delivered his lecture on sleep at the University of Chicago in July of 1923, a young physiologist named Nathaniel Kleitman [Figure I] submitted his first publication to *The American Journal of Physiology* (Kleitman 1923). The paper was the first of a

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[1]Kusch's study of the "imageless thought" controversy is a case in point—it was just this controversy that is said to have brought down experimental introspection as a psychological research method, thus ushering in the modern era of functionalist and behavioural approaches in psychology. Kusch does not question this narrative, he simply attempts to explain it in sociological terms (Kusch 1995).

Figure I
Nathaniel Kleitman
(Current Biography 1957)

long series of articles that Kleitman was to publish in this journal over the next several years, and would ultimately form the core of the book—*Sleep and Wakefulness as Alternating Phases in the Cycle of Existence*—that came to define him as the world's leading sleep researcher (Kleitman 1939).

Kleitman was already well-aware of Pavlov's research in sleep, and he indicated in this paper that he had been communicating with Pavlov on the topic. It is not clear whether Kleitman actually attended Pavlov's lecture at the University of Chicago, which took place in July, when the regular academic session was out. But this hardly mattered, as Kleitman, who was fluent in Russian, would have gotten as much information out of a letter from Pavlov as anyone who listened to Pavlov's son deliver the brief translated version of his father's lecture.[2] The high visibility of Pavlov's work in Kleitman's 1939 textbook is further testimony that Pavlovian ideas play an important role in Kleitman's thinking about sleep. The only individual that is cited more frequently in the first edition of *Sleep and Wakefulness* is Henri Piéron. It is Piéron's intellectual lineage that has endured in historical accounts of sleep research, while Pavlov's name has been virtually erased from the annals of the field (Lemaine *et al.* 1977; Schiller 1982). My intent is to demonstrate that Kleitman was not only aware of Pavlov's research, but he approved of it at the outset, and pursued a line of inquiry that was, in effect, a hybrid of the experimental styles of Pavlov and his other mentor, Henri Piéron.

Kleitman, who was born in 1895 in Kishinev, Bessarabia (now Moldavia), had been in the United States a mere eight years before Pavlov arrived. According to Peretz, who interviewed Kleitman when he was ninety-five, "the story of his life exemplifies every word—written or related—about the fate of the Wandering Jew" (Lavie 1996:18). The Jews of Bessarabia suffered greatly during the last years of the nineteenth century and the early years of the twentieth. The

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[2]My request to see copies of the letters exchanged between Kleitman and Pavlov were politely turned down by Kleitman's family in California. Kleitman was almost certainly encouraged to communicate with Pavlov by A.J. Carlson, who, like W.B. Cannon and several physiologists in the U.S., developed a friendship with the aging Russian scientist [**slide #2**].

reign of Alexander III (1881-1894) saw the extension of "Russification" throughout the Russian empire, and this program had devastating effects among the non-Russians living in the peripheral states of southeastern Europe. The extreme social agitation that marked Russian history during the three decades that culminated in the October Revolution in 1917 included an exceptionally high degree of resentment among the peasantry against the Jews, who, in Bessarabia at least, controlled a great deal of the trade and soon became the officially-sanctioned target of prejudice (Ussoskin 1975). Kleitman surely remembered—if he did not experience its effects himself firsthand—the pogrom that resulted in the death of forty-five Jews and the beating of hundreds in his hometown during Easter of 1903 (Harcave 1968:329).

By 1912, labour unrest and strikes were sweeping across the industrialized cities of Russia, and Balkan nationalism was beginning to have negative repercussions among the Jewish communities there. Kleitman decided, at seventeen years of age, to leave for Palestine, in order to study medicine at the American College in Beirut (Lavie 1996:18). With the outbreak of war, he became concerned that he would come under the suspicion of the Turkish authorities in Palestine because he was a Russian subject. So he fled to Rhodes, where he boarded the first ship that would take him out of Europe. The freighter happened to be American, and it took him to New York City sometime in 1915, thus making Kleitman one of the almost 18 million immigrants that came to the U.S. during the period 1890-1917 (Cashman 1998:88).

A.J. Carlson & physiology at the University of Chicago

Kleitman received a BS degree from the City College of New York in 1919, and taught chemistry at CCNY while completing his MA at Columbia University. In 1922, he started working on his PhD on the physiology of sleep under A.J. Carlson at the University of Chicago. Anton Julius Carlson (1875-1956) headed the physiology department at Chicago from 1904 until his retirement in 1940. Along with William H. Howell (Johns Hopkins), Walter B. Cannon

(Harvard), and Joseph Erlanger (Washington University at St. Louis), Carlson was one of a handful of elite researchers and educators that helped to direct the growth of physiology in the United States in the early twentieth century. During this period, physiology began to develop as a discipline with greater independence from the medical practice that underwrote its growth in the nineteenth century. By the time Carlson arrived at Chicago, a "research imperative" had become well-entrenched in the American university system, and the small number of full-time faculty in physiology departments no longer divided their time between clinical practice and teaching medical students (Fye 1987). They were now expected to devote all their time to research and teaching.

Experimental practice in physiology was also becoming more dependent upon technological innovation (Borell 1987; Clarke 1987; Marshall 1987). This was particularly the case in the United States, where F.W. Taylor's reorganization of factory production, had started to restructure America's industrial capacity to better reflect the capabilities of machines, rather than employees (Rabinbach 1990; Braun 1992). Physiologists themselves became more closely allied to industry, both as critics and adjuncts, as they attempted to understand the capacities of the human body in terms of its ability to perform work (Gillespie 1987, 1991).

Carlson's tenure at Chicago saw American physiology transformed from a marginal enterprise to a position of world leadership, which happened to roughly coincide with his becoming a full professor in 1914, at which time he became head of the Physiological Institute at Chicago (Ingle 1979). This the beginning of a new era for physiological investigation in the United States, as the Guggenheim and Rockefeller Foundations, along with the National Research Council (NRC), began to support postdoctoral research in this field in 1920. Between 1920 and 1940, Carlson trained as many postdoctoral fellows at Chicago (31) as Cannon did at

Harvard (32).[3] He oversaw the completion, in 1926, of the enormous six-storey Hull Biological Laboratories, which rivalled the massive labs at Michigan, Harvard, and St. Louis.

Carlson arrived in the U.S. from Sweden in 1891, speaking no English (Howell & Greene 1938:122-124; Ivy 1959; Dragstedt 1961; Ingle 1979). After working as a carpenter's assistant for several years in Chicago, he saved up enough money to go to the Augustinian Academy and College at Rock Island, Illinois, with the hope of becoming a pastor in a Swedish Lutheran church. He earned an MA in philosophy, but his time behind the pulpit was brief. He soon lost his faith, and decided to take up the study of the physiology of the nervous system. Carlson borrowed enough money to attend Stanford, and worked there under O.P. Jenkins, studying the rate of nervous conduction in slugs. From there, he went to the Marine Biological Station at Woods Hole, Massachusetts and examined the role of the cardiac nerves in the coordination of the heart beat in the horseshoe crab. His work, which was published in the *American Journal of Physiology* in 1904, helped demonstrate that the automatic rhythmic behaviour of the heart was due to the action of the autonomic nervous system, not the muscle.

The success of this work brought a job offer from Chicago, which he accepted in 1904, and where he remained for the rest of his career. His work there was divided between teaching physiology to medical students (Carlson himself had no background in clinical medicine) and overseeing experimental research in the physiology department. One estimate suggests that he taught at least five thousand medical students, participated in 151 MSc degrees in physiology, and personally supervised 112 PhD's (Ivy 1959). According to Geison, Carlson was "directly responsible for the surge of productivity at Chicago" during the early years of the twentieth century (Geison 1987:148). The number of papers that had his name attached to them was greater than any of his American competitors, and, on his own, he generated more publications between 1898 and 1918 than most physiological *institutions* (Geison 1987:148). He was also influential as

[3]Geison 1987. An incomplete list of some of his students—more than 170 of them—can be found in Ingle 1979.

the President of the American Physiological Society from 1923-1925, and as the Chairman of the Board of Editors for *Physiological Reviews* from 1932-1950.

Unfortunately, Carlson is perhaps best remembered for preventing, rather than encouraging, a major discovery in physiology. He was the cautious professor and hard-nosed experimentalist who either rewrote or demanded the revision of a paper by his student, Ernest L. Scott, for publication in the *American Journal of Physiology*. The paper had been taken from Scott's thesis on the effects of injecting a pancreatic extract in diabetic dogs. It was rewritten to emphasize the fact that the lowered sugar excretion and dextrose-to-nitrogen ratio of the animal's urine was not *necessarily* due to the injected substance. This effectively eliminated any priority claim Scott might have had against the physiologists at the University of Toronto who discovered insulin almost ten years later (Ingle 1979; Bliss 1982).

Carlson's best-known research focussed on the physiology of digestion, which was itself an extension of his early work in the role of the nervous system in generating and sustaining the body's rhythmic behaviour. In 1916, he published *The Control of Hunger in Health and Disease*, a book that focussed on the sensation of hunger as a function of the sensory impulses that arose from the gut. His experimental studies included both human and animal research. His star subject was a young man, Fred Vleck, who had drunk a solution of caustic soda at the age of six, which eventually led to the closure of his esophagus. He underwent surgery that enabled him to feed himself through an opening in his stomach six years later. When more surgery was planned, the boy took flight.

Carlson's method consisted primarily of taking recordings of stomach contractions by inserting a balloon, attached to a flexible tube, through Vleck's fistula and into his stomach. The balloon was then inflated, and the tube attached to a manometer and kymograph [Figure II]. "Our gastric fistula man, Mr. V," argued Carlson, "offers an exceptional opportunity for studying the relations of certain conscious states, particularly those associated with foods and with eating, on the activities of the empty stomach" (Carlson 1916:161). Because Vleck could chew food, but

Figure II
A.J. Carlson's experimental assemblage
(Carlson 1916)

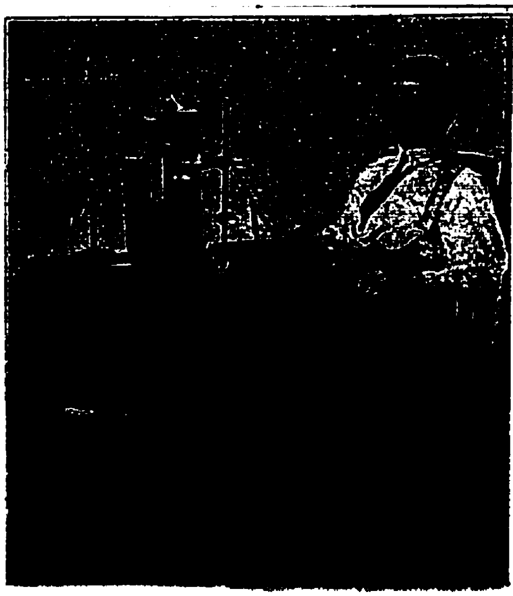

FIG. 3.—Photograph showing arrangements for simultaneous recording of the gastric hunger contractions and the vasomotor and cardiac changes (arm plethysmograph) of Mr. F. V.

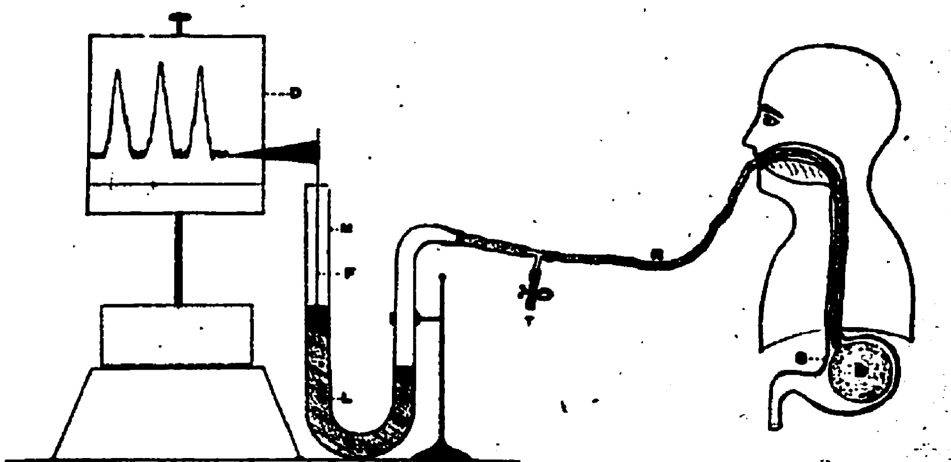

FIG. 4.—Diagram showing method of recording gastric hunger contractions of the empty stomach of normal persons. *B*, rubber balloon in stomach. *D*, kymograph. *F*, cork float with recording flag. *M*, manometer. *L*, manometer fluid (bromoform, chloroform, or water). *R*, rubber tube connecting balloon with manometer. *S*, stomach. *T*, side tube for inflation of stomach balloon.

was unable to swallow, Carlson used Vleck to study what Pavlov had dubbed the "psychic secretion" of digestive fluids.

Against Pavlov (and Walter Cannon), Carlson argued that both the tonus of the stomach and gastric secretion were not triggered by the appearance of food, or of any stimulus that had been conditioned to be associated with food. Instead, the activity of the stomach followed a rhythmical pattern. "The extrinsic nerves to the stomach," Carlson claimed, "play a rôle similar to that of the nerves to the heart in the regulation of the heart rhythm" (Carlson 1916:160). The rhetorical force of Pavlov's experiments had depended upon a clear relationship between a conscious state—the feeling of hunger—and its physiological sign, salivation, which triggered the activity of the digestive process. In contrast, Carlson insisted that the stomach went about its business of contracting and secreting apart from any sensory stimulus that had been registered in the cerebral cortex. The responses Pavlov had isolated were merely the phenomena of "central reinforcement." The precious drops of saliva that Pavlov had so carefully collected and measured were merely the product of *attention*. They had nothing to do with the regular and automatic behaviour of the digestive apparatus, the rhythmic contours of which Carlson had captured on his kymographic drums [Figure III]:

> In the normal individual the empty stomach exhibits periodic hunger activity, and there is no evidence to show that this primary automatism of the empty stomach is in the least influenced by eating one or by eating five meals a day...the milder huger contractions do not enter consciousness as pangs of hunger if the individual's attention is directed into other channels. They are felt as hunger pangs if the individual's attention is directed toward food and eating. The attention is thus directed, consciously or subconsciously, about the time the individual is accustomed to eat. The periodicity of this subjective attention to the milder hunger cravings can probably be altered by training (Carlson 1916:153-154).

Carlson's work on digestion emphasized the interaction between consciousness and the rhythmic autonomy of the body. He considered mind and body to be two separate entities. Pavlov's work, on the other hand, always invoked the immense flexibility of the cerebral cortex, which seemed capable of associating practically any stimulus to an unconditioned one through training. One of the more infamous experiments to come out of Pavlov's laboratory, for example,

Figure III
Tracings of rhythmic & invoked stomach contractions
(Carlson 1916)

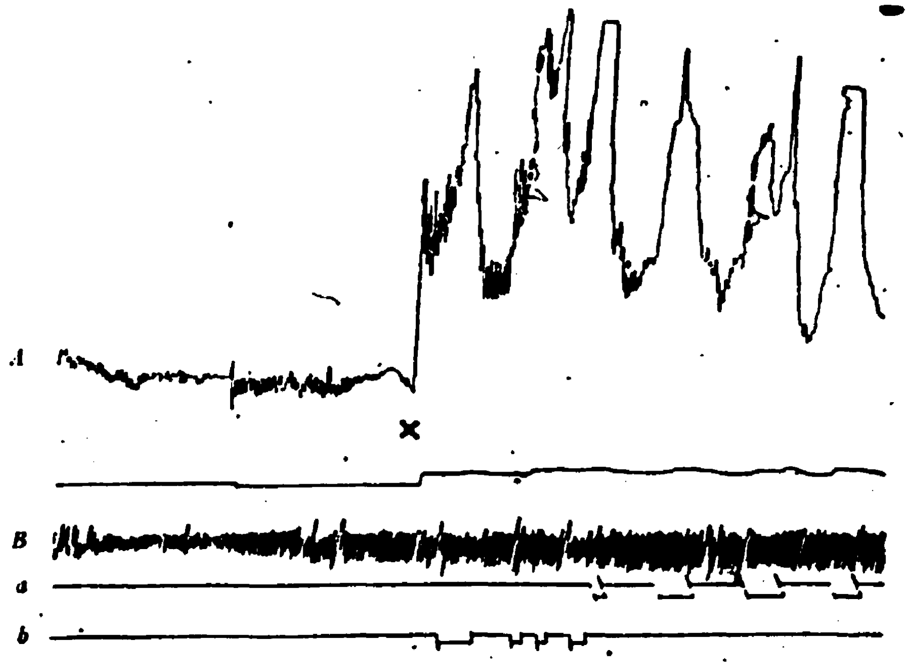

FIG. 12.—Two-thirds the original size. *A*, stomach contractions; *B*, respiratory movements; *a*, *b*, signals for moderate and strong hunger respectively. The pressure in the balloon is slight. There is no evidence of strong stomach contractions, and Mr. V. feels no hunger. At *x* the pressure in the balloon is suddenly increased. This distension of the balloon initiates a few strong stomach contractions, which in turn cause the hunger states. A demonstration of the gastric genesis of hunger.

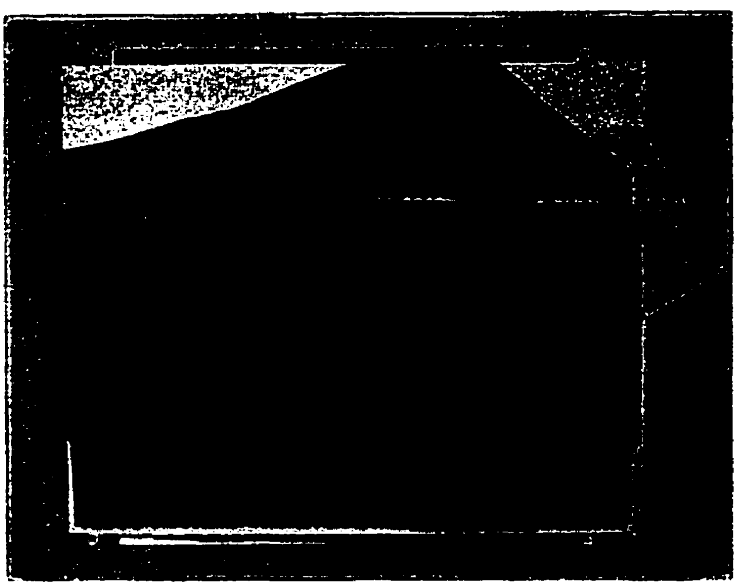

FIG. 5.—The three upper tracings are typical records of the gastric hunger contractions of normal adult persons toward the end of a hunger period. The tracings are recorded and are to be read from left to right, and in each case the gastric hunger contractions cease spontaneously near the right end of the tracings. The more rapid excursions are due to the movements of respiration. The bottom tracing shows typical gastric hunger contractions of a normal dog.

involved conditioning a dog to salivate while its skin and flesh was burnt by an electrode. Mind was nothing more than trained reflex. Carlson did not share Pavlov's rejection of introspective evidence. On the contrary, his use of human subjects (which included himself) underwrote his attempts to reveal the limits of conscious awareness in physiological function. His work was the result of a dedicated dualism that sought to identify the relationship between mind and body by articulating the mutual influence between the mind's perceptions and the body's rhythms.

Kleitman's early research

Kleitman's study of sleep followed this same path. He attempted to sort out the psychological and the physiological aspects of sleep through the common currency of rhythm. Like Carlson, Kleitman did not have a background in clinical medicine. The practical trajectory of his physiological research was not aimed at any theory of disease. Surrounded by functionalist psychologists who drew their scientific legitimacy from physiology, Kleitman approached sleep as a question of biological rhythms, perception, and social influences conjoining to create the phenomena of sleep and wakefulness.

Kleitman published his first paper on sleep just as he was completing his PhD under Carlson. He had just been awarded an NRC Fellowship, and was preparing to travel to Europe to study with Piéron and Louis Lapique in Paris. Publishing in the *American Journal of Physiology* made a great deal of sense: his supervisor was one of the most important physiologists in the country, and the *AJP*'s editors gave precedence to American authors over their European counterparts.[4]

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[4]According to a letter written by Walter Cannon to Alexander Forbes, the *AJP* followed the policy of "not publishing papers submitted by foreign workers who have journals to resort to in their own countries"(Letter, Cannon to Forbes, June 10, 1931, WBCA, 5.57).

Kleitman began the paper with a self-conscious, almost ironic tone. He repeated the standard complaint of those who studied sleep—that it had been largely overlooked by physiologists. But even as he made this claim, he recognized it as a rhetorical form unique to the field. It was as though he felt obliged to demonstrate that this subject, which held such innate interest for him, also had practical consequences:

> Most investigators of the physiology of sleep, in reporting their findings, remind their readers, by way of apology, of the tremendous importance of the subject for the advancement of our knowledge of physiology as a science, as well as for the rational treatment of insomnia (Kleitman 1923:67).

Kleitman argued that questions of time management and efficient performance were equally important aspects of sleep research. His Taylorist ideal of improving productivity contrasted sharply with Mosso's vision of ameliorating the burdens of fatigue for the common labourer:

> They also like to record the fact that the average individual spends more than a third of his life in sleep, has been doing this from time immemorial, and raise the question whether eight hours or more of sleep a day really constitutes the minimum penalty for keeping awake the rest of the time. It seems reasonable to suspect that as in the case of protein consumption, there is a large "factor of safety" in the amount of sleep we are getting, and that it could be considerably reduced without impairment of health or loss of efficiency. This and other questions related to industrial physiology, especially physiology of fatigue, can be answered only by a thorough systematic study of the subject (Kleitman 1923:67).

Finally, Kleitman made it clear that his work would return the study of sleep to the physiologists:

> But curiously enough, there are scarcely a dozen investigators engaged at any one time in the study of this great physiological mystery, and most of these workers are psychologists. It seems that because animals are not very well adapted for this work and the human beings that are available cannot be dissected, physiologists have allowed the psychologists to tackle this problem as best they could (Kleitman 1923:67).

Kleitman aimed to attack traditional psychological problems from the perspective of physiology. Naturally, he was attracted to Pavlov's method of conditioned reflexes as one solution. Kleitman singled out Pavlov's experiments, which had not yet been presented outside of the Soviet Union, as being particularly noteworthy:

> Pavlov and his co-workers, in their study of the conditioned reflexes, found that their animals frequently fell asleep during the experiment. They found that as a result of prolonged action of a uniform excitant their animals invariably fell asleep. The complete data of their experiments have not been published as yet, but in a personal communication to the writer Pavlov states that their results indicate that sleep and the so-called internal inhibition of a conditioned reflex are identical phenomena, the former being diffuse and the latter localized (Kleitman 1923:83).

But Kleitman was more interested in adapting Piéron's method of experimental insomnia to human subjects than he was in Pavlov's method of conditioned reflexes. The former would allow him to frame sleep in terms of states of consciousness, just as Carlson had done with digestion. It would provide him with a means to wrest sleep from the psychologists, many of whom had adapted Pavlovian methods themselves. But he was by no means indifferent to the method of conditioned reflexes. In 1925, he happened to witness such an experiment at the Laboratory of Pharmacology at the University of Chicago, in which the unconditioned stimulus to make the dogs salivate was the injection of morphine, rather than the presentation of food (Collins & Tatum 1925). The experimenters had observed that the dogs began salivating when the experimenter entered the room, before the morphine had even been injected. "It occurred to us," Kleitman and a colleague later remarked, "that this reflex might be used to test Pavlov's theory of sleep" (Kleitman & Crisler 1927:571).

Kleitman and Crisler took the experimental context itself as the conditioned stimulus. After several days of placing their dogs in their stands, and then injecting them with morphine, Kleitman and Crisler observed that they could make the dogs drool for 15 to 20 minutes, even without the injection. All they needed to do was initiate the experiment by putting the animal in its stand. One particular dog salivated excessively and continuously, even after the morphine had worn off. This same animal also tended to fall asleep once it was in the stand—both before the injection and after its effects had worn off. The most curious aspect of the dog's behaviour, Kleitman and Crisler argued, was that it continued to drool even after it had fallen asleep. They thought this demonstrated that Pavlov had been wrong to argue that sleep was the result of a generalized inhibition. On the contrary, the authors suggested, sleep was itself an independent inhibitory state. Kleitman presented this material at the XII International Physiology Conference, which was held in Boston in 1929. Pavlov was also at the conference, and presented a paper on inhibition. Kleitman's was the only presentation on sleep that year, and it seems quite possible that they attended each other's talks.

Pavlov certainly would have had good reason to pay attention to Kleitman, as the latter's experiments threatened the epistemic status of salivation as an index of inhibition. Kleitman was refashioning sleep as a unique object of physiological research. He had no doubt that sleep was in some sense a product of conditioning. But it was not readily subsumed under the rubric of generalized inhibition.

Neuropsychiatry & biomedical holism at the University of Chicago, 1928-39

In the late 1920s and 1930s, American physiologists took Pavlov's work as a model that could incorporate body and mind into their experimental research. Pavlov shunned psychological terminology in favour of nomenclature of brain activity, yet his method of conditioned reflexes attempted to analyse the way the organism and its environment interacted. Pavlov worked on a macro-level of behaviour, with organisms (usually dogs) that were entirely intact. His work was severely criticized by some philosophers, who argued that the study of conditioned reflexes could never capture the entirety of an organism's behaviour (Merleau-Ponty 1963). Nonetheless, I want to suggest that Pavlov's work fit easily into the holistic rhetoric that permeated much of American biomedicine and physiology during the interwar period.

Pavlovian research can be contrasted, for example, to Sherrington's work on spinal reflexes. Sherrington (1857-1952) assumed the classical physiological position that the function of the nervous system would be best understood by tearing it down to its most basic element, testing it, and then rebuilding the whole in these elemental terms. Pavlov, on the other hand, took the entire organism as his experimental object, fitting well with the holistic axiom that the whole could not be reduced to the mere sum of its parts. But the reception of Pavlov's work was ambivalent on this point, which perhaps explains why there is no mention of him in a recent work on biomedical holism (Lawrence & Weisz 1998). His methods were attacked by self-proclaimed holists, like the German émigré psychiatrist, Kurt Goldstein (1878-1965), who felt that Pavlov's work had no relevance outside of the phenomena of "training" and "drill" (Goldstein 1995). Self-described mechanists, on the other hand, approved of Pavlov's neurophysiological explanations of psychological phenomena. One such devotee, the Chair of Zoology at Cape Town, South Africa, declared in 1929 that "in the light of Pavlov's work we can now envisage the possibility that the methods of physical science will one day claim the whole field of what can properly be called knowledge" (Lancelot Hogben, as cited in Hancock 1968:191). Hogben was taking aim against his fellow countryman, Jan Christiaan Smuts, the sometime philosopher of science and South African statesman. Smuts (1870-1950) had coined the term "holism" in 1926, in his attempt to demonstrate how the synthetic drive to create "wholes" was an innate part of the natural order. Yet even this holist cited Pavlov's work approvingly. Smuts argued that Pavlov's study of conditioned reflexes not only demonstrated that the synthetic work of consciousness was amenable to experimental investigation, it also showed that such synthesis could be inherited by later generations (Smuts 1926:205).

Kleitman certainly did not describe himself as a holist. Nor was he a Pavlovian, as he rejected Pavlov's theory of sleep as "generalized inhibition." In fact, he, like Claparède, eventually abandoned all of Pavlov's theoretical proclamations as being "more of a problem in semantics than in physiology" (Dumas1934:IV.455-522; Kleitman 1939:471). But Kleitman found himself in the midst of an institutional push for holistic approaches to the medical and biological sciences that certainly bankrolled much of his research, if it did not determine his direction altogether. As the editors of a recent study of holism point out,

> philosophers can provide formal and rather elaborate definitions of the term, but in the world that historians inhabit, holism is essentially relational; it constitutes a rhetorical claim made in opposition to other approaches that are characterized as excessively narrow or reductionist in focus. Indeed what is holistic for one individual is frequently perceived as reductionist by another (Lawrence & Weisz 1998:2).

The organism's need to adapt to "natural forces and rhythms" in order to harness the healing power of nature was a fundamental tenet of many interwar holists (Lawrence & Weisz 1998:5). Although Kleitman did not explicitly adopt the rhetoric of holism, his efforts to understand sleep in terms of adaptation *as* rhythm put his work firmly within the holistic biomedical discourse surrounding him at the University of Chicago.

Medical research at the University of Chicago was in a unique position during the late 1920s. It was, in many ways, the fulfilment of Piéron's dream of a scientific environment entirely void of historical pretensions and tradition. The University had originated as a small Baptist institution, but was completely transformed through an enormous endowment by John D. Rockefeller in 1890. It was unusual among the major American universities in that it had no medical school. All clinical instruction took place at Rush Medical College, which became affiliated with the University in 1898 (Irons 1953). When, in 1927, a four-year medical program was finally created through the opening of the University of Chicago Clinics, its founders and faculty adopted a self-consciously modern approach: clinical medicine at Chicago was to be identified with biological science (Blustein 1993).[5]

The clinical case report, the mainstay of medical communication well into the twentieth century, was to be replaced by controlled experiment. As Franklin McLean, a physiologist by training and the Associate Dean of Biological Sciences, remarked in 1930, Chicago needed "to recognize Medicine as Biology rather than to incorporate various biological sciences within Medicine as is usually done" (as cited in Blustein 1993:417). Turning the clinic into a laboratory, rather than simply applying laboratory research to clinical medicine, implied an explicitly

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[5]On holism and interdisciplinarity at Chicago in the 1930s, see the following articles: Mitman, Maienschein, and Clarke 1993; Blustein 1993; Kingsland 1993; and Pressman 1998. The urban sociology of Robert E. Park, which was an example of "Chicago Functionalism" at work in the social sciences, was likewise a product of holism. Park argued that cities were more than the sum of the people who inhabited them, and thus needed to be analysed as objects in their own right (Nye 1985).

holistic and interdisciplinary approach to medical research. This was particularly the case in psychiatry between the wars, when patients were frequently treated as little more than experimental material. In 1929, a committee was created with a mandate to study the possibility of organizing a Department of Psychiatry at Chicago. They adopted the rhetoric of holism and interdisciplinarity, indicated in their proposal to the General Education Board of the Rockefeller Foundation:

> Human behavior has heretofore been studied from two more or less opposing points of view. On the one hand the physiologist, the psychologist, the neurologist, and the psychiatrist have been mainly concerned with the <u>individual</u>, regarding <u>society</u> as a whole merely as a part of his environment. On the other hand the social scientists, including the social psychologist, the sociologist, the economist, the jurist, and the anthropologist, have recognized that society as a whole is more than the sum of its constituent parts: - i.e., that society itself has its own peculiar characteristics not predictable on the basis of the study of the individual alone.
>
> There has thus developed a distinct gap between these two groups, which must be bridged before even present knowledge concerning human behavior can be considered as a coordinated whole. The importance of the bridging of this gap has been recognized by the social scientists at the University of Chicago in their program for realistic research in the Social Sciences, recently financed in part by the Laura Spelman Rockefeller Memorial. The present document rests upon the assumption that the effort in psycho-neurology is to be coordinated with that in the social sciences...The so-called functional disorders, including the psychoses and psychoneuroses, far from being in a class by themselves, are ultimately to be understood only through an integration of anatomy, physiology, psychology, social behavior, chemistry, and medicine.[6]

As Blustein has observed, this attempt by the members of the "Committee on Psychiatry" to institute such a program at Chicago was rather ill-timed. The General Education Board at the Rockefeller was in the process of being dismantled, and its Progressive-era rhetoric of social amelioration was being traded in for an emphasis on fostering scientific research as an end in itself (Blustein 1993:421). There was also internal conflict among its members, which included one of the founders of the Chicago Psychoanalytic Institute, Franz Alexander, whose appointment in the medical faculty simultaneously infuriated some of the biologically-oriented neurologists, even as it charmed some others.

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[6]"Part II of a Document Entitled 'The Biological Sciences and Psycho-Neurology' as Prepared for the Consideration of the General Education Board and Dated April 10, 1929," RAC (1.1, 216A, 6, 73).

The original committee disbanded, but a new one was struck under Franklin McLean's auspices two years later. A letter from the Office of the President of the University to Max Mason of the Rockefeller Foundation, dated June 26, 1931, indicated the problems with the first committee:

> Dear Max: For your information I send you herewith the minutes and program of the new Committee on Psychiatry. The old Committee which went to pieces on the rocks of psychoanalysis concluded its labors by recommending the appointment of a new one. You will observe that every University interest remotely connected with psychiatry is represented in the present group. The Committee plans to present definite proposals in October (RAC 1.1, 216A, 6, 73).

Earlier that year, Alan Gregg, the Director of the Rockefeller Foundation, had met with McLean to discuss the organization of psychiatry at Chicago. The problem, thought McLean, was the extreme variety of approaches to mental illness that would ultimately hamper the Psychiatry Department's development. Percival Bailey, C.J. Herrick (both neurophysiologists) and Karl Lashley (a psychologist) were all identified as being against psychoanalysis, while Franz Alexander had made plans to leave for Germany, because of the hostility towards him on the part of the Medical Faculty. Gregg himself was plagued by doubts about the direction of the Medical School at Chicago, and recommended that the second committee make no formal proposal for the time being.[7]

Despite Gregg's concerns, the interdisciplinary aspects of the new psychiatry program were stressed in the second committee's proposal. This was likely due to the fact that psychiatric issues were already being studied in the existing departments of physiology, neuroanatomy, psychology, and treated in the new medical facility. The decision before the committee, then, was how it could institute holism in practice: should they opt for rigid centralization, and create an independent Department of Psychiatry? or should a tolerance of approaches prevail? The committee chose a middle road: they suggested that a Department should be established, but that appointments should be drawn from the full spectrum of approaches to mental illness, including

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[7]Diary entry, Alan Gregg, May 8, 1931 (RAC 1.1, 216A, 6, 73).

psychoanalysis. But this interdisciplinary approach was adopted on the principle that it would eventually yield a unified vision of mental illness and treatment. In an address delivered at the meeting of the American Orthopsychiatry Association in New York, February 1931, McLean, who had begun by drawing a distinction between the social and biological sciences, moved to the "psychological and strictly bio-medical aspects of mental disorders," and declared that

> It is my conviction that both types of approach are necessary for continuing progress in the study of mental disorders and that although they may appear to be following parallel lines, they must eventually converge. The most important contribution to psychiatry during the past few decades has been the introduction of methods for investigation of psychic phenomena, but these methods, important and far reaching as they appear to be, do not relieve us of the necessity of carrying on appropriate studies of a more strictly biological nature.[8]

In what may have been an oblique reference to Alexander's provocative work in psychoanalysis, the committee resolved that "in the absence of a Department of Psychiatry there is a tendency for these independent efforts to become firmly established as independent units and that unless some temporary means of coordinating these efforts is established they may prove to be a source of embarrassment to a future Department of Psychiatry."[9]

Biological, sociological, psychoanalytic and psychiatric perspectives were to be brought together at Chicago in an effort to understand disease at a meta-level. These changes also brought with them transformations in professional organization. Clinical faculty members were no longer able to teach part-time and supplement their income through private practice: they were now hired by the university as full-time researchers and obliged to give up their private (and often lucrative) careers.

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[8]Franklin C. McLean, "A University Department of Psychiatry," (RAC 1.1, 216A, 6, 73).

[9]Franklin C. McLean, "Minutes of the first meeting of the committee appointed by the President to advise as to the development of psychiatry in the university, June 18th, 1931," Record Group 1.1, series 216A, box 6, folder 73, Rockefeller Archive Center, Sleepy Hollow, New York.

Kleitman, who was promoted to associate professor of physiology at Chicago in 1929, found himself in the midst of vast and optimistic institutional reforms. He had received his PhD in physiology under Carlson in 1923, and then spent two years abroad, studying under Magnus at Utrecht, and under Piéron and Lapicque in Paris.[10] He was immediately hired as Assistant Professor at Chicago upon his return in 1925, and spent the next few years publishing a number of papers on the physiology of sleep. After Kleitman was granted tenure, the next step was to secure the financing of his research. Carlson was an effective fund-raiser, and often managed to procure gifts for the department from wealthy friends, as well as arranging toxicology and drug tests for manufacturers (Ingle 1979). In 1934, the Wander Company, a food manufacturer, gave Kleitman a grant of $3,500 for one year, which was subsequently renewed the following year.[11] One of the products Kleitman was required to test was Ovaltine, a drink mix that was purported to encourage sleepiness. After a few trials, Kleitman tentatively concluded that three tablespoons of the drink might enhance sleep, although there was no evident effect with only two tablespoons. The manufacturer immediately initiated an advertising campaign, complete with individual testimonies, to the effect that research at the University of Chicago had confirmed that Ovaltine brought on sleep. Carlson objected, and the advertisement was pulled. But Carlson and

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[10]Louis Lapicque (1866-1952), was one of France's premiere neurophysiologists in the early twentieth century. His work centred around the study of "chronaxie," the comparative study of nervous excitation times in various tissues. A 1923 letter to the Harvard neurophysiologist, Alexander Forbes, indicates Lapicque's passion for investigating nervous rhythms: "...I will not renounce my opinion that there is a low-frequency nervous rhythm followed exactly by a muscular rhythm. It is well-understood that this rhythm, which I call elementary, is considered as a little system formed of a single neuron and several muscular fibres. For ensembles of muscle or nerve, the frequency of electrical variation could take the form of synchronous salvos or any other form; but it is only in the first case that we are able to read the rhythm of the traces...From the perspective of general physiology, elementary rhythm is the only interesting thing—is it not so? The rest is contingent and secondary" (AFA11, 498). Lapique's emphasis on synchronous rhythms languished outside of France, just as Keith Lucas's "all or none" hypothesis of nervous transmission took shape in Edgar Adrian's work. The move to nominate Lapicque for the Nobel Prize, headed by Charles Richet (fils) and Henri Laugier in 1932, fell flat. Sherrington and Adrian shared the prize instead. On Lapicque, see Jean-Claude Dupont 1994.

[11]See Kleitman's letter (February 18th, 1936) to Dr. Robert A. Lambert of the Rockefeller Foundation, outlining his funding sources (RAC 1.1, 216A, 7, 88).

Kleitman were the butt of jokes around the department for a long time thereafter. A cartoon depicting a newly-wed bride gazing at her sleeping husband on the bed with the caption "Damn that Ovaltine!" became a permanent feature on the departmental bulletin board (Ingle 1979:S126).

Carlson was a great supporter of Kleitman's research, and he made a concerted effort to get his student funded by the Rockefeller Foundation. Just as the neuropsychiatry was beginning to take shape at Chicago in 1935, Carlson forwarded Kleitman's name as a possible candidate who could contribute some physiological expertise to the new program. The program, which would eventually be located in a twelve-bed unit at Billings Hospital, was to be led by psychiatrist Roy Grinker. Grinker, who would abruptly quit the program in 1936, was scheduled to return from two years of psychoanalytic training in Europe in 1935 (Blustein 1993). By March of 1934, Carlson had already arranged for Alan Gregg to meet with Kleitman during a conference in New York City.[12]

Kleitman on sleep

Shortly after Carlson contacted Gregg, Kleitman sent Gregg an outline of the work he had conducted on sleep since 1922.[13] Kleitman focussed on two classic problems: what was the immediate cause of sleep? and what caused the diurnal (24-hour) rhythm of sleep?

To answer the first question, Kleitman had simply adopted Piéron's method of "experimental insomnia" in his animal studies, and applied a variation with human subjects. (Kleitman 1923). Six young male students at Chicago had been deprived of sleep for 40 to 115

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[12]Letter, Carlson to Gregg (March 12, 1934); letter, Gregg to Carlson (March 15, 1934) (RAC 1.1, 216A, 7, 88).

[13]Letter, Kleitman to Gregg (March 21, 1934) (RAC 1.1, 216A, 7, 88).

hours, submitting to a battery of physiological and psychological tests throughout the period of wakefulness. Mental tests revealed that the ability to conduct routine calculations remained constant, but attention frequently strayed. Subjects reported a "slight buzzing in the head," or a "sensation of emptiness," as the period of insomnia wore on. Kleitman, the only subject who was able to stay awake for 115 hours, even became somewhat delusional during a routine mental test:

> He [the observer] found that the subject invariably located the logarithms and called them off correctly, but once in response to a number whose logarithm he had located, instead of calling off the latter, said: "It is because they are against the system." On being questioned the subject admitted that all the time he was looking up the logarithms he had been under the impression that he was having a heated argument with the observer on the subject of labor unions (Kleitman 1923:73).

Physiological parameters also changed: the heart rate slowed; blood pressure dropped. Core temperature, however, remained stable. One of Kleitman's most important observations, however, had come from introspection. Subjects found that sleep became irresistible when they were allowed to relax. Sleep was most easily brought on not by depriving the subject of external stimuli (light, sound, etc.), but by eliminating proprioception—the feeling of one's own muscle tone. Although he did not claim to have isolated any single cause of sleep, Kleitman concluded that muscular relaxation was an essential factor for the appearance of sleep; conversely, muscular tension was necessary for wakefulness. After all, no one fell asleep at dance marathons! Fatigue favoured relaxation, which in turn generated sleep. Extreme fatigue, however, caused pain, thus increasing the afferent stimuli from the muscles, and ending in insomnia. With this relocation of the cause of sleep from external stimulus to proprioception, Kleitman perpetuated the functionalist tradition of assigning purpose to feelings—the sensations that came from one's own body.

Kleitman's emphasis on the *physiological* parameters of sleep, however, encouraged him to describe consciousness in passive terms; it appeared when there was enough stimulus, and disappeared when this amount slipped below a required threshold. Kleitman recognized that such a mechanistic theory could not account for the diurnal periodicity of sleep, which he explained in terms of conditioned reflexes (Kleitman 1923:90). All Pavlovians had agreed that conditioned reflexes originated in the cerebral cortex. Kleitman fit this theory of cortical activity in with the British neurologist Hughlings Jackson's "doctrine of levels," which stated that the higher functions of the brain (located in the cortex) were the most recent, in evolutionary terms, and were also the first to break down with the onset of neurological disease. The lower, sub-cortical regions were the source of vegetative functions, such as the regulation of blood pressure, respiration, heart rate, muscle tone, and so on. Kleitman had observed that the diurnal rhythms of these latter functions could be discerned throughout periods of experimental insomnia. The psychological phenomena of attention and association could, with effort, function normally at any time. In his letter to Gregg, Kleitman suggested that this dualistic approach put him in a position to "settle the controversy concerning localization of a sleep 'center' in the cerebral cortex or in the subcortical structures."[14] This question had, of course, been rendered more acute by the outbreaks of encephalitis lethargica during the 1920s. Kleitman's hypothesis was that there were actually *two* centres for sleep. The first was sub-cortical—it worked on a diurnal basis, "without any relation to day and night," and could be found in "lower animals, the very young of higher animals and man, and in decorticated dogs." The cortical mechanism, on the other hand, only developed in higher animals "as a result of individual experience and is responsible for adapting sleep and wakefulness to the 24-hour cycle of day and night."

Kleitman was constructing an experimental program of sleep research that was simultaneously physiological, psychological, and sociological. It resonated with the interdisciplinary and holistic tenor of the proposal put forward by the Committee on Psychiatry. Sleep was an internal physiological necessity, but its rhythm, Kleitman argued, was learned—it was a psychological adaptation to the social and cultural conditions that demanded long periods of wakefulness. Kleitman's research was tailor-fit for the new unit at Billings Hospital, where he told Gregg that he hoped to study "the sleep and diurnal variation in performance of mental defectives, from the lowest imbecile to the highest grade moron, to determine the extent to which

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[14]Letter, Kleitman to Gregg (March 21, 1934) (RAC 1.1, 216A, 7, 88).

the cortical center of sleep dominates the subcortical one." In addition, he wanted to study the sleep disturbances in cases of encephalitis lethargica, "the sleep habits of psychopathic individuals," and "the various types of insomnia met with clinically."[15]

Gregg was cautious at first. In the interview, Kleitman had emphasized the fact that his research could continue at the physiology department, but that he would need more money to expand it. The greatest cost involved was in finding human subjects that were willing to participate in such long-term experiments. This problem could be avoided, argued Kleitman, if Gregg would integrate sleep research with the psychiatry project at Billings Hospital.[16] Kleitman must have told Carlson of Greg's hesitant reaction. In May, Carlson wrote Gregg, thanking him for meeting Kleitman, whose work he described as "a significant part of the problems in neuro-psychiatry that are being pursued on this campus." Unfortunately, Carlson continued, he was unable to support Kleitman's research, because of reductions that had been made to the Physiology Department's budget.[17] He intimated that Gregg should perhaps grant Kleitman a second audience.

That September, Kleitman, who had spent the summer in Europe, tried to meet with Gregg upon his return to the U.S.[18] He only managed to see Gregg's secretary, who assured him that Gregg had not forgotten about the issue, and was planning on discussing it with colleagues at the Rockefeller Foundation in the fall. Winter arrived with no word from Gregg. In January, 1935, Carlson crafted a stronger plea on Kleitman's behalf.[19] He reminded Gregg that "you seemed to be interested in this extension [of Kleitman's work] but at that time were not certain in

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[15]Letter, Kleitman to Gregg (March 21, 1934) (RAC 1.1, 216A, 7, 88).

[16]Alan Gregg, diary entry (March 28, 1934) (RAC 1.1, 216A, 7, 88).

[17]Letter, Carlson to Gregg (May 8, 1934) (RAC 1.1, 216A, 7, 88).

[18]Letter, Kleitman to Gregg (September 10, 1934) (RAC 1.1, 216A, 7, 88).

[19]Letter, Carlson to Gregg (January 8, 1935) (RAC 1.1, 216A, 7, 88).

your own mind as to whether it would be best to arrange for independent support of the work on sleep or to merge it into the larger project of psychiatric research which it [*sic*] was proposed to launch at the University of Chicago and financed by your Foundation." "Dr. Kleitman and I," he continued, "are naturally anxious for some word from you concerning this matter."

Gregg arrived in Chicago a few weeks later. In his diary entry for January 20th, Gregg carefully noted Carlson's unfailing enthusiasm for Kleitman's work, even though Carlson was "in characteristic turmoil" in his struggle against the antivivsectionists.[20] But Gregg was more impressed with how very differently Kleitman presented himself and his work in this second interview, which was held at Kleitman's laboratory instead of Gregg's office. Surrounded by the various devices—many of which he had invented himself—that provided a visual picture of sleep, Kleitman was, to Gregg's surprise, animated, precise, and clear [Figure IV]. Kleitman's transformation clearly made a good impression on Gregg, who decided to fund the project:

> Dr. Kleitman: Review of his project for study of sleep. Told him I would recommend for favorable action. Interview is remarkable illustration of advantages of seeing a man in his own laboratory as contrasted with interview at RF [Rockefeller Foundation] offices.[21]

Several years later, when Gregg's own optimism about the value of Kleitman's work had disappeared, he continued to emphasize the importance of the laboratory setting when making funding decisions:

> Though not peculiar to this particular project I shall always remember this undertaking for one point that was conspicuous: it is dangerous to believe that the presentation of a request in our

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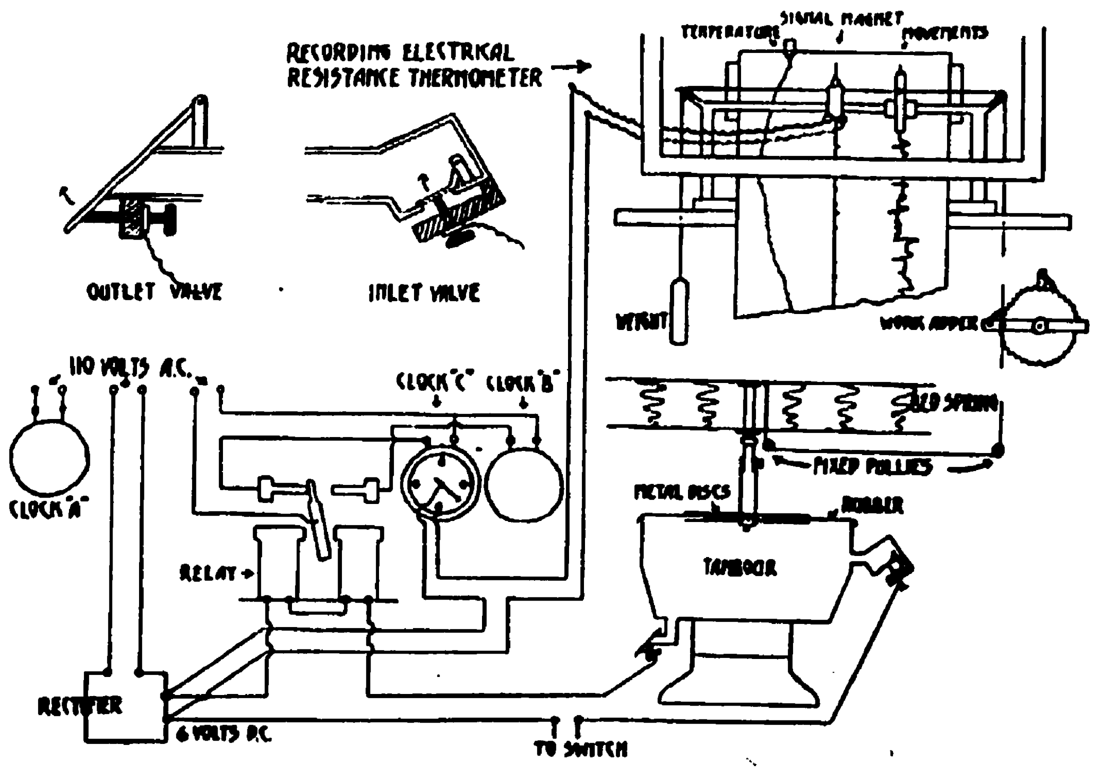
[20]Alan Gregg, diary entry (January 20, 1935) (RAC 1.1, 216A, 7, 88).

[21]Diary entry, Alan Gregg, January 20, 1935, Record Group 1.1, series 216A, box 7, folder 88, Rockefeller Archive Center, Sleepy Hollow, New York. Gregg was not the only one to be charmed by such apparatus. Press coverage of Kleitman's work, as well as some biographical sources, frequently referred to his graphical devices, which were sometimes described as having been "inspired by Rube Goldberg's inventions." See his entry in *Current Biography 1957* (p. 308), and *The New York Times* (Nov. 12, 1939, II, p. 7; Aug. 7, 1949, p. 45; May 17, 1953, IV, p. 11).

Figure IV
Kleitman's device for recording motility in sleep
(Kleitman 1923)



> offices is likely to be about the same as the presentation in the laboratory of its origin. Kleitman had begun his scientific career as a diener [a medical laboratory assistant] at the Rockefeller Institute. He must have reverted to his old mood in his first presentation in our office, for only on second presentation in his laboratory did he do himself justice. My conclusion is never to judge a man out of his laboratory if you can avoid it.[22]

From April of 1935 until October of 1938, Kleitman received funding totalling just under $16,000 from the Rockefeller Foundation.[23] His project, however, was plagued with difficulties. Grinker returned from Europe to start up the department in the spring of 1935, and, in a letter to Gregg in October of that year, Grinker offered an almost idyllic description of the twelve-bed unit. There was a hydrotherapeutic room, a psychology laboratory for the study of aphasia and the behaviour of patients with frontal lobe damage, and the rudiments of a chemical laboratory for drug studies. Ninety percent of the patients were schizophrenics, and Grinker was conducting pharmaceutical, metabolic and "electrical" studies in order to determine "what differentiates schizophrenics biologically."[24] But "modified analytic methods" were also used in such cases, and were also applied to the study of phobias in children in the pediatric unit. Clinical conferences were held four mornings every week, and were well-attended. Although the Sociology Department had not yet set up formal relationships, the Department of Education had expressed considerable interest. Sleep research had not yet been established, but Gregg's optimism about future interdisciplinary relationships took Kleitman's work as an exemplar: "Cooperation with other people on the campus has been much better than I had anticipated. We are arranging so that Kleitman can do shis [*sic*] work on "Sleep" on [*sic*] our unit, and will admit patients especially interesting to him."[25] In a letter Kleitman wrote to Gregg early in 1936, he

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[22] "University of Chicago—Physiology of Sleep. Appraisal, June, 1939" (RAC 1.1, 216A, 7, 89).

[23] "University of Chicago—Physiology of Sleep. Appraisal, June, 1939" (RAC 1.1, 216A, 7, 89).

[24] Letter, Roy Grinker to Alan Gregg (October 23, 1935) (RAC 1.1, 216A, 6, 75).

[25] Letter, Roy Grinker to Alan Gregg (October 23, 1935) (RAC 1.1, 216A, 6, 75). The quotation marks around the word "sleep" might be an indication of how novel such research would have appeared to psychiatrists at the time. Blustein claims that, in 1935, "Grinker was still

cited the extraordinary length of time it took him to set up his laboratory at the hospital as the reason he had failed to spend an appropriate amount of his funding before the end of March.[26]

Things quickly began to fall apart. Just as a divisional committee on neurobiology was struck in early 1936, presumably to protect the biological orientation of the psychiatry unit, Grinker left the university to take up a position at the Michael Reese Hospital. Grinker had already expressed his dissatisfaction with the domination of the unit by medical interests. When he discovered that he was paid less than his clinically-oriented colleagues, he revolted.[27] Several other members, in particular Bailey, were keenly dissatisfied with their situation. Blustein argues that the problem was simultaneously institutional and economic (Blustein 1993). Biology departments did not recognize the work done in the psychiatric unit as being properly biological, which stripped researchers like Bailey of their authority in the laboratory sciences. Clinicians in the unit, on the other hand, were obliged to pool their fees received from the private patients they saw at the university hospitals, because Chicago, unlike most other medical schools, did not have access to the charity patients at a public hospital. Patients were being charged for "the dubious privilege of being used as 'clinical material'." On the clinician's side, the university's level of remuneration was not competitive when compared to private practice. Research-oriented neuropsychiatrists quickly discovered that their clinically-oriented colleagues were better-paid, and the clinicians themselves recognized they could make more money in private practice. The

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optimistic about 'the development of a real psychobiology encompassing...disciplines from chemistry to psychoanalysis as representative of the polar extremes' in his psychiatry department" (Blustein 1993:425). Although this sentiment certainly captures the tone of Grinker's letter, I have been unable to find these words in this four-page document, which Blustein cites as her source.

[26]Letter, Kleitman to Gregg (January 20, 1936) (RAC 1.1, 216A, 7, 88).

[27]Letter, Grinker to Woodward (March 27, 1935) (RAC 1.1, 216A, 6, 76); letter, Grinker to George F. Dick (Chair, Medical Department, University of Chicago) (January 27, 1936) (RAC 1.1, 216A, 6, 76).

full-time system of research and teaching that had marked physiology's independence from medicine had forced investigators to choose between the two alternatives.

The interdisciplinary aspect of the department of psychiatry had almost completely unravelled by 1939. In July of 1936, Grinker was replaced by David Slight, a psychiatrist from McGill University. Although his research interests—migraine, endocrine function in emotional states, the physiological effects of psychotherapy, the role of allergies in mental disease—were diverse, he seemed to impress no one, and was described in an early assessment as "a competent teacher and an able administrator," but "not a brilliant investigator."[28] More importantly, his background and research direction was strictly clinical. This proved to be a problem for Kleitman, who had forged an alliance with Grinker on the basis of their common biological interests, only to see it shattered upon the former's departure.

Kleitman published a mere four papers during his three-and-a-half years at Billings Hospital, and none of them were in medical journals, despite the fact that the motion for his original grant cited "catatonic states, post-encephalic sleepiness, catalepsy, insomnia, and narcolepsy" as the important medical questions that would be illuminated by his work.[29] As he informed Alan Gregg on several occasions, he was working on a monograph on sleep—the massive tome that would become *Sleep and Wakefulness* in 1939. Kleitman's continuing biological orientation was revealed most clearly to Gregg when he made a request for additional funding to travel to Norway in order to conduct research on the stability of the diurnal rhythms of

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[28]"University of Chicago—Sub-Department of Psychiatry. Appraisal, June, 1938 (RAC 1.1, 216A, 6, 78). On Slight's research interests, see Arthur C. Bachmeyer to Gregg (letter, June 22, 1937) (RAC 1.1, 216A, 6, 76).

[29]"University of Chicago—Department of Physiology. RF 35026" (RAC 1.1, 216A, 7, 88). The fact that Kleitman was able to continue his research for such a long period after Grinker left was probably due to the timing of his grant. Less than a month before Grinker left, Gregg extended Kleitman's grant to October 1938. See diary entry, Alan Gregg (January 14, 1936) (RAC 1.1, 216A, 7, 88).

sleep in the continuous summer light of the Arctic summer.[30] This was clearly a continuation of his famous "Mammoth Cave" experiments, in which he and an associate spent six weeks in a cave in Kentucky attempting to adjust themselves to sleep/wake schedules other than the standard 24-hour rhythm [Figure V].[31] Gregg was unimpressed, and turned him down flat.[32]

When Kleitman's funding expired in October of 1938, Gregg was clearly in no mood to resuscitate any part of it. His final appraisal was harsh, and it had been constructed from an eminently practical standpoint. "It is not evident," declared Gregg, "that the grant for Kleitman's work on sleep has provided results proportionate to the total expenditure of $15, 583.74."[33] In Gregg's mind, Kleitman's research had been a dead end, despite its empirical productivity: "there is little to report beyond saying that the grant produced facts about sleep which are not of much significance for all that they are conscientiously planned and accurately obtained."

Worse still, Kleitman's research had failed to encourage the sort of internal cohesion that Gregg had hoped to see at the University of Chicago: "the hope that in aiding in physiological study of a condition intimately related to exhaustion and to the recuperation of the activity of the nervous system, an affiliation with psychiatry would be established as a characteristic of the Department of Physiology at Chicago has not been realized." Gregg recognized that this was not entirely Kleitman's fault, however, and he cited the early departure of Grinker, as well as the fact that an association with Slight "never materialized," as reasons for the project's failure. Kleitman was described as a "thoughtful, hardworking, unobtrusive subordinate" who was unable to gain

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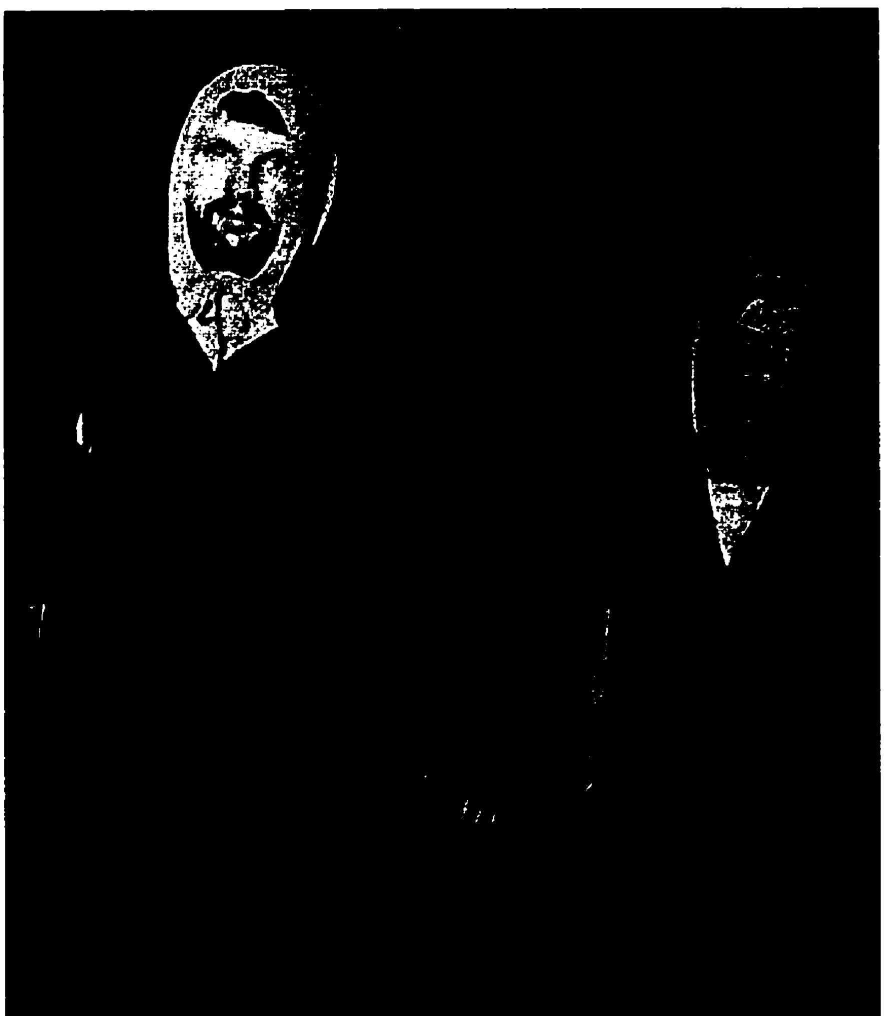
[30]Letter, Kleitman to Gregg (January 14, 1938) (RAC 1.1, 216A, box 7, 89).

[31]Kleitman was unable to adjust to such schedules, but his younger colleague, Bruce Richardson, was able to survive on a 28-hour schedule. For popular coverage of the event, see *The New York Times* (July 4, 1938, p. 10) & (November 12, 1939, II, p. 7).

[32]Letter, Gregg to Kleitman (January 20, 1938) (RAC 1.1, 216A, 7, 89).

[33]"University of Chicago—Physiology of Sleep. Appraisal, June, 1939" (RAC 1.1, 216A, 7, 89).

Figure V
Nathaniel Kleitman (l.) and B.H. Richardson (r.)
emerging from Mammoth Cave, Kentucky, 1938.
Dement (1972)



the attention of Slight. Gregg decided that in future projects, interdepartmental ties should be forged between departmental heads, and not by junior members.

Gregg, whom Kleitman wanted to personally thank in his preface to *Sleep and Wakefulness*, concluded his assessment with a classic trope in the annals of sleep research: "the research results are negligible without being false or useless...Sleep remains a most common phenomenon, marvellous to the few and attractive as a major interest to almost no one. It is possible that this grant illustrates the error of attacking a large task in a small way."[34]

Sleep and Wakefulness, 1939

Kleitman's *Sleep and Wakefulness as Alternating Periods in the Cycle of Existence* (1939) represented the culmination of Kleitman's experience under Carlson. It was also a testament to the early failure of the Rockefeller Foundation to establish an interdisciplinary neuropsychiatric unit at Chicago. Kleitman's precise experimental style, his description of physiological activity as inherently rhythmical, and his quest to design and construct instruments that could visualize sleep can all be traced back to Carlson's influence. Kleitman's "evolutionary hypothesis," which argued that sleep's diurnal rhythm was not inherited but conditioned, reflected the Rockefeller's attempt to bring social and cultural factors under the purview of biomedical research.

Like Carlson, Kleitman was inspired by, but strongly critical of, Pavlov's approach to physiology. Pavlov had constructed an elaborate theoretical structure and nomenclature to

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[34]"University of Chicago—Physiology of Sleep. Appraisal, June, 1939" (RAC 1.1, 216A, 7, 89). Kleitman was encouraged by his colleagues at Chicago not to acknowledge Gregg by name in *Sleep and Wakefulness*, so he sent Gregg a letter of thanks instead. Gregg was relieved not to have been mentioned in Kleitman's book (letter, Kleitman to Gregg, January 19, 1940; letter, Gregg to Kleitman, January 12, 1940, in RG 1.1, 216A, 7, 89).

explain how the cerebral cortex functioned in conditioning. Carlson and Kleitman, on the other hand, relied on self-inscribing instruments to make their case about the periodicity of digestion or sleep. The graphical method was a far more potent and well-established physiological tool than the collection, measurement, and tabulation of drops of saliva. The former offered a medium of communication that could easily be standardized to inscribe and calibrate almost any physiological phenomenon (blood pressure, heart, pulse, and respiratory rate). The epistemological force of the latter, however, depended upon an experimental practice that was difficult to standardize, and could always be contaminated by uncontrolled stimuli, as Pavlov himself recognized.

In terms of the study of sleep, the method of conditioned reflexes had grave shortcomings that hearkened back to the heyday of hypnotism. Sleep was always induced in Pavlovian experiments—the phenomenon itself was artificial. This did not fit well with the subjective experience of sleep as a routine of fatigue emerging at the end of a long period of wakefulness. Claparède had expressed such concerns in 1934, and Kleitman had embraced them in his adaptation of Piéron's method to the study of human subjects (Kleitman 1923; Dumas 1934:IV.509). In Kleitman's hand, the graphical method became a visual analogue of the subjective experience of embodiment, as it applied to sleep.

Yet Kleitman did not entirely abandon Pavlov's conception of the conditioned reflex. By combining it with a Jacksonian perspective on cerebral organization, Kleitman arrived at an evolutionary theory of sleep (Kleitman 1939). This theory could account for both the physiological mechanism of sleep onset *and* the diurnal alteration of sleep and wakefulness. The mechanism functioned just like the stomach contractions of Carlson's subject Vleck, which operated entirely independently of any external stimuli. The effects of this mechanism, Kleitman argued, were best observed in decorticate dogs, newborns, and anencephalous babies, because all these subjects tended to remain awake only so long as they needed to meet their internal demands of nutrition and excretion. Kleitman named this phenomenon the "wakefulness of necessity." It was phylogenetically primitive, and Kleitman associated it with the sub-cortical brain structures

Economo had claimed were damaged in victims of encephalitis lethargica. "Wakefulness of choice," on the other hand, was an example of the vast evolutionary advance over the restrictive demands of these sub-cortical structures. It enabled higher organisms to maintain a state of full awareness even when there were no internal bodily demands to be satisfied. This was a phylogenetically young system that Kleitman attributed to the powerful influence of the cerebral cortex on behaviour.

The 24-hour rhythm of sleep and wakefulness, Kleitman argued, was in no way directly caused by the alteration of day and night. Its causal influence was mediated through the culture of training—the process of establishing conditioned reflexes—that connected the two rhythms. The very local context of the neuropsychiatry project at the University of Chicago, which attempted to integrate sociological, physiological, and psychological phenomena into a holistic and interdisciplinary program of research, left an imprint on Kleitman's thinking about sleep. His evolutionary theory of sleep had substantial social consequences, as it argued for a flexibility of the sleep/wake schedule that, Kleitman hoped, had consequences for fields as disparate as work physiology and child rearing (Kleitman 1943, 1949a, b; Kleitman & Jackson 1950; Kleitman & Engelmann 1953). Like the functionalists, lead by Harvey Carr in the Psychology Department, Kleitman had framed wakefulness itself in terms of purpose.

Edmund Jacobson

This connection between psychology and physiology was being pursued by Edmund Jacobson, one of Kleitman's colleagues in the Department of Physiology. During the 1920s and 30s, Jacobson, who had trained as a clinician and as a psychologist, had created a form of relaxation training that would eventually lead him to examine some of the same phenomena of sleep that interested Kleitman. Although Jacobson's work culminated as a clinical therapy, rather than an investigative technique, he, like Kleitman, invoked an image of sleep that emphasized the concepts of efficiency and purpose, as well as the importance of proprioception.

Carlson's department at Chicago was of a decent size by contemporary American standards. Its share of leading researchers (as judged by the number of papers they published) consistently compared to Harvard, Washington University at St. Louis, and Johns Hopkins by around 1900 (Geison 1987). In the academic year 1930-1, shortly before neuropsychiatry was established in Chicago, Carlson's department boasted three professors, two associate professors, one assistant professor, two research associates, five assistants, and six fellows, for a total of nineteen people actively engaged in research (Ingle 1979:S125-S126). By this time, Kleitman, who had just been made an associate professor in 1929, was completely consumed by his work on sleep. Yet even in this small department, he was not the only one interested in sleep. Although it is not clear to what extent Kleitman and Jacobson influenced each other's work, each was certainly aware of what the other was doing.

Edmund Jacobson (1888-1983) approached sleep less from a biological perspective than from a clinical and psychological one. The pressing clinical issue for him was not the function of sleep or its evolutionary development. It was instead the problem of insomnia, and its relationship to hypertension. Jacobson was the early twentieth-century heir to neurologists such as Hammond and Weir-Mitchell, rather than a direct descendent of physiologists such as Bernard, Marey, or Mosso. But despite these differences, the interdisciplinary bent of the University of Chicago in the 1930s brought he and Kleitman together. They both worked on sleep and relaxation in Carlson's department, as well as in Grinker's neuropsychiatry unit at Billings Hospital.

What was Jacobson's role in the formation of sleep research as a scientific field in Chicago in the interwar years? Because I have attempted, to a certain extent, to follow the pathway of citations found in major texts, Jacobson rarely finds a place in the annals of sleep research, either as a physiologist or a clinician. His work was marginal and unorthodox, and has been ignored in sociological and historical accounts of sleep research (Dement 1972; Lemaine *et al.* 1977; Schiller 1982; Hobson 1988). His work has been touched upon by historians interested in the concept of inhibition, the "imageless thought" controversy that plagued early twentieth-century psychological research, and the rise of psychosomatic medicine (Tweney 1987; Gessel 1989; Smith 1992; Levenson 1994). The simple fact that he and Kleitman both worked on sleep at Chicago during the 1930s is not enough to warrant including him in a history of the discovery of REM. But if we examine some of his research, as well as several of the connections that he made with Kleitman (and his student, Aserinsky), certain similarities that were simultaneously intellectual, institutional, and technical appear. Both emphasized the relationship between sleep and the internal sensation of muscular tension. Both participated in the interdisciplinary efforts in neuropsychiatry at Chicago. And finally, both emphasized the importance of technical innovation and graphic representation in their respective studies of sleep. An examination of Jacobson's work, then, will provide some crucial insight into the context in which sleep research developed at Chicago in the interwar period.

Jacobson was born in Chicago in 1888, the only child of a Jewish-American entrepreneur.[35] His interest in the psychological and biological effects of nervous tension apparently date back to when he was ten years old. A fire broke out in a hotel that his father owned, and in which the family lived, and the young Edmund was surprised to see how the residents of the hotel, whom he had come to know, behaved like completely different people when confronted with this terrifying situation. Regardless of the veracity of this story, the study of nervous excitability was a prominent aspect of psychological and neurological research in the U.S. around the turn of the century, spurred on by George Beard's 1881 book, *American Nervousness* (Grob 1983; Lutz 1991; Shorter 1992). Jacobson's interest in nervous illness was just as much a reflection of his age, as it was a product of a traumatic childhood event.

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[35]Biographical material on Jacobson comes from Gessel 1989, interviews I conducted with Helene Morcos and Richard Lange, and personal communication with Arnold Gessel and Jacobson's son, Edmund, Jr.

The "imageless thought" controversy

Jacobson's clinical interests, however, were preceded by a curiosity about the relationship between body and mind. He completed his undergraduate study of psychology at Northwestern University in 1908, and immediately began graduate work in psychology and philosophy at Harvard under William James and Josiah Royce (1855-1916). After completing his doctoral thesis on inhibition, he received his PhD in 1910 (Jacobson 1911b, 1912). He then went to Cornell University to study "systematic experimental introspection" with the psychologist Edmund Bradford Titchener (1867-1927). Titchener, who had studied under Wundt from 1890 to 1892, at which time he took a job at Cornell, was the great bulwark of experimental introspection in America. Titchener's physiological training under Burdon-Sanderson at Oxford did not make him any more sympathetic to functionalist explanations, nor did he approve of the decidedly practical bent of the behaviourist upstarts. Titchener's passion for the study of the normal human mind never really fit in with the mainstream of American psychology, which was rapidly turning towards the study of mental performance and individual difference. Just as psychological research was turning towards the testing and classification of naïve subjects, Titchener remained mired in the baroque practice of training students in the art of experimental introspection.

Jacobson arrived at Cornell just as Titchener's laboratory was becoming embroiled in an international debate that would eventually contribute to the collapse of introspection as an investigative method (Tweney 1987; Kusch 1995). At the centre of the dispute was a question about reductionism: was thought composed merely of sensations, feelings and images (Titchener's "structuralist" position)? or was thought a non-reducible, independent component of mental activity? Karl Bühler, who worked under Oswald Külpe at Würzburg, held the latter position. By deploying a method of introspection (*Ausfragemethode*) that allowed for a free and sympathetic communication between the experimenter and observer, Bühler claimed to have discovered thought-elements having no relationship to any of the structuralist components, which were themselves psychological analogues to physiological processes. Proper introspective training, Bühler argued, could generate evidence that thought could appear without images. An examination of how the meaning of a proverb was grasped by the introspective observer could, for example, reveal a process that made no reference to sensory events.

Titchener challenged the methodology of the "Würzburg School," by arguing that it had allowed too great a role for suggestion (Tweney 1987). Between 1909 and 1912, his laboratory at Cornell produced a number of studies that addressed the question of imageless thought.[36] These studies included an important paper by Jacobson, whose technique of examination amounted to demanding that the observers separate "conscious processes" from "statements concerning meaning:"

> What we desired was that attributive description of conscious processes should be marked off, by the observers themselves, from whatever else might enter into the reports, and we accordingly required them to *put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside.* The procedure was justified by the results: for though failure to specify now a meaning and now a process was at first not infrequent, it grew less and less common with practice, until the twofold report became characteristic of the experiments (Jacobson 1911a:554-555. Italics original).

Jacobson readily accepted the fact that this amounted to a new form of training—an extension of the introspective observer's task in the psychological laboratory. But this in no way damaged the value of their reports, because this new training effectively transformed Bühler's argument for the need for an *empathetic* relationship between the experimenter and the observer into a new set of explicit instructions that the observer was obliged to follow.

Although Jacobson left Cornell after only a year, his work continued to examine how the abilities of an introspective observer could be expanded through practice and training. The problem of imageless thought was never resolved; rather, it was simply abandoned by behaviourists who rejected experimental introspection as an investigative practice. This left Jacobson somewhat hesitant to theorize about the nature of consciousness at all. Instead, he tried

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[36]There were six papers published in the *American Journal of Psychology* (which was edited by Titchener himself): two were by Titchener, and the remainder were by Jacobson, T. Okabe, H.M. Clarke, and W.H. Pyle.

to turn introspection into a useful practice by harnessing it to the clinical problem of hypertension and relaxation. In a paper that described his technique of "progressive relaxation" to psychologists, he insisted that he was "not seeking to prove a motor or any other theory of consciousness" (Jacobson 1925a:74).[37] The motor theory of consciousness was an attempt to resolve the introspective evidence of mental imagery with the behaviourist's emphasis on the objective observation of movement. It held that all mental images were accompanied by a movement in the corresponding sensory system (Washburn 1916). Despite his protestations, Jacobson's work was clearly in this same vein. He used the language of inhibition to describe how muscle tension and thought were intimately connected. A decrease in muscle tension, brought about through the practice of relaxation, effectively inhibited thought. Jacobson's original definition of "inhibition" had separated the data of psychology and physiology along the fault line of fatigue: "Inhibition may be defined as the reduction of any conscious activity while the stimulus is in operation and undergoes no corresponding diminution. The reduction may be either partial or complete, but must not be due to fatigue. This definition is strictly empirical, and is designed to cover all past work in the psychological field" (Jacobson 1912:345). Thirteen years later, Jacobson had turned this early research into "progressive relaxation," a prototype of the psychosomatic medicine that was just coming into vogue in the 1920s and 30s.

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[37]Titchener himself must have reviewed this paper initially as he wrote to Jacobson in 1924, saying that he was awaiting the arrival of Jacobson's manuscript, and that he would be "very glad to look over the paper," and was "very glad indeed" that Jacobson had "found the time to write." A year and a half later, Titchener thanked Jacobson "for the two papers on Voluntary Relaxation." Although Titchener's first letter indicates that he had a quite close personal relationship with Jacobson—Jacobson and his mother had gone on "excursions" with Titchener and his family—these two letters are all the correspondence held in Titchener's archive at Cornell (TC, letters, Titchener to Jacobson, April 10, 1924 & October 23, 1925).

Progressive Relaxation at the University of Chicago

After his year with Titchener, Jacobson returned to Chicago to study medicine at Rush Medical School, which was then affiliated with the University of Chicago. He joined A.J. Carlson in the Physiology Department, teaching a course there for one quarter, and then began his studies at Rush. His decision to pursue medicine appears to have been at least partly determined by family responsibilities: his father was a transient businessman who had little to do with the family, and his mother, who refused to leave Chicago, had put the young Edmund through university by working as a court reporter and a typist.[38] After finishing his courses at Rush, Jacobson planned to split his internship between Johns Hopkins in Baltimore and the Cook County Hospital in Chicago. He wrote a letter to Adolf Meyer in 1915, indicating both his desire to study under the leader of American psychiatry, and his keen financial need:

> Dear Dr. Mayer [*sic*]:
> Enclosed please find a card of introduction from Dr. Bassoe, in connection with my desire to put in about six months—up to about March 1st—in psychiatrical [*sic*] work.
> I am a graduate of Rush Medical College and have the PhD in experimental psychology from Harvard. Also I have been associated with the psychology department at Cornell, and the physiology department at the University of Chicago. My publications have appeared in the psychological journals, and I should be glad to send reprints if desired.
> I should greatly appreciate an opportunity to work at your institute. As I must earn my expenses, it would be necessary for me to arrange for some salary if your arrangements permit.[39]

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[38]Jacobson apparently assumed a great deal of responsibility for his family at a very young age. Morcos (*1998) told me that Jacobson's father "would make money, and they would live in a nice place, and he would lose all the money, and they wouldn't have any place. And he'd [Edmund] have to go out...I think they depended on him, at least his mother depended upon him to take care of the business things. From the time he was...I mean, he talked about being basically a child, ten years old, going out finding an apartment for them to live in, or a house."

[39]Meyer Papers, Alan Mason Chesney Medical Archives (July 12, 1915).

Meyer was pleased to bring Jacobson into the Phipps Psychiatric Clinic, but advised him only his room and board would be paid for, "unless some means can be secured for some addit[ional] remuneration for special services." His internship was to begin in September of 1915.[40]

Jacobson was pleased that he could work six months at Hopkins and six months at Cook County, and told Meyer that he would accept the offer. There was only one caveat: "In case of the death or resignation of one of the men [at Cook County] whose service begins September 1st, I might have to undertake the service at that date. But I assume this will not occur." Meyer wrote back August 7th, saying that "I am very glad to hear of your decision," and that he was "looking forward to an interesting winter of collaboration with you"[41]

Unfortunately for Jacobson, the unhappy event actually occurred:

August 21, 1915

Dear Dr. Meyer:

In the midst of my preparations to go to Baltimore I have been suddenly notified of the death of Dr. Ellison. He was drowned while out swimming. Because of this I have word from the Cook County Civil Service Commission that my service must begin Sept. 1 instead of March 1, and their letters state that a failure to commence on that date will constitute a rejection of the service.

In my letter to you of July 31 I mentioned this possibility of death of one of the men whose service begins on Sept.1; but naturally, I assumed it would not occur. It seems strange that I should be held back in this way. I need not say how deeply disappointed I am, as I have been looking forward with great relish to a winter with you in Baltimore. And I hope and trust that the matter is only postponed, and that I can arrange to come to you after my service here at Cook County Hospital, when I shall have no ties in the West at all.

Again let me express to you my deep appreciation, and believe me

Sincerely yours
Edmund Jacobson[42]

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[40]Meyer Papers, (July 23, 1915).

[41]Jacobson to Meyer (July 31st, 1915), Meyer to Jacobson (August 7th, 1915).

[42]Jacobson to Meyer (August 21st, 1915).

Jacobson thus began his internship at the County Hospital, a large institution of some 2,000 beds, in the autumn of 1915. He never worked with Adolf Meyer, whose influential and eclectic approach to psychiatry would certainly have complemented Jacobson's own efforts in this field.

Jacobson's experience at Cook County seems to have left a great deal to be desired. Helene Morcos, who was his assistant at Jacobson's Laboratory for Clinical Physiology from 1961 until his death in 1983, described a number of difficulties that Jacobson had as an intern at the hospital. Initially, the hospital refused to allow him to intern there at all, simply because of his Jewish background. After he threatened to sue the hospital, they accepted him, but did not assign him to any particular ward. So he would "make his own assignments" and take whatever patients he could (Morcos* 1998). This experience, he told Morcos, was what had made him such a good diagnostician.

After he left Cook County, Jacobson divided his time between research at Carlson's laboratory, clinical work at the Michael Reese Hospital, and developing his own private practice. He also worked closely with Harvey A. Carr (1873-1954), the Chair of Psychology at Chicago, who had just published what has been described as the "official textbook of functionalist psychology" in 1925 (Carr 1925; Heidbreder 1933).[43]

Jacobson's private practice apparently started out very slowly (Morcos* 1998). It was nearly impossible for him to attract any patients to his office, and his disastrous internship at Cook County meant that he had few referrals. Frustrated, he obtained a part-time instructorship under Carlson in 1919, and also began clinical work at Michael Reese, which was a Jewish hospital in the Chicago suburbs. By the end of the decade, however, Jacobson had established the

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[43]Harvey Carr, *Psychology* (Longmans, Green & Co.: New York, 1925). On the contemporary reaction to Carr's book, see Chapter VI, "Functionalism and the University of Chicago," of Edna Heidbreder, *Seven Psychologies* (Appleton-Century-Crofts, Inc.: New York, 1933).

basis of his practice, which finally received the name "progressive relaxation" in an article that appeared in Jelliffe's *Journal of Nervous and Mental Disease* in 1920.

Progressive relaxation was nothing less than the practice of systematic experimental introspection transferred into the clinic. Between 1917 and 1924, Jacobson conducted a series of studies at the University of Chicago on the effects of relaxation training on gastric hyperacidity, esophageal irritability, arterial hypertension and nervous excitability (Gessel 1989). Using x-rays and graphical recording, Jacobson demonstrated that relaxation training could have a substantial impact on each of these disorders. Jacobson's technique involved an initial patient interview, careful observation by the physician, and systematic record-keeping of the progress the patient was making in learning how to relax properly. Jacobson advised his clinical audience that learning his method demanded a substantial amount of tacit knowledge, which could be passed on from doctor to patient:

> Like any new procedure, for instance a surgical operation, the present method is best learned by the physician who sees what is done rather than from a written description. The aim is to train the patient by his own metal [*sic*]—*his own initiative*. He learns to localize tensions when they occur during nervous irritability and excitement and to relax them. It is a matter of *nervous reëducation* (Jacobson 1924:570. Italics original).

Jacobson also insisted that his technique had its origins in the physiological laboratory. His technique was a product of scientific research—the result of a series of investigations that he was currently conducting in Carlson's laboratory. At his private "Laboratory for Clinical Physiology," Jacobson conducted his relaxation training *en masse*, with eight to ten patients, each in their own rooms, practicing the techniques that Jacobson had taught them (Lange* 1998). Jacobson would monitor their progress through electrophysiological traces generated in a separate control room.

Progressive relaxation, claimed Jacobson, was not to be confused with traditional ideas of relaxation, which simply referred an absence of overt activity. Thanks to the technologies of clinical medicine and the physiological laboratory, the continued presence of "tension" could be demonstrated:

> When an individual lies "relaxed" in the ordinary sense, the following clinical signs reveal the presence of residual tension: respiration is slightly irregular in time or force; the pulse-rate, although often normal, is in some instances moderately increased as compared with later tests; voluntary or local reflex activities are revealed in such slight marks as wrinkling of the forehead, frowning, movements of the eyeballs, tenseness of muscles about the eyes...The additional relaxation necessary to overcome residual tension is slight indeed. Yet this slight advance is precisely what is needed. Perhaps this explains why the present method has hitherto been overlooked (Jacobson 1929:29).

Jacobson taught his patients to recognize the feeling of muscle tension in their own bodies by concentrating their attention on their muscular sense. They were instructed to move from a specific muscle group (such as the flexors of the right forearm) throughout all the muscle groups of the body under volitional control. At the same time, they were taught to relax each group, "progressively," until they reached the point that they could no longer sense any tension at all. The goal was to create a habit out of relaxation, one that could be practiced anywhere. Jacobson went on to describe the results of this version of what he called "residual tension," in both behavioural and quantitative terms:

> ...respiration loses the slight irregularities, the pulse-rate may decline to normal, the knee-jerk diminishes or disappears...mental and emotional activity dwindle or disappear for brief periods. The individual then lies quietly with flaccid limbs and no trace of stiffness anywhere visible...(Jacobson 1929:30).

Despite the fact that Jacobson was selling progressive relaxation as a form of introspective training that could both prevent and cure certain forms of disease, the question of sleep was never far from his mind. Indeed, in his first monograph on relaxation, published by the University of Chicago Press in 1929, Jacobson included a personal anecdote, describing "a nightly insomnia which persisted for hours, while mental activity continued regardless of need of rest." He had suffered from this insomnia in 1908, while working on his dissertation under James (Jacobson 1929:111). The clinical use of sleep was featured throughout Jacobson's book, which began with a discussion of the famous "rest cure" of Silas Weir-Mitchell. Jacobson found it strange that Weir-Mitchell had clearly recognized the importance of nutrition in health and disease by advocating an active choice of diet in his 1879 text, *Fat and Blood and How to Make Them*, but was unable to frame relaxation in similar terms. Weir-Mitchell's "rest cure" advocated a kind of passivity of the body that would, he had argued, eventually transfer its effects to the neurasthenic mind.[44] Weir-Mitchell thought of relaxation as something that happened when bothersome and distractive stimuli were eliminated. Jacobson, in contrast, described the hygiene of relaxation in terms of an active training of the will.

Sleep, Jacobson argued, was not a uniform state. It could be improved through cultivating the habit of relaxation, which itself often led to sleep:

> Subjects independently agree in reporting that this resulting condition is pleasant and restful. If persistent, it becomes the most restful form of natural sleep. No university subject and no patient has ever considered it a suggested or hypnoidal or trance state or anything but a perfectly natural condition. It is only the person who has read a description without witnessing the actual procedure who might question this point (Jacobson 1929:31).

Like Kleitman, Jacobson tied sleep to the subjective experience of relaxation. Their respective methodologies were perfectly complementary: Kleitman's subjects did everything they could to prevent relaxation and extend wakefulness, while Jacobson's patients spent endless hours trying to learn how to relax.

Neither Jacobson nor Kleitman were able to successfully incorporate their work into the neuropsychiatry unit at Chicago. In Grinker's first report to the Rockefeller Foundation in October of 1935, he depicted Jacobson's efforts in a very poor light:

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[44]The literary work of Charlotte Perkins Gilman (in particular, her short story, "The Yellow Wallpaper") might be considered a testimony to the failure of Weir-Mitchell's ideas—Gilman's imagination runs riot as her body endures an enforced rest (Showalter 1985).

> We have had no real problem arise except one in which Doctor Edmund Jacobson became practically psychotic over a patient of his who entered our unit, and unfortunately exposed the weak status of his own medical work by doing so. I have no doubt that you have heard reverberations of this through his patients among the officers of the Foundation. I regret to say that he has utilized every possible means to hurt us but at the same time has exposed himself.[45]

Given Grinker's earlier comment that ninety percent of the patients in the small ward were diagnosed schizophrenics, it is certainly possible that Jacobson might have encountered some serious difficulties in teaching his patients his version of experimental introspection. But the fact that some of the officers of the Foundation were also his patients betrays the growing success of his private clinical practice. He treated a number of prominent and wealthy people over the course of his long career, including Mrs. John D. Rockefeller, Jr., the Chicago meat packer, Oscar Meyer, members of the Vanderbilt family, and even the Harvard physiologist, Walter B. Cannon.[46] The "weak status" of Jacobson's medical work notwithstanding, Simon Flexner, who met Jacobson in New York in 1930, was pleased to hear that his nephew's wife was under Jacobson's care later in the year. It seems that she was suffering from "a mild encephalitis," and Flexner wrote Jacobson, inquiring about her condition. Jacobson cheerfully responded, stating that "she promises to continue to co-operate and if she does so, it would seem as if the outcome should be favorable."[47]

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[45]Letter, Grinker to Gregg (October 23, 1935) (RAC 1.1, 216A, 6, 75).

[46]When Jacobson applied for Rockefeller money to establish his "Foundation for Scientific Relaxation" in 1966, he reminded John D. Rockefeller III that his mother had once been under his care (letter, Jacobson to Rockefeller, RCA, 2, GC531). Arnold Gessel reports that Oscar Meyer was both a contributor to Jacobson's Foundation For Scientific Relaxation, as well as a patient (Gessel* 1996-1997). Jacobson saw Walter Cannon for treatment at least once, in November of 1940, but it does not seem that they met again (letter, Jacobson to Cannon, October 18, 1940; letter, Cannon to Jacobson, October 21, 1940; letter, Jacobson to Cannon, October 23, 1940; letter, Cannon to Jacobson, October 28, 1940; letter, Jacobson to Cannon, November 26, 1940; letter, Cannon to Jacobson, November 29, 1940, WBCA, 144, 2032). Helene Morcos suggested that the "father of cybernetics," Norbert Wiener, might also have been a patient of Jacobson's. At the very least, he visited Jacobson's clinic frequently. She also stated that he treated members of the Vanderbilt family (Morcos* 1998).

[47]Letter, Flexner to Jacobson (December 16, 1930); letter, Flexner to Jacobson (May 7, 1931); letter, Jacobson to Flexner (September 3, 1931) (SFP, B:F 365)

Such success eluded him at Chicago. He left the University for good in 1936, in a dispute over the fees he earned from his private practice, which the University administration insisted should be pooled along with those from other clinical researchers (Morcos* 1998). Jacobson, who was apparently charging as much as one hundred dollars an hour for consultation, refused, and proceeded to set up his Laboratory for Clinical Physiology in downtown Chicago, where he continued his investigations until he closed his practice in the late 1970s.

Jacobson's Laboratory was the realization, on a somewhat smaller scale, of what the University of Chicago had apparently promised to build for him in the 1930s: a laboratory expressly designed for precise electrophysiological measurement, complete with several rooms shielded from electrical interference by thin sheets of copper, exceptionally sensitive galvanometers, and a central control room where the experimenter could monitor numerous electrical recordings taken from several subjects simultaneously.[48]

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[48]Both Morcos and Lange claim that the University intended to build Jacobson a new laboratory (Morcos* 1998; Lange* 1998). Lange reports that Jacobson kept these original plans on display in his new private laboratory. Lange also suggested that the new laboratory was built for Kleitman, rather than Jacobson, because the former had more "clout." Jacobson told Morcos that Kleitman "had come into his [Jacobson's] laboratory and <u>seen</u> the work that he was doing with eye movements...And had <u>stolen</u> his work. That's what he said." Aside from the obvious bias of Morcos in favour of Jacobson's side of the story, the accuracy of this statement is open to question, as Morcos later insisted that this event took place in the mid-1930s, when Jacobson was still at the University, and that William Dement was present as well. Dement (b. 1928) became Kleitman's student in the mid-1950s. Morcos is probably confusing both the dates and the individuals involved, and is more likely thinking of Eugene Aserinsky's visit to Jacobson's laboratory in 1953, rather than any events during the 1930s. Regardless, Morcos' and Lange's statements are a testimony to the similarities between Jacobson's and Kleitman's research, as well as the level of professional jealously between the two.

The electrophysiology of mental activities

Although Jacobson indicated that he used graphical recordings in his technique of progressive relaxation, they do not appear to be an integral aspect of his practice in the mid-1920s. Jacobson's method was an amalgam of experimental introspection and therapeutic technique. It was the training of the experimental subject—or the patient, as the two were equivalent in Jacobson's mind—that was the central focus of progressive relaxation. Thus his papers assumed the classic, and by this time, extremely outdated, style of experimental introspection: verbose descriptions of the experimenter's commands and the subject's self-observations, followed by an assessment of the subject's progress in learning the technique and the experimenter's description of the subject's transformed behaviour, buttressed by some physiological measurements (blood pressure, pulse or respiratory rate, and the like). Graphical traces played little role here, and were not reproduced in his articles.

Shortly after Jacobson became a research associate under Carlson (in 1926), graphical evidence took on a more significant role. In 1926, he wrote Alexander Forbes, a neurophysiologist at Harvard, inquiring about the design of some of his instruments. He wanted to build a device sensitive enough to demonstrate the presence of the least amount of current:

> I have been wondering whether your apparatus described in July 1920 would meet my purposes. I wish to try to amplify action currents from <u>extremely slight</u> movements of the (human) eyes and some larger muscles. I presume that the condenser method with a high resistance string would be appropriate. I presume also that a camera such as you describe would be required. I have been considering the purchase of a large Hindle galvanometer. Any suggestions or advice from you will be appreciated, and I should like to have any of your reprints that may be available.[49]

Forbes responded a few days later, sending Jacobson reprints of his paper, as well as some new circuit diagrams that had improved on his original work from 1920.[50]

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[49]Letter, Jacobson to Forbes (September 14th, 1926) (AFA 10.443).

[50]Letter, Forbes to Jacobson (September 23rd, 1926 (AFA 10.443).

The following year, Jacobson published the first of a series of papers that indicated his new devotion to electrophysiology. He wanted to use the new generation of vacuum-tube amplifiers to drive extremely sensitive instruments that could indicate the presence of small action currents associated with mental activity (Jacobson 1927). Using seasoned introspectionists from Harvey Carr's Psychology Department, Jacobson asked his subjects to relax, and then made them perform various tasks involving association or reflection. They found that relaxation and mental activity were incompatible. Their eye and facial muscles, which had been photographed while the subjects relaxed, lost their flaccid appearance when their minds became engaged. "We are reminded," said Jacobson, "of the assertion of Hughlings Jackson that a motor element is involved in every conscious activity" (Jacobson 1927:404).

To demonstrate the parallel activity of mind and body, Jacobson turned to the graphical method. Using a string galvanometer connected to a vacuum-tube amplifier, he recorded the action currents of various muscle groups in subjects, who were trained in progressive relaxation, while they performed various mental tasks.[51] Subjects were asked, for example, to imagine or to remember bending their left arm, without actually moving it. The string in the galvanometer, calibrated to deflect one centimetre per millivolt, would move, and the winding roll of bromide paper in the camera would dutifully record the event [Figure VI].

Such experiments demanded precise and sophisticated instrumentation. Although Jacobson claimed he had considered using the string galvanometer for such work as early as 1912, it was not until the invention and mass manufacture of vacuum-tube amplifiers that deflections as small as a microvolt could be measured. Jacobson clearly revelled in the technical details of his new device, and included a schematic diagram of the amplifier and calibration

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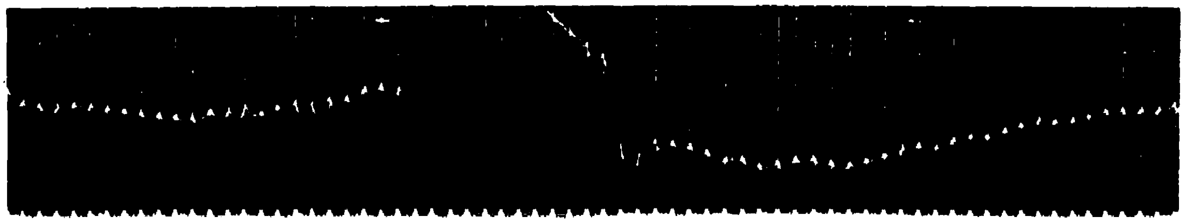
[51]Jacobson published his results in a series of seven articles in the *American Journal of Physiology* between 1930-1931 (Jacobson 1930a-d, 1931a-c). The following year, he published a summary of his results, which was later singled out in the centennial issue of the *American Journal of Psychology* (1987) as one of the most important contributions to the study of "electrophysiological measures" of the last century (Jacobson 1932).

Figure VI

Records of eye movements during acts of imagination

(Jacobson 1930c)



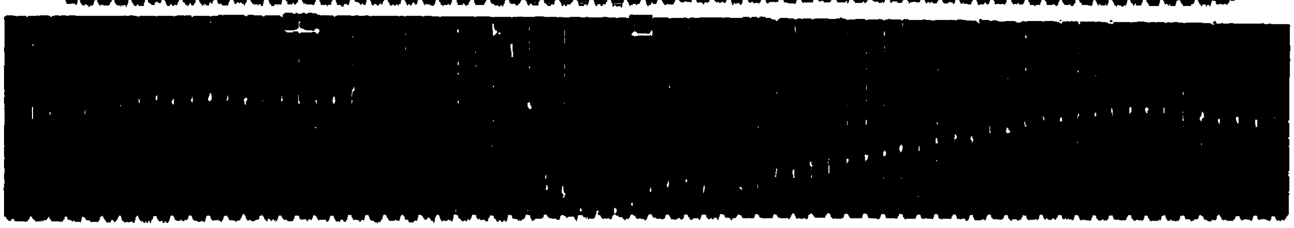


Fig. 5. ***Photographic record of potential differences during eye-movement,—looking right.*** **Electrodes as in figure 5, in article I (1930a). One is placed about 2.5 cm. above the inner canthus of the right eye, that is, about 1 cm. above the orbital ridge. The other electrode is placed just to the right of the right eye, its lower edge on a level with the external canthus, 1 cm. from the orbital ridge. Other conditions same as for figure 1.**

About 0.4 second after the beginning of the signal to look to the right, the string shows a marked deflection exceeding 2.5 cm., and passing off the film. The pattern traced is characteristic for this subject with this set-up for looking to the right. Subject B. R.

Fig. 6. ***Photographic record of potential differences during visual recollection.*** **Conditions as for figure 5. But the instruction has been, "When the signal comes, recall this morning's newspaper." Before this signal the string has been fairly quiet. About 0.8 second after the beginning of the signal, the string shows a marked deflection exceeding 2.5 cm. and passing off the film. The string then proceeds to trace a pattern on the photograph characteristic for this subject with this set-up for looking to the right. Note that figure 6 strikingly resembles figure 5.**

potential of his instrument, which had been custom-built for him by Bell Laboratories (Jacobson 1930a). He also recounted his sources of error and frustration in considerable detail. The Edison storage battery would lose its charge. His carefully-designed electrodes would become polarized, sometimes because of temperature difference between them, causing the string to fluctuate. Electrical motors in the Hull Laboratories (which had naturally been outfitted with an elevator) would start and stop, thus disturbing the string. An AC light circuit had to be removed, and replaced by a direct current system. Even after shielding the room, the subject's couch, and all the apparatus with metallic screens and sheets of galvanized iron, Jacobson still found that "the experimenter needed to remain as quiet as possible while the record was being taken," in order to prevent an accidental inscription, or worse, a broken string.

Subjects were a source of error as well. If they moved when the circuit was closed, and the specially-designed shunt was set to provide the greatest sensitivity, the string would snap. The emotional reactions also created a problem. They could create polarizing effects in the skin (the galvanic skin response, or GSR, a mainstay of lie detectors) and distort the string readings. This issue had come to preoccupy a number of physiologists in the early twentieth century, who were beginning to incorporate such responses into their experiments (Dror 1999). Jacobson was no exception to this trend, and turned to the value of his method of relaxation as a solution. The subject apparently needed to be as well-heeled as the apparatus, although Jacobson stopped short of saying that they had to have been trained by his own method:

> In order to study electrical effects arising from peripheral nerves and muscles during a particular type of mental activity, it is necessary to have a state of mental rest in the preceding and succeeding moments. This can be accomplished in persons trained by the method of progressive relaxation and is found in some persons who have no such training. An individual, with or without such training, who persisted in unabated mental activity throughout the period would obviously be unsuitable as a subject (Jacobson 1930a:579).

Although Jacobson claimed, in 1925, not to be testing a "motor theory of consciousness," it was abundantly clear that all of his experimental efforts were gathered together under this rubric. Every task he examined—imagining, abstract thinking, or remembering—was accompanied by a graphic trace taken from some part of the body, indicating that all these mental activities involved the appropriate sensory organ or organs—in particular, the throat (for "inner speech"), the eyes, and the skeletal musculature. His work implied that the internal life of the mind was nothing more than the inhibition of the movements of the appendages and speech organs. This was an extension of James' theory of emotion into the realm of thought.[52] Where James argued that emotion could not exist without proprioception, Jacobson's work demonstrated that thought could not occur without an accompanying action current to the muscles. If visible movement itself was not essential to mental activity, as behaviourists argued, Jacobson's "graphical microscope," when hooked up to an appropriate subject, proved that at least the electrophysiological essence of movement—the action current—accompanied every mental activity. He dubbed his device the "integrating neurovoltmeter" because it demonstrated the close alliance between the musculature and the brain.

You can sleep well, 1938

Progressive relaxation provided Jacobson with a profitable clinical practice, both in Chicago, and in New York.[53] But, while *Progressive Relaxation* (1929) was expressly written for clinicians dealing with hypertensive patients, by the mid-1930s, Jacobson had begun to

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[52]Jacobson's work appeared at exactly the time that the Jamesian theory of emotions was beginning to unravel. In 1927, Walter B. Cannon argued that emotions were impulses that originated in subcortical centres of the brain—in particular, the thalamus—rather than from sensory impulses from the viscera that were registered in the cortex. Cannon's theory turned the emotions into a question about the relationship between various parts of the brain, rather than the "mind and muscles" concept that Jacobson relied upon.

[53]Jacobson certainly had other sources of revenue. Before the market crashed in 1929, he had made a large amount of money trading stocks. Morcos claimed that he ultimately made over six million dollars this way (Morcos* 1998).

disseminate his technique through popular literature. In 1934, he published *You Must Relax*, which went through numerous editions over the next forty years.

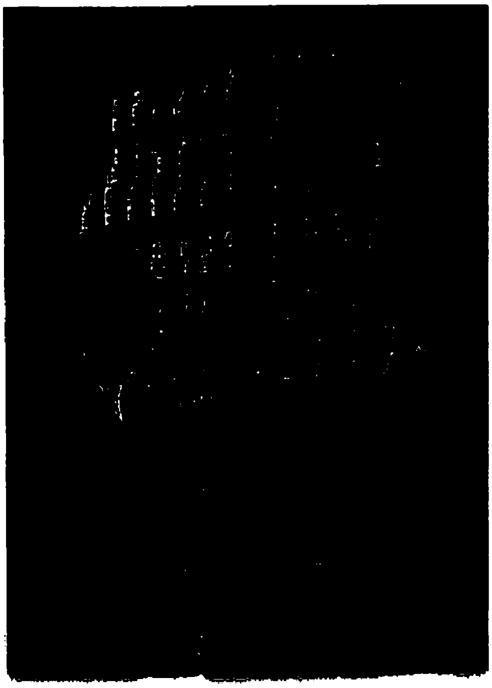
In 1938, just after he left the University of Chicago, Jacobson published a popular book on sleep, entitled *You Can Sleep Well: The ABC's of Restful Sleep for the Average Person* (Jacobson 1938). Like Pavlov's dogs, Jacobson's subjects had often interrupted the course of his experiments by falling asleep (Jacobson 1925a:82ff). But because Jacobson looked at sleep as the natural outcome of relaxation, he easily incorporated sleep into his therapeutic practice. By 1938, he had completed what he felt to be a comprehensive work on the subject, written from the perspective of a friendly (but stern) family doctor advising an insomniac 1930s American Everyman: a salesman with a wife and children, struggling to make ends meet in a tough economy, and surrounded by disease and misfortune on all sides.

This was Jacobson's first publication after leaving the University of Chicago and he did little to hide his disdain for academic science. At the same time, he flattered his readers by encouraging them to believe they had the democratic right to learn about the latest advances in medicine:

> ...who but the public must finally benefit from scientific work well done, or suffer from lack of it? On whose shoulders does the daily expense for this work ultimately rest in a democracy, where research depends upon many donors and upon taxes for that purpose? Shall we keep our facts to ourselves or, mindful of what has occurred in certain foreign lands when science has fallen before political outbursts, do what we can to share our secrets, so that a greater number will take interest in them and, deriving profit, may support and sustain our further scientific ventures? An increasing number of physicians today favor the latter course, because they believe that the *public has a right to be educated* (Jacobson 1938:ix-x. Italics original).

To this end, the images in Jacobson's book were almost equally divided between illustrations of his relaxation technique and pictures of his laboratory and his recording apparatus, complete with photographic tracings [Figure VII]. These tracings, which Jacobson had originally used to verify that his relaxation technique actually did serve to inhibit reflex activity, had become incorporated

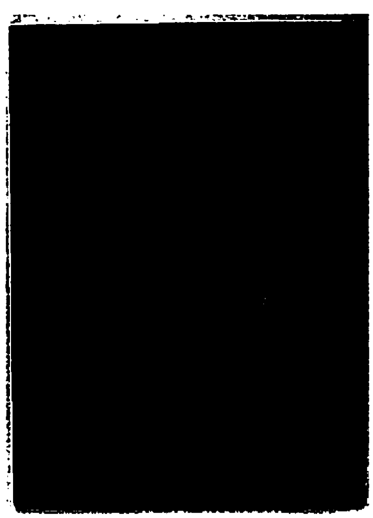
Figure VII
Jacobson's Laboratory for Clinical Physiology
(Jacobson 1938)



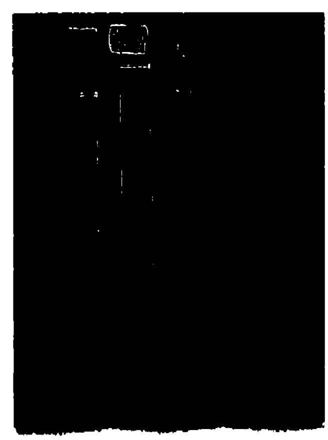
Photographic record storage

A "relaxation room"
in the laboratory
(shielded from electrical interference &
wired for electrographic measurement)

Jacobson's assistant, Miss Brand,
wired for recording



galvanometer string with index



electromyograms
(jaw on left, eye on right)

into his therapeutic technique. He would show them to his patients as a record of their progress.[54] Despite the fact that he no longer worked in a University laboratory, the scientific validity of his efforts was preserved—at least in the public eye. The graphic method still held its charm, even though seven decades had passed since Angelo Mosso was so deeply moved by the inscriptions he saw in Étienne-Jules Marey's laboratory.

Educating the public was a serious task for Jacobson, and when his fictional "family doctor" got the opportunity to take his patient on a tour of his laboratory, the reader ends up with a brief survey of the history of sleep research, which included a reference to Kleitman's famous Mammoth Cave experiments (Jacobson 1938:204-229). Naturally, a popular work on sleep had to say something about dreaming. Jacobson stuck to his motor theory of consciousness, and claimed that it was possible to estimate the beginning of a dream by observing the movements of the string galvanometer, which recorded the action currents in the jaw and the arms [Figure VII]. When the record showed movement after being relatively quiet for a period of time, a dream was certainly taking place (Jacobson 1938:199).

There was an even easier way of discerning whether or not someone was dreaming, however. And it was readily observable, even without the sophisticated apparatus used to measure action currents and brain waves:

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[54]For Jacobson's use of graphical traces in the demonstration of the efficacy of his method, see Jacobson 1925b. Without access to Jacobson's personal papers or laboratory notebooks, it is impossible to determine just when he started showing graphical records to the patients themselves. Lange reports that when he started to work for Jacobson in 1948, measurements were already being taken from the graphical records, and then shown to the patients, who were instructed that the records were a measurement of their ability to relax. Sometime in the early 1960s, Lange had the idea to use an oscilloscope to watch the action potentials himself. Soon after, he and Jacobson decided to show them to the patient, thus inaugurating an important technique in what would become "biofeedback" therapy [Figure VIII] (Lange* 1998).

Figure VIII
Jacobson's "biofeedback" therapy
(Jacobson 1976)

25. In the present studies, tension has become a measurable reality (replacing the vague use of this term commonly employed). Here is a young doctor learning to be relaxed as he sits at a desk. He is helped when he can see on the visioscreen just how tense he is at any moment in his forearm muscles. Platinum-iridium electrodes lie on his skin above these muscles. From these electrodes, wires pass to recording instruments developed in our laboratory.

> "When a person dreams [said the family doctor to his patient], he is tense in some locality. Most often his eyes are active. Watch the sleeper whose eyes move under his closed lids, but be very quiet as you do so. Awaken him in some unobstrusive [*sic*] way, such as clearing your throat, rustling a paper or whatever slight disturbance you need to create; but be sure not to startle him. After you have done this a few times, if he is willing to answer your questions, he will tell you whether or not he has been dreaming. Dreams are quickly forgotten as a rule, but if you and the sleeper go at the matter seriously, you are likely to find after awakening him that he has seen something in a dream. You are less likely to get a report of some visual picture if you watch him closely and arouse him at a time when his eyes have been completely relaxed or approximately so" (Jacobson 1938:144-145).

Jacobson's spokesman went on to claim that "'the complete rest of the eyes has been recorded in this laboratory, in subjects both awake and asleep'" (Jacobson 1938:146).

Dreams, argued the good doctor sitting on the porch with his patient one warm summer's eve, were the product of a troubled mind. Progressive relaxation would probably not eliminate dreaming altogether, but it would certainly diminish their frequency and their vividness—something quite positive for the patient in question, who suffered from disturbing dreams almost every night. Worse, the patient had a passing familiarity with the Freudian theory, which rendered his dreams even more troubling. Jacobson rejected psychoanalysis as mere suggestion; progressive relaxation, in contrast, was based on laboratory science. More importantly, it provided its practitioner with the right tools to succeed in life:

> If you and I assumed [said the doctor to his patient] that you would feel better after having your dreams analyzed, I still would be against it; any more than I would be in favor of reducing the burdens on your nervous system by having you leave your work, and rest at home. You should learn to shoulder normal burdens and should not desire to be relieved of them. Relaxation aims to make you fitted for life in this sense (Jacobson 1938:156).

* * * * *

Jacobson was well-equipped to discover rapid eye movement. He had both experience with and access to sophisticated electrophysiological equipment, particularly involving ocular movements. He had made a clear connection between eye movements in sleep and dreaming, something he could certainly have exploited further in his research. He had an active interest in constructing an image of sleep in graphical terms. He was even familiar with the technique of electroencephalography, which he, like many others, took to be a valuable instrument in the study of sleep (Jacobson 1938). Why didn't he pursue the investigation of eye movements during sleep? Jacobson's approach to dreams, which was nothing more than an extension of the motor theory of consciousness, offers a clue. His interest quite clearly lay in eliminating, to the greatest extent possible, the phenomena of dreaming. Complete and utter relaxation, achieved through the exercise of the will and a strict discipline, was his constant goal. It was signified by the absence of movement in his physiological recordings. Any voltage fluctuation, no matter how slight, represented a failure on the part of his subjects. Automatic eye movements that were closely associated with florid imagery could have no place in such a practice.

Secondly, and of equal importance, was the fact that Jacobson was working outside of the mainstream of physiological science. This is not to say he was a complete outsider: although he had distanced himself from the University of Chicago after 1936, he was nevertheless active in a number of societies, including the American Medical Association, and the American Academy for the Advancement of Science. But these could not supply him with the students nor the institutional impetus to pursue his research, which began to focus more on mental hygiene in the form of progressive relaxation. While he continued to publish articles from his Laboratory for Clinical Physiology in academic journals, he became more and more oriented towards finding new consumers—businessmen, athletes, pregnant women, nurses—for his technique. His work was largely ignored by professional physiologists. And how could it be otherwise? The day of the scientific amateur in the United States was over, and, despite Jacobson's quite remarkable training, his exclusion from mainstream laboratory science meant that only clinicians interested in marginal or unusual therapies would listen to him.

Without the appropriate archival materials, it is difficult to properly assess the nature of the intellectual relationship between Jacobson and Kleitman. But there are some remarkable similarities worthy of comment. Both emphasized a passive vision of sleep. For Jacobson, writing primarily as a psychologist, sleep was the annihilation of consciousness, and was signified by the absence of any movement that could ordinarily be controlled by the will. Likewise, Kleitman thought sleep was a passive process—it was a sort of "default state" of the organism, initially interrupted only by the active "wakefulness of necessity." Conditioning also played an important role for both investigators, thus bringing social and environmental factors into their analyses. Kleitman argued that the 24-hour sleep/wake cycle was a product of training, and represented the victory of social forces over the tyranny of physiology, encouraging individuals to preserve their "wakefulness of choice" rather than be reduced to experiencing only the "wakefulness of necessity." Jacobson emphasized the importance of training in sleep, but this was no less a victory of social forces, as it restored the pathological insomniac or hypertensive back to their proper physiological functioning and economic and emotional prosperity.

Finally, both Kleitman and Jacobson shared a common heritage in psychological functionalism, which maintained a steady presence at the University of Chicago well into the 1930s. Psychological functionalism relied upon teleological reasoning. It explained the nature of mental activities according to the purpose they were thought to serve. Unlike Titchener's "structuralism," which described consciousness through an analysis of its elements, functionalism strove to understand consciousness as an evolutionary advantage in the struggle for existence. And where behaviourists rejected any claims about consciousness as unverifiable and therefore metaphysical, functionalists continued to invoke introspective observations, calibrating them—particularly in Jacobson's case—to the narratives spun by graphical traces. This was particularly evident in Jacobson's work, which quite literally re-inscribed introspection—a failed methodology for the study of aggregate subjects in the wake of the "imageless thought" controversy—as a psychological method of investigation *for the individual patient.*

The functionalism espoused by Kleitman in his evolutionary hypothesis of sleep, and the pragmatism of Jacobson's arguments for the practical utility of physiological knowledge came together in their common emphasis of the role of muscular relaxation for sleep onset. The sensation of the activity of the voluntary muscles, or proprioception, was the very essence of will, a notion that had been well-propagated in the United States through the writings of William James. The will had no place in Piéron's method of enforced wakefulness, which tried to reveal the chemical basis of sleep in terms of fatigue. When Kleitman began to apply this method to himself and other human subjects, the role of the will (as muscular relaxation) inevitably returned to the study of sleep. Jacobson extended this same theme, but in a clinical direction. Such a return fit well with the depiction, during the 1920s and early 1930s, of encephalitis lethargica as an infectious pathology of the will, manifested by the inability to stay awake, to sleep, or to maintain normal sleep/wake schedules.

The vision of holism that counted most for the exploration of sleep in the United States during this period rejected ontological dualism. Mind and body were one and the same, although the methods through which they were examined differed greatly. The key was to bridge the yawning chasm between introspective and behavioural observation. The most effective rhetorical device in this struggle involved the expansion of the graphical method—particularly as it could be applied to the skeletal musculature, the locus of the will. But the discovery of REM depended in part upon a particular application of the graphical method that, curiously enough, underwrote the extinction of the will in the study of sleep. It is this mutation of the graphical method, in the form of the electroencephalograph, to which we must now turn.

Chapter VI

The organization of sleep & the human electroencephalogram 1929-1939

Electroencephalography offered an entirely new way of looking at sleep. Physiological recording had demonstrated that sleep was a complex process. But investigators continued to depict physiological changes in sleep as the result of reflex activity. The ability to record the brain's electrical activity, on the other hand, helped to frame the question of sleep in terms of internal organization. The endogenous electrical rhythms of the brain in sleep quickly became a key factor in the argument that sleep was an active process. The concept of "sleep stages" was the visual analogue of Claparède's "biological theory" and Economo's "sleep centre."

When the EEG first took hold of the scientific imagination in the mid-1930s, its potential applications seemed almost limitless. Its proponents hoped it would reveal the nature of the human mind; and, in a sense, it did. It revealed the importance of having a well-organized system of lower brain functions in the service of higher function. This approach to the notion of progress perfectly reflected the re-organization of the American political economy that was then taking place under Franklin Delano Roosevelt's "New Deal." But the EEG was material as well as conceptual—it was the child of vacuum-tube amplification, a technological development that transformed both the electronics and the communications industries. The EEG was equally the product of philanthropic investment that encouraged the biomedical, rather than industrial, use of new instruments. The Rockefeller Foundation's program of "psychobiology" attempted to bring the latest developments from all scientific disciplines to bear on the human organism as a psychological and social being. The Josiah Macy, Jr. Foundation's support of "psychosomatic medicine," on the other hand, aimed at recovering "the whole patient" as the object of biomedical research and practice.

In the 1930s and early 1940s, these conceptual, material and disciplinary interests converged on the EEG. The epistemic authority of electroencephalography re-defined the mind-body problem as a question about the development and application of biomedical technology. Sleep and dreams were ultimately transformed as a result.

Neither Nathaniel Kleitman nor Edmund Jacobson discovered rapid eye movement. Despite their mutual interest in the nature of sleep and their familiarity with the latest biophysical technologies, Kleitman and Jacobson both turned a blind eye to the prospect that dreams might have a rigid regularity about them. This was not a real question for them, because, as I argued in chapter five, they held fast to a view of sleep that was first and foremost a *subjective* one. Sleep was a product of relaxation. Relaxation described a sensation of one's own musculature. Thus, internal sensations, be they a "craving" for sleep after a hundred hours of wakefulness, or an incessant feeling of tension that prolonged insomnia, were an important part of any discussion of sleep. Kleitman and Jacobson continued to write the physiology of sleep from the perspective of how it was experienced by an introspective subject; that is, they described it as the progressive elimination of conscious awareness. They portrayed sleep as the negation of physiological and psychological activity, and in this way, continued to uphold the notion that sleep was a passive process.

The arrival of electroencephalography in the mid-1930s changed all that. It offered a new way of studying sleep by providing information on brain states with little recourse to the experimental subject's introspective reports about her mental states, or even the observation of her overt behaviour. Sleep stages, the description of different brain states within sleep, revealed an internal rhythm of sleep that was unrelated to sensation and response. Sleep stages demonstrated that sleep was an active process—an idea that had been formulated only once Claparède began to regard sleep in the diachronic light of organismic survival, rather than the synchronic light of sensation. The EEG fuelled this approach to sleep by providing an objective—that is to say, instrumental—visual trace of the brain's organization in time.

How did the EEG come to wield such epistemic authority? This question has particular relevance for the discovery of REM, because, as we will see in chapter seven, when Aserinsky needed to be able to prove that his subjects were indeed asleep, even though their eyes were moving, he turned immediately to the EEG. Yet, as I pointed out in my introduction, the EEG's authority as a sign of sleep has not remained unchallenged. In 1998, investigators interested in behaviour had to calibrate the EEG to a system of video surveillance (Mason* 1998). If Peggy Mason's video evidence indicated that her experimental animal was moving and the EEG record suggested that it was actually sleeping, Mason concluded that the animal had been awake. Aserinsky, on the other hand, relied on the evidence of electroencephalography to describe the sleep state. He used it to calibrate records of eye movement in sleep. Why?

The title of Francis Schiller's paper—"The Semantics of Sleep"—seems to suggest that the question of sleep has always been a problem of *defining* sleep (Schiller 1982). So Schiller begins with Aristotle and Galen's definitions of sleep and moves forward to the technical definitions provided by twentieth-century electroencephalography. But Schiller's narrative of discovery offers no explanation as to why these definitions might have changed, or what relationship the shifting fortunes of sleep might have enjoyed with other ill-defined concepts. A better clue is afforded by the French physician who composed the entry for "sleep" in Nicolas Adelon's sixty-volume *Dictionaire des Sciences médicales* (1812-1822). "It is no less difficult," the author began, "to give a good definition of sleep than it is to provide a proper definition of life" (as cited in Peter 1996:579). An examination of how organic life itself was defined in America in the 1930s by the philanthropic work of the Rockefeller and Josiah Macy, Jr. Foundations will reveal how these organizations were directly responsible for the epistemic authority of the EEG. Even as Gregg pulled the rug out from under Kleitman, agencies like the Rockefeller Foundation were financing work that applied the EEG to problems as diverse as epilepsy and psychological personality. As a state that was often defined as an extended period of unconsciousness, sleep was a natural target for an instrument that seemed capable of replacing subjective testimony.

The organization of American biomedicine in the 1930s

The first English-language review of electroencephalography indicated the new directions of electroencephalographic research (Jasper 1937). It was written by Herbert Jasper, who was studying the EEG as part of a joint effort between the Departments of Psychology and Psychiatry at Brown University (1932-1938), and shortly before he went to the Montreal Neurological Institute to work with the neurosurgeon, Wilder Penfield (Jasper 1975). Jasper had spent three years studying neurophysiology under Lapicque and Alfred Fessard in Paris, and was excited about the possibilities of the EEG to overcome the narrow strictures of behaviourist psychology. "The discovery," he began, "in recent years that rhythmic electrical impulses arise almost continuously from the gray matter within the central nervous system and that these potential waves are signs of excitation processes within central neurons has important consequences for our understanding of the neurological basis of behavior." The advent of electroencephalography would enable the neurophysiologist to transcend the mere observation of reflexive behaviour, and to investigate instead the "centrally maintained activity which 'selects' those afferent stimuli to which it 'will' respond" (Jasper 1937:411).

Rhythm would henceforth delineate the limits of the old reflex paradigm. "This rhythmic continuity of cortical activity," Jasper concluded, "intimately related to a complex integration of centripetal sensory impulses, is not dependent upon the afferent system for its existence, and serves as a dynamic central intermediary between stimulus and response as well as for *centrally* mediated response" (Jasper 1937:470. Italics original).

Jasper's emphasis on the brain as a site of "centrally maintained activity" could have been written by any number of enthusiasts of electroencephalography in the late 1930s. But the idea that there had to be a centralized system that supported the ability to "select" from incoming stimuli and enable a "willed" response transcended the boundaries of brain research. This was a metaphor that described the structure of many aspects of American life in the interwar period. In her historical analysis of the "protein paradigm" that dominated molecular biology research before 1953, Lily Kay has argued that interest in such research can be traced back to the Rockefeller Foundation's mandate of "social control" dating back to the Foundation's inception in 1913 (Kay 1993). This was a period of considerable transition in American life; Progressive-era precepts regarding the value of scientific solutions to social problems were melding with a recognition that giant industrial interests were the wherewithal that could effect the "Americanization of the world." President Theodore Roosevelt's 1902 revival of the discredited Sherman Antitrust Act started the process of taming some of America's largest corporations—the Northern Securities Company, E.I. Dupont, and American Tobacco, among others (Cashman 1988; Eisenach 1994). Roosevelt's efforts culminated in 1911, with the dissolution of Standard Oil, an event that helped give birth to the Rockefeller Foundation in 1913.

The Foundation was an example of this new hybrid of science, industry and philanthropy. According to Kay, its officers "regarded medicine, education, and public health as part of a larger process of enculturation leading to social control and economic stability" (Kay 1993:26). It promoted a new kind of eugenics, one based less on supposed racial difference than it was on the industrial fitness of the American population. By 1920, this concept had been taken up under the rubric of "social control," which promoted the applied knowledge of the social and behavioural sciences in the organization of all aspects of American life. The end of the decade witnessed the emergence of the largest philanthropic merger in history: in 1929, the Laura Spelman Memorial fund and the Rockefeller Foundation were combined to create a two-hundred-and-sixty million dollar behemoth. Herbert Hoover, who would soon mobilize science and industry in vast engineering projects, had just taken office in 1928. Four years later, in the grips of the Depression, Hoover was replaced by Franklin Delano Roosevelt. Roosevelt's "New Deal" represented the American response not only to the Depression, but to the rise of the centrally-planned economy in Soviet Russia through Joseph Stalin's "Five-Year Plans," the first of which began in 1929 (Westwood 1993:299ff). In contrast to Stalin's violent expropriation of goods and property from those who refused to participate in the Great Soviet Experiment, Roosevelt asked for, and received, vast executive powers to implement a series of institutional controls encouraging the cooperation of business, labour and government (Cashman 1998:280-318). The era of centralization had begun.

The Rockefeller Foundation's program to orchestrate American scientific progress in the life sciences must be read in this context. Indeed, Robert Kohler has noted that the era of an "activist system of sponsorship" at the Rockefeller began only with the arrival of Warren Weaver in 1932 (Kohler 1991: 265-302). Weaver was a new breed of philanthropic manager: as an applied mathematician, he had a singularly deep scientific expertise; yet at the same time, he was encouraged to define a program that was focussed in its intent, yet all-embracing in its reach. Weaver, working under the shadow of his old mentor and colleague, Max Mason, who then headed the Rockefeller Foundation's Division of Natural Sciences, turned towards biology. At first, Weaver relied heavily upon Alan Gregg, who had been named Director of the Medical Sciences Division in 1930. Gregg had trained in neurophysiology under Alexander Forbes at Harvard, and was, as we have seen in chapter five, dedicated to merging psychiatry, psychology, and the biological sciences. Weaver borrowed Gregg's idea of "psychobiology," but quickly extended it to encompass the physical and chemical sciences as well. Weaver called this extension the investigation of "vital processes." This was, in essence, a managerial, rather than conceptual, approach to science. "Vital processes" referred less to *what* was being studied than *how* these investigations were conducted. Under this standard, Weaver brought representatives from all natural science disciplines around a single problem—be it the study of the effects of radiation on growth and development, or the spectral analysis of organic compounds. More often than not, such problems were formulated around a particular instrument. As Kohler points out, the profusion of instruments (ultracentrifuges, electron microscopes, spectroscopes, and cyclotrons) sponsored by Rockefeller philanthropy in the late 1920s and 30s was less a function of Weaver's scientific background, and more the product of his management strategies (Kohler 1991:358-391). In comparison with the "grants-in-aid" concept, which allowed a large degree of freedom to the individual researcher, the team-based research that Weaver supported obliged scientists to formulate problems around the data their instruments produced. This, explains Kohler, represented a compromise in the production of scientific knowledge:

> Organizing projects around instruments enabled Weaver to delegate the choice of problems to scientists without completely giving up control to the hazards of disciplinary logrolling...Weaver did not have to risk everything on predicting which problems would pay off or what groups of experts would excel. Nor did physicists and biologists have to struggle to discover problems in which both sides took an equal interest. Selecting instruments, rather than problems or disciplines, gave researchers complete freedom within definite boundaries, and gave Weaver a degree of control that was not too heavy-handed and not too loose. More would have invited criticism from his clients and the trustees; less would have put him at their mercy (Kohler 1991:389).

On this basis, Kohler describes Weaver as a sort of bureaucratic holist, who, in grand Midwestern fashion, ignored "biophilosophical" metaphysical debates, and simply encouraged interdisciplinarity for its managerial possibilities (Kohler 1991:331).

The EEG was rapidly incorporated into American medical science along just these lines. Although the instrument was initially developed by others—in particular, by Alfred Lee Loomis, who will be discussed shortly—Weaver's patronage helped get the EEG into the mainstream of medical practice. But the EEG did not thrive simply because of Rockefeller support. Indeed, some instruments that were backed by massive support from the Rockefeller Foundation, such as the use of the spectroscope to analyse biological materials, simply flopped (Kohler 1991:364-371). One of the reasons the EEG succeeded where the spectroscope failed was because the former was set within the context of an institution that had recently been completely restructured to readily incorporate new technologies.

Between 1900 and 1925, the institution of the American hospital changed dramatically. Where it had once focussed on the housing and care of patients, by 1925, the bulk of hospital resources were now directed towards the efficient diagnosis of disease. In his fine-grained study of the relationship between science, technology and medical practice in this period, Joel Howell argues that this transformation in health care represented a new vision of what constituted "scientific medicine" (Howell 1995). Howell's study focusses on three technological forms that transformed the nature of the modern hospital during the first quarter of the twentieth century: urinalysis; the x-ray; and blood exams. Howell argues that none of the advances in these diagnostic technologies were incorporated into the mainstream of medical practice simply because they revealed the truth about disease. In the case of the x-ray, for example, Howell's study reveals that, in cases of bone fracture, it was not a routine part of medical care to take an x-ray until after the First World War (Howell 1995:103-132). This was not because x-ray technology was expensive, difficult to learn, or because physicians had failed to recognized its medical potential. On the contrary, cathode ray tubes were fairly cheap and simple to use. Wilhelm Röntgen's discovery of x-rays had also received a massive amount of scientific and popular attention soon after he announced it in 1895. But in order for x-rays to become part of medical practice, certain bureaucratic structures—fee arrangements, a specialized profession of radiology, standardized techniques of making and reading an image—needed to be there to receive the technology (Howell 1995:114ff). This happened only when hospitals began to be managed according to the dictates of industry. By the turn of the century, middle-class patients had begun to make up more and more of the hospital population (Rosner 1982; Vogel 1989; Howell 1995:30-68). Hospitals were also increasing in size and finances were becoming more complicated, particularly as hospitals were obliged to attract paying patients through the use of new technologies, such as x-rays and the electrocardiogram. As Howell points out, hospitals tried to apply scientific technology to their administration as well; like for-profit industries, hospitals were expected to be managed efficiently. Consequently, the management of hospitals went scientific as well, adopting Taylorist ideas about efficiency in production. And what was being produced, above all else, were records. These became standardized in the interwar period. Rubber stamps depicting a generic human thorax replaced idiosyncratic hand-drawn images. Specialists filled out forms in a unified manner for the physician to inspect at a glance. Typewritten reports and Hollerith punch cards came to populate patient records.

When electroencephalography first appeared in North America in 1935, all these bureaucratic institutions were firmly in place, set to welcome any new technology that could produce a record and be processed efficiently. In this instance, room had been prepared for the EEG by the electrocardiogram, a detailed description of which had been provided by Willem Einthoven in 1902 (Frank 1988; Fye 1994; Howell 1995:122-125). The ECG seemed to offer great clinical promise for the diagnosis of myocardial infarction (heart attack). Yet hospitals were slow to adopt the technology: the New York Hospital purchased an ECG machine in 1914, while the Pennsylvania Hospital in Philadelphia did not buy one until 1921. In the latter case, the widespread application of the ECG did not take place until the late 1920s. Howell argues that the ECG followed the bureaucratic trail blazed by the use of x-ray machines, which brought not only technological sophistication, but a system of fee-retainment by specialists, a standardized means of reporting tests, and an "ideology that made it desirable to take precise, quantitative measurements with machines" (Howell 1995:124).

The EEG relied on almost exactly the same technology as the ECG. But, in the 1930s, there was an additional reason for hospitals to welcome the introduction of a machine that could provide a record of the electrical activity of the brain and could be correlated to mental activity: asylum reform. The 1930s saw a massive reorganization of the asylum that was patterned upon the reforms that had already been instigated in hospitals around 1900. Reformers were trying to bring American psychiatry into mainstream medicine during the 1930s. This coincided with the rise of a number of new organic therapies—insulin coma, metrazol shock, electroshock, and lobotomy—all of which have received considerable attention by historians (Grob 1984, 1993; Valenstein 1986; Shorter 1997). But the diagnostic end of this drive for technological solutions to madness has generally gone unnoticed. The one exception to this rule has been the use of the EEG in the diagnosis of epilepsy, which has been ritually celebrated by electroencephalographers and neurologists themselves, but received little attention from historians of psychiatry (Brazier 1961; Grass 1984; Empson 1986; Goldensohn 1991; Hughes 1994; Jasper 1997).[1] One sociological study of twentieth-century sleep research simply assigns the EEG to an appendix, and offers little more than a chronicle of advances that led to its discovery by Berger in 1925 (Lemaine *et al.* 1977).

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[1]This pattern appears to be changing. One sociological study has treated the rise of the EEG as a case study in how the ability to simply accumulate information can take precedence in twentieth-century science (Spear 1998). The plasticity of the information that the EEG was able to produce in its early years has also been examined (Millett & Borck 1999).

The EEG had a social, institutional and cultural context. It was immediately inserted into the business of American biomedicine almost as soon as it appeared in the country in 1934. The Rockefeller Foundation alone supported numerous researchers whose efforts revolved around this one instrument: Stanley Cobb and William Lennox at Harvard; Frederic A. Gibbs at the University of Illinois; and W. Grey Walter at the Burden Neurological Institute in England.[2] The Josiah Macy, Jr. Foundation supported Hallowell Davis' research at Harvard. The rapid growth of electroencephalography was an extension of the institutional reform of mental health care in the 1930s. The discovery of sleep stages, which reinforced the idea of sleep as an active process, was a by-product of the technological orientation of American medicine.

The EEG

The electroencephalogram, or EEG, refers to a trace that reflects the voltage change taking place between two electrodes (Empson 1986:9). The trace can appear on a kymographic drum, a sheet of paper, photographic film, a cathode ray oscilloscope, or, more recently, a computer screen. Likewise, the electrodes can assume a variety of forms, from extremely fine platinum-iridium needles piercing the scalp to metal surface electrodes ten or twelve centimetres in diametre. The electrical arrangement of the electrodes (mono- or bi-polar), and their positioning on the scalp are further variables that electroencephalographers have had to deal with in the past. The close relationship between this experimental assemblage and the practice of electroencephalography is betrayed by the fact that the acronym "EEG" refers equally to both.

The most important phenomenon of early EEG work was the "alpha rhythm." This is a regular, sinusoidal waveform of about 10 per second that appear in most (but not all) subjects when they are in a state of relaxed wakefulness. The neuroanatomical origins of this

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[2]On the work at Harvard, see RAC (1.1, 200A, 86 & 87); on Illinois, see RAC (1.2, 200A, 153); on the Burden Neurological Institute, see RAC (1.1, 401A, 15).

"synchronized" rhythm are still unknown (Empson 1986). Its central importance has been as a demonstration the brain was itself a source of electrical energy, regularly discharging accumulated reserves in an orderly manner, and in the absence of external stimuli. When Alfred Lee Loomis observed similar patterns in sleep, he considered them in the same light as alpha. Although he was unable to determine the origins of these rhythms, he took them as a sign that sleep followed an orderly course throughout the night—a course that could only be charted by the EEG.

The pre-history of electroencephalography

Most historical accounts of the development of electroencephalography are taken from Mary Brazier's bibliographic research and her examination of how the brain was studied as an electrical organ up to Berger's discovery of the EEG in 1925 (Brazier 1950; Brazier 1961). Attempts to construct a scientific canon are often tied to the emergence of scientific fields, and Brazier's history of the EEG must be read in a similar light. She wrote just as the interdisciplinary field of the "neurosciences" was beginning to take shape, and as electroencephalography was forming as a clinical specialty, with its focus on the diagnosis of epilepsy. The most important journal in the field, *Electroencephalography and Clinical Neurophysiology*, was established in 1949, just a year before Brazier published her bibliography.

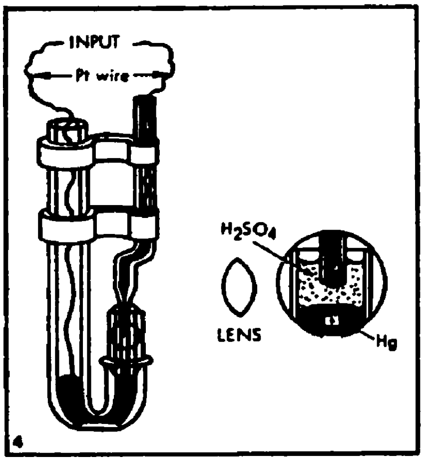
Not surprisingly, Brazier's history is mired in contemporary concerns about the debate between holists and localizationists in neuropsychiatry. With the sole exception of Hans Berger, a psychiatrist at the University of Jena, all Brazier's historical subjects were proponents of the doctrine of brain localization, which drew its inspiration from the announcement, in 1871, by Gustav Fritsch and Eduard Hitzig that the electrical stimulation of parts of the cerebral cortex would produce specific motor reactions (Star 1989). According to Brazier, it was Richard Caton, a Professor of Physiology at University College, Liverpool, who first discovered the electrical activity of the brain in 1875. Using a Thomson reflecting galvanometer [Figure I], which measured the reflected beam of light from a negatively-charged mirror suspended by a string within a coil of wire, Caton demonstrated that "on the areas shown by Dr. Ferrier [a prominent localizationist] to be related to rotation of the head and to mastication, negative variation of the current was observed whenever those two acts respectively were performed" (Caton 1875).

Caton was trying to map functional brain areas by detecting and measuring the electrical activity that resulted from a natural movement, instead of adopting the more traditional routes of either stimulating the cortex directly, or surgically removing a portion of the brain, and then observing the results. Caton's efforts were comparatively non-invasive, and, in this regard, seem to correspond to Marey's work in France that was taking place at about the same time. But it is a stretch to say, with Brazier, that Caton first discovered what Berger would later confirm. Caton was completely uninterested in the "continuous spontaneous electrical activity" on the brain's surface that he had observed with his electrometer. After all, how could this phenomenon possibly be used to relate brain regions to movement? The neuophysiologists that followed Caton—Danilevsky, Beck, Pravdich-Neminsky, Cybulsky—all shared this similar viewpoint. Although they did some tentative work with this "spontaneous current," it always took a back seat to the real problem of brain localization. Pravdich-Neminsky, a physiologist at Kiev, recorded this "spontaneous current" in 1912 with an Einthoven string galvanometer [Figure I] connected to the exposed cortex of a dog. He named it the "electrocerebrogram." Yet Pravdich-Neminsky treated this phenomenon as little more than a baseline, from which he could transform by applying various stimuli. The rhythm itself was of little interest to him.

Hans Berger was different from his predecessors who studied the brain's electrical activity. Not only was he the first to record Pravdich-Neminsky's "electrocerebrogram" in human beings; he was also deeply influenced by the rhetoric of holism. Berger, who was a psychiatrist with no background in electrophysiology, was not interested in extending the theory of brain localization. Instead, he attempted to use the "electroencephalogram" (as he renamed Pravdich-Neminsky's phenomenon) as a tool to investigate how psychological, rather than motor phenomena, could be produced by the brain as a whole. His hopes for the EEG were two-fold: he

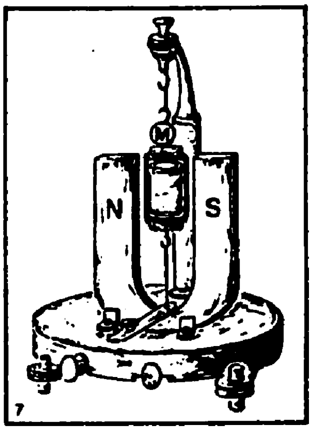
Figure I

Galvanometers c.1873-1901 (Grass 1984)



Capillary Electrometer (circa 1873) Lippman

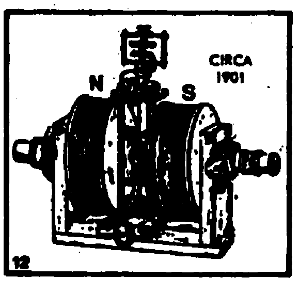
An electrometer operates according to the laws of static electricity. The capillary electrometer relied upon the change in curvature of a tiny meniscus of mercury within a glass capillary when an electric potential was applied between the mercury in the pool and that in the capillary. The meniscus changes were observed with a microscope and/or photographed with a shadowgraph moving film camera. Although it was "sluggish" and the frequency response very poor, Waller is credited with demonstrating the first EKG in 1880. Caton is also credited with using it to demonstrate brain potentials in 1875. The capillary continued to be refined up until 1912 (Lucas). One is at the Harvard University museum.



The d'Arsonval Galvanometer (1882)

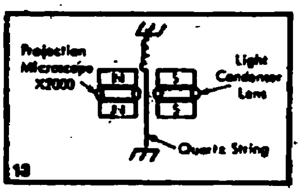
When d'Arsonval introduced the mirror reflecting moving-coil galvanometer, it had immediate application for very slow potentials. A large U-shaped electromagnet creates a magnetic field. A soft iron core is placed in the center of the field creating air gaps near each magnetic pole. A rectangular coil of fine wire (resistance about 5 KΩ), is suspended by wire in the gaps. A concave mirror (M) to reflect a light beam is attached to one-end.

As current flows through the coil, it rotates. The attached mirror deflects light beam proportionately. Current in the wires of the coil near the North Pole are opposite in direction to the current in the wires near the South Pole which causes the rotational force. When the current reverses, the rotation reverses. The relatively large mass of the moving coil gave early designs a time constant of about 4 seconds.



Einthoven String Galvanometer 1901

Einthoven's String Galvanometer provided an instrument sensitive enough to directly record the EKG without distortion. For this achievement, Einthoven received the Nobel Prize in 1924.



Sensitivity was 1 mV/cm and frequency response to 200 Hz. Visualization of the string movement was possible through a hole drilled in the magnet. A microscope magnified the image, and an optical system projected it onto moving film. The string was a gold plated quartz filament. Resistance was 4 KΩ to 8 KΩ.

wanted to use it as a way of measuring the brain's consumption of energy, thus providing a way to quantify thought; and he felt that it could be used to diagnose mental diseases (Gloor 1969). Berger had little luck with the first problem, but achieved some limited success in the second, particularly in the case of epilepsy. But his introduction of the EEG into the field of human research helped to change the orientation of electroencephalographic research away from brain localization and towards the phenomenon of spontaneous rhythm.

Hans Berger & the first human brain waves

Although he was not Mosso's student, Hans Berger (1873-1941) pursued psychophysiological problems just as Mosso had done. He strove to find a way of correlating psychological activity to physiological measurement. Consequently, he emphasized the importance of *states* over *reactions*, which had been the dominant paradigm of both psychophysics and the physiological psychology advocated by Wundt. Berger graduated with an MD from the University of Jena, and in 1897, became assistant to Otto Binswanger at the University's psychiatric clinic, where he remained until his retirement in 1938 (Gloor 1969).

Much of Berger's early research had been dedicated to uncovering the changes in brain circulation brought on by various drugs, using a plethysmographic assemblage identical to the one described in Mosso's book on fear. These changes, he had hoped, would allow him to correlate the dissipation of energy in the brain to mental processes. After his initial efforts ended in failure, he again followed Mosso by turning to a study of the temperature of the brain in 1907. These also resulted in failure, and, according to Berger himself, it was around this time that he decided to study the brain's electrical activity.[3]

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[3]Berger, "On the Electroencephalogram of Man. Third Report," *Archiv für Psychiatrie und Nervenkrankheiten* **94** (1931): 16-60, as translated and reproduced in Gloor 1969.

Berger's first line of research involved the study of nervous transmission by electrically stimulating the cortex, and then measuring the delay until the motor reaction appeared (Gloor 1969:4ff). He was not particularly interested in developing localizationist theory, as he simply used existing maps of the motor cortex to determine which regions were suitable for his experiments. After the First World War, Berger slowly began to abandon this research in favour of the detection, measurement and recording of the spontaneous current that seemed to originate from the cortex. As the chair of psychiatry at Jena, Berger had virtually unlimited access to a large group of patients with skull injuries incurred during the war. By 1924, he forgot about simulation experiments entirely. "What mattered to me now," he wrote in his first report on the human EEG, "was the investigation of the current oscillations present *at all times* that can be recorded from the surface of the cerebral cortex."[4] Upon recording the first human EEG in 1924, taken from a 17-year old patient named Zedel (who, like Mosso's star patient in *Fear*, had suffered from a cranial fissure), Berger made the following entry in his diary:

> Cortical currents (circulation, temperature, electrical processes!) and the hope so beautifully expressed by Mosso, which I experience time and again when I apply precise measuring instruments to the brain. A type of work which agrees well with me and my whole — psychophysiological — attitude (Berger, as cited in Gloor 1969:8).

It is not clear why Berger waited until 1929 to publish his work on the EEG. But in his first report, he does express considerable concern that his traces might actually be artefacts, and he goes to great lengths to demonstrate that they were not due to blood flow, muscle currents, eye movements, or interference from the electrocardiogram. If these spontaneous currents were as well-established as Brazier made them out to be, Berger's extreme caution would be surprising. Instead, it seems more likely that Berger felt himself to be on the verge of a momentous discovery with deep implications for the study of the human mind, and he wanted to be sure that what he was observing was not an artefact.

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[4]Berger, "On the Electroencephalogram of Man. First Report," *Archiv für Psychiatrie und Nervenkrankheiten* **87** (1929): 527-570, as translated and reproduced in P. Gloor 1969, p. 45. Italics original.

Berger's hopes, like his practice, rode on the success of the ECG, which had only been established as a diagnostic tool at the dawn of the First World War (Frank 1988; Fye 1994; Howell 1995). The string galvanometer, which used a silvered quartz string drawn taut within an electromagnetic field, had been designed by Willem Einthoven around the turn of the century. It overcame the limitations of earlier instruments, which, using magnetized mirrors or a narrow column of mercury to display voltage fluctuations, were unable to display the complicated waveform of the ECG [Figure I]. By around 1906, stripped-down and inexpensive versions of Einthoven's device were available on the German market.

Almost all of Berger's observations of the EEG were made on instruments that had originally been developed for the ECG. In 1926, he acquired a Siemens double-coil galvanometer, which he used for most of his publications [Figure II]. More importantly, the ECG provided Berger with an electrical rhythm that was easily displayed, and to which the EEG could be calibrated. By the time Berger first observed the EEG, the ECG was a well-established phenomenon, in terms of the technology that engineered it, and the professional organization that interpreted it. It provided Berger with a stable physiological backdrop against which he could display his new discovery. His efforts to show that the EEG was not influenced by the ECG introduced the idea that the brain, like the heart, kept its own electrical rhythm. Indeed, he visualized it in just these terms. The majority of his published inscriptions followed a similar pattern: the EEG on top, followed by the ECG, and a time code (a trace made by a fork that oscillated at 10 Hz) at the bottom [Figure II]. Even Berger's nomenclature was taken from the ECG. He disliked the combination of Greek and Latin elements in Pravdich-Neminsky's "electrocerebrogram," and instead proposed "in analogy to the name 'electrocardiogram', the name '*electroencephalogram*' for the curve which here for the first time was demonstrated by me *in man*."[5]

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[5]Berger 1929, in Gloor 1969, p. 70. Italics original.

Figure II
Berger & the EEG

Hans Berger in 1925

Fig. 14. 36 year old bald-headed man. Double-coil galvanometer. Condenser inserted. Record from forehead and occiput with lead foil electrodes. Resistance = 140 Ohms. Electrocardiogram with lead foils from both arms. At the top: the curve recorded from the scalp; in the middle: the electrocardiogram; at the bottom: time in 1/10ths sec.

Fig. 7. 38 year old woman with large right-sided bone defect in the region of the motor area. Double-coil galvanometer. Condenser inserted into the circuit. Subcutaneously inserted needle electrodes within bone defect, 6.5 cm apart. Resistance = 1500 Ohms. Electrocardiogram recorded from both arms with lead foil electrodes. At the top: curve recorded from the epidural space; in the middle: electrocardiogram; at the bottom: time in 1/10ths sec.

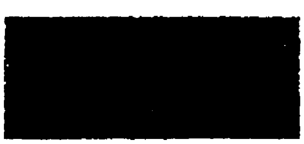

Fig. 8. 38 year old woman. Exactly the same recording conditions as in Figure 7; recorded on the same day.

Fig. 5. 19 year old girl. Bilateral bone defect in the temporal area after palliative trepanation. Double-coil galvanometer. Condenser inserted into the circuit. Needle electrodes subcutaneously, right and left, in the upper parts of the bone defects. Electrocardiogram recorded from both arms by means of lead foil electrodes. At the top: curve recorded epidurally; in the middle: electrocardiogram; at the bottom: time in 1/10ths sec.

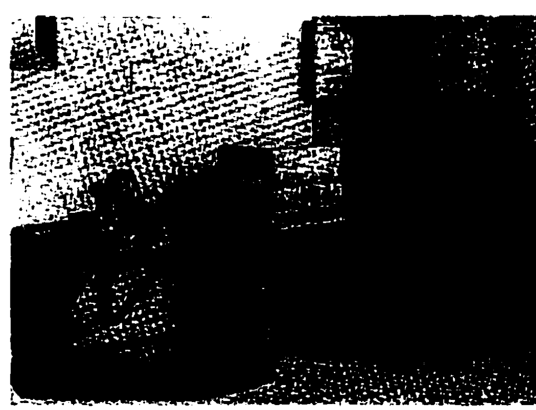

Plate 5
Hans Berger's EEG laboratory in the main building of the University Clinic of Jena with the equipment used between 1926 and 1931. The double coil galvanometer stands on the left side. On its right is the recording apparatus with the camera. On the extreme right is the equipment for the resistance measurements. The subject lay on a couch on the left side of the coil galvanometer. (From R. Werner, *Jenaer EEG Symposium*. V.E.B. Verlag Volk und Gesundheit, Berlin, 1963).

Berger identified two distinct rhythms in the EEG, which he called "alpha" and "beta." The alpha rhythm had a frequency of about 10 per second, and ranged from 7-15 microvolts, while the beta waves were much faster (about 20 per second), and had a lower voltage (around 2 or 3 microvolts).[6] It was the alpha rhythm, however, that captured Berger's imagination. He observed that the alpha waves would disappear whenever the subject responded to any stimulus, be it the sudden noise of a cap gun being fired, or being asked to solve a mathematical problem.

In such situations, only the minuscule beta waves remained. This confounded Berger at first, who, it seems, had anticipated that the current would *increase* with stimulation.[7] Clearly, Berger was again following Mosso, who argued that even the slightest stimulus in sleep brought blood rushing to the brain. In his explanation of this disappearance of alpha, Berger drew upon that great catch-all of psychophysiology: inhibition. He argued that the waxing and waning of the alpha rhythm in relaxed wakefulness, as well as its extinction with mental effort, was due to a "narrowing of consciousness." Berger simply appropriated Pavlov's expression to describe this phenomenon in physiological terms: the excitation caused by a stimulus provoked the radiation of inhibition over the surface of the cerebral cortex, resulting in the disappearance of alpha.[8]

Berger extended this interpretation into his analysis of two states of unconsciousness: sleep and epilepsy. Both featured the "flattening" of the alpha rhythm, which Berger described, not as a form of inhibition, but as "disinhibition." Inhibition, as Smith has argued, was a term that described control, order and self-restraint (Smith 1992). "Disinhibition," then, described the removal of any active force by which attention could be focussed and an orderly consciousness maintained. For Berger, the confused, florid images of dreams showed that attention, the product of an inhibitory force, was annihilated in sleep. He described sleep and epilepsy as "a

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[6]Hans Berger, "On the Electroencephalogram of Man. Second Report," *Journal für Psychologie und Neurologie* **40** (1930): 160-179, as translated and reproduced Gloor 1969.

[7]Berger 1930, in Gloor 1969, p. 81.

[8]Berger 1930, in Gloor 1969, p.88-93.

disconnection of the cerebral cortex" resulting from "an intense excitation" that was conducted to the sub-cortical centre of the thalamus. Berger concurred with Economo's arguments for a regulatory centre that actively blocked incoming stimuli in sleep.[9] The only major difference between sleep and epilepsy was that the latter included an inhibition of respiration and a disturbance of cerebral circulation.[10]

Berger's was a holistic interpretation of the EEG that depended upon a particular experimental assemblage involving human, rather than animal, subjects. His work relied on the ability of his subjects to report on their own mental states, which he then correlated to brain *states* as a whole, rather than localizing muscular responses in one or another area of the cortex. But this did not prevent him from making arguments about particular brain centres, especially in the cases of sleep and epilepsy. But these were regulatory centres, not the sensory or motor points on the cortex that had been the stuff of localization theory for the last sixty years. The rhythms of the EEG, which seemed to inscribe the brain's activity as a whole, created a new language through which such systems could be described.

If Berger's work did ride on the coattails of the ECG's success, why didn't anyone pick up on Berger's work in Germany (or anywhere else) until the mid-1930s? Electrocardiology was well-entrenched in German medicine by the dawn of the First World War—Bad Nauheim, for example, boasted a centre for the treatment of heart patients that drew students from around the world (Fye 1996:26-27). Nor was there a lack of technical expertise in the construction of the instruments—all of Berger's instruments came from the German manufacturer, Siemens. Indeed, electrocardiography was almost *too* well installed for some of Berger's fellow psychiatrists. One psychiatrist later recalled that "medicine as it was practised in university clinics then [around

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[9]Berger, "On the Electroencephalogram of Man. Eighth Report," as translated and reproduced in Gloor 1969, p.209-223.

[10]Hans Beger, "On the Electroencephalogram of Man. Ninth Report," as translated and reproduced in Gloor 1969, pp. 225-242.

1914] awakened in me and in many other young physicians the greatest doubt. Rounds were becoming increasingly impersonal; the wards, the dominance of laboratory tests and [electrocardiogram] curves revolted me. There was never a one-on-one conversation between doctor and patient..." (Viktor von Weizsäcker, as quoted in Shorter 1997:156). Weizsäcker was only one of many psychiatrists who began to turn towards psychotherapy in the 1910s and 20s as a means of encouraging communication with their patients. Berger published in psychiatric journals almost without exception. Those who agreed with Weizsäcker would have had little interest in Berger's discovery. Nor did Berger's work jibe well with the Nazi psychiatric practices that shortly followed. The racial psychiatry of the Nazi era adopted a genetic standpoint on mental illness that bore no relationship to Berger's study of the EEG. In fact, Berger was clearly opposed to Naziism, and was eventually forced to retire because his work received no support under the new regime. In despair over his isolation and his failed career, he killed himself in 1941 (Gloor 1969).

Technology & the growth of neurophysiology, 1900-1935

It was neurophysiologists, not psychiatrists, who first responded to Berger's discovery in any systematic way. In the United States, the EEG offered neurophysiologists the possibility of extending their technologically-driven research into the field of neuropsychiatry, which was undergoing a substantial restructuring during the 1930s. The Rockefeller Foundation supported the shift in American medicine towards an increasing reliance on diagnostic technologies, of which the EEG quickly became a relevant part. The mandate of the Josiah Macy, Jr. Foundation, on the other hand, was to bring a knowledge of "the whole patient" to bear on the practice of modern medicine (Josiah Macy, Jr. Foundation 1955:63-70). The officers of the Macy Foundation placed particular importance on understanding the effects of the emotions upon body function and the process of healing. They supported early EEG work, because they hoped it would supply yet another physiological index of emotion. As we have seen in the case of Mosso, there was no contradiction between the investigation of emotional states and the technology of the graphical method. The rhetoric of holism frequently positioned itself against the technological "reduction" of the patient's illness to a series of mechanically-derived signs. But technological progress and holistic medical research could operate in tandem. It is surely no coincidence that Helen Flanders Dunbar, one of the most outspoken proponents of psychosomatic medicine, was at the very first scientific conference on the EEG in America in November of 1935, right alongside Warren Weaver and Max Mason of the techno-centric Rockefeller Foundation [see Appendix].

Edgar Adrian, a neurophysiologist at Cambridge, was the first person outside Germany to pay attention to Berger's work. Like his colleagues across Europe and America, Adrian was absorbed by the problem of the nerve impulse. How was it conducted along the length of the nerve fibre? How did it pass across the synapse, that tiny gap between nerve endings? Neurophysiologists have traditionally represented the history of these questions as problems that could be addressed by technological innovation alone (Adrian 1965; Young 1975). In his historical study of neurophysiologists at Cambridge, Harvard, and Washington University at St. Louis, Robert Frank has shown that technological developments were not simply grafted on to existing practices. The advent of vacuum-tube amplification in the early 1920s actually transformed experimental technique, making it more dependent upon improving instruments than on reconfiguring the experimental assemblage as a whole (Frank 1994).

Adrian (1889-1977), who shared the Nobel Prize in Physiology or Medicine with Charles Sherrington in 1932, was the product of just this sort of "engineering approach" to neurophysiology. He had made his reputation through his work under Keith Lucas at Cambridge. Lucas (1879-1916), was part of the first generation of British neurophysiologists to reject Sherrington's macro-level experimental approach in favour of an assemblage radically reduced in scale (Marshall 1987; Frank 1994). Sherrington had studied spinal reflexes by stimulating the skin of a decerebrate animal, and measuring the subsequent muscular response. Lucas, on the other hand, was less interested in integration than he was in the mechanics of nervous transmission, so he avoided the spinal cord altogether. His approach was to isolate the smallest possible nerve-muscle preparation, stimulate the nerve, and then measure the muscular movement (Frank 1994:213-217).

Between 1904 and 1908, Lucas conducted a series of experiments suggesting that the "all-or-none" principle applied both to muscle fibre and to the motor unit (the group of muscle fibres stimulated by a single axon of a motor nerve). "All-or-none" referred to the hypothesis that all nervous response (motor or sensory) depended only on the number of nerve impulses, per unit of time, that were transmitted. In other words, the nervous system operated in a binary mode: nerves either fired, or they did not. When Adrian became Lucas' student in 1911, he immediately set to work confirming his teacher's results, and published them in an award-winning summary of Lucas' research in 1914 (Adrian 1914).

The war interrupted Adrian's career just as he was beginning his MD in London. Adrian was not particularly interested in medicine, nor was he turning to the clinic because he wanted to help the war effort, as Frank suggests (Frank 1994:217). He looked to medicine as a possible field of application for his work, even though he was not particularly enthusiastic about this prospect. In a letter to Alexander Forbes, Adrian expressed his frustration at not being able to "say something useful about the cable theory [of nervous conduction]." Forbes (1882-1965), had been a student of Cannon's at Harvard, and worked with Sherrington at Liverpool during the academic year 1911-12. On a side trip to Cambridge, Forbes had managed to work in Lucas' laboratory with Adrian for three weeks. Adrian and Forbes quickly developed a close relationship, and communicated regularly thereafter. Adrian clearly hoped to repay Forbes' visit in 1914, and described his regret at not having the time to come to Harvard and work with Forbes for a term. But Adrian hoped that medical training would give him an advantage over his peers:

> Unfortunately I haven't got a medical degree yet & I shall have to spend the next two years remedying this defect. Most of the younger physiologists in Cambridge are not medical, but this is

> all the more reason why I should be, if I want a job here. So I shall have to stop research for two years, & after that I don't quite know what I shall be doing.[11]

The letter was dated May 16, 1914, three months before the war broke out. Adrian, who soon became a staunch supporter of Britain's war effort, made no mention of contributing to the national cause as a reason for studying medicine. After the war, he was pleased to leave medicine for good. A paper on the electrical testing of muscles that he managed to get published in 1919, was, he complained to Forbes, only based on a few cases as he "didn't get many peripheral nerve injuries at Aldershot [the British Army base where the Cambridge Military Hospital was located]." He looked forward to returning to the laboratory. "I shall have to look around for some ideas," he told Forbes, "at present my mind is a complete blank as regards physiology proper."[12]

"Proper" physiology, in Adrian's case, referred to laboratory work on nervous conduction at the micro-level, far removed from the clinical problems of neurology. Sherrington had taken "all-or-none" as a conceptual framework that could help guide his study of nervous integration:

> The 'all-or-nothing' principle [he wrote Forbes], has general bearings some of which I think one can apply fruitfully already to special problems in nervous system reactions...have you noted how it applies to the question of pain-production? The view of pain-production as due to the excessive stimulation of afferent nerves which under ordinary circumstances do not evoke pain is still fairly widely held & taught. As regards skin-nerves I have myself for a long time been of those who hold that skin-pain is due to a special set of cutaneous afferent fibres whose specific office it is to be excited by stimuli tending to do damage...And it brings a like argument for actual pain. And as reflex producers they may in case of viscera often be called into activity & produce this or that effectual protective reaction (e.g. squeeze a small bile-stone along the bile-duct) without our knowing anything about it—their reactions not spreading to cerebrum & so not 'becoming painful'.[13]

Adrian, on the other hand, framed the solution to the all-or-none hypothesis in terms of instrumental solutions. In 1919, his most pressing difficulty was his inability to graphically

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[11]Letter, Adrian to Forbes, May 16, 1914, AFA, 1.2.

[12]Letter, Adrian to Forbes, March 21, 1919, AFA, 1.2.

[13]Letter, Sherrington to Forbes, June 18, 1916, AFA, 15.730.

represent the activity of a single nerve fibre. The mirror galvanometer was extremely sensitive, but slow. The capillary electrometer and the string galvanometer, on the other hand, had quick response times, but were not sensitive enough (Frank 1994). For Adrian, progress in neurophysiology relied on developments on the technological, rather than the organismic, side of his experimental assemblage.

Alexander Forbes & the vacuum tube

"The valve idea for magnifying the electric response sounds an excellent idea," wrote Adrian to Alexander Forbes, early in 1919. "If you don't make it work we shall have to breed a new kind of frog with a large electric response."[14] In his Nobel Lecture, delivered some thirteen years later, Adrian continued to depict the past (as well as the future) of neurophysiology almost exclusively in terms of technological developments:

> The signals which they [the nerves] transmit can only be detected as changes of electrical potential, and these changes are very small and of very brief duration. It is little wonder therefore that progress in this branch of physiology has always been governed by the progress of physical technique and that the advent of the triode valve amplifier has opened up new lines in this, as in so many other fields of research (Adrian 1965:293).

The vacuum tube, which could amplify voltages in the order of two thousand times, was first introduced to Adrian's laboratory by Forbes, in 1921 (Bradley & Tansey 1996).[15] Ten years after the two had first met, Adrian persuaded Forbes to return to Cambridge in order to help him develop his own amplification system. In the interim, Forbes had established his reputation as a talented physiologist in his own right. Like Lucas, Forbes had been raised in a family deeply involved in the communications revolution brought on by the introduction of the telephone and

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[14]Letter, Adrian to Forbes, March 21, 1919, AFA, 1.2.

[15]Bradley and Tansey report that Keith Lucas was the first to suggest to Adrian that vacuum tubes could be used to amplify nerve currents, but they offer no source for this claim. Lucas died in a flying accident in 1916. Adrian inherited Lucas' laboratory the following year.

wireless telegraphy.[16] Forbes had received his MD from Harvard in 1910, but pursued physiological research rather than a medical career. He returned from England with an Einthoven string galvanometer in 1912, and used it to try and combine the work of Sherrington and Lucas by recording the motor impulse in a spinal reflex, rather than measuring the movement of the muscle. Although Forbes' early records were not of a particularly high calibre, this work was important, because it performed "the essential task of cutting the muscle out of the study of the reflex arc, thereby eliminating a half a century's obstacle to understanding what was really happening in the nerve" (Frank 1994:220). Reverberations of Forbes' early work could be felt throughout the 1930s, when his former student, Alan Gregg, became head of the Medical Sciences Division at the Rockefeller Foundation. Gregg would perpetuate Forbes' technological vision of scientific progress that had produced their first joint publications in 1915 (Forbes & Gregg 1915a & b).

The United States' entry into the war in 1917 proved equally influential to Forbes' research. Forbes, whose devotion to the Allied cause was clearly influenced by the letters he had received from his British colleagues, was an outspoken supporter of the war effort (Forbes 1916). He began to work on wireless apparatus for airplanes in the Harvard Physics Department in 1916. Here, he was introduced to the three-electrode vacuum tube—the triode or "audion"—which was also being developed for government and military purposes by Harold Arnold, who worked in Robert Millikan's laboratory at the University of Chicago, as well as with Bell Laboratories.

The thermionic valve invented by John Ambrose Fleming in England in 1905. It was able to rectify high-frequency oscillations, transforming them into a unidirectional flow of current

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[16]Lucas' father was a self-taught engineer who developed several improvements in submarine telegraph cables, and eventually became managing director of the Telegraph Construction and Maintenance Company located near London. Forbes' father was not an engineer, but a distinguished businessman—he was the president of Bell Telephone Company. His mother, Edith Emerson, was the daughter of the American poet and philosopher, Ralph Waldo Emerson.

(Tyne 1977; Aitken 1985). Drawing upon his knowledge of the "Edison effect" (the shadow cast on the inside of an incandescent bulb by the unidirectional flow of electrons from a heated, negatively-charged cathode to a positively-charged anode), Fleming (who was growing increasingly deaf) developed the thermionic valve in an effort to create a wireless system that featured a visible, rather than audible, display mechanism. His "valve" was able to convert the oscillating audio signal of a wireless set to a direct current capable of driving a mirror galvanometer.

Meanwhile, in the United States, Lee de Forest was creating a similar device. De Forest's "Audion," or triode, included a third plate—a grid inserted between the anode and the cathode that allowed for the control of current by changing the voltage between the grid and the cathode. A very small change in the grid current would produce a much larger change in the anode current, thus making electronic amplification possible.

The triode was quickly deployed as a telephone repeater, and was used in the first transcontinental wireless communication (from New York to San Francisco) in 1915. This was also the dawn of amateur radio, and those that could afford the equipment began receiving and sending their own wireless broadcasts.

With the United States' increasing participation in the European war, however, military applications took precedence. The triode's ability to tune in to specified frequencies had potential applications for navigation. In mid-1916, Forbes began working for the navy, developing "radio compasses" that used rotating loop antennas to determine the direction of a broadcasted signal. The use of several, well-separated broadcast signals made it possible for sailors to determine their precise location at sea. By the time he finished with his field tests and was demobilized in early 1919, Forbes was convinced that the audion could be a key component in the growth of neurophysiological knowledge. He immediately contacted Harold Arnold, who had left the University of Chicago to work at Bell Laboratories, about the possibilities of using the triode to amplify action currents. Forbes produced a ground-breaking paper on vacuum-tube amplification the following year (Forbes & Thacher 1920).

Forbes' paper coincided with the return of amateur broadcasting in the United States in 1919. By 1922, there were 253 broadcasting stations in the U.S., and sales of triodes, as "receiving tubes" sold through the Radio Corporation of America (which distributed the wares of Westinghouse and General Electric), were going through the roof. In 1922, 1.25 million of these tubes were sold. The following year saw sales of 4.26 million, and, in 1924, RCA sold an astonishing 11.35 million tubes.[17] The audion (and its imitators) was rapidly becoming an ubiquitous feature of American life.

The records that accompanied Forbes' 1920 paper illustrated the new degree of magnification that could be obtained with the triode. The action currents of frog nerves that had once been almost completely illegible at one millimetre in height were now visible as a clear spike of twenty-five millimetres or more (Frank 1994:224). Adrian was clearly impressed, and, seeing an opportunity to chart out new regions in physiological research, invited Forbes to return to work with him in Cambridge.[18] Adrian, who seems to have been unsure as to whether or not such things could be obtained at Cambridge, reminded Forbes to bring his own valves with him:

> Do please bring over some valves; I believe they can be obtained over here but at any rate you know the habits of those you have used...We are just getting a new C.S.I. [Cambridge Scientific Instruments] string galvanometer & the lab should be particularly good for valve amplification as our 100 V DC mains are supplied by accumulators...However, I haven't yet thought of any epoch

[17]Tyne 1977, pp 307ff. The Radio Corporation of America was set up by General Electric in 1919, to compete with the British Marconi Co., which was due to have its station in New Brunswick, NJ returned after being appropriated by the U.S. Navy during the war.

[18]Letter, Adrian to Forbes, July 11, 1920; letter, Adrian to Forbes, August 20, 1920, AFA, 1.3. Bradley and Tansey report that Adrian invited Forbes to Cambridge in early 1921, but it is clear from their exchange that Adrian had already invited Forbes by August of 1920 (Bradley & Tansey 1996).

> making research. I should like to find out more about the reactions of sensory nerves e.g. the optic or a dorsal root of the chord, but they are all so horribly small![19]

The tide had clearly turned. Where Forbes had left Britain with a remarkable new instrument in 1912, he was now returning with evidence of American technological ingenuity in hand. And as Adrian's inability to think of any "epoch making research" clearly indicates, the technological capacities of Adrian's amplifier was beginning to shape the course of neurophysiological research.

Neurophysiology & the EEG

It was in this technologically-charged era of progress in neurophysiology that the EEG was first taken up outside of Berger's laboratory in Jena. Adrian is generally held to be the first investigator to give the EEG the seal of scientific legitimacy (Jasper 1937, 1975; Grey Walter 1938; Marshall & Magoun 1998). After winning the Nobel Prize in 1932, Adrian began searching for a new direction once again. He abandoned the fine-grained detail of nervous transmission for a new challenge that was more in keeping with the holistic tenor of the times: Adrian began research on the electrical activity of the cortex in 1933. He had come across spontaneous electrical activity in his study of non-mammalian brains in 1931, but had dismissed them as artefacts (Adrian 1971:1A-7). When he and a colleague, Brian Matthews, turned to studying the cerebral cortex of rabbits two years later, the only literature they could find was on localization. But they wanted to study how an entire *layer* of nerve cells in the cortex reacted to injury and stimulation, rather than the reactions in a particular *region*. When they came to examine the literature, they soon came across Berger's unusual paper (Adrian 1971:1A-7).

Adrian and Matthews successfully recorded the alpha rhythm shortly thereafter. They arranged for a live demonstration at an upcoming meeting of the British Physiological Society in

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[19]Letter, Adrian to Forbes, March 1, 1921, AFA, 1.3.

Cambridge in 1934, and published two papers on the EEG that same year (Adrian & Matthews 1934a, b). Adrian had already planned a tour of the most important centres of neurophysiology in North America. He used it to propagate Berger's discovery. Adrian's formidable international reputation practically guaranteed that the EEG would become a mainstay of neurophysiological research.

While Adrian and Matthews were enthusiastic about Berger's findings, they were less so about his interpretations. They disagreed with Berger's argument that the alpha rhythm represented the activity of the entire cerebral cortex in relaxed wakefulness. They noted that the rhythm was strongest near the visual centre of the occipital cortex, and demonstrated that the alpha rhythm could be induced by setting the "flicker rate" of a light stimulus at 10 Hz. On this basis, they argued that "the essential condition for the appearance of the Berger rhythm is that pattern vision should be absent" (Adrian & Matthews 1934b:382). Its significance for a global theory of attention was thus circumscribed. Their eponymous renaming of the "electroencephalogram" was a curious attempt to defuse Berger's own view that the entire cortex was the source of the rhythm. They also rejected Berger's arguments about the EEG being a sign of generalized inhibition. Rather, they claimed that the rhythm represented the "negative rather than the positive side of cerebral activity, [as] it shows what happens in an area of cortex which has nothing to do" (Adrian & Matthews 1932b:383).

Given this approach, it is hardly surprising that Adrian abandoned EEG work two years later. How could a neurophysiologist study the absence of activity? More importantly, Adrian's thought had been shaped by problems in nervous transmission. He was not interested in psychophysiology, where a non-invasive technology like the EEG was most at home, and he was inexperienced at handling human subjects. It was his student, W. Grey Walter, who pursued electroencephalography in England for the next twenty years. Unlike Adrian, Grey Walter followed a clinical trajectory that was backed by the Rockefeller Foundation. In 1935, he took up residency at Maudsley Hopsital, where he found something that Adrian had lacked in Cambridge—an almost unlimited supply of human subjects.[20]

The EEG was warmly embraced on the other side of the Atlantic, where philanthropic agencies were lined up to support home-grown developments in diagnostic technology, as well as research into the role of the emotions in medical practice. As I have already argued, both of these ideals were embodied in the EEG. American neurophysiological research was also deeply influenced by the work of Alexander Forbes, whose research program, again as I have noted, was broader than Adrian's. Unlike his mentor, Walter Cannon, Forbes pursued a form of research that was itself driven by the developments in communication technology that were transforming American life. Cannon's concept of "homeostasis," a term Cannon coined in 1926, forged an analogy between the self-regulation of the physiological body and that of the political body (Cannon 1932, 1941; Fleming 1984; Cross & Albury 1987; Young 1998). Forbes' analogy, on the other hand, was not forged in concepts, but in practices: he relied on the same technological advances that were restructuring social communication (the public address system, the radio, the telephone) to investigate the nervous system. This was not a case of natural order reflecting social order; it was an identification of practices in two different fields. Analogy became identity. The insertion of the EEG into such a scheme reinforced the idea that sleep was a self-regulating rhythm.

Upon his return to Boston from Cambridge in the middle of 1921, Forbes began preparing a massive review of the current state of neurophysiological research in the light of several recent advances made by Lucas, Adrian, Lapicque, Lillie, and himself (Forbes 1922). The focus of his article reflected his own attempts at reconciling the vast differences that separated

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[20]The Rockefeller Foundation sponsored Grey Walter's research at the Burden Neurological Clinic from 1939 until 1955 (RAC 1.1, 401A, 15).

the phenomena of spinal reflexes from the work done on nerve-muscle preparations.[21] These differences, he thought, must be chimerical. Following Lucas' lead, Forbes speculated that a greater understanding of nerve conduction would inevitably dovetail with knowledge about the reflex arc. At the very least, this was the ideal to be achieved by twentieth-century neurophysiologists: "a reduction of the elements of neural activity underlying consciousness and behavior to the single basis of the nerve impulse," he declared, "would be a generalization comparable to the reduction of all the various chemical elements to their constitutent [*sic*] protons and electrons" (Forbes 1922:365). But this reduction was merely objective. It did not threaten the subjective meaning of consciousness:

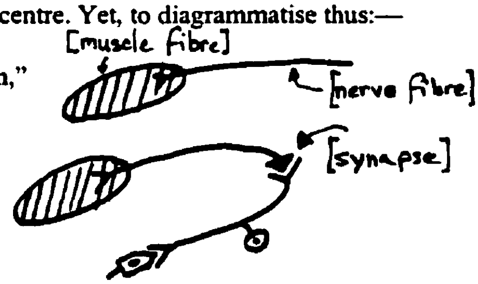
> Such a generalization need not affect any philosophical position we may take as to the relation between cerebral activity, as viewed from the objective standpoint, and consciousness as known to us through our own subjective experience. It should not seek to rob conscious life of any of its subjective properties. It would merely, if substantiated, effect a simplification of the physical aspect of the activity of the nervous system (Forbes 1922:412).

Forbes had sent Sherrington a draft of the paper sometime during the latter half of 1921. Like most of the responses that Forbes gleaned from Sherrington, this one went straight to the heart of the matter. Sherrington visualized Forbes' work as a valuable simplification of Sherrington's own study of spinal reflexes:

> By the way one interesting point about your & Lucas's thesis of making the essential properties of the nerve muscle prep. carry out all the essential functions of the C.N.S. seems to me this: C.N.S. includes psychical functions: ∴nerve-muscle prep....contains psychical functions in germ. This, though at first startling me, is of course not really more than when we say (& I suppose most of us do say) the same of a spinal centre. Yet, to diagrammatise thus:—

"germ of psychosis [mind] in,"

is more trenchant than
"germ of psychosis in"



[21]Forbes offered a long list of such differences. In comparison to results gained from nerve-muscle experiments, for example, the reflex arc exhibited: slower rates of conduction; fatigue upon continued stimulation; after-discharge (continued response after stimulation has ceased); and a greater variability of threshold.

Speaking for myself the simpler case would content me the better.[22]

Clearly, Sherrington recognized that "the simpler case" was only possible through a transformation of the neurophysiological assemblage, a transformation that was only possible through electronic amplification, which did away with the muscle as an indicator of electrical activity altogether. In Sherrington's view, the reduction of psychical function to the activity of the nerves was the same, regardless of whether the "activity of the nerves" referred to the activity of the gray matter of the spinal cord (as a simplification of the brain's activity), or the activity of a single nerve fibre that stood in place of the entire system.

I wonder what William James would have said [Sherrington continued]. Would his eyebrow have lifted? perhaps not; perhaps his answer would have been:—

"germ of psychosis in" [amoeba] 

But, it brings home the scope of the stake before you.[23]

Forbes' response to Sherrington's rather whimsical analysis indicates the extent to which he aimed for a theory of nervous conduction that could somehow integrate all aspects of organic life:

What you say about the "germ of psychosis" is most interesting. I think I am in the amoeba camp. I discussed the question the other day with L.J. Henderson, and he said it depended upon what you mean by "germ"; if potentiality, then ameba [*sic*]; if beginning actuality, then perhaps integration; i.e. the entire organism minus all its component parts, thus stressing the principle of organization as against the units organized. He also said that in one sense he would be inclined to place the "germ of psychosis" in C, H, O and N.[24]

This "principle of organization" would soon come to dominate American neurophysiologists' thinking about the nervous system. Encouraged by Rockefeller support of technologies that could

[22]Letter, Charles Sherrington to Alexander Forbes, December 15, 1921, AFA, 15.733.

[23]Letter, Charles Sherrington to Alexander Forbes, December 15, 1921, AFA, 15.733.

[24]Letter, Alexander Forbes to Charles Sherrington, February 2, 1922, AFA, 15.734.

bridge the gap between the laboratory and the clinic, and spurred on by the holistic rhetoric financed by the Macy Foundation, neurophysiologists found systems of self-regulating order mirrored throughout nature.

By 1940, the idea of self-regulation had been enrolled in the democratic struggle against fascism. Ralph Waldo Gerard, a neurophysiologist at Chicago and a former student of A.J. Carlson's, argued that the very idea of freedom was based upon the principle of organization, which was itself the driving force of evolution (Gerard 1940). Abandoning the traditional distinction between living and non-living entities, Gerard instead focussed on organizational structure, using the term "org" to denote any system, be it animate ("animorgs") or inanimate ("inanimorgs"). All orgs, he argued, evolved towards greater integration and specialization. But in the process, freedom was lost amongst its constituent units. Human beings, for example, had no subjective awareness of the behaviour of any one of their nerve or blood cells. The mechanical behaviour of these component parts was due to the evolution of regulatory structures that always aimed at complete integration. Thus a greater amount of mechanized responses engendered a higher degree of freedom. Gerard drew upon his recent work on the EEG to drive home this point. The fact that "nerve cells can continue to beat electrically in a constant environment within the body," and that "this beat can be modified by all sorts of external circumstances" did *not* imply that all volitional activity was determined (Gerard 1940:416). It simply demonstrated the immense amount of organizational work required to maintain a stable state in which the smallest amount of freedom could appear. All systems, argued Gerard, "evolve towards greater control of their units, towards totalitarianism." Having made this claim, he immediately rejected the "terrorism, falsifications, irrationalism and martial orientation" of the Nazi state as an indication that it had failed to respect the natural process of integration, in which man's needs and desires would eventually converge, until complete stability was achieved. "As history unfolds," Gerard concluded, "I am confident that man will find himself more subject but less slave" (Gerard 1940:427).

Did the EEG signify a form of neural organization that provided the basis upon which human freedom was founded? Given Gerard's expansive rhetoric, it is possible to interpret his work in the EEG in the light of such a claim. In two studies on the EEG of sleep that Gerard co-authored in the 1930s, EEG patterns were calibrated to stimulus thresholds in sleep (Blake & Gerard 1937; Blake, Gerard & Kleitman 1939). The authors argued that wave frequencies rose and fell along with neural excitation levels, as determined by the ease or difficulty with which a sleeping subject returned to consciousness. The spontaneous beat of the cortical neurons—the rhythm that Adrian had found to be so "disappointingly constant"—was now being interpreted as a sign of a rigid neural organization in the absence of consciousness.

Alfred Lee Loomis & the Tuxedo Park laboratory

Curiously enough, the distinctive EEG patterns that Gerard and his co-workers used to determine levels of neural excitation were not discovered in the sleep laboratories at Chicago. It was not Kleitman who discovered these "sleep stages," but Alfred Lee Loomis, at his private laboratory in Tuxedo Park, just outside of New York City. Loomis' lab was like a Rockefeller Foundation in miniature. A wealthy entrepreneur who supported the sciences exclusively through the construction of instruments, Loomis founded the laboratory in the early 1920s, when his interests were purely in the physical sciences. But by the early 1930s, Loomis, like his friend at the Rockefeller, Warren Weaver, had started to sponsor the design and construction of instruments for use in the biological sciences.

Alfred Lee Loomis (1887-1975) is certainly best known for his work in forming the Radiation Laboratory at the Massachusetts Institute of Technology in the summer of 1940, and for directing the radar research conducted through the National Defence Research Council

(NDRC) and Office of Strategic Research and Development (OSRD) during World War Two.[25] The submarine blockade of Britain had prompted the Royal Air Force to develop a microwave radar system that could turn their planes into more effective night fighters and anti-submarine patrols. A microwave set, based on a cavity magnetron, had already been developed in Birmingham by John T. Randall and Henry Boot. But there was a chronic shortage of British physicists to turn the prototype into a viable radar set for airplanes. Prime Minister Churchill decided to approach the U.S. for help, offering the secret of the radar's design in exchange for assistance in developing the device for practical use.

Loomis immediately became part of the "Tizard Committee" (named after the British emissary, Sir Henry Tizard), which was responsible for developing microwave radar. Loomis was an obvious choice. For one thing, a working microwave radar set that used a klystron tube, rather than a magnetron, had already been built at his laboratory. But Loomis also enjoyed a reputation among scientists, businessmen, and politicians as one of the most powerful and trustworthy patrons of science. Loomis knew practically everyone. He had made a small fortune as a Wall Street lawyer before the stock market crash of 1929. His cousin, Henry L. Stimson, had been Secretary of State under Hoover, and became Secretary of War during Second World War. Loomis was also close friends with Robert W. Wood, a well-known experimental physicist (Seabrook 1941). Wood, who worked at Johns Hopkins, complained bitterly to the officers of the Rockefeller Foundation about his chronic lack of funding to procure new instruments, and thereby attract new students (Kohler 1991:201). Around 1924, Wood turned to Loomis, an old friend, for support. Together, they founded a laboratory in Loomis' mansion in Tuxedo Park, where they developed a number of optical spectrographs, and studied the lethal effects that inaudible high-frequency waves (then called "supersonics") had on living organisms (Alvarez

[25]Biographical information on Loomis is taken from Alvarz 1980, 1983. Valuable information about Loomis can also be gleaned from the numerous interviews conducted with electrical engineers for the RadLab oral history project, sponsored by the Institute of Electrical and Electronics Engineers (IEEE) and Rutgers University. See the IEEE History Center website at http://www.ieee.org/organizations/history_center/.

1980). With his extensive and reliable network of contacts, Loomis was the perfect choice to help orchestrate a scientific enterprise at a time of crisis.

All of the scientific work at Tuxedo Park revolved around the development and deployment of large and expensive precision instruments. It was exactly this approach that led Loomis to first discover the EEG (without any knowledge of Berger or Adrian's work), and to pursue the study of sleep stages. His laboratory featured, among other things, a 40' spectrograph, a "microscope-centrifuge," three Shortt pendulum clocks (there were only five others, all in major astronomical observatories), and a private telephone line that carried a calibration signal from the quartz crystal oscillators developed by Bell Laboratories. Loomis' passion for time-keeping coincided with his growing interest in biology in 1934, when he and E. Newton Harvey, a general physiologist at Princeton, built a kymograph with an eight-foot long drum [Figure III]. The instrument, reputed to be the largest in the world, generated inscriptions on a single piece of paper, thus enabling an experimenter to visually compare a large amount of information in a single glance, instead of going through hundreds of feet of paper tape. Loomis and Harvey had built the kymograph in order to observe slow changes in physiological activity over long periods of time. Its horizontal drum featured a red and a green pen that travelled once every minute around its forty-four-inch diameter, inscribing heart rate, blood pressure, and the like for several hours at a time.[26]

In keeping with the technological developments that were then taking place in American medicine, Loomis was particularly interested in improving the electrocardiograph, which was still based upon Einthoven's string galvanometer. He began experimenting with the placement of the electrodes, and discovered that, when he placed one electrode on a subject's head, the baseline was unsteady. Loomis suspected that this rhythmic activity was not artefactual, but was

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[26]Hallowell Davis, "Alfred Lee Loomis: American discoverer of the EEG," typewritten MS (21 pages) of a paper delivered at "Hans Berger Day," a conference held at the Medical College of Virginia, Virginia Commonwealth University, May 21-22, 1979 (HDP FC022, 21, 60, 2).

Figure III
The kymograph at Tuxedo Park
(Loomis, Harvey & Hobart 1936)

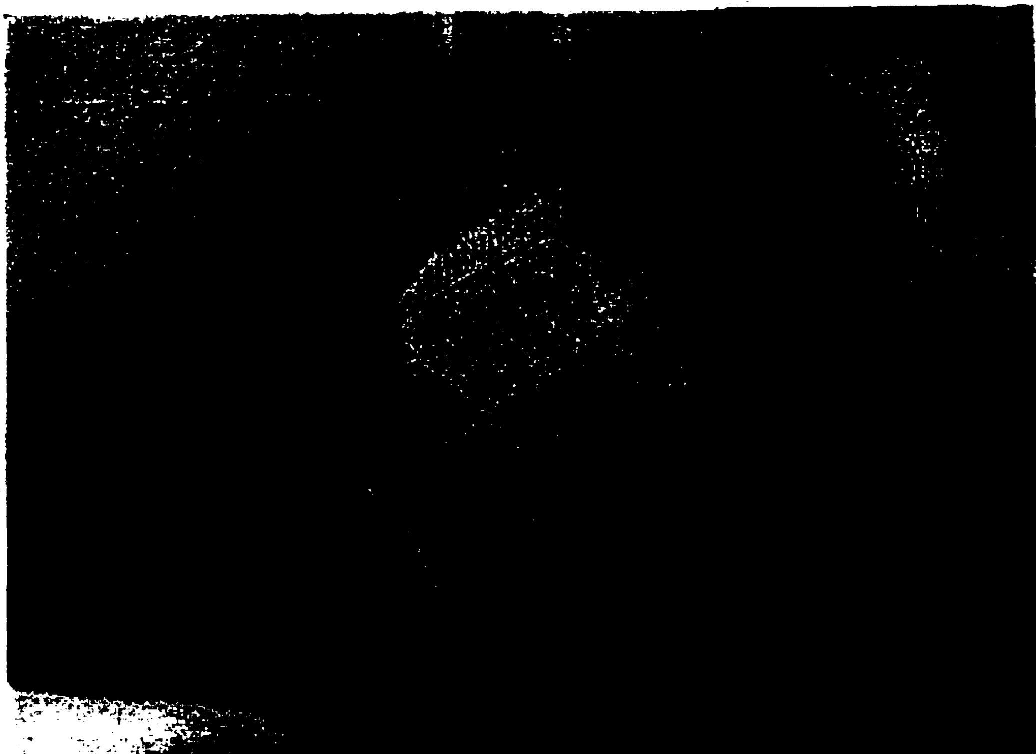

FIG. 3. Photograph of eight foot drum with pen carriage and racket wheels (upper left) for integrating heart beats or brain potentials.

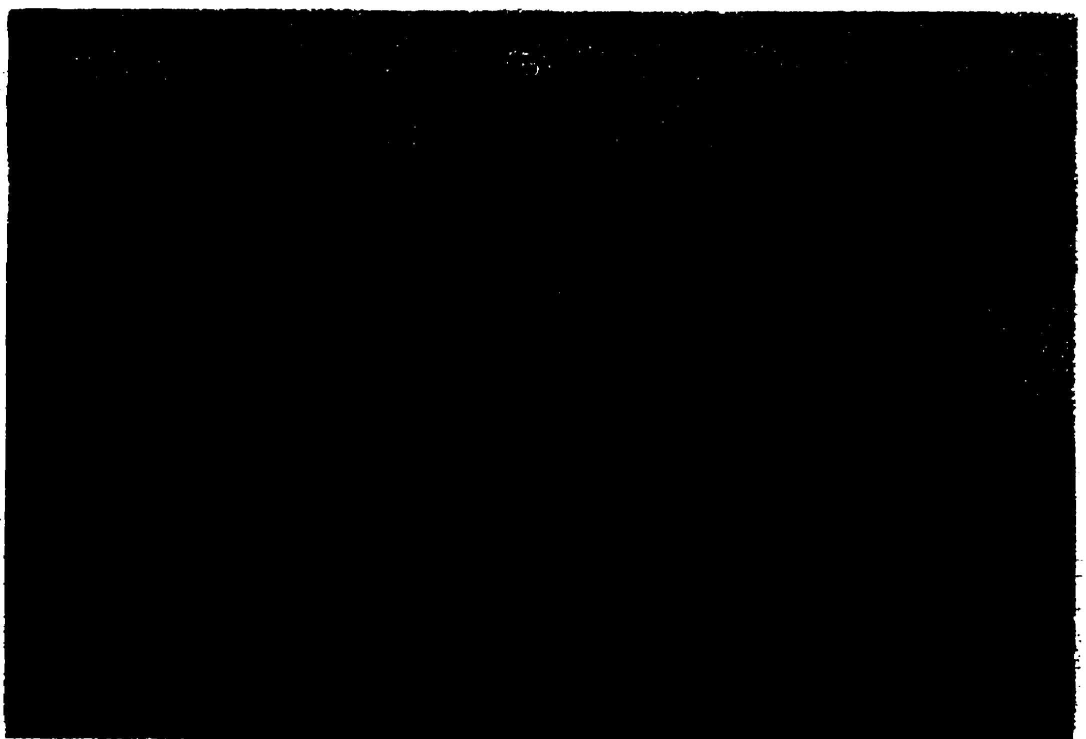

FIG. 2. Photograph of pens (*P*) in position to write on drum. *I*, ink reservoirs. *E*, electromagnets.

coming from the brain. In the autumn of 1934, he turned to Alexander Forbes for advice regarding this strange phenomenon. Forbes immediately introduced him to one of his star students, Hallowell Davis, who had only just begun to study the EEG.

Hallowell Davis & the EEG

Hallowell Davis, along with his Harvard colleagues, Erna and Frederic A. Gibbs and William G. Lennox, was one of the most important EEG researchers of the 1930s. But while the Gibbses and Lennox made their reputations by successfully using the EEG to diagnose temporal lobe epilepsy, Davis was not so fortunate. For years, he attempted, unsuccessfully, to use the EEG as a means of uncovering individual difference in personality. Davis' interest in providing a biological basis for psychoanalytic theory, an interest that was shared by many prominent neurophysiologists at the time, led him to approach the EEG somewhat differently than his more medically-oriented colleagues. Because it revealed information about the brain, Davis did not treat the EEG as just another diagnostic tool. He felt that it could reveal something about the nature of the mind. Davis' aspirations for the EEG dovetailed with the holistic turn in medicine that was then financed by organizations like the Josiah Macy, Jr. Foundation. The EEG, he thought, could be used to graphically depict an individual's emotional and mental state, thus providing psychologists, psychiatrists, and neurologists with the information needed to treat "the whole patient."

Davis' desire to extend the EEG's application beyond the clinic and into the psychophysiology of work was not simply due to his interest in psychoanalysis. In fact, a more likely source of Davis' ambitions would be his mentor, Lawrence J. Henderson. Henderson was a physiologist whose work in the biochemistry of fatigue eventually led him, around 1926, into what he called "clinical sociology" (Cross & Albury 1987). With Rockefeller funds, he established the Harvard Fatigue Laboratory in 1927, and left Cannon's Physiology Department for the Harvard University Graduate School of Business Administration. He was deeply involved in a number of projects in the study of fatigue and workplace management—the most famous being the series of psychophysiological experiments carried out among workers at the Hawthorne Western Electric Plant in Chicago during the 1920s (Gillespie 1987, 1991). Henderson used the science of fatigue to construct a social theory that was drawn from practical experiments in the management of industrial workers. With strong financial backing for such a vision, it was a successful way to create an experimentally-based physiology outside of the medical setting (Cross & Albury 1987:179).

Davis, who had been deeply influenced by Henderson as an undergraduate, clearly had similar aims. After graduating with a BA in chemistry in 1918, Davis decided to enroll at Harvard Medical School upon his return to the U.S., rather than pursue a PhD. While at medical school, he made contact with both Cannon and Forbes, but he also continued to keep in contact with Henderson. When Davis completed his MD in 1922 and won a fellowship to study overseas for a year, Henderson expected him to work with Joseph Barcroft in Cambridge, and then return to the Fatigue Laboratory at Harvard. Davis, however, had been offered a position in Forbes' laboratory. Forbes encouraged Davis to study with Adrian. "Some instinct told me," Davis later recalled, "that this particular problem [in the physiology of blood chemistry] was too far advanced to make it profitable to enter as a beginner" (Davis 1975:312). So he decided to study with Adrian, and received a position as an instructor in Cannon's department upon his return in 1923, two years after Forbes had shown Adrian how to build a vacuum-tube amplifier.[27]

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[27]Cannon originally offered Davis a position for 1924, but when Davis petitioned him for support for 1923 (as he was getting married that year), Cannon agreed. Davis' research at Cambridge was cut short in May of 1923, when his wife contracted typhus fever while doing relief work in Constantinople (Letter, Adrian to Forbes, March 26, 1922, AFA, 1.7; letter, Cannon to Davis, February 12, 1923, WBCA, 2.20; letter, Davis to Cannon, March 8, 1923, WBCA, 2.20; letter, Davis to Cannon, April 25, 1923, WBCA, 2.20; letter, Cannon to Davis, April 30, 1923, 2.20; telegram, Davis to Cannon, May 14, 1923, WBCA, 2.20; letter, Davis to Cannon, May 16, 1923, WBCA, 2.20).

For most of Davis' long tenure at Forbes' laboratory (1923-1946), his work was devoted to the physiology of hearing. This field, unlike the study of blood chemistry, was obviously ripe for change with the development of vacuum-tube amplification. The signals from individual sensory nerves could be recorded with a degree of precision that was inconceivable only a few years earlier. Davis immersed himself in this study, and endeared himself to Forbes, who suffered from partial deafness, in the process. In the 1940s, Davis was continually developing new types of hearing aids for Forbes to use.[28]

Electroencephalography presented another such opportunity to Davis. Late in 1933, and well before Adrian presented his experiments on the "Berger rhythm," Davis' first PhD student, Bill Derbyshire, came across Berger's work.[29] Davis immediately proposed that Derbyshire and a medical student, Howard Simpson, test Berger's results. Initially, Davis was very sceptical about the EEG, because it seemed impossible to him that the activity of millions of neurons could be so perfectly synchronized as to produce such slow, regular potentials (Davis 1975:316). There were problems right from the start. Despite the fact that Forbes' laboratory featured some of the most advanced electrophysiological equipment then available, no alpha rhythm could be detected from either Derbyshire or Simpson. Finally, they were able to detect a distinct rhythm of 10 Hz, taken from Davis' own scalp. Davis immediately solicited his colleagues to volunteer for the experiment, and found that they could be almost evenly divided between those who showed alpha, and those who did not.

Unlike Adrian, who followed the micro-anatomical strategies of Lucas, Davis' study of auditory physiology had made him familiar with psychophysiological investigative techniques. Davis knew many psychologists, and had even debated E.G. Boring on the nature of Helmholtz'

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[28]In 1941, for example, Forbes, who was at a conference in Chicago, asked Davis to send him his new "hearing machine," or at least some fresh batteries for his old one. "I hate to bother you," he wrote, "but I'll get most of what's said, with the gadget;–very little without" (letter, Forbes to Davis, April 8, 1941, AFA 5, 247).

[29]Davis 1975, and Davis "Alfred Lee Loomis" (HDP FC022, 21, 60, 2).

"mechanical acoustic analyser" (Davis 1975:312). Individual difference had been merely a technical problem for Adrian and Matthews, who had experienced some difficulty getting all their subjects to "show alpha." This phenomenon soon became the focus of Davis' EEG research, and it guided many of the technical developments that followed. Photographic records had been the mainstay of most electrophysiological research to that point. While this method of recording was useful for creating a clear, reproducible image of an electrical event in a nerve, it was useless for an experimental assemblage that relied on the active participation of a subject. To properly calibrate introspective and behavioural observations to physiological measurements, Davis needed to develop "a recording instrument that would give a permanent on-line record"—something that could only be accomplished through writing (Davis 1975:317). Within a few months, Davis' laboratory technician, E. Lovett Garceau, had developed an ink-writing oscillograph. Once again, communications technology played an important role in technical innovation: Garceau had modified a Western Union "undulator,"originally designed to record signals from trans-Atlantic submarine cables on a 5/8" wide strip of paper, to inscribe brain waves.[30]

Archival records indicate that Davis' enthusiasm for the EEG was somewhat tempered. In his routine correspondence with Cannon, the Departmental Director, Davis did not mention the EEG at all until July 31. At that time, Davis mentioned that Frederick A. Gibbs, "who is working with us at present and plans to work with us next year on cerebral action currents," wanted to renew his position as a Research Fellow in Neuropathology, funded by the Josiah Macy, Jr. Foundation. Davis then went on to describe his work in auditory physiology, with no mention of

[30]Although they were less interested in psychophysiology, Adrian and Matthews had also developed an ink-writer sometime in 1934, because they found the loss of precision with an ink-writer was less important than its ability to generate results more quickly, and with less expense. Their first paper (1934a) in the *Journal of Physiology* used photography, but the images in their paper in *Brain* (1934b) was based mostly on ink-and-paper recordings.

EEG at all, or of Garceau's work on the ink-writer.[31] The fact that Davis called the EEG "cerebral action currents" indicates that he initially felt this phenomenon to be merely the cerebral equivalent to the action currents then studied by neurophysiologists.

Davis' communication with Forbes, on the other hand, tells a somewhat different story. His first mention of the EEG comes in a letter dated June 9, 1934, which he sent to Forbes, then in Naples, Italy. Davis had obviously just attended—or at least heard about—the American Neurological Association meeting that had taken place in Atlantic City on June 4, when Adrian announced his study of the "Berger rhythm." Davis' comments suggest that he was already considering using the EEG as a tool for investigating individual difference:

> The dope from Adrian and Mathews [*sic*] that the slow cortical waves of about 10 per second are actually composites is surprising. I shall be extremely interested to see their evidence. The big waves ~~often~~ seem to have *such* an individuality, often a personality, that I find [it] difficult to accept *Adrian's dope* offhand; yet from the point of view of explanation it is very very comforting.[32]

Adrian and Matthews' view that the alpha rhythm was simply the synchronization of millions of neurons with nothing to do was a "comforting" physiological explanation. But Davis, like Berger, clearly thought that the EEG presented an entirely new kind of psychological evidence. His allusion to the "individuality" and "personality" of the waves was taken up two years later, in 1936. At the annual meeting of the American Medical Association in Kansas City, Davis and his wife, Pauline, presented a paper in the "Section on Nervous and Mental Diseases"

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[31]Letter, Davis to Cannon, July 31, 1934, WBCA, 8.102. Garceau, whose low salary was a constant concern to Davis, left the department after Cannon complained about the constant presence of Garceau's dog, as well as his "use of his radio for sounding broadcast through the first floor the music that he pleases to listen to." Cannon instructed Davis that the dog had to be "kept away from the laboratory and the radio no longer used except for strictly experimental purposes." Garceau left within a week, and became a successful instrument manufacturer (letter, Cannon to Davis, September 27, 1935, WBCA, 10.123; letter, Davis to Cannon, October 3, 1935, WBCA, 10.123).

[32]Letter, Davis to Forbes, June 9, 1934, AFA, 5.245. The letter is typed, and edited by hand. The strike-out and the italicized words were additions made by Davis.

as part of a symposium on "The Action Potentials of the Brain." Their paper was entitled "I. In Normal Persons and in Normal States of Cerebral Activity," and was accompanied by two other presentations, one dealing with the EEG in "mental deficiency" and the other on epilepsy.[33] Evidently the Davises argued that brain potentials were distinctive to each individual, because two speech pathologists from the State University of Iowa, who had commented on the paper, tested this claim later in 1936. The Iowa researchers found that those with experience at reading brain waves were able to correctly match one portion of a recording with another to a high degree of accuracy.[34]

Adrian's authority in identifying alpha as the prominent feature of the EEG held great sway, however. In a letter dated July 25, 1934, Davis told Forbes (who was still in Italy) of the ongoing difficulties in his auditory experiments, complaining that "we are about ready to climb a tree." But there was also good news. Davis had managed to abolish alpha rhythms by visual stimulation: "on the other hand we used our heads to good advantage the other night and got *our own* cortical potentials meeting Adrian's description. This can be done without putting the needle into the scalp. The currents are abolished by low visual stimulation."[35]

Shortly after Adrian left, Davis began conducting his own public demonstrations of the EEG. Visualizing the electrical activity of the brain evoked a great deal of popular interest in the 1930s, just as x-rays had done at the turn of the century. Such demonstrations resonated with the drama of stage performance, and helped bring other researchers to the field. Davis' first public

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[33]See "Kansas City Session," *Journal of the American Medical Association* **106** (June 6, 1936): 2001-2002. Hallowell and Pauline Davis worked together on the EEG for many years. Mrs. Davis' official position was as a "technical assistant," although Hallowell tried on at least one occasion to get her a promotion (letter, Davis to Forbes, January 15, 1939, AFA, 5.247).

[34]Travis & Gottlober 1936. A reprint of this article is held with the Forbes-Davis correspondence in AFA 5.

[35]Letter, Davis to Forbes, July 25, 1934, AFA, 5.246. The italicized term was added in hand-writing.

demonstration was at Harvard, which soon led to the arrival of Fred and Erna Gibbs (Davis 1975:317). The Gibbses had been working with epileptic patients under William G. Lennox at Boston City Hospital. They brought an epileptic patient into Davis' laboratory in December of 1934, and, according to Davis, "within twenty minutes we had the story and our first records of the spike-and-dome petit mal pattern" (Davis 1975:318). Their report—one of the first on brain waves in North America—was published a year later. The EEG also had a dramatic effect on how Lennox conceptualized epilepsy, and he soon began to frame the disease in terms of its rhythm. In 1941, he concluded a popular account of epilepsy with a description of the illness as a part of "the rhythm of nature:"

> In digging for the root causes of seizures of various forms (whether muscular, sensory, mental, or emotional) the searcher will finally hit on the tap root. Possibly when he knows the ultimate cause of seizures he will then understand much about Nature. The characteristic of alternate accumulation and discharge, which is the dominant feature of seizures, may be a fundamental characteristic of nature itself; ebb and flow, growth and decay, calm and storm, peace and war, life and death; and alternating rhythm (or purposeful dysrhythmia) of the universe. Faced with the tragedy of seizures, a person's spirit is revived and his inner defences strengthened if he sees that even these are an integral part of the universe, a universe which man is commissioned to improve (Lennox 1941:239).

Lennox expressed a similar sentiment in a textbook on epilepsy, which he published nearly twenty years later. "Like the rest of the universe," he wrote, "the nervous system beats with its own peculiar automatic periodicity...The origin and nature of this ceaseless automatic beating are simply wonders of nature. A poet might say that in the beginning, when God taught the morning stars to sing together, He also composed the symphony of the neurons" (Lennox 1960:5).

Davis' demonstrations began to resonate in the obscure corners of unorthodox medicine. In October of 1934, Cannon complained to Davis of having received "some queer mysticism which is the outcome of newspaper publicity."[36] Cannon was not impressed. Two months later, he was solicited by a group calling itself "The Eastern Electronic Research Association," who wanted to have Davis and Derbyshire demonstrate the EEG at their winter meeting in New York

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[36]Letter, Cannon to Davis, October 22, 1934, WBCA, 8.102.

in January, 1935.[37] The EERA's mandate was to encourage "the development and application in practice of methods employing the detection and identification of human radiations as an aid to diagnosis and the use of short radio waves as a means of treatment." Cannon and Davis shared a common suspicion of the group, and Cannon fired off a curt rejection.

The following year, the EEG became front-page news. A day after Davis, Gibbs, Garceau and Derbyshire demonstrated the EEG at the annual meeting of the Federation of American Societies for Experimental Biology in Detroit, the *New York Times* carried the story on the front page of its Sunday edition, trumpeting "Electricity in the Brain Records A Picture of Action of Thought."[38] The article interspersed references to a "thought recorder" with a "mind-reading needle" between several columns of print describing the experiment. A subject was attached to an electroencephalograph and asked to relax, until the EEG showed the alpha rhythm—"'such stuff as dreams are made on'," quipped the reporter. Then various stimuli (light, noise) were applied to show how the rhythm could be blocked. Finally, the subject was asked to perform a mathematical operation, which again eliminated the alpha rhythm. The height of the "thought-reading" demonstration came when the subject, upon hesitantly giving an answer to the mathematical problem, showed an EEG that went into alpha, and then "changed once more into a thought-wave." Davis apparently whispered to his colleagues that "'He [the subject] is now checking the answer to see if he was right'," which the subject later testified was exactly what he had been doing.

Davis had carefully orchestrated this publicity coup. He had heard in advance that the press would attend the demonstration, so he arranged for the science reporter for the *New York Times* to act as the experimental subject. Davis told Cannon, who was himself engaged in some curious speculation regarding the explanatory value of "homeostasis," that he had been very careful to explain what the demonstration "did and did not mean." Regardless, the potential

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[37]Letter, EERA to Cannon, December 9, 1934, WBCA, 8, 102.

[38]*New York Times* (April 14, 1935), pgs.1 & 32.

benefits such coverage could have for the science of physiology outweighed the possibility of popular misinterpretations:

> We got what I consider a particularly good write-up [wrote Davis]...Since returning home we have been besieged by reporters, photographers, movie men and representatives of radio broadcasting companies. We have drawn the line on the movies, but did allow our picture to be taken, with the apparatus as we had it in Detroit...In regard to the radio, my first impulse was to turn it down immediately as we did with the suggestion of an auditory broadcast through the cat last fall [a reference to the experiments he and Forbes had conducted on auditory nerves]; but, on second thought, it occurred to me that perhaps we were wasting an excellent opportunity for a counter-attack ~~by~~ *on* the antivivisectionists...I have been somewhat emboldened in this policy of giving material to the newspapers, with the question of antivivisection very much in mind, by the remarks made by Dr. Whipple at the Federation banquet. He strongly urged the utmost coöperation with the newspapers...I hope that you do not feel that it cheapens our work or seems like undue seeking of notoriety.[39]

Anti-vivisectionists and animal rights advocates in the United States lobbied against physiologists and psychologists throughout the 1920s and 1930s (Dewsbury 1990; Rupke 1990). Both Cannon and Carlson had been instrumental in organizing the physiologists' response to these charges.

For the next week, the *Times* carried a number of stories and editorials that offered a paradoxical message.[40] On the one hand, they reassured their readers that it was merely electrical activity, not thoughts themselves, that were being recorded. Thought, argued one editorial, was impermeable to materialism, which was an outdated philosophy that physiology would probably soon abandon anyway. On the other hand, the press drummed up the EEG as a breath-taking scientific advance. Charles Judson Herrick, a neuroanatomist and experimental biologist at the University of Chicago, was quoted as saying, "I venture the prediction that the electrobiological

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[39]Letter, Davis to Cannon, April 18, 1935, WBCA, 10.123.

[40]"Electrical Mind-Reading," *New York Times* (April 15, 1935), p. 3; "The Week In Science," *New York Times* (April 21, 1935), p. 16.

era now beginning will yield as revolutionary conceptions of the physiology of the nervous system as the invention of the microscope inaugurated in anatomy."[41]

This was not simply hype for mass consumption. Many neurophysiologists clearly believed the EEG would transform their field, and Davis was no exception. With such brilliant prospects for a relatively simple instrument, it was no wonder that Davis and Loomis quickly joined forces after they first met, late in 1934.

Sleeping in Tuxedo Park

One of the first projects that began to take shape around the EEG in Tuxedo Park was the study of sleep. I have already described the popular and scientific interest in sleep in the 1920s and 30s that grew out of the episodes of encephalitis lethargica, Pavlov's announcement of a new theory of sleep, and the use of sleep in psychiatric therapy. In Loomis' laboratory, the fascination with this null state of the brain converged upon the use of a giant kymograph, one of the many unique instruments he had in his collection. After he met with Davis, Loomis returned to Tuxedo Park, where he, Harvey, and Hobart, a technician, quickly converted their instrument to record brain waves for a full eight hours. Until this instrument demonstrated that sleep could be carved up into distinctive "stages," the physiology of sleep had been pictured from the perspective of subjective consciousness; that is to say, it had been perceived as a unity of unconsciousness.

Notwithstanding the endless debate over whether dreams continued throughout the night, or came in brief flashes, physiologists had assumed that there was only a quantitative difference between, for instance, the physiological measurements taken from the first hour of a sleep period, and those taken from the sixth or seventh hour. Loomis and his co-workers offered graphic evidence that there were *qualitative* differences within the unity of sleep—"quantum jumps," as

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[41]*New York Times* (April 21, 1935), p. 16.

they have been described by one physicist (Alvarez 1983:30). This discovery liberated sleep from a restrictive focus on the reflex physiology of fatigue. By the end of the 1930s, sleep was taking shape as one of the most fundamental manifestations of biological time. It had become a rhythm.

Although both Davis and Loomis began to study the uses of the EEG in sleep during 1935, they did not immediately begin to work together. Davis remained at Harvard, and his first publication on the EEG, which he wrote in conjunction with Gibbs and Lennox, appeared in *Archives of Neurology and Psychiatry* in December of that year (Gibbs, Davis & Lennox 1935). Loomis produced two papers, which appeared in *Science* in June and August (Loomis, Harvey & Hobart 1935a, b).

Sleep was little more than a side issue for the Harvard researchers, who were more interested in applying the EEG to the diagnosis of epilepsy. The research at Tuxedo Park, on the other hand, focussed almost exclusively on sleep. Why? Access to scientific resources can certainly be held accountable for this difference. Epileptic patients were available for experimentation at Harvard Medical Schools, whereas Loomis, in his private laboratory, could only solicit colleagues, friends and family members as volunteers. Loomis also lacked the medical training that the Harvard group enjoyed. Loomis' instrumentation, on the other hand, was far superior than anything at Harvard for recording all the brain potentials of a single night's sleep. In his papers, Loomis routinely pointed towards the virtues of his kymograph, noting that "a sheet of paper 8 feet long and 44 inches wide" was "a great improvement over the use of ticker tape which would require a ribbon of paper over half a mile long" (Loomis, Harvey & Hobart 1936:255).

This difference in material conditions of experimentation made itself felt in the interpretation of the phenomena themselves. The Harvard group soon began to emphasize their successful localization of certain epileptic seizures. The Chicago group held fast to Kleitman's program of correlating depth of sleep to the sleep stages already established at Tuxedo Park.

Loomis' group ignored questions of localization, and concentrated on perfecting the technical dimensions of their sleep laboratory. It featured a shielded, furnished sleeping room separated from the control room by some 66 feet, and an amplifier that could be "tuned" to a desired frequency and then connected to an "integrator," which could automatically detect and record fluctuations in rhythmic physiological activity, and calibrate it to other recorded material [Figure III]. It had microphones and mirrors to monitor the subject's behaviour at a distance, and to help determine which waveforms were the product of brain activity and which were artefacts of facial movements or other muscle contractions. There was even a phonograph to play a recording of Ravel's *Bolero* that provided a standardized background stimulus to help determine normal alpha patterns for each subject. "Perhaps the most important point to emphasize in regard to our technique," Loomis emphasized, "is the automatic control of all recording at a distance from the subject, who remains undisturbed throughout the experiment."[42]

The first EEG conference

Sleep was a key part of Loomis' work on the EEG right from the start. On November 10th, 1935, Loomis held a conference on "The Electrical Potentials of the Brain" at Tuxedo Park. Six papers were featured: Davis gave a literature review; Herbert Jasper described the localization of the EEG; Loomis described his unique kymograph and gave a tour of the laboratory; and Harvey described the cortical potentials he and Loomis had recorded during sleep.[43]

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[42]Loomis, Harvey & Hobart, 1936, p. 255. The reference to Ravel is taken from Loomi Harvey & Hobart 1937, p. 129.

[43]Program, Conference on "The Electrical Potentials of the Brain," AFA, 11.527. The final papers of the day are described in the program as "Potential pattens from the brains of animals. (Speaker to be announced)" and "Paper to be announced." Davis notes that "there were about fifty participants in all," which corresponds exactly to the number of guests that had confirmed their attendance as of November 2 (List of Guests, AFA, 11.527).

Although no attendance record was kept, it seems there were about fifty participants, drawn mostly from the northeastern United States. Davis described the conference in unequivocal terms as "the most exciting scientific conference" he had ever attended.[44] At that point, only five papers had been published on the EEG in English—for many participants, it was probably the first time they had seen the phenomenon demonstrated. The presence of Max Mason and Warren Weaver from the Rockefeller Foundation would only have added to the sense that something important was about to take place.

The guests came from an eclectic range of disciplines that represented the growing interdisciplinary trend of American science in the 1930s [see Appendix]. Physiologists composed the largest single group, accounting for about a third (17/50) of the total number of participants. But it could hardly be said that physiologists dominated the conference: Loomis was more of an engineer than anything else, and the total number of participants from the physical and engineering sciences made up almost a fifth (9/50) of the audience. Only one guest (Cole) described his research area as "biophysics," but it is clear that this interaction between physiologists, physicists and electrical engineers pointed towards a future collaboration.

The guest list also indicates another disciplinary direction: the fusion of psychiatry and neurology. We have already discussed this phenomenon in chapter five, in conjunction with the Rockefeller's attempt to bring these two fields together under the rubric of "neuropsychiatry" at the University of Chicago. Although this project floundered at Chicago during the late 1930s, because of the entrenched professional interests of the physicians and the poor leadership of Roy Grinker, the participants at the Loomis conference seemed poised to ride the brain waves from Tuxedo Park all the way to an interdisciplinary neuropsychiatry unit at their home institutions. Psychiatrists and neurologists were about equally represented, and together made up roughly a quarter (12/50) of the participants. Among these clinicians were a number of individuals who

[44]Davis, "Alfred Lee Loomis," p. 7 (HDP FC022, 21, 60, 2).

would soon help to forge such interdisciplinary connections, such as Stanley Cobb (Harvard), David Slight (Chicago), and Joseph Erlanger (Washington University at St. Louis).[45]

This coupling of psychiatry and neurology through the EEG coincided with professional efforts towards a similar end. In 1934, the American Board of Psychiatry and Neurology was formed after the Cleveland Session of the American Medical Association. Its job was to certify specialists in both fields, finally gathering together the asylum and private practice under a common rubric of professional legitimacy grounded in modern medical science. The Board's representative at the A.M.A. meeting at Atlantic City in June of 1935, Dr. Walter Freeman (of lobotomy fame), announced the results of the first examinations that had occurred just days earlier: of the thirty-one candidates who had been examined between 1919 and 1929, twenty-one passed, four were "conditioned," and six failed.[46] The following year, Freeman offered a similar report, as well as a call to participate in a "survey of public mental hospital services in the United States," at the opening of the three-day "Section on Nervous and Mental Diseases"—the same section that featured a symposium on "The Action Potentials of the Brain," which was attended by Freeman himself.[47]

Equally important to the Tuxedo Park conference was the fact that Loomis had invited a number of power brokers from the Rockefeller, including Max Mason, Warren Weaver, and Herbert Gasser. Mason was the President of the Rockefeller Foundation, Weaver was the Director of Physical Sciences there, and Gasser had just left Cornell to become Director of the Rockefeller Institute for Medical Research (1935). Loomis played the same role in the development of the EEG as he would in the formation of the Rad Lab at MIT in 1939. He

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[45]On Cobb, see Taylor 1984. On Erlanger, see Marshall 1983.

[46]See "Atlantic City Session," *Journal of the American Medical Association* **105** (1935): 44.

[47]See "Kansas City Session," *Journal of the American Medical Association* **106** (1936): 2002.

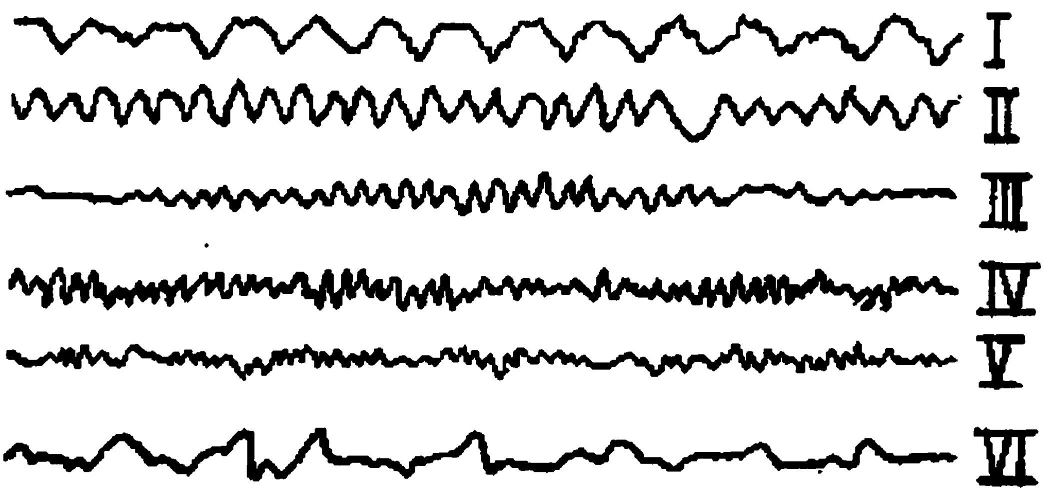
brought together individuals from all sides of the scientific enterprise around a single piece of exotic technology that was capable of creating new representations of phenomena with practical implications. It is somewhat inaccurate to suggest, as Luis Alvarez does, that Loomis' work was eclipsed by the arrival of "big science." On the contrary—Loomis should be credited as one of the creators of just this style of interdisciplinary, large-scale, capital-intensive scientific research.

The unfolding of sleep stages

The idea that sleep came in distinctive stages slowly developed at Tuxedo Park over the next two years. The first two *Science* articles, as well as the first article that appeared in the *Journal of Experimental Psychology* in 1936, simply introduced a number of new waveforms. The massive patience of Loomis' kymograph had revealed that the world of EEG was populated by more than just alpha and beta. Loomis had found several different types of waves: there were "trains" of regular 10 Hz waves; "spindles," which were short bursts of activity in a low-voltage background; "saw-toothed" waves that were slow (4-7 Hz) and had a high voltage; and random activity that seemed to have no discernable pattern at all.

Loomis initially adopted a rather traditional psychophysiological approach to interpreting these waveforms. In his 1936 paper in the *Journal of Experimental Psychology*, he abandoned the naming of the waveforms altogether, largely because "a logical system of nomenclature" could not be established until the local origins of the waves were revealed (Loomis, Harvey & Hobart 1936:250). Loomis gave up the colloquial names, and simply numbered them, I-VI [Figure IV]. Not all of them were related to sleep, however. Loomis' tentative use of the numbers to label his subjects as "Type II" or "Type IV" indicate his interest in using the EEG to study individual difference. The average frequency of alpha (Type II) was also determined for each subject, and subjects were also classified according to the average frequency of "spindles" (Type III) that appeared in their sleep.

Figure IV
The first sleep stages
(Loomis, Harvey & Hobart 1936)



One of the more unusual aspects of the sleep EEG was that most subjects showed alpha during sleep, rather than simply in the relaxed state of wakefulness that preceded sleep. Loomis demonstrated that alpha could actually be provoked by presenting a mild stimulus that would disturb, but not awaken, the sleeper.

Hypnosis quickly entered the picture at Tuxedo Park, and the EEG simply confirmed what had long been suspected regarding the vast differences that separated hypnosis from sleep. Grinker's replacement at Chicago, David Slight, brought a patient whom he had hypnotized several times to Loomis' laboratory. The EEG soon provided just one more bit of evidence that the term "hypnotic sleep" was a misnomer (Loomis, Harvey & Hobart 1936:275-276). Loomis demonstrated that alpha would still be blocked when the hypnotised patient opened his eyes, just as in normal wakefulness. If it was suggested to the patient that he was blind, however, the alpha rhythm would persist, even when his eyes were open.

It was not until the middle of 1937 that Loomis began to organize the different wave forms into distinctive patterns. After assessing 84 records from 30 different subjects, Loomis and his collaborators concluded that "we have been able to establish very definite *states of sleep* which change suddenly from time to time, and to correlate these with movements, with dreams, and with external stimuli applied to the sleeping subject" (Loomis, Harvey & Hobart 1937:128). Loomis began with an enthusiastic discussion of individual difference: "If we divide persons into two great groups, the alpha and the non-alpha, it is an intriguing problem to correlate the extreme types of record with some characteristic of the personality, the mental or emotional make-up of the individual" (Loomis, Harvey & Hobart 1937:132). But, he admitted, all such attempts had not produced any results to date. Sleep, on the other hand, showed five distinctive EEG patterns, or "sleep states," that seemed to appear in all individuals:

> A—*Alpha*. Alpha rhythm appearing in trains of various lengths. The eyes may be slowly rolling, under closed eyelids, as indicated by an electrode over the eyebrow.
> B—*Low voltage*. A quite straight record, with no alpha rhythm and only low voltage changes of potential. Rolling of the eyes may occur.

> C—*Spindles*. Line slightly irregular with 14 per second spindles of 20-40 microvolts every few seconds.
> D—*Spindles plus random*. The spindles continue together with large random potentials 0.5 to 3 per second. The random voltages may be as high as 300 microvolts.
> *E—Random*. The spindles become inconspicuous, but the large random potentials persist and come from all parts of the cortex...[48]

Loomis had been able to correlate dreams to the *B* stage only twice. In one instance, a subject's record went from a *D* stage to a *B* stage without any observed bodily movement. Eight minutes later, he moved, woke up, and reported a dream that seemed to be associated with the movement. In the second case, the subject's record jumped from a *D* to a *B* stage after an experimenter turned the door knob in the laboratory. The sleeper awoke a minute later, and reported a dream that seemed as though it was provoked by the noise. Loomis' straightforward conclusion that "dreams are not likely to be associated with any unusual pattern of electrical potentials but with a state of sleep" indicated that he, like so many other physiologists before him, continued to picture dreams as the product of stimuli (Loomis, Harvey & Hobart 1937:142).

Not to be outdone, several physiologists at Chicago were also using electroencephalography to study sleep. Their first paper confirmed what the Loomis group at Tuxedo Park had already argued: there were a number of distinct EEG patterns that appeared during sleep (Blake & Gerard 1937). But Blake and Gerard added another dimension to these brain waves by correlating them to depth of sleep. They quietly whispered "Are you awake?" to their sleeping subjects, while running an amplified 1000 Hz signal through a loudspeaker in the sleep room. The number of seconds that elapsed before the subject responded provided a quantitative measure of sleep depth. On this basis, they argued that the first two hours of a sleep period were spent in deep sleep. Sleep then became progressively lighter until the fifth hour, at which time it either stayed constant, or cycled again before awakening. More importantly, this measurement of sleep depth correlated to brain wave patterns. Deep sleep was associated with a

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[48]Loomis, Harvey & Hobart 1937, pp. 133-134. "Rhythm" is consistently spelled "rythm" in the text. Italics original.

prominence of slow, high-voltage delta waves (½ to 3 per second), while lighter sleep featured the faster, low-voltage alpha and beta waves.

Their next paper clearly showed the influence of Kleitman's passion for identifying endogenous rhythms of sleep. The Tuxedo Park group had argued that a sleeper's EEG continually shifted from one pattern to another throughout the night. Movement or external stimulus tended to shift the record from a "deeper state" (*D* or *E*) to a "lighter state" (*A* or *B*). But there was no discernable pattern of stages that appeared throughout the night. The Chicago group argued that uninterrupted sleep generated a cycle of six EEG stages (Blake, Gerard & Kleitman 1939). Even if the EEG pattern was briefly interrupted by movement or stimulus, these patterns soon returned to their predictable cycle.

In their brief investigation of dreaming, the Chicago group followed a similar logic. Instead of attempting to identify dreams with a particular stimulus, as the Tuxedo Park group did, the Chicago investigators simply woke up their subjects in different stages of sleep and asked them if they had been dreaming. They noted that "dreaming was present most of the time" and that "a subject abruptly awakened, almost at any time during the night, can recall having dreamed." The only factor that seemed to influence the ability to recall a dream was the presence of alpha waves: "the longer the immediately preceding period with no alpha waves, the less is the [dream] recall, and when this period is about a minute (especially if delta waves are present), there is no trace of a dream's having been in progress" (Blake, Gerard & Kleitman 1939:58). This experiment, while it did not depict the presence of dreaming in the stark binary terms that Aserinsky and Kleitman later described as REM and non-REM sleep, certainly carried the similar assumption that dreaming itself had a rhythm.

Hallowell Davis & psychoanalysis

In 1937, Hallowell and Pauline Davis began to receive funding from the Macy Foundation to incorporate the EEG into psychological and neuropsychiatric research. According to a report Davis filed with the Foundation in 1938,

> The purposes of our survey have been to test our previous conclusions as to the individuality and the stability of the EEG pattern; to describe and characterize EEG patterns for purposes of classification and comparison; to seek for possible correlations between features of the EEG pattern and other characteristics of the subjects, whether mental, physical, or physiological; and to determine the range of variation of normal patterns, in order to establish a background against which abnormalities of clinical significance may be recognized.[49]

This research included three summers spent with Loomis at Tuxedo Park, from 1937 to 1939, where the Davises studied the EEG changes provoked by stimuli presented in sleep, as well as the correlation of EEG patterns to the subjective experience of "floating" with the onset of sleep.[50] The official mandate of the Josiah Macy Foundation, which had been formed only seven years earlier, was "'to develop more and more in medicine, in its research, education and ministry of healing, the spirit which sees the center of all its efforts in the patient as an individuality'."[51] The Foundation sponsored a variant of biomedical holism that aimed at "'not only a clear conception of the patient as a total organism but also a concern for the patient as an individual personality often in need of sympathetic insight which the family physician of old was able to offer'."[52] In Davis' hands, the EEG provided a technological link between the progress of modern American biomedicine and the development of a holistic medical practice.

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[49]Letter, Davis to Dr. Frank Fremont-Smith, cc'd to Cannon, January 17, 1938 (WBCA 15, 179).

[50]Davis, Davis, Loomis, Harvey & Hobart 1938. See also Davis, "Alfred Lee Loomis," p. 8 (HDP FC022, 21, 60, 2); and Davis' letter to Cannon, September 8, 1938, WBCA, 15, 179.

[51]This quote was taken from an address written by Ludwig Kast, the President of the Macy Foundation, in 1936, as quoted in Pandora 1997, p. 74).

[52]Kast, as cited in Pandora 1997, pp. 74-75.

Hallowell Davis was equally interested in psychoanalysis, as were many neurologists and psychiatrists during the 1930s (Shorter 1997). Psychoanalysis, as we have already seen in the formation of the neuropsychiatry unit at Chicago, was a controversial facet of this push for holism in neuropsychiatry. But some neurophysiologists were equally influenced by psychoanalytic theory. Davis' attempt to apply electroencephalography to the question of personality was a case in point. Already in 1935, Davis had teamed up with Leon J. Saul, a psychoanalyst, to use the EEG to study patients at Franz Alexander's Chicago Institute for Psychoanalysis.[53] Their plan was to use the EEG to detect physiological changes brought on by psychotherapy. They wanted to test the EEG's ability to measure emotional states evoked in psychoanalysis, as well as its potential for classifying neurotic patients. Their project fit in perfectly with Alexander's attempt to bring psychotherapy to bear on orthodox medical practice through the new field of psychosomatic medicine. Alexander, who argued that the treatment of the emotional and psychological effects of disease had to be brought into regular medical therapeutics, was supported by the Rockefeller Foundation (Brown 1987). Davis' work clearly fit within this same framework, despite the fact that he came from a tradition of laboratory research, rather than clinical practice.

Davis' first public discussion of this work took place in June of 1937, during the convention of the American Neurological Association. At the "Neurological Supper Club," a gathering of elite members of the ANA, Davis outlined his interests in psychoanalysis. His work must have met with substantial protest, because in a letter to Cannon later that year, Davis thanked his departmental head for "passing along" some of the negative comments about Davis' presentation. The fact that these comments were anonymous (Davis was obliged to guess who was responsible for the remarks made to Cannon) made them all the more threatening. "I am not

[53]Letter, Davis to Cannon, May 3, 1935, WBCA, 10.123. This was the first time that Davis had proposed such a study to the Macy Foundation, whose response he described to Cannon as "definitely cool, giving as the particular reason the fact that psychoanalysis was involved and that their Foundation had, as a matter of policy, drawn the line on anything connected with psychoanalysis." Davis was unconcerned, however, as Gregg at the Rockefeller had expressed interest in the project.

surprised," wrote Davis, "that such a comment as you quote should come back to you, as two or three of the members, notably Dr. Myerson and [the] Bronson brothers, I could feel bristling with hostility (and perhaps disgust?) from the moment I mentioned psychoanalysis."[54]

While Davis readily agreed that "psychoanalytic description" suffered from "shortcomings," he clearly hoped that the EEG would serve as a bridge between his own field and Saul's, just as electroencephalography had started to bring together neurophysiology and clinical research through the Gibbs' studies of epilepsy. Refuting his anonymous critic, Davis insisted that "of course he has not seen any correlation between psychoanalysis and electroencephalograms. I believe that our own group is the only group which has had that opportunity." But despite the fact that Davis and Saul had decided to be "much more cautious" in their decision to publish their findings, Davis remained deeply devoted to the project: "In fact," he wrote, "I believe so strongly in the ultimate validity and importance of this field of inquiry that I am planning to be psychoanalyzed myself in order to obtain first-hand insight into the field and not be forced to rely on second-hand opinions."[55]

What, exactly, were the correlations that Davis and Saul saw between the EEG and psychoanalysis? Very little, it seems. In a progress report he sent to the Macy Foundation, Davis announced that the EEGs of patients at the Chicago Institute did not differ substantially from their control groups, and that "in spite of considerable changes in the emotional state of many of the individuals" treated by psychoanalysis, their EEG patterns remained the same. The only

[54]Letter, Davis to Cannon, December 21, 1937, WBCA, 13.158. Cannon's original letter is not in the archive, and Davis never cites the comments directly in his reply. They must have been severe, however, to warrant Davis' closing remarks: "I have written at some length because I think that it is quite possible that there may be further reverberations from my venture into dangerous territory, and I should like you to have on record these comments on the situation."

[55]Letter, Davis to Cannon, December 21, 1937, WBCA, 13.158.

major difference seemed to be that "persons who tend to be passive and dependent show many alpha waves, while those who are very active and driving do not."[56]

The transformation of American psychiatry

Why would an experimental physiologist, particularly one who was a member of the elite group of "axonologists" that studied the mechanisms of nervous transmission, bother with psychoanalysis?[57] Davis' hopes for the EEG were clearly framed by the massive asylum reform then taking place in the United States (Grob 1983, 1994; Shorter 1997). Psychiatry, which had been based on the care of the mentally ill housed in huge asylums, was undergoing somewhat of a revolution. Even as the numbers of patients in such state-run or charity institutions were expanding, psychiatrists began to see the future of their profession in private practice. But the stigma of asylum medicine precluded this possibility. Psychiatrists thus began to model themselves upon the neurologists, who had forged a lucrative practice in an urban setting, treating the neuroses of the wealthier ranks of American society. Outpatient clinics that treated acute cases of mental illness rather than warehousing chronic cases, began to spring up across America. In the wake of Clifford Beers' *A Mind That Found Itself* (1908), an autobiographical tale of a former psychiatry patient, psychiatrists began to forge alliances with prophylactic movements through the National Committee for Mental Hygiene (NCMH) in 1909 (Shorter

[56]Report from Davis to Dr. Frank Fremont-Smith, January 17, 1938, WBCA, 15.179.

[57]Davis, along with such luminaries as J. Erlanger, A. Forbes, H. Gasser, J.F. Fulton and R.W. Gerard, was one of the approximately thirty "axonologists" (Marshall 1983). Gerard, who was largely responsible for organizing the group, even considered Davis to be a "super-axonologist," indicating that Davis was a leader within this elite group. See the "axonologist" letters of October 22, 1932, March 10, 1933, and March 7, 1935 in AFA, 19.948 & 19.950.

1987:160-166; Grob 1983:279-316).[58] Thus began the move of psychiatry out of the asylum and into the mainstream of medical practice.

Psychiatrists also began to ally themselves with their medical counterparts, the neurologists. Where the term "neuropsychiatry" had once been a "purely political accommodation" to bring together these fiercely opposed groups to care for the wounded during the First World War, by 1934, the American Board of Psychiatry and Neurology had been established to certify both groups in concert (Grob 1983:279). At the same time, a plethora of new organic therapies for the treatment of mental illness were appearing, which were mentioned in chapter four. In 1936, Walter Freeman, a neurologist at George Washington University Hospital in Washington, D.C., chaired a special commission to study the situation of public mental hospitals in the U.S. That same year he conducted his very first lobotomy, a procedure he then proceeded to enthusiastically disseminate throughout the United States and Canada (Valenstein 1986; Shorter 1997). At the same time, psychosomatic medicine was also receiving widespread philanthropic support (Levenson 1994; Pressman 1998). Clearly, the 1930s was a time when virtually anything could, and did, pass for progress in the medical treatment of the mentally ill, and this included psychoanalysis. Hallowell Davis' interest in applying the EEG to psychoanalytic practice was merely one manifestation of the exceptional situation brought on by the convergence of a reformist drive in the care of the mentally ill, the appropriation of psychoanalysis by neuropsychiatrists, the reification of technology as a sign of biomedical progress, and the holistic rhetoric that permeated the scientific atmosphere of the 1930s.

The Davises' work on sleep at Tuxedo Park was minimal, but it was indicative of a deep-seated desire to accommodate, in a reliable and scientific manner, the emotional aspects of health

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[58]According to Grob, the NCMH began to enjoy great support from the Rockefeller Foundation through Alan Gregg, until the Committee began to turn towards the assessment of asylum practices as a central part of its mandate. Gregg pulled the plug on their "mental hygiene propaganda" in 1939, just around the same time he cut off funding to Nathaniel Kleitman's sleep research.

within the boundaries of modern medicine. Even when Loomis abruptly closed his laboratory in the summer of 1939 to work on microwave radar, Hallowell Davis did not have to look far to find an another avenue of EEG research. In the spring of 1940, Roosevelt had asked Congress for one billion dollars to "mechanize" the American armed forces and prepare for war (Cashman 1998:405ff). By May, Congress had approved spending $1.5 billion to improve the material infrastructure of the U.S. military, and gave F.D.R. an additional $1.7 billion to expand the army. The draft was introduced in September. By the summer of that year, Davis and Forbes were using the EEG to assess the emotional, physical and mental attributes of pilot trainees at the Pensacola Naval Base in Florida. The project lasted until 1943, but it ended in failure:

> Our results at the present stage of our analysis do not yet warrant the recommendation of the EEG as a routine instrument for pilot selection unless it be to exclude the rare pathological case. It might well be considered as a court of appeal when the instructor is in doubt concerning an erratic student. But it is worth considering the possibility that if the problem is shifted from simple success or failure in flight training to the question of differential selection for different types of service, e.g., bomber vs. fighter, the EEG may prove more useful. The differential attributes may prove to be correlated with recognizable features in the objective record of brain activity.[59]

After almost three years of research, Davis and Forbes had come up with no evidence that the EEG could be related to personality. The fact that they continued to think of the EEG in just these terms indicates the extent to which American neurophysiologists hoped to use this instrument to extend their disciplinary authority. Forbes and Davis' work at Pensacola simply extended the Macy Foundation's project of biomedical holism into the field of psychophysiological testing of military personnel. Like the patients whose complete medical treatment should include sympathetic insight from their physicians in addition to a battery of diagnostic tests, the trainees at Pensacola had to be evaluated in terms of their mental, physical and emotional performance before they ever stepped into the training planes. In both instances, the treatment of the "whole" person would, with the help of the EEG, produce an efficient and scientific result.

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[59]Hallowell Davis and Alexander Forbes, "The Selection of Naval Aviators—Pensacola Project, Final Report," p. 56 (PP).

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Electroencephalography was not simply taken out of the hands of Hans Berger and delivered to neurophysiologists by a great scientific authority, Edgar Adrian, during his visit to the United States in 1934. Rather, the EEG was born out of a climate of medical and scientific reform that emphasized technological innovation in biomedical research. The fact that Alfred Lee Loomis, one of the greatest scientific entrepreneurs in America, took such an active interest in the EEG testifies to the widespread potential that this instrument seemed to have in the 1930s. Because it graphically recorded the once-hidden electrical activity of the brain, the EEG transcended disciplinary fields, just as Mosso's ergograph had done forty years earlier. But where Mosso's device was merely an innovative way of isolating and recording the fairly well-understood phenomenon of fatigue, the EEG presented investigators with an entirely new kind of physiological activity. While they were not records of thought, brain waves clearly seemed to be records of thinking. That is to say, the EEG provided evidence of different brain states that could be correlated, albeit very roughly, to mental states.

This correlation between mind and brain was especially promising in the case of sleep. The great success the EEG enjoyed in this rather obscure physiological field changed the nature of sleep itself. For decades, physiologists had described sleep in terms of rhythms. But these rhythms had appeared only on the periphery, as blood pressure curves, changing respiration rates, or the decreased circulation of the blood. They also appeared in consciousness, as dreams. But in every case these rhythms were treated as the passive effects of fatigue. The sleep stages charted by the EEG, however, pointed towards a dynamic vision of sleep precisely because the EEG recorded the autonomous activity of the brain. Brain waves showed sleep stages to cycle independently of any stimulus, thus reinforcing the idea that sleep was not a mechanical reflexive act, but a manifestation of endogenous rhythm.

Chapter VII

Rapid eye movement
& the rhythm of dreaming
1953-1960

Electroencephalography framed sleep as a rhythmic cycling between qualitatively different stages. Electroencephalographers argued that these stages were signs of the depth of conscious awareness in sleep. Consciousness thus had its own complex rhythm in sleep that was neither a slow, continuous recovery from fatigue, nor a reflex response to external stimulus.

Did dreaming also have its own rhythm? This became a real question after the appearance and widespread application of the EEG in the late 1930s. The EEG made it possible to think about dreaming as a phenomenon that could be calibrated to the brain's internal regulation, rather than to external stimuli. Physiologists could now conceive of dreaming as a brain state, instead of as a narrative forged out of mental images.

But, on their own, sleep stages proved to be unreliable indicators of dreaming. It was not until the early 1950s, when Eugene Aserinsky began to study eye movements in the hope of discovering a new index of wakefulness, that dreaming became rhythmical. Dreaming was not initially part of Aserinsky's research program, but, by relating dreaming to periods of rapid eye movement, he turned an otherwise insignificant discovery about the cyclic motility of the eyes in sleep into an important revelation about the relationship between body and mind.

Rapid eye movement languished for several years after Aserinsky and Kleitman first announced their discovery in 1953. It was not until William Dement began trying to establish REM as a physiological surrogate for dream reports that REM began to attract any significant attention. The detection and measurement of dreaming with the tools of electrophysiology and clinical neurology held great appeal for American neuropsychiatrists, many of whom were determined to uncover a neurophysiological basis for psychoanalytic theory. Like the brain waves of the late 1930s, REM held out the promise that mental states could be reduced to brain states through the medium of rhythm. The research based on this prospect helped turn REM into a scientific fact.

The invisibility of rapid eye movements

In retrospect, the research conducted at Tuxedo Park in the late 1930s seemed to offer ample opportunities for the discovery of rapid eye movements, just as Nicholas Vaschide's all-night observations of sleepers had done at the turn of the century. Why did REM remain invisible, despite such an intensive study of sleep? Hallowell Davis himself has suggested that the group at Tuxedo Park was dedicated to investigating the EEG phenomena that were unique to sleep (slow, high-voltage waves and singular bursts of high-voltage activity known as "K complexes"), rather than the low-voltage waves that resembled alpha, and which later turned out to be associated with REM:

> Alfred was greatly interested in rhythms, particularly the sleep spindles (14 Hz) and the delta waves and the K complexes as sleep counterparts of the alpha rhythm, and he used them to define his stages of the onset of sleep. He gave very little attention to the low-voltage state of sleep later in the night that is now known as REM sleep. We let a big one get away that time, although the sheets from the chronograph, laid out on the ping-pong table in the game room, revealed at a glance the periods of slow-wave sleep and the periods of low-voltage sleep. We simply set the latter aside as uninteresting,—a return to light sleep. It was more fun to work with floating [a term they used for the "lapses" of consciousness just before sleep onset] and K complexes, which could be obtained very reliably on the drum and try to make something happen in the late low-voltage stage...We saw REM but didn't identify it as a separate state. We worked where the action was.[1]

"The action" for Davis and Loomis was in defining deep sleep in the terms of the EEG. Although they had attempted, as I described at the end of chapter six, to correlate dreaming to a particular stage of sleep, they never considered the possibility that dreaming *itself* might have a rhythm. Instead, they adapted the EEG to a traditional psychophysiological approach by trying "to make something happen" in slow-wave sleep by applying stimulus and waiting for a reflex response.

Rapid eye movements remained invisible to Edmund Jacobson for similar reasons. It would have been impossible for him to reconcile progressive relaxation with the idea that dreaming was a rhythmic brain activity. Jacobson thought dreaming disturbed, rather than protected, sleep—so how could dreaming have a physiological function? Progressive relaxation

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[1]Davis, "Alfred Lee Loomis," p. 9 (HDP FC022, 21, 60, 2).

was a method of restoring health, and part of this restoration involved the annihilation of dreaming. Jacobson had the technical ability to detect REM periods, but such a discovery had no place in his biomedical theories.

Like Jacobson, Kleitman thought sleep was an extension of relaxation. Dreaming represented the intrusion of mental activity into the quiescence of the night. Kleitman, like Piéron, treated dreaming as the epiphenomena of sleep. Their physiological studies of endurance and performance emphasized the reconciliation of sleep's periodicity with its ability to restore proper sensori-motor functioning. Dreaming had little place in such an investigative scheme. Even when he introduced electroencephalography into his research, he relegated the problem of dreaming to "the vast psychiatric literature dealing with the dynamic properties of dreams" (Blake, Gerard & Kleitman 1939:58). The Chicago group concluded that dream recall might be associated with brain wave patterns, but they cautiously interpreted this fact in terms of the waxing and waning of conscious activity in sleep, not as the binary appearance or non-appearance of dreaming itself. REM remained invisible for them because the ability to recall dreams did not fit into Kleitman's concept of sensori-motor performance.

Sleep research in the 1940s and '50s

The trajectory of Davis' investigations reveal the early hopes of the EEG in terms of its ability to classify human subjects, both for medical and for military purposes. The stability of an individual's brain waves were thought to reveal a hidden trait that could be correlated to an existing category, such as "emotionally unstable," "nervous," "calm," "dominant," "aggressive," and the like. Davis' work with the EEG did not differ a great deal from Jacobson's experiments with relaxation: they both used electrographic traces as a sort of sophisticated graphology (or perhaps even physiognomy) thought to betray personality differences, themselves located in physiological performances.

Sleep was only a marginal feature of such a research program. After all, what relevance could it have for the psychology of personality? Davis and Loomis had demonstrated that the EEG of sleep, like alpha, was surprisingly constant. But Loomis' passion for detecting and measuring rhythms was set within Davis' ambitions for the EEG as a tool for the study of individual difference—a study that relied on how people behaved, not how they retired from the world in sleep. Jacobson, and even Kleitman, adopted a similar perspective, depicting sleep exclusively as a passive state. Its relationship to normal psychology existed only insofar as it could be modified by habit, through conditioning that was either self-imposed, or a product of childhood training.

During the 1940s and 50s, Kleitman expanded his research into three main areas: shift work; biological rhythms; and sleep hygiene for infants. The first was the realization of his suggestion that the study of sleep would be relevant to the physiology of work (Kleitman 1923). In 1950, while preparing a chapter for a study of human performance in submarines, Kleitman spent several weeks on the U.S.S. *Dogfish*, conducting mental and physiological tests on sailors.[2] He found that the sailors' ability to perform various psycho-motor tests decreased along with their body temperatures, and used this as an argument to change the then-current system of split watches (four hours on, eight hours off) to correspond more closely to the diurnal temperature rhythm.

The study of shift work was merely an applied aspect of another of Kleitman's longstanding projects: the study of biological rhythms. The study of recurrent biological changes as a research interest can be traced back to the formation of the International Society for the Study of Biological Rhythms, a group of mostly medically-oriented researchers from Sweden, Germany, and the Netherlands, which held its first conference at Ronneby, Sweden, in August,

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[2]The book was entitled *Human Factors in Undersea Warfare*, and was published in 1950 by the National Research Council (see Kleitman's entry in *Current Biography*, 1957, pp. 306-308). The report was also published Kleitman & Jackson 1950. See also Kleitman 1943 & 1963, pp. 157-158.

1937 (Kleitman 1949a; Cambrosio & Keating 1983). Kleitman's 1949 review on the subject carefully wrote his own research into this history, noting that *Sleep and Wakefulness* had five chapters devoted to the topic of periodicity. As we have already seen in chapter three, rhythm had long been an important dimension of sleep research. Piéron, in his effort to avoid the teleology embraced by Claparède's concept of sleep as an "active defence," relied on the phenomenon of rhythm in organic and inorganic matter to fit sleep within the context of organic memory. Sleep was a habit, not a defence. In 1949, Kleitman offered a similar analysis of rhythm in Pavlovian terms: "a rhythm may be likened to a conditioned response, which is also individually acquired and depends on an extrinsic reinforcement for its establishment, yet will persist for a shorter or longer period of time in the absence of such reinforcement" (Kleitman 1949a:1).

Kleitman continued to characterize sleep as a rhythm throughout the 1940s.[3] By the end of the decade, however, he had found another field of applied research: infant sleeping patterns. The study of infants' sleep was a natural merger of Kleitman's evolutionary theory of sleep, the post-war "baby boom," and the rise of developmental psychology in the United States. In 1939, Kleitman had proposed a developmental account of sleep, which we discussed in chapter five. Sleep was a passive, "default" state punctuated by episodes of wakefulness that were linked to the physiological demands of nutrition and excretion. The 24-hour rhythm of sleep was only instilled in the infant through "family and community routines." It was not until 1949, however, that Kleitman began to study the sleep of infants for himself, rather than simply relying on published reports.

Kleitman's funding for this research came in 1949. In August, the *New York Times* reported that scientists at the University of Chicago had just received a $10,000 grant "for a

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[3]In his 1949 review, Kleitman refers to eight of his articles on sleep and biological rhythms that appeared in two different periods: 1937-1940; and 1946-1948. Clearly the war intervened, but I have not been able to discover what, if any, wartime research Kleitman conducted.

study of the sleeping habits of babies."[4] The money came from Swift, a meat-packing company based in Chicago, and Kleitman's project was clearly geared towards the corporation's interests: "the scientists will seek to determine," the *Times* reported, "whether a 25 per cent increase in protein content of the infants' diet will induce a more restful slumber. The babies will have specially prepared meats. An apparatus attached to the crib will record every movement made by the child, indicating the soundness of the sleep...[the scientists] added that distraught parents might receive some welcome news this fall." Kleitman's research came in the wake of a flurry of popular publications on child-rearing, the most notable of which were authored by Frances L. Ilg and Arnold Gesell (not to be confused with Arnold Gessel), from the Yale University Child Development Clinic, and, of course, Dr. Benjamin Spock.[5] Kleitman published the first of several articles on infant sleep by the end of 1949 (Kleitman 1949b, Kleitman & Engelmann 1953; Aserinsky & Kleitman 1955).

Discovering rapid eye movements: Eugene Aserinsky

It was in this context that Eugene Aserinsky first met Nathaniel Kleitman in 1950.[6]

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[4]"Study of Sleeping Babies Offers Hope to Parents," *New York Times*, August 7, 1949, p. 45. It should be noted that "the scientists" from the University of Chicago are never named in the article. Given the nature of the research, Kleitman's previous success at raising funds by product-testing for the food industry, and the fact that he published a popular article on sleep hygiene for infants two years later, it seems impossible that the story could be referring to anyone *but* Kleitman.

[5]Arnold Gesell and Frances L. Ilg, *Infant and Child in the Culture of Today: The Guidance of Development in Home and Nursery School* (Harper: New York, 1943); Benjamin Spock, *The Common Sense Book of Baby and Child Care* (Duell, Sloan & Pearce: New York, 1946). See Graebner 1980.

[6]I am not entirely sure whether Aserinsky began his studies at the University of Chicago in 1950, or 1951. I have chosen the former date on the basis of his statement that "when Dement entered my laboratory in December, 1952, a year had elapsed since I had seen that first REM disgorged by the erratic Offner polygraph." Given that Aserinsky described his first project under Kleitman as "lengthy," and also goes into substantial detail regarding the effort required to repair the Offner polygraph for use in his second project, the year in which he first observed REM (academic year 1951-2) was probably not his first year of research. I am thus suggesting that

Aserinsky (1921-1998) had an eclectic educational background: he had studied social science and Spanish, and been both a pre-medical and a dental student at several different institutions, yet he had no degree when he enrolled as a graduate student at Chicago.[7] Aserinsky hoped to study organ physiology, but, as virtually the entire Physiology Department was populated by cellular physiologists by 1950, Kleitman became his choice by default.

Aserinsky soon began working on the problem of infant sleep. Naturally, Kleitman gave him the task of studying a sleep rhythm. In this instance, Kleitman hoped that rates of blinking could be used as a criterion of sleep onset in infants. Aserinsky then proposed a new project in which a binary system was applied—he would simply compare periods of eye movement to periods of no eye movement in infants. Kleitman agreed, and Aserinsky eventually discovered a period of approximately twenty minutes in which the sleeping infants' eyes did not move at all.

Aserinsky's observation revealed nothing about sleep onset, but as it added yet another sleep rhythm to Kleitman's growing collection, Kleitman suggested that Aserinsky start working towards a PhD immediately, without finishing his master's degree. Aserinsky accepted, but recognized it for the risk that it was: if he failed to find something noteworthy, he could well end up with no degree at all (Aserinsky 1996; Lemaine *et al* 1977).

Kleitman encouraged Aserinsky to begin to conduct similar research on adult subjects, in an effort to connect it with his evolutionary hypothesis of sleep. Evidently, Kleitman's interest in the project was not enough to warrant the purchase of new equipment for this task—Aserinsky was stuck with an archaic Offner Dynograph "stored in the bowels of Abbott Hall" (Aserinsky 1996:216). This was a pen-and-ink device, but it had never been used for recording eye movement potentials, which, Aserinsky soon discovered, were difficult to distinguish from skin and EEG potentials, as well as from the 60 Hz interference coming from the electrical wiring of

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1950-1 was probably his first academic year (Aserinsky 1996).

[7]Biographical information is taken from Aserinsky 1996, and Lemaine *et al.* 1977.

the laboratory. In desperation, Aserinsky approached Frederic Gibbs, whose experience in recording EEG was unrivalled by the early 1950s. Gibbs recommended that Aserinsky abandon the electrical recording of eye potentials in favour of mechanical recording, as the problem of artefacts was endemic to the EOG (electrooculogram).

It was at this point that Aserinsky paid a visit to Edmund Jacobson. Jacobson had already established himself as an authority on the EOG through his electrophysiological studies of the late 1920s and 30s. Jacobson had also made some tentative suggestions about the relationship between dreaming and eye movements in sleep. Although never publically revealing that he had asked Jacobson for assistance, Aserinsky was quick to defend his own priority to the discovery of REM in a letter written to me in 1995:

> Dr. Edumund Jacobson and I were not at the University of Chicago at the same time. However, I did meet with him once in his office in downtown Chicago to discuss his method of recording eye muscle potentials. At that time, I had not yet discovered REM and was completely unaware of any connection between REM and dreaming. Also, while it is true that Jacobson speculated on a connection between eye movements and dreaming he was dead wrong because the eye movements seen at sleep onset are slow eye movements—except for pathological conditions such as narcolepsy...[8]

Regardless of how he settled his recording difficulties, Aserinsky soon began all-night sleep recordings of adults. He needed to discover something comparable to the twenty-minute period of ocular quiescence found in infants. Lacking any apparatus similar to Loomis' giant kymograph, Aserinsky was obliged instead to watch his archaic machine devour the requisite half-mile of paper tape, hoping to notice some transformation of the electrical record as it was happening. What he found instead were periods of eye movement. Having conducted a thorough literature review on the subject of eye movements (as well as having visited Jacobson),

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[8]Eugene Aserinsky to Kenton Kroker, July 19, 1995. Of course, it turned out that Jacobson (and Aserinsky) were not recording "eye muscle potentials" at all, but the change in direction of the electrical field created by the small electrical potential between the cornea and the retina (Richard Lange, interview). The phenomenon, however, was the same—it indicated eye movement.

Aserinsky was well-prepared to relate this phenomenon to dreaming.

Indeed, the 1950s were a period of exceptional growth in neurophysiology and neuropsychiatry (Marshall 1987; Marshall & Magoun 1998). The chemical theory of nervous transmission achieved consensus in 1953, when the neurophysiologist John C. Eccles finally recanted his belief in a purely electrical theory of nerve impulse conduction. The Horsley-Clarke stereotaxic instrument, revived in the late 1920s by S.W. Ranson at his Institute of Neurology at Northwestern University in Evanston, just north of Chicago, gave neurosurgeons a precise and standardized means of identifying sub-cortical centres. The greatest fruit of such research was the identification of regulatory centres deep within the brain. The "stereotack" was teamed up with the EEG to locate regions such as the reticular formation, which Giuseppe Moruzzi and Horace Magoun demonstrated in 1949 to be the centre responsible for blocking afferent stimuli during sleep (Lemaine *et al.* 1977:66-68). The most important EEG journal in North America, *EEG and Clinical Neurophysiology*, was founded that same year.

Several leading American neuropsychiatrists felt that these advances in neurophysiology would eventually come to provide a biological basis for psychoanalytic ideas. Stanley Cobb, for example, argued in 1949 that Freud's categories of "Super Ego," "Ego," and "Id" could be directly mapped on to brain areas identified by neurologists and neurophysiologists [Figure I] (Cobb 1949). Cobb was at Harvard at the time, but Freud's influence was just as strong in Chicago. Horace W. Magoun, an experimental neurologist at the Illinois Neuropsychiatric Institute as well as at Northwestern, agreed that psychoanalytic theory could, just like evolutionary concepts, be incorporated into current brain research (Magoun 1960).[9] Neuropsychiatrists at the University of Chicago seem to have been equally dedicated to psychoanalysis. When Aserinsky asked Dr. Nathaniel Apter, who headed the Psychiatry Department at Chicago, whether or not infants dreamed, "his reply was in the affirmative and

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[9]In contrast, there is no mention of Freud's influence on the neurosciences in Marshall & Magoun 1998.

Figure I
The synthesis of neurology & psychoanalysis
(Magoun 1960)

ENGLISH NEUROLOGY Hughlings Jackson	RUSSIAN NEUROPHYSIOLOGY Ivan P. Pavlov	COMPARATIVE NEUROANATOMY Edinger, Kappers, Herrick	PSYCHOANALYTIC PSYCHIATRY Sigmund Freud	SYNTHESIS
HIGHEST LEVEL	SECOND SIGNAL SYSTEM		SUPER EGO	ABSTRACTION DISCRIMINATION SYMBOLIZATION COMMUNICATION
MIDDLE LEVEL	CONDITIONED REFLEX		EGO	ACQUIRED ADAPTIVE BEHAVIOR
LOWEST LEVEL	UNCONDITIONED REFLEXES		ID	INNATE STEREOTYPED PERFORMANCE

FIG. 1.—Chart comparing the evolutionary concepts of the organization and function of the brain which developed after Darwin and Spencer. (Modified from a chart by Stanley Cobb, 1949. "Human Nature and the Understanding of Disease." In FAXON, N. W., *The Hospital in Contemporary Life*. Cambridge: Harvard University Press.

predicated entirely on Freudian concepts." "As an experimentalist," Aserinsky mused, "I considered his reliance on Freud as a sort of religious faith, and therefore his answer was of no value in helping me understand why I had apparently not seen REM in infants" (Aserinsky 1996:221).

Of course, there was nothing about being an experimentalist that ruled out an interest in psychoanalysis. As we have seen in the case of Hallowell Davis, it was possible to be part of the inner circle of neurophysiology, and maintain an active research program inspired by psychoanalysis. As an integral part of psychoanalytic therapy, dreams were naturally an active area of investigation. Dreaming had long been a favourite topic of Franz Alexander, who had published an elaborate quantitative study comparing the frequency of "oral" and "anal" dreams to incidence of peptic ulcer, chronic diarrhoea, and constipation (Alexander & Wilson 1935). In 1953, a Jungian psychologist, Calvin S. Hall, published a major study of dreaming (Hall 1953). Hall collected over ten thousand dreams by questionnaire, and then analysed them according to their theme, plot, setting, number and sex of characters. His conclusions were not particularly dramatic: he argued that dreams were far more mundane than Freudians had led people to believe, and that they were a forum for the expression of archetypal ego conflicts. My point, however, is simply this: dreams were a vital aspect of the psychoanalytic theory and practice that permeated neuropsychiatry in the United States in the 1950s. Any correlation between dreaming and physiological activity would represent an important discovery for any investigator, regardless of whether or not they approved of psychoanalysis.

Once Aserinsky had determined that adult sleep was punctuated by several periods of eye movements, he placed investigating an association between these periods and dreaming "very high on the agenda for further exploration" (Aserinsky 1996:219). Aserinsky reported that Kleitman remained quite sceptical about the existence of such a relationship, which is somewhat surprising given that Kleitman had co-authored an article almost fifteen years earlier relating dreaming to brain wave patterns (Blake, Gerard & Kleitman 1939). But we must remember that Kleitman did not include the ability to recall dreams as part of his concept of mental performance. Indeed, Kleitman himself performed rather poorly as Aserinsky's experimental subject on two occasions. The first night, Kleitman exhibited no periods of REM at all. On the second night, Aserinsky managed to record three different episodes of REM, but Kleitman definitively acknowledged that he was dreaming only once. Upon the other two awakenings, Kleitman reported that he might have been dreaming, or that he was simply awake.

Nonetheless, Kleitman encouraged Aserinsky to present his research at the upcoming meeting of the Federation of American Societies for Experimental Biology (FASEB), which was to be held in Chicago in 1953. Before the meeting, however, Kleitman insisted that Aserinsky use his daughter, Esther, as an experimental subject, perhaps because Kleitman felt she would offer reliable testimony as to whether or not she was actually dreaming. Aserinsky later claimed that he "could not fathom" why Kleitman "would subject his daughter to an experience that was less than pleasant" (Aserinsky 1996:222). But the use of family members as subjects for sleep research was not, in itself, particularly unusual. As we have seen, sleep researchers typically used whatever subjects were available. Hallowell Davis used his children, his colleagues, and Loomis' servants as subjects at Tuxedo Park. Aserinsky himself first observed REM on his own son. Moreover, Kleitman's daughter (who was almost thirty by this time) had conducted numerous experiments with him, already published one paper with her father, and later helped prepare the second edition of *Sleep and Wakefulness* (Kleitman & Kleitman 1953; Kleitman 1963).[10]

The trials must have been successful, because Aserinsky made the first public announcement of REM at the FASEB meeting in 1953 (Aserinsky & Kleitman 1953a). In their paper that appeared in *Science* that same year, Kleitman and Aserinsky used the EEG not only to show that these rhythmic eye potentials were not artefacts, but that these movements were clearly related to a low-voltage brain wave pattern [Figure II] (Aserinsky & Kleitman 1953b). In case the EEG proved to be insufficient evidence that the subjects were not actually awake, Aserinsky

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[10]I asked Esther Kleitman if she could comment on her memory of this Aserinsky's experiment, but unfortunately, she declined (letters, Esther Kleitman to Kenton Kroker, February 28, 1999, and April, 17, 1999).

Figure II

Vertical & horizontal eye movements
calibrated to respiration, motility & EEG in sleep
(Aserinsky & Kleitman 1953b)

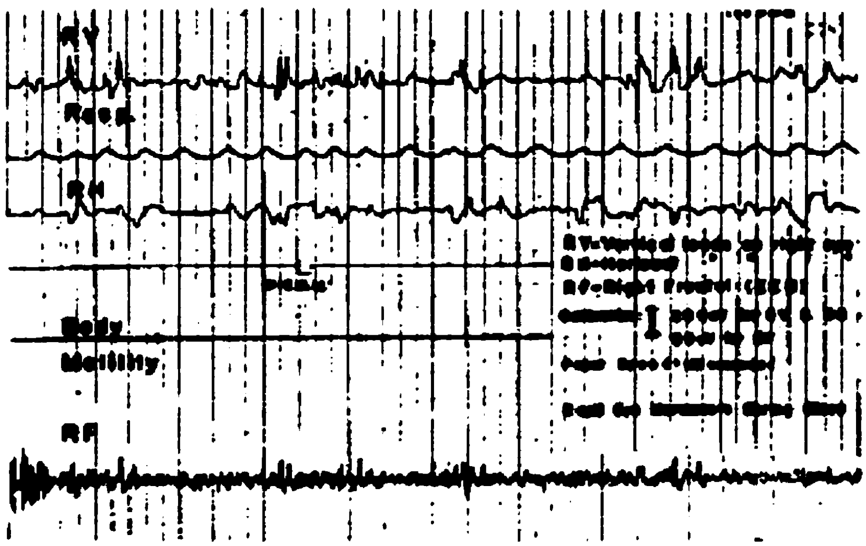
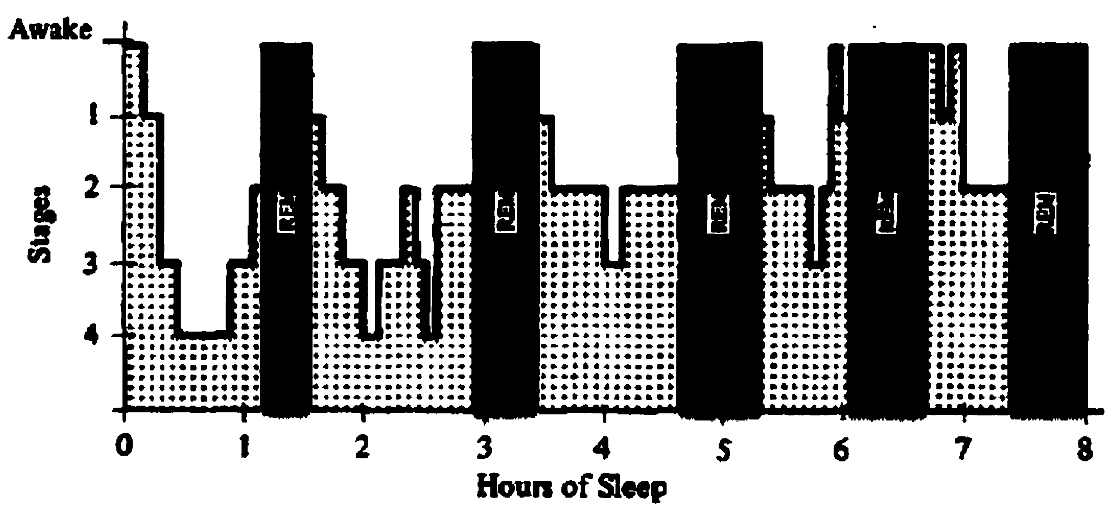


FIG. 1. Sample record exhibiting rapid eye movements in a sleeping subject. RV = vertical leads on right eye; RH = horizontal leads on right eye; RF = right frontal (EEG). Calibration: 200 μv for RV & RH; 50 μv for RF; paper speed: 10 sec.

Sequences of States and Stages of Sleep on a Typical Night



Relationship between sleep stages and REM periods
(Dement 1972)

filmed his sleeping subjects, and also indicated that he had seen the eye movements without the aid of any instruments (Aserinsky & Kleitman 1953b; Aserinsky 1996).

Aserinsky and Kleitman's paper in *Science* made it clear that the most significant aspect of their discovery was the fact that eye movements could now be related to periods of dreaming. Only ten of the twenty experimental subjects had been questioned as to whether or not they had been dreaming, but, in twenty-seven interrogations made during ocular activity, twenty revealed dreams "usually involving visual imagery," while the remaining seven indicated "failure of recall" or else "the feeling of having dreamed" (Aserinsky & Kleitman 1953b). When the eyes were motionless, twenty-three interrogations produced nineteen incidences of "complete failure of recall," with four reports of either having dreamed, or the feeling of having dreamed.

As we have already discussed in chapter five, eye movements in sleep had received attention by physiologists long before Aserinsky announced his discovery. Jacobson had related eye movements to dreaming in 1938. But Jacobson's casual observation, which was in keeping with the self-observations made in the nineteenth century by Wilhelm Griesinger and George Trumbull Ladd, said nothing about the *regularity* of these movements (Griesinger 1868; Ladd 1892; Jacobson 1938). They were random episodes of night life, just as dreams themselves were. The simple fact that there were regular periods of REM during sleep would have been an important contribution to sleep physiology. But Aserinsky's decision to relate them to dreaming made them part of the expansionary program of neurophysiology described above. Because of its new regularity, which Aserinsky was obliged to calibrate against the sleep stages of the EEG, dreaming was now a physiological event. The graphical traces of eye movements turned dreaming into a rhythm, rather than a response. Aserinsky's discovery suddenly brought dreaming into the foreground of sleep physiology:

> The fact that these eye movements, EEG pattern, and autonomic nervous system activity are significantly related and do not occur randomly suggests that these physiological phenomena, and probably dreaming, are very likely all manifestations of a particular level of cortical activity which is encountered normally during sleep. An eye movement period first appears about 3 hr after going to sleep, recurs 2 hr later, and then emerges at somewhat closer intervals a third or

> fourth time shortly prior to awakening. This method furnishes the means of determining the incidence and duration of periods of dreaming (Aserinsky & Kleitman 1953b:274).

The psychoanalytic triumph: William Dement

Aserinsky left Chicago as soon as he completed his dissertation in mid-1953. His relationship with Kleitman had been a rocky one, at best, and he was eager to move on. He took a one-year research position at the School of Fisheries at the University of Washington (Seattle), and then obtained a position at Jefferson Medical College, where he remained until 1976. If the citation record is any indication, Aserinsky's research attracted little interest—his two 1953 papers received a mere handful of citations until the mid-1960s [Figure III]. This was not due to a lack of publicity: the *New York Times* covered the research, and quoted Aserinsky and Kleitman boldly declaring that "'it will be possible, for the first time, to obtain objectively data on incidence, frequency and duration of dream episodes and to relate a dream pattern to other characteristics of personality or living habits.'"[11] It seems more likely that Aserinsky's discovery did not immediately fall into the right hands. He defended his dissertation before a group of cellular physiologists, who were largely uninterested in his work. And, although he and Kleitman published two more articles on eye movements, dreaming hardly figured in either paper (Aserinsky & Kleitman 1955a, b). Aserinsky did not publish again on the topic for another ten years. Aserinsky has complained that Kleitman himself was "uninterested in dreaming."[12] This was not quite accurate—Kleitman co-authored a paper on the subject, and he also discussed it in the first edition of *Sleep and Wakefulness*. But it is true that Kleitman was unable to capitalize on Aserinsky's discovery, simply because it did not fit into his program of research.

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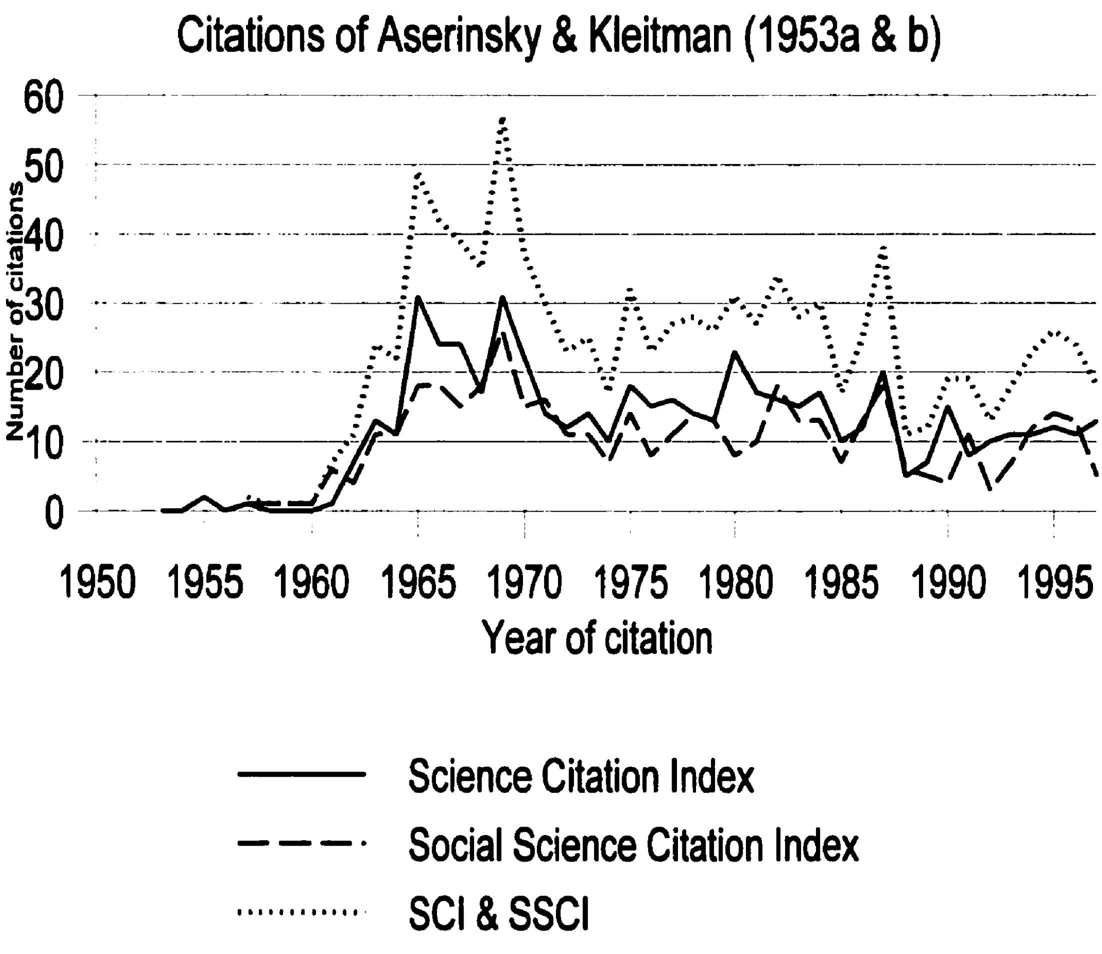
[11]*New York Times* (May 17, 1953, section IV, p. 11). Aserinsky reports that Kleitman arranged to discuss the discovery with the University's Public Relations Department in the most reprehensible manner. Kleitman spoke with them alone, asking Aserinsky to hide *behind* the door of his office, which was open just enough to allow him to hear what Kleitman said, and correct him later, if need be (Aserinsky 1996:223).

[12]Letter, Eugene Aserinsky to Kenton Kroker, July 19, 1995.

Figure III


Citations of Aserinsky & Kleitman (1953a & b)
60
50
40
30
20
10
0
Number of citations
1950
1955
1960
1965
1970
1975
1980
1985
1990
1995
Year of citation
Science Citation Index
Social Science Citation Index
SCI & SSCI

The future of REM was not with Aserinsky or Kleitman, but with a young medical student named William Dement (b. 1928). Dement joined Aserinsky in December of 1952, while he was pursing his MD at Chicago. He nursed a keen interest in psychiatry, and ended up in Kleitman's laboratory after hearing him lecture on sleep. Kleitman immediately threw him in with Aserinsky, who was then immersed in the question of REM. Although Dement himself ran very few of the experimental trials that led to the first publication on REM, his contribution was indispensable to its development. It was through Dement's efforts that REM was brought to the attention of psychiatrists and neurologists in the 1960s, and it was these groups—not physiologists—that turned REM into an idea that mattered.

Dement's first paper on REM appeared in 1955, just as he was finishing his MD (Dement 1955). Within five years, he had established three major directions in which experimental work on REM would later develop: the comparative study of REM in normal and abnormal subjects; the attempt to link eye movements to dream content; and the examination of the effects of REM deprivation. In Dement's hands, REM became the bridge that finally united Kleitman's sleep laboratory with the neuropsychiatry unit at Chicago.

Aserinsky has claimed that "the real purpose" of Dement's 1955 paper on REM was "to validate the original work" he and Kleitman had published in 1953 (Aserinsky 1996:224). This is not entirely accurate. Dement certainly confirmed that there were regular periods of eye movements in sleep, but this was not the purpose of the paper. Rather, he set out to extend the scope of REM's relevance by using it to compare the physiology and psychology of normal and schizophrenic subjects. This clinical application of REM, replete with psychoanalytic overtones, gave it an interdisciplinary scope that had been completely lacking in Aserinsky's research.

Dement found that the schizophrenics Nathaniel Apter had allowed him to "use" had the same REM periods as normal subjects. The difference was in their description of their dreams, which, Dement claimed, featured "isolated, inanimate objects, apparently hanging in space, with no overt action whatsoever" (Dement 1955:265). Dement interpreted the schizophrenics' frequent inability to offer a dream narrative as a manifestation of their mental pathology, and he illustrated this by citing their simplistic dreams of "hats" and "ripped coats" (Dement 1955:266). Because these dream reports had been produced from a REM-period awakening, Dement argued that these dreams were simply too mundane and fragmented to be normal. He went on to suggest that such research might shed light on the nature of schizophrenia, because it evoked a kind of dream material that was inaccessible "even during such heightened states of recall as psychoanalytic therapy." By using rapid eye movement as an index of the normal physiology of dreaming in clinical research, Dement was opening the door that connected physiology to neuropsychiatry.

After receiving his MD, Dement continued to work in Kleitman's laboratory, and he earned a PhD in physiology in 1957. He interned for a year, and then became a resident fellow in psychiatry in 1958. During this period, he continued to use REM to blur the boundaries between psychiatry and physiology. In two papers published in the *Journal of Experimental Psychology*, Dement began to develop what he later called the "scanning hypothesis," arguing that eye movements in sleep were actually following the activity retrospectively described in a dream report (Dement & Kleitman 1957; Dement & Wolpert 1958; Dement 1972:47-52).

Dement's "scanning hypothesis" was an attempt to drive REM beyond its role as a physiological index of dreaming, and relate it to the actual content of dreams themselves. The hypothesis was highly contentious, because it tended to rely on isolated incidences of high correspondence between dream reports and eye movements awash in a sea of dream narratives, bearing little or no relationship to the recorded data. Like the nineteenth-century physiological theory that claimed dreams were simply distorted perceptions of external stimuli, Dement's arguments took several simplified cases and used them to represent the nature of all dreams. Three records showing prominent vertical movement of the eyes corresponded to dreams of a hoist going up and down a cliff, climbing a series of ladders, and shooting baskets. A record with mostly horizontal movement related to a dream in which two people were throwing tomatoes at each other (Dement & Wolpert 1958).

Dement's attempts to develop REM as a form of dream recording evoked some pointed responses. A philosopher of mind, Norman Malcolm, argued that physiologists like Dement had misunderstood the normal usage of the verb "to dream." It could never be uttered in the present tense ("I'm dreaming") to mean an activity going on during sleep, which was, by definition, a state of unconsciousness. The meaning of the verb could only be inferred from the meaning assigned to the act of describing or reporting a dream. REM represented a physiological activity, but it did not represent what was ordinarily meant by "dreaming."[13] David Foulkes, a psychologist who worked in Kleitman's laboratory shortly after Dement, argued that dream reports could be elicited from non-REM and REM periods alike. The nature of the reports differed, but they were equally incidences of "mentation" during sleep (Foulkes 1962, 1966, 1996).

These critiques, however, did not prevent a growing number of psychologists and neuropsychiatrists from using REM to develop their own laboratory-based dream research (Foulkes 1996). The EEG was a familiar and popular tool of clinical research by this point, and it took little effort to add two more channels to measure eye movements. Because the detection of eye movements relied on the application of a well-established technology, REM sprang up everywhere there was an interest in dreaming. Many of the most outspoken critics were obliged to experiment with REM in order to counter Dement's ideas. Calvin Hall, for example, founded an "Institute of Dream Research" in Florida in 1962. He received several grants from the National Institute of Mental Health and began to study REM dreams. In 1966, he published the first of a series of works attacking the very idea that dreams recorded in sleep laboratories could be considered representative of the normal dreams of home (Hall 1966, 1967). The former, Hall argued, were unusual, because they often featured conflicts symbolizing the laboratory itself.

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[13]Malcolm 1959. For a critique of Malcolm's position, see Lewis 1968. Hilary Putnam rejected Malcolm's claim that the term "I am dreaming" is either ungrammatical, or that it presents a problem concerning meaning that is somehow unique to dreaming (Putnam 1962). For an analysis of this debate that sets it within the shifting styles of philosophy of language, see Hacking 1975.

Even though Hall was defending his survey-based method of content analysis by attacking the status of REM as an investigative tool, the fact remained that he was really doing little more than extending the reach of REM, just as Dement had hoped:

> It seems reasonable to conclude [wrote Dement] that an objective measurement of dreaming may be accomplished by recording REM's during sleep. This stands in marked contrast to the forgetting, distortion, and other factors that are involved in the reliance on the subjective recall of dreams. It thus becomes possible to objectively study the effect on dreaming of environmental changes, psychological stress, drug administration, and a variety of other factors and influences (Dement & Kleitman 1957b:346).

Dement's work on REM had started to invert the significance of dreams. Where Freud had once proposed that dreams were the surest point of access to the unconscious mind, Dement was now arguing that dreams, like temperature or blood pressure, could signify internal psychophysiological changes. REM had become one of a battery of diagnostic indicators, many of which relied on the graphical method of visualization. The maintenance of this "dream system," like that of any other system of physiological self-regulation, was depicted by Dement as a key ingredient in the preservation of health.

Dement continued this theme of REM as a diagnostic sign in his next major article (Dement 1960). In this paper, Dement outlined his theory of "dream deprivation," which argued that a certain amount of dreaming—equivalent to the sum total of all REM periods in a single night—was required for psychophysiological health. When deprived of this "dream time," subjects' brains would automatically and consistently attempt to increase their REM sleep on subsequent nights. If the deprivation continued, the subjects would begin to suffer adverse psychological effects.

Dement's experiment was little more than Piéron's and Kleitman's "experimental insomnia" adapted to the discovery of sleep stages through the EEG, and refined further by the discovery of REM. The method employed was relatively simple, but extremely time-consuming.

Each subject was obliged to endure five consecutive nights of "dream deprivation," in which they were awoken every time they entered a REM state, determined by characteristic eye traces and an active, low-voltage EEG pattern.[14] They were then allowed a number of "recovery nights," in which their sleep was monitored in the laboratory, but not disturbed. An identical routine was then employed targeting non-REM sleep. In all, the experimental trial involved between 20 and 30 nights for each of the eight subjects, including several nights of obtaining baseline values for each subject.

Dement found that subjects experienced a dramatic increase in their "dream attempts" after being deprived of REM sleep. The average amount of dream time had been 19.5% (80 minutes out of 6 ½ hours) of the total sleep time. In the recovery night that followed the first series of deprivation experiments, the demand for REM sleep rose to 29% of the total sleep time. A similar increase was not observed when subjects were deprived of non-REM sleep.

Psychological changes, including anxiety, irritability, and difficulty in concentrating, appeared after the dream deprivation periods. Non-cooperation was also a factor—Dement noted that one subject "quit the study in an apparent panic," and two others were only able to endure four nights of interrupted sleep, rather than the five that had been adopted as experimental protocol. While noting that none of these changes were "catastrophic," he concluded that "it is quite possible that if the dream suppression were carried on long enough, a serious disruption of the personality would result" (Dement 1960:1707).

A new vision of the functional significance of dreaming was emerging—one which treated the duration of dreaming as a measurable component of physiological and psychological health. Dement had taken the tools, concepts and practices of the sleep laboratory, and used them to turn Freud's claim that dreams protected sleep into a claim that dreams themselves needed protection. Censorship and repression were rendered irrelevant, and replaced by the testimony of

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[14]Dement and Kleitman calibrated REM to the EEG in 1957 (Dement & Kleitman 1957a).

the inscribed record. This was a far cry from Freud's insistence that "whatever the dreamer tells us must count as his dream, without regard to what he may have forgotten or altered in recalling it." Dement had dispensed with Freud's argument that a comparative study of the manifest dream report, with the latent dream wish, revealed the function of dreaming. For Dement, the sheer regularity of REM indicated that dreaming must serve some sort of physiological or psychological function. This regularity had been entirely invisible to physiologists and psychologists alike, until it could be harnessed to a graphical trace. As far as sleep was concerned, the EEG marked the first foray in this direction by calibrating the testimony of subjects to that of machines. Once this practice was thoroughly instilled in neuropsychiatry, it was only a matter of time before dreams, which played such an important role in psychoanalytic therapy, were brought under the reign of the rhythms inscribed by the EEG. Aserinsky was certainly the first to detect REM, but it took a neuropsychiatrist like Dement to propagate it.

Conclusion

The discovery of the electroencephalogram was the single most important development in the history of sleep physiology before 1953. The ability to graphically depict brain activity reconfigured the relationship between sleep, dreaming and consciousness. With Berger's discovery of alpha, the EEG quickly became a rudimentary index of conscious activity. The EEG seemed to transcend the narrow strictures of brain localization by depicting the activity of the cerebral cortex as a whole. Armed with this graphical surrogate for introspection, Berger immediately began to apply the EEG to pathological and quasi-pathological states of consciousness in which introspection was either unavailable, or of little value. Epileptics and sleepers could offer little subjective information about the nature of their state while it was happening. Berger anticipated that electroencephalography could reveal the behaviour of the brain in the absence of mind.

The trajectory of electroencephalography took a similar path in England and North America, even (in the case of Loomis) in the absence of any knowledge about Berger's work. Almost immediately after electroencephalography took hold of the scientific imagination in the U.S., the study of sleep and epilepsy became dominated by EEG research. Because the EEG construed brain activity in rhythmic terms, investigators began to emphasize the characterize sleep and epilepsy in terms of rhythm. The discovery of sleep stages broke up the unity of sleep by inscribing a series of discrete physiological events that cycled throughout the night. The idea that mental activity in sleep could follow a rigid and rhythmic pattern, rather than a constant gradient, was hardly conceivable outside of the graphic evidence afforded by the EEG. Earlier studies of sleep turned on interventionist methodologies. The course of sleep had to be interrupted or somehow manipulated in order to elicit phenomena that corresponded to wakeful consciousness. Varying levels of noise awakened sleepers to determine their "thresholds" of attention. Animals were deprived of sleep for weeks on end to identify the physiological origins of "vigilance." Such experimental practices reinforced the idea that sleep had meaning only insofar as it could be compared to wakefulness. The EEG changed this by creating a series of internal differences within sleep. Sleep began to be understood in terms of an autonomous, self-regulatory state.

The electroencephalographic trace became a surrogate for the speaking subject. But to create this effect, the epistemic authority of the EEG relied on a tradition of graphical investigation pioneered by Ludwig and Helmholtz during the 1850s, and expanded into the pivotal problem of fatigue by Marey and Mosso during the 1880s and 1890s. Electroencephalography brought instrumental phenomena to the forefront of sleep physiology. It was by no means the first graphical instrument to be applied to the problem of sleep. But electroencephalography was unlike any other graphical method in one important way: where Marey's pulse-writers and Kleitman's motility recorders simply extended the experimenter's powers of perception (of touch, or of sight) across the field of time, the EEG inscribed an electrical record that could not in any way be known without the instrument that created it. Alpha and beta corresponded to no sensory experience the experimenter could have *as* experimenter. The EEG, then, was not a kind of microscope that amplified the power of the senses. The EEG did not allow investigators to "see through" the instrument and grasp the essence of brain activity. Instead, the EEG seemed to coincide with the experience of the experimental subject, as attentive, concentrating, thinking, suffering from a seizure, or sleeping. It was the fact that the electroencephalograph worked on the same electrical principles as the brain that made the EEG an authoritative source of evidence. Electroencephalography inverted the traditional relationship between organism and instrument in sleep physiology, just as the advent of electronic amplification had done for neurophysiology in the 1920s. Just as the muscle response was supplanted by an amplified signal in neurophysiological research, so too was the subjective response displaced by the EEG trace in sleep research. The EEG was preeminently a physiological, rather than an anatomical or morphological tool. It spoke the dialect of time, not space. Having adopted the instrument, sleep researchers began to concentrate on charting the temporal, rhythmic nature of sleep. Framing dreaming in temporal terms through rapid eye movement was merely the extension of this same research program.

Alfred Lee Loomis' giant kymographic drum is perhaps the best illustration of the new primacy that sleep physiologists began to put on recording and rhythm after the EEG took hold of sleep research. Loomis' device encapsulated the direction that American biomedical research had adopted during the first three decades of the twentieth century. Instruments had become the focal point of scientific funding—they were easier to manage than individual researchers, and they held out the promise of having some practical application in the near future. Advances in diagnostic technology had become the centrepiece of the modern hospital, and were correlated with the standardized patient record in an effort to process patients efficiently and in a uniform manner. The development of diagnostic recording instruments became an integral part of the future of American biomedical science, because there was a market prepared in advance for such innovations. Entrepreneurs like Loomis recognized this, and set out to improve upon existing devices, such as the ECG, by extending the temporal scope of these instrumental recordings. His kymograph was able to calibrate the vital processes of the body over long periods of time, making it a natural fit with the problem of sleep. Opaque to introspection and shrouded in behavioural stillness, sleep had to first be rendered visible in order to be studied. Loomis' EEG allowed investigators to visualize sleep as a brain activity by compressing the brain's once-invisible electrical performance into a manageable bit of space. His work helped to set up rhythm as a new standard for information about sleep.

Electroencephalography provided the most dramatic way to render sleep into a visible object of research. But it was only one step in a series of developments that changed sleep from a reflexive response to fatigue into a rhythmic regulatory system. I have argued that this transformation of sleep began in the last quarter of the nineteenth century. Cast in the terminology of reflexes, sleep had once stood for nothing more than the quiescence of the body, and the derangement of consciousness. Sleep was the negation of life. The clinical problem of insomnia presented a challenge to this viewpoint. Doubtless, the inability to sleep had been a complaint of patients since the earliest days of medical practice. But when Hammond began to structure an entire nosology of nervous illness around the problem of insomnia, sleep attained a new importance in medical theory and physiological investigation. The rise of hypnotism as a physiological problem contributed to the new visibility of sleep by creating, at least for a few decades, a credible experimental analogue to sleep. The application of the graphical method to sleep's closest relative, fatigue, closed the circle. At the dawn of the twentieth century, the problem of fatigue was one of the most poignant ways for physiologists and psychologists to demonstrate how their knowledge could be relevant to social reform beyond the boundaries of clinical medical practice. Physiologists like Mosso and psychologists like Binet saw the deleterious effects of fatigue everywhere, from the violence between the classes to the inability of children to learn in school. The investigation of sleep fit perfectly within the disciplinary ambitions of psychophysiology. Not only was sleep thought to be caused by fatigue—it was also amenable to the same graphical methods of visualization. Sleep, like fatigue, was a subjective experience that could be made visible by tracing and calibrating the physiological responses to the will's exertion in wakefulness and in work. The graphical method turned sleep into a physiological performance.

Functionalist explanations borrowed from evolutionary theory also brought about a change in how the problems of sleep and dreaming were framed. In the 1890s, Delage and Freud assigned a physiological function to dreaming for the first time. Rather than serving as an example of how perception was crippled during sleep, dreaming began to take on a psychophysiological life of its own. Freud's attempt to describe dreaming in terms of how it regulated the mental economy was particularly important. Dreaming became a "safety valve" that prevented the unnecessary, or even dangerous, accumulation of nervous energy.

Sleep soon took on similar characteristics. Physiologists had always recognized that sleep served a function, in the sense that it was a recovery from the fatigue brought on by the physical and mental demands of wakefulness. But such an analysis continued to depict sleep as a negative state, thus rendering it inaccessible and largely uninteresting to any study of organic life. Sleep was a physiological, but not a biological, object. It was merely something that happened when the organism no longer had the energy to stay awake. Shortly after Freud introduced his "wish-fulfilment" theory of dreaming, Claparède proposed that sleep was not simply the mechanical product of fatigue, but an active instinct of defence. Sleep was the consequence of the innate ability of complex organisms to project themselves into the future. He agreed with Bergson—to sleep was indeed to be disinterested. But this lack of interest in sleep masked its important purpose. Inspired by James' attempts to describe consciousness in terms of function, Claparède argued that consciousness actively solicited sleep to defend the precious cortical cells against any physiological damage that might be caused by continued exertion.

Claparède's theory was popular among many psychologists, who were then struggling to establish the scientific authority of their field by using the concepts of evolutionary biology to frame questions about the mind. Neo-Lamarckian theory, which insisted that life normally progressed towards a more perfect form, dominated evolutionary thinking at the time. Yet in France, a stronghold of neo-Lamarckist biology, the teleological overtones of Claparède's idea did not sit well with many psychologists who were then trying to ground their knowledge as an experimental science. This curious paradox emerged because French psychologists, who were by training mostly philosophers, were engaged in a campaign to eliminate all metaphysical presuppositions from the scientific discourse of the new psychology. So they adopted the neo-Lamarckian emphasis on laboratory-based research, but dropped the teleological references so dear to their psychophysiological predecessors, Ribot and Richet. Could sleep be best described in terms of its purpose? Vaschide, for one, thought this was simply subterfuge. He insisted that any scientific account of sleep must be made in terms of physical and chemical mechanisms. Piéron's method of "enforced wakefulness" represented a sort of compromise between Claparède and Vaschide. Like Vaschide, Piéron wanted to maintain a clear separation between metaphysical ideas and empirical facts. Yet Piéron, like the neo-Lamarckian biologists who trained him, was not satisfied with simply amassing facts about sleep. He wanted to somehow reconcile the two dominant phenomena of sleep—fatigue and periodicity—without turning sleep's function into its cause. To accomplish this, he turned to animal experimentation, which effectively eliminated the introspective observations that had formed the core of Claparède's "biological" theory of sleep. Piéron felt he had discovered the chemical basis for sleep in "hypnotoxin." But he recognized that this chemical product of fatigue could not account for sleep's periodicity on its own. It had to be attached to a behaviour. To explain the diurnal rhythm of sleep, Piéron invoked the concept of organic memory. He argued that the routine activities of cells, tissues, organs, or organisms transformed their physiology, making them predisposed to behave in a certain way, given the right environmental conditions. These conditions could then trigger physico-chemical mechanisms that had been established by habit, not though a pre-ordained purpose. Piéron's work attempted to purge neo-Lamarckism of its teleology, and applied its physico-chemical approach to inheritance to the ontogeny of rhythms.

In organismic terms, Piéron described the idea that past events had an influence on the present in terms of an "instinct of anticipation." By the time that Nathaniel Kleitman studied with Piéron in the early 1920s, this idea was better known through the "conditioned reflexes" studied by Ivan Pavlov. But Pavlov himself said very little about rhythmic behaviour. He simply argued that all mental associations had a basis in the physiology of the cerebral cortex, and that the method of conditioned reflexes provided an ideal experimental model of how such associations were formed. Pavlov's argument that sleep was nothing more than generalized inhibition remained at the level of stimulus and response. Unlike Piéron's scheme, in which the creation of internal rhythms gave sleep an endogenous cause, Pavlov stuck to the older reflex idea. His inhibition theory suggested that all sleep was somehow engendered by sensation. Economo's explanation of encephalitis lethargica extended Pavlov's description of sleep as a cortical inhibition into the realm of a strange new disease. Yet encephalitis lethargica offered little evidence that excessive sleep was somehow caused by an external stimulus. So Economo broke with Pavlov and followed Claparède and Piéron in asserting that a sub-cortical centre for sleep must exist, and that it could become damaged in the course of an infectious disease. Economo's hypothesis of counterbalancing centres of sleep and wakefulness in the brain gave the active theory of sleep an anatomical basis. Epidemics of encephalitis gave sleep a space, both in the brain, and in the field of physiological research.

Graphical technologies supported this new vision of sleep by giving it an objective presence that subject accounts of sleep onset, dreaming, or waking could not provide. True, the evidence of these technologies could equally uphold a passive theory of sleep. Mosso, for example, used the graphical method to illustrate how respiration, pulse rate and the circulation of blood to the brain were all reduced during sleep. But graphical technologies also demonstrated hidden forms of activity in sleep. Kleitman, for example, used the graphical method to demonstrate that the degree of motility in sleep was much greater than had been thought. Jacobson's theory of progressive relaxation also depended heavily upon the rhetorical power of graphical technologies to help train his patients to feel their own musculature. Yet both Jacobson and Kleitman described sleep in terms of muscular relaxation, and explicitly spoke out in favour of a passive theory that took wakefulness as the standard against which sleep was measured. But their experimental practices expressed the problem somewhat differently. Their common dedication to constructing instruments that registered and recorded sleep phenomena inevitably illustrated the multifaceted activities of the body in sleep, even if these activities were, as in the case of inhibition, cast in negative terms. Although I have used the terms "passive" and "active" to describe theoretical positions taken up by generations of sleep researchers, these theories themselves are an entirely reliable guide to the history of the field. As one sociological study pointed out, passive and active theories of sleep have coexisted since the nineteenth century—there has not been a radical theoretical revolution in the field (Lemaine et al. 1977). My current focus on the practices of sleep research has suggested that changes that sleep research has undergone are best revealed through an examination of the instruments sleep researchers have deployed. It is these instruments, rather than any theoretical shift, that are primarily responsible for the most important event in modern sleep research—the discovery of REM.

But these instruments do not themselves encompass the entire practice. If they did, it would be impossible to explain why Jacobson never capitalized on his observation that dreaming could be correlated to eye movements, or why Kleitman never pursued the early work he conducted on the EEG and dreaming. Both emphasized the overwhelming importance of instruments to the study of sleep. But their emphasis on sleep as an essentially passive process meant that REM periods remained invisible to them, just as they had to Vaschide three decades earlier. Theories, practices, and disciplinary interests had to shift as well.

The ever-increasing role of instruments in American biomedicine also had a curious effect on the direction of medical practice from the 1920s to the immediate postwar period. In the face of the cold technological visage of modern health care, early proponents of psychosomatic medicine advocated a return to an emphasis on the "whole patient" that had characterized the medical practice of the past. To American neuropsychiatrists, psychotherapy offered one potential route back to medicine's idyllic and holistic traditions. Psychoanalysis in particular was embraced by numerous psychiatrists, neurologists and even some neurophysiologists, all of whom wanted to incorporate the problem of the emotions into their experimental and clinical practices. But, at least in the United States, the image of scientific medicine as technology-driven could not be discarded entirely. A technological means had to be devised to recover the whole patient in medical practice. Physiologists had been grappling with the question of the emotions through the graphical method at least since Mosso published his study of fear in 1884. In the 1930s, the hope that diagnostic technologies could contribute to holistic biomedicine took shape around the electroencephalogram. The EEG was not quite the graphic equivalent of thought, but it did seem to be capable of correlating brain states with mental states in a remarkable new way. Where older graphical technologies could only trace out the peripheral effects of brain activity, the EEG went directly to their source. The use of electroencephalography to describe epilepsy in terms of pathological electrical discharges in the brain was a potent example of what this new diagnostic phenomenology could accomplish. Organizations that supported biomedical holism, such as the Macy Foundation, anticipated that electroencephalography offered an entirely new diagnostic window on the concept of personality. Davis and Forbes, like so many proponents of the EEG, hoped that a person's brain wave patterns could reveal something about their mental and emotional constitution, and lead to improvements in both psychophysiological classification and in medical practice.

It was in the atmosphere of a holism permeated by technological innovation that rapid eye movements first began to take shape as a scientific fact. At first, the only interest that Eugene Aserinsky took in eye movements was as an extension of Kleitman's longstanding project of charting the rhythms of sleep. Even though Kleitman depicted sleep as a passive state, his devotion to the study of the alternating rhythms of sleep and wakefulness was perfectly embodied by the oscillating phenomena of the EEG. Kleitman had initially hoped that eye movements could be used to signify periods of wakefulness in infants, who could not offer their own testimony as to whether they were awake or not. Aserinsky dutifully watched sleeping infants for hours, made careful note of his observations, and concluded that regular periods of eye quiescence were a part of normal infant sleep. But the force of the graphical tradition meant that such physiological data had to be recorded. So when Aserinsky began studying adult subjects, a natural shift for anyone working under the shadow of Kleitman's evolutionary hypothesis of sleep, he was soon confronted by the EEG. By calibrating eye motility to the EEG, Aserinsky soon correlated periods of rapid eye movements to a low-voltage EEG pattern, which had already been understood as a return to light sleep. As he had already asked for Jacobson's assistance in recording eye potentials, Aserinsky was well aware of Jacobson's support for a motor theory of consciousness that associated muscular movements with mental images. From here, it was a small step to asserting that these REM periods were related to periods of dreaming.

Aserinsky, the EEG, Jacobson, REM periods and dreaming all converged around the technical problem of recording eye movements. On their own, the discovery of regular periods of eye movements in sleep would have been just one more curio in the cabinet of sleep physiology. For Aserinsky's discovery to achieve any substantial significance, it had to be related to states of consciousness. In the holistic, psychotherapeutically-oriented atmosphere of postwar neuropsychiatry, Aserinsky turned to dreaming as the phenomenon that would make REM count. Yet Aserinsky, who was dismissive of the Freudian bent of contemporary neuropsychiatry, was unable to capitalize on the value of his discovery. Rapid eye movement remained an insignificant episode in the annals of sleep research until it was brought to the fore by a psychiatry student, William Dement. Unlike Aserinsky, Dement did not reject psychoanalysis as irrelevant, or consider it to be beyond the pale of experimentation. Like many of his colleagues, Dement felt that some of Freud's ideas could be supported by experimental research. To this end, he developed a program that treated the rhythmic regularity of rapid eye movements as an indication that dreaming served some important biological function. Dement's efforts brought REM to the attention of fellow psychiatrists, whose interest in dreams had been fuelled by their appropriation of psychoanalytic ideas and practices. The ease with which the electroencephalograph could be adapted to the study of dreams gave their work exactly the sort of technological, laboratory-based scientific legitimacy neuropsychiatrists were seeking. Dement's work under Kleitman finally consolidated the relationship between the sleep laboratory and the psychiatric clinic, thus bringing to a close the project that had foundered in Kleitman's hands in the late 1930s.

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Invention or discovery? Social construction or empirical observation? In this study, I have tried to minimize the differences between these competing explanatory forms by paying close attention to the forces that shaped the history of sleep research up to 1960. Although I have used the phrase "discovery" to characterize how rapid eye movements first came to the attention of sleep researchers, I might just as well have used the term "invention." Each term hides as much as it reveals. If we abandon the practice of writing history around the idea that REM always existed but was simply overlooked (an unverifiable claim if there ever was one), we gain an appreciation for the enormous social, cultural, disciplinary and technological work that went into REM's discovery. This does not in any way deny REM's status as a scientific fact. On the contrary, it is Kleitman's version of events, with which I began this investigation, that tends to deprive REM of its history, by excluding from historical analysis key aspects of REM's factual status. The idea that the discovery of REM was nothing more than a felicitous event in the annals of sleep research is bizarre. It presumes that the structure of the sleep research field before REM had no effect whatsoever on the cognitive abilities of its members. And what scientist could possibly be satisfied with such an explanation, based as it is on nothing more than a theory of happy accidents? The present historical examination of the experimental practices that brought sleep research up to the dawn of REM indicates that scientific cognition itself can be transformed in a myriad of ways, helping to create a phenomenal basis for facts where none existed before. Rapid eye movement was no less socially constructed than it was observed and empirically verified, because the practices, instruments and theories that made such verification possible were themselves the product of social forces. It seems to me that it is the task of the historian to illustrate this dialectic between nature and culture, not to undermine it.

Appendix

Participants at the conference on
"The Electrical Potentials of the Brain"
held at the Loomis Laboratory, Tuxedo Park
November 10, 1935

The following names and institutional affiliations (mailing addresses) are taken from a list of confirmed guests (as of November 2), held in the Alexander Forbes Archive at the Countway Library (HMS c22, box 11, folder 527). Unless indicated otherwise, the corresponding disciplines and research areas are taken verbatim from *American Men of Science*, volume V (1933). If the individuals are not listed in volume VI (1938), they are considered "not listed."

ANDREWS, Howard L. *Physics*. Emma Pendleton Bradley Home, Rhode Island. High frequency oscillations.

BARTLEY Dr. S. Howard. *Psychology*. Washington University, School of Medicine, St. Louis. Vision; physiological psychology; theory, vision and electro-physiology of brain.

BRAY, Dr. Charles W. *Psychology*. Dept. of Psychology, Princeton University. Learning; audition.

BRONK, Prof. Detlev W. *Physiology, Physics*. Johnson Foundation (Director), University of Pennsylvania. Infra-red spectroscopy; electrical properties of glands; volume flow of blood; physiology of sense organs and nervous system

COBB, Dr. Stanley. *Neuropathology*. Prof. of Neuropathology, Harvard Medical School, Massachusetts General Hospital. Carbon monoxide asphyxia; mesencephalitis syphlitica; micrometry and the area striata; electromyography; intra-vital staining; the tonus of skeletal muscle; epilepsy; cerebral circulation.

COLE, Prof. Kenneth S. *Physics*. Prof. of Physiology, Columbia University, College of Physicians and Surgeons. Biophysics; electroscope theory; heat production of Arbacia eggs; photographic action of low speed electrons; electrical phenomena in living systems.

COMPTON, Dr. Karl T. *Physics*. Massachusetts Institute of Technology (President). Wehnelt interrupter; photoelectric effect; contact difference of potential and Peltier effect; structure of crystals by x-ray photography; soft x-rays; spectroscopy and extreme ultra violet; thermal effects produced by stretching of wire; ionization, fluorescence and dissociation of gases; low voltage arcs; spectral radiation; electric arcs and other types of gas discharge; thermionic emission.

DAVIS, Dr. Hallowell. *Physiology*. Prof. of Physiology, Harvard Medical School. Neuromuscular physiology.

DAVIS, Mrs. Hallowell. Not listed. Harvard Medical School.

DERBYSHIRE, Dr. A.J. *Medical physiology*. Dept. of Physiology, Ohio State University. Hearing and the nervous system (volume VI).

DUNBAR, Dr. H. Flanders. *Internal Medicine, Psychiatry*. New York Academy of Medicine. Symbolism; physiological changes accompanying emotions; psychiatric education of medical students; education of theological students in social and preventative public health problems (volume VI).

ERLANGER, Dr. Joseph. *Physiology*. Prof. of Physiology, Washington University, School of Medicine, St. Louis. Metabolism in dogs with shortened small intestine; principles of sphygmomanometry; mechanism of sound production in arteries; impulse initiation and conduction in heart; influence of pulse pressure on renal secretion; induction shocks as stimuli; nerve action currents.

EVANS, Dr. Gerald. Not listed. Cox Institute, University of Pennsylvania.

FINESINGER, Dr. Jacob. *Psychiatry*. Psychiatric Department, Massachusetts General Hospital. Cerebral circulation; autonomic nervous system; conditioned reflexes (volume VI).

FORBES, Dr. Alexander. *Physiology*. Prof. of Physiology, Harvard Medical School. The psycho-galvanic reflex; physiology of the central nervous system; reflex inhibition; electrophysiology of the nervous system; conduction of the nervous impulse.

FORBES, Dr. T. W. Not listed. New York State Psychiatric Institute and Hospital.

FULTON, Dr. John F. *Physiology*. Prof. of Physiology, Yale Medical School. Blood pigments of invertebrates, especially of Ascidians; affinities of the genus Trichodina pediculus; nature and significance of the electrical response of contractile tissues; reflex coordination of movement and posture; comparative physiology of the primate brain.

GAMMON, Dr. George. Not listed. Johnson Foundation, School of Medicine, University of Pennsylvania.

GARCEAU, E. Lovett. *Electrical engineering*. Diamond Hill, Rhode Island. Design of electrophysiological apparatus; electroencephalography (volume VI).

GASSER, Dr. Herbert S. *Physiology* (at Cornell, 1933). Rockefeller Institute for Medical Research (Director). Coagulation of blood; traumatic shock; electrophysiology of nerve with cathode ray oscillograph.

GERARD, Dr. Ralph W. *Physiology*. Prof. of Physiology, University of Chicago. Nerve metabolism and conduction; cell oxidations.

GIBBS, Dr. Frederic A. *Experimental neurology*. Dept. of Neurology, Harvard Medical School. Symptomatology of brain tumours; cerebral circulation; cerebral action potentials; epilepsy (volume VI).

GIBBS, Mrs. Frederic A. Not listed. Harvard Medical School.

HARTLINE, Dr. H. Keffer. *Physiology*. Johnson Foundation, School of Medicine, University of Pennsylvania. Physiology of photoreception; phototropism; electric responses of vertebrate and arthropod eyes; kinetics of osmosis in living cells.

HARVEY, Prof. E. Newton. *Physiology, Biochemistry*. Prof. of Physiology, Princeton University. Light production by animals; permeability of cells; oxidation; rate of nerve impulse; supersonic waves; centrifugal force.

HERVEY, Dr. John P. *Physics*. Johnson Foundation, School of Medicine, University of Pennsylvania. Thermionic vacuum tubes and associated circuits as applied in medicine and physiology (volume VI).

HILL, Dr. Samuel E. *Physiology*. Dept. of Physiology, Rockefeller Institute for Medical Research. Permeability of luminous bacteria; action potentials in Nitella.

HOAGLAND, Prof. Hudson. *Physiology*. Prof. of General Physiology, Clark University. Mechanism of tonic immobility; physiology of learning; mechanisms of sense organs; tropisms; effect of temperature on mechanisms of behaviour; electrical response of nerve.

HOBART, Mr. Garret A. Not listed. The Loomis Laboratory.

JASPER, Dr. Herbert H. *Psychology*. Emma Pendleton Bradley Home, Rhode Island. Neurophysiology; neurology and psychology of the bilateral neural organization, especially in stutterers and normal speakers; chronaxie studies of the effect of cortical tissue upon the functioning of peripheral nerves.

KREEZER, Dr. George. *Psychology*. Training School at Vineland, New Jersey. Experimental psychology.

LENNOX, Dr. William G. *Neurology*. Neurological Unit, Boston City Hospital. Epilepsy; blood chemistry; health of missionaries.

LINDEMANN, Dr. Erich. *Psychiatry, Psychology*. Dept. of Physiology, Harvard Medical School. Psychology and psychopathology of perception; psychopathology of drug action; neurophysiological studies by action currents and chronaximetric investigation; psychotherapy of mental diseases.

LOOMIS, William F. Not listed. Harvard University.

MASON, Dr. Max. *Mathematics*. Rockefeller Foundation (President). Differential equations; calculus of variations; electromagnetic theory; submarine detection devices; acoustical compensators.

MAX, Dr. Louis William. *Psychology*. Dept. of Psychology, New York University. Time relations of electrical and mechanical response of the heart muscle; relation of the metallic ions of blood to origin of cardiac impulse; vacuum-tube amplifiers; physiological basis of thinking.

McGRAW, Dr. Myrtle B. *Psychology*. Babies Hospital, New York City. Genetic psychology; reflexive and adaptive behaviour in infants (volume VI).

MEANS, Dr. J. H. *Medicine*. Professor of Medicine, Harvard Medical School. Respiratory metabolism in disease; functional pathology of the respiratory and circulatory systems; diseases of the thyroid gland.

MENKE, Mr. John T. Not listed. Johns Hopkins Medical School.

ODLUM, Mr. Floyd B. Not listed. The Neurological Institute (President), New York City.

ROBERTS, Dr. Dudley. Not listed. The Neurological Institute, New York City.

RHEINBERGER, Miss Margaret. Not listed. Emma Pendleton Bradley Home, Rhode Island.

SLIGHT, Dr. David. Not listed. Dept. of Psychiatry, McGill University.

SMITH, Dr. J. Roy. *Neurophysiology*. Babies Hospital, New York City. Sense organ function; nervous processes of the vertebrate retina; electric action potentials of the human brain (volume VI).

TILNEY, Dr. Frederick. *Neurology*. The Neurological Institute (Director), New York City. Behaviour and brain development; brain lipoids and brain development; comparative sensory analysis; the pineal gland; the brain from ape to man; brain of prehistoric man; epidemic encephalitis; form and function of the central nervous system; the hand and brain in the evolution of intelligence.

WEAVER, Dr. Warren. *Electromagnetism*. Rockefeller Foundation (Director of Physical

Sciences). Electrodynamics; theory of probability; applied mathematics.

WEAVER, Dr. E.G. Not listed. Dept. of Psychology, Princeton University.

WILLIAMS, Dr. Horatio B. *Physiology*. Dept. of Physiology (Director), Columbia University. Animal calorimetry; electrophysiology; sounds; nerve-action current phenomena; x-rays; theory and design of string galvanometer; electrocardiology; electric shock.

WOLFE, Dr. Theodore P. Not listed. New York Academy of Medicine.

WOOD, Prof. Robert W. *Experimental physics*. Dept. of Physics, Johns Hopkins University. Physical optics.

Breakdown by discipline:

Physiology (includes Medical Physiology, Neurophysiology, Medicine, two multiple listings—Bronk & Harvey, and two not listed)	17
Physical sciences (incl. Experimental Physics, Electrical engineers, Electromagnetism, and one not listed)	9
Psychology (incl. one not listed)	7
Neurology (incl. Experimental Neurology, Neuropathology, and three not listed)	7
Psychiatry (incl. two multiple listings—Dunbar & Lindemann, and two not listed)	5
Other (Mathematics)	1
Unknown	4
Total:	50

Sources

Archival

Abbreviations following collection names correspond to those used throughout this dissertation.

i. Countway Medical Library, Harvard University, Cambridge, Massachusetts
a) Alexander Forbes Archives [*AFA (box,folder,item)*]
b) Pensacola Project [*PP*]
c) Walter Bradford Cannon Archives [*WBCA (box,folder,item)*]

ii. Rockefeller Archive Center, Sleepy Hollow, NY [*RAC (record group,series,box,folder)*]

iii. University of Chicago Archives, Chicago, Illinois
a) Association for the Psychophysiological Study of Sleep, records, MS 92-2

iii. American Philosophical Society
a) Simon Flexner Papers [*SFP*]

iv. Cornell University Archives, Ithaca, New York
a) Titchener Collection [*TC*]

v. Alan Mason Cheney Archives, Johns Hopkins Medical School, Baltimore, MD
a) Adolf Meyer Papers [*MP*]

vi. Washington University at St. Louis, St. Louis, Missouri
a) Hallowell Davis Papers [*HDP*]

Interviews & Personal Correspondence

The following interviews were utilized in the composition of this dissertation. They are identified in the text with the name of the interviewee, followed by an asterisk, and the date the interview was conducted (for example, "Broughton* 1999"). The same procedure has been adopted to mark any reference to personal correspondence, which includes e-mail and letters. All interviews were conducted and transcribed by the author.

Broughton, Roger. February 5th, 1999. Interviewed in person at his office at the University of Ottawa Hospitals, Ottawa, Ontario.

Gessel, Arnold. 1996-1997. Personal correspondence.

Jacobson, Edmund Jr. 1996-1998. Personal correspondence.

Lange, Richard. December 18th, 1998. Interviewed over the telephone from his home in Chicago, Illinois.

Mason, Peggy. December 11th, 1998. Interviewed in person at her office and in her laboratory at the Surgical Brain Research Institute, University of Chicago Hospitals, Chicago, Illinois.

Morcos, Helene. December 11th, 1998. Interviewed in person at her home in Chicago, Illinois.

Rechtschaffen, Alan. February 18th, 1999. Interviewed over the telephone from his residence in Fountain Hills, Arizona.

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