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Wired Together: The Montreal Neurological Institute and the Origins of Modern Neuroscience,
1928-1965

A dissertation presented by

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to

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Abstract

This dissertation presents a reinterpretation of the historical development of modern neuroscience during the middle decades of the twentieth century. Contrary to the existing historiography, I argue that an interdisciplinary approach to unravelling the mind/brain relationship did not develop first at the Massachusetts Institute of Technology (MIT) under the aegis of F.O. Schmitt and the Neurosciences Research Program in the 1950s. Rather, modern neuroscience was the product of a relatively small group of historical actors operating at the Montreal Neurological Institute (MNI) between 1928 and 1965. Under the leadership of its founder, the neurosurgeon Wilder Penfield, the MNI became a unique site for medical research into the human brain, and achieved an unprecedented level of disciplinary integration. Chapter One examines the origins of Penfield’s vision for an interdisciplinary neurological institute; the structure of the MNI was rooted not only in the broader agenda of his Rockefeller Foundation patrons for interdisciplinary medicine, but also in Penfield’s scientific biography, which included considerable experience in laboratories and clinics in Europe as well as the United States. Penfield sought to create a new kind of neurological clinic that could employ the insights of the emerging science of histology and cytology – the microscopic study of tissues and cells – to advance the professional standing of neurosurgery. Penfield ultimately established a clinic in Montreal, a city that he felt would allow him access to the scientific and medical cultures of Europe and North America. Surgery for epilepsy became the signature operation of the new clinic. Chapter Two traces the development of neuropsychology at the MNI. Penfield recruited a series of young psychologists to address post-operative intelligence and memory deficits. The nearly two-decade collaboration between the MNI's surgeons and the psychologists D.O.
Hebb, Molly Harrower and Brenda Milner led to the emergence of cognitive neuroscience as a new field of inquiry. Chapter Three details the contributions of Penfield’s closest collaborator, Herbert Jasper, whose pioneering work with the electroencephalograph (EEG) not only reinvigorated the study of the functional anatomy of the human brain, but also acted as a crystallization point for global efforts to create an Interdisciplinary Brain Research Organization (IBRO). The IBRO became the acknowledge origin of neuroscience outside of the United States. Jasper’s brand of neuroscience displayed a notably different style from that emerging at MIT. An examination of how the MNI and MIT groups addressed the issue of memory illustrates these different styles; while the MIT group searched fruitlessly for a ‘memory molecule’ akin to DNA, the MNI group engaged in more profitable studies of the functional anatomy of memory. Chapter Four examines the troubled relationship between the MNI and the field of psychiatry as a case study in failed interdisciplinarity. Penfield was initially optimistic about incorporating psychiatry into the interdisciplinary community at Montreal; however, the psychiatrist selected to run Montreal’s new training facility for psychiatrists, the Allan Memorial Institute (AMI), was uninterested in collaborating, except on the issue of psychosurgery. The issue of psychosurgery led to a breakdown of communication between MNI and AMI, leading Cameron to disassociate himself from the Montreal neuroscience community. This disassociation afforded him the opportunity to engage in ethically questionable experiments in ‘psychic driving’ that were later revealed to be funded by the American CIA. In the Conclusion, I reflect upon the reasons for the decline of the MNI, and the ascendance of MIT’s molecular approach to neuroscience. Drawing on the insights of Mark Granovetter, I argue that the MNI’s brand of neuroscience was a product of its strong assemblies of historical actors, which were enriched by its ‘weak ties’ to other scientific cultures.
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Acknowledgments

Wilder Penfield was an ever-present figure in my childhood, as he was for many Canadians of a certain generation, even if only through the medium of television. As a child growing up in Canada during the 1990s, I can vividly remember being introduced to this fascinating character through the ubiquitous Canadian Broadcasting Corporation “Heritage Minute” short films, which aired on television between commercials, and which aimed to inculcate a sense of national pride. The Heritage Minute that focused on Penfield was always my favorite. I was captivated by the figure of Penfield, as he gently probed the exposed brain of a French-Canadian woman in order to locate the origin of her epileptic seizures. I never dreamt that I would have the opportunity to investigate the figure of Penfield and his Montreal Neurological Institute on the scale that I have, or to do so at a place as remarkable as Harvard University.

My research into Penfield revealed a much deeper, richer historical character than was portrayed in those ubiquitous Heritage Minute films, but also helped me to appreciate the remarkable community of individuals who helped make the Montreal Neurological Institute what it was. I initially dismissed the title of Penfield’s autobiography, No Man Alone, as an exercise in false modesty. It is only through years of my own research that I came to realize how honest the title was, and how meaningful. In many ways, this dissertation is about the history of collaboration itself, and it has made me acutely aware of how important the contributions of others are to any intellectual product – scholarly or scientific. In that spirit, I will ask the reader’s indulgence for a fairly extensive list of ‘thank yous.’

This dissertation, and indeed my scholarly career, would have been inconceivable without the wisdom, knowledge and phenomenal support of my advisor, Anne Harrington. Anne nurtured this project from almost impossibly vague beginnings, and I have benefited from her insight and encouragement in ways too numerous to count. When I first became aware of her scholarship, it
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Discovering one intellectual kindred spirit would have made coming to Harvard exciting enough. To discover two more, both of whom agreed to serve on my committee, was an embarrassment of riches. Rebecca Lemov and David Jones proved to be perfect compliments to Anne, and this dissertation was immeasurably improved by their guidance. Rebecca’s infectious sense of curiosity proved to be the perfect supplement to my own, and her enthusiasm for even my most nebulous ideas provided the encouragement I often needed. David’s insightful questions and enthusiasm for the project, both born of extensive first-hand knowledge, convinced me that I could write about the most technical and complex aspects of science and medicine in a way that was historically sensitive and compelling. Together Anne, Rebecca and David convinced me that this project could be done, and thanks to their unwavering support, it is.

Beyond my committee, the faculty of Harvard’s History of Science Department proved to be just as supportive and enlightening as I had hoped. I couldn’t ask for better teachers than Allan Brandt, Janet Browne, Alex Csiszar, Peter Galison, Evelyne Hammonds, Matthew Hersch, Shigehisa Kuriyama, Gabriela Soto Laveaga, Elizabeth Lunbeck, Hannah Marcus, Naomi Oreskes, Ahmed Ragab, Sarah Richardson, Sophia Roosth, Steven Shapin and Victor Seow. Special thanks in particular are owed to Sophia and Alex, whose mentorship (and friendship) to young scholars is a considerable inspiration. Special thanks are also owed to the History Department’s Andrew Jewett, who supervised one of my general reading fields, and whose encyclopedic knowledge and generous spirit informed every aspect of this dissertation. I have also benefited from numerous conversations with Melinda Baldwin, Hanna Rose Shell, Bruce Moran, Evan Hepler-Smith and Julia Reed.
I began my graduate education in 2012 with a remarkable cohort of fellow students, and my appreciation for the graduate student community has only grown with time. Coming in for particular thanks are Lisa Haushofer, Danielle Hallet-Inkpen, Katherine Heintzman, Anouska Bhattacharyya, Miriam Rich, Ángel Rodriguez, Eli Nelson, James Bergman, Gustave Lester, Devin Kennedy, Jacob Moses, Allyssa Botelho, Gili Vidan, Anya Yermakova, Katie Baca, Colleen Lanier Christnsen, Cara Fallon, and Michelle Labonte. My officemates, Lisa Haushofer, Wythe Marshall, Ángel Rodriguez and Leena Akhtar deserve special thanks for having to be in such close proximity to my occasional bouts of neurosis. A few friends outside of Harvard helped to keep me sane (no small task). Thanks in particular to Colin Phillips and Crystal Marko, Richard Krueger, Ryan Davidson and Britany Luby.

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Much of the research that comprises this dissertation took place in the Osler Library for the History of Medicine and the McGill University Archives, and I would like to extend my sincere thanks to the librarians and archivists who indulged my endless requests for material. Thanks to Chris Lyons, Lily Szczygiel and Duncan Cowie of the Osler Library, and Gordon Burr and Isabelle Morissette of the McGill Archives. Thanks are also due to the Social Sciences and Humanities Research Council of Canada, and Harvard’s Weatherhead Center, for their generous financial support, as well as the Dr. Granville Nickerson, whose memorial fellowship for his late wife (The Mary Louise Nickerson Fellowship in Neuro History, administered by the Osler Library), provided much needed travel funds.
Finally, the contributions of my family to this project are too numerous to count. To my
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personal. And most of all to my parents, who influenced this dissertation in ways that I cannot even
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brain sciences long before I came to Harvard, and their influence can be felt on every page, along
with their love. I thank them from the bottom of my heart.
For my mother, Glenda, who taught me to love the brain, and my father, Ken, who taught me to love the mind.
Archival Sources

The primary archival sources used for this dissertation are listed below. All material is used with permission from the relevant archives.

Osler Library of the History of Medicine, Montreal, Quebec, Canada

Wilder Penfield Fonds, P142 (Abbreviated as WP Fonds)
Jefferson Lewis Collection, P190 (Abbreviated as Jefferson Lewis Collection)

McGill University Archives

Donald Olding Hebb Fonds (Abbreviated as DO Hebb Fonds)
Herbert H. Jasper Fonds MG 4253 (Abbreviated as Jasper Fonds)
Ewen Cameron Fonds (Abbreviated as Cameron Fonds)
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Introduction - Montreal and the Origins of Neuroscience

If one were to stand at the corners of Callière and Commune West streets in downtown Montreal in April of 1939 and look eastward, one might catch a glimpse of transport ships sailing in and out of the Lachine Canal on the St. Lawrence river, the waterway that turned the city into the economic heart of British North America in the nineteenth century. If one turned west and walked through Place d’Armes square, one would pass both the Notre Dame Basilica, the spiritual center of the city’s French Catholic community, and the imposing neoclassical headquarters of the Bank of Montreal, whose capital financed much of Canada’s construction, and turned Montreal into a major financial center by the turn of the twentieth century. The city’s strategic position as a transportation hub, and as a meeting place for the Old World and the New, had transformed it from a minor seventeenth-century fur-trading post into a major cosmopolitan metropolis on the eve of the Second World War; this fact would become more and more apparent as one passed through the bustling taverns and pubs of St. Catherine Street, whose prosperity had been fueled by American prohibition. This striking contrast, between a modern, cosmopolitan hub and an antiquated, provincial refuge, would only be reinforced if one continued northwest along University Street, toward the architectural elegance of the Royal Victoria Hospital - one of the most advanced hospitals on the continent in 1939, yet perched on the slopes of Mount Royal beneath the 100-foot crucifix that had been a resident on the hill, in one form or another, since 1642.¹

And if one turned off University Street and entered the Montreal Neurological Institute, one might observe an even more remarkable sight. In the Institute’s Operating Room 1, the neurosurgeon Wilder Penfield stands over the exposed brain of a male patient. Penfield is operating

on the man’s left temporal lobe. The patient, a 32-year old male school teacher, has a scar on the surface of his brain, created by an automobile accident in Philadelphia three years earlier. The scar has caused him to suffer from epileptic fits, and Penfield is attempting to remove it. Standing next to Penfield is another man, Donald Olding Hebb, a psychologist who is speaking continuously with the patient. In the coming months, Hebb will make a close study of the patient during his recovery, but for the moment, because the scar is located close to one of the brain’s speech centers, Hebb is drawing the patient out on various subjects in order to ensure that his ability to speak will not be affected. And standing just off of the main operating room floor, peering over his electroencephalogram (EEG), is Herbert Jasper, a young psychologist and physiologist who is the acknowledged master of this new technology. Jasper had initially used the EEG to localize the offending lesion, but subsequent intracranial recording of electrical signals has refined the localization of the epileptogenic tissue, and Jasper is monitoring the operation in real time so that he can further perfect his technique for recording brainwaves.2

To a modern observer transported back to 1939, the above scene might appear remarkable because of the high medical drama inherent in brain surgery, or the macabre spectacle of a conscious patient reporting, in real time, the experience of having his brain stimulated with an electric probe. A witness with some knowledge of the history of neurosurgery might recognize the scene as the first use of intracranial EEG recording during a brain operation, or might be aware of Penfield’s contributions to our understanding of epilepsy, or his speculations about how memory is recorded in the human brain.3 Yet a more modest fact about the events in Montreal that day is equally worthy

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of note, and in many respects is more historically significant: all three men who stood over the exposed brain of the surgical patient in April of 1939 were working as a team. In the cosmopolitan and bilingual city of Montreal, at the heart of what the Canadian historian Donald Creighton referred to as “the commercial empire of the Saint Lawrence,” a new, interdisciplinary, collaborative scientific field - neuroscience - is being born. This dissertation is a history of that birth.

Modern Neuroscience and its Origins

It might come as a surprise to many readers that the Montreal Neurological Institute (MNI) constitutes the birthplace of neuroscience. Moreover, modern readers might be equally surprised to learn that ‘neuroscience’ is such a recent invention. After all, scientific and medical specialties that take the nervous system (and the mind) as their subject are at least as old as the seventeenth century (for instance, the English doctor Thomas Willis coined the word ‘neurology’ - or the doctrine of the nerves - in the 1660s). Over the course of the eighteenth and nineteenth centuries, a number of scientific and medical disciplines arose that treated the brain and mind as an object of inquiry; psychiatry, neurophysiology, clinical neurology and experimental psychology all came into existence, and tried to unravel the workings of the mind, the brain or both. Many historians use the word

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‘neuroscience’ to refer to these scientific fields unproblematically, projecting into the past an anachronistic notion of shared purpose and mutual understanding that would have seemed alien to the scientists and physicians involved. Yet if ‘neuroscience’ is defined, as it often is, as an interdisciplinary scientific field that aims to understand how the brain creates the mind, and controls the behavior of organisms, then ‘neuroscience’ is a uniquely modern field of study - a product of the twentieth century.

To the extent that there is a traditional origin story of the emergence of neuroscience in the twentieth century, it goes like this: in 1941, Vannevar Bush and Karl Compton recruited a young American biologist named Francis O. Schmitt to revitalize the biology department at the Massachusetts Institute of Technology (MIT). Schmitt certainly had the credentials; as a talented laboratory worker, he had spent time at the Marine Biological Laboratory at Woods Hole, Massachusetts, had worked with Jacques Loeb and T.H. Morgan, and had spent the previous decade at Washington University in St. Louis, Missouri pioneering x-ray diffraction analysis, a technique that would become fundamental for the emerging science of molecular biology. Schmitt’s mandate was clear: retool MIT’s biological research in a way that would combine biology with physics, mathematics and chemistry. Schmitt’s success in this field made him one of the primary figures in the consolidation of molecular biology and biophysics by the 1950s.6

Inspired by the elegant molecular model of DNA announced by Watson and Crick in 1953, and by his own understanding of the ‘Unity of Science’ movement postulated by the logical

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positivists of the Vienna Circle, Schmitt began to organize a “Mind-Brain” research group that could develop a unified approach to understanding the workings of the brain. According to Judith Swazey, a historian and participant in Schmitt’s efforts, “if any one man can be credited with the concept and genesis of neuroscience, a field uniting historically disparate disciplines concerned with brain structure and function and behavior, it is Francis O. Schmitt.” Swazey went on to say: “Prompted by advances in molecular biology, ideas about a multidisciplinary attack on brain research that focused on biochemical and biophysical aspects had begun to germinate in Schmitt’s mind by the fall of 1958.” The theoretical orientation was resolutely reductionistic, and in Schmitt’s words, aimed “to investigate the ‘wet and dry’ biophysics of central nervous system function…by effective utilization of the biophysical and biochemical sciences.” The product would be a new ‘mental biophysics,’ a name Schmitt later changed to ‘neuroscience.’

By 1962, the “Mind-Brain” group and its associates grew into the Neurosciences Research Program (NRP). The NRP, which established itself at Brandegee Estate in Brookline, Massachusetts, began accruing members and holding tutorials and work sessions, publishing bulletins, and arranging conferences. In 1969, the NRP published a 900-page tome entitled The Neurosciences: A Study Program, marking the first use of the term ‘neuroscience’ in a major publication. The Society for Neuroscience was also founded in 1969, giving the new field a professional organization. By the 1970s, the Society for Neuroscience had several thousand members, and the

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8 Swazey, 530.
9 Schmitt quoted in Swazey, 532.
10 Swazey, 530–32.
NRP was a going concern.¹¹ Neuroscience, it would seem, had emerged from the ‘big science’ of post-war America, right at the very heart of the military-industrial-academic complex.¹²

Modern historians of science, suspicious as they often are of ‘great man’ stories, have of course pushed back against this narrative, looking to emphasize larger historical forces which may have led to the rise of neuroscience. To the extent that these efforts have been successful, they have typically focused on the role of new technologies in the post-war environment. As Anne Harrington has put it, “in the postwar era, technological innovation would soon drive research at least as much as theoretical preoccupation.”¹³ The development of the microelectrode, Seymour Kety’s use of nitrous oxide to track changes in cerebral blood flow, and the emergence of positron emission topography (PET) and functional magnetic resonance imaging (fMRI) all created conditions of possibility whereby modern biology might finally make significant progress towards understanding the relationship between the mind and the brain.¹⁴ This historical narrative portrays the emergence of neuroscience as the product of a series of disconnected scientific developments, driven forward by a technological imperative.¹⁵ This perspective correlates nicely with the changes in scientific

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¹⁴ Harrington, 521–23.

¹⁵ A similar argument about the rise of the ‘neuroscience’ in the 1950s can be found in the work of Gordon Shepherd, who rightly points to that decade as key in the emergence of a recognizably
organization brought about by interdisciplinary ‘big science,’ and the rise of large-scale national laboratories, lending plausibility to the argument that neuroscience was simply another form of post-war techno-science.\(^{16}\) Physics and chemistry, having unraveled the power of the atom and the physical basis of life, would now turn their attention to the brain and mind, and would complete the unification of natural knowledge along reductionistic lines.

A closer look at the historical record, however, simply does not support either of the above interpretations. Modern neuroscience began not with grand philosophical agendas for the unification of science (or with any particular technology) but with a hospital. The improvised interdisciplinary environment created within the walls of the Montreal Neurological Institute constitutes the key site for the foundation of modern neuroscience. Between the time of its founding in 1934 and the breakup of its key players in 1965, the MNI was the location for interdisciplinary scientific research on the human brain, primarily because of the way that it brought together the clinic and the laboratory, and scientists and doctors from different national and disciplinary traditions. This unique interdisciplinary environment was the product both of the scientific biography of the MNI’s founder, Wilder Penfield, and of the position of Montreal as a liminal space where scientists from different areas and nations could collaborate to solve specific clinical problems. Having forged a new perspective on the mind and brain, participants of the MNI diffused outward to spread their new interdisciplinary ethos to the rest of the world’s brain researchers. Neither the inevitable outcome of the development of the brain and mind sciences, nor the product of a post-war technocratic elite,

interdisciplinary neuroscience instead arose because of the actions of a relatively small group of historical actors whose lives (and their associated national scientific styles, tools and backgrounds) became connected at a specific historical site.

A brief overview of this dissertation’s narrative will be useful here. This dissertation argues that, contra the existing historiography, modern neuroscience developed first not at MIT, but rather amid the interdisciplinary clinical environment created at the Montreal Neurological Institute. The MNI was founded and developed by the neurosurgeon Wilder Penfield, who hoped to bring the insights of laboratory physiology, and particularly the microscopic study of tissues and cells (histology and cytology), to bear on surgical problems in order to advance the therapeutic value of neurosurgery, and the professional position of the neurosurgeon. With support from the Rockefeller Foundation, Penfield opened his own clinical research institute in Montreal in 1934, specializing in the surgical treatment of epilepsy. Penfield’s operative techniques were daring, involving electrical stimulation of the exposed brain of conscious patients, and the radical removal of brain tissue. Penfield initially hoped to combine neurosurgery with clinical neurology and laboratory neuropathology in a grand attempt to master all of the disciplines that he felt were germane to the study of the brain. However, his radical surgeries soon led him to embrace other disciplines and scientific fields that could aid him in his professional mission. Curiosity about the mechanisms of consciousness that his operations were revealing, and a concern for the possible cognitive deficits brought about by his radical operations, led him to ‘reach out’ to neighboring disciplines, first by hiring a pair of psychologists, D.O. Hebb and Molly Harrower, in 1937. This collaboration inaugurated a tradition of interdisciplinary research fellows at the MNI (one that continued in the field of psychology with the hiring of Brenda Milner, whose work on memory deficits was foundational for the field of cognitive neuroscience).
Penfield continued to reach out to other areas of knowledge in an effort to expand the research base of the MNI. His most successful collaboration would be with Herbert Jasper, an American physiologist, psychologist and electroencephalographer. Jasper, long recognized as a master of the new technology of EEG, was an equally masterful laboratory physiologist and scientific organizer. Jasper’s efforts to organize the emerging community of electroencephalographers in the 1940s and 1950s led, in 1960, to the founding of the International Brain Research Organization (IBRO) following a conference in Moscow. Modeled on the interdisciplinary ethos of the MNI, the IBRO was recognized by many of the world’s neuroscientists as the field’s foundational organization. Remarkably, Jasper’s efforts to organize the IBRO occurred in almost total isolation from F.O. Schmitt and his own efforts with the NRP; the IBRO constituted a wholly separate origin point for interdisciplinary neuroscience, and one that was far more influential.

Finally, while the MNI constituted an active, vibrant scientific community, in which psychologists, physiologists, chemists and other scientists found a home, the community conspicuously did not include psychiatrists. Psychiatry, which had been a part of Penfield’s initial vision for interdisciplinary collaboration, was never able to fully integrate into the emerging neuroscience of the MNI, largely because of the disastrous relationship between Penfield and the psychiatrist Ewen Cameron, who briefly attempted to inaugurate a program in psychosurgery at the institute. The story of their failed collaboration, and Cameron’s later estrangement from the MNI community, and his subsequent turn to the ethically questionable practice of ‘psychic driving,’ can be used to place the more successful form of interdisciplinary collaboration at the MNI in greater relief.

**Group Biography, Institutions and Interdisciplinary Science**
In order to understand the historical emergence of a new interdisciplinary science, we need to examine the lives of its creators, and the institutions and settings that brought these actors together; thus, the primary method of this dissertation will be a group biography approach that examines how the participants of the MNI developed in their separate scientific and intellectual milieux, and how they combined their disciplinary approaches. By tracing the interactions of these actors, we not only gain a deeper understanding of the historical forces that brought modern neuroscience into existence, but we also gain a new perspective on the development of interdisciplinary science. While interdisciplinary science has been a subject of fascination for scholars and scientists of all stripes, detailed studies of the emergence of new interdisciplinary fields are surprisingly rare.\textsuperscript{17} The work of Peter Galison has proven exceptionally influential here. By analyzing the historical circumstances that allowed for the trading of tools, techniques, and theories across the diversity of actors within ‘big science,’ Galison has supplied a useful model for how to think about the creation of new scientific fields, and about how they relate to one another. Borrowing from the field of linguistic anthropology, Galison suggests that, by collaborating on projects in specific ‘trading zones,’ scientists from different disciplines can hammer out coordinated trading languages that allow for local cooperation, even when they disagree about the global meaning of their work. In time, these ‘trading languages’ can become robust enough to blossom into entirely new fields of inquiry. Galison’s emphasis on the importance of material culture and locally coordinated activity has the advantage of making his enterprise thoroughly historical; the analysis of trading between

different subcultures takes place under particular historical circumstances, and at a particular moment in time.¹⁸

While this dissertation makes use of Galison’s concept of a trading zone, the narrative approach to this study, with one exception, is in the form of group biography.¹⁹ The goal is not to merely shift the history of neuroscience from a ‘great man’ approach to one that emphasizes ‘great people’ or a ‘great institution.’ By tracking the lives of the different actors that created the MNI and passed through its halls, we can gain an appreciation for how their scientific biographies brought them into a position to trade in the first place. In his study of the development of the Cambridge school of physiology, Gerald Geison notes the importance of understanding the role of individuals in the development of science, adding that “neither scientific ideas nor scientific institutions float in ethereal isolation from the men and women who give them life.”²⁰ Paradoxically, this had the effect, for Geison, of reinforcing his view of the central importance of the school’s founder, Michael Foster. Indeed, for Geison, Foster proved Ralph Waldo Emerson’s dictum that “an institution is the lengthened shadow of one man.”²¹ In that spirit, it would be impossible to deny the central importance of Wilder Penfield in understanding how the MNI became an incubator for interdisciplinary neuroscience. Yet it is my intention, by examining the lives and contributions of other key actors in the MNI story, to move beyond a hagiographical account of Penfield’s

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¹⁹ This exception comes in Chapter 4, in which I examine the interactions of a number of actors in great detail surrounding one particular issue, psychosurgery.
²¹ Quoted in Geison, xiii.
contribution to neuroscience, and instead understand the role of his institute in bringing together different scientific and medical traditions in new and fertile ways.\textsuperscript{22}

An emphasis on following the lives of the MNI’s key players can also help us to understand how an institution can shape the development of a scientific field more generally, by bringing different scientific practices and traditions into contact with each other. Here, I would like to add a new analytic metaphor to complement Galison’s ‘trading zone,’ one drawn from a participant of the MNI during its heyday, and which gives this dissertation its title. During his time at the MNI, the psychologist D.O. Hebb began the work that would lead to his theory of ‘cell-assemblies.’ Expressed in his 1949 masterwork,\textit{The Organization of Behavior}, Hebb’s theory began with a hypothetical model of how neurons might become associated over time as a result of simultaneous activity. Expressed as a physiological postulate, Hebb’s theory stated that “When an axon of cell A is near enough to excite a cell B and, repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells such that A’s efficiency, as one of the cells firing B, is increased.”\textsuperscript{23} These learned associations of interacting cells might, in Hebb’s estimation, form the physiological basis of such mental phenomena as concepts, ideas and thoughts. A foundational concept for the modern study of neural networks, Hebb’s postulate is often recalled today by the

\textsuperscript{22} Hagiographies of Penfield do exist. To date, the most extended discussion of the MNI and its role in inaugurating neuroscience is the work of his friend and former colleague William Feindel. While Feindel’s book is a fine reference work, it is historically unsophisticated. The biography of Penfield written by his grandson, Jefferson Lewis, is insightful, but hagiographic, and elides the important role played by Penfield’s collaborators. Ironically, while Penfield’s own autobiography is bombastic in tone, it gestures to his dependence on others, both in its title (\textit{No Man Alone}), and throughout the text. William Feindel and Richard Leblanc, \textit{The Wounded Brain Healed: The Golden Age of the Montreal Neurological Institute, 1934-1993} (Montreal: McGill-Queen’s University Press, 2016); Wilder Penfield, \textit{No Man Alone: A Neurosurgeon’s Life}, 1st ed (Boston: Little, Brown, 1977); J. Lewis, \textit{Something Hidden: A Biography of Wilder Penfield} (Formac Publishing Company Limited, 1983).

The colloquial expression “neurons that fire together, wire together.”

The participants of the Montreal Neurological Institute became ‘wired together’ in a fashion similar to the neurons of Hebb’s cell assemblies; through persistent contact with each other, as they ‘fired’ in sync over just the right combination of circumstances, they altered and were altered by each other’s disciplinary, national and technical traditions, producing an amalgam approach that was entirely novel.

Thinking of historical actors as part of interacting assemblies also provides a way of understanding how the vision of interdisciplinarity developed at the MNI spread, how it was enriched by outside sources, and for understanding instances in which it failed. When acting in concert, the members of the Montreal neuroscience community formed strong assemblies that were capable of effectively employing their varied skills for a common purpose. This is seen most clearly in the collaborations between Penfield and the psychologists who joined him, and between Penfield and Herbert Jasper. Yet because these actors were semi-autonomous, they could also spread their style of work through weaker connections that went beyond the MNI, and could enrich their own assemblies through weak connections to other disciplines and national traditions. This is seen most clearly in Brenda Milner’s work on the brain structures of memory, which helped launch the broader field of cognitive neuroscience, and in Herbert Jasper’s connections to the world-wide community of neurological researchers, a connection that allowed him to organize those researchers into the IBRO, an organization that could then spread the MNI’s style of neuroscience around the globe.

Thinking about how assemblies of historical actors worked together can also point us to important instances of failed collaboration, as in the disastrous relationship between Penfield and the psychiatrist Ewen Cameron, so that we can examine instances in which productive assemblies failed to form. Tracing the intersecting biographies of those that worked at the MNI, then, becomes a way

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24 Although often attribute to Hebb, this particular version of the rule was first coined by Siegrid Löwel and Wolf Singer, “Selection of Intrinsic Horizontal Connections in the Visual Cortex by Correlated Neuronal Activity,” *Science* 255, no. 5041 (1992): 209–12.
of understanding how their interactions added up to a vision of neuroscience that was more than the sum of its parts – one that combined tools, techniques, and national scientific traditions in a cosmopolitan setting.  

**Styles of Neuroscience**

Along with the primary argument of this dissertation (that the MNI constituted the most important site for the emergence of interdisciplinary neuroscience), I will also advance a secondary, related argument: the vision of neuroscience that emerged from the MNI was of a qualitatively different style than the one developed at MIT. This was for two reasons. First, the interdisciplinary

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25 At this point, astute readers will no doubt see parallels between my own notion of wired together ‘assemblies’ of historical actors, and the ‘assemblages’ of the Actor-Network Theory of Bruno Latour, Michel Callon and John Law. In *Reassembling the Social*, Latour argues that the job of the sociologist should not be the study of ill-defined social ‘things’ (as distinct from biological, psychological or physical things), but rather should be one of tracing the social connections between actors, in such a way as to demonstrate how these interactions lead up to larger social change. Of particular relevance for my own project is Latour’s notion of ‘localizing the global’ – that is to say, examining the local sites in which larger global trends interact. I see the MNI as just such a site, and as such, I see my own study as an example of how a historian might ‘reassemble’ the social. That being said, I emphasize, much more than Latour, the importance of biography. Further, in thinking about the social element in the creation of ‘schools’ of research and the dissemination of their work, I have found enormously helpful the insights of the sociologist Mark Granovetter, whose classic article, “The Strength of Weak Ties,” helped me to understand how a group of scientists and doctors could function as a collective unit, yet still influence outsiders to a greater degree than if they had been a closed, self-referential school. For a brief discussion of these issues, as well as the relative strengths of biography, group biography and prosopography, see Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Philadelphia: Open University Press, 1987); Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network Theory* (Oxford: OUP Oxford, 2005); Mark S. Granovetter, “The Strength of Weak Ties,” *American Journal of Sociology*, 1973, 1360–1380; Anne Harrington, *Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler* (Princeton University Press, 1999), xxiii–xxiv; Steven Shapin and Arnold Thackray, “Prosopography as a Research Tool in History of Science: The British Scientific Community 1700–1900,” *History of Science* 12, no. 1 (1974): 1–28.

26 The concept of ‘styles’ in science has undergone a series of transformations in twentieth-century historiography, and a complete bibliography on the issue would be unwieldy. The two names most commonly associated with this discussion are Ludwik Fleck, who developed the concept of a *Denkstil*, or ‘thought style,’ in his *Genesis and Development of a Scientific Fact* (1935), and Ian Hacking, who pursued the concept of ‘styles of scientific reasoning,’ most notably in his 1992 article “‘Style’ for historians and philosophers.” For Fleck, the concept of style referred to a semi-closed, self-
interactions at the MNI crucially involved the clinic as well as the laboratory. Because of this, the MNI produced deep collaborations over clinical puzzles and problems; the MNI surgeons and scientists could not afford to engage in idle speculation – they needed results. The NRP’s form of neuroscience, by contrast, because it eschewed the clinic, collaborated more by speculative conversation, using the primary scientific ‘language’ of molecular biology. Second, the MNI was profoundly internationalist in orientation. While the community of scientists that assembled at MIT were drawn from multiple areas of the United States, those that assembled at Montreal were drawn

referential way of thinking that was characteristic of groups of scientists, and was maintained by a ‘thought collective.’ The thought collective, a collection of individuals, developed a kind of ‘group think’ that conditioned perception of scientific facts, and controlled what sort of explanations were acceptable at any given moment. “The individual within the collective is never, or hardly ever, conscious of the prevailing thought style, which almost always exerts an absolutely compulsive force up his thinking and with which it is not possible to be at variance.” For Hacking, styles of scientific reasoning were larger in scope, and encompassed such innovations as experimentation or taxonomy or statistical analysis. Although thoroughly historical (different styles arose at different times, and under different conditions), Hacking’s ‘styles’ were seldom the product a specific event or place.

While my use of style is informed by both of these authors, I find most amicable Gerald Geison’s use of the concept of style, particularly his discussion of ‘national styles’ as rendered in his study of the development of Michael Foster’s school of physiology in the late-nineteenth century. Geison argued that the resulting ‘Cambridge school’ of physiology displayed a distinctive English national style because of its use of certain Darwinian concepts - concepts which were unpopular in the continental physiology of France and Germany. However, in contrast to Fleck and Hacking, Geison shows how this national style crystallized around a specific laboratory problem, the myogenic origin of the heartbeat. For Geison, the laboratory problem of the heartbeat best reveals the overriding national styles of English and Continental physiology; one was successful in its resolution of the problem of the heartbeat because of the influence of evolutionary theory, and the other failed to successfully resolve the problem because it ignored Darwin and his insights. As will be seen in the chapters that follow, I see a similar comparison to be made between the Montreal and MIT styles of neuroscience, styles that are most clearly revealed in their different approaches to the problem of memory. Moreover, I see in the Montreal school an instance to examine a space in which different national styles combined and transformed into a novel, local style. For more on the issue of ‘styles’ in the history of science, see Ludwik Fleck, *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1935); Ian Hacking, “Style’ for Historians and Philosophers,” *Studies in History and Philosophy of Science Part A* 23, no. 1 (1992): 1–20; Lorraine Daston and Michael Otte, “Introduction,” *Science in Context* 4, no. 02 (1991): 223–32; Anna Wessely, “Transposing ‘Style’ from the History of Art to the History of Science,” *Science in Context* 4, no. 02 (1991): 265–78; Anne Harrington, “Interwar ‘German’ Psychobiology: Between Nationalism and the Irrational,” *Science in Context* 4, no. 02 (1991): 429–47; Jane Maienschein, “Epistemic Styles in German and American Embryology,” *Science in Context* 4, no. 02 (1991): 407–27; Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society.*
from around the globe, and this intermingling of nationalities made the MNI a fertile space for collaboration and the mixing of different national research traditions and perspectives.

The most notable difference between the Penfield’s MNI and Schmitt’s “Mind-Brain” group was the inclusion of the clinic. While the NRP did not include clinical practitioners among its initial circle of participants, the MNI was explicitly built as a neurological clinic that was, in the words of its founder, “dedicated to relief of sickness and pain and to the study of neurology.” By tightening the feedback loop between the neurosurgical clinic and the laboratory, Penfield not only brought the power of laboratory science to bear on surgical practice, but also turned his surgical theater into a kind of laboratory. This crucial feedback loop between the clinic and the lab meant that the MNI participants were able to benefit from clinical knowledge as well as laboratory experiment, and could pass material back and forth.

The second notable difference between the MNI and the NRP was the national makeup of their respective participants. Echoing the international scientific education of its founder, the MNI became a place where internationality and interdisciplinarity were related scientific virtues. In the cosmopolitan, bilingual city of Montreal, national scientific traditions could interact and transform one another. In addition to bringing scientists with different perspectives together, this internationalism prevented the MNI participants from becoming beholden to a number of scientific

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trends that were becoming hegemonic in the United States, such as cybernetics and molecular biology. This made the MNI’s style of neuroscience a striking contrast to that of MIT; more holistic than reductionist, more medical than philosophical, and more pragmatic and applied than theoretical and basic. It was around particular clinical problems that the MNI approach crystallized, and it was around these clinical issues that the national styles of its different members evolved into something entirely new.

The chapters that follow begin with an examination of the life of the MNI’s founder, Wilder Penfield, and then branch out to understand how Penfield successfully (or unsuccessfully) incorporated new scientific fields into his institute. In *Chapter 1 – A Neurological Surgeon: Wilder Penfield’s Interdisciplinary and International Project in Neurosurgery*, I will examine the origins of Penfield’s vision for an interdisciplinary neurological institute that could combine the power of the lab with the work of the clinic. The MNI was the product not only of the Rockefeller Foundation’s push for an interdisciplinary ‘psychobiology,’ but also Penfield’s own experiences with European research traditions: the laboratory-based neurophysiology of England, the neurohistology of Spain, and the surgical practices of Germany. Penfield’s experience in Charles Sherrington’s physiology lab convinced him that to advance the professional status of neurosurgery, he would have to incorporate the insights and tools of laboratory physiology. This collaboration with Sherrington,

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and an intense desire to expand the scope of neurosurgery, led Penfield more and more toward the microscopic study of the cell, which eventually brought him to the histological laboratories of Spain. Penfield became the first English-speaker to work in the lab of the Spanish neurohistologist Pio del Rio-Hortega, and their early collaboration convinced Penfield that laboratory techniques could help him to understand (and surgically treat) epilepsy. The Spanish episode was equally important for Penfield’s sense of scientific internationalism. Penfield bemoaned the sad fate of Spanish histology - locked away behind the barrier of language, and occasionally shunned because of anti-Spanish bigotry among other European nations. In addition to acquiring the new skill of ‘Spanish’ staining methods, Penfield took from his time in Madrid a profound belief in the value of scientific internationalism.

Penfield’s notion for combining the work of the neuropathologist and the neurosurgeon in one location made him a prime candidate for Rockefeller Foundation funding. By combining laboratory investigation with the surgical innovations of Otfrid Foerester following a brief sojourn to Germany, Penfield hoped to build an institute for neurological research that could bring the power of the laboratory to bear on surgical problems. However, and crucially, the increasingly collaborative nature of his enterprise meant that his new institute had to be built in an environment where he could realize his professional vision, unmolested by competing professionals. For Penfield, Montreal became a prime site for his new vision of an interdisciplinary neurological clinic, both because it lacked any competing neurosurgeons (thus allowing unfettered access to clinical material for study) and because, as a city, Montreal was both connected to, and separate from, the differing

medical and scientific traditions of America and Europe. This made Montreal a perfect site for Penfield to imitate the lessons of scientific organization he learned in Spain, bringing different scientific workers together under one roof, and with a common sense of purpose.

Chapter 2 – “The Montreal Method: Epilepsy Surgery, Neuropsychology and the Birth of Cognitive Neuroscience” tells the story of a triumvirate of psychologists who joined Penfield at the MNI beginning in 1937. Unlike many neurosurgeons of his era, Penfield had a strong interest in psychological issues. The reasons for this were twofold. First, Penfield had imbibed from Charles Sherrington an abiding belief that neurophysiological investigation could unlock the secrets of the human mind. On a more practical level, Penfield’s radical surgery for epilepsy began to create problems. A growing concern that his extensive removal of brain tissue might be creating cognitive disturbances in his patients led Penfield to search for a psychologist who could bring greater clarity to the effects of his surgeries. Penfield realized that he could not embody all of the appropriate disciplines for the investigation of the human brain, and reached out to psychologists for help.

Donald Hebb, the psychologist whom Penfield recruited, was an ideal candidate for the job. Trained in the physiological psychology of his mentor Karl Lashley, Hebb and Penfield would collaborate on a series of studies that documented the effects on personality and intelligence of frontal lobe operations (this collaboration was launched because of the dramatic effects of Penfield’s operation on his sister’s brain tumor in 1928). Hebb incorporated the results of his work with Penfield into a neuropsychological theory of learning and memory that formed one of the first challenges to the post-war dominance of behaviorism in American psychology.

Despite its success, Hebb and Penfield’s collaboration on the effects of frontal lobe surgery was patchy and uneven. Penfield, who had functioned for years as his own psychologist, gradually accepted the value of psychological expertise thanks largely to the presence of Molly Harrower, whose Rorschach and Gestalt-based testing of epileptic patients created a professional space for a
neuropsychologist at the MNI - a role that would later be filled by the young English psychologist Brenda Milner.

Milner, whose professional history neatly illustrates the combination of national scientific styles (in this case American and British) at the heart of this dissertation, arrived at the MNI as the result of a long-standing arrangement between Hebb and Penfield. A more proximate cause, however, was a series of memory problems that had begun to surface amongst Penfield’s surgical patients. Milner’s investigation of these memory problems, with the tests and methods of Hebb, paved the way for her later work with the famous patient H.M. The approach to memory research that characterized Milner’s work, with its emphasis on fine brain anatomy and interacting systems, stood in stark contrast to the later theories of memory emerging from MIT, whose participants hoped to find a ‘memory molecule’ analogous to DNA. Moreover, the development of neuropsychology and cognitive neuroscience illustrates the improvised and pragmatic nature of the neuroscience developing at the MNI; while all of the participants were able to collaborate effectively, they often did so despite theoretical differences.

Chapter 3 - “From Mind to Brain: Herbert Jasper, the EEG and the International Brain Research Organization,” tells the story of Herbert Jasper, the physiologist, psychologist and electronencephalographer who not only brought a crucial new technology to bear at the MNI - the electroencephalograph - but also brought the MNI’s interdisciplinary scientific community to its greatest level of integration, and helped spread this emerging form of neuroscience to the rest of the international community. Jasper began his career not in physiology, but rather in theology and philosophy. He embraced experimental psychology as a way of posing deeper questions about the nature of the mind, a path that ultimately lead him to neurophysiology and the EEG. Indeed, the prospect of collaborating with psychologists such as Hebb and Harrower drew Jasper to the MNI in 1937, when Penfield recruited him to apply his expertise with the EEG to the problem of localizing
epileptogenic scar tissue. Jasper and Penfield’s collaboration eventually inaugurated a new era of cortical localization research, and added a new discipline to the MNI’s burgeoning community. Jasper also spearheaded the incorporation of new laboratory actors and scientific disciplines within the MNI; most notably, his collaboration with the South African chemist K.A.C. Elliot not only discovered the inhibitory properties of the neurotransmitter gamma aminobutyric acid (GABA), but also inaugurated the hybrid field of neurochemistry.

Jasper’s leadership role within the developing EEG community ultimately led to the creation of the International Brain Research Organization (initially named, tellingly, the Interdisciplinary Brain Research Organization) following a conference in Moscow in 1958. It is noteworthy that the Moscow conference, on the electrophysiological investigation of conditioning and learning, was one that Jasper was uniquely suited to participate in because of his work at the MNI; since the 1940s, much of Jasper’s experimental work, following Hebb’s theories of cell assemblies, involved the use of the EEG and the microelectrode to investigate learning at a cellular level. Jasper and others would later comment that the Moscow conference was the origin point for ‘neuroscience’ globally, and that the IBRO (modeled on the MNI and incorporated in Ottawa in 1960) was the most important leadership organization in that area. Additionally, archival documents suggest that the development of the IBRO took place largely in parallel with, and unbeknownst to, the Neuroscience Research Program at MIT.

Finally, a comparison of the research priorities of Schmitt and Jasper’s groups, particularly in the area of memory, will serve to highlight the competing visions of neuroscience that had developed by 1960. Whereas Schmitt’s group, committed to a reductionist molecular framework, searched fruitlessly for a ‘memory molecule’ until the 1980s, the Jasper-led labs at McGill conducted more successful research on conditioning, learning and localization.
While neuropsychology, electrophysiology and neurochemistry were successfully incorporated into the growing community of researchers in Montreal by 1960, one group remained on the outside: psychiatrists. In 1963, Jasper gave a talk at Montreal’s largest psychiatric hospital, the Allen Memorial Institute, entitled “Psychiatry and Neurology: Two Solitudes?” in which he noted that, while the MNI had initially been founded with the goal of integrating neurology, neurosurgery and psychiatry, by 1963 the relationship between the city’s psychiatrists and the neurological institute had completely broken down. Chapter 4 - “Two Solitudes: Psychosurgery and the Troubled Relationship between Wilder Penfield and Ewen Cameron,” examines the historical factors that led to this breakdown. The chief reason for this broken relationship was the failed collaboration between Penfield and the controversial psychiatrist Ewen Cameron, particularly over the issue of psychosurgery and lobotomy. While hagiographic accounts have suggested that Penfield always opposed psychosurgery, a closer look at the archival record reveals an episode in which he and Cameron (whom Penfield was instrumental in bringing to Montreal), briefly collaborated on a small series of experimental psychosurgeries. The fallout from this disastrous collaboration doomed the relationship between the MNI and psychiatry, isolating Cameron and his AMI from interaction with other medical and scientific groups in Montreal, and leaving him free to perform ethically and scientifically questionable experiments. An examination of the relationship between the MNI and AMI reveals two completely different modes of interdisciplinarity – one based on deep collaboration, the other shallow, opportunistic and ethically fraught.

By the 1960s a fully formed ‘neuroscience’ had developed in Montreal, one independent and qualitatively different from that of Schmitt’s NRP. In the Conclusion – Wired Together: Reassembling Montreal Neuroscience, I trace the ways in which the perspective of the Montreal group survived, transformed, and perished as the molecular neuroscience of MIT became increasingly dominant.
The retirement of Penfield in 1960, and the departure of Herbert Jasper for the French-speaking Université de Montreal in 1965, fractured the Montreal group. This fracture coincided with the growing political instability of Quebec itself, as French-Canadian separatism became a potent political force in the 1960s. Simultaneously, changes in medical practice in the province of Quebec undermined the scientific mission of the MNI, as it was placed under greater pressure to take on more patients and spend less time on research. Despite its decline in prominence, however, the fingerprints of the MNI’s brand of neuroscience are traceable in some of the most important areas of modern brain research. In order to understand both how the MNI was able to flourish, and how its form of neuroscience continued to survive, I compare my own notion of assemblies of historical actors to the insights of the sociologist Mark Granovetter and his notion of the ‘strength of weak ties.’ Granovetter argued that communities that were strongly linked together were paradoxically less able to connect with other groups in order to diffuse their innovations. By contrast, communities with extensive weak ties were more able to interact with other groups, which both prevented them from becoming too insular and inward looking, while simultaneously allowing their innovations to affect larger macrosocial developments. While the MNI developed strong assemblies of actors in its clinical and laboratory collaborations, it was because of its weak ties that it remained a vibrant research site, and was able to affect the larger development of modern neuroscience. I briefly conclude with a discussion of the career of David Hubel, whose receipt of the Nobel Prize for Physiology and Medicine can be traced back to a collaboration undertaken at the MNI. This story is indicative of how, by examining the assemblies of people that the MNI created, we can understand the development of neuroscience as a phenomenon that was at once scientific, technological, and profoundly social. An examination of the history of the MNI can also help us to critically reflect on the meaning of ‘interdisciplinary science’ in the modern world.
Chapter 1 – A Neurological Surgeon: Wilder Penfield’s Interdisciplinary and International Project in Neurosurgery

In a 1907 issue of The Lancet, William Osler commented on the slowly-emerging specialty of neurosurgery. Osler’s contemporary and friend, the English surgeon Victor Horsley, had recently argued that it was a mistake to only pursue surgical solutions to diseases of the central nervous system as a last resort. Brain surgery was, despite its risks, often preferable to more drawn-out and ineffective pharmacological treatments, which were the monopoly of physicians. According to one observer, Osler noted that “A great deal of [the] skepticism in regard to the value of operation in cases of cerebral tumor was the result of the bicipital [two-headed] condition of neurology.” Osler went on to:

depreciate operation in these cases at the hand of general surgeons and would prefer to see neurology [become] a special department, so that there would not be neurological physicians and surgeons, but medico-chirurgical neurologists properly trained in the anatomical, physiological and surgical aspects of the subject.\(^1\)

For Osler - who had himself published over 200 articles on neurological topics, attended the lectures of Jean-Martin Charcot in Paris, and honed his skills as a neuropathologist over the course of hundreds of autopsies - the Janus-faced separation of physician and surgeon was not appropriate for illness of the nervous system. What was needed, if medicine was to make any headway on these diseases, was a new kind of surgeon-physician who could conduct neurological surgery in accordance with the knowledge then emanating from the physiological laboratories of the world’s research universities.\(^2\) What was needed, in short, was a not just a ‘neurosurgeon’ (one who specialized in surgery of the nervous system), but a neurosurgical surgeon, who combined surgical


intervention with an intimate knowledge of the physiology of the brain and nervous system, the traditional bailiwick of the neurologist.

Osler had good reason to think that new knowledge of the nervous system might lead to radical new therapies. Only a year earlier, the Spanish microscopist Santiago Ramon y Cajal accepted the Nobel Prize for his convincing demonstration that the cells of the nervous system were separate, rather than linked. Nerve cells, then, were just like other cells in the body, and conformed to the cell theory of Theodor Schwann, Matthias Schleiden and Rudolph Virchow. That same year, the English physiologist Charles Sherrington published his monumental *Integrative Action of the Nervous System* (1906), in which he “almost single handedly crystallized the special field of neurophysiology.” From many corners, patient laboratory work seemed to be elucidating important new knowledge of the most complicated of human organs, and Osler was convinced that the surgeon who could combine laboratory and clinical knowledge could also unite the fields of clinical neurology and neurosurgery.

This chapter is about a particular surgeon, Wilder Penfield, who answered Osler’s call. Although Osler served as a mentor to many surgeons and physicians who would transform their respective fields (notably Harvey Cushing, who professionalized neurosurgery in the United States), none equaled Penfield in his enthusiasm for laboratory science, his clinical acumen, or his ability to integrate the two. Remembered as a towering figure in the history of brain surgery, primarily for his

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pioneering surgical treatment of epilepsy and his dramatic operations on conscious patients who could report their experience of electrical brain stimulation, Penfield is less well-remembered today for his efforts to integrate laboratory research with clinical neurology and neurosurgery. However, this dissertation will argue that one of Penfield’s most important legacies was to bring together the laboratory and the surgical clinic under one roof, and in such a way as to lay the groundwork for the emergence of the interdisciplinary field of neuroscience in the twentieth century.

The intention of this chapter, however, is not to replicate a hagiographic account of Penfield’s life, nor to argue that neuroscience sprang fully-formed from Penfield’s mind. Rather, a scientific biography of Penfield can illuminate the ways in which this actor interacted with, assimilated, and creatively recombined some of the crucial developments of early-twentieth century science and medicine. In particular, an examination of his experimental and laboratory work, and critically, his time in the laboratory of the Spanish neurohistologist Pio del Rio-Hortega, will reveal how Penfield attempted to answer Osler’s call for a physiological surgeon by demonstrating the value of laboratory work for surgical intervention. By tightening the feedback loop between the surgeon’s

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6 A brief word should be said here about historical source material, and its relation to previous biographies of Penfield. Penfield wrote an autobiography, published posthumously after his death in 1976. However, this autobiography only covered the period from his childhood until the founding of the Montreal Neurological Institute in 1934 (he described the book as “the biography of an idea” for the MNI). Penfield’s grandson, Jefferson Lewis, composed an insightful (if hagiographic) biography of Penfield that covered his entire life, but was written for a popular press, and thus unreferenced. Both books depended crucially upon Penfield’s diaries, and his extensive correspondence with his mother, as source material. Following Penfield’s death, these diaries and letters were made the property of his literary executor and surgical protégé, William Feindel, who intended to use them for his own book about Penfield and the MNI. Feindel never completed his book, but early drafts were later combined with new writing by Richard LeBlanc into the 2016 volume *The Wounded Brain Healed*. Feindel kept the diaries and letters from public view, and they only became available for researchers following Feindel’s own death in 2014. The letters between Penfield and his mother constitute a rich source base for historical writing, and will be referred to extensively in this chapter, particularly when they contradict or expand upon Penfield’s own comments in *No Man Alone*. For more on Penfield, see Wilder Penfield, *No Man Alone: A Neurosurgeon’s Life*, 1st ed (Boston: Little, Brown, 1977); J. Lewis, *Something Hidden: A Biography of Wilder Penfield* (Formac Publishing Company Limited, 1983); William Feindel and Richard Leblanc, *The Wounded Brain Healed: The Golden Age of the Montreal Neurological Institute, 1934-1993* (Montreal: McGill-Queen’s University Press, 2016).
scalpel and the pathologist’s microscope, Penfield hoped to transform the profession of neurosurgery by linking it to the growing prestige of laboratory science, while simultaneously providing theoretical justification for new surgical practices.

Moreover, his time in Spain (often overlooked in historical writing about Penfield), formed his attitude toward collaboration and internationalism in science. Penfield was, in fact, the first English-speaking scientist to spend time in the Spanish labs of Rio-Hortega, and he was deeply impressed both by the collaborative environment of the laboratory, and its isolation from the rest of the scientific world. Penfield abhorred the nationalistic chauvinism that came to characterize many scientific communities in the years between the World Wars, and his success in traversing national boundaries in search of good science and medicine led him to embrace a vision of collaboration in which interdisciplinarity was linked with internationalism; to combine the disciplines of the brain sciences, one had to master the languages and styles of the world’s clinics and laboratories. Yet, when faced with the practicalities of putting his plan into action, this led to a paradox. As much as he desired to create an international clinic, he also embraced a romantic view of the isolation of the Spanish labs. In order to enact the international, he would have to embrace the local, and build up his interdisciplinary clinic in a space that was connected to, but separate from, the major metropolitan hubs of neurological science. Montreal offered an ideal venue – one that was connected to the neurological centers of the world, yet with minimal competition from an existing neurological establishment. In this unique setting, Penfield could recombine the neurological sciences of the world to create something entirely new.

7 I see Penfield’s vision of the MNI as a local/international institute, one located at a specific place that it aims to transform through global connections, as an instance of what Bruno Latour has called ‘localizing the global.’ According to Latour, “no place can be said to be bigger [i.e. more global] than any other place, but some can be said to benefit from far safer connections with many more places than others.” It would seem that Penfield embraced the local site as a place that offered ideal connections to other sites, and which could be used to build up a local school in an international image. Bruno Latour, Reassembling the Social: An Introduction to Actor-Network-Theory (Oxford: OUP Oxford, 2005), 171–90.
A reluctant doctor

Given his later renown and medico-scientific accomplishments, it is something of a surprise to discover how unlikely it was that Wilder Penfield ever became a doctor at all. Born in Spokane, Washington in 1891, Penfield displayed a great distaste for medicine at a young age. The reasons for this were likely personal; Penfield's father Charles, a country doctor, had abandoned his young bride Jean while Wilder was just a boy. This early trauma left the young Penfield with contempt for the medical profession, and simultaneously an unusually close attachment to his bright and pious mother. Jean Jefferson Penfield had grand ambitions for Wilder, but they were not initially related to the emerging profession of medicine. Given his mother's strong Victorian sense of morality and mission, it seemed much more likely that Penfield was destined for the pulpit rather than the clinic. This sense of a shared life goal led Jean Jefferson Penfield to pour much of her frustrated ambitions and intellect into her precocious son. By the time Wilder was twelve years old, Jean had established a private boys school for him (The Galahad Academy) in their new home of Hudson, Wisconsin, where Jean had moved following the breakdown of her marriage. The school was part of her overall plan to ensure that Wilder would eventually be the recipient of a Rhodes Scholarship.  

Two factors initially drew Penfield back to his father's profession, neither of which were related to the clinic or hospital. The first was an overriding sense of mission inculcated into him by his mother from an early age. While an undergraduate at Princeton, he wrote out a list of possible careers, at the top of which he scrawled his life's goal: “To support myself and my family, and somehow to make the world a better pace in which to live.” Scratching out possible careers one-by-one, he was left with doctor and minister.

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8 The Rhodes scholarship had been established only two years earlier. Penfield's mother had learned about them during a lecture by a recently returned American Rhodes scholar. Penfield, No Man Alone, 3–52; Lewis, Something Hidden: A Biography of Wilder Penfield, 2–28.
The other factor that drew Penfield to medicine was his introduction to laboratory biology at Princeton by E.G. Conklin. Both Penfield and his later biographer would note the profound influence of Conklin, who introduced him to Darwinian evolution in a way that could be reconciled with the Presbyterian faith of his mother. More directly, he introduced Penfield to the value of experimental and laboratory research. Conklin belonged to a generation of American biologists - including T.H. Morgan and Ross Harrison - who had turned increasingly to the laboratory, rather than to the fieldwork of the previous generation of naturalists. Conklin himself had conducted much of his research on embryology at the Marine Biological Laboratory at Woods Hole, Massachusetts, and his lectures on the “tiny cells within the living, growing body” that could be seen through the microscope entranced Penfield.9

One additional component of Conklin’s program is worthy of note, as it serves as a useful contrast to Penfield’s later attempts to cross disciplinary lines. By the early 1920s, Conklin, who had been an active member of the Rockefeller-backed National Research Council, was involved in a series of ambitions proposals to Rockefeller’s General Education Board to revamp biological education. While Conklin proposed that physics and chemistry could be united with embryology, the proposal ultimately hewed to more traditional disciplinary divisions. It is unclear if Conklin’s interdisciplinary agenda affected Penfield directly, but the relative failure of the GEB proposal hints at the rigidity of disciplinary specialization in early-twentieth century scientific education.10

It is notable that surgery, his later métier, appealed to Penfield in a way that other medical specialties did not. Toward the end of his sophomore year at Princeton (1911), Penfield and a friend

9 Woodrow Wilson, President of Princeton University when Conklin was hired, increased the focus on scientific research at the school, leading to the creation of one of the most complete biological laboratories in the country, run by Conklin, and to which Penfield was now a beneficiary. Penfield, *No Man Alone*, 18; Lewis, *Something Hidden: A Biography of Wilder Penfield*, 37; E. Newton Harvey, “Edwin Grant Conklin,” *Biographical Memoirs, National Academy of Sciences* 31 (1958): 54–91.
snuck into New York’s Presbyterian Hospital, and spent nearly four hours in an amphitheater observing operations. A few weeks later Penfield reported to his mother that:

I don’t know that I want to study medicine. Don’t think that because I sat through some butchering that I believe I am fitted for a doctor. In this country there are about three times too many of them now. I was not particularly fascinated with the operations except that the wonderful skill and knowledge of the surgeon appealed to me. I believe that a practitioner (who can have only a little of regular operating work) should not attempt to be a surgeon but he should let the surgeons in the hospital do it all.11

Penfield’s reverence for the skill and intelligence of the surgeon was unsurprising given the changes that had been underway in American medicine during the previous several decades.12 By the 1910s, the slow and tentative adoption of aseptic surgical procedures that began following Joseph Lister’s initial discoveries in the 1860s had been completed, and asepsis had transformed surgery into the primary mode of therapeutics in American hospitals. Indeed, by the 1890s the introduction of rubber gloves, autoclaves and sterilized dressings had largely replaced the carbolic acid sprays and reused sponges and bandages of the nineteenth century, and the operating room had taken on a recognizably modern character. Combined with the adoption of anesthetic procedures to dull operative pain and growing knowledge of microorganisms, within another decade surgery would become the primary reason for admission to American hospitals. The growing safety and efficacy of surgery had also emboldened American doctors to explore, probe and intrude upon cavities of the

11 “1909-1913 Princeton Personal Corresp WGP to JJP typescript extracts” D C/D 33-2/1 1, Box 41a, Wilder Penfield Fonds.
body that had previously seemed forbidding. With the abdomen finally unlocked as a potential site of therapeutic intervention, the skull was not far behind. As one surgeon explained in 1888, “Abdominal surgery is now the field where the most brilliant successes are to be attained...It is from the work we are now doing & hope to do in abdominal surgery (&...cerebral surgery as well) that the Hospital must gain its position among the hospitals of the world at the end of the next ten years.”

Indeed, echoing Penfield’s observation that general practitioners ought not to intrude on the surgeon’s turf, surgeons themselves were beginning to carry increasing weight within the social world of American medicine, at least partially due to their embrace of the cultural capital of laboratory science. As Charles Rosenberg has observed, “[Surgery now] seemed to represent the innovative spirit of science made clinical reality.” Occurring only one year after the publication of the famous Flexner Report, which recommended reorientation of American medical education towards scientific research and laboratory knowledge, Penfield’s intrusion into the Presbyterian operating room in 1911 was, despite his protests to the contrary, likely the last in a long line of personal and social developments that would draw a precocious and ambitious young man like himself into the world of professional medicine. The growing emphasis on laboratory investigation and research was reinforced for Penfield following a six-week anatomy course at Harvard University in the Spring of

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13 This thumbnail sketch of the history of modern surgery is, of course, incomplete with regards to the numerous debates and controversies that surround the adoption of specific technologies. Again, providing a complete bibliography for these issues would prove impossible. Some useful guides include Thomas Schlich, “Asepsis and Bacteriology: A Realign...” 14 M.H. Richardson, quoted in Rosenberg, 148-9. For more on surgery in American hospitals, see Charles E. Rosenberg, The Care of Strangers: The Rise of America’s Hospital System (New York: Basic Books, 1987), 147-50. 15 Rosenberg, 150.
1914. Robert M. Green, the lecturer in anatomy, captivated Penfield and led him to spend hours alone in the evening working in the dissection room.

Penfield finally received his long sought-after Rhodes Scholarship on the eve of World War I, and arrived at Oxford nine months later to find it eerily empty, as many of its young men had gone to fight on the battlefields of Europe. It was here that Penfield made two acquaintances that would fundamentally alter his life’s course. The first was Sir William Osler, who would become Penfield’s medical idol for the remainder of his life. Osler’s influence on Penfield was two-fold; first, his genial insistence on the importance of patient testimony for proper diagnosis formed much of Penfield’s medical ethos and approach to the clinical aspect of neurology. Penfield would frequently accompany Osler on consultations, and observed the elder man’s renowned bedside manner, and gentle probing of patient testimony. The second influence on Penfield was more direct; during the course of his time as a practicing doctor in Montreal between 1871 and 1884, Osler had personally performed over a thousand autopsies, many of which were for patients who had died as a result of brain and nervous system illnesses. Indeed, by the time he left Montreal, Osler had become a first-rate neuropathologist and neuroanatomist, and stressed to Penfield the importance of correlating patient case histories with post-mortem pathology (and particularly microscopic observation). It was from Osler that Penfield learned how to remove brains properly at autopsy, and, more importantly, he imbibed the older man’s view that autopsy was not a macabre practice, but rather a valiant act of humanistic medicine.

A physiological surgeon

16 Penfield was “inpatient to begin his medical training,” but still a student at Princeton, which had no medical school, so he enrolled in a summer course in anatomy offered at Harvard. Penfield, No Man Alone, 29.
The other Oxford presence that was imprinted upon Penfield was that of Charles Sherrington, who had, in his epochal *The Integrative Action of the Nervous System* (1906), transformed neurophysiology into a mature, robust field of study.\(^{20}\) Many authors have noted the pivotal influence of Sherrington, most notably Penfield himself, who spoke of him in exalted terms: “Sherrington was my master,” Penfield noted in his memoirs.\(^{21}\) He later described the effect of a number of Sherrington’s lecture courses thusly:

> I looked through his [Sherrington’s] eyes and came to realize that here in the nervous system was the great unexplored field - the undiscovered country in which the mystery of the mind of man might someday be explained.\(^{22}\)

Penfield’s immediate impression of Sherrington was clearly positive, but his tutelage with the senior man would have to wait. For the moment, Penfield had to master a traditional medical education, a task aided by Osler’s personal endorsement that he could master three-year’s worth of material in only two years. Indeed, Penfield’s impatience to begin medical work nearly cut short not only his career, but his life. During a vacation period in 1916, Penfield travelled to France to gain medical experience by working as a paramedic in a hospital treating wounded soldiers. On his return voyage to England, the ship on which he travelled, the *SS Sussex*, was hit by a German torpedo. Penfield survived the explosion and sinking of the ship, and spent several weeks recuperating in the home of William Osler and his wife, further tightening the relationship between the Regius Professor and the young medical student.\(^{23}\)

Penfield returned to American in 1916 following his first period in Oxford, and entered Johns Hopkins to finish his medical education, which he completed in June of 1918. Penfield hoped to gain surgical experience immediately, and spent a brief stint observing the surgical practice of

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\(^{21}\) Penfield, *No Man Alone*, 288.

\(^{22}\) Penfield, 36.

Harvey Cushing at the Peter Bent Brigham Hospital. This year of observation, and Penfield’s subsequent trips to record Cushing’s technique, have often been used to imply that he was a member of the ‘Cushing generation’ of surgeons who were tutored in neurosurgery by the man who established the profession on a firm basis. While there is some truth to this, the influence of Cushing on Penfield has been overstated. Although Penfield did make a close study of Cushing’s surgical technique, he never worked directly with Cushing, and the later relationship between the two men could best be described as cool. Rather, it was largely, and somewhat surprisingly, the example of Sherrington that seemed to inspire Penfield:

> It was not the example of Horsley or Cushing that led me into the surgery of the nervous system. It was the inspiration of Sherrington. He was, so it seemed to me from the first, a surgical physiologist, and I hoped then to become a physiological surgeon. As years passed his influence did not grow less but stronger.

Indeed, the influence of Sherrington on Penfield’s medico-scientific orientation is difficult to overstate. Moreover, the contrast between Penfield and Cushing is worth lingering over. While Cushing had briefly observed Sherrington’s laboratory in Liverpool, he had not been a regular worker there. While Cushing did display scientific curiosity about the pituitary gland and the pathological structure of tumors he removed, he had less interest in the classic reflex-based neurology or laboratory physiology undertaken by Sherrington, and in fact spoke rather disparagingly of Sherrington in private letters. By contrast, Penfield participated extensively in Sherrington’s laboratory exercises, which involved painstaking animal experimentation in which dogs

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26 Of Sherrington, Cushing stated, “He operates well for a ‘physiolog’ but it seems to me much too much. I do not see how he can carry with any accuracy the great amount of experimental material he has under way….As far as I can see, the reason why he is so much quoted is not that he has done especially big things but that his predecessors have done them all so poorly before…. ” Quoted in Bliss, *Harvey Cushing: A Life in Surgery*, 149–51.
or monkeys would be decorticated, and their physiological responses precisely measured on kymographs. Following his brief period at the Peter Bent Brigham hospital, Penfield returned to Oxford to work with Sherrington more directly in his physiological laboratory. It is worth pausing to consider the nature of this crucial influence.

More than almost any other scientist in the late-nineteenth and early-twentieth century, Sherrington brought the new insights of the microscopic world of the cell to bear on the oldest puzzles of the nervous system. Emerging from Michael Foster’s physiology laboratory at Cambridge in the late-nineteenth century, Sherrington had, over the course of his career, employed exacting experimental techniques to restructure understandings of the reflex, and particularly inhibitory reflexes, and their role in coordinating movement within vertebrate animals. As Judith Swazey has observed, Sherrington’s major contribution was to ground the investigation of spinal reflexes within a more precise understanding of the micro-anatomy of the nervous system. Sherrington embraced the findings of the then-obscur e Spanish histologist Santiago Ramon y Cajal, whose refinement of the Golgi staining techniques had provided crucial evidence that the nervous system was composed of discrete cells, rather than a continuous ‘reticulum’ network in which nerves grew into one another. The ‘neuron doctrine’ that resulted from Cajal’s microscope studies allowed Sherrington to re-center studies of reflex action on the cellular unit, and particularly on the integration of reflexes at the junction between nerve cells - the synapse (a word he coined).

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27 Some of Penfield’s research work appeared in Sherrington’s later Mammalian Physiology, which Sherrington dedicated to his students, of which Penfield was one. C. S. Sherrington, Mammalian Physiology: A Course of Practical Exercises (London: Clarendon Press, 1919), 44, 49.

It was the hope of working with Sherrington that brought Penfield back to Oxford in October of 1919. His plan of study was grandiose, to say the least:

if I could return to Oxford for graduate study, and learn what was known about the neurophysiology of animals, I might gain a broader view of man, and find more constructive approaches to human neurosurgery. I wanted to know all that was known about the human brain, neuropathology, neuroanatomy, neurocytology. Then, I argued, I would learn clinical neurology and finally go on to the operative technique of neurosurgery.29

In short, Penfield hoped to absorb the entirety of knowledge about the human nervous system himself, and bring that knowledge to bear on surgical problems. In so doing, he might distinguish himself from the efforts of Cushing, whose primary contribution had been, in Penfield’s eyes, merely to prove that brain surgery, and particularly tumor removal, “could be carried out with a reasonably low death rate.”30 Ultimately, Cushing remained an anatomical surgeon; the primary goal was the removal of disease-causing bodies. Penfield, by contrast, hoped to bring the power of laboratory physiology to bear in order to expand the scope of problems that neurosurgeons could attack.31

Of crucial importance for Penfield’s later scientific project was his increasing turn, in Sherringtonian vein, to the microscopical. While Sherrington himself was unable to directly oversee Penfield’s Oxford thesis, his assistants, Cuthbert Bazett and Harry M. Carleton, supervised Penfield’s research, which included extensive histological work enabled by cellular staining. In this regard, the example of Sherrington was crucial. Sherrington had championed the cellular staining methods of Cajal and his relatively unknown Spanish school of histology, and had made the neuron doctrine central to his own theories of neural integration. For his part, Penfield absorbed Sherrington’s

29 Penfield, No Man Alone, 40.
30 Penfield, 40.
31 On the distinction between ‘anatomical’ and ‘physiological’ approaches to surgery, see Schlich, The Origins of Organ Transplantation: Surgery and Laboratory Science, 1880-1930. It is notable that Victor Horsley, who had attempted early epilepsy surgery at Queens Square, and who Penfield deeply admired, was also a major player in early thyroid transplant efforts, which were a key example of a physiological approach to surgery.
emphasis on the importance of the micro-architecture of the nervous system through his work with Carleton, which led to one of his earliest scientific publications, a study of the Golgi apparatus. This research was of a high enough quality that it led both to Penfield’s BSc degree from Oxford, and his first publication in *Brain* in 1920.32 While Penfield never published his work on decerebrate animals with Bazett, he remarked privately that he would “make it the nucleus of my work for the next two years…”33

At the same time, Penfield began specialized training as a clinical neurologist under the supervision of Gordon Holmes at the National Hospital at Queen Square. The National Hospital had been the site for the professionalization of neurology in England, and was home to the second generation of professional neurologists in England, of which Holmes was the acknowledged leader. Holmes, an athletic Irishman, had initially trained as a pathologist, rather than a clinical neurologist, and had developed a fascination with the microscopic anatomy of the nervous system after training in Germany with the pathologist Karl Weigert, who himself had practiced careful staining of neuroanatomy (Weigert used staining techniques to identify the myelin sheath that surround the axons of nerves). In Penfield’s estimation, the more precise anatomical knowledge that Holmes could bring to his clinical neurological work was a great improvement on the clinical neurology of the past: “Holmes was famous as a neuroanatomist. He would, the students knew, give these patients simple tests and turn to penetrating discussion of the mysteries of the structure of brain and spinal cord, instead of repeating the descriptions called ‘syndromes’ in the textbooks of older neurologists.”34

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33 Penfield to Jean Jefferson Penfield, 20 February 1920, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
34 Penfield’s account falls into the somewhat hagiographical tradition of describing Holmes identified by Stephen Casper. The common factor, however, is the emphasis Holmes placed on setting neurology on a firm, organic basis, and banishing the more confusing functional ‘syndromes’ to the
While Penfield was becoming increasingly competent in the diagnostic field of clinical neurology, he was also assisting in surgery at Queen Square. The specialty of neurosurgery had begun at the Queen Square National Hospital under the direction of Victor Horsley, who by all accounts ought to have been able to fulfill the role of ‘physiological surgeon’ that Osler had recommended in 1907. Indeed, as Thomas Schlich has shown, Horlsey’s earlier work in the field of thyroid transplantation had seen him move seamlessly from the laboratory to the clinic and back again, in a way that moved beyond the traditional ‘anatomical’ approach to surgery. Horsley was an accomplished physiological investigator, and had even performed early attempts at epilepsy surgery at the National Hospital in the late nineteenth century. However, as Penfield later acknowledged, Horlsey was hampered by not himself being a clinical neurologist, and had to remain in a supporting role to the neurologists at Queen Square. As Penfield assisted the surgeon Percy Seargent with operations at Queen Square, he tied many of the preceding themes together - physiology, animal experimentation, the microscope, pathology, and their applications for surgery - in two lengthy comments to his mother:

I acted as [Seargent's] first assistant on Friday and am to write up the description of the operations each time and the post-operative notes.
I brought my [experimental] cat up from Oxford, the one with half a brain. I carried her in a closed hamper together with the brains of some of her sisters, but she didn't object in the slightest, and my fellow travelers never suspected. There is a whole epoch of neurological knowledge in that cat’s symptoms, if only one could see clearly enough to interpret it. I shall do more work along that line someday.
Gordon Holmes looked at her yesterday and was very helpful as long as I could keep her pinned down. I never could get Sherrington to commit himself on her. She dies soon and

realm of psychiatry; this motive became more pronounced for Holmes after WWI and his experiences with shellshock. Penfield, No Man Alone, 46; Casper, The Neurologists: A History of a Medical Speciality in Modern Britain, c. 1789-2000, 77–80.

then the long microscopic hunt [begins]. She eats now, purrs, growls, walks, but cannot find her milk etc.37

Two weeks later, Penfield wrote:

The surgical cases are tremendously interesting. The best part of it is that I have the chance of studying them carefully and getting the opinion of the best neurologists and then, instead of wondering vaguely whether or not the diagnosis was correct, I have the opportunity of seeing their brain, then seeing them recover, or die, and conclude for myself whether or not the pre-operative conclusions were justified.

The English work in a peculiar way. We [Americans] pride ourselves on having the last word in equipment at home, and then may not use it very well. The surgeon over here does not mind making use of make-shift instruments, and does not seem to worry much about the operation beforehand. The result is that he does the ordinary cases very well and with consistently good results. But in the unusual crisis he fails because he is not equipped for it. Also his easy going carelessness with regard to asepsis must be responsible for occasional failure. Britain in surgery at present saves so few patients. Most of the cases are beyond hope anyway and the only possible thing is to relieve them from headache and to save their eyesight for a little while, and the number of immediate deaths are so enormous in comparison with other branches of surgery that many physicians advise against turning their cases over to the surgeon even when they do realize that they have a brain tumor (which they rarely do before the postmortem).

An old intern from the National Hospital told me the other day that he had made a note of 150 cases operated on while he was on service and only two of them walked out of the hospital entirely cured of all symptoms.

I know the surgeon who was working there… and he knows nothing about the brain, and also the statistics are not fair as… most of the cases, could only be improved.

But it is such a little thing that turns the scale in brain operations and I see things done even by Sargeant who is a splendid surgeon which I, personally, think are the cause of the occasional death. That sounds egotistical no doubt. At any rate, in the next 30 years I believe there will be great strides in our knowledge of the nervous system and our treatment of it and I want to take part in both changes.38

Penfield’s aspiration, after nearly two years of graduate study in England - at bench, bedside, and operating theater - was to unite the insights of laboratory physiology with clinical neurology and pathology, and the surgical precision of the American schools with the neurological sophistication of the English National Hospital. While other surgeons of the early twentieth century often spoke of the promissory value of laboratory science for medical practice, Penfield’s remarks to his mother

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37 Penfield to Jean Jefferson Penfield, 9 January 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
38 Penfield to Jean Jefferson Penfield, 22 January 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
indicate that he believed such rhetoric wholeheartedly. However, Penfield’s belief in the forward-looking orientation of neurosurgery would be strained considerably as he embarked upon his own surgical practice.

A terrible profession

If Penfield left England in 1921 with a sense of cautious optimism about the future of his chosen profession, his next several years in the United States brought him headlong into the grim reality of early-twentieth century surgery. Initially intent on joining the staff of the newly formed Henry Ford Hospital in Detroit, Penfield turned down the offer of a thriving neurosurgical practice because it did not include any possibility for conducting research. He ultimately took a position at New York’s Presbyterian Hospital, which had just signed an agreement to become the primary teaching hospital for Columbia University’s medical school. This agreement had netted the Presbyterian a considerable grant from the Rockefeller Foundation, and Penfield was pleased that his immediate supervisor, Allen O. Whipple, was enthusiastic about Penfield’s training in physiology and pathology, and was willing to teach him the ropes of general surgery.

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39 Penfield discussed this incident in his autobiography. According to Penfield, the chief surgeon at the Ford Hospital, R.D. McClure, flatly denied the possibility of doing animal research at an otherwise “splendid modern hospital.” Privately, Penfield commented to his mother that it was not in fact McClure who was the problem, so much as the superintendent, Henry Ford’s friend and private secretary, Ernest G. Liebold. According to Penfield, Liebold “is hopeless. I told him I wanted to continue research. He was ridiculous. He said, well we have a good laboratory and if there is any problem that needs solving, we will just turn it over to specialists there and we will get things solved. He talked about medicine as any conceited ignoramus might.” Elsewhere, Penfield speculated that animal research was likely not carried on at the Ford hospital because it was, in his words, “Ford Driven,” and that “no animal experimentation is allowed as Henry [Ford] is prejudiced against it.” For his part, Liebold’s judgment was open to question; a rabid anti-Semite, he had been the prime-mover behind Ford’s republication of The Protocols of the Elders of Zion forgery in 1918, and was later an open Nazi sympathizer. Penfield to Jean Jefferson Penfield, 8 and 19 June 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds; Penfield, No Man Alone, 55.

40 Penfield commented privately on his good fortune: “Best of all the research facilities are great and someone else will prepare my sections, thank goodness.” Penfield to Jean Jefferson Penfield, 13 June 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
While Penfield began to find his feet as a surgeon at the Presbyterian, he also became increasingly despondent; the high hopes he had for the saving power of brain surgery did not immediately materialize. Despite his enthusiasm for research and teaching opportunities, he lamented the depressing state of his surgical interventions: “I was supposed to take over the brain and nerve operations which is the most hopeless and difficult field.” The union of the Presbyterian with Columbia’s college of Physicians and Surgeons initially stirred great hope in Penfield:

This is a great mine of clinical material….if the group of which I am a part can only work with the scientific enthusiasm that Osler, and Welch, and Halstead etc. instilled at the beginning of Johns Hopkins thirty years ago, I might live to see a real clinic develop. The trouble has been that the profession of New York has been too commercial. Perhaps the temptation is great for there certainly is a pile of money here.41

In addition to his distaste for the commercial concerns of the New York medical world, the operations themselves were distressing. For weeks Penfield would wait for a genuine neurosurgical case, biding his time with general surgery on hernias, and often traveling to Baltimore to observe the more exciting brain surgeries of Walter Dandy.42 When he finally got the chance to operate on a brain tumor patient, Penfield was unable to remove a malignant tumor from a female patient who died shortly thereafter. Describing the incident to his mother, Penfield lamented that “brain surgery is a terrible profession. If I did not feel that it will become very different in my life-time, I should hate it. It seems a bit depressing.”43

In contrast to the depressing world of neurosurgery, and his growing disputes with the ranking neurologists at the Presbyterian and elsewhere, Penfield increasingly found solace in the

41 Penfield to Jean Jefferson Penfield, 2 October 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
42 Penfield admired Dandy considerably more than Cushing, and learned a number of techniques from him, most notably Dandy’s novel technique of ventricularography, which involved injecting a bubble of air into the brain ventricles by way of the spinal cord, in order to visualize the brain interior with an x-ray. Penfield to Jean Jefferson Penfield, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
43 Penfield to Jean Jefferson Penfield, 20 November 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
laboratory. Friction with Fredrick Tilney, the neurologist in charge of the New York Neurological Institute, led Penfied to write to his mother:

I told you that there was talk of my going over to work a certain amount of time at the [New York] Neurological Institute. Dr. Tilney is anxious to have our neurological patients sent there. Dr. Whipple has said that he did not want me to get out of touch with the medical neurologists as the bulk of one's operative cases come from them. I have been thinking it over and I now see that the thing for me to do is not to go over there [the New York Neurological Institute] where I may work under neurologists with a certain amount of friction and never get any farther than they are already, which isn’t far. What I want to do is to put my time in working at animal neurology and if I succeed in making any advance, life will be worthwhile and if not, I will at least have tried. If I do learn to cure something or other, hydrocephalus for instance, there will be plenty of material.44

Research on hydrocephalus, the dangerous expansion of the brain's fluid-filled ventricles that often threatened the life of newborns, was the initial problem that Penfield felt he might attack. In February of 1922, he made a list of his career goals:

1. Perfect myself in the surgery of stomach and intestines by operation and dog work. I am teaching in a course of that kind of surgery on dogs to the medical students.
2. Study of hydrocephalus.
   A. in the laboratory with dogs—I’d like to put in full time, on this. It seems like my proper work and everything else a side issue, yet there seems very little time for it.
   B. Study of the same condition in babies as they come under my care at the hospital.
3. Coordination and supervision of the long continued administration of sugar solution by vein. This I stick at because Dr. Whipple desires it.
4. Finish microscopic study and write up the series of tumors of the skull for publication from a surgical point of view. Read a paper and perhaps publish the same thing from a neurological point of view.
5. Watch Elsberg, (the best brain surgeon here) and learn his technique.
6. Get into touch with Neurological Society and Foster Kennedy, Prof of Neurology at Cornell.
7. Attend conferences at Neurological Out-Patients
8. Make a study of an interesting brain I operated on, with a promising student.
9. Do reading in relation to Clarke's course in Surgical Pathology in which I teach two mornings a week.45

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44 Penfield to Jean Jefferson Penfield, 4 December 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
45 Penfield to Jean Jefferson Penfield, 22 February 1922, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
The above list gives a sense of Penfield’s eclectic surgical and scientific interests, and his improvised research program. While he considered hydrocephalus to be the most promising line of attack, it was the final entry on this list that would fundamentally alter his life’s course.

Epilepsy

Penfield would later credit his turn to epilepsy research to his work with the surgical pathologist William C. Clarke, about whom he wrote in bombastic terms; “Bill Clarke was, in appearance and in turn of mind, a modern version of the Greek philosopher Socrates.” Nevertheless, it is hard to overstate the importance of Clarke’s influence.

Surprisingly little is known about Clarke’s life or impact on the medical world of early-twentieth century New York, but a few relevant details are worth mentioning. Clarke’s appointment was part of a larger trend, in the early years of the twentieth century, for surgical departments to further define themselves as a distinct specialty apart from general medicine. While pathology had been firmly established as part of the modern medical world in the early eighteenth-century clinics of Europe, pathologists connected with university teaching hospitals preferred to conduct complete autopsies, and displayed a bias against the Stückchen-Pathologie (bit pathology) that might come from examining tissues removed by surgeons. As such, a small number of research and diagnostic laboratories began to develop within surgical departments. It was for this reason that the Columbia College of Physicians and Surgeons established a department of surgical pathology in 1903, and appointed Clarke to run it in 1905. Clarke, trained initially as a surgeon and called “Wild Bill” by his colleagues, instituted a regime of microscopical examination and animal experimentation that was oriented towards the investigation of wound healing. It was this course of teaching and research that

46 Penfield, No Man Alone, 63.
Penfield referred to in his list of 1922, and it was Clarke’s inquiries about the nature of brain-wound healing that re-oriented him towards epilepsy.47

While he had published successfully with Carelton on the Golgi apparatus, Penfield’s research projects which were more directly related to surgery had been marked by false starts and dead ends. An early study of possible replacements for blood during surgery had gone nowhere, and his initial studies of hydrocephalus had been “too ambitious, too premature.”48 However, in epilepsy Penfield found his calling. Beginning in 1923, Penfield began assisting Clarke in his instructional laboratory as part of a general course on the healing of tissues. Clarke had inquired into the processes by which the brain heals following injury, and the ways in which the healing process might influence epilepsy. Since antiquity, medical writers had noted that traumatic injury to the brain might, if the victim survived, be followed by epilepsy. Treatment of soldiers who had survived gunshot injuries following WWI confirmed that trauma to the brain tissue would often be followed by epileptic seizures. Although this observation ignored the more nebulous problem of non-traumatic epilepsy, it did provide a circumscribed medical problem that Penfield could tackle. At Clarke’s suggestion, Penfield began a study of dogs, in which he attempted to understand the way in which puncture wounds to the dura and pia matter might form cicatrices (scars), and how these scars might trigger epileptic attacks.49

Penfield had a problem, however. Visualizing the complex tissues of the mammalian nervous system with a microscope was difficult, even under the best of circumstances. Staining techniques developed first by Camilo Golgi in Italy in the nineteenth century, and later refined by Santiago Ramon y Cajal in Spain, had proved crucial to delineating the nature of the nerve cell, and

48 Penfield to Jean Jefferson Penfield, 6 May 1922, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
49 Penfield, No Man Alone, 63–65.
establishing the so-called ‘neuron doctrine’ at the beginning of the twentieth century; however, incorporating that knowledge into medical practice had been slow-going, and visualizing other types of cells in the nervous system had proven difficult. It was clear that different staining methods would be necessary to analyze the fine cellular structure of the non-neuronal elements in the nervous system. Penfield searched desperately for a method that might allow him to learn more about the growth of cerebral scars.\textsuperscript{50}

Penfield had learned a number of staining methods during his time as a graduate student in England. In 1919/1920, Penfield and Carleton, on Sherrington’s advice, had tried to stain a number of nerve-cell preparations, with little luck. However, Sherrington had prophetic advice for them. “Don’t give up until you have tried the methods of Ramon y Cajal.”\textsuperscript{51} Given the crucial importance of Cajal’s cellular staining for Sherrington’s entire theoretical program, this advice was unsurprising. However, Cajal and his growing school of histologists had taken to publishing their results almost exclusively in Spanish, a language unknown to most Anglo-American scientists at the time. Armed with a Spanish dictionary, Penfield and Carleton had translated a number of articles from the \textit{Cajal Transactions} on staining techniques, and were amazed to discover the clarity of the results. This amazement led Penfield and Carleton to develop an immediate affection for the Spanish school of histologists. At Oxford, Carleton and Penfield had taken, when working in the lab, to referring to each other as ‘Ramon’ and ‘Pio’, after Ramon y Cajal and Pio del Rio-Hortega, the founder and most prominent student of the Spanish school.\textsuperscript{52}

Four years later, in 1923/4, Penfield again attempted to solve his staining problem with a Spanish dictionary. New publications from Cajal’s laboratory in Spain seemed promising, in

\textsuperscript{50} Penfield spent time with Frank Mallory at Boston City Hospital learning additional staining techniques. Penfield, 64–84.
\textsuperscript{51} Penfield, 91.
\textsuperscript{52} Penfield to Jean Jefferson Penfield, 23 July 1927, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
particular those of his most talented student, Pio del Rio-Hortega. Penfield translated the articles and attempted to recreate the technique. The results were promising, but imperfect; some cells “stood out, clear and complete, as I had never seen them before,” while others remained murky. Still, they provided enough of an incentive. Recently released from the demands of general surgery, Penfield proposed to Whipple that he be permitted a six-month sabbatical in order to travel to the Instituto Cajal in Madrid to learn the methods of staining non-neuronal brain cells from Rio-Hortega himself. Penfield undertook intensive Spanish language lessons, and in the Spring of 1924 boarded a ship bound for Spain. Aboard the ship, Penfield dispatched a letter to his mother that mixed his own scientific curiosity with professional and financial concerns:

I can get no further until I learn something about neuroglia cells in Madrid - and here we are in mid-Ocean - and where are we going as far as the future is concerned!...My ability to build up a practice is very doubtful. I am concerned as far as private neurological cases are concerned....The point is I have done nothing to justify consulting practice. These articles have all been pot boilers. I’ve done nothing but prepare and am still preparing. I do not see the way toward hydrocephalus or epilepsy or any worthwhile problems and so I go on trying to learn, hoping the method will become apparent. But there is not the slightest guarantee that any clue lies in the direction I am taking. I am at the height of my power and still reaching out for new weapons - using none.

Spanish Methods

At the time that Penfield and his wife Helen arrived in April of 1924, Spain was far from a scientific powerhouse. Despite the economic boom of the immediate post-World War I period, science in Spain lagged far behind the rest of Europe. The purported backwardness of Spanish science had even given rise to a scholarly debate, the polémica de la ciencia española. In 1897, one year before the disastrous Spanish-American war that cost Spain its American colonies, Santiago Ramon y Cajal had addressed the Academy of Science of Madrid, calling for more state-sponsorship of science, and a better connection to the great scientific nations of Europe. For his own part, Cajal

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53 Penfield, No Man Alone, 92.
54 Penfield to Jean Jefferson Penfield, March 1924, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
walked the walk of internationalism; his trips to international scientific congresses in the late-nineteenth century had spread his histological discoveries to the non-Spanish-speaking world, and secured for him the Nobel Prize in 1906 (shared with his rival, the Italian Camillo Golgi, whose cellular staining methods Cajal had vastly improved upon).\textsuperscript{55}

Owing to the prestige of the prize, Cajal ultimately prevailed upon the Spanish government to endow him with a laboratory in Madrid, in which he had built up a small but prolific school of Spanish histologists who steadily turned out publications during the first-third of the twentieth century. Despite their productiveness, the scientific output of the Instituto Cajal had made only a small impact on the international scientific community.\textsuperscript{56} In his memoirs, Cajal laid the blame for this state of affairs squarely on his decision to publish primarily in Spanish:

I was always anxious, especially after the State placed in my hands a suitable and well-equipped laboratory, to found a genuinely Spanish school of histologists and biologists…The desired school of Spanish histology and neurology exists and is a permanent focus of activity. Its discoveries...have spread beyond the frontiers and its methods and inventions are applied in foreign laboratories. Moreover, they would be applied more widely if, recognizing the almost total ignorance of the Spanish language among scientists, we were to publish all our works in foreign periodicals. For it must be stated…that hardly a third part of the Spanish histological publications \textit{are} known abroad.\textsuperscript{57}

Indeed, it was by way of Sherrington’s knowledge that Penfield had become aware of Cajal’s methods in the first place, and it was only through painfully slow translation that he had discovered that Cajal’s protégé, Rio-Hortega, had succeeded in staining non-neuronal elements within the nervous system.

Much of Cajal and Rio-Hortega’s knowledge remained locked away in Spain. Penfield was the first American not only to work with Rio-Hortega, but to work in the Cajal labs at all; an earlier

\textsuperscript{56} Nieto-Galan, “The History Of Science In Spain: A Critical Overview.”
overture from Percival Bailey and Harvey Cushing to bring Rio-Hortega to America had been unsuccessful, and few American doctors or scientists had the requisite language skills to join in the bench work at Madrid.\(^{58}\) Indeed, Penfield fretted about whether his Spanish lessons would prove thorough enough to allow him to work with Rio-Hortega. However, despite these concerns, Penfield was able to begin histological studies with Rio-Hortega almost immediately. Rio-Hortega taught Penfield the requisite tacit knowledge that was required to properly stain and view non-neuronal cells in the sections of rabbit brains. Blocks of rabbit brain were fixed in formalin, and then sectioned with a microtome. The resulting paper-thin slices were then washed in successive baths of alcohol and carbo-xylol, and then stained with silver carbonate. The difficulty that Penfield had in reproducing the procedure in New York was mostly related to the precise timing and heating procedures that were used; different timing and heating would lead to different cells being stained, at different depths.\(^{59}\) However, “with care and patience, selective staining might be possible for almost any and all of the cell structures within the brain. This was truly selective photography.”\(^{60}\) In the subsequent months in Spain, Penfield would prove himself a capable bench worker, and would make two important contributions to Rio-Hortega’s research program. By making modifications to Rio-Hortega’s techniques, Penfield developed a reliable method for staining a cell type that the pair christened ‘oligodendroglia’ cells.\(^{61}\) To understand the importance of this discovery, we must digress briefly and examine the career of Rio-Hortega himself.

Pio del Rio-Hortega was, in many respects, Cajal’s opposite. Whereas Cajal had pulled himself up from modest origins, served in the Spanish army in Cuba, and established a laboratory at

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\(^{58}\) Penfield, *No Man Alone*, 108.

\(^{59}\) Penfield to Percival Bailey, 15 January 1940, C/G 40 B, Box 55, Wilder Penfield Fonds. See also Penfield, 97–101.

\(^{60}\) Penfield, 100.

great personal cost, Rio-Hortega had been born to nobility and considerable wealth. Born on 5 May 1882, Rio-Hortega grew up in the town Valladolid in Old Castile, and obtained a medical degree at the University of Valladolid. Unsatisfied by medicine, Rio-Hortega travelled to France and Germany to learn microscopy, but was forced to return following the outbreak of World War I. In 1916 Rio-Hortega joined Cajal’s lab, and in 1918 accomplished a feat at which Cajal had failed. While Cajal had been able to stain and visualize two of the elements of the central nervous system - neurons and astrocytes - he had been unsuccessful in making visible what he called the ‘third element’ - a type of cell that remained impervious to his gold and silver stains. Rio-Hortega had succeeded where Cajal had failed, using ammoniacal silver carbonate instead of gold chloride-mercury bichloride, to selectively stain microglia cells. The discovery of the ‘third element’ by the most junior member of Cajal’s lab, however, had caused a rift between the older and younger men. Rio-Hortega remained estranged from Cajal, working in a small lab on the outskirts of Madrid. Penfield’s delineation of a second type of glial cell - the oligodendroglia - provided definitive evidence that Rio-Hortega’s methods were correct, and Penfield’s article in Brain in 1924 constituted the first confirmation of the lab’s discovery in a foreign language - a feat that had eluded Cajal. Taken together, the discoveries of the young American surgeon in Rio-Hortega’s lab could be counted on to widen the gap between the older and younger Spanish histologists.62

The second major discovery Penfield made during his time in Rio-Hortega’s lab was more consequential for his own surgical program. Penfield commenced a study of experimental wounds in the brains of rabbits - the same study he had begun in New York, but this time equipped with Rio-Hortega’s more precise staining methods. The results were not conclusive, but they were

intriguing. The glial cells observed by Penfield in the wounded brain formed scars that contained amoeba-like macrophages that destroyed wounded cells. However, the scar formed by the glial cells continued to irritate the brain of the experimental animal, causing seizures. While the studies of experimental scar formation did not provide a definitive explanation of post-traumatic epilepsy, they did convince Penfield of the value of the ‘Spanish methods’ for neuropathological study.63

Penfield’s time in Spain had two major effects. First, it solidified his stance towards the relationship between the laboratory and the clinic. In the coming years, he would write at great length about how the histological methods he learned in Spain served as the crucial link between his surgical procedures and laboratory research. By applying precise laboratory methods to surgical pathology, he hoped to unravel a number of medical mysteries that might be amenable to surgical intervention. This would serve his purpose of advancing the professional authority of the brain surgeon, as against the overbearing neurologists with whom he contended in New York. Prior to his trip to Spain, Penfield could grouse to his mother that, “I have the appointment at the [New York] Neurological Institute but look for little from it. The battle ground is the pathological and research labs.”64 Upon returning from Spain, Penfield now had a powerful weapon in his campaign to wrest professional and scientific leadership from neurology.

Second, Penfield’s time in Spain reinforced an internationalist orientation that had been developing in him for some time. From a young age, Penfield scorned what he considered the pettiness of national loyalties, and the destruction and wasted potential that might come from them. One month after the beginning of World War I, Penfield wrote in his diary:

The terrible war that is raging over in Europe affects us only to fill the newspapers and awe us sometimes. It is a wicked thing and so useless! What difference does it make to the…working man whether the plot of ground on which he works belongs to a Germany

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63 Penfield, No Man Alone, 104–7, 111, 117; del Río Hortega and Penfield, “Cerebral Cicatrix: The Reaction of Neuroglia and Microglia to Brain Wounds.”
64 Penfield to Jean Jefferson Penfield, 7 January 1923, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
which owns Alsace Lorraine [and] part of France or whether France owns it all? Will a Parliament of men ever come?  

While it was common for young scientists to express an internationalist orientation in the years before 1914, the First World War served as a crippling blow to such sentiments. The years after 1918 saw tentative steps toward a renewed scientific internationalism, but they were largely piecemeal and unsuccessful, a period Elisabeth Crawford has referred to as one of “international science without internationalism.” Attempts were made to reconstruct scientific communication and travel between the wars, but much of the European scientific community retained chauvinistic and isolationist attitudes well into the 1920s. 

In this respect, it becomes clear just how unique Penfield’s work in Spain was. The combination of an overtly peaceful Europe in the 1920s, combined with the largesse of Rockefeller money, allowed Penfield to gain international scientific experience at a time when many scientists, in both America and Europe, were increasingly looking inward. Moreover, Penfield developed an orientation towards scientific work that viewed internationalism and interdisciplinary as related, and often overlapping, scientific virtues. The enemies of good science and medicine were linguistic and geographic isolation, nationalistic chauvinism and narrow-mindedness, ego and pride. As his time in Spain came to a close, Penfield was offered an opportunity to meet the elder Cajal, whose work had been so influential for him as a graduate student in England. Penfield recorded the meeting at great length in a letter to his mother. At the time, the feud between Rio-Hortega and Cajal was at its

65 File 9 - Wilder Penfield Diary, 1914, Harvard, Box 1, Jefferson Lewis Fonds.
68 For more on the notion of scientific and epistemic virtues, see Lorraine Daston and Peter Galison, Objectivity (New York: Zone Books, 2007).
height, and Rio-Hortega had refused to accompany Penfield to meet his old mentor. Penfield remarked on the sad state of affairs:

Rio-Hortega, who has time to go everywhere else with me, was too busy to go introduce me to Cajal. Of course it was because he has had difficulty with his old mentor… Cajal. The trouble arose when Rio-Hortega described something that Cajal had written about as microglia and apparently Cajal did not agree with him. Of course Cajal started working as a young man in Spain and is now 72. When he began there was no productive work here in biology. He did investigation with much the spirit of Pasteur and published every year the fruits of his labor, chiefly on the nervous system, in Spanish, a language which at that time was practically unknown to men of science….Recently the work has attracted more general interest but as the methods cannot be easily re-produced, little more than interest and very scrappy knowledge has been accorded the Spanish school. I think they are looked upon with much respect and some suspicion.  

Penfield continued, describing his first encounter with Cajal:

He passed his hand over a long shelf of books. “Look at those buried, almost lost in Spanish. Every day I read some publication, especially in German, published as new work and it was done here carefully by many methods, sometimes 35 years ago….If my young pupils do not take care the Spanish School will be lost sight of.”

**Neurocytology**

By the time he returned from Spain in September of 1924, Penfield was the undisputed master of the ‘Spanish methods’ of cytology and pathological anatomy. These methods would form the cornerstone of his emerging interdisciplinary approach to neuropathology. Penfield immediately proposed the creation of what he termed a ‘laboratory of neurocytology’ at the Presbyterian Hospital, “for the study of the central nervous system, particularly by means of staining methods used by certain Spanish investigators.” Penfield’s plans for the use of the Spanish techniques were ambitious. The laboratory would gather pathological samples from the Presbyterian pathology labs and the surgical laboratories of Columbia College of Physicians and Surgeons, and would also work with animal samples from Columbia, and animal material maintained on-site. Neural material from

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69 Penfield to Jean Jefferson Penfield, 11 May 1924, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
70 Ibid.
71 “Proposed Laboratory of Neurocytology,” A/N 16-1, Box 14, Wilder Penfield Fonds.
around the city of New York would flow to Penfield's small laboratory, and be subjected to the illuminating Spanish staining techniques. Penfield even obtained cooperation from his nemesis at the New York Neurological Institute; Tilney agreed to courier their pathological samples to him for analysis, as the NYNI had no laboratory on site.\(^{72}\) Elsewhere, he displayed a keen sense for how to promote his new techniques as a promising advance in the study of intractable medical problems. In a proposal outlining his ‘laboratory of neurocytology,’ Penfield argued that:

There remains [a] large group of mental and nervous ailments about the causes of which little or nothing is known. It is evident that the complaints of the patients are not imaginary and yet microscopical examination of the brain and nerves of such cases has not yet yielded the secret that must be hidden there. Examples of the commoner of such uncontrolled infirmities are epilepsy, neuritis, insanity and possibly certain types of so-called hysteria.\(^{73}\)

In the same document, Penfield argued that the physical location of the laboratory was crucial to its success. “Having a laboratory next to the operating room” would allow for neurologists and neurosurgeons to “see the operative specimens,”\(^{74}\) and get quick feedback, tightening the loop between diagnosis, operation and post-operative study. Penfield also felt that this interdisciplinary orientation would make the proposed laboratory particularly appealing to the Carnegie corporation. In a letter to his former Sherrington laboratory mate Cuthbert Bazzett, he wrote that “I could put it up to the [Carnegie] Corporation as a combined team back[ing] of a research problem, and…it would hit it because the Trustees of that fund are very much in favor of breaking down inter-departmental and interscholastic division, in the consideration of any particular research problem.”\(^{75}\) Penfield concluded that “Within the next five years these Spanish methods are certainly going to have a tremendous vogue throughout the country, as people are constantly asking me to show them to them.”\(^{76}\)

\(^{72}\) Ibid.

\(^{73}\) “Proposed Laboratory of Neurocytology,” C/G 3-1/1, Box 43, Wilder Penfield Fonds.

\(^{74}\) Ibid.

\(^{75}\) Penfield to Cuthbert Bazett, 21 January 1925, C/G 2-2, Box 43, Wilder Penfield Fonds.

\(^{76}\) Ibid.
From a modern perspective, where the use of laboratory techniques to compare diseased tissue with healthy tissue, and animal cells with human cells, is a commonplace in medical research, it is easy to lose sight of what an ambitious and unique approach Penfield’s proposed laboratory represented. Simply put, neurosurgeons of the 1920s - even ambitious and smart ones - didn’t do this kind of thing. At the time, the most prominent neurosurgeon in America, Harvey Cushing, who had professionalized the field in the 1910s, was less enthusiastic about the scientization of medicine, as represented by the Flexner Report of 1910, and the growing use of laboratory technology in American hospitals. Indeed, while Cushing had brushed up against many of the figures that had shaped Penfield’s approach, he seemed to take significantly different lessons from them. Cushing had not absorbed Osler’s belief that neurology and neurosurgery ought to be served by a single combined profession. He had spent a brief period in Charles Sherrington’s laboratory, but was positively dismissive of his approach. And while Cushing was not dismissive of the value of laboratory pathology, he was largely unwilling to learn the techniques himself. Cushing’s most important scientific research was on the pituitary gland, and his greatest scientific contribution, the classification of brain tumors, was done mainly by his then-assistant Percival Bailey. Bailey, who was interested in histological research, had been unsuccessful in convincing Rio-Hortega to come to America, and had been unwilling to undertake the language training necessary to learn the Spanish staining methods. In fact, when Bailey and Paul Bucy published their definitive findings on the histology of oligodendroglial tumors, they were largely reliant on Penfield’s improved staining methods.

While Penfield’s research approach may have been anathema to Cushing, it was considerably more appealing to another set of actors on the American medical scene in 1926. In many ways,

Penfield was paddling up the crest of a wave of biomedical funding that was just about to break. As numerous historians have documented, the rise of laboratory-based biomedicine in the United States in the twentieth century was very much the result of the funding and guidance provided by private philanthropic foundations such as the Carnegie and Rockefeller Foundations. As Jack Pressman succinctly put it, “In some measure, to tell the story of the Rockefeller Foundation is to trace the ascendance of the laboratory.” Penfield’s laboratory of neurocytology seemed to be a perfect fit.

Indeed, the Rockefeller family had paid for Penfield’s initial trip to Spain, albeit in a less formal sense. After operating on the daughter of Percy and Isabel Rockefeller, Allen Whipple had been able to secure from her the initial funding for Penfield’s Spanish voyage. Upon returning to New York in 1925, Penfield dispatched a letter to Isabella that made the effects of his trip clear:

In Madrid there is a sort of scientific oasis. Dr. Ramon y Cajal is the center of it and about him has sprung up a group of brilliant disciples that are doing valuable work in neurology with little recognition from the outside world, least of all from Spain. They have had little opportunity to apply their new methods to disease problems and there lies the value of their work. Since returning I have taken every opportunity to tell and write about their work for they long for recognition above all else, and I have received a number of requests for instruction in the Spanish methods of staining nervous cells with silver and gold compounds. The growth of the Spanish School of Biology is a real romance in the scientific world. Thank you for helping me to discover them. That sounds as though they were unknown before. That is not true of course. But scientific men rarely read Spanish and no Anglo-Saxon had gone there to study before.

80 Penfield, No Man Alone, 94.
81 Penfield to Isabella Rockefeller, 25 January 1925, C/G 2-1, Box 43, Wilder Penfield Fonds.
Penfield’s letter reflected the degree to which his time in Spain had informed both his laboratory technique, and his philosophy on scientific organization, and he was ultimately successful in securing from Isabel Rockefeller a sum of $24,000 to endow his new laboratory for two years.\(^8^2\)

This sum of money also brought two additional figures into Penfield’s circle. The first was William V. Cone, a young doctor from Iowa then on a National Research Council fellowship. Cone, who had worked in the laboratory of Samuel Orton (a pioneer in the field of learning disabilities), had come to New York originally to train at the NYNI but, finding that Tilney had no laboratory on site, had made his way over to the Presbyterian Hospital, and Penfield’s emerging neurocytology lab. In the coming years, Cone would re-train as a neurosurgeon, master the Spanish methods of staining, and become Penfield’s “alter ego,” and lifelong surgical partner.\(^8^3\) The second was Edward Dockrill, a moody and nomadic Cockney who had begun work as an orderly at the Presbyterian Hospital, and became an assistant to Penfield prior to his trip to Spain. Dockrill had initially lied about his training as technician at the Queens Square National Hospital in England, but showed such an aptitude for laboratory work that when the deception was discovered, Penfield decided to keep him on at the Neurocytology lab, despite his frequent clashes with hospital staff. Around Penfield and the Spanish methods, a small circle of interested parties began to accrue.\(^8^4\)

At the same time, Penfield’s scientific vision expanded outwards. Since January of 1927, he had begun to contemplate a book on neuropathology and the staining of nerve cells. He had received enthusiastic support from his friend Adolf Meyer, then the most well-known psychiatrist in

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\(^8^2\) The money given by Isabella was private, but she later recommended that Penfield apply to Rockefeller’s General Education Board for formal funding. Penfield, *No Man Alone*, 120.

\(^8^3\) Penfield, 118.

\(^8^4\) Penfield and Cone spoke about their work at a conference in Atlantic City in May of 1927. Following their presentation, they received three applications to come to the laboratory and learn the methods. Penfield to Jean Jefferson Penfield, 29 May 1927, Penfield to Jean Jefferson Penfield, 9 January 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
America, as well as others.\textsuperscript{85} However, the sheer size of the undertaking was overwhelming. In a letter to his mother, Penfield expressed his concern that the hypothetical book would “embody only what I know already and would mean no advance but consume much time.”\textsuperscript{86} He continued, mixing research concerns with his own anxieties about professional advance and financial security: “If we throw the book over I can put that time in on constructive research and developing the clinical field, and goodness knows our family needs that….What’s the use of scientific distinction if you can’t meet life’s emergencies with something besides financial worries? My practice is rotten right now.”\textsuperscript{87}

His solution to the problem was to appeal to a growing international community of microscopists, starting first with his Spanish collaborators. In 1927 Penfield wrote to both Cajal and Rio-Hortega asking them to contribute to what would eventually become a three volume, 1200-page edited collection entitled \textit{Cytology and Cellular Pathology of the Nervous System} (1932).\textsuperscript{88} Still considered a classic in the field, the collection was the first dedicated English-language study of the microarchitecture of the human nervous system, and contained contributions from around the globe. As editor, Penfield’s internationalist vision was evident in the volume’s preface:

…the editor may describe the motives that brought the book into being…. His own initial desire for complete knowledge of the microscopic structure of the nervous system must be shared by every student of biology at some time. This naive desire led him to a still more naive determination to write a cytology of the nervous system single-handed. But more intimate acquaintance with the field of cytology soon showed its boundary to be like the horizon, since it recedes no matter how far one may travel or in what direction. In short, authoritative description of the cytology and cellular pathology of the nervous system is an impossible task for one man or, indeed, for the microscopists of one country. It has therefore been necessary to appeal to men working in different parts of the world to contribute separate chapters….The work is therefore an international, and not in any sense an individual, contribution to science.\textsuperscript{89}

\textsuperscript{86} Penfield to Jean Jefferson Penfield, 14 August 1927, Penfield to Jean Jefferson Penfield, 9 January 1921, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.  
\textsuperscript{87} Ibid.  
\textsuperscript{89} Penfield, vii.
Behind the scenes, Penfield had to battle with his publisher about the multinational character of the volume. “I have not given in to Hoeber, and will not, on the number of foreign chapters. He fears the cost of translation…”

In the same letter, Penfield expressed a concern that his scientific pursuits, and particularly his editorship of *Cytology and Cellular Pathology of the Nervous System*, were beginning to drown out his clinical activities. “I feel scientifically dry. I hope the time of organization will not last too long, so I can get to curing people who would otherwise not be cured, and get close to the clinical problems.”

While he had turned initially to scientific pursuits as a more optimistic activity (and an avenue for advancement) than the rough-and-tumble world of clinical practice in New York, by the late 1920s he began to express a frustration that he was unable to pursue more clinical work. A feeling of alienation from his surgical practice began to creep in. Penfield had long nurtured a notion of combining clinical neurology with surgical practice. As early as 1922, while considering an offer to join Johns Hopkins, Penfield wrote of the desirability of a combined neurological and neurosurgical clinic. “It is along that line that the real future of neurology lies.” By combining neurologist and neurosurgeon in a single person by way of solid physiological knowledge, he hoped to fulfill the vision laid out by his mentor William Osler in 1907. Yet by the late 1920s, Penfield’s vision for a combined neurological/surgical institute had shifted in a subtle, but profound way. His experience in Spain had altered his view of the relationship between the laboratory and the clinic. In a letter to Isabel Rockefeller thanking her for her continued support of the laboratory, Penfield wrote: “To be able to make a group attack on the problems of neurological surgery with really adequate facilities will make these three years of the laboratory of neurocytology memorable ones for me….To do this

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90 Penfield to Jean Jefferson Penfield, 26 September 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
91 Ibid.
sort of research is a joy for me. It is also a sort of expression of my philosophy of life.”

If Penfield’s ambition was to master the nervous system, then the locus of that mastery had shifted from the individual to the group, and from the clinic to the laboratory and back again.

At about the same time that he was developing a new plan for a group approach to the problems of the brain, a visitor to his New York practice provided an avenue to escape what Penfield saw as an unsuitable environment. Penfield’s emerging vision of an institute that could encompass neurology and neurosurgery, and the laboratory and clinic, was an intrinsically social vision. Although he was certainly a man with a robust sense of self, his statements about the value of collaboration, across disciplines and across borders, were genuine. If the Spanish methods of Cajal and Rio-Hortega had supplied the means, then the Spanish laboratories had supplied the model. At the same time, his position in New York made the fulfillment of such a vision difficult. Butting up against the professional ambitions of other neurologists and surgeons rankled, and more importantly, prevented access to the surgical and pathological material that would be needed to fulfill his scientific ambitions. If his vision for an interdisciplinary and internationalist ‘group’ approach to the brain was an intrinsically social vision, then New York was the wrong society.

While mulling over these concerns, Penfield encountered a fellow surgeon who would fundamentally alter his professional life. On 8 June 1927, Penfield received a letter from Edward Archibald, the chief of general surgery at Montreal’s Royal Victoria Hospital. Archibald had, in fact, been Canada’s first neurosurgeon, although in this case the title was somewhat misleading. Trained in medicine in the late nineteenth century (he received his M.D. in 1896), Archibald was a member of an older generation of doctors who came to neurosurgery by way of general surgery (his 1908

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92 Penfield scratched out “religion” in the draft, and replaced it with “philosophy.” Penfield to Isabella Rockefeller, 21 May 1927, C/G 2-1, Box 43, Wilder Penfield Fonds.
monograph on the topic, *Surgical Affection and Wounds of the Head*, gives a sense of his approach). Wishing to specialize in thoracic surgery, Archibald was looking for a young surgeon who could take neurosurgical cases off his hands. “The field is open; there is nobody else doing it in Montreal.”

Penfield responded with a voluminous list of demands, should he accept. He insisted that “Neurosurgery must go hand-in-hand with Neurology” and that “there must be adequate provision for research as well as the development of clinical and operative team work.” Such an arrangement would require segregated surgical beds, a neurosurgical intern, a research fellow, academic standing for the neurosurgeon “equal to that of the neurologist,” an association with all of the city’s major hospitals in order to ensure enough patients, and most importantly, a neuropathological laboratory. From this laboratory would “come most of the publications,” and would be a place where “the staff of neurology and neurosurgery should work together with common interest and mutual benefit.”

In a letter to his mother, Penfield put it more bluntly: “this lab would be the heart of the Clinic. It is here that men should be attracted to come from away. And it is here where most of the investigation should be done.” Following a visit to Penfield in New York, Archibald replied that “If [you come to Montreal] I expect that ten years from now the Hub of surgical neurology, in this continent, will be transferred from Boston to Montreal.”

Penfield accepted the offer from Archibald in January of 1928, noting to his mother that “it boils itself down to the fact that Montreal offers a chance to build a complete clinic, to be a

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94 Edwin Archibald to Penfield, 8 June 1927, A/M 11/1-1, Box 4, Wilder Penfield Fonds.
95 Penfield to Archibald, 17 July 1927, A/M 11/1-1, Box 4, Wilder Penfield Fonds.
96 Ibid.
98 Penfield to Archibald, 9 August 1927, A/M 11/1-1, Box 4, Wilder Penfield Fonds.
personality, i.e. - "To have a place in a community that does not drown you." However, he secured a provision that he be granted a six-month leave before coming to Montreal so that he might tour the surgical clinics of Europe. Penfield began a series of German lessons so that he might converse with the man he most wanted to meet, Otfrid Foerester, in his native tongue. In March of 1928 he boarded the S.S. America for Europe, along with his wife and growing family of four children.

**Foerster and the Clinic**

Penfield arrived again in Europe in April of 1928, hoping to meet the man who might help him translate his laboratory investigations into clinical practice. Otfrid Foerster, who at this point ran his own clinic in Breslau, had in many ways combined neurology and neurosurgery in just the fashion that Penfield had endorsed. As Katja Guenther has shown, Foerster's project at Breslau was, in many ways, the mirror image of Penfield’s. Trained by the first generation of professional neurologists, notably Karl Wernicke, Jean-Martin Charcot, Wilhelm Henrich Erb and others, Foerster had watched as neurology began to separate itself from psychiatry and neuropsychiatry in Europe, primarily through its clinical practice of expert diagnosis and localization of neurological disorders. Now, Foerster hoped to bolster neurology's limited therapeutic armamentarium by methods such as physical therapy and surgery. His attempts at surgery had produced exciting results, particularly in the area of epilepsy. World War I left Foerster with a wealth of clinical material in the form of soldiers suffering from epileptic seizures as a result of gunshot wounds that had left scars

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99 Penfield to Jean Jefferson Penfield, 29 January 1928. Against this stain of grandiosity, Penfield fought hard to ensure that his partner, William Cone, would accompany him to Montreal. This involved briefly battling his surgical mentor, Allen Whipple, who wanted Cone to remain in New York. “I really believe he [Cone] will be better off in Montreal for the next five years and I am ready to plan a big clinic, not an individual show.” Penfield to Jean Jefferson Penfield, 5 February 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
on their brains. To treat these patients, Foerster developed an innovative surgical procedure: using local anesthetic to allow for operation on a conscious patient, Foerster used a gentle electrical stimulator to probe the brain's surface, allowing him to ‘zero in’ on the epileptogenic focus while sparing healthy and vital brain tissue. Penfield had first heard of the therapy in New York, and hoped to learn more about it in Germany. “Our headquarters shall be established in Breslau if Prof. Foerster proves to be the sort I suspect,” Penfield commented. “I shall study German very hard, and later take side trips.”

Much has been made of Penfield’s adoption of Foerster’s surgical technique. Indeed, Penfield’s ability to operate on conscious epileptic patients, who could report the experiences provoked by his electrical probe, would later make his international reputation; more than that, it

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100 It is worth pausing here to consider Penfield’s use of the phrase ‘clinical material’ in reference to patients. I have used the term repeatedly, both in direct quotations and in text, in order to preserve a sense of Penfield’s state of mind, and to avoid anachronism. At the same time, historians and critics of medicine from Michel Foucault to Ivan Illich have seen this phrase, and the attitude it represented, as the epitome of the dehumanization of patients by the so-called ‘clinical gaze’ of modern medicine, one that reduced humans to a laboratory specimen, and produced a kind of ‘therapeutic nihilism’ that became particularly rampant in the anatomo-clinical medicine that dominated Europe in the nineteenth century. Penfield’s use of the phrase, however, deserves some qualification. Certainly, Penfield did not see the use of this phrase as representing a lack of compassion; indeed, as will be seen in Chapters 1 and 2, it would be difficult to accuse Penfield of a lack of concern for the patient ‘as a person.’ Much of Penfield’s later work with psychologists was meant not only to study the cognitive deficits produced by operation, but to alleviate them; thus for Penfield study of ‘clinical material’ was an act of compassion, meant not only to advance knowledge for the sake of future patients, but for those already under his care. Moreover, Penfield’s training with Osler had convinced him that the anatomo-clinical method could in fact be a form of humanistic medicine if it was supplemented with a deep understanding of the living patient, their life details, and an awareness of their suffering. At the same time, Penfield’s use of the phrase ‘clinical material’ is indicative of his desire to erase the boundaries between the clinic and the laboratory; for Penfield, the laboratory was as much a part of the healing experience of medicine as the clinic. For more on issues related to ‘clinical material’ and the clinical gaze, see Michel Foucault, The Birth of the Clinic (New York: Taylor & Francis, 1963); E. H. Ackerknecht and C. E. Rosenberg, A Short History of Medicine (Baltimore: Johns Hopkins University Press, 2016); Ivan Illich, Medical Nemesis: The Expropriation of Health (New York: Pantheon, 1976).


102 Penfield to Jean Jefferson Penfield, 26 February 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
transformed him into a celebrity brain surgeon, a philosopher of the mind/body problem and an authority on social issues. However, two additional aspects of his 1928 European sabbatical are worth examining in some detail. The first is the nature of his collaboration with Foerster.

Penfield was immediately impressed by Foerster's surgical technique. Penfield had already experimented with a surgical treatment for epilepsy that involved extensive removal of scar tissue. However, Foerster's approach, and his scientific character, impressed him:

He has the best mind with which I have come in contact with exception of Sherrington and their mental type, simplicity, accuracy and logic are very much alike. Neither has any pretense. I get much from Foerster for he has done what I want to do and deals with the same problems. He doesn't operate as well as we do in many ways, but he is wiser in many ways.

However, while Foerster impressed Penfield with his skill and knowledge, he proved less capable as a laboratory scientist. Foerster had been collecting the excised scar tissue from over a dozen surgical patients, but he lacked the laboratory skills to analyze them himself. He had previously sent portions of the samples to the well-known German neuropathologist Wilhelm Spielmeyer, but because he was restricted to traditional staining techniques, his findings had been unhelpful. Equipped with his now well-practiced Spanish methods, Penfield set up a small laboratory next to the patient beds in the Breslau clinic. “[Foerester] did a splendid operation removing a 16 year old gunshot wound

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104 Penfield, No Man Alone, 156-8. The patient, a young boy named Willie Hamilton, had been struck on the head and at the age of 15, and had later developed epilepsy. Penfield’s treatment involved removing the epileptogenic scar tissue with a method that prevented the creation of an additional scar. In Hamilton’s case, this involved a radical removal of nearly half of the boy’s right frontal lobe (quite possibly the first successful frontal lobectomy). The role that this operation played in the incorporation of psychologists into the Montreal Neurological Institute’s interdisciplinary team will be examined in Chapter 2.
105 Penfield to Jean Jefferson Penfield, 4 June 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
106 Penfield, No Man Alone, 164.
scar from the brain….He made it possible to get hold of some chemicals and I took the specimen for examination. During the next three or four days I worked hard at the tissues and succeeded in getting some very pretty stains of different kinds of glia cells. In such tissue lies hidden some of the secrets of epilepsy, if not the whole story.”

Although the ultimate pathology of traumatic epilepsy eluded Penfield and Foerster (it was still unclear exactly what about the scar led to convulsions, although Penfield maintained a fondness for the idea that the contraction of the scar tissue over time might irritate the rest of the cortex in an undefined way), the histological samples did show conclusively that the scar tissue produced by injury of the brain was different from the results of Foerster’s surgery, which left the patient seizure-free. Penfield’s experimental studies of brain wounds in rabbits, conducted with Rio-Hortega in Spain four years earlier, showed that the process of scar formation in rabbits (which frequently produced epilepsy) was the same as in Foerster’s epileptic patients, occurring slowly over time, and coinciding with the onset of seizures months or years later.

Penfield’s histological examination was in many ways an exemplar of the kind of laboratory-clinic collaboration that he envisioned, and Foerester’s operative technique gave him a way to tie clinic and lab together.

The second aspect of Penfield’s European sabbatical that deserves examination is his survey of European clinics and laboratories. Penfield travelled, often with Foerester, to a number of neurology clinics and neurohistology laboratories, and submitted a detailed report of his observations to the Rockefeller Foundation (a surprising act, given that the foundation had never asked for such a document). The nineteen-page report submitted by Penfield (see Appendix 1) is a

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107 Penfield to Jean Jefferson Penfield, 22 April 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
109 Wilder Penfield, “Impressions of Neurology, Neurosurgery, and Neurohistology in Central Europe.” W/U 17, Box 155a, Wilder Penfield Fonds. The text of this report is included in Appendix 1.
remarkable survey of the state of the neurological sciences in Europe, and is even more revealing of
Penfield’s growing sense of the importance of interdisciplinary collaboration, and the strengths and
weaknesses of different national scientific and medical cultures. “At the present moment,” wrote
Penfield, “there is no other branch of medicine in which clinic organization is of greater importance
than in the study and treatment of nervous and mental disease.”\textsuperscript{110} Tying disciplinary and national
cultures together, he went on to note that “If I presume to criticize [different clinics] then it is
against the background of my familiarity with English Neurology, American Neurosurgery and
Spanish Neurohistology, and not from any personal feeling of superiority.”\textsuperscript{111} What followed was a
revealing glimpse not only of European science and medicine, but of Penfield’s own ideas about
how to integrate the neurological sciences. Foerster’s clinic came in for the greatest praise: “So far as
my experience goes neurosurgery in this clinic is unequalled outside of the United States and such a
well-balanced combination of sound neurology and neurosurgery is to be found nowhere [else].”\textsuperscript{112}
However, Penfield made his opinion clear on what he considered Foerster’s great weakness. “The
complete lack of neuropathology in this clinic is its greatest drawback.”\textsuperscript{113}

Penfield took shots at a number of clinics, laboratories and cities that he felt were in some
way retrograde. For instance, of Viktor von Weizäcker, Penfield reported that “There seemed to be
little enthusiasm and no original points of view in this clinic. The mere creation of a department of
Neurology does not seem to have improved upon the condition of affairs found in Berlin.”\textsuperscript{114} Paris
came in for particular scorn: “It is obvious that the successors of Marie and Dejerine (Guillain and

\textsuperscript{110} Emphasis in original. Ibid.
\textsuperscript{111} Ibid.
\textsuperscript{112} Ibid.
\textsuperscript{113} Ibid.
\textsuperscript{114} Ibid.
Crouzon) are not the teachers that their masters were.”\textsuperscript{115} However, the most common lament for Penfield, even regarding clinics and men he admired, was the lack of integration between lab and clinic, neurology and neurosurgery, anatomy and physiology. Spielmeyer’s pathological laboratory could be faulted for its “isolation” and “little direct connection with clinical problems and … clinical points of view.”\textsuperscript{116} Even the clinic of Bernardus Brouwer in Amsterdam, which Penfield described as having “the most complete neurological organization of any in Europe,” was found to be inadequate. In Brouwer’s clinic “ward and laboratory are interrelated in such a way that all assistants have some activity in both places,” but “the laboratory is an anatomical rather than a pathological one,” and “Brouwer, though a splendid anatomist, was deficient as a clinician.”\textsuperscript{117} Again and again, Penfield found much to admire in each city and nation, but no clinic that could match the emerging vision in his own mind.

The Kaiser Wilhelm Institute für Hirnforschung deserves a brief digression. Founded in 1914 by Oscar and Cecil Vogt, the institute was, in many respects, the leading center for brain research in Europe for much of the 1920s. The Vogts’ primary method was careful histology of the human and animal brain. Employing over a dozen assistants and technicians, the Vogts launched the specialized field of cytoarchitectonics through painstaking work with the giant microtome. Thin slices of an intact brain were examined microscopically, and cell types were described and photographed in an attempt to correlate areas of the brain with psychological functions – in effect a kind of micro localization. Penfield was certainly overawed by the scale of the Vogts’ enterprise: “To prepare one brain for study requires the exclusive work of one technician for one year…Three of the technicians have been with Vogt for twenty years. Two assistants do nothing but photography.

\textsuperscript{115} Ibid. In a private letter, Penfield put is more bluntly. “I have little respect for Parisian science right now.” Penfield to Jean Jefferson Penfield, 26 August 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
\textsuperscript{116} Ibid.
\textsuperscript{117} Ibid.
sections. The preparations are literally crowding them out…” Yet Vogt had, at that time, no clinical beds or physiologically-trained assistants.118

Penfield concluded his report with a discussion that tied together many of the themes of his career up to that point, and set an agenda that was both ecumenical and ambitious. He admired particularly the clinic of Brouwer, where “the chief understands both the microscopical and clinical aspects of neurology.”119 In the clinic of Foerster “neurology is…unexcelled, diagnosis is accurate and….neurosurgery has reached a state of development not to be equaled in any other European clinic.” And “neurohistology [is] brilliantly done in the laboratories of Vogt and Bielschowsky” but “these men are all essentially neurologists, [and] the microscopy of the brain must belong to both psychiatry and neurology.” The inclusion of psychiatry here was important. By incorporating neurology, neurosurgery and neuropathology, Penfield’s ideal neurological clinic could overcome the disciplinary division between neurology and psychiatry that had grown in Europe since the turn of the century.120 “My personal conclusion is that the division, which can never be hard and fast, must eventually fall between organic and functional….It may be of course that some of these [functional] cases will be found to be organic some day, which is the strongest argument for complete facility for neurohistology in psychiatric departments.”121 Ultimately, Penfield’s European sabbatical did more than supply him with the surgical technique of Foerster; it reinforced his conviction that only by tightening the links between the microscopic and the clinical, and between European neurology and American surgery, could there be “hope for solution of some of the riddles presented by the sufferers from nervous and mental diseases.”122

118 Ibid.
119 Ibid.
120 For more on this division, see Guenther, Localization and Its Discontents: A Genealogy of Psychoanalysis and the Neuro Disciplines, 96–125.
121 Ibid.
122 Ibid.
At the crossroads of scientific medicine

By the time he returned to North America in September of 1928, much of the plan for what would become the Montreal Neurological Institute (MNI) was latent in Penfield’s mind. Yet many challenges remained. The creation of an institute that could encompass his vision for an integrated neurological clinic would take six more years and entail a number of challenges and reversals of fortune. For instance, almost immediately upon his acceptance of the position from Archibald, the initial offer from the Royal Victoria Hospital to pay for the costs of the pathological laboratory was rescinded, and alternative arrangements had to be made. Moreover, Penfield had to secure the cooperation from the city’s French and English-speaking neurologists in order to ensure a sufficient supply of patients for his practice, and for examination in the neuropathological laboratory that he and Cone were able to cobble together. Indeed, in order to establish their pathological laboratory, central to Penfield’s vision of an integrated clinic, he had to circumvent the pathology department of the Royal Victoria Hospital entirely, as it was currently headed by an ornery pathologist who was hostile to the entire venture.123 What once seemed a rosy outlook for a neurological clinic separated from the professional jealousies of New York now seemed imperiled. A frustrated Penfield confessed to his mother in 1928 that:

I’m a little frightened today, to tell you the truth. To build up a clinic and practice in the most narrow of all specialties here in the Province of Quebec where the majority are French Catholics, seems to me a doubtful undertaking. I am willing to develop a clinic under some such institution as the Rockefeller, but to expect to build up a practice quickly that will support us - I don’t know….Now, I have no salary and the guarantee of $10,000 yearly income is made rather informally through Archibald by a friend of his. The Laboratory is supported by gifts of friends of Archibald also rather than by the more secure Hospital or University.124

123 The period between 1928 and 1934, when Penfield consolidated his practice in Montreal, and constructed the Montreal Neurological Institute, could easily fill its own chapter. For brevity’s sake, readers who wish for more detail should consult Penfield, No Man Alone, 183–338.
124 Penfield to Jean Jefferson Penfield, 26 September 1928, D-C/D 33-3 1917-1928, Box 41a, Wilder Penfield Fonds.
Yet over time, Penfield won over the city’s neurologists, French and English alike, and secured a more permanent source of funding for the laboratory from local backers. By the end of 1928, he felt ready to make an approach to the Rockefeller Foundation in order to secure a grant to build his institute. It came as something of a shock to Penfield, then, when his first proposal was refused. Despite its obvious connection to the Rockefeller Foundation’s mission in medical science, one that was increasingly embracing an interdisciplinary approach, Richard Pearce, then the director of the foundation’s medical philanthropy, refused Penfield’s proposal after a one-on-one interview in February of 1929.\footnote{The reasons for Pearce’s initial refusal are somewhat complex. At the time, the Rockefeller Foundation was deeply interested in funding neurological ventures, but had overextended itself, funding the clinics of Vogt and Foerster in Germany. Meanwhile, the foundation had also received proposals for institutes from Philadelphia and Boston (Penfield expressed some surprise that one of the applicants was Cushing – it is unclear how much scientific research Cushing had proposed). Also, McGill had been the recipient, in 1920, of a considerable Rockefeller grant for medical education, and it seems likely that Pearce simply felt that McGill had overplayed its hand. Finally, he suggested to Penfield that the local governments would have to commit a comparable sum of money for patient care, as the foundation would not pick up the bill for hospitals stays – only laboratories and research. Penfield, \textit{No Man Alone}, 226–28.}

Although a blow to his ego, it proved to be only a temporary setback. As Robert Kohler has observed, the priorities of the philanthropic foundations that funded the bulk of American science spending before World War II were, as often as not, determined by the individual personalities of the foundation officers involved.\footnote{Kohler, \textit{Partners in Science: Foundations and Natural Scientists, 1900-1945}, 406.} As such, Penfield’s report on the state of the neurological disciplines in Europe, which stressed the value for psychiatry and mental health of an integrated approach to mind and brain, found its way into precisely the right hands. Alan Gregg, who received Penfield’s report in 1928, was the foundation’s champion for the distinctly American doctrine of ‘psychobiology,’ which stressed the interrelation of mind and brain, and an interdisciplinary approach.\footnote{Pressman, \textit{“Human Understanding: Psychosomatic Medicine and the Mission of the Rockefeller Foundation”}; Kohler, \textit{Partners in Science: Foundations and Natural Scientists, 1900-1945}, 244.}

When Richard Pearce died suddenly of heart failure in 1930, Gregg was moved into a position in which he could put Penfield’s vision into action. Indeed, it seems likely...
that, had Gregg never been moved into such a central position at the Rockefeller Foundation, Penfield's plan would never have come to fruition; Pearce's death moved Penfield's ideological ally right to center of philanthropic power. As Jack Pressman puts it, "each of the major research sites funded by Gregg had at its core a commitment to the collapse of disciplinary borders, and a faith in psychobiological unity." It should come as no surprise, then, that Penfield's proposal for the Montreal Neurological Institute was the very first that Gregg put into motion.

The founding of the MNI was itself something of a dramatic epic, which could easily fill an additional chapter. One aspect, however, deserves consideration. That Penfield's institute would take shape in Montreal was not immediately obvious. As negotiations with the Rockefeller Foundation developed, Penfield received a counter offer from the University of Pennsylvania that nearly tempted him away from Quebec. Two factors stopped him. First, the Philadelphia offer would dramatically curtail Penfield's own ability to conduct research, as his position would involve much more in the way of administrative duties. Second, and more importantly, Penfield had come to see Montreal as having a strategic position on the North American continent that was advantageous. In October of 1931 Gregg visited Penfield in Montreal, while the offer from Philadelphia was still on the table. As Penfield recalled in his autobiography:

I told him what I had been thinking. Montreal is a quieter place for study, I explained, quieter than New York or Philadelphia or even Baltimore. Tradition and awareness link Montreal with Europe, especially Great Britain and France, as well as with the United States. Our location here, above the American border and just off the main highroad to the great American university centers - might well prove to be the best place in which to be influenced by the work in other centers. It might be the ideal place in which to do constructive scientific work on the brain and the mind of man, work that might in time influence thinking in other centers.

It is difficult not to see a resemblance between Penfield's description of Montreal, as a liminal space between Europe and the United States, and his romanticized vision of the Instituto Cajal in Spain.

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129 Penfield, No Man Alone, 298.
Yet Penfield’s international experience now revealed a paradox; in order to enact his interdisciplinary vision, he would have to embrace the local. His quest to become a physiological surgeon had taken him from the United States to England, Spain and Germany, and it therefore seems fitting that, while on a trip to Europe in 1932 to recruit employees for his new institute, Penfield had a final revelation about his mission shortly after visiting his old friend Rio-Hortega at his laboratory in Spain. According to Penfield, Rio-Hortega “had mellowed, hair greyed a bit, but he was the same high-strung, sensitive, quick-moving little man….He is still in charge of the Residencia Laboratory where I worked for four months….but [he] is now an important figure and Director of the National Cancer Research Institute, where he spends his mornings and has elaborate and satisfactory equipment, as good as ours will be.” Following their meeting, Penfield and his wife attended a concert:

I had been rather depressed after leaving London as far as our plans (re Institute) were concerned….[At the concert later] The orchestra was all Spanish, and here cut off by the Pyrenees it was playing lovely Spanish music that made me thrill. At one high point a sigh passed like a wind through the responsive listeners. Suddenly it dawned on me that it was so well done because it was themselves doing it on their own. I thought in a flash of the good English, good Dutch, good French, good Portuguese and good Spanish clinics I had seen, and I knew that I must give up transplanting anyone and build up a school of Canadians in Montreal, for Montreal, and do it all on our own and from our own students….If it is not possible to do it with Canadians it must be done by those who want to become Canadians, so that eventually there may grow up a local school of Neurology.130

Penfield’s ‘vision’ for a Canadian school of neurological science, built up in the cosmopolitan and bilingual city of Montreal, could easily be dismissed as a sentimental recreation, or an attempt to curry favor with his Canadian patrons. However, as this chapter has endeavored to show, his scientific biography argues against such an interpretation. When Penfield finally attained a grant from the Rockefeller Foundation to build his institute in 1931, his plans for the building reflected his own sense of how an interdisciplinary clinic ought to be organized (see figure 1.1). It seems

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reasonable to think, then, that his statements about the importance of the institute’s location were just as much a reflection of his idiosyncratic vision. In a pamphlet published for the opening of the Montreal Neurological Institute on 27 September 1934, Penfield noted that Montreal “more than...any other city on this continent ...is situated on a cross-roads of scientific medicine.”¹³¹ In the coming years, the various travelers who would meet at that crossroads would transform the neurological sciences.

This chapter has attempted to keep the narrative focus on the intellectual precursors to the MNI’s institutional ethos, which were embodied in the biography of its founder. Penfield had initially hoped that, by becoming a physiological surgeon, and extending his approach from its microscopic architecture, up through its gross anatomy, he might encapsulate all knowledge of the human nervous system, and stake out a professional identity as its primary interpreter and healer. His experiences in Spain convinced him of the centrality of the pathological laboratory to his project, but also that he would be unable to master all of the appropriate disciplines himself. In the coming years, this ethos of cooperation and collaboration across disciplines would encourage Penfield to incorporate more and more scientific and medical fields into the MNI’s institutional structure. Thus, the MNI served as an incubator for an improvised approach to interdisciplinarity that could fairly be called an embryonic form of neuroscience, one whose distinct style was shaped by the international character of its participants. In Chapter 2, we will see how, even before the founding of the MNI, the consequences of Penfield’s radical surgeries encouraged him to reach out to the field of psychology, a move which had a number of crucial consequences not only for the MNI, but also for the field of psychology itself. In Chapter 3, we will see how Penfield’s embrace of

a young neurophysiologist, Herbert Jasper, not only transformed his approach to neurosurgery, but also served as a launching pad for a fully-fledged ‘neuroscience,’ one that was distinctly different from that being developed in the United States, most prominently at the Massachusetts Institute of Technology. Finally, in Chapter 4 we will examine an instance in which Penfield’s embrace of an outside discipline - psychiatry - failed. Because the brand of neuroscience that was developing at the MNI was so dependent on the diverse backgrounds of its participants, the remaining chapters will move beyond Penfield’s own contributions, and will examine the biographies and key contributions of a growing circle of collaborators. However, given his centrality to the MNI’s activities, Penfield will remain a central character throughout.

Figure 1.1 - Penfield’s initial sketch for the Montreal Neurological Institute. Note the floors reserved for laboratories and animal research, as well as the arrow pointing toward R.V.H. [Royal Victoria Hospital].
On 12 December 1928, Wilder Penfield could be found with his surgical colleague William Cone, and his laboratory technician Edward Dockrill, in a surprising place: combing through piles of trash in Montreal’s Rosemont landfill. The dumping ground, located about 20 minutes from the Royal Victoria Hospital, would seem an unlikely place to find a future celebrity brain surgeon and his most talented collaborators, but the three men were searching for something very precious that had been accidentally lost: a barrel containing over 300 brain tissue samples from their early work at the Presbyterian Hospital in New York City. The barrel had been accidentally thrown out while Penfield and his colleagues were moving their surgical operation from the New York to Montreal, in accordance with the offer given by David Archibald to establish a neurosurgical practice and laboratory at the Royal Victoria Hospital (see Chapter 1). Contained in the barrel were hundreds of brain tissue samples that constituted the bulk of Penfield’s scientific work up to that point. Many of the samples had yet to be analyzed under the microscope, and a great deal of Penfield’s enterprise - to combine the surgical theater with the pathological laboratory - was in jeopardy. Recalling the incident shortly before his death in 1976, Penfield wrote:

I thought of the bits of preserved human tissue in the vanished barrel. We needed them for comparison in future studies. I had removed some of those blocks of tissue at grueling operations. Others I had taken, at autopsy, from the patients I could not save. The men and women and children who died were my friends, as well as patients. I had made a promise to their loved ones. I thought of the malignant tumor from the little Italian boy, the brain abscess from the son of Acosta Nichols, the many abnormalities that had produced epilepsy… and the scars from the New York experimental animals. They were all there. I remembered brave little Jennie Hummel, whose seizures taught us where it is [that] the brain controls the heart and the respiration. She died in one of her strange seizures while I was preparing to operate, hoping to cure. I did the autopsy, instead of the operation, at the scheduled time. Her husband himself asked me to do it, saying, in his anguish, he had to know why.1

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For Penfield, these histological samples had both scientific and moral value - they represented not only the best hope for scientific understanding, but also the sacrifices made by his patients. Always methodical, Penfield and his colleagues drew up a plan to survey the dumpsite to find the missing vials. Plagued by sub-zero temperatures and pained by his sore knee, Penfield had to retreat from the dump site after only a dozen or so vials had been retrieved (the rest would later be discovered under an overturned Ford truck), as his attentions had been called to a different sort of emergency; his sister Ruth was extremely ill.  

Ruth Ingliss had a serious problem. While she had suffered since childhood from ‘fits’ that would often incapacitate her for days, she had been capable of maintaining something close to an ordinary life. Born in 1885 and married to Jack Ingliss in 1905, it was not until the age of 43 that Ruth’s fits became so severe and common that they necessitated treatment. Frequent severe headaches and vomiting had convinced Ruth and her husband that something had to be done, but prospects looked grim. However, Ruth had one important advantage - she had been born Ruth Penfield, and her younger brother was rapidly becoming one of the world’s most respected brain surgeons.  

Ruth travelled with her mother from California to Montreal and arrived on the same day that Penfield surveyed the Rosemont dump for brain tissue samples in 1928. Ruth was greeted by her brother, who immediately took her to his Westmont townhouse. Penfield noticed that Ruth’s walking was unsteady, and that she had trouble seeing objects. Accompanying her upstairs, Penfield produced his ophthalmoscope in order to perform a rudimentary neurological exam.

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2 The Rosemont dump incident is relayed in detail in Penfield’s autobiography. Penfield, 199–206.
3 Penfield’s surgery on his sister Ruth has become something of a legend in the historical writing related to him. The most comprehensive accounts remain those in his autobiography, and the biography of him written by his grandson Jefferson Lewis, who had access to Penfield’s diaries. This account is compiled from the two. See Penfield, 208–21; J. Lewis, Something Hidden: A Biography of Wilder Penfield (Formac Publishing Company Limited, 1983), 118–224.
Penfield’s initial discovery was devastating. The ophthalmoscope revealed that the head of Ruth’s optic nerve was swollen and distended, accompanied by hemorrhaging of the retina. Upon discovering the telltale signs of a massive tumor, Penfield later noted that “My knees grew suddenly weak and for a moment I thought I might fall.”

What to do? Penfield convened a meeting of his Montreal collaborators - Colin Russel and Bill Cone - to confer. If things continued as they were, Ruth might die. At the very least, she would soon be blinded by the growing tumor. However, surgery to remove the tumor (now confirmed by an x-ray to be in her right frontal lobe) carried risks. If the tumor was not encapsulated, removal would be exceedingly dangerous, and possibly debilitating. At the very least, it would be the largest removal that Penfield had yet attempted, and surgical difficulty was not the only issue. The nearest competent brain surgeon, Harvey Cushing in Boston, was a man for whom Penfield harbored no small distaste. Part of this distaste was personal, but part of it was professional - Cushing was, for Penfield, too cautious a surgeon, too willing to declare defeat. He would likely never attempt such a radical removal, and as long as Penfield could avoid causing extensive paralysis, he wanted to attempt the operation himself.

Two days later, Penfield operated on his sister. Removing a portion of her skull, he discovered the presence of an enormous brain tumor in the right frontal lobe. Using a modification of the surgical curtain procedure that he had learned from Otfrid Forester in Breslau, Penfield was able to communicate with his conscious sister during the removal. Slowly and methodically, Penfield removed the tumor. Then, tragedy struck. Penfield discovered that not only did the tumor extend

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5 Penfield spent a year learning from Cushing, the senior statesman of neurosurgeons, in 1918-19. He clearly had a great deal of respect for the man, but their relationship was complex, and Cushing certainly did not trust the younger surgeon (Cushing often hid research findings from Penfield). For his part, Penfield considered Cushing much too cautious, and personally difficult. For more on their fraught relationship, see Lewis, *Something Hidden: A Biography of Wilder Penfield*, 77, 121; Penfield, *No Man Alone*, 228, n57; M. Bliss, *Harvey Cushing: A Life in Surgery* (Oxford: Oxford University Press, 2007).
back into the motor gyrus, which might lead to paralysis, but that it encapsulated a number of cerebral veins that, if cut, could lead to rapid bleeding and death. Moreover, the tumor appeared to extend into the left hemisphere as well. This was already one of the most radical and extensive removals Penfield had ever attempted. Should he continue in the hope of saving his sister’s life, or admit defeat? A glance at the face of Bill Cone, his most trusted partner, answered the question: “Don’t chance it Wide.” Penfield continued to remove as much of the tumor as possible, but ultimately had to give up. The tumor that he had excised would regrow and eventually kill his sister. While he had saved her eyesight, and bought her some additional time, the experience of defeat was so emotionally devastating that Penfield, never a man to relinquish control easily, felt incapable of continuing the operation, and had to let his assistant takeover while he regrouped.6

Ruth Inglis recovered from her surgery and lived for nearly two more years at her home in California before dying on 14 July 1931. Penfield’s surgery on his sister was one of the more harrowing and dramatic stories in the annals of modern brain surgery - a tragic family affair rendered all the more intriguing by the fame of the surgeon. However, the operation had a crucial afterlife. In the intervening years following Ruth’s surgery (1928-1931), Penfield carefully observed the changes in his sister’s behavior that signaled the possible effects of her frontal lobe operation. He concluded that, because of the subtle changes in his sister’s behavior, the function of the frontal lobes was likely related to the ability for “planned administration,” but that this deficit could only be observed by careful study – a person with frontal lobe removal or damage might pass for normal, most of the time, but might also suffer profound deficits that were not always obvious. This conclusion was both contrary to the existing wisdom of the day (which tended to emphasize that frontal lobe damage or removal led to obvious behavioral changes), and indicative of a crucial shift in Penfield’s research program, from pathology to psychology.

We saw in Chapter 1 how Penfield hoped to build an interdisciplinary neurological clinic that would bridge the gap between the operating theater and the pathological laboratory. In part a move to expand the scientific and professional purview of the neurosurgeon, it was also part of Penfield’s intellectual agenda to encompass all of the available approaches to understanding the nervous system. However, the growing boldness of his surgical interventions for epilepsy had a side-effect; epilepsy surgery almost always involved loss of brain tissue, and therefore brain function. While Penfield could map cortical functions in some areas of the brain with his electric probe, the function of other areas, particularly the frontal lobes, remained mysterious, and this uncertainty made him uncomfortable. Realizing that he was ‘out of his depth,’ Penfield reached out to a new professional group that had previously been anathema to both himself, and brain surgeons generally - psychologists. Penfield’s trip from hunting for pathological samples in the Rosemont landfill to his sister’s bedside foreshadowed this coming change in research focus, one inaugurated not by a grand scheme for disciplinary consilience, but by the realities of brain surgery. The frontal lobes, because of their mysterious ‘silence’ to Penfield’s electrical probe, became the first site around which the interdisciplinary approach of the newly built MNI could expand to encompass new disciplines beyond the pathological laboratory.

However, this shift from brain to mind was not a simple affair. The incorporation of psychology at the MNI was marked by contingency, and demonstrates the improvised nature of the MNI’s early interdisciplinary agenda. Moreover, the role of the MNI as a meeting site for trading between disciplines was crucial. The incorporation of psychology was not enabled by any theoretical agreement; indeed, participants often disagreed about the theoretical import of their findings. Nevertheless, the frontal lobes provided the object of inquiry, and the MNI provided the site, for surgeons and psychologists to negotiate a working relationship. This pattern would be replicated a decade later when the functions of the temporal lobes became an object of concern. Operations on the temporal lobes solidified the growing acceptance of psychological expertise at the MNI, and
culminated in the development of the tellingly-named Montreal Method of temporal lobe operations, a procedure that was only possible because of a fertile trading zone between surgeon and psychologist.

In order to understand the development of psychology at the MNI, we must examine the interconnected biographies of three scientists: Donald Olding Hebb, who would become one of the most prominent psychologists of the twentieth century; his student, Brenda Milner, whose later investigations of the amnesic patient H.M. launched the field of cognitive neuroscience, and a crucial but more obscure figure, the largely forgotten Molly Harrower, who made a space for psychologists at the MNI. The overlapping biographies of these three scientists demonstrates the eclectic nature of the MNI's emerging psychology community, and the crucial role of Montreal itself as an interdisciplinary ‘contact zone’ for scientists from different national backgrounds. If, as Penfield suggested, Montreal sat at the ‘crossroads of scientific medicine,’ then it was this small group of psychologists who best-exemplified the importance of this intersection point. Moreover, the growing assembly of surgeons and psychologists depended crucially on its weaker connections to other disciplines and national traditions, in order to continually enrich itself and bring new perspectives and methods to bear.

**From Pathology to Psychology**

By the time that the MNI formally opened in 1934, a small but thriving community had developed around the pathology laboratories that Penfield felt would be at the heart of his new clinic. Word of Penfield’s mastery of the ‘Spanish methods’ of metallic cellular staining had spread through his earlier publications, and the monumental *Cytology and Cellular Pathology of the Nervous System* (1932), and a small community of international scientists had descended on Montreal to learn
the techniques. These included the American neurosurgeons Ottiwell Jones and Joseph Evans, the Polish surgeon Jerzy Chorobski, the Norwegian surgeon Arne Tokilsdsen, and a number of local surgeons and pathologists. Most notably, the English pathologist Dorothy Russell learned in Montreal the staining techniques that would later make her the first woman to hold a pathology chair in Western Europe.

Despite the emerging enthusiasm for pathological research, its early successes were modest. The first research program developed by Penfield - an attempt to understand idiopathic epilepsy as a result of vasomotor reflexes - was largely a failure. The knowledge of the Spanish staining techniques, now practiced in the new MNI pathology laboratory in what were called “Madrid cubicles,” enabled new knowledge of tumors and epileptogenic scar tissue, but no immediate breakthroughs emanated from the clinic on the slopes of Mount Royal.

What ultimately shifted Penfield’s (and the MNI’s) focus was a two-fold development in his surgical practice. First, Penfield embraced a more aggressive approach to brain surgery, and particularly the surgical treatment of epilepsy. Like many of the ‘second generation’ of professional neurosurgeons, Penfield began to venture deeper into the cortex, whereas previous neurosurgeons tended to be more cautious and conservative, sacrificing as little brain tissue as possible. Penfield’s approach to epilepsy surgery, while revolutionary in its own way, was not without risks. The removal

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7 In 1931 Penfield attended the first International Neurological Congress in Berne, Switzerland, with proofs of the soon-to-be-completed *Cytology and Cellular Pathology of the Nervous System*. In a letter to his mother, Penfield noted that “most of the people who wanted to talk to me were interested in macroscopical work. I suppose that shows that my clinical work is not important yet. The new book – “Cytology and Cellular Pathology of the Nervous System” was on exhibition in incomplete form in two large volumes.” In 1932 John Fulton wrote to Penfield that he had recently been in Europe, and had surveyed in which countries the cytology book, his “magnum opus,” was available; Fulton reported that copies were in Stockholm, Lund, Copenhagen, Berlin, Munich, Paris and Rome, along with several copies in England, and that more were clamoring for it. “1929-1932 Personal Corr WGP to JJP” D C/D 33-4, Box 41a, Wilder Penfield Fonds; Fulton to Penfield, 27 September 1932, C/D 16, Box 29, Wilder Penfield Fonds.


9 Evans, “Wilder Penfield.”

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of brain tissue always entailed potential loss of function, and Penfield more than anyone was aware that his aggressive operations could lead to paralysis, aphasia, or other debilitating losses for the patient. Neurosurgeons lived in a professional world of long odds, last-resort operations, and calculated trade-offs between therapeutic success and potentially devastating loss.

Penfield’s attempt to mitigate the risks of his epilepsy surgeries led to the second component of the new operation that would bring psychologists to the MNI. Penfield continued to employ the electrical brain stimulation techniques of Otfrid Foerster, both to map individual cortexes, and to locate specific epileptogenic foci. This technique, in addition to serving Penfield’s surgical aims, also reinvigorated the localization-of-function project in neurology that had languished for several decades.\(^\text{10}\) Not since the late-nineteenth century’s so-called ‘golden age of neurology’ had the relationship between brain form and brain function, between mind and matter, been made so dramatically explicit. However, because the frontal lobes were typically silent to electrical stimulation (and experimental manipulation in animals), the potential losses of surgical intervention were difficult to calculate. By following Penfield’s interactions with a series of patients, we can begin to understand how psychological expertise became a part of his surgical interventions. At the same time, the psychological phenomena that could be triggered in conscious patients by his electrical probe proved an irresistible draw for a small number of psychologists who retained an interest in the nervous system.

\(^{10}\) It is the consensus opinion of most historians that Penfield’s efforts with the electric probe, employed on the brains of conscious patients, reinvigorated the localizationist project in neurology after it lay relatively dormant for decades, opposed by different forms of neurological holism. Anne Harrington states this consensus opinion most clearly: “…the attempt to localize directly sensory-motor functions in the human cortex took on new life and promise in the middle decades of this century, with the work of neurosurgeon Wilder Penfield and his colleagues in Montreal, Canada. For the first time, ‘mappings’ of human sensory-motor functions were made directly through systematic electrical stimulation of the exposed brains of conscious epileptic patients prepared for surgery….” See Anne Harrington, “Beyond Phrenology: Localization Theory in the Modern Era,” in The Enchanted Loom: Chapters in the History of Neuroscience, ed. Pietro Corsi, vol. 4, History of Neuroscience (New York: Oxford University Press, 1991), 213.
Willie and Ruth

In 1925 a fifteen-year old boy from New Jersey named William Hamilton was struck on the head by a brick that had fallen from a chimney. Taken unconscious to a local hospital, a surgeon removed the fragments of brick and bone that had forced their way into Hamilton’s right frontal lobe, leaving the young boy with a “soft pulsating place in his forehead.” Three weeks later, he began to experience seizures that would begin with a scream, followed by convulsions and collapse. By the time Wilder Penfield, who was still working in the New York Presbyterian Hospital in 1927, came to examine him, the attacks had become increasingly frequent and severe. Hamilton was referred to Penfield by a friend of William Clarke, the New York colleague who had encouraged him to undertake experimental studies of brain scar formation (see Chapter 1). Now it seemed that Hamilton had offered Penfield an opportunity to put his laboratory findings into practice. If he could remove the scar tissue from Hamilton’s brain without leaving a second scar, he stood a chance of relieving the young boy’s epilepsy.

But at what cost? Penfield’s approach was experimental at best. Victor Horsley’s attempts in the late-nineteenth century to alleviate epilepsy by surgical means had been partially successful, but had been discontinued. Moreover, Hamilton’s condition demanded the removal of a substantial portion of brain tissue – much of his right frontal lobe. Was the relief of the seizures worth the potential cost to Hamilton’s mind? Moreover, how to judge? The clinical evaluation of postoperative patients in American neurosurgery was, in 1927, more art than science, resting typically on a surgeon’s own subjective judgments, and often with little in the way of formal follow-ups. While

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11 Penfield, No Man Alone, 156.
amputation of brain tissue was never undertaken lightly, the costs and benefits were difficult to weigh. Percival Bailey perhaps spoke for his profession in 1933 when he stated, in regard to the removal of cancerous brain tissue that “one cannot do this with impunity. It is always necessary to weigh the result for the patient. I am not one of those who would remove half of the brain without regard to the fact that I might leave the patient without his intellect or the means of making it effective.” On the other hand, the relative value of this intellect, and the ways in which its loss might be evaluated, rested on the subjective judgment of the surgeon; this judgment often reflected social prejudices. In an infamous passage, Bailey noted that:

I hesitate before amputating a frontal lobe. This procedure is always followed by a more or less great alteration in character and defects in judgment. In a washerwoman these results may be of little concern, but when the patient is a professional business man, who must make decisions affecting many people, these results may be disastrous.

Bailey went to some lengths to counter the then-prevalent notion, emanating from the animal experiments of Karl Lashley, that intelligence and memory were distributed evenly throughout the cortex – a doctrine known as ‘equipotentiality’ - but he provided no clear rules on what areas of the brain could be removed and how to determine if they should be. The decision to operate, then, was largely in the hands of the consulting physician and the surgeon.

At the same time, interest in frontal lobe function was beginning to grow, both inside and outside of the surgical theater. As Jack Pressman has noted, the reasons for this were multiple. The dominant models of neurophysiology that had developed since the late-nineteenth century “held that within the nervous system a component’s function was dependent upon its date of evolutionary appearance….The most recently evolved parts of the nervous system – and thus by inference the

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14 Percival Bailey, Intracranial Tumors (Springfield, Ill., Baltimore, Md.: Charles C Thomas, 1933), 432.
15 Bailey, 433.
16 Bailey made his disagreement with Lashley quite explicit when he wrote that “I merely wanted to impress upon you that in the human brain the parts are not equipotential and that even the defect of intelligence does not, as is sometimes stated, depend only upon the quantity of cerebral tissue removed or destroyed.” Emphasis in original. This passage includes a footnote to Lashley’s 1929 monograph Brain Mechanisms and Intelligence. Bailey, 54, 67, 69.
most dominant control center in the cerebral hierarchy — were the frontal lobes of the human brain.”

The deluge of frontal lobe injuries following World War I solidified this understanding, providing the first major clinical and theoretical treatises on frontal lobe function. By 1930, Penfield’s New York rival, the neurologist Frederick Tilney, put it more melodramatically: “It seems reasonable…to speak of the entire period of human existence as the Age of the Frontal Lobe.”

However, while Pressman is right to assert that the craze for frontal lobe research in humans was kicked off in 1930 by Walter Dandy’s bilateral frontal lobectomy of the patient ‘Joe A.’ (later reported by Richard Brickner in 1932), he is wrong to assert that this was the first lobectomy in the United States. That distinction belonged to Penfield, and his teenage patient Willie Hamilton in 1927, before much of the frontal lobe craze began.

Penfield made the decision to operate on Hamilton shortly before the end of 1927, removing more than half of the young boy’s right frontal lobe, “a very radical procedure.”

Penfield’s 1927 operation was almost certainly the first successful frontal lobectomy, although it was unilateral, rather than bilateral. Three weeks later (during which time Penfield visited Archibald in Montreal), Hamilton was “up and about” and asking to be sent home. Penfield’s subjective judgment of Hamilton’s condition was positive, and notably, “the psychological tests, as far as they could go, showed no loss of mentality in spite of the wide removal of right frontal lobe.”

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18 Early studies of the frontal lobes in animals came from the Italian neurologist Fernando Bianchi, whose finding tended to reinforce the idea that the frontal lobes were “the intellectual basis of civilization.” Pressman, 51–52.
19 This quotation is often misattributed to Tileny’s monumental two volume work The Brain from Ape to Man (1929). It does not appear in that work, but rather in F. Tilney, The Master of Destiny: A Biography of the Brain (New York: Doubleday, Doran, Incorporated, 1930), 302.
22 This claim of priority is difficult to verify, but is substantiated by Edgar A. Kahn, Journal of a Neurosurgeon (Charles C. Thomas Publisher, 1972), 111.
23 Penfield, No Man Alone, 157–58.
Penfield never specified what psychological tests were used to establish Hamilton’s mental health (in most cases, no tests at all would have been performed), but in 1928 it would likely have been some variation of the Binet intelligence test and possibly the Rorschach test. Developed initially in 1904 by the psychologist Alfred Binet for the French Ministry of Education, the Binet-Simon test was originally meant to identify intellectual disabilities in young children so as to provide remedial education. While the Binet-Simon test would later be adapted into a popular procedure for the selecting and sorting of soldiers during World War I, it also remained in use within psychiatry and clinical psychology, and became particularly popular following the publication of the revised Stanford-Binet testing procedure in 1916.\(^\text{24}\) Despite its popularity, its value in the evaluation of brain injuries was unclear. Meanwhile, the iconic inkblots of the Rorschach test had entered the world of psychometric evaluations in 1921, and the New York Neurological Institute, affiliated with Penfield’s Presbyterian Hospital, had been ahead of the curve in applying these tests.\(^\text{25}\) Across the Atlantic, the neurologist Kurt Goldstein and his Gestalt-psychologist collaborator Adhemar Gelb were in the process of developing a systematic set of ‘frontal lobe sign’ tests, but this work was not well known in America in 1927.\(^\text{26}\) Penfield may have been relieved that these tests had shown no loss of ‘mentality,’ but the value of these tests was still secondary to his own clinical judgment.


\(^\text{25}\) The New York Neurological Institute, with which the Presbyterian Hospital was affiliated, had a Social Research Department that conducted some of the earliest testing of the Binet-Simon and Rorschach tests in America. However, this testing was still firmly entrenched in the ‘maladjustment’ paradigm of 1920s-era psychiatry, and it is unclear how systematically this department tested brain surgery patients until after 1938. James Lawrence Pool and Charles Albert Elsberg, *The Neurological Institute of New York, 1909-1974: With Personal Anecdotes* (Pocket Knife Press, 1975), 115–16.

\(^\text{26}\) For more on Goldstein, see Anne Harrington, *Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler* (Princeton University Press, 1999), 145–53, 167–69.
Penfield was unable to conduct much in the way of follow-up evaluations of Hamilton, given that he was about to relocate to Montreal. As detailed in Chapter 1, Edward Archibald’s offer to establish a neurosurgical practice that would combine surgery and neuropathological research had won Penfield over. Following the Hamilton operation, Penfield spent half-a-year in Germany, learning the surgical techniques of Otfrid Foerester for using local anesthetic and electrical stimulation to operate on the brains of conscious epileptics. It is worth noting that, even at this early moment, Penfield was very aware of the potential research value of this new surgical technique:

The prospect of operating, under local anesthesia, on a long series of patients who might be cured of their epilepsy had another very exciting aspect. The electrical stimulation that must be used to guide the surgeon in his removal of the cause would perhaps tell the thoughtful surgeon many secrets about the living, functioning brain. He could learn what the conscious patient might tell him. This would help the neurosurgeon to understand the interrelationship of the mind to localized functional mechanisms in the brain.

For Penfield, Foerester’s surgical procedure was exciting as much for its knowledge-generating possibilities as for its therapeutic value.

Arriving in Montreal, Penfield had barely had a chance to unpack before being presented with his sister’s emergency. The experience of the Hamilton operation, and the young child’s relatively minimal loss of function and ‘mentality’ at least partially enabled his therapeutic boldness. Looking back on the experience, Penfield recalled that:

If [Ruth’s] tumor…is really growing within the right frontal lobe, this [would] prove to be the largest removal I have ever made. It might well be very like the operation I had carried out just before leaving New York, on the patient William Hamilton.

After the operation, Penfield asked:

What had I done to her mind and her personality when I made such a large removal of the right frontal lobe? Would she be different as a person, a wife, a mother? I had been concerned about that after the operation on William Hamilton. He did seem to be

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27 Penfield did publish the case of Hamilton in a relatively obscure Canadian medical journal, where he reiterated that he suffered no psychological effects. Penfield, “The Radical Treatment of Traumatic Epilepsy and Its Rationale.”
28 Penfield, No Man Alone, 168.
29 Penfield, 211.
unchanged, but I had not been able to study his postoperative course carefully enough in New York. I must do better than that here in Montreal…

For Penfield, the experience of the Hamilton operation was a double-edged sword. While it highlighted the possibility of successfully operating on the frontal lobes, it also brought into relief his own ignorance about their functions. The ‘silent lobes,’ as they were often called (because they did not produce obvious effects when stimulated) revealed a contradiction between the radicalism of Penfield’s surgical approach (aggressive removal of tissue), and his inherent surgical conservatism (remove no more tissue than necessary, and never remove tissue that might leave the patient permanently incapacitated if it could be helped). Seen in this light, the resolution of the problem of the frontal lobes was paramount to Penfield’s entire approach to therapeutic brain surgery.

Penfield wrote up his sister’s case, along with two other comparable removals, in 1932, and later published it in a more extensive form with his colleague Joseph Evans in 1935. He began the 1935 paper in a somewhat macabre tone, noting that “Removal of very large areas of the human brain in neurosurgical practice offers an opportunity for detailed neurological study, especially in the occasional case which fortuitously presents many of the conditions which would be demanded of a physiological experiment.” While tumor removal was a less-than-ideal way of examining the functions of the brain (epilepsy was better, because the tissue had not been compressed or

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30 Penfield, 217–18.
31 The terminology of the ‘silent lobes’ emerged slowly during the late nineteenth and early-twentieth century, as the frontal areas of the brain refused to respond in any meaningful way to the electrical stimulation experiments in animals that began with Gustave Fritsch and Eduard Hitzig in Germany, and which were perfected by David Ferrier in England. By 1928, the time of Penfield’s operation on his sister, Frederick Tilney could note that “The frontal lobe, often spoken of as the silent area, is, as a matter of fact, now credited with such functions as those connected with the regulation of the higher faculties of the mind, the development of personality, the formation of all those associational memories which enter into and form personal experience and thus bespeak the degree of intellectual development.” See Frederick Tilney, *The Brain from Ape to Man: A Contribution to the Study of the Evolution and Development of the Human Brain*, vol. 2 (New York: Paul B Hoeber Inc, 1928), 789; Francis Schiller, “The Mystique of the Frontal Lobes,” *Gesnerus* 42, no. 3–4 (1984): 415–24.
32 Of the approximately 15 front lobe removals Penfield had conducted up to that point, he had done almost no preoperative psychological evaluations. This would have to wait until his collaboration with Hebb in 1937. Wilder Penfield and Joseph Evans, “The Frontal Lobe in Man: A Clinical Study of Maximum Removals,” *Brain* 58, no. 1 (1935): 115.
distended), the cases remained compelling. The frontal lobes were “terra incognita” for laboratory researchers, since animals were incapable of speech. Penfield’s sister was clearly the star of the paper, despite his “instinctive reaction…to withhold this case from publication”:

The patient was my only sister. But the close bond of sympathy that had existed between us for many years makes it possible for me to evaluate the effect of the loss of the frontal lobe upon her personality and her mental capacity. My opinion at all events must be interpreted in the light of this personal relationship and if she were alive I am sure she would approve of such an analysis in the hope that it might help others.\(^{35}\)

Penfield noted that the case of his sister provided an “unusual opportunity” because “the patient was observed closely from childhood onward,”\(^ {34}\) in contrast to the perfunctory pre- and post-operative neurological examinations that were then common. Penfield could thus compare Ruth’s post-operative state with his own intimate understanding of her personality, skills and temper, an opportunity rarely afforded (or even desired) by most brain surgeons.

The initial evaluation of Ruth seemed to confirm the notion that frontal lobe removal was attended by few cognitive side effects. Penfield’s colleague, Colin Russel, reported a conversation with Ruth after the operation: “she expressed her gratitude so nicely that one could not help wondering how much the frontal lobe had to do with the higher association processes.”\(^ {35}\) This impression was at least partially confirmed in Penfield’s two other cases by the Montreal psychiatrist David Slight. According to Slight, the ‘mental content’ of these other patients was good, with “no delusions, hallucinations or other abnormal phenomena.”\(^ {36}\) Of one patient Slight reported:

…he can relate what he had for supper last night…and can repeat six digits forward and five backwards…Intellect is good and he tests up to the fourteen years old level according to the Binet-Simon scale….To sum up: It would appear that the patient shows no demonstrable

\(^{33}\) Penfield and Evans, “The Frontal Lobe in Man,” 117.
\(^{34}\) Penfield and Evans, 115.
\(^{35}\) Russell also noted that “Of course, as you have already said, it is not so much the frontal lobe as a slow growing tumor that involved the frontal lobe, and probably the function of that lobe had been gradually taken on by other parts of the brain.” This reflected the then-prevailing belief that certain brain functions could be taken on by other parts following injury. Penfield and Evans also noted that the two other patients seemed “mentally clear” following their operations, despite some vomiting, amnesia and other postoperative complications. Penfield and Evans, 118.
\(^{36}\) Penfield and Evans, 122.
mental change at the present time, and to judge from his previous occupation and other information it would seem that there is no mental loss.\textsuperscript{37}

Despite such rosy results, Penfield’s discussion revealed doubts about both the problem-free nature of his operations, and the value of current psychometric tests, which seemed superficial.

Describing one patient, Penfield noted:

\begin{quote}
this patient is a likeable fellow, a good workman and a useful citizen. It is quite certain he will never be a revolutionary. He has lost something that psychometric examination does not evaluate. He has lost initiative; not all of it, but much of it.\textsuperscript{38}
\end{quote}

But more than the obvious superficiality of Slight’s tests, the ability of Penfield to compare his sister’s behavior over the long span of her life with that of her post-operative condition convinced him that something was amiss:

\begin{quote}
To judge what this patient had lost by being deprived of nearly all of her right frontal lobe demands that an estimate be made of what she would have been had she had a normal right frontal lobe at that age. Because one of us (W. P.) [Penfield] had known her intimately from childhood we believe it is possible to evaluate the mental and personal changes.\textsuperscript{39}
\end{quote}

What followed was a lengthy description of Ruth’s post-operative life, much of it in her own words (drawn from letters to her younger brother). Ruth was “conscious of not being quite as alert mentally as she should be.”\textsuperscript{40} Ruth told her brother:

\begin{quote}
Dr. Taylor asked me if I felt that mental activity was improving, and I said 'Yes,' but it seems as though each time I feel encouraged that way I do a series of very stupid things….I am trying to be more alert, sometimes it seems to be very slow progress, but still it is progress I am sure. If you have any advice to offer as to how to learn to think, how to get something of an education when old, &c., it will be gratefully received.\textsuperscript{41}
\end{quote}

Penfield’s own account of his sister was detailed and complex. Ruth’s “greatest difficulty was in household administration…Her sense of humor, memory, and insight into the thought and feeling of others was altogether unimpaired. She was capable of intelligent conversation and did not talk

\textsuperscript{37} Penfield and Evans, 123.  
\textsuperscript{38} Penfield and Evans, 129.  
\textsuperscript{39} Penfield and Evans, 130.  
\textsuperscript{40} Penfield and Evans, 130.  
\textsuperscript{41} Penfield and Evans, 130.
either more or less than good taste demanded.” 42 Fifteen months after the operation, Penfield travelled to Ruth’s home, and was able to observe first-hand the subtle changes in her daily life:

…her own home provided in some ways a better background for study than the consulting room of the psychologist. The following test, though not sanctioned by psychological usage, may illustrate her shortcomings.

One day about fifteen months after operation she had planned to get a simple supper for one guest (W. P.) and four members of her own family. She looked forward to it with pleasure and had the whole day for preparation. This was a thing she could have done with ease ten years before. When the appointed hour arrived she was in the kitchen, the food was all there, one or two things were on the stove, but the salad was not ready, the meat had not been started and she was distressed and confused by her long continued effort alone. It seemed evident that she would never be able to get everything ready at once. With help the task of preparation was quickly completed and the occasion went off successfully with the patient talking and laughing in an altogether normal way.

Although physical examination was negative and there was no change in personality or capacity for insight, nevertheless the loss of the right frontal lobe had resulted in an important defect. The defect produced was a lack of capacity for planned administration. Perhaps the element which made such administration almost impossible was the loss of power of initiative. If we express it as she did with no attempt at analysis: she could not “think well enough,” was a little “slow,” a little “incapable.” 43

Read carefully, Penfield’s analysis of his sister reflected a doctor who, although clearly operating within the social and gender norms of his time, was nevertheless acutely sensitive to the human needs of his patients. It is worth here pausing to contrast Penfield’s lengthy examination of his sister with Percival Bailey’s infamous description of the effects of frontal lobe operation. According to Bailey, operation of the frontal lobes might result in defects of judgement, but that “in a washerwoman” these results would be “of little concern.” While Penfield’s case was not exactly analogous, the difference in his evaluation was striking. Ruth’s social position made her cognitive deficits both more devastating to her happiness, and more detectable.

Moreover, the ability to carefully observe Ruth for an extended period within an ecologically valid setting revealed the limitations of existing psychological tests. “Neurologically each [patient] was normal. By the ordinary psychometric tests each would have to be judged normal although

42 Penfield and Evans, 130–31.
43 Penfield and Evans, 131.
neither would rank very high. Each patient was lacking in initiative.”

Ruth’s operation, then, revealed both the potential for psychological insight, and the need for psychological expertise, that might emanate from the natural experiments emerging from Penfield’s Montreal operating theater.

**Psychological Implications**

If Penfield was increasingly aware of his lack of psychological expertise, then some psychologists were also becoming increasingly aware of Penfield’s operations. By the mid 1930s, Penfield had conducted more than 160 operations, using an electric probe to map the sensory and motor areas of his patients’ brains while under local anesthesia. In many ways, Penfield had now turned his surgical theater into a kind of psychological and physiological laboratory, with the patient as an active participant in the experiment. The early publications of Penfield’s mapping of the sensory and motor pathways of the brain did not escape the notice of those psychologists who were still interested in brain mechanisms, particularly perception. At the same time, Penfield’s increasing interest in the psychological ramifications of his operations broke through a prejudice against academic psychology that he had nurtured for some time.

In early 1936 Penfield submitted a paper for presentation at the Royal Society of Canada meeting that detailed his early stimulation surgeries

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44 Penfield and Evans, 132.

45 Wilder Penfield and Edwin Boldrey, “Somatic Motor and Sensory Representation in the Cerebral Cortex of Man as Studied by Electrical Stimulation,” *Brain: A Journal of Neurology* 60, no. 4 (1937): 389–443. This article contained the first appearance of the infamous ‘homunculus’ representation of the somatosensory cortex, and is widely seen as a watershed in the project of cortical localization in the twentieth century.

46 Penfield’s attitude toward academic psychology can be gauged from a number of sources. While they are far from definitive, they give the picture of a man who viewed the field with varying degrees of skepticism. As early as his college days, Penfield has been dismissive of the work of William James, whose *Principles of Psychology* he had as required reading. It is likely that Penfield began to equate academic psychology almost wholly with Freudian theory in the 1930s, and his attitude toward the latter was captured in a letter to Abraham Myerson in 1937. In response to Myerson’s question about Penfield’s attitude towards psychoanalysis, Penfield replied with a single sentence: “I should place myself in the second group, of those who feel very favorably inclined towards psychoanalysis but do not wholly accept it and are, to a certain extent, skeptical.” Penfield to Myerson, 16 December 1937, Box 54, C/G 37 M, Wilder Penfield Fonds.
entitled “The Psychological Level of Cortical Activity.” Meanwhile, his early publications on the subject of visual perception and the sensory cortex had attracted the attention of the Gestalt psychologist Wolfgang Kohler, recently arrived in America following the rise of National Socialism in Germany.

Kohler took a special interest in Penfield’s mapping of the visual system in epileptic patients, and following a visit to the two-year-old MNI in 1936, sent Penfield an extended series of comments on his paper with Evans regarding the representation of the macula within the visual pathways. Kohler seemed both intrigued and troubled by Penfield’s results in equal measure. “About epilepsy I had as yet not known very much. Your paper showed me most clearly why you take so much theoretical interest in such cases.” Kohler went on to argue against Penfield’s finding of a point-to-point connection between the ocular macula and the striate cortex. Using an analogy that prefigured Kohler’s later use of field theory in studies of visual perception, he asked:

In an analogy: If a tube is wide open for a stream of water, particles of water on the left side will remain on the left side throughout the tube, if, however, in some place the left side is blocked by an obstacle, the “lines of current”, as the physicists say, would, to some degree, bend around the instance and pass over to the right side. Of course, according to this idea, there would not be an altogether fixed point-to-point correlation between retina and striate areas. But it is precisely this point in which I am not yet quite convinced by available evidence....My present work depends so definitely upon the solution of these problems that I cannot stop thinking about them....The “case of the water in the tube” has, I believe, been too much neglected. Still, from the standpoint of physics it is a perfectly genuine possibility,

47 “Royal Society of Canada,” File 30-3, Box 419, Wilder Penfield Fonds. Alternative titles included “The psychological level of cortical activity. Observations upon conscious patients,” and “The relation of cortical activity to consciousness.” A similar sentiment is expressed in the beginning of a 1938 article. In that article, Penfield noted that his data were relevant to psychology, but that he felt it difficult to communicate with academic psychologists. “A neurosurgeon has a unique opportunity for psychologic study when he exposes the brain of a conscious patient; no doubt it is his duty to give account of such observations on the brain to those more familiar with the mind. He may find it difficult to speak the language of psychology, but it is hoped that material of value to psychologists may be presented, the application being left to them. It seems quite proper that neurologists should push their investigations into the neurologic mechanism associated with consciousness and should inquire closely into the localization of that mechanism without apology and without undertaking responsibility for the theory of consciousness.” Wilder Penfield, “The Cerebral Cortex in Man: I. The Cerebral Cortex and Consciousness,” Archives of Neurology and Psychiatry 40, no. 3 (1938): 417.
though of course in many other respects streaming water is not comparable with nerve currents.\textsuperscript{49}

Kohler also seemed disturbed by the body movements that were evoked by Penfield’s electrode, but suggested an experiment to resolve the matter:

I feel relieved when I hear that Miss C. [a patient] calls her movements under brain stimulation involuntary. The contrary would psychologically be a paradox. As to localization - I have one wish. Could you, during some forthcoming operation, ask the patient to put his hand on several places on the surface of his body, or also somewhere aside, and then see whether the occurrence of such involuntary hand movements as happened with Miss C…depends upon the place of the hand at the time of stimulation?…I must say that the scientific situation is simply fascinating.\textsuperscript{50}

For Kohler, Penfield’s electrical stimulation studies provided an exciting chance to test his own psychological theories of perception. Unlike many psychologists in the United States, Kohler retained an interest in the nervous system and its psychological concomitants, a relationship which Penfield’s electrode might now unlock.

Kohler seemed less interested in providing Penfield with the psychological help that he required. Evidently, Penfield had asked him a question about the frontal lobes, to which Kohler could only respond by referring him to the recent paper on primate frontal lobe function by Carlyle Jacobsen, B.J. Wolfe and T.A. Jackson (the very studies that would later provide the loose theoretical underpinning for the frontal lobotomy).\textsuperscript{51} Nevertheless, Penfield had been impressed by Kohler’s visit to Montreal, and offered him a job:

From a psychological point of view there is an important piece of work to be done on these patients. As I told you the other day, I would like very much to be able to get you to come here for a year or two or permanently, if we could induce you to do so. If the problem seems challenging to you, what sort of an arrangement would make it possible?\textsuperscript{52}

\textsuperscript{49} Kohler to Penfield, 10 March 1936, File C/G 36 K-L, Box 53, WP Fonds.
\textsuperscript{50} Kohler to Penfield, 4 April 1936, File C/G 36 K-L, Box 53, WP Fonds.
\textsuperscript{52} Kohler to Penfield, 6 March 1936, File C/G 36 K-L, Box 53, WP Fonds.
While the plan to make Kohler the MNI’s ‘in-house’ psychologist was ultimately scuttled by costs and Kohler’s own commitments to Swarthmore University, Penfield’s job offer indicates the extent to which psychological concerns had become central to the business of brain surgery in Montreal by 1937.53

**Not Many Psychologists**

Kohler’s refusal left Penfield in a difficult situation. Penfield’s attitude towards academic psychology was less than positive, but he was increasingly becoming aware of his own limits.54 The mysterious frontal lobes were crucial to Penfield’s surgical agenda, yet their lack of obvious function (and silence to electrical stimulation) left him operating partially blind.

Montreal itself offered little in the way of psychological expertise to be tapped. David Slight, the psychiatrist who had conducted the initial psychometric analyses, had recently retired, and Penfield seemed unimpressed with him as a colleague. The psychology department at McGill University provided little help; founded in 1910 with the appointment of William Dunlop Tait, who moved over from the department of philosophy, McGill’s new psychological laboratory was only the second in the country (the first had been in Toronto). Tait’s approach was experimental and

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53 Penfield and Kohler kept in touch. Kohler was particularly interested in Penfield’s joint article with Ediwin Boldrey. See Kohler to Penfield, 10 March 1940; Penfield to Kohler, 14 March 1940, File C/G 40 K-L, Box 55, WP Fonds. Penfield and Boldrey, “Somatic Motor and Sensory Representation in the Cerebral Cortex of Man as Studied by Electrical Stimulation.”

54 Alison Winter and Katja Guenther have both commented on Penfield’s aversion to academic psychology in different ways. Winter has noted that “for Penfield, psychology was common sense,” and that he had relatively little patience for the Freudian turn in American psychology and psychiatry. Winter also notes that Penfield would turn to psychological expertise in 1952 when his patients began to suffer from memory disorders. This is not correct; Penfield turned to psychological expertise much earlier. Guenter has noted that Penfield’s post-WWII work included a ‘turn to psychology’, and that he disagreed with some of the interpretations of his later psychological colleagues, but does not elaborate on this point. I here pursue a different line of inquiry - what brought psychological expertise to the MNI in the first place, and how was that expertise integrated over time? Alison Winter, *Memory: Fragments of a Modern History* (University of Chicago Press, 2012), 82–87; Katja Guenther, *Localization and Its Discontents: A Genealogy of Psychoanalysis and the Neuro Disciplines* (Chicago: University of Chicago Press, 2015), 153–84.
biological (he had trained with Hugo Munsterberg at Harvard), but was also heavily influenced by
the growing movement to apply psychology to broader social problems, and away from the clinic.\textsuperscript{55}

If Montreal offered no help with psychological expertise, Penfield’s options were not much
better in his native United States. By the 1930s, the broad move away from physiological psychology
was well underway, with adherents roughly grouped under the banner of behaviorism. Beginning
with J.B. Watson’s pronouncement in 1913 that the proper study of psychology was neither mind
nor brain but ‘behavior,’ American psychology had come to gradually de-emphasize study of the
nervous system. Within a year of Penfield’s request to Kohler, B.F. Skinner sounded the battle cry of
the behaviorists in \textit{The Behavior of Organisms} (1938) in which he made the influential argument that
psychology ought to limit itself to understanding whole organisms, without reference to any
underlying neurological mechanisms. While Skinner’s argument was radical, many other American
psychologists agreed at least partially.\textsuperscript{56} Thus, the examination of the human and animal nervous
system had been left largely to neurologists and psychiatrists. At the same time that Skinner
disavowed investigation of the nervous system, John Fulton, perhaps the foremost
neurophysiologist in America, remained largely in the dark on frontal lobe function. In his standard
textbook, \textit{The Physiology of the Nervous System} (1938), Fulton’s main source of information on the
frontal lobes in humans was Penfield and Evans’ 1935 paper on Ruth.\textsuperscript{57} Finally, the most prominent
psychologist in America who retained a strong interest in the nervous system, Karl Lashley, was
clearly the wrong man for the job. In addition to being strongly wedded to his animal research at the

\textsuperscript{55} Stanley Brice Frost, \textit{McGill University: For the Advancement of Learning}, vol. 2 (Montreal: McGill-

\textsuperscript{56} For more on behaviorism in American psychology, see John B. Watson, “Psychology as the
Behaviorist Views It.,” \textit{Psychological Review} 20, no. 2 (1913): 158; B. F. (Burrhus Frederic) Skinner, \textit{The
Behavior of Organisms} (New York, London, 1938); John M. O’Donnell, \textit{The Origins of Behaviorism:

\textsuperscript{57} J. F. Fulton, \textit{Physiology of the Nervous System} (New York: Oxford University Press, 1938), 460.
University of Chicago, Lashley advanced a holistic theory of brain function that was diametrically opposed to the kind of localizing work then taking place in Montreal.\textsuperscript{58}

The archival record is unclear about when Penfield realized that he would need to engage with an outside psychologist, but by 1937 his desire for psychological expertise dovetailed with another development; McGill University was considering founding an independent department of psychiatry, the first in the country. Penfield was intimately involved in the preparations for this new department, as he had long nurtured the notion of incorporating psychiatry into his multidisciplinary institute (see Chapter 4 for an extended discussion of the origins and outcome of this desire). “A really close relationship [between psychiatry and neurology] is desirable. I do not believe in combining the two completely as has been done in so many continental [European] universities. But at present the psychiatrists at both the Royal Victoria and Montreal General Hospitals are neuropsychiatrists….I would suggest, therefore, that certain of our members be included in the Department of Psychiatry according to the discretion of the Professor of psychiatry…” Then, almost as an afterthought, Penfield added that “We are at present planning to secure one or two psychologists to work at the Neurological Institute and it can be seen that such persons could well function in both departments.”\textsuperscript{59}

The idea that psychologists could act as liaisons between the MNI and the forthcoming psychiatry department seemed a reasonable one, but ultimately disintegrated thanks largely to the man who ended up running the psychiatry department, Ewen Cameron (see Chapter 4); but in 1937 it appeared that the major impediment to enlisting psychologists at the MNI was simply finding qualified candidates. In a letter to Grant Fleming, the McGill Dean of Faculty and a fellow doctor,


\textsuperscript{59} Penfield to Grant Fleming, 11 January 1937, File A/M 2/2-2, Box 1, WP Fonds.
Penfield described what he envisioned would be the responsibilities of the two psychological positions he had created. “Their problem would be to make psychological investigations of patients in whom areas of brain had been removed.” He then added, “there are not many psychologists who are fitted for this kind of work.”

The Stickit Novelist

If Penfield found it hard to secure a pair of psychologists who were ‘fitted’ to the kind of work he needed done, then the two he selected more than made up for the difficulty of the search. In order to understand the development of neuropsychology at the MNI, one needs to understand the lives, careers, and even personalities of two individuals: D.O. Hebb, and Molly Harrower.

Born in Chester, Nova Scotia in 1904 to a pair of country doctors, Donald Olding Hebb’s Scottish, German and English ancestors had been resident in the Canadian Maritime region since the late-colonial period. A precocious child, Hebb taught himself to read at a young age, and was educated by his mother, who had been impressed by the philosophy of home-schooling promoted by Maria Montessori. Despite his extensive reading and home-based education, Hebb was a bored student when he enrolled in formal schooling, and graduated from high school with, in his own words, “a low estimate of the value of scholastic achievement, [an attitude] that lasted into my college years and beyond.”

Despite his talent for mathematics and physics (he graduated from Dalhousie University in 1925 with distinction in those subjects), Hebb emerged into the Canada of the 1920s with aspirations to be a novelist, an ambition that he later described using his ethnic vocabulary: “The Scotch…have a name for the lad who sets out to be a preacher but does not make it: He is a stickit

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60 Penfield to Grant Fleming, 11 February 1937, File A/M 2/2-2, Box 1, WP Fonds.
Hebb also described himself as a stickit reformer of elementary schools, and indeed, his earliest job as an elementary school teacher in Montreal in the late 1920s was punctuated by a formal experiment in teaching methods that prefigured much of his later career. “In Montreal, I became so interested in the problems of elementary education that I might have made a career in that field, but I was defeated by the rigidity of the curriculum in Quebec’s Protestant schools.” The experiment in question was not a haphazard empirical test, but rather a methodical nine-month evaluation of pedagogical methods, conducted in cooperation with McGill University’s psychology department. Hebb had IQ tests administered to demonstrate that many of the failing students were in fact of ‘superior intelligence,’ and instituted a regime that banished corporal punishment, and treated education as a reward rather than an obligation. The results were remarkable - grades and behavior improved across the board - and it is a testament to how important this period was in Hebb’s life that it resulted in both his first scientific publication in 1930, and also consumed four pages of his brief (30-page) autobiography.

Hebb would, for the rest of his career, display a fascination with learning (broadly conceived), intelligence, and the relationship between the two.

At about this time, a tubercular infection of the hip that left him in bed for nearly a year began Hebb’s “training as a Pavlovian.” Previously, Hebb had read the collected works of Freud (“Obviously a very interesting fellow but, it seemed to me, not too rigorous.”) and had decided to enter the field of psychology. While teaching in the Quebec school system, he began a psychology degree at McGill, and was assigned William James’ *Principles of Psychology* as required reading. Now, laid up in bed with little to do, Hebb immersed himself in Pavlovian theories of conditioned reflexes, and Charles Sherrington’s *Integrative Action of the Nervous System* (1906). Hebb produce a

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62 Hebb, 273.
63 Hebb, 273.
master’s thesis, which argued that even skeletal reflexes were learned (learned by the unborn fetus, in utero), and began a series of Pavlovian experiments under the tutelage of one of Pavlov’s most loyal disciples, Boris Babkin, then part of McGill’s physiology department following exile from his native Russia. While Hebb was unimpressed by his own experimental findings (work done with conditioned dogs), he received a thorough education in Pavlov’s theories from Babkin and Leonid Andreyev, both of whom “considered that American psychologists had bastardized Pavlov’s methods. Andreyev was to provide me with…training, and I would be a proper psychological and North American representative of conditioning as it should be, Russian style.”

Despite the antipathy that Babkin had for the theories of the American Karl Lashley, he recommended, following the unexpected death of Hebb’s first wife in a car accident, that he consider a graduate education with Lashley in Chicago. Hebb arrived to begin his PhD in Chicago at a propitious time. The University of Chicago was, in the 1930s, perhaps the most exciting and eclectic location to engage in any form of psychological training. Behavioral and learning theorists sat uneasily next to biological determinists such as Lashley, whose own nemesis, C.J. Herrick, lectured alongside the recently arrived Gestalt refugee Wolfgang Kohler. Hebb absorbed as much as possible, feeling both distinctly unprepared by his earlier training in the writings of Pavlov and James, and stimulated by the heterodox environment. A lecture course by L.L. Thurstone was particularly influential, and worth noting in connection to Hebb’s later theories. Thurstone, in postulating multiple forms of intelligence (verbal comprehension, word fluency, number, space, memory, perceptual speed, and reasoning), served as the primary antagonist to Charles Spearmen, whose concept of a single, general intelligence level (g) would cause great controversy in the coming years.

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66 Hebb, 284–85.
decades. Thurstone’s use of multiple factor analysis to argue for multiple ‘primary mental abilities,’ while still preserving a space for the possibility of a general intelligence level, was foundational for Hebb’s later open-mindedness and eclecticism on the issue of intelligence.

Hebb followed Lashley to Harvard in 1934 and began an experimental program that was designed to test whether the visual system of rats was innately organized, or the product of experience. Hebb’s findings would later trigger an important divergence from the neuropsychology of his mentor, and are worth examining in some detail. Since the early 1920s, Lashley had attempted to show, through painstaking experimentation with rats, that the prevailing doctrine of cerebral localization (that certain mental functions could be ‘mapped’ onto particular areas of the brain) was incorrect. While Lashley conceded that there was some localization of function in the brain, he argued vehemently against all attempts to localize ‘higher’ functions such as memory and intelligence. For Lashley, the brains of rats and men were ‘equipotential’ - all areas were equally capable of fulfilling these higher functions, and memory ‘engrams’ (the physical site of a memory) were diffused throughout the cortex. At the same time, Lashley’s equipotential theory was vehemently not a theory of reflex connections, associations or behavioral learning. As Nadine Weidman has shown, Lashley’s equipotential theory was deeply rooted in his commitment to a genetic theory of intelligence. For Lashley, intelligence was a single entity (much like Spearman’s ‘g’) that could be accurately measured with a single test, and which was inherited from parent to offspring. Because of this hereditarian commitment, Lashley found common cause with those like the Gestalt psychologist Wolfgang Kohler who argued that the structure of the brain’s visual system was innate, rather than learned. To test this proposition, Hebb raised one group of rats in total

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68 Weidman, Constructing Scientific Psychology: Karl Lashley’s Mind-Brain Debates, 66; Lashley, Brain Mechanisms and Intelligence: A Quantitative Study of Injuries to the Brain; Lashley, “In Search of the Engram.”
darkness, and another under normal conditions. He initially found that the rats raised in darkness
had no obvious visual deficits, a finding that would have pleased Lashley.\(^9\) While Hebb would later
construct his entire theory of brain function in opposition to these initial findings, at the time it
sufficed to earn him a Harvard PhD in 1936.

Hebb emerged from Harvard in 1936 with an eclectic education in modern psychological
theories, one that crossed national and intellectual borders. From his mentor Lashley, Hebb
inherited an abiding materialism, and an affection for cleverly constructed animal experiments, but
little else. Most notably, Hebb seemed immune to the underlying racial politics and hereditarianism
that motivated much of Lashley’s work. Hebb was as comfortable with C.J. Herrick’s progressive
psychobiology as he was with Kohler’s Gestalt perceptual psychology, and although he was critical
of Clark Hull’s theory-centric behaviorism and B.F. Skinner’s radical, atheoretical behaviorism, he
acknowledged the value of their experimental programs. Unfortunately, this eclectic education, and a
Harvard PhD, did not translate into immediate job offers. According to Hebb, “physiological
psychology was in the middle of its long period of decline, between 1930 and 1950, and at the end
of this postdoctoral year, job prospects were poor for a physiological psychologist.”\(^{70}\) Indeed, against
the rising tide of behaviorism, Hebb was unlikely to find much more than assistant work in
laboratories such as the Yerkes Primate Lab, where psychological tools were being used to
investigate the nervous system.

Hebb found employment back where he began his education in psychology, and through a
peculiar route. In 1937, Hebb’s sister Catherine, then training in physiology at McGill with Babkin,

Perception of Figures by Rats Reared in Total Darkness,” The Pedagogical Seminary and Journal of Genetic
Transfer of Response in the Discrimination of Brightness and Size by Rats Reared in Total
Darkness,” Journal of Comparative Psychology 24, no. 2 (1937): 277; D. O. Hebb, “The Innate
Organization of Visual Activity. III. Discrimination of Brightness after Removal of the Striate

\(^{70}\) Hebb, “DO Hebb,” 287.
had heard that Wilder Penfield was looking for someone “to study the psychological status of his patients after brain operation.”

Hebb applied, and while his appointment was initially ‘hanging fire,’ held up by budget constraints and interdepartmental bickering, he became one of the MNI’s first external research fellows in 1937. According to Penfield, “I have been in correspondence with two [psychologists] who would be used entirely for research undertakings….One would study consciousness, if she comes, and the other, Dr. Hebb, would study probably vision.”

**Thursday's Child**

The woman that Penfield hoped to recruit in 1937, along with Hebb, was Molly Harrower, a young English psychologist then completing a research fellowship in New York. While Harrower is mostly remembered today (when she is remembered at all) for creating the first large-scale Rorschach test for use in evaluating military recruits, this was a far cry from how her career began.

Harrower’s attempt to convert Gestalt perception tests into diagnostic neurological tests was crucial in legitimizing the status of psychology at the MNI.

Born in South Africa in 1906 to Scottish parents, Harrower grew up in the small city of Cheam, twelve miles south of London. Harrower’s path to science was considerably more circuitous than Hebb’s. The child of a depressive father and a smart-but-subordinated mother, Harrower excelled at sports and dance. She spent a portion of her teenage years at a finishing school in Paris, a development she resented because of the “frivolity” of the education. In her unpublished autobiography, “Thursday’s Child,” Harrower recalled that the first time she heard the word ‘psychology’ was in a church sermon that stressed that the modern sciences of the mind made it

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71 Hebb, 287.
72 Penfield to Grant Fleming, 11 February 1937, File A/M 2/2-2, Box 1, WP Fonds.
possible for one to control one’s destiny, a prospect that appealed to her independent spirit. Her exposure to academic psychology came about through a journalism program at the University of London that required a course in psychology. Her performance was strong enough that she was invited to become a full-time psychology student. Yet Harrower's education in psychology conformed to the peculiarly English brand of humanistic psychology that still saw the field as part of the moral sciences of the nineteenth century. Thus, it was not unusual for Harrower to combine her studies of reaction time experiments or Gestalt theory with summers spent at the Margaret Morris School for dancers and artists in the south of France. It was through this community of “artists, writers, social reformers, and delightful eccentrics who formed the outer circle of Margaret Morris’s friends and dance school” that Harrower was introduced to C.K. Ogden, the eccentric English scholar who combined studies of literature with his editorship of the psychological journal *Psyche*. Ogden introduced Harrower to the work of Gestalt psychologists Wolfgang Kohler and Kurt Koffka, and ultimately secured a place for Harrower to study with Koffka in America. 

Raised in an environment that ‘downgraded’ everything American, including American behaviorism and other trends in science, Harrower’s decision to come to United States was as much a product of a rebellious streak as anything else. She arrived in 1928 to work at Smith College with Koffka, himself recently arrived in 1927. The laboratory set up by Koffka was, in Harrower’s words, “100% international,” with psychology students from China, Russia, Western Europe and the United States working on a diverse array of projects, from “the effects of magic on the child mind”

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75 Harrower, 132.

76 Harrower, “Thursday’s Child,” WP Fonds.
to “frustration and incomplete tasks.” Harrower was assigned to work on Koffka’s own project on color and organization, and spent two years “absorbing the meaningfulness of Gestalt Theory from its very source.” Her early research interests, which included the somewhat nebulous notion of applying Gestalt ideas to ‘higher mental processes,’ received Koffka’s approval, and Harrower graduated with a PhD in 1934. At this point, fate intervened, and altered Harrower’s research trajectory; a close friend underwent a brain operation and emerged “from [the] drastic surgical procedure as a seemingly different person.” Harrower found herself questioning her commitment to the sterile world of the lab:

What…was evoked by obvious traumatic events, physically, such that behavioral changes were noticeable? How could one study such a happening? How, perhaps even to restore people to their former, rightful selves? All of a sudden these questions seemed to become the province of psychology that I wanted to explore. I wanted to deal with the individual person, not make studies with his retina.

An equally coincidental run-in with a sympathetic surgeon led Harrower’s proposal to study surgical patients to the desk of the Rockefeller Foundation’s Alan Gregg, who was only two-years removed from his efforts to establish Penfield’s Montreal Neurological Institute (see Chapter 1).

Surprisingly, Harrower’s proposal did not meet with unconditional enthusiasm. Both Penfield and the Harvard neuropsychiatrist Stanley Cobb read Harrower’s initial Rockefeller proposal, and their divergent reactions are instructive. Cobb was intrigued, noting that he and his colleagues were then carrying out experiments on surgical shock, and that Harrower might enjoy studying the psychological effects of this phenomenon. Penfield, who read Cobb’s remarks on the proposal, crossed out the word ‘psychological,’ and noted “typographical error, surely physiological is meant. There is no such thing as psychological effects of surgical shock.” In 1936, Penfield was only slowly coming to his interest in the psychological effects of surgical procedures. However,

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77 Harrower, “Thursday’s Child,” WP Fonds.
78 Harrower, “Thursday’s Child,” WP Fonds.
79 Harrower, “Thursday’s Child,” WP Fonds.
80 Harrower, “Thursday’s Child,” WP Fonds.
Harrower prevailed by appealing to Penfield's interdisciplinary sensibilities, his recent experience with his sister, and his desire to heal:

I am more than ever convinced that psychology has a real contribution to make in the fields of neurology and neurosurgery. In saying this I am not thinking of myself, or any other one psychologist, but of the contributions that can ultimately be made by the interaction of these fields of knowledge. This realization makes me feel that any psychologist who goes to the Institute has a tremendous responsibility. For he cannot only content himself with working on specific and isolated problems that will be of interest to his fellow psychologists, but must also attempt to bridge a gap and justify psychology's inclusion among that constellation of subjects which are being brought to bear on nervous diseases.\(^{81}\)

Harrower went on to note that, despite his good intentions, Penfield’s lack of psychological sophistication might be hampering his ability to understand the frontal lobe operations that were causing him such concern:

I believe for instance the very different kinds of memory disturbances would come to light for, after all, the term memory is but a convenient short cut for many different psychological activities and a comparison of these in the light of the different physiological and operative findings would be of value. Then I believe it would be possible to get a little nearer to an exact formulation of what this ‘lack of initiative’ that you speak of, and ‘lack of concentration’ mean. Such studies would be of interest to the theoretical psychologist and such defects if properly understood might prove amenable to some form of psychotherapy.\(^{82}\)

Harrower’s appeal came at exactly the right moment, and played precisely to the nagging concerns about precision and patient care that haunted Penfield following his sister’s operation. At the same time, Penfield offered Harrower the opportunity not only to conduct investigations amongst a unique group of patients, but also the rarefied experience of studying the “most intriguing’…changing states of consciousness during cortical stimulation.”\(^{83}\)

Harrower’s colleagues encouraged her, before going to Montreal, to spend some time in the clinic of Kurt Goldstein at the Montefiore Hospital in New York. Like Koffka, Goldstein had recently arrived from his native Germany following the rise of National Socialism. While Kofka’s had been a self-imposed exile, the Jewish Goldstein had been forcibly ejected from Germany when

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81 Quoted in Harrower, “The Birth of Neuropsychology,” 2.
his clinic at the Moabit state hospital was raided by SA officers. Goldstein, whose pioneering collaboration with the Gestalt psychologist Adhemar Gelb had essentially launched the field of clinical neuropsychology after World War I, was at the time attempting to adapt his studies of brain-damaged veterans into a series of clinical tests that could accurately diagnose brain dysfunction. However, Goldstein’s holistic conception of brain function, steeped in German cultural preoccupations, seemed less and less tenable. As Anne Harrington points out, Goldstein had “[come] close to compromising his globalist approach to brain functioning by admitting a localized link between abstract capacity and intact frontal lobe functioning.” As such, most of Goldstein’s instruction to Harrower was in the tacit skill of interviewing patients:

Goldstein’s teaching turned out to be highly personal; it involved being his shadow…being with him as he spoke to patients, discussing the notes he wrote about them. The experience [was] similar, I think, to that of the apprentices of old, where the master conveys his techniques indirectly, by example.

Elsewhere, Goldstein demonstrated how to interpret the ambiguous and confusing responses patients might give:

He would point to the importance of wrong answers or failures in test situations, and how these wrong answers gave clues to the individual’s understanding of the total situation. Wrong answers were more important in understanding a defect than the correct one. To all of this, my Gestalt training made me temperamentally akin, and my own interest in wrong answers was endorsed at that point. Throughout my professional life, particularly in the Wechsler Comprehension and Similarities, I have used wrong answers in the same way as if they had been projective material…

Harrower’s own experimental work involved using ambiguous figure drawings to test the perceptual ability of brain-injured patients. Variations on the classic Rubin Vase drawing (see Figure 2.1) revealed evidence in support of Goldstein’s argument that those suffering from frontal lobe
damage lost a sense of ‘abstract attitude’ - the ability to categorize perceptual wholes.

Figure 2.1 - Harrower’s modification of the classic Rubin ambiguous figures, used to test patients with brain damage. From Harrower, “Changes in Figure-Ground Perception in Patients with Cortical Lesions,” British Journal of Psychology 30 (1) (1939), 48.

However, while Harrower praised Goldstein’s commitment to his patients, she never seemed to wholly endorse his particular take on brain injury. Stripped of the overarching commitment to German cultural ideals, Goldstein’s clinical testing material became just one among a growing number of psychological tests at Harrower’s disposal. At about the time that Harrower’s apprenticeship with Goldstein was coming to an end, she had two encounters that would alter her scientific mindset and practice; the first was her encounter with the Rorschach test.

Harrower was introduced to the Rorschach by none other than its most vocal proponent, Bruno Klopfer, who paid occasional visits to Goldstein’s clinic. According to Harrower:

I was immediately struck by the research and clinical possibilities of the method….Before long [Zygmunt] Piotrowski and I had projects which we planned to interrelate in the use of the Rorschach with organic, or brain damaged patients. We decided then and there to make comparisons between cortical involvement and non-cortical involvement, between localized tumors and general diffused tumors, and between frontal lobe tumors and other locations.

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88 For more on Goldstein’s German context, see Harrington, Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler, 140–74.
89 Harrower, “Thursday’s Child.”
Indeed, Harrower and Piotrowski published extensively on the use of the Rorschach as a diagnostic test, designed to reveal aspects of brain injury, and to differentiate organic and non-organic mental disturbance.80

Harrower’s growing belief in the value of different types of psychological tests was reinforced by a remarkable chance encounter during the waning days of her apprenticeship with Goldstein. In 1937 Harrower made a trip to the Bradley Home in Providence, Rhode Island, where a young psychologist-cum-physiologist from Oregon named Herbert Jasper was beginning his early studies of the electroencephalograph. Jasper, who would himself be relocating to Montreal in a few months, was in the midst of a collaborative research project with Penfield that would extend and legitimate the use of the EEG as a means of localizing epileptogenic foci. This project would lead to a nearly 25-year collaboration between Penfield and Jasper that would be crucial for the creation of interdisciplinary neuroscience (see Chapter 3). At the time, however, Jasper was frantically attempting to meet his new obligations to the MNI while continuing his program of psychological investigation with the EEG in Rhode Island. He and Harrower discussed their mutual interest in psychological testing, and traded notes on their upcoming place of employment. According to Harrower:

I enjoyed two-and-a-half hours solid discussion with Jasper, who is also going to the Montreal Neurological Institute in a few months. By strange coincidence, he has just discovered a test of apparent movement which reflects dominance. It is of vital importance in Montreal, he tells me, because you must not operate on the right hemisphere of suppressed left-handed people. And now, here I have been finding the same thing with my gamma movement test. He has helped me a lot by showing me the kind of criticisms which Montreal will produce against Goldstein and the kind of wrong conceptions their ‘localization mania’ leads them to.91

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80 Molly R. Harrower, “Changes In Figure-Ground Perception In Patients With Cortical Lesions,” *British Journal of Psychology* 30, no. 1 (1939): 47–51; Molly Harrower-Erickson, “Personality Changes Accompanying Cerebral Lesions: I. Rorschach Studies of Patients with Cerebral Tumors,” *Archives of Neurology & Psychiatry* 43, no. 5 (May 1, 1940): 859–90; Molly Harrower-Erickson, “Personality Changes Accompanying Cerebral Lesions: II. Rorschach Studies of Patients with Focal Epilepsy,” *Archives of Neurology & Psychiatry* 43, no. 6 (June 1, 1940): 1081–1107; Zygmunt Piotrowski, “Positive and Negative Rorschach Organic Reactions,” *Rorschach Research Exchange* 4, no. 4 (1940): 147–51.

91 Harrower, “Thursday’s Child.”
Thus, before Harrower had even arrived in Montreal, she was already engaged in successful coordination of research and testing procedures with one of the MNI’s most influential participants. This pattern would continue.

**The Exacting Requirements of an Extirpation Experiment**

Hebb and Harrower arrived at the MNI in 1937. For Harrower the entire environment of Montreal and the MNI seemed worlds apart from New York. In contrast to the “higgledy-piggledy set up” of Goldstein’s clinic, “Nothing could have been more startling than the difference in atmosphere between the Montefiore Hospital and the Neurological Institute, for the whole staff at the Institute was unified, dedicated, and untiring. One got caught up in the esprit de corps, the likes of which I have never again encountered.”

While for Hebb Montreal was familiar ground, for Harrower it was the latest in a long line of unfamiliar destinations. That being said, if Montreal was foreign ground for Harrower, working with patients was not, given her apprenticeship with Goldstein. By contrast, Hebb felt like a “babe in the woods in that clinical setting.”

“I would have preferred to work with rats or dogs, since human beings are so complicated. Also, the patients usually keep on living and don’t make their brains available for study.” Despite his initial discomfort with clinical work, Hebb’s ability to make inroads at the MNI is instructive. Trading on his knowledge of rat-brain anatomy acquired with Lashley, Hebb was able to gain the trust of his surgical colleagues:

>a fortunate circumstance got me off on the right foot. I had studied the rat’s diencephalon; now I wanted to know about man’s. When I got stuck, I would consult one of the others. Their answers were vague at best, and presently I realized that none of my fellow Fellows—none of these young neurologists and neurosurgeons—knew one end of the diencephalon from the other. What they had to know was the gross structure of the brain, the shape of the ventricles, the course of blood vessels, and so on. But that interest in anatomy established

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92 Ibid.
94 Hebb, 290.
Given the later importance of the diencephalon to both Hebb and Penfield’s theoretical writings, it is notable that Hebb was the agent who introduced much of the interest in that anatomical structure to the MNI. However, for the moment, Hebb’s knowledge of brain anatomy served mainly to allow him to masquerade as a physiologist, and to introduce his subsequent research to a larger audience.

Harrower and Hebb arrived with very different assignments. Penfield’s initial notion that Hebb would study vision had transformed, by the time of his arrival, into “a specific assignment to study the effects [of surgery on] intelligence as measured by tests of the standard kind.” By this, Hebb meant the standard Stanford-Binet I.Q. test and the Weschler Intelligence Test. By contrast, Harrower was more frequently ‘the woman under the tent’; that is, the surgical tent set up that allowed for the monitoring of the conscious patient during Penfield’s operations:

my most vivid memories are being closeted under the sterile green drapes with the [conscious] patient, for 8 hours at a time, while stimulation of the cortex proceeded. [M]y task was to give tests, ask questions, record answers….What Penfield needed at this stage of the operation was someone to observe what actually went on in the patient’s experience, in his mind or emotions if you will, while this artificial production of excitation in the brain was produced by the surgeons….My task was, of course, completely undefined, for no one knew what would be happening in the patient’s mind. I had to improvise, to choose my questions to elicit what would be most helpful to the surgeon. I might have to ask a patient a question which, if he could not answer it, might indicate that the removal of a particular area was dangerous to his mental achievements. Or, if a patient was describing an experience (evoked as if from nowhere, by the external electrical stimulus), I had to ask questions which would enable the patient to contrast this immediate experience with comparable experiences from everyday life. If the patient said, for example, that she suddenly heard the voices of children,

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95 Hebb, 290.
97 In this change of assignment, Penfield was clearly influenced by his investigations of his sister’s post-operative decline. According to Harrower, the emergence of neuropsychology within a hospital setting was unique at the MNI, and this emergence was largely the result of “personal experiences which may provide the motivating force necessary to carry through an idea to completion. I am thinking of Penfield’s observation of his sister’s changes in personality following the beginnings of her frontal lobe tumor, and my own determination to forsake the abstract study of perception for insight into changes in personality which had occurred in a close friend following drastic operative procedures.” Harrower, “Birth of Neuropsychology,” 1.
I would ask her, were they her children? Did they say anything they had said before? Was an old experience happening again, or was it something new?98

Additionally, Harrower conducted preoperative tests, control tests on patients who would not undergo surgery or cortical excision, post-operative follow-up testing, and attended x-ray and diagnostic meetings, and the daily rounds. Harrower’s psychological test were revealing, but the routine was physically. Operations could run from 7 to 10 hours and would leave all involved physically and intellectually drained. Following her first experience with an 8-hour brain operation, she returned to her Montreal apartment, drew a bath, and immediately fell asleep, resulting in flood damage and a memorable run-in with one of the city’s notoriously unpleasant landlords.99

While Hebb did occasionally take Harrower’s place under the green surgical tent, he was a man more comfortable with experimental animals and bloodless clinical testing. Almost immediately, however, he ran into a problem - the tests appeared to be useless. Nowhere was this more apparent than in one particular case.

In 1928, at about the same time that Penfield had operated on his sister’s tumor, 500 miles away in Nova Scotia, a 16-year-old lumber worker named Ken Matthews was struck on the forehead by an overhead log carrier at a sawmill. Matthews was rendered unconscious for ten days from the depressed fracture of his skull, and six months after his recovery he began to experience major epileptic attacks. Following arrival at the MNI in 1937, Penfield made the decision to operate, in the hope of removing the epileptogenic scar tissue. This would, however, involve a fairly extensive removal of tissue from Matthews’ frontal lobes - a risky bilateral procedure, even more radical than Penfield’s earlier operations on Ruth and William Hamilton.100

“At rare intervals,” as Penfield and Hebb later noted in their co-authored paper on the case, “chance presents to a neurosurgeon a lesion of the brain demanding treatment that would satisfy the

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98 Harrower, “Thursday’s Child.”
99 Harrower, “Thursday’s Child.”
exacting requirements on an extirpation experiment.” Indeed, the operation on Matthews (later anonymized as K.M.) presented as close to a controlled surgical experiment as Penfield and Hebb had ever encountered. Not one to let the opportunity pass, Hebb performed a battery of pre-operative tests on Matthews, and it is worth examining those tests in some detail. The tests included the Stanford-Binet IQ test, a revised version of the Army beta test, and the Pinter-Paterson test. During the first round of testing, Hebb noticed that K.M. underwent “a transient epileptoid state, of which the patient seemed unaware....For the first half-hour of the examination…the patient’s answers were slow and of poor quality.” Against the possible criticism that the testing was therefore invalid, due to K.M.’s subclinical seizure activity, Hebb ran the test again the next day, pausing and making adjustments to compensate for possible epileptic interference. “The answers were qualitatively as well as quantitatively improved....There was little change in the vocabulary test except for increased speed of response.” K.M.’s score of 83 on the Stanford Binet, then, “showed the patient at his best before operation,” and was as consistent and ‘clean’ a score as he was likely to get.

When Matthews was tested again following operation, the results were stunning; his IQ was now a consistent 94. By all accounts, this improvement in intelligence following extensive removal of the frontal lobes came as a shock to all involved, but was confirmed in follow-up testing conducted over a year after the operation. In addition to the intelligence testing, Hebb and Harrower conducted extensive supplementary testing in the form of picture-anomaly tests, block-stacking and sorting tests, maze-completion tests, vocabulary tests and Harrower’s modification of the Rorschach, all designed to find a consistent deficit in the man who was now missing over a third of his frontal lobes. As far as the intelligence testing was concerned, they came up trumps. “At once I

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101 Hebb and Penfield, 421.
102 Hebb and Penfield, 431.
103 Hebb and Penfield, 431.
104 Hebb and Penfield, 433.
found myself in trouble,” Hebb later recalled, “because when a clean removal was made from the frontal area, there was no loss of I.Q.”

Notably, the so-called ‘frontal lobe signs’ of Goldstein were absent, and Matthews seemed perfectly capable of categorizing and sorting objects, indicating “a reasonable degree of capacity for abstract thought.” The more formal testing of Harrower and Hebb was supplemented by extensive observation of Matthews in a less formal, but equally revealing way. Family members reported that Matthews seemed happier, and less agitated. Matthews himself report that his memory had greatly improved, allowing him to resume work on the family farm.

The strange case of K.M. came as a genuine surprise to all of the participants, but it served as the focal event around which the MNI’s interdisciplinary community first crystalized. For everyone involved, the peculiar results required intensive study and interpretation, and a rethinking of the most basic ideas about the functions of the frontal lobes. Most importantly, the clean link between an unproblematized ‘intelligence’ and the frontal lobes, which had first come into question with Ruth’s operation, was decisively shattered. At the same time, the experience with K.M. showed Penfield the value of collaboration with psychologists. According to Harrower, who spent a great deal of time hashing out the K.M. episode with Penfield:

The most remarkable characteristic of these discussion in letters was his [Penfield’s] ability to admit that his original point of view was wrong. He had placed great faith in standard I.Q. tests to show changes after removal of tumors….When the I.Q. showed no change, Penfield was at first loath to admit that other tests could show differences, but once convinced, urged the publication of the findings. A long letter of mine to Penfield, explaining my position, was, I believe a turning point in our relationship. My work mattered enough to me to confront him, and this was what he respected.

Harrower went on to quote Penfield stating, “Your point is very well taken. With the same pre-and post-operative Binet…K.M. now show[s] clearly the usefulness and superiority of your tests.”

107 Hebb and Penfield, 426.
109 Ibid.
This shift in Penfield’s attitude towards collaboration with psychologists is noteworthy. Previously, Penfield had regarded academic psychology with disdain, seeing it as the province of speculative Freudians and animal experimenters. For Penfield, the mechanisms of the mind were best uncovered through the application of experimental physiology, scrupulous clinical observation, and common-sense theorizing. The early work with Hebb and Harrower did not produce a complete about-face on these issues; for the rest of his career, Penfield retained the right to engage in his own speculation of the mechanisms of the mind. Moreover, his collaborators were often skeptical of Penfield’s own psychological theories. Looking back on his time at the MNI, Hebb recounted that:

Penfield was an innovator and a discoverer, a brilliant observer whose observations have fundamentally affected physiological and psychological thought, but he was not really a theorist and certainly not a psychologist, though in later years he lectured freely on the psychology of language. I think he had no great interest in finer physiological mechanisms, and his theoretical stance was a sort of phrenology. Taking for granted the separate existence of such things as memory and consciousness, he looked for their separate locations in the brain (memory in the temporal lobe, planning in the frontal lobe, consciousness in the brain stem). But he had the eye of an observer, he saw things that others did not see, and he saw that they were important and must be recorded. He was second only to [Karl] Lashley as a formative influence on me, but, on the whole, I learned from his remarkable therapeutic results; he offered little guidance.¹¹⁰

A similar disagreement over the nature of memory would occur years later with another psychological collaborator, Brenda Milner (see below). However, their disagreements over the finer details of psychological theory did not prevent Penfield from finding value in Hebb and Harrower’s contributions, nor from Hebb and Harrower finding value in their experiences. Penfield and Hebb, in their discussion of the K.M. case, came together to propose a plausible explanation for Matthews’ improved post-operative condition. The earlier low IQ score was not only the result of the lack-of-function of the injured frontal lobe tissue, but also the effects of that frontal tissue on the surrounding brain tissue. In effect, the diseased frontal lobes interfered with the functioning of the healthy surrounding tissue, resulting in a low IQ score. Once the diseased tissue was removed, and

the resulting sub-clinical petit mal seizures ceased, Matthews could perform more effectively on the IQ test (a phenomenon still reported today following certain types of brain surgery).\textsuperscript{111}

In subsequent years, Hebb developed his experiences in Penfield’s clinic into a grand theory of intelligence and brain function that became hugely influential for the development of cognitive neuroscience, and was crucial in bringing Brenda Milner to the MNI. However, in 1940, the main impact of the K.M. episode was to legitimize the value of psychologists at the institute. In her diary, Harrower recorded that:

Hebb has done an enormous amount of work to help “the psychologist at work” in the Institute. He has systematically broken down weird ideas and prejudices, and he has done it without the wear and tear to himself that it would have done to me. There is no question but that he is accepted and respected, and his re-education work seems to me excellent, he was a school master before going into psychology. The more one is accepted the more one is teased, at present they are a little shy of teasing me, but that much will come!\textsuperscript{112}

Elsewhere, Harrower’s efforts to develop reliable diagnostic tests were impressing her surgical colleagues. Certain variations of her Rorschach and picture anomaly tests had proved valuable in localizing hard-to-find tumors. In February of 1939 Penfield wrote to Alan Gregg, stating:

Allow me to express our gratitude to the [Rockefeller] Foundation for the help which they have given us at the Montreal Neurological Institute in psychology by means of a fellowship to Dr. Molly Narrower and the grant-in-aid which has defrayed a part of the salary of Dr. Donald Hebb. Each is carrying out work of real scientific value and somewhat unexpectedly, we have come to lean upon them for help in our clinical problems.\textsuperscript{113}

A month later, Penfield again requested funds from Gregg to keep on his two psychologists, adding that:


\textsuperscript{112} From an extract of Harrower’s diary held in the Hebb papers. 23 January 1938. File 1991-0040.01. DO Hebb Fonds. Although Harrower did not elaborate on the ‘weird ideas and prejudices’ Hebb had encountered, they likely related to preconceptions the surgeons had about the relationship between laboratory psychology and psychoanalysis, which the surgeons at the MNI viewed with either disinterest or suspicion.

\textsuperscript{113} Penfield to Gregg, 23 February 1939. File C/G 5-3/3 Rockefeller Gregg 1935-1955. Box 44. WP Fonds.
Their work is just coming to the stage of bearing fruit both from a psychological and from a practical neurological point of view. Their results must now be carefully balanced against clinical standards in each case. In fact the Rorschach method seems capable of making reliable organic diagnoses. When this is made known in clinical circles it will, in my opinion, awaken as much of a demand for this adjunct [of clinical neuropsychology] as there is at present for neurological electrography [EEG].

Penfield’s mention of electroencephalography is noteworthy. At the same time that he was collaborating with Hebb and Harrower, Penfield was also working with Herbert Jasper to apply the EEG to localize epileptic foci, a procedure that would become standard worldwide, and produce some of the MNI’s most important brain localization work (see Chapter 3). That Penfield considered his psychological collaborators to be in the same league as Jasper indicates the degree to which the once-foreign specialty had made inroads within the institute. For her part, Harrower found herself becoming a more-and-more integral member of the MNI team, called upon to present her work at the weekly Fellows meetings:

Once a week one of our number talked of his specialty. The occasion when “she” [Harrower] first talked about “her” specialty was a milestone, the focus of great anxiety, but with comparable relief and even triumph afterwards! One by one, the Fellows found meaning in the psychological tests for themselves, which had seemed so alien at first. And when the culprit of a far-reaching stealing incident [cerebral ‘stealing’ of blood from one area to another that can result in cell death] was discovered by the association test, my stock soared. Likewise, the prediction by psychological tests of a tumor in a silent area, clinically, brought forth a pleasantly joking remark, as I blushed scarlet at the chief’s [Penfield’s] praise, “Can you turn any other color?”

Penfield continued to collaborate with Hebb following his departure for a professorship at Queen’s University. Their paper on Matthews was published in 1940, to considerable interest.

Meanwhile, Harrower continued to refine and expand her battery of clinical psychometric tests at the MNI, most notably the Rorschach. This resulted in her other major contribution, the definitive rejection of the notion of an ‘epileptic personality.’ According to Harrower:

At that time there was a controversy over the legitimacy of the concept of the “epileptic personality.” Some Rorschach workers had claimed to have demonstrated a recognizable test

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115 Harrower, “Thursday’s Child.”
entity to support the clinical position. My studies showed exactly the opposite, namely persons with focal seizures, resulting from scar tissue, reflected a wide variety of personality patterns, with a scatter identical to unselected “normals.”

This negative result served to support the consensus, emerging from EEG research, that epilepsy was not, in fact, a psychiatric disorder of psychogenic origin, but rather an organic disease. It is fitting then that Harrower’s efforts to legitimate the role of the psychologist appeared in a chapter alongside Jasper’s EEG work in Penfield and Theodore Erickson’s *Epilepsy and Cerebral Localization* (1941), which served as a capstone to the first phase in the MNI’s remarkable growth.

**The Organization of Behavior**

The outbreak of WWII did much to disrupt activities at the MNI. Part of this was by design. Penfield, since his youth, had harbored an almost fanatic devotion to the idea of duty, and retooled much of the institute for wartime research. Herbert Jasper, quickly emerging as the institute’s most exciting young research talent (see Chapter 3), began work on the problem of pilot ‘blackout’ that posed such a threat to Allied fighters then attempting to stave off the German bombing campaigns against Britain. This meant that much of the civilian peacetime research of the institute had to be put on hold, as the MNI prepared to treat brain-injured soldiers returning from Europe, as well as to field its own specialist expeditionary surgical hospital in England.

The early war years also brought changes for Harrower. For one, she became Molly Harrower-Erickson, following her marriage to Penfield’s colleague Theodore Erickson, co-author of *Epilepsy and Cerebral Localization* (1941). Second, the emphasis on wartime research shifted Harrower’s career in a new direction. While Harrower’s Rorschach work at the MNI had been mostly diagnostic, she never lost sight of its possible use as a test for uncovering underlying personality

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116 Harrower, “Thursday’s Child.”
characteristics. Indeed, she had long hoped to expand its use at the MNI, both for the purposes of enhancing post-operative therapy, and for expanding the treatment regimen to include more psychiatric cases of ‘psycho-neurosis.’ While little came of her therapeutic efforts, Penfield had been so impressed by Harrower’s diagnostic abilities that he supported the expansion of her work for screening pilots and airmen for the Royal Air Force. The study, which began in October of 1939, was supported by a grant from the National Research Council, and was enthusiastically reported to Frank Fermont-Smith of the Macy Foundation:

In the hope of providing a method of predicting those men who will be most apt to break under strain, a psychologist in our department, a Dr. Mollie [sic] Harrower-Erickson...has made a study of the psycho-neurotics appearing in civil practice in the Outdoor department here in Montreal, using the Rorschach tests. This work was carried out under a grant from the National Research Council of Canada. I have been impressed by the results and have watched the method used on cases who had lesions of the brain, and a large number of psycho-neurotics and it seems to me that there is a personality structure which can be outlined by the method.119

Penfield’s belief in the diagnostic value of Harrower’s Rorschach tests led him to endorse the method more generally. Harrower adapted the traditional Rorschach into a shortened form that could be administered in as little as 15 minutes to large groups of potential recruits. The method was adopted by the American armed forces, and Penfield’s endorsement to Frank Fermont-Smith led to Harrower’s participation as one of only two women at the Macy Conferences, which served as the launching pad for the post-war cybernetics movement (the other was Margaret Mead).120 Perhaps even more importantly, Harrower copyrighted her emerging group Rorschach test, forever linking her name to a psychometric test that was enjoying considerable popularity, and ensuring her

119 Penfield to Frank Fermont-Smith, 14 November 1940. File C/G 40 E-F, Box 55, WP Fonds.
120 Space does not permit a more extended discussion of Harrower’s participation at the Macy Conferences, or her relationship with cybernetics, but a few points are worth mentioning. Harrower had been invited by Frank Fermont-Smith, and her initial work with the Macy Foundation largely pertained to extending and developing her Rorschach technique for use in the State Department and US Armed Forces. Her participation at the Macy Conference often involved discussions of perceptual processes and Gestalt theory, and she often related stories of testing patients in Montreal. Moreover, Harrower often acted as a sort of translator of Gestalt ideas for the more behaviorally inclined Macy Conference members. For more, see Steve J. Heims, The Cybernetics Group (Cambridge, MA: The MIT Press, 1991), 138–40, 154–55, 234–35.
financial independence following her later separation from Erickson. The pair had left Montreal in 1942 following a prestigious offer for Erickson from the University of Wisconsin, Madison; unfortunately, the offer did not include a spousal hire for Harrower, and the marriage did not survive the move. The two divorced amicably in 1945.\textsuperscript{121}

Harrower’s involvement with the MNI ended in 1941, although she carried on correspondence with Penfield for decades. By contrast, while Hebb had left Montreal for a teaching position at Queen’s University in 1940, his experience at the MNI never strayed far from his mind. Hebb would return to Montreal in 1947, but the intervening development of his own thought is important to examine in order to understand the subsequent rebirth of neuropsychology at the MNI.

Having the standard intelligence tests fail so dramatically to detect any abnormality in Penfield’s patients was both frustrating and intriguing. As Hebb later recollected, “The frontal-lobe work in Montreal was truly revolutionary, and I met frank skepticism in reporting, for example, that a young man made a perfect Stanford-Binet score after removal of the left prefrontal lobe or that the removal of both prefrontal lobes in another patient [clearly Matthews] had raised that subject’s IQ from 80 to 95.”\textsuperscript{122}

Hebb continued to puzzle over these peculiar results during his time at Queen’s while completing his write-up of the Matthews case with Penfield. Indeed, Matthews himself became something of an obsession of Hebb’s. Writing to Penfield in 1940, Hebb effused that, “I have had a number of favorable comments on the paper on Matthews - I believe more firmly than ever that you made neurosurgical history when you operated there. The case is one that will have to be taken into

\textsuperscript{121} Space also does not permit an extended discussion of Harrower’s post MNI career, but briefly, after her war work and her participation in the Macy Conferences, Harrower retrained as a psychotherapist, and continued to work on her various Rorschach tests. Notably, 1976 she published an extensive analysis of the Rorschach tests given to the Nazi war criminals at Nuremberg. For more on Harrower, see Harrower, “Inkblots and Poems”; Searls, \textit{The Inkblots: Hermann Rorschach, His Iconic Test, and the Power of Seeing}, 229–80.

\textsuperscript{122} Hebb, “DO Hebb,” 291.
account in any discussion of localization of function from now on - may you have more like it.'" A year later, a letter requesting Penfield’s advice on an upcoming experiment with dogs made the stakes of the work with Matthews clear:

I have worked out a hypothesis to account for such cases as that of Matthews, to the effect that an intact brain is necessary to develop ordinary habits, perception and comprehension, but that it may not be necessary to retain these things - and the clinical data support this idea rather well. I would hope then that the dogs which had been trained with a normal brain might retain some of their simple training after the frontal regions had been removed, but that they would lose in initiative and power of new acquisition.

For Hebb, the case of Matthews, and other similar frontal lobe removals, provided strong evidence that the prevailing theories of intelligence in both humans and other animals were wrong. Echoing the theories of L.L. Thurstone that he had imbibed at Chicago, and in direct contradiction to his mentor Lashley, Hebb argued that intelligence was not a single discrete mental entity, but rather a collection of interrelated components. But Hebb went beyond Thurstone’s faculty approach to intelligence. In Hebb’s view, the development of any given skill, concept, or scrap of knowledge might be dependent on one brain system, while the retention of that knowledge might depend on a wholly different system - one that might be spared by a brain operation. Hebb took the opportunity to re-examine the data from his early study of visual perception done for Lashley, and concluded that, while rats reared in darkness could still develop normal visual perception, they did so at a massively reduced rate. Thus, Hebb concluded, from an increasing body of overlapping experimental and clinical data, that even visual perception and the perception of abstract forms (the sacred innate Gestalts of Kohler, Kofka and Goldstein, and the inherited basis of intelligence of Lashley), had to be *learned from experience*. At the very least, the ‘frontal lobe signs’ of Goldstein would

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123 Hebb to Penfield, 24 October 1940. File C/G 40 H-I-J, Box 55, WP Fonds.
124 Hebb to Penfield, 5 October 1941. File G/G 41 H-I-J, Box 56, WP Fonds.
have to be radically revised, given that in all of Penfield’s carefully examined cases, “intelligence as measured by current tests is not affected by removal of tissue from the frontal pole.”

Hebb rejoined Lashley in 1942 after he had become the director of the Yerkes Primate Lab in Orange Park, Florida. While there, he continued to rework his new theory of intellectual development, combining it with the updated neurohistology then emerging from the laboratory of Raphael Lorente de Nó. de Nó, who had studied in the Spanish laboratory of Santiago Ramon y Cajal, had recently proposed that parts of the mammalian brain might not act as simple stimulus-response reflex arcs. Instead, certain neural structures might be capable of autonomous activity, independent of sensory input. These independent constellations of cells, which de Nó came to call ‘reverberating circuits,’ might be capable of explaining the ability of a mammalian brain to retain information over extended periods of time. For Hebb, this notion could be combined with his own training in Pavlovian conditioning methods to suggest how these reverberating circuits could become associated with one another into sequences of activity that corresponded to perceptions, thoughts, and actions. When supplemented with the emerging evidence from the EEG – evidence that suggested that parts of the brain were always active, independent of stimulus, Hebb’s theory emerged as a powerful new way of linking mind, brain and behavior. Moreover, it served as a persuasive rejoinder to both the dominant behavioral school of psychology. It was also a trenchant criticism of the neuropsychology of Lashley, who insisted that perception and intelligence were innate. Finally, it cut the Gordian knot between the strictly localizationist school of neurology, and the equipotential or holistic one; because mental activities were dependent on certain interacting groups of cells, the higher mental faculties were diffused throughout the cortex, but might still be identified.

dependent on certain crucial brain structures. Thus, intellectual deficits following brain injury would still be present, but would not be revealed by such crude tests as the Stanford-Binet or Weschler intelligence tests. Rather, these deficits could be found with subtler, more carefully designed tests.\textsuperscript{126}

The Organization of Behavior (1949), the monograph that outlined Hebb’s new theory of brain activity, and its relation to psychology, was a masterwork of cautious speculation, combined with a close and careful reading of an eclectic range of experimental and clinical data.\textsuperscript{127} Indeed, Hebb made it clear that his intention was “to bring together a number of different lines of research, in a general theory of behavior that attempts to bridge the gap between neurophysiology and psychology, as well as that between laboratory psychology and the problems of the clinic.”\textsuperscript{128} From his work with Matthews, a tentative yet insistent theory had emerged that aimed to reconcile psychology and neurology, and do justice to both. Employing a nautical metaphor befitting a Nova Scotian, Hebb argued that:

psychologist and neurophysiologist thus chart the same bay - working perhaps from opposite shores, sometimes overlapping and duplicating one another, but using some of the same fixed points and continually with the opportunity of contributing to each other’s results. The problem of understanding behavior is the problem of understanding the total action of the nervous system, and vice versa. This has not always been a welcome proposition, either to psychologist or physiologist.\textsuperscript{129}

Taking aim at the radical behaviorism of Skinner and the rest of American psychology, Hebb went on to argue that the way forward in psychology was to engage more seriously with the work of neurology, rather than the models of Skinner and Hull, or the hypothetical mathematical modeling

\textsuperscript{128} Hebb, The Organization Of Behavior: A Neuropsychological Theory, vii.
\textsuperscript{129} Hebb, xiv.
of neurons advanced by the cyberneticists Warren McCulloch and Walter Pitts.\textsuperscript{130} While Hebb’s own model of the brain was in many ways crude and simplistic (a point he acknowledged), it served as a beacon for all of those who wished to return to the study of the brain as the basis of mind.

Hebb returned to Montreal in 1947 with The Organization of Behavior in manuscript form. The war had ravaged McGill’s psychology department (most of its faculty had been dispersed by war work), and the recently hired R.B. Macleod had been tasked with reviving it, and putting it on a track that was more in line with the natural sciences than the traditional orientation towards philosophy and the moral sciences. With a strong recommendation from Penfield, who had become a considerable force in McGill politics, Hebb was hired with a mandate to expand the experimental operations of the department. Additionally, Hebb inaugurated a series of evening seminars built from the manuscript of his book.\textsuperscript{131} These seminars, and Hebb’s expansive ideas about mind and brain, would prove life-altering for many students that passed through them, but none more so than one newly-married English woman, recently arrived in post-war Montreal.

\textbf{A Kept Woman}

Brenda Langford (1918-) was born into a Manchester home that valued art and education, music and language. Her father, a music critic for the Manchester Guardian, and her mother, a music teacher at a local elementary school, nurtured their precocious daughter’s intellectual interests, in spite of their bitter disappointment at her total lack of musical talent. A robust education in languages, notably conversational French, combined with a passion for algebra and mathematics that led Brenda to Cambridge University as a young woman. Almost immediately, however, her hopes for a career in mathematics were dashed by a realization of her own limitations with the subject,
along with the sex discrimination that put her in tight competition for the limited number of scholarships for which women were eligible. Langford considered the possibility of switching into logic and philosophy, but was dissuaded from doing so by the repeated refrain of her college classmates that one needed to earn a living; as a daughter of England’s struggling middle class, training of more practical significance would have to be found.¹³²

Experimental psychology hardly seemed an appropriate solution to the problem of financial stability, but the subject appealed to Langford, and in the worst-case scenario she could always move to London and become a factory inspector, the only vocation for which psychology seemed applicable. Still, Langford began training in the psychology department at a perspicuous time, and with two crucial advisors. The first, F.C. Bartlett, had reinvigorated the relatively small community of Cambridge psychologists with his experimental studies of memory. His 1932 monograph on the subject, Remembering: A Study in Experimental and Social Psychology, was a minor success in England, but remained relatively unknown outside of Great Britain until the 1970s, when it was rediscovered and consecrated as one of the intellectual forerunners to cognitive psychology. Bartlett had argued, contra the prevalent notion of memory as an indelible trace left on brain, that remembering was in fact a reconstructive enterprise, in which memories were altered and edited during the act of recall. Langford’s immersion in the details of Bartlett’s memory theory proved important later in her disagreements with Penfield, but for the moment it sufficed that Bartlett was a protective and helpful mentor in psychology, and one who maintained his connection with the neurological sciences.¹³³ For much of the late nineteenth and early twentieth centuries, psychology in Britain remained aloof from the natural sciences and medicine generally (at Cambridge, it was categorized as

¹³² Much of the material for this chapter comes from an oral history interview conducted with Milner by Michael Bliss of the University of Toronto. Unless otherwise stated, quotations are from this interview: Aubie Angel, The FCIHR Video History of Medicine in Canada Project: Brenda Milner and Memory, The FCIHR Video History of Medicine in Canada Project (Toronto: Toronto: Friends of Canadian Institutes for Health Research, 2013).
part of the Moral Sciences tripos, rather than the Natural Sciences). Bartlett, however, was an exception, and drew a great deal of inspiration for his theories of memory from his friend, the English neurologist Henry Head, who had insisted that an understanding of the nervous system could shed light on psychological issues.¹³⁴

The close relationship between neurology and psychology was paramount for Langford’s other early influence, Oliver Zangwill.¹³⁵ Zangwill, who was later the prime-mover for the development of neuropsychology in Britain during World War II, had been supervised by Bartlett, and had maintained the close ties between academic psychology and the British medical establishment that had emerged during World War I. Significantly, it was through Zangwill that Langford developed her interest in organic brain damage.¹³⁶ “It was he who first taught me the value of studying the behavioral effects of brain lesions, because he believed that through an analysis of disordered function one could gain insights into the functioning of the normal brain.”¹³⁷

Following Langford’s undergraduate degree, she hoped to continue her research on sensory conflict at Cambridge, but the outbreak of World War II set her career on a different path. In 1939 Langford was recruited by C.P. Snow to be an experimental officer for the Ministry of Supply, a job that appealed to Langford, despite being the only woman in the unit. “I had a good war,” Langford later recalled. Co-ed living quarters led her to form close associations with a number of other young scientists, who pooled their ration booklets to improve the quality of their diets while the Battle of Britain raged in the skies above London.¹³⁸

One member of the unit with whom Langford became particularly close was a young electrical engineer named Peter Milner. Their career began as a professional collaboration on the

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¹³⁴ Winter, Memory: Fragments of a Modern History, 197–224.
¹³⁸ Milner, 280–82; Angel, Brenda Milner and Memory.
creation of a control mechanism for radar, the new technology that was so crucial to the war effort. Milner designed a radar simulator, and Langford experimented with different methods of display and control in order to determine the most effective way for the operator to track the target. Over the next two years at the radar research establishment at Malvern, their professional relationship became romantic. The pair became close enough that when Milner was asked by John Cockcroft (later a Nobel laureate physicist) to move to Montreal after the war to help initiate the Canadian Atomic Energy project, Langford decided that the relationship was worth preserving. Nevertheless, she was conflicted about the possibility of giving up a promising career, and concerned about the possibility of moving to a different continent.¹³９ “I was dragged screaming into Canada,” she later reflected.¹⁴⁰ Nevertheless, in late 1944 Peter and Brenda were married (henceforth referred to as Brenda Milner), and two weeks later set sail for Boston with a party of war brides in a converted troopship, zigzagging across the Atlantic to avoid German submarines.¹⁴¹

After a tour of the MIT physics labs, Peter and Brenda finally learned that they would be settling in Montreal, a fact that had been kept from them until that point owing to wartime secrecy. The most attractive component of Montreal to Milner at that point was its bilingual population. “I was so excited to be in a French-speaking city,” Milner recalled, and vowed to make use of her childhood French-language lessons to gain employment. “I couldn’t be a kept woman,” Milner recalled, and continued to search for work in her new home, a task made somewhat difficult by the fact that her Cambridge BA degree was considered less valuable than a North American Masters or Doctorate degree.¹⁴²

Milner’s ability in French, however, did make her an attractive applicant for the French-language Université de Montreal, on the northern side of Mount Royal (the mountain served as the

¹⁴⁰ Angel, Brenda Milner and Memory.
¹⁴² Angel, Brenda Milner and Memory.
demarcation line between Montreal’s English and French-speaking populations). The emerging psychology department at the Université de Montreal was the peculiar creation of Father Noël Mailloux, a Dominican priest who spent his days teaching young Catholics about Freudian psychoanalysis, and his evenings teaching the same students the beatitudes of Thomas Aquinas, a program in which he apparently saw no contradiction. Milner was hired to give a series of lectures on Bartlett’s theories of memory, which eventually secured her a full-time position.\textsuperscript{143}

Milner was invited to attend the evening seminars at McGill led by Hebb, who was seeking feedback on his manuscript for \textit{The Organization of Behavior}. Milner attended the seminars (along with the future NIMH chief scientist Mortimer Mishkin) and “discussion after the seminars often continued late into the night. It was an exciting time and hastened my decision to do a Ph.D. at McGill.”\textsuperscript{144} Reflecting further on her first encounter with Hebb, Milner noted that what made Hebb attractive as a doctoral adviser was not his theories \textit{per se}, but rather that his biological focus made her own experience in the more biologically-oriented psychology of England relevant and applicable within an environment dominated by behaviorism. “Cambridge was really very biological in its orientation…and in North America you [had] very brilliant psychologists - very theoretical - who have all sorts of mathematical models of behavior, but didn’t feel that it was time to talk about the brain….Hebb really did feel that it was time that you had to try to relate brain and behavior,” Milner recalled.\textsuperscript{145} Thus, Hebb’s theory of multisynaptic connections, and a loosened scheme of localization, served as a way to translate the national styles of American and British psychology within the liminal space of Montreal, which emphasized the practical value of psychological theory, and mixed those theories in a heterodox environment.

Milner began her PhD with Hebb in 1949, intending to work on the topic of tactile form perception in the congenitally blind, but once again, circumstances intervened. Hebb had, upon

\textsuperscript{143} Angel.

\textsuperscript{144} Milner, “Brenda Milner,” 282.

\textsuperscript{145} Angel, \textit{Brenda Milner and Memory}. 
returning to Montreal in 1947, secured a promise from Penfield that he could send one graduate student to the MNI to study his patients. Milner was the perfect candidate, given her previous training with Zangwill. In June of 1950 Milner began investigating perceptual deficits in patients who had undergone the relatively novel procedure of temporal lobe resection for epilepsy.146

The Montreal Method

Beginning in the 1940s, and inspired by animal work, Penfield had been gradually extending his surgical removals into deeper brain structures within the temporal lobes, eventually including the amygdaloid-hippocampal complex, the horseshoe-shaped structure that straddled the brainstem and extended into the temporal lobes. The results had been encouraging, especially when combined with careful pre- and intra-operative EEG testing which could help to localize the epileptogenic foci. By 1950, Penfield could report that of 68 cases of temporal lobe resection, 65% of operations had been successful, with 25% experiencing complete relief of their seizures. The field was expanding rapidly – within a year, Penfield had done 50 more such operations.147

By all accounts, the creation of the new temporal lobe operations, later dubbed the ‘Montreal method,’ breathed new life into the MNI in the late 1940s. A funding crisis had been underway from 1947 to 1949; while patient volume was high, the “clinical set up lag[ged] behind the scientific and professional work.”148 Demand for operations was great, but the current facilities were inadequate to the task. Patients who had travelled from the United States and Europe for operations were housed in a temporary wooden annex that had been built for military patients during the war. The annex had now become a fire hazard and Penfield petitioned anyone who would listen of the need for a new wing to house the incoming patients. In 1949, Penfield could state publicly that

146 Angel; Milner, “Brenda Milner,” 283.
148 Penfield to John Bassett, 9 February 1949, A/N 16-2, Box 14, WP Fonds.
“either the public must support voluntary hospitals, or medicine must be socialized.” He continued:

The Institute might well have been built in a large city in the United States instead of Montreal. Plans were laid for that at one time. But the decision was made to build it here because of the hospitality of the citizens of Montreal, the City Council, and the Provincial Government….We have worked hard. We have stated the case of the Montreal Neurological Institute. If there is no Canadian, or group of Canadians, ready to make permanent its organization – then let the doors of its hospital close.150

Privately, Penfield used the emerging promise of the temporal lobe operations to secure the funding he needed. In a letter to John Bassett, the publisher of the Montreal Gazette, Penfield related the story of Madame Poinso-Chapuis, the wife of France’s Minister of Health, who was presently giving lectures in Ottawa and Montreal in order to pay to bring her son to Montreal so that he might receive the new operation. “The operation happens to be of the kind that could only be done [here]…because Jasper…and others have developed special techniques of study and localization and because special work is going on in our laboratories that guides the surgeon.”151 Bassett evidently leaned on a number of connections, because Penfield was eventually accorded an audience with the Premier of Quebec, Maurice Duplessis, who secured the funding necessary to settle the MNI’s debts and construct a new wing to handle the increasing volume of temporal lobe cases. Only a year later, in 1950, Penfield could report that “The hospital doors do not need to close. The Montreal Neurological Institute may now fulfill its destiny as a provincial hospital and a national institute, and its doors will never be closed.”152

150 Montreal Neurological Institute, 7.
151 Penfield to John Bassett, 9 February 1949, A/N 16-2, Box 14, WP Fonds.
At the same time, operations for temporal lobe epilepsy presented the most intriguing scientific possibilities that the Montreal team had yet encountered. The remarkable responses drawn forth by Penfield's electrical probe during operations, including hallucinations, emotions, and even recall of what appeared to be accurate memories, called for detailed exploration. “The temporal lobe is the most common site of onset of focal cerebral seizures. This provides us with abundant material for study,” stated Herbert Jasper in an internal document that outlined what was coming to be known within the MNI as the “Temporal Lobe Research Project,” the most expansive interdisciplinary study that had yet been undertaken at the institute:

Further analysis of such material as is offered by patients with temporal lobe seizures, if made by a team of workers, should yield information of importance to our understanding of the normal function of this large portion of the human brain, and the mechanisms whereby its dysfunction may result in mental disorder and abnormal behavior. Such a project requires a coordinated program of work in psychiatry, psychology, neurology and neurophysiology to achieve the greatest success.\(^{153}\)

It was under the auspices of this temporal lobe study that Milner first began working at the MNI, testing for perceptual deficits following temporal lobe operations. An immediate difference of opinion between Milner and Penfield serves to highlight their divergent perspectives. Inspired by animal experimentation with monkeys, Milner hoped to study the effect of temporal lobe resection on visual perception:

These surgeons can be quite naive. I can remember Dr. Penfield saying, “the temporal lobe is so far away from the occipital [visual] cortex, why are you working on vision? Well, you know, there are multi synaptic connections between the two areas, but surgeons were not thinking about things like that in those days.\(^{154}\)

Here, the loosened localization of Hebb’s connectionist framework ran headlong into Penfield’s strict surgical localization. Despite their theoretical disagreement, Milner continued her early testing


\(^{154}\) Angel, Brenda Milner and Memory.
work, commuting from her day job on the French side of Mount Royal by streetcar, and trying to
guess which patient Penfield planned to operate upon so that she might manage to run a few tests.\(^{155}\)

Milner finished her PhD in early 1952, having found some small but intriguing visual deficits
in the temporal lobe patients. Despite the difficulties of those early days, Milner had become
enchanted by her encounters with surgical patients, and hoped to stay on at the MNI, a decision that
led Hebb to warn her, rather surprisingly, that “no psychologist can survive for long at the MNI.”\(^{156}\)
Hebb’s warning, however, was overruled by a development that would once again emerge from a
single operative procedure, much in the way that Ruth and K.M.’s had. The patient was a young
engineer named (P.B.), and the memory deficit that he developed following his operation cemented
Milner’s place at the MNI. “I’ll get you an office in the building,” Milner recollected Penfield saying.
“We need you.”\(^{157}\)

**The Temporal Lobe Research Project**

Penfield’s operations for epilepsy had great scientific potential, but also entailed great risk.
The early optimism of the temporal lobe research had generated some of the MNI’s most exciting

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\(^{156}\) The reasons for this unusual warning are not what they might initially appear to be. By all
accounts, Hebb considered his working relationship with the MNI to be crucial for building his own
school of physiological psychology at McGill; he had made a point of sending his graduate students
to attend Herbert Jasper’s neurophysiology seminars, and maintained a warm relationship with
Penfield. Reflecting on Hebb’s warning, Milner remarked: "Maybe he felt that not having an MD, in
the long run, would be a disadvantage. Maybe it was too hierarchical an environment for
him….Maybe, though he never said this, maybe because he thought Penfield thought he could be
his own psychologist. You see Penfield was a surgeon who was interested in memory - he wasn't
trained to be a psychologist, but he was interested in psychological issues." Despite his productive
relationships with Hebb and Harrower, Penfield remained aloof to the world of academic
psychology. Moreover, as will be discussed in Chapter 4, the period between 1941 and 1950, during
which there was no permanent psychologist at the MNI, had seen a rather disastrous breakdown in
communication between the MNI and the newly formed Allen Memorial Institute of Psychiatry,
headed by Ewen Cameron. It is entirely likely that Penfield wished to keep his own council when it
came to issues of the mind. Meanwhile, for Hebb, the issue was largely one of self-determination; he
disliked serving as the technical assistant of a neurosurgeon, spending weeks idly waiting for a

\(^{157}\) Angel, *Brenda Milner and Memory*. 

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scientific publications; the remarkable responses drawn forth by the electrical probe during operation, including hallucinations, emotions, and even recall of what appeared to be accurate memories, produced Penfield’s most exciting publications since his early microscoptal research. In 1951, Penfield presented a new theory of memory formation that was in direct opposition to the prevailing wisdom of Lashley and other psychologists. Memory, according to Penfield, was a permanent record of conscious experience that was stored in the temporal lobes and could be accessed by electrical stimulation. Furthermore, the psychical hallucinations and automatism that occurred during temporal lobe seizures, and their reproduction during surgery, suggested possible mechanisms of psychiatric disturbance. As Penfield wrote to the new director of Medical Research at the Rockefeller Foundation, “exciting information is at hand on memory, dreams, interpretation of perception, deviations of normal behavior and psychoses.” Penfield continued:

A large number of patients have presented themselves in whom there were behavior abnormalities among these temporal lobe cases. Many of the psychical hallucinations which they experience have resembled those seen among psychiatric patients, and as a result of a series of local epileptic discharges they have not infrequently become psychotic, a condition which clears up when the attacks are controlled…. these facts open a field for a psychological-psychiatric-neurological study which can only be undertaken adequately by a group working together. We have the material, we have the cases, we have a group of relatively young people ready now to tackle the job.

Despite this optimism, Penfield added one somewhat ominous detail: “In some cases removal of one whole temporal lobe has reduced the capacity for memory; in other cases there has been no interference whatever.”

Brain surgery always involved risks. Penfield had initially hoped to confine his removals to the surface of the temporal lobe, sparing the subcortical structures of the amygadaloid-hippocampal complex. Yet conflicting EEG data, combined with early use of microelectrode stimulation,

159 Penfield to Robert S. Morrison, 10 October 1952. A/N 16-1, Box 14, WP Fonds.
160 Ibid.
161 Ibid.
suggested that to relieve temporal lobe seizures, Penfield would have to venture further and further into subcortical structures. On the one hand, this produced crucial scientific discoveries. By correlating EEG data, microelectrode stimulation, animal experimentation and careful observation of the memory, hallucinatory and emotional reactions of surgical patients, the MNI research fellow Maitland Baldwin managed to conclusively demonstrate that the cause of temporal lobe seizures typically lay in subcortical structures, rather than on the lateral surface of the lobes. This correlative work not only explained the aberrant EEG findings, but unified the entire study of temporal lobe seizures. It was this kind of interdisciplinary coordination of clinical and laboratory work that lay at the heart not only of the MNI, but of its novel Montreal method.\footnote{Feindel, Leblanc, and Villemure, “History of the Surgical Treatment of Epilepsy,” 476–81; William Feindel, “Brain Stimulation Combined with Electrocorticography in the Surgery of Epilepsy: Historical Highlights,” n.d., 8; Penfield and Flanigin, “Surgical Therapy of Temporal Lobe Seizures”; Wilder Penfield and Maitland Baldwin, “Temporal Lobe Seizures and the Technic of Subtotal Temporal Lobectomy,” \textit{Annals of Surgery} 136, no. 4 (1952): 625.}

Yet at the same time, the Montreal method was one of the riskiest of Penfield’s surgical procedures. Much like the earlier frontal lobe work, Penfield was often removing brain structures whose functions were poorly understood. He was, in effect, partially ‘flying blind.’ Yet the risks seemed worth it – a surgical treatment for temporal lobe epilepsy seemed close at hand, and it was largely taken for granted that if one removed one half of a cortical structure (such as the hippocampus), the other half could compensate.

This understanding of hemispheric redundancy was shattered by the surgical patient P.B. An American engineer, P.B. underwent a temporal lobectomy that initially involved surface removal of the portions of the temporal lobe, in accordance with Penfield’s initial understanding of temporal seizures. However, this had produced unsatisfactory results, leading to a second operation in which Penfield removed more of the deeper, mesial structures of the temporal lobe, including the left horn of the hippocampus. The results were disastrous. Emerging from his post-operative recovery, P.B.
apparently said to Penfield in an accusatory tone, “what have you people done to my memory!” P.B. had been stricken with anterograde amnesia, a debilitating condition which rendered him incapable of holding on to experiences for more than a minute or two. According to Milner, “He did not remember what he’d had for breakfast, he did not remember whether his wife had been to see him that day…he did not recognize me.” Milner tested P.B. extensively, using many of the intelligence tests bequeathed to her by Hebb when she had begun work at the MNI, and found that his intelligence was unaffected, along with his short-term memory. Penfield consulted with Milner and Jasper on this peculiar case; Jasper argued that the cause was probably an unpredictable anomaly relating to the patient’s brainstem, and was unlikely to be repeated.

Within a month, it happened again. This time, the patient (F.C.) was a young glove-cutter, who had the same left temporal lobectomy and removal of the left hippocampus, and experienced precisely the same memory deficit. “This really stopped Penfield in his tracks,” Milner recalled. Indeed, the possibility that temporal lobe operations would need to end altogether stood in the way of a great deal of Penfield’s surgical agenda. It was these deficits, as much as the scientific promise of temporal lobe research, that led to Milner’s invitation to join the MNI, and the temporal lobe research project, in 1953.

The temporal lobe research project had its first formal meeting on 31 January 1953. The minutes of the first meeting offer a remarkable window into the improvised interdisciplinary environment of the MNI as it prepared to tackle its most complex puzzle to date. In attendance were Penfield, Jasper, Hebb and Milner, along with MNI neurologist Lamar Roberts, Penfield’s surgical protégé William Feindel, the neuropsychiatrist Stanley Cobb and the Turkish psychiatrist

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163 Angel, Brenda Milner and Memory.  
164 Angel.  
165 Angel.  
166 Angel.  
167 Milner, “Brenda Milner”; Angel, Brenda Milner and Memory.  
Shafica Karagulla. After Penfield detailed the typical surgical approach to temporal lobe surgery and common symptoms, Jasper was immediately able to relate those symptoms to ones observed in recent animal research. Most importantly, Milner could draw a clue about the nature of the memory impairments from her collaboration with Jasper; the most profound memory impairments seemed to occur with patients who had EEG abnormalities on both sides of the hippocampus, and that this finding could be verified against a control patient who had temporal resection, but no EEG abnormality on the opposite side. This was the most important clue as to the nature of the memory dysfunction; if the patient had an undiagnosed abnormality on the remaining horn of the hippocampus, the surgical procedure would have been effectively bilateral, leaving the patient with no compensating structure. Such a conclusion was only possible because of the coordination of surgical, psychological and electrophysiological data.

Despite the emerging teamwork, disagreements amongst the group abounded. In particular, the contributions of the group’s psychiatrists were voluminous, but unhelpful. Karagulla was unable to make any comments on the hallucinations experienced by the temporal lobes patients, and when pointedly asked by Hebb, revealed that she had no control subjects for any of her reports. Hebb himself was able to comment more productively on the nature of the hallucinations; he had recently completed a series of sensory deprivation experiments that produced reliable hallucinatory experiences that seemed similar to those experienced by temporal lobe patients (these sensory deprivation experiments will return in Chapter 4). Indeed, Hebb commented on the exciting scientific interchange that was made possible the by temporal lobe project, despite the theoretical differences of the participants:

> the conferences were very valuable from my own point of view. This is most exciting work, clearly breaking new ground. As you know, I’m not much of a localization-of-function man, so I hope that Herb Jasper’s hypothesis of amygdaloid-area function can be kept in mind as well as your funneling-through hypothesis, wherever the two ideas might have different

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implications. But the main thing is, how exciting the whole work is: more power to your elbow!\footnote{Hebb to Penfield, 5 February 1953, File A/N 16-1, Box 14, WP Fonds.}

Hebb’s comment, that he was skeptical of strict localization, but that the work itself remained valuable, is illustrative of the working ethos of the MNI more generally. While theoretical orientations might vary, the different participants were able to forge a working relationship by correlating data and observations emerging from the operating theater. Correlation of findings across disciplines, even more than the strict surgical procedures involved, was the essence of the ‘Montreal method.’ Much like the trading zones of twentieth century physics identified by Peter Galison, the Temporal Lobe Research Project had evolved into just such a trading zone – one where different scientific workers could coordinate their activity, while still disagreeing on issues of theory and overall meaning.

The productive working relationship established during the Temporal Lobe Research Project continued, despite considerable disagreement between Penfield and Milner over broader findings. Penfield felt that the discovery of these temporal lobe deficits confirmed that memory was a localizable function (like speech or sensation), and that memory was a constant recording of the stream of consciousness, accessed as necessary by the central directing mechanisms of the higher brainstem. This, of course, was in line with Penfield’s surgical perspective, and Sherringtonian emphasis on reflex and integrative action.\footnote{Brenda Milner, “Memory Mechanisms,” Canadian Medical Association Journal 116, no. 12 (1977): 1374–76; Winter, Memory: Fragments of a Modern History, 75–102.} Milner was more cautious. She was steeped in the theories of Hebb and Bartlett, which emphasized both a loosened form of localization, and argued that psychological functions that appeared to be singular and discrete (like intelligence or memory), might in fact be separable and dependent on different brain structures. In writing up the cases of P.B. and F.C., Milner and Penfield negotiated these disagreements in exacting detail. For instance, Penfield argued that removals from the hippocampal zone indicated that memory and recall were
localizable to these regions; Milner objected that ‘recall’ ought not be included, because “so much can be recalled by our patients from the past, despite…some retrograde amnesia.” These subtle distinctions, which survived into the published work, were evidence not only that psychology had become an established part of the MNI community once again, but that the Montreal method was more than an operative procedure; it was a formula for combining disciplines and knowledge.

Ultimately, Penfield and Milner solved the riddle of P.B.’s memory loss. Following his death in 1966, a post-mortem examination revealed that their initial hypothesis had been correct; P.B. had suffered from an unusually atrophied right hippocampus. Milner developed a better pre-operative test (the amobarbital memory test) that ensured that the temporal lobe operations could continue, and that the experience of the 1952 operations was not likely to be repeated. However, Milner’s investigation of P.B. and F.C. would have one more outcome that would establish Milner’s reputation as on equal footing with her Montreal mentors.

The Sleeping Beauty of the Brain

173 This interchange is documented in the manuscript drafts of Penfield and Milner’s paper, latter published as Wilder Penfield and Brenda Milner, “Memory Deficit Produced by Bilateral Lesions in the Hippocampal Zone,” A.M.A. Archives of Neurology and Psychiatry 79, no. 5 (1958): 475. The drafts of the manuscript can be found in W/P 266, Box 144, WP Fonds.

174 Milner’s work to develop a predicative test of memory localization, the amobarbital memory test, is worthy of a brief digression, as it demonstrates not only the international character of the MNI, but the degree to which Milner’s contributions had become vital for the community. While Milner, Penfield and Jasper had realized that the problem of P.B. and F.C. stemmed from an atrophied hippocampus, they lacked a predictable test that could help them screen out such patients. At the same time, it was necessary to determine which hemisphere was dominant for speech in any given patient, so that the patient was not rendered aphasic by the operation. A Japanese neurosurgical fellow, Juhn Wada, suggested at one of the weekly fellows meetings that he had a test that could determine which hemisphere was dominant for speech. By injecting sodium amobarbital into either the left or right carotid artery, the patient would be rendered temporarily aphasic if the contrary brain hemisphere was dominant for speech, an effect that would wear off in a few minutes. Milner adapted this test for memory, using her experiences with the American patient H.M., to develop a memory test that could be given in three minutes in order to determine whether there was existing damage to the hippocampus. It was largely the invention of amobarbital memory testing by Milner that allowed temporal lobe surgeries to continue at the MNI. Brenda Milner, “Amobarbital Memory Testing: Some Personal Reflections,” Brain and Cognition 33, no. 1 (1997): 14–17.
The story of how Brenda Milner came to make a pivotal discovery with the single most studied human of the twentieth century, the amnesic patient Henry Molaison (H.M.) is, to some extent, well known. Briefly, in the mid-1950s, Milner became aware of a surgical patient who had had a radical, bilateral removal of his entire hippocampus by the American neurosurgeon William Scoville, and this patient appeared to have suffered total anterograde amnesia as a result. Milner used the case of H.M. not only to show that the hippocampus was vital for memory formation, but that other forms of memory (such as the learning of physical skills) was unaffected and therefore dependent on different neural systems, an epoch-making discovery which cemented her reputation as perhaps the world’s premier neuropsychologist.

The story of H.M. has recently been described in a best-selling expose, and the discoveries of Milner and her student Susanne Corkin have appeared in countless psychology, neuroscience and neurology textbooks. The preceding chapter has given that story greater historical context, by showing how the interdisciplinary environment of the MNI, and the earlier contributions of Hebb and Harrower, laid the groundwork - intellectual, institutional, and biographical - for Milner’s later work.

Milner and Penfield reported their initial findings with P.B. and F.C. at the 1955 meeting of the American Neurological Association in Chicago, and were contacted by the neurosurgeon William Beecher Scoville of Hartford, Connecticut. Scoville had become a neurosurgeon of some importance in the United States, known for being as innovative as he was aggressive in his treatments. Scoville had been in periodic contact with Penfield over the years, including a lengthy phone call in 1952 on the issue of temporal lobe surgeries. Scoville had conducted a vast number of psychosurgery operations at the Institute for Living (a psychiatric asylum in Hartford), delving

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deeper and deeper into the subcortical brain structures of back-ward schizophrenics. “His psychiatric results were meager,” Penfield recorded of the phone call. He went on to note that, “[Scoville’s] own philosophical conclusion from all of this work [was] that there are no specific cortical effects; the effect on psychoses of cortical removal is quantitative. He feels that the essential process in psychosis is going on in a subcortical area.” Nevertheless, Penfield did record that Scoville operated on two patients who were both schizophrenic and epileptic: “He did bilateral temporals [temporal lobectomy] on them and both were much improved [in their epilepsy]…Their psychological state was not apparently improved.”

It was this improvement of epilepsy following bilateral temporal lobectomy that led Scoville to recommend the procedure for a young man, Henry Molaison, whose seizures were no longer being controlled even by maximum doses of anti-convulsant drugs. Molaison, who had suffered from seizures since a childhood bicycle accident, was 27-years old when Scoville operated on him, removing both horns of his hippocampus using a surgical procedure of his own design; holes were trephined in the patient’s facial bones, the frontal lobes were lifted up with a spatula-like device, and then the appropriate structures were sucked out with a small vacuum pump. Following the procedure, Molaison experienced precisely the same memory deficits that Milner and Penfield had observed in P.B. and F.C., and it was this similarity that led Scoville to contact Penfield again in 1955 following Milner’s presentation at the Chicago ANA meeting. Would Milner like to come to Hartford and observe this patient, now christened H.M.?

Milner began examining H.M. and other patients of Scoville’s in April of 1955. While the schizophrenic patients were difficult to test and gave inconsistent data, H.M., who suffered from no prior psychiatric difficulties, was remarkable. His capacity for sustained attention, much like P.B., was unaltered, and his IQ had gone from a pre-operative 104 to a post-operative 117. Yet H.M.

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177 Telephone conversation with Dr. William B. Scoville, Hartford, Conn., regarding his experience with temporal removals,” 4 November 1952. File A/N 16-1, Box 14, WP Fonds.
could remember no new information for more than a minute or two. One exception, however, was intriguing. Asked to remember the number 584, Milner discovered that H.M. did in fact retain the number for nearly 15 minutes “by continuous rehearsal, combining and recombining the digits according to an elaborate mnemonic scheme.” Yet when distracted even briefly, the numbers vanished, along with Milner’s name, the reason for her visit, and any other developments since his operation. H.M., it appeared, really had lost the ability to translate his immediate experience into any kind of long-term memory, a mental operation that appeared to be decisively dependent on the hippocampal structure.

Milner, however, was not satisfied. Her training with Hebb and Bartlett suggested that, despite H.M.’s apparent anterograde amnesia, more careful testing might reveal intact memory operations of a different kind—a far cry from Penfield’s own strict localization of memory in the temporal lobes. Before returning to Connecticut for a follow-up visit, she “picked up two different learning tasks from the McGill experimental psychology laboratory,” where Hebb had continued to tinker with different psychometric tests since his return in 1947. “One of these tasks, a 28 choice-point stylus maze, proved to be impossibly difficult for H.M. to learn, since by the time he reached the end of the maze he had completely forgotten the beginning.”

By contrast, a simple motor test yielded a pivotal insight. The test involved tracing a star diagram with a pencil, being careful not to stray outside of the lines. The catch was that the subject

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179 Milner, 287.
180 For the purposes of this chapter, the continued story of H.M. has been streamlined somewhat. While the findings of Milner, that the hippocampus is vital for long-term memory formation, remain accepted, subsequent work by Milner’s graduate student, Suzanne Corkin, suggested that, while H.M.’s autobiographical memory remained disrupted, it did seem to develop certain kinds of residual feelings and judgments about persons, places and things that he encountered frequently. That being said, the long-term value, and ethical acceptability, of Corkin’s later work with H.M. remains contentious. For more on this issue, see Milner, 287–88; Suzanne Corkin, Permanent Present Tense: The Unforgettable Life of the Amnesic Patient, (New York: Basic Books, 2013), 151–200; Dittrich, Patient H.M.: A Story of Memory, Madness, and Family Secrets.
182 Milner, 288.
could only see his hand in a mirror, producing all sorts of errors that could be slowly corrected over repeated trials, leading to a smooth learning curve. Despite his anterograde amnesia, H.M. produced a completely normal learning curve for the star diagram test over three days of trials (10 per day), despite having no memory of having learned the skill. “I thought that was going to be difficult,” H.M. reportedly said to Milner after completing the final trial perfectly, “but it looks as though I’ve done it rather well.”

Milner’s discovery of independent autobiographical and motor memory systems in the human brain (and that autobiographical memory depended crucially on the functions of the hippocampus) was, for all its later romantic retelling, an event of true importance in the history of modern neuroscience, and one that proved the value of the interdisciplinary community that had developed in Montreal. Neither wholly a product of Hebb’s connectionist theories, nor Penfield’s surgical localization, Milner’s neuropsychology had blossomed into a fully autonomous scientific subfield that amalgamated the two perspectives using the material culture of laboratory tests and the clinical data of individual patients. Moreover, the reliability of Milner’s testing work, combined with her growing reputation, cemented the presence of neuropsychology at the MNI. In the coming years, Milner’s advice was sought more and more on clinical cases, particularly as her growing battery of tests proved useful in making diagnoses. The legitimacy of the presence of psychologists, established by Hebb and Harrower over a decade earlier, had now come to full fruition. Milner’s position became a permanent one at the MNI; her name graced the institute’s letterhead, listed as the head of neuropsychology - a hybrid field that she helped to create. Milner relinquished this title in 1994 to a student, but at the time of writing, still conducts research at the age of 99, an ironic coda to Hebb’s warning in 1953 that “a psychologist can’t survive long at the MNI.”

Milner’s hybrid approach to studying the mind, brain, and behavior - one that combined insights from different national traditions and disciplines - sits uneasily within the history of the

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183 Angel, Brenda Milner and Memory.
mind and brain sciences more generally. Milner is often referred to in textbooks and popular accounts as the ‘mother’ of cognitive neuroscience, a title she resists. “I don’t think of myself as a cognitive neuroscientist - I think of myself as a behavioral neuroscientist. I think cognition is too limiting.” This comment reflected not only her unease with certain trends in ‘cognitive science,’ but also the importance of the tools of behaviorism in her experimental agenda. Indeed, her approach remained aloof from the growing dominance of cognitive psychology developing in the United States in the 1950s around institutions like MIT. Milner’s use of simple behavior-based tests was a far cry from the computational and cybernetic theories of American researchers such as George Miller or Herbert Simon. Yet her hybrid approach and careful observation of patients was, in many ways, a prerequisite for the cognitive reaction to behaviorism in the 1960s. Much in the same way that her advisor Hebb had splintered the notion of intelligence into different components that could be disassociated and recombined, Milner’s work fractured memory into component parts that provided grist for the cognitive scientist’s mill. More than that, it reinvigorated the study of memory more generally, rescuing it from the dead end of Lashley’s equipotential engrams. Following the publication of her paper on H.M. with Scoville in 1957, Milner received a congratulatory letter from the Russian neuropsychologist Alexander Luria. Luria’s own study of the patient S (a man with apparently unlimited memory and recall), became an inspirational example of romantic science, much in the same way that Milner’s work had become one of the most romantic episodes in the history of the modern neurosciences. “Memory,” Luria wrote to Milner, “was the sleeping beauty of the brain - and now she is awake.”

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184 Angel.
186 Angel, *Brenda Milner and Memory.*
The birth of the Montreal method and the work of Brenda Milner and her predecessors is one of the most enduring legacies of the Montreal Neurological Institute for modern neuroscience. By the 1950s, a fully-formed interdisciplinary neuroscience community had grown up in Montreal, one that served as a contact point for different national scientific traditions that could amalgamate, and then diffuse back out to other parts of the world. In assembly of disparate actors – Penfield, Hebb, Harrower, Milner, Jasper and others – had become effectively wired together, much like the neurons of Hebb’s cell assemblies, through repeated, synchronized interaction. At the same time that these assemblies became more strongly connected, they also remained invigorated by their loose ties to other areas and national scientific styles, including American behaviorism, Gestalt perceptual psychology, British neurology, and American surgery.

In Chapter 3, we will examine the life of one of the MNI’s most important contributors, who has appeared briefly in the preceding story – Herbert Jasper. In so doing, we will see how Jasper’s mastery of a new technology, the EEG, spread the MNI’s interdisciplinary perspective to the rest of the emerging global neuroscience community. We will also see how much the Montreal community differed from that which was developing around F.O. Schmitt and the Massachusetts Institute of Technology.
Reflecting on the life and career of his closest collaborator, Herbert Jasper (1906-1999) wrote of Wilder Penfield that:

It was Penfield’s dream to create a multidisciplinary neuroscience institute in which the basic sciences worked closely with the clinicians and the laboratories of radiology, neuropathology, neurochemistry, neuroanatomy, neuropsychology, and...electroencephalography and neurophysiology, in a fusion of clinical and basic research. This was a forerunner of what soon became what we now know as neuroscience. I was delighted to take part in the realization of Penfield’s dream, which soon became my own as well; it became for me an international as well as an interdisciplinary dream. ¹

Indeed, in the life of Herbert Jasper we see not only the continuation of Penfield’s vision for an interdisciplinary neuroscience, but also a contributor to that vision who did more to extend it beyond the city of Montreal than almost anyone. Jasper’s pioneering application of the novel technology of electroencephalography to the surgical treatment of epilepsy is well-known within the history of neurosurgery. Indeed, the paired names of Penfield and Jasper are almost synonymous with the resurgence of the localization-of-function project in post-war neurology; the brain maps produced by Penfield and Jasper, including their ubiquitous sensory-motor ‘homunculus’, became emblematic of a new conception of brain function as one of interacting parts that could be localized to discreet areas in the brain (Figure 3.1).²

However, the depiction of Jasper as a secondary player in the Penfield story obscures both his own scientific trajectory, and his pivotal role as a leading scientific organizer. It was largely through Jasper's own scientific sophistication, and his weak ties to other assemblies of scientific actors, that turned Penfield's local operation into an international project. Jasper's contribution to developing the notion of an interdisciplinary 'neuroscience' spans much of the twentieth century, and brings us almost up to the present day. Moreover, Jasper's career brings us into our closest contact with the traditional origin story for neuroscience, centered around F.O. Schmitt and MIT, but also significantly reframes that history. An examination of Jasper's scientific biography decisively shatters the historical myth that modern 'neuroscience' was born at MIT.
This chapter will use Jasper's biography to examine this alternative vision of neuroscience, how it grew in a different technical, philosophical, and practical context than that of Schmitt at MIT, and how it became crucial for organizing neuroscience world-wide. We will see how the novel technology of the EEG provided Jasper with a technical doorway into questions of physiology and psychology that had intrigued him for much of his life. The EEG allowed Jasper not only to make constructive contributions to Penfield's surgical program for epilepsy, but also to make common cause with scientists in other areas of research, such as chemistry and psychology. Moreover, Jasper's role as a leader in the emerging community of electroencephalographers after World War II allowed him to spread his program for an interdisciplinary neuroscience to other parts of the world, ultimately culminating in the founding of the International Brain Research Organization in 1960. Finally, we will see how Jasper's more holistic, clinical form of neuroscience contrasted with the reductionistic, molecular neuroscience that developed in parallel at MIT, and how these differences led to profound contrasts of research focus, especially in the area of memory research.

Restless Genes

Herbert Jasper was born in La Grande, Oregon in 1906, seven years after Wilder Penfield's family left the Pacific Northwest and returned to Wisconsin following the breakdown of his parents' marriage. The two men, who would eventually share a fascination with what epilepsy could teach about the functions of the human brain, also shared similar upbringings (along with Jasper's later foil, F.O. Schmitt). Both men were raised by a religious parent whose influence shaped their choice of vocations, and both came to embrace laboratory research as a window into the human mind. However, whereas Penfield chose medical research as the most obvious route into the mysteries of
the mind, Jasper came to medicine only after a lengthy personal journey through theology and philosophy, and ultimately to experimental psychology and the electroencephalograph.3

Jasper’s birth in Oregon was the product of an unlikely pairing of settlers: his mother was the descendant of French Huguenots who escaped persecution in France and fled first to Switzerland and later the United States, while his father’s family originally settled in Oregon in 1882 following a covered wagon journey from Missouri. Nearly ninety years after the fact, Jasper would attribute to this particular combination of ‘restless genes’ a wanderlust that he felt was directly responsible for his entering the world of brain research. While growing up in Oregon, his mother instructed him in French from an early age, which eased his later studies and work in both Europe and Quebec.4

Jasper’s philosophical and psychological interests were cultivated at a young age by his father, a Presbyterian minister who had graduated from Willamette University in Salem, Oregon with a degree in theology and social studies. A “brilliant religious scholar and minister,” Jasper’s father was also an accomplished mathematician, an admirer of Ghandi, and a conscientious objector during the First World War, which led him to become a captain in the Salvation Army. Jasper spent World War I as a messenger boy in the YMCA unit of an army camp where his father provided social services, and it was “through his inspiration [that] I went into philosophy and comparative religions and hoped through that to get an understanding of the mechanisms of the mind.”5

Jasper’s early college years were spent at Willamette University, immersed in religious studies and philosophy. After having spent his first several years ‘saturated’ in the works of the Descartes,

4 Herbert Jasper Fonds, Box 2, 4.
Bacon, Kant, Hegel, Spinoza, Berkley, Locke, Spencer, Hume and Bergson, Jasper largely abandoned philosophy, although he retained a certain enthusiasm for both Kant and Bergson. Beyond his immersion in classic works of European philosophy, Jasper was also given heavy exposure to American pragmatism by his professor Charles L. Sherman. His first introduction to experimental psychology came in the form of William James’ classic *Principles of Psychology* (1890), and Jasper spoke positively of James concept of the ‘stream of consciousness’ for the rest of his career - a concept that later found resonance in Penfield’s notion of the ‘permanent record of experience’ and the ‘encephalocentrencephalic integrating system.’ More directly, Sherman spent considerable time discussing what was then known about the functional anatomy of the brain (information almost certainly pulled from James’ discussion of the same topic in *Principles*), and Jasper would later note that “it seemed that many of the philosophical problems that had been bothering me...might well be understood if we knew more about the brain mechanisms involved.”

It would be tempting to read Jasper’s conversion to experimental psychology as a logical progression of his philosophical studies, following from the more abstract epistemology of Hume and Kant to the physiological psychology and pragmatism of James. Yet practical experience of another kind was even more important in informing Jasper’s trajectory. An internship in at the Oregon State Mental Hospital led Jasper to think seriously about psychiatry and mental health, for which he retained a passion for much of his life (and which will be discussed more in Chapter 4). More directly, two personal experiences colored Jasper’s own move into experimental psychology;

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6 Jasper later said of Kant that he was “very interested in Kant’s *Critique of Reason* for it seemed that his view of metaphysics and epistemology and his psychological analyses of human behavior and mental processes in relation to brain activity was quite reasonable.” After taking a recently-introduced course on Einstein’s theory of relativity, Jasper felt compelled to note that he saw a special affinity between Bergson’s emphasis on the importance of time in developing new evolutionary forms, and Einstein’s “treatment of time as a fourth dimension.” Jasper, 320–21.

7 Jasper, 321.

8 The internship was secured for him by the father of a young lady with whom Jasper had become enamoured. Her father was the superintendent of the Oregon State Mental Hospital. Jasper, 321.
the first was an early dalliance with illicit drugs. A lecture by the psychology professor William ‘Monty’ Griffith on the known effects of psychotropic drugs led Jasper to experiment with a number of his roommates on the effects of injected mescaline. According to Jasper:

I tried the drug and was astounded by the profound effects of a few drops of injected mescaline. The whole world changed. I was disoriented completely, had hallucinations and delusions, and sensations of floating in air. It was a most disturbing and frightening experience, with some rather pleasant and exhilarating feelings as well...I have never forgotten the dramatic effect of such a small amount of a chemical substance upon the mind. I was determined to include brain chemistry in my future program of brain research.9

While it is questionable whether this episode really led Jasper to his later collaboration with the South-African neurochemist KAC Elliot (discussed below), it is certainly true that his experiences with mescaline reinforced an abiding materialism that served to differentiate him from the later dualistic neurology of Penfield.10

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9 Jasper, 323.
10 The issue of Penfield’s dualism is a complex one, given his regular disagreements with other members of what Delia Gavrus has described as a generation of (mainly British) dualistic neurologists, notably F.M.R. Walshe. Penfield was clearly a devotee of the Sherringtonian project of understanding the mind in physical and physiological terms, but as Gavrus points out, Sherrington himself, especially later in life, made numerous pronouncements that could be read as implying dualism, or at least not advocating a stringent materialism or monism. Given Penfield’s strong connections with Sherrington, it’s unsurprising that he espoused similar comments, often with an even more religious convictions that often sounded outright dualistic and mystical. Penfield laid out much of his own thinking on this issue late in his life in a book entitled The Mystery of the Mind (1976), which he completed shortly before his death. However, private comments from Jasper suggest that the dualistic position taken in that book was a more recent evolution of his views. According to Jasper, “Many of us regretted that Penfield published this book which he did during his later years when his mind was not as clear as it was earlier….With regards to his having changed his point of view in his later years, I can assure you that it was the emphasis that changed ….Certainly, he was always a deeply religious man and in a rather mystical manner. He did have faith though in neuroscientific research on the brain and he believed it was leading us to a better understanding of brain mechanisms underlying conscious mental life. Throughout my quarter of a century of work with him, he did not allow his deep-seated religious beliefs to temper his enthusiasm for brain research into mechanisms of the mind, and although he may have had his reservations in private, he did not express them and kept looking ahead as though he believe that understanding of the mechanisms of the brain would lead us closer to the mechanisms of the mind. It was at the end of his life when he looked back and found…what he thought how little progress he and others had made in a real overall understanding that he became….Certainly, this was a recently developed attitude, so that one cannot say that he held this view a very long time, or he would not have joined in our work over the years so enthusiastically.” Herbert Jasper to Herbert C. Lansdell,
The second experience that moved Jasper in the direction of psychological research was considerably more tragic, but also more revealing. The suicide of a close friend during his years at Willamette College, combined with his exposure to the custodial psychiatry of the Oregon State Mental Hospital, gave Jasper an abiding interest in the troubled mind. According to Jasper:

I had a very bright and close friend who was a classmate in my philosophical and psychological studies who was also brought up in a very religious home. He was unable to adjust to the change in his religious views and ended his life by committing suicide. This was a terrible shock which made me think that our researches should also involve an attempt to understand a student’s reaction to our formal teaching as well as a scientific understanding of how the brain works.¹¹

Jasper followed through on his commitment to study the causes of student suicide, and his resulting thesis, “The Objective Determination of Intellectual and Non-Intellectual Factors Influencing Student Success in College,” is a revealing document when considered in its historical context. Completed in 1927, Jasper’s thesis came at the height of a rash of student suicides across American college campuses that triggered a moral panic among the nation’s religious leaders and educators. As Christopher Loss has noted, while some religious leaders blamed the suicide epidemic on the spread of scientific materialism, students themselves often embraced scientific psychology as a means to encourage greater student success and mental wellbeing. Jasper’s early study is certainly an example of the latter trend. Simultaneously, Jasper was critical of the then-prevalent use of IQ tests to determine student fitness, noting that the IQ tests were often poor predictors of student suicide, and that ‘non-intellectual’ factors were more important in determining student adjustment.¹² For our


¹¹ Jasper Fonds, Box 2, Folder 121, 5

¹² As Jasper noted in his thesis, “The increasing use of intelligence tests in schools and colleges has contributed greatly to the solution of problems of student adjustment caused by intellectual handicaps. It has also raised nearly as many problems as it has solved because of the inadequacy of
purposes, the most salient fact about Jasper's early psychological studies, which produced a pair of publications, was its demonstration of his early commitment to the importance of psychiatric and psychological investigation.\textsuperscript{13} This trend continued during his master's degree at the University of Oregon in abnormal and experimental psychology, where Jasper conducted studies under the professor of abnormal psychology Edmund S. Conklin on the phenomenon of perseveration, or the repetition of words and phrases following a brain injury.\textsuperscript{14}

While Jasper continued his psychological investigation of speech problems during his PhD training in psychology at the University of Iowa, this period also constituted an important break in his career. Studying under Lee Edward Travis, the American founder of speech pathology, Jasper undertook an examination of the relationship between brain hemisphere dominance and severe stuttering, reasoning that incomplete coordination between the two hemispheres might account for intelligence tests in diagnosing student difficulties. The evidence from such cases as the Leopold and Loeb case in Chicago and the recent attention being drawn to student suicides...is impressing educators of today with the fact, which their own experience has validated, that their problem is not solved when they have classified their students on an intellectual basis. There are evidently non-intellectual factors which are at least as important as intellectual factors in the determination of student success.” Jasper Fonds, Box 6, Folder 320, 1. On student suicides and responses, see C. P. Loss, \textit{Between Citizens and the State: The Politics of American Higher Education in the 20th Century} (Princeton University Press, 2012), 49–50.

\textsuperscript{13} Jasper’s later colleagues had some difficulty believing that the great electroencephalographer and neurophysiologist got his start in psychological research. The two publications generated by Jasper’s thesis, “Optimism and Pessimism in College Environments” and “The Measurement of Depression-Elation and Its Relation to a Measure of Extraversion-Introversion” in fact represented the first attempt to construct a rating scale for depression in the United States. Commenting on this fact, the psychiatrist George B Murray wrote to Jasper in 1994, noting that “I have had some fun around here talking about Herbert H. Jasper, one of the original investigators in depression. They all know you as a learned electroencephalographer, and therefore they try to correct me and say that I have it mixed up, I must be thinking of Karl Jaspers, the German psychiatrist. So I whip out this paper and show ‘em that your early training was in psychology.” George B. Murray to Jasper, 25 February 1994, Jasper Fonds, Box 3, Folder 167. Herbert H. Jasper, “Optimism and Pessimism in College Environments,” \textit{American Journal of Sociology}, 1929, 856–873; Herbert H. Jasper, “The Measurement of Depression-Elation and Its Relation to a Measure of Extraversion-Introversion,” \textit{The Journal of Abnormal and Social Psychology} 25, no. 3 (1930): 307.

such cases. It was through this study that Jasper was introduced to a series of technical innovations that would considerably alter his career course; Travis and his engineer had developed a device for the electrical measurement of the nerve action potential, the electrical signal that passed along and across nerve fibers.  

The Electric Brain

It is difficult for a modern reader, steeped in notions of the ‘chemical’ brain that have grown out of the discovery of psychopharmaceuticals and neurotransmitters, to appreciate the importance that electricity and electrical investigation had for brain researchers in the first half of the twentieth century. Beginning with the discovery of the electrical excitability of the cortex by Gustav Fritsch and Eduard Hitzig in 1870, the ‘electric brain’ became the dominant metaphor for understanding and investigating brain function. Jasper’s first encounter with electrical techniques for investigating the brain and nervous system came at the tail end of this paradigm; already in Europe Otto Loewi and Henry Dale were beginning the investigations that would launch the so-called ‘war of the soups and the sparks,’ the name given to the conflict over the discovery of neurotransmitters and the debate over synaptic transmission. However, in 1929, when Jasper first began his electrical investigations of stuttering, the issue was far from settled, and for a time he became fascinated by the work of Louis Lapicque, the French neurophysiologist who had proposed a mathematical model of electrical transmission across the synapse known as ‘chronaxie.’ According to Jasper, Lapicque thought that ‘chronaxie,’ a mathematical model of the time constants of nerve excitability “might

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determine the selective transmission of nerve impulses across synapses and neuromuscular junctions by a sort of tuning as in radio waves.”

However, while the electrical metaphor for understanding the brain was still the dominant one, the value of different forms of electrical investigation remained up for debate. It was during this period that Jasper became acquainted with Herbert Gasser who had, along with Joseph Erlanger, used the cathode ray oscilloscope to visualize the nerve action potential and study the differences created by variations in nerve diameter and myelination. At the same time, Jasper also began to hear about an obscure German psychiatrist, Hans Berger, and his new device known as the *elektrenkephalogram*, which measured electrical activity of the brain by means of electrodes attached to the surface of the skull. Although later spoken of in the same breath, as part of the exciting developments in the brain sciences in the 1920s, those working on nerve transmission viewed the EEG with suspicion. According to Jasper, Gasser repeatedly told him that, “it would be a waste of time to try to record electrical activity from the brain since it would be a composite of the action potentials from millions of simultaneously active nerve cells, impossible to interpret.”

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17 Jasper Fonds, Box 2, Folder 121, 8.
19 Jasper, for instance, lists the EEG along with a number of other discoveries in the 1920s and 30s as crucial to the development of neuroscience. Notably, Louise Marshall lists the EEG along with other instruments around which American neurophysiology focused, but argues that it was inconsequential to that development. This chapter will demonstrate that Marshall’s dismissal is ill-considered, and reflects an excessive focus on the American story. Jasper, “Herbert H. Jasper”; Louise H. Marshall, “Instruments, Techniques, and Social Units in American Neurophysiology, 1870–1950,” in *Physiology in the American Context 1850–1940* (Springer, 1987), 359–65.
20 Jasper Fonds, Box 2, Folder 121, 7–8. Jasper referenced this story elsewhere, adding that “this was proved to be quite a wrong conclusion. There was an awful lot in the brain that was not action potentials and this was shown first by showing graded potentials at the synapse. These were called synaptic potentials and dendritic potentials, which were not action potentials, but were the basis for the integrative activity of the synapse. We were recording these, rather than action potentials....” Jasper Fonds, Box 4, Folder 202, 3.
Jasper first met Gasser and Erlanger during the winter of 1929/1930, when he attended the American Physiological Society meeting in Chicago. Gasser was part of a growing cadre of physiologists working out of Washington University in St. Louis Missouri that also included George Bishop, Howard Bartley, Raphael Lorente de Nó and significantly, Francis O. Schmitt, who had just begun his studies on the molecular structure of nerve membranes with his brother Otto. While Jasper expressed interest in the work of the St. Louis group, it was at this conference that he first encountered the French neurophysiologist Ali Monnier, who would have a special impact on his career. Monnier, had come to the United States to study with Gasser, and was engaged in building a set of cathode ray oscilloscopes to take back to France in order to further Lapicque’s study of chronaxie. Monnier invited Jasper to accompany him. With money from the Rockefeller Foundation (specifically supplied by Allan Gregg, who just one year later would provide Penfield with the scientific funding for the MNI), Jasper set sail for Paris in the summer of 1931.21

It would be fair to say that by the time Jasper was twenty-five he sat at the crossroads of a number of scientific cultures and national traditions that were of particular importance in the development of neurophysiology generally. As Louise Marshall has noted, neurophysiology developed in the United States around a relatively small number of key research schools, each devoted to a particular instrument and mode of experimentation; for Gasser and Erlanger in St. Louis, the cathode ray oscilloscope opened up research into the nerve action potential, whereas for Ralph Gerard in Chicago, the microelectrode served to initiate investigation of individual neurons.22 Simultaneously, Cornelius Brock has noted that the adoption of the electroencephalograph, first

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21 It is noteworthy that Jasper later described Monnier, who had begun his career in physics before moving into physiology, as following “a course opposite to my own.” Jasper Fonds, Box 2, Folder 121, 7-8; Jasper, “Herbert H. Jasper,” 326.
introduced to relatively little fanfare in Germany by Hans Berger, was taken up differently in
different national and local contexts. As Brock puts it:

The initially somewhat obscure German observation [by Berger of the EEG] was rescued by
British pragmatism but was converted into a clinical diagnostic routine only in consequence
of the typical American reliance on technology and because of the advanced American
standard of industrial production in electronics.\(^{23}\)

The development of EEG and neurophysiology, then, were dependent on a number of local and
national scientific styles and cultures.\(^{24}\) Yet, by the time he arrived in Paris to study with Monnier,
Jasper was wedded to no specific research culture or tradition; his interests ranged from the
transmission of individual nerve impulses to the psychological states of the mentally ill. Jasper was
greeted in Paris by Lapicque and his wife, and began work in his laboratory with Monnier, in which
“everyone seemed to be working on chronaxie in one way or another.”\(^{25}\) This research would later
form the basis of the first half of his second doctoral dissertation, which contained two parts:

“Research On The Excitability And The Characteristics Of The Response In The Muscular System
of Crustaceans. Influences of Ganglionic Centers,” and “Preliminary Studies on
Electroencephalography in Humans. Responses to Visual Stimuli and Hearing.”\(^{26}\)

The remainder of Jasper’s time in Europe reinforced his growing internationalism and
further exposed him to different scientific cultures. Notably, his friendship with the French
biochemist and future Nobel-prize winner Jacques Monod, whom he met while working at the

\(^{23}\) Cornelius Borck, “Between Local Cultures and National Styles: Units of Analysis in the History of

\(^{24}\) Gerald Geison makes a similar argument for the development of physiology more generally in his
examination of Michael Foster and the ‘Cambridge school’ of physiology. Geison argues that the
physiology that developed under the guidance of Foster displayed a certain English national ‘style’
because of its reliance on Darwinian evolutionary ideas, in contrast to the German schools of
physiology that developed under Herman von Helmholtz. Gerald L. Geison, *Michael Foster and the


\(^{26}\) Ibid.
Marine Biological Station in Brittany, introduced him to the “mysteries of molecular biology.”

Jasper also spent considerable time in Great Britain, becoming closely acquainted with E.D. Adrian, as well as observing numerous other European laboratories. By the time he returned from Europe, Jasper had, thanks to Rockefeller funding and his own ‘restless genes,’ been immersed in much of the neurophysiological work occurring in Europe and the United States. While the actual phenomenon of chronaxie turned out to be something of a ‘blind alley’ in terms of research findings (the issue of electrical transmission mechanisms across the synapse was later rendered moot by the discovery of neurotransmitters and chemical transmission), Jasper’s work on the fundamental mechanisms of nerve activity, and his desire to reconcile different national laboratory traditions, helps to explain his particular successes and failures when he turned to a more promising experimental line, the perplexing but promising electroencephalograph.

**Enter the EEG**

While the first half of Jasper’s doctoral dissertation investigated the operations of the nervous system at the level of cellular activity, the second half, ‘electroencephalography in humans,’ tackled the problem of the nervous system from the other direction: trying to make sense of the newly discovered phenomenon of the electroencephalogram - the very approach that Herbert Gasser had warned Jasper would be unfruitful. Indeed, the discovery of regular brainwaves, first recorded in humans by the German psychiatrist and physiologist Hans Berger in 1924, was greeted with skepticism by the physiological community when Berger published his carefully collected findings in 1929. Gasser was not alone in thinking that it was unlikely that studying the aggregate

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electrical activity of the human brain, composed of countless separate action potentials, would be pointless. As Louise Marshall has noted, the EEG attracted relatively little attention from American neurophysiologists before 1935.29

Jasper returned to the United States briefly in 1932 (while in the midst of research for his PhD in Paris) to begin setting up an electrophysiology laboratory at Brown University in Rhode Island. During this trip, Jasper was told by William Malamud, the German psychiatrist with whom he had studied at Iowa, that he should try to repeat and verify Berger’s research with the EEG. Jasper was able to do this easily, both because of his own technical expertise with other forms of electrical instrumentation, and because of the high quality of the electrical apparatus at his laboratory in the Bradley Hospital at Brown, which featured lead-lined walls to prevent distortion, and soon attracted the physicist and engineer Howard Andrews, the neurophysiologist Margaret Rheinberger (a student of John Fulton), and the psychologist Andrew Carmichael. By 1934 Jasper had assembled a fully functional and robust EEG lab, while simultaneously conducting his investigations of chronaxie for his doctoral dissertation.30

Jasper observed Adrian’s confirmation of brainwaves in 1934, and Jasper’s own confirmation of Adrian’s results (the first American publication on EEG in Science in 1935), helped to establish the validity of brainwaves for a skeptical American scientific community.31 However, while the validity of brainwaves was accepted, the value of the new technology was more nebulous. As a number of

30 Jasper Fonds, Box 2, Folder 121, 11; Box 2, Folder 109, 3; Jasper, “Herbert H. Jasper,” 328.
historians have pointed out, the immediate value of ‘brainwaves’ for either scientific or medical use was unclear.\textsuperscript{32} The alpha rhythm, the regular oscillation of the resting brain, was relatively easy for Adrian and others to confirm, and the discovery that sensory information disturbed the alpha rhythm confirmed the biological nature of sensory phenomena. Beyond that, it was not until William Lennox and Frederick and Erna Gibbs of Harvard discovered its use in diagnosing epilepsy in 1935 that the EEG began to yield significant medical benefits. Borck has noted that the success of Lennox and the Gibbses was likely the result of their being able to fuse the EEG into a pre-existing epilepsy research agenda; the Harvard group had “studied the pathophysiology of epileptic diseases along various investigative pathways for more than a decade without many conclusive results.”\textsuperscript{33} Borck goes on to argue that this insight was possible only at Harvard, where “these two fields of expertise, clinical experience plus basic research into epileptology, on the one hand, and neurophysiology of the central nervous system, on the other, coexisted.”\textsuperscript{34} Lennox and the Gibbses’ discovery led to a reconceptualization of epilepsy as a purely neurological, rather than psychiatric, condition.

By contrast, Borck notes that Jasper, in many ways the ideal candidate to make research findings with the EEG, was initially unsuccessful in achieving the high level of integration that he desired. Jasper’s longstanding goal, to use physiology to investigate the more fundamental mechanisms of human psychology, “aimed at too high a level of consistency,” and his “abstract rationalism was doomed to failure.”\textsuperscript{35} This is both true, and an oversimplification. By the time he began his experimental program with the EEG, Jasper sat at the confluence of a number of

\textsuperscript{33} Borck, “Between Local Cultures and National Styles,” 455.
\textsuperscript{34} Borck, 455.
\textsuperscript{35} Borck, 457.
instrumental and national research traditions, and was attempting to reconcile them. At the same time, the technical expertise that he gained in both the European and American settings gave him an edge in refining the technique of EEG recording. Jasper first undertook work with the EEG following his return to the United States in 1933, at which time he set up a neurophysiology laboratory at Brown University. He also spent considerable time at the Woods Hole Marine Biological laboratory, completing his research on crustacean neuromuscular systems for his second PhD, and getting to know many of the most important players on the American neurophysiological scene, including Ralph Gerard, Lorente de No, J.Z. Young, Stephen Kuffler, Harry Grundfest, and becoming reacquainted with Schmitt and his brother Otto.36

It is here worth pausing to consider the interpretation of this period in Jasper’s career by Borck. In Borck’s view, these early investigations with the EEG, prior to the discoveries of Lennox and Gibbs in 1935, amounted to little. In Borck’s words:

> in Jasper’s laboratory at the Bradley Home...the specific circumstances appeared to be perfect. The individual training was excellent, the local context particularly rich and valuable, the funding and availability of materials far above average. In short, every contributing factor seemed to have added positively to an ideal setting of support, and yet, the experimental system quickly ran into serious difficulties...[Jasper’s] openmindedness, the simple fact that he did not yet share a particular point of view from a pre-existing research program, caused a peculiar epistemic problem when he tried to mediate between the different strategies....In addition, and in contrast to Lennox or Walter, Jasper did not hit upon a spectacular new observation that would eventually push him to put his more fundamental questions aside...37

In Borck’s estimation, Jasper’s early EEG research was unsuccessful because he was not sufficiently embedded in an existing research tradition, national or otherwise. Yet this interpretation is unfair to Jasper’s initial research, and may obscure as much as it enlightens. A more plausible reading of Jasper’s early work with the EEG emerges when one considers the direction of his intellectual trajectory, and his international experience. Jasper’s psychological and psychiatric experience,

37 Borck, “Between Local Cultures and National Styles,” 456.
combined with his basic physiological work on the transmission of neural impulses, meant that he was particularly sensitive to any device or technique that might allow for the physiological investigation of psychological phenomenon. For Jasper, the EEG was just such a tool.

At the same time, Jasper’s initial experiments with the EEG that ultimately formed the basis for the second half of his doctoral dissertation read as the efforts of a scientist looking to bridge the gap between different laboratory cultures and styles of experimentation. Indeed, because of Jasper’s early training with Monnier, he was able to refine and expand the use of the EEG to investigate psychological phenomena. According to Jasper: “I had been accustomed to using cathode ray oscilloscopes in my work with Ali Monnier in Paris, and I was not about to use any of the ink writers available at the time for fear of missing the more rapid components of the EEG.” The more precise recording equipment constructed by Andrews allowed Jasper to obtain more detailed and precise EEG readings in experimental animals and humans. Because of this precision, Jasper and Carmichael were able to show that the alpha rhythm was sensitive not only to gross changes in states of consciousness (e.g. sleep or wakefulness), but also to more subtle and complex psychological changes. For instance, while Adrian had demonstrated that the alpha rhythm could be blocked by visual stimulation, Jasper and his team showed that an after-image could produce the same effect. While European labs had shown the presence of slow (or delta) waves during sleep, Jasper used recording electrodes in cats to demonstrate that a disturbance of these waves could be used to predict the awakening of an animal. Jasper used experiments on human subjects to reveal that the alpha wave pattern was sensitive to anxiety states. Chemical experimentation revealed that the EEG could be predictably disturbed by such anesthetic agents as benzedrine, as well as other metabolic activities, pH, oxygen tension, blood flow, brain lesions and of course epilepsy.  

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Borck is right to observe that Jasper's broader research agenda, to consolidate knowledge about nerve activity with experimental findings from the EEG, produced no result as spectacular as that of Lennox and Gibbs, Jasper's success in demonstrating the utility of the EEG for investigating psychological phenomena produced a number of more subtle successes that reinforced his view both in the value of physiological psychology, and in the melding of different research traditions within a single laboratory.

with their new histological science of ‘cytoarchitectonics’ (See Chapter 1). By producing maps of the different cell types in the brain, the Vogts hoped to unravel the physiology of human thought, and particularly the basis of individual mental difference. While Oscar Vogt believed that he could reconcile the new, more holistic EEG with his own work, Jasper was skeptical:

They [Toennies and Kornmuller, Vogts’ assistants] thought that the pattern of brain waves from different areas of the cerebral cortex in monkeys corresponded to the cytoarchitectonic areas that had been described by the Vogts from their anatomical studies. We had also documented differences in patterns from different sensory, motor, and association areas of the cortex, although not corresponding so precisely to the cytoarchitectonic areas that the Vogts had described.40

Elsewhere, Jasper wrote that he was “skeptical of their overrefined cytoarchitectonic subdivisions of the brain.”41

By 1935 then, Jasper had emerged from an intellectual journey that had taken him from the abstract, existential questions of theology and philosophy, through the more precise statistical and instrumental methods of experimental psychology, and finally to the more basic research of experimental neurophysiology and the clinical application of the EEG. While the EEG had failed to produce a unified theory of physiological psychology in the manner that Jasper had hoped, his exposure to different laboratories and research programs convinced him of the value of the new tool for investigating the physical basis of the mind. Indeed, while Louise Marshal has noted, in her review of the development of neurophysiology in America, that the EEG “in fact proved to be

41 This skepticism would be echoed by Penfield, despite the increasing popularity of cytoarchitectonics and Broadman maps in the more reductionistic programs of the Schmitt and the Neuroscience Research Program. In 1948 Penfield was asked by the American geneticist H.J. Muller if he would support Oscar Vogt’s candidacy for the Nobel Prize. Penfield responded that “I am afraid that I would not be willing to support Vogt for a Nobel Prize. His work and that of Brodmann and others upon the cytoarchitecture of the cortex has been much criticized recently by Lashley, Percival Bailey and others, and I am afraid that no very good case could be made out for his candidacy.” Herbert H. Jasper, “Philosophy or Physics—Mind or Molecules,” in The Neurosciences: Paths of Discovery, I, ed. Frederic G. Worden, Judith P. Swazey, and George Adelman (Boston, MA: Birkhäuser Boston, 1992), 409. Penfield quote from Penfield to H.J. Muller, 22 January 1948, C/G 48 M, Box 60, WP Fonds.
unproductive in yielding fundamental information about the activity of single nerve cells,” Jasper’s exposure to the subtler traditions of both European and American labs, combined with his appreciation for the more holistic application of the EEG as a clinical tool, convinced him of its value for psychological research. Simultaneously, Jasper’s open-mindedness and cosmopolitan exposure to different intellectual and national traditions did not preclude a growing skepticism about the direction of some neurophysiology in America. Reflecting on this period in his life, Jasper later wrote that:

My contact with such biophysicists as Ali Monnier, Frank Schmitt, and K. C. Cole had provided me with exciting prospects for the future, but it seemed that it would be a far distant future before our knowledge of the molecular properties of nerve tissue could be applied to an understanding of mind-brain relationships. My philosophical studies had raised questions in my mind regarding the reductionist molecular approach to this problem. Was this not a false path leading only to greater understanding of less and less?

For Jasper, the reductive studies of Monnier, Schmitt and other proto-biophysicists and axonologists were not valueless; indeed, their subtle instrumental techniques had done much to convince him not only of the value of the EEG, but also of the futility of studying only the action potentials of the human brain and nervous system. At the same time, Jasper’s holism never rose to the level of fervent obsession observed in European countries (particularly Germany), where inter-war hostility to mechanistic and reductionist biology took on a nearly religious zeal. Rather, Jasper emphasized that the EEG would prove most valuable for investigating an intermediate level of brain organization - something between the neuron itself, and the brain as a whole - “the microstructure

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43 Jasper, “Philosophy or Physics—Mind or Molecules,” 409.
44 As Anne Harrington has argued, holism had resonances not only with German cultural traditions extending back as far as the Reformation, but also took on a particular urgency for many German scientists during the Weimar years. Yet, as Harrington and other have noted, holism also took a number of different trajectories during the first half of the twentieth century. See Anne Harrington, Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler (Princeton, NJ: Princeton University Press, 1996); Christopher Lawrence and G. Weisz, Greater Than the Parts: Holism in Biomedicine, 1920-1950 (Oxford University Press, 1998).
and interconnections of large assemblies of neurones and their functional organization in relation to mental processes and behaviour. Moreover, his more sophisticated exploration of the EEG allowed Jasper to not only replicate the work of Gibbs and Lenox on epilepsy, but would allow him to use the EEG to localize epileptogenic lesions, an ability that would soon bring him to the attention of Wilder Penfield.

Penfield and the Montreal School

It was Jasper’s ability to localize epileptogenic foci, only one part of his broader research program, that fundamentally altered his life’s course and inaugurated his most productive working relationship. By 1937, Penfield’s ‘radical surgery’ for focal epilepsy had achieved considerable success, but Penfield himself was largely ignorant of the developing field of electrophysiology, and of the EEG in particular. For Penfield, the combination of careful pre-operative case history, neurological examination, x-ray study and pneumoencephalography, combined with his electrical stimulation of the exposed cortex, seemed to provide enough information to localize epileptogenic scar tissue. Indeed, along with good operative success, Penfield felt that he had enacted what “I had dreamed of doing when a student in Charles Sherrington’s Mammalian Laboratory of Physiology. We could question what Sherrington would have called the ‘preparation,’ and listen to his answer. What a future it seemed to open! - for anatomy and physiology and psychology.”

It was the psychological implications of Penfield’s operations that initially interested Jasper, and this is why he

45 Jasper, “Philosophy or Physics—Mind or Molecules,” 409.
invited Penfield to give a series of lectures to the psychology department at Brown University in 1937. 47

While Jasper shared Penfield’s enthusiasm for the possibility of uniting psychology and physiology, he was also more cautious about the technical abilities of his new-found apparatus. Following the discovery by Gibbs, Davis and Lennox of the abnormal EEG patterns of epileptics, Jasper followed up with his own contention that epileptic foci could be localized through the skull of the patient. In making this claim, Jasper cautioned that the EEG, while an extraordinarily sensitive device, was prone to error if not properly controlled:

Since the recent statement of Gibbs, Davis and Lennox...that the method of electroencephalography is exceedingly simple and that the technic is as free from sources of error as that of electrocardiography, I believe that it is important to point out that the method is actually exceedingly complicated and that even the most experienced electrophysiologist cannot always at present distinguish between the potentials of the brain and those of extracranial origin. 48

Jasper succeeded in controlling for the various artifacts that could be introduced into EEG records (sometimes by behaviors as trivial as the blinking of an eye), as well as demonstrating that

47 My interpretation of this period in Jasper’s career differs considerably from that of Cornelius Borck. Borck has argued that Jasper’s program of research at Browne was too ambitious, and lacked a ‘local scientific culture’ that could successfully guide research in productive directions (he contrasts this with Gibbs and Lenox’s group, who were able to slot the EEG into an already existing program of epileptology). It was, in Borck’s view, Penfield who saved the flagging Jasper by immediately recognizing the significance of the EEG for clinical work, and offering Jasper a job. As Borck puts it, “Jasper’s abstract rationalism was doomed to failure. With his scrupulous questioning of all available evidence in search of the internationally true significance of the EEG, he did not advance much further than publishing some of the best early reviews in this field. It must have come as a God sent when Penfield visited his laboratory and offered him to open a clinical EEG laboratory at the Montreal Neurological Institute.” This interpretation seems to ignore the fact that it was Jasper’s interest in Penfield’s psychological findings that initiated the contact between the two men, and the Penfield himself was initially skeptical of the value of the EEG for clinical work. It seems more likely that, rather than lacking a local scientific culture, Jasper’s international experience with different labs and different technical traditions made him the ideal candidate to refine and perfect the EEG technique, and show its value for clinical work to a skeptical Penfield. Borck, “Between Local Cultures and National Styles,” 457.

localization of the epileptic foci through the skull was possible if the conditions were properly controlled.\textsuperscript{49} One such control measure involved the use of chicken-wire to counteract electrical interference, and it was within a maze of chickenwire, in the basement of the Bradley Home hospital that Penfield first encountered Jasper. Recalling the incident decades later, Penfield could not help the comparison between his soon-to-be partner and the chicken-wire cage he occupied:

Inside the maze was a young man, moving about like a bird in an aviary. This was a rare bird, a\textit{ rara avis}, Herbert Jasper, a young man driven by one creative idea after another. He could, he said, localize the focus of an epileptic seizure by the disturbance of brain rhythms outside the skull. I doubted that but hoped it might be true.\textsuperscript{50}

Despite his skepticism, Penfield accepted Jasper’s offer to collaborate on two specific cases of epilepsy. The successful localization of the epileptic foci and subsequent operations inaugurated what, in Penfield’s words, was an almost “unthinkable commuters research project.”\textsuperscript{51} Piling his EEG equipment into the front seat of his car, Jasper would make a weekly trip from Rhode Island to Montreal for nearly a year, and with the assistance of Penfield succeeded in having his Rockefeller grant transferred. According to Penfield, “It was as though far-away Rhode Island were a suburb of Montreal.”\textsuperscript{52}

The success of the Penfield-Jasper collaboration was evident almost immediately, and the commuters project became one of permanent residence. Jasper moved to Montreal in 1938 and opened a dedicated EEG lab in the basement of the MNI in February of 1939. By incorporating the EEG into pre-operative and intra-operative procedures, Jasper not only increased the operative success of Penfield’s surgical excisions, but inaugurated something of a new era for the study of

\textsuperscript{49} Jasper and Hawke, “Electro-Encephalography: IV. Localization of Seizure Waves in Epilepsy.”
\textsuperscript{50} Penfield, “Herbert Jasper,” 10.
\textsuperscript{51} Penfield, 11.
\textsuperscript{52} The trips from Rhode Island to Montreal, a distance of over 370 miles, were likely grueling. Arriving on Monday evenings, Jasper would take up residence in the small quarters on the roof of the MNI, assist Penfield in operations on Tuesday, Wednesday and Thursday, and return to Rhode Island on Friday to carry out the remainder of his work at the Bradley home. Penfield, 11.
cortical localization. Jasper was awash in data; during his first two years in Montreal, he had the opportunity to conduct more than two thousand EEG examinations of patients, as well as participate in epilepsy operations. What began as a single chapter in Penfield and Erickson’s 1941 text *Epilepsy and Cortical Localization* expanded into a book-length collaboration, *Epilepsy and the Functional Anatomy of the Human Brain*, by 1954.53

It would be tempting, then, to read the arrival of Jasper at the MNI in strictly functionalist terms: Penfield, having realized the value of the EEG for the success of his own medical program, brought Jasper on board as a particularly talented technician who could contribute to Penfield’s overall scientific program. In this telling, Jasper might be thought of as an example of what Steven Shapin has called ‘the invisible technician,’ a scientific worker whose contributions to theory are minimized because of narrative biases that privilege theory over experimentation and instrumentation.54 Yet this functionalist interpretation won’t do. Jasper was no shrinking violet, content to act as a mere diagnostician; he brought his own research program and objectives to the MNI, along with an abiding interest in collaboration and psychological investigation. Indeed, in addition to his excitement about the possibilities of stimulating the exposed cortex of a conscious human, Jasper was also animated about the opportunity to collaborate with the emerging field of neuropsychology:

> Our work in neurophysiology and electroencephalography was greatly aided by the presence of strong departments of neuropsychology at the MNI from the beginning. In fact it was Penfield’s interest in neuropsychology, and the presence of Donald Hebb and Molly Harrower in the Institute at the beginning, that played a role in my decision to come from the excellent department of psychology at Brown in 1938.55

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55 Jasper Fonds, Box 2, Folder 107, 3-4. Jasper went on about the importance of continuing collaboration with neuropsychologists at the MNI in the form of Brenda Milner, Hebb and
Indeed, Jasper’s later collaboration with a number of McGill psychology students, as detailed below, would lead to a series of crucial research programs that became foundational for the international organization of neuroscience. In the interim, however, Jasper not only consolidated much of what was then known about the electrophysiology of epilepsy, but also began to make a number of contributions to its theoretical understanding, and to the knowledge that could be obtained from it regarding mechanisms of consciousness. Convinced that he could best contribute to the growing interdisciplinary mission of the MNI by improving his ability to interact with patients, Jasper undertook a medical degree in 1940, and married a Canadian nurse, Margaret Goldie. The second of these events prompted Penfield to write to the then dean of medicine at McGill that Jasper, “has married a charming Canadian wife, and I may say confidentially that I hope and expect that he will become a Canadian citizen.”

**WWII, Neurochemistry, and the Post-War Research Fellows**

The coming of the Second World War altered the research mission of the MNI, but had the side effect of enhancing its growing interdisciplinarity and internationality; indeed, Jasper’s own research program and collaborative ventures were fundamentally altered by it. By the end of WWII, Jasper emerged not only as a leader of research at the MNI, but as an international leader of the emerging profession of electroencephalographers.

Harrower’s successor: “Since then, Brenda Milner has developed a large and productive department of neuropsychology from which we have benefitted greatly over the years. Her work has given us a broader perspective, since we were using the electrical activity of the brain and clinical studies of seizure patterns in epileptic patients for what they could teach us about brain mechanisms involved in states of consciousness, sensory-motor function and perceptual and cognitive mental processes in man.”

56 Wilder Penfield to G. Lyman Duff, 27 February 1950, Wilder Penfield Fonds, Box 2, Folder A/M 3 2/3.
The war brought a number of changes to the MNI. Convinced of the need to aid the war effort, Penfield retooled the MNI’s research agenda for war work. Jasper’s contributions ran in two directions: first, a study of the phenomenon of pilot ‘blackout’, which included attempts to use the EEG to determine if certain pilots were vulnerable to the phenomenon.57

The second research project undertaken by Jasper and his colleagues demonstrated both the growing international dimensions of the MNI, and hinted at his future role as a coordinator of different scientific disciplines. Jasper was, at this point, no stranger to collaboration with colleagues from other countries; his primary technician, Andre Cipriani, was a gifted physics student from Trinidad who would go on to work at the Canadian Atomic Energy Commission, and the MNI had already welcomed the young pathologist Dorothy Russell from England and the future founder of neurosurgery in Poland, Jerzy Chorobski. However, the arrival of K.A.C. Elliot, a South-African chemist recruited from the Psychiatric Institute of the Pennsylvania Hospital, inaugurated an entirely new field of research at the MNI. Initially recruited to study the problem of brain edema (the swelling of the brain following injury), Elliot eventually began a collaboration with Jasper that produced the first textbook of neurochemistry; indeed, Elliot was the first person to carry the professional title of ‘neurochemist,’ which Penfield himself invented. According to Elliot, “Everything at the MNI was Neuro-something. So my labs were Neurochemistry and I was the first person anywhere to be officially called a Neurochemist.”58

Elliot’s career itself is worthy of a digression. Born in Kimberly, South Africa in 1903, Elliot’s family had lived in the imperial colony for over two centuries. A graduate of Rhodes University, Elliot’s early career as a research chemist was eclectic to say the least; he took his first job at a mine in Northern Rhodesia where he helped to develop a process for extracting zinc from mine slag, then worked for a time in a dynamite factory near Johannesburg (working from 6am until 2pm so as to be clear of potential explosions caused by regular lightning storms), and published his first research paper on the medicinal qualities of a local African plant species, all while regularly commuting over a thousand miles by motorcycle along dirt roads to visit his gravely-ill mother in Port Elizabeth. Elliot transitioned to organic and biochemistry following a degree in Cambridge, and spent a number of formative years in the laboratory of Sir Frederick Gowland Hopkins, the Nobel-prize winning discoverer of vitamins. According to Elliott, Hopkin’s lab hummed with two notable qualities - a collegial emphasis on research over status, and a palpable left-wing politics embodied in the unofficial laboratory slogan ‘Scholarships not Battleships.’ Following the rise of National Socialism, many members of Hopkins’ lab dipped into their own funds to bring Jewish chemists fleeing Germany to their lab. 59

Elliot wished to return to South Africa, but the prospects of post-doctoral fellowships and employment were poor, so he moved to America in the late 1930s, and got a job in a laboratory sponsored by the Dupont company. Elliot spent six years at the Dupont laboratory, investigating various aspects of metabolism, before transitioning to the psychiatric institute of the Pennsylvania Hospital. It was a Rockefeller grant that brought Elliott to the psychiatric institute, and while there he instigated a number of studies of the metabolic effects of metrazol, insulin and electroshock therapy, leading to an early symposium on “Biochemistry and Physiology in Relation to Mental Disease.” Simultaneously, Elliott and his new wife, the chemist Frances Howland, helped to organize

59 Elliott, “An Unorthodox Career.”
an American adjunct to the British Association of Scientific Workers, a socialist scientific union. Increasingly troubled by the rise of fascism in Europe, but now too old for active service, Elliot instead chose to spend his summers volunteering on a farm in New York state, and it was here that he received a telegram about the possibility of a job in Montreal.\footnote{Elliott, 16–17.}

Elliot initially interviewed at both the MNI and the Allan Memorial Institute (Montreal’s newly opened psychiatric institute, which will be discussed in Chapter 4), but was hired at the MNI for the war-time edema project. This edema research would eventually transform into a decade-long project that ultimately identified gamma-Aminobutyric acid (GABA) as the primary inhibitory neurotransmitter, and an examination of this collaborative project provides a prime example of the trading relationship between scientific disciplines, and Jasper’s role as a coordinator. While the initial investigation of edema had produced a number of valuable research discoveries (including the development of ‘Elliot’s solution,’ an artificial cerebrospinal fluid), it had failed to make any inroads on the issue of epilepsy, the MNI’s \textit{raison d’être}. Following WWII, however, a collaboration between Jasper and Elliot broke the stalemate. Using epileptogenic scar tissue, confirmed by Jasper’s EEG and excised from surgical patients by Penfield, Elliot initially investigated the possibility of metabolic differences with healthy tissue, and concluded that there was no obvious difference between the two. As tissue samples were passed from the operating theater to the laboratory, Jasper and Elliot began to speculate that they were on the wrong track: “We often discussed the possibility that the abnormality in epileptic tissue might be due to defective inhibition rather than to excessive excitation, the latter resulting from a release from normal inhibitory controls.”\footnote{Jasper, “The Saga of KAC Elliott and GABA,” 450.}

This speculation proved fruitful; beginning in the early 1950s Jasper and Elliott invited the Australian zoologist Ernst Florey and his wife Elizabeth to the MNI to experiment with a substance
they had isolated from beef brain. Because this substance produced inhibition in the nervous systems of multiple species, it was labelled ‘Factor I’ – I for ‘inhibitory.’ Because of his familiarity with the emerging world of neurological chemistry, Elliott could correlate this work with the initially unrelated discovery that brain tissue often contained high quantities of GABA. According to Jasper, “Since we were searching for seizure mechanisms as well as an inhibitory neurotransmitter substance we were impressed by the fact that experimentally produced pyridoxine (B6) [a GABA inhibitor] deficiency produced seizures in experimental animals....The stage was then set for the possibility that the most active component of Ernst Florey’s Factor I might be GABA.”62 Through the use of infrared spectroscopy and chromatography, Elliott and his team were able to confirm that Factor I was in fact GABA, and to launch a number of research projects on its epileptogenic properties.

For our purposes, the collaboration between Elliott and Jasper illustrates a number of crucial points about the emerging post-war scientific culture of the MNI. First, the emphasis on a specific medical puzzle, that of epileptogenesis, created a focal point around which materials and techniques could be passed back and forth between clinic and lab, and between scientific and medical disciplines. Second, the collaboration drew together a number of scientific workers from diverse international backgrounds; while the actors at the MNI formed a strong assembly and became ‘wired together’ around a common problem, their weak ties to other scientific circles proved crucial in unravelling the GABA mystery. Thirdly, Jasper’s intimate knowledge of epileptic physiology allowed him to correlate his own EEG findings with those of Elliott - in effect, Jasper could use the EEG to coordinate both scientific work, and scientific workers.

The additional scientists who arrived at the MNI as part of the GABA research prefigured the surge of research fellows who would arrive at the MNI following WWII. Between 1945 and 1955, more than 200 fellows passed through the halls of the MNI, with many choosing to stay for

62 Jasper, 452.
extended periods; from the Netherlands, the neuroanatomist Jan Droogleever Fortuyn, from Italy, the electrophysiologist Cosimo Ajmone-Marsan, from Japan the neurologist Juhn Atsushi Wada. While by this point the reputation of Penfield had circled the globe, in no small part due to a high profile articles on the MNI in *Time* magazine and the *Saturday Evening Post*, it was Jasper who drew in the visiting researchers who wished to learn from the master of the EEG. Indeed, in a 1951 letter to the Dean of Medicine, recommending that he be promoted to the rank of full professor, Penfield noted that “about half of the letters that come to the Institute from abroad from graduate doctors seeking advanced training are requests to work with Jasper.”

Simultaneously, Jasper had emerged as the leader of a growing number of electroencephalographers around the world. Jasper founded the Eastern Association of Electroencephalographers in 1939, served as the inaugural president of the American Electroencephalographic Society (AEEGS) in 1946, and as the president of the International Association of Electroencephalographers (founded in 1947). Jasper sat at a prime position to observe and coordinate the emerging profession of electroencephalographers worldwide, and he used that position to consolidate his own direction of the field. Despite its later primary importance as an instrument of clinical diagnosis, Jasper originally envisioned the EEG as part of his broader program for experimental neurophysiology. Reflecting on the fate of the AEEGS later in its existence, one of Jasper’s colleagues remarked to him that “the need to diversify our activities to other areas of cerebral function...[.]this was your intention when you began the society, and...until the early 1970s we were the neuroscience society, with a broader mission than just clinical EEG.”

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65 Penfield to Duff, 27 February 1950, Penfield Fonds, Box 2, Folder A/M 3 2/3.
66 Jasper also acted as the editorial director of the *Journal of Electroencephalography and Neurophysiology*. It is notable that the executive editors of that journal consisted of an American, and Englishman, and Jasper, who acted as coordinator from the journal’s editorial offices, which were in Montreal. Ibid.
This assessment fits a number of Jasper’s own research projects (including those undertaken with Elliott) which adapted the EEG to investigate more basic neurological processes at multiple levels of cerebral organization. From his early interest in the physical correlates of psychological processes, Jasper had used the EEG and his work at the MNI to tunnel down to deeper levels of brain processes, while simultaneously serving as a mentor to an entire generation of neurophysiologists.

Interlude: Molecular Biology, the Unity of Science and the NRC Committee for Neurobiology

It is here worth pausing to compare the MNI and its community of researchers with the situation developing in the United States during the same period. One of the central contentions of this dissertation is that the approach to integrating the brain and mind sciences at the MNI displayed a fundamentally different ‘style’ than the approach developing south of the border. A brief interlude examining the prehistory of ‘neuroscience’ in the United States will hopefully place the uniqueness of the MNI story, and Jasper’s role in it, in greater relief.

Two developments served to fundamentally shape the emergence of neuroscience in the United States in the middle of the twentieth century; the first was a shift of leadership in the Rockefeller Foundation; the second was the resurgence of the Unity of Science movement following World War II. As we have seen in Chapter 1, Penfield’s vision for the MNI fell neatly in line with the funding priorities of Allan Gregg and the Rockefeller Foundation, particularly in regard to their interest in psychobiology and psychiatry. Reflecting on his role as head of Rockefeller’s Medical Sciences Division, Gregg later commented that “If I were asked to name a single grant that the Medical Sciences Division…has made since 1931, that I consider ideal in purpose, in performance, in local response and in national and international influence…I would say without a moment’s
hesitation the grant to the Neurological Institute of McGill University.”68 This also meant that Gregg considered similar grants made under his aegis, notably to John Fulton’s laboratories, Yale’s Institute of Human Relations, and the Chicago Institute for Psychoanalysis, to be of less significance. In many ways, the MNI represented the zenith of Gregg’s approach to funding medical research – a fusion of clinical and basic research.

Gregg’s tenure as the guiding hand of the Rockefeller Foundation’s medical research, however, was not to last. In 1932, the mathematician and engineer Warren Weaver became director of the Foundation’s Natural Sciences Division, which began to assume much of the funding responsibility previously held by the medical sciences division. While Weaver initially relied on Gregg for advice on how to fund research in the medical and human sciences, by 1935 Gregg’s influence had been considerably reduced. Weaver’s program of ‘molecular biology’ began to dominate. Weaver’s goal was to combine the physical and biological sciences, focusing on “vital processes,” “subcellular biology” and the “biology of molecules.” References to “psychobiology,” and clinical research more generally, began to disappear from Rockefeller Foundation funding documents. As Lily Kay has argued, Weaver’s program for the life sciences displayed a singular ‘molecular vision,’ one that took the fundamental unit of analysis in biology down to the subcellular level. This molecular vision profoundly shaped the funding practices of the Rockefeller Foundation’s program in the life sciences after 1938, when Weaver consolidated his control of the foundation’s funding in the natural sciences.69 Weaver began funding the careers of scientists that had the potential to be ‘transdisciplinary’; in practice, this meant scientists moving from mathematics, physics, and chemistry into biology. As Robert Kohler has observed, Weaver’s program led to the proliferation of

68 Montreal Neurological Institute, Prospect and Retrospect in Neurology: Second Foundation Volume, Pub. for the Staff, to Commemorate the Opening of the McConnell Wing and the Second Foundation of the Montreal Neurological Institute, of McGill University (Little, Brown, 1955), 23.
a number of hybrid disciplines in the post-war period, notably biophysics. Weaver’s primary protégé in this area was Francis O. Schmitt. More will be said about Schmitt below, but for the moment it is enough to note that, by the end of World War II, and under the leadership of Weaver, the emphasis of funding for the Rockefeller Foundation had changed dramatically. Rather than attempting to unite the human sciences under the banner of psychobiology and holism, the Foundation now hoped to unite the biological sciences through reduction to basic molecular systems – from the bottom up.

Weaver’s reductionist and unifying perspective was shared by the resurgent Unity of Science movement, centered around Cambridge, Massachusetts, and the flood of scientists returning home from World War II. As Peter Galison has shown, the Unity of Science movement, born in the coffeehouses of Vienna during the interwar years, underwent a substantial transformation following the end of World War II. Gone was a commitment to a metaphysical unity of knowledge, built from the ground up upon protocol statements. In its place was an idea “of unity that emerged from American collaborative war work” such as the MIT Radiation Laboratory or Harvard’s Psycho-Acoustic laboratory. Weaver became enamored by the resurgent Unity of Science movement, and attempted to implement this vision of collaborative science by promoting the careers of scientists who might be able to transcend disciplinary divides. In this regard, Weaver’s promotion of Schmitt fit a broader pattern.

At the same time, the post-war emphasis on uniting the mind, brain and social sciences was often more rhetoric than reality. As Tara Abraham has summarized in her analysis of the Macy Foundation meetings, which served as the launching pad for cybernetics (a key avenue for the Unity of Science movement) in post-war America:

In a quest for scientific unity that had a decidedly imperialistic flavor, cyberneticians sought to apply practices that were common in the exact sciences - mainly mathematical and theoretical modeling - to problems in disciplines that were traditionally defined by highly empirical and experimental practices, for example, neurophysiology and neuroanatomy.  

While cybernetics talked a big game about unifying the mind and brain sciences, in practice, the results were underwhelming. The most prominent example of cybernetic neurophysiology in the United States was the neural net modeling of Warren McCulloch and Walter Pitts, which aimed to understand the structure of the visual system with the use of computer analogies. While the career of Warren McCulloch was certainly an example of the kind of transdisciplinary career that Weaver might hope to promote, in practice the ideas of McCulloch proved more valuable to electrical engineering than to any understanding of the functions of the human or animal nervous system.

Perhaps the most accurate view of the brain and mind sciences in the United States in the post-war environment comes from the National Research Council's Survey of Neurobiology. In June of 1945, three weeks before the bombing of Hiroshima and one month before the official end of World War II, the chairman of the American National Research Council, Ross Granville Harrison, wrote to Penfield to solicit his input on the possibility of setting up a ‘Committee on Neurobiology,’ whose express purpose was the “promotion and integration of the various lines of research dealing with the nervous system, including the neural basis of behavior.” The meeting to discuss such a committee was set to convene on 29 June of that year in Washington, D.C., and was to feature contributions from a veritable ‘who’s who’ of mid-century neurological and psychological science, including Karl Lashley, John F. Fulton, Rafael Lorente de Nó, Detlev Bronk, Paul Weiss,  

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74 Ross G. Harrison to Wilder Penfield, 15 June 1945, Penfield Fonds, Box 58, Folder C/G 45 N-O.
Louis Weed, and former Jasper associates Leonard Carmichael and F. O. Schmitt, among many others.

Penfield ultimately declined to participate in the Washington meetings of the committee of neurobiology, although he “heartily endorse[d]” the initiative, and contributed a brief paper on “Basic And Applied Science In The Field Of Neurology.” The committee met periodically between 1945 and 1948 but did not produce its final report until 1952. The report painted a picture of an American brain science community that was poised for greatness, but disappointingly disunified:

Research on the nervous system has followed the lines of specialized techniques, and therefore, has become highly departmentalized. with the various lines, such as physiology, psychology, anatomy, histology, embryology, clinical neurology, psychiatry, neurosurgery, and comparative biology, following, for the most part, independent courses.  

According to the report authors, “Lack of cross correlation of information and the reliance upon excessive superstructures of speculation, built upon narrow and often shaky foundations of fact, have led to serious incongruities among the views prevailing in the various separate disciplines, all purportedly pertaining to the same nervous system.” Most importantly, the failure was not one of lack of technology or will, but rather coordination and integration:

A large part can be blamed on the failure to evaluate, correlate and integrate properly the available knowledge so that different lines of research might utilize each other’s sources of information, techniques and results to best mutual advantage.

Thus by 1952, the contrast between the community that had developed around the MNI, and the nascent neurosciences in the United States could not have been more different. The special level of integration that had been achieved at the MNI was noted by the visiting neurologist Paul D. Maclean, who had just visited the institute to observe their operations. Maclean confided to Penfield that:

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76 Committee on Neurobiology, 2.
It is always a thrill to visit the Neurological Institute, but I think it is time you began to charge some of us old returnees tuition for all the instruction we acquire while we are with you. I have frequently wondered why we have nothing comparable to the Institute in the United States. As some of us were saying the other day, it is certainly not because of lack of means.  

Brain Mechanisms, Consciousness and Conditioning  

In late summer of 1953, Maclean and others descended on Montreal for the annual meeting of the International Physiological Congress, held for the first time in a Canadian city. Montreal at that point seemed like an obvious choice. The post-war economic boom had been particularly good to the city, with new construction and revitalization of the downtown area, and a considerable influx of American capital. Toronto had not yet overtaken Montreal as the country’s largest city, and while an emerging group of French intellectuals discussed the problems of American influence on the traditionally Catholic province, the winds of militant language politics had not yet risen to the surface; college and university education had expanded for both French and English-speaking populations, and the Canadian government generally had seen fit to fund science and education at higher levels. While Montreal had been, before the war, a large city with a cosmopolitan flair, it had emerged from the immediate post-war environment with these features singularly enhanced.

Over 700 scientists participated in the International Physiological Congress, but in many ways the most exciting discussions were being had not in Montreal itself, but 80 kilometers northwest, at a small ski resort in the Laurentian Mountains. The satellite conference, entitled “Brain Mechanisms and Consciousness,” was Jasper’s brain child, and the Laurentian Mountains were the

77 Maclean continued: “So this leads one to conclude that the thing we have really lacked is the personality and genius such as yours to head such an institution.” Paul Maclean to Penfield, 25 November 1951, WP Fonds, Box 61, C/G 51 M.  
obvious choice for the venue. The mountains had been the home of the annual ski meetings of the Eastern Association of Electroencephalographers since Jasper founded the organization in 1939, and it was the implications of recent EEG findings that had prompted the conference. By 1953, Jasper was the undisputed master of the EEG, and was on the verge of publishing the culmination of a decade-and-a-half of collaboration with Penfield – their classic text *Epilepsy and the Functional Anatomy of the Human Brain* (1954), which contained their most thorough classification of the epilepsies, and the localization of function within the brain that could be determined from combined EEG and stimulation operations.

The satellite conference (whose proceedings were later published as *Brain Mechanisms and Consciousness* in 1954) was attended by the premier neurological thinkers in North America and Britain, including E.D. Adrian, Mary Anne Brazier, Frederic Bremer, Lawrence Kubie, Karl Lashley, Horace Magoun, Guiseppi Moruzzi, Walle Nauta and Robert S. Morrison and others, along with MNI veteran D.O. Hebb. Penfield was also in attendance, but unlike during the Montreal congress, he hung back - this was Jasper’s show.

The ostensible purpose of the conference was to discuss and compare two separate discoveries about the mammalian nervous system that had been enabled by the EEG. The first was the so-called ‘reticular activating system,’ a brain system delineated and investigated by the American neurologist Horace Magoun and the Italian neurophysiologist Giuseppe Moruzzi in 1949. The second was Penfield and Jasper’s ‘centrencephalic system,’ a concept they had developed over hundreds of epilepsy operations. Briefly, both of these systems were composed of portions of the

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82 Jasper and Bremer, *Brain Mechanisms and Consciousness,* v–xii.
higher brainstem, and were thought to regulate levels of consciousness and awareness. However, the
differences in how these systems were discovered and described are crucial to note. Moruzzi and
Magoun initially severed the reticular formation of cats, and discovered that the cats would reliably
descend into a coma or deep sleep from which they would not awaken. By inserting a stimulating
electrode in the same formation, Moruzzi and Magoun could reliably awaken the cat. By correlating
these findings with EEG readings, the pair argued that the cat’s diencephalon controlled states of
arousal - that, in effect, they had discovered a sort of ‘wakefulness center.’

Penfield and Jasper’s ‘centrencephalic system,’ by contrast, was orders of magnitude more
grand. Through their examination of thousands of epileptic patients, the pair argued that the
diencephalon in fact constituted the site of the ‘highest level of neural integration,’ a concept
borrowed from the nineteenth-century English neurologist John Hughlings Jackson; however,
Penfield and Jasper turned Jackson’s idea on its head. While Jackson had speculated that the final
area of sensory-motor integration - the area of the brain where all sensation and motor functions
would be integrated - would be found in the frontal lobes of the neocortex, Penfield and Jasper
reversed this perspective, and argued that the final area of integration was in fact in the ‘old brain.’
The lowly brainstem was, in fact, the coordinating center for all sensory-motor pathways, and this
fact explained the most perplexing type of epileptic seizure - those seizures that began with a
complete loss of consciousness and had no obvious foci. By reasoning from their studies of
electrical stimulation in conscious patients, Penfield further argued that this centrencephalic system
was the area of the brain where ‘attention’ was controlled. The system acted as a sort of
‘switchboard’, through which all sensory and motor information flowed. Careful not to lapse into

83 Giuseppe Moruzzi and Horace W. Magoun, “Brain Stem Reticular Formation and Activation of
the EEG,” *Electroencephalography and Clinical Neurophysiology* 1, no. 1 (1949): 455–73; Louise H.
the Cartesian thinking of old, Penfield noted that this system was not the ‘seat’ or center of consciousness, but rather the most crucial link in the chain of neural activity that might integrate the activity of the different regions of the brain to create the moment-to-moment experience of conscious life.84

It is worth here pausing to consider the differences between Moruzzi and Magoun’s reticular activating system (RAS) and Penfield and Jasper’s centrencephalic system (see Figure 3.2). While the RAS was the result of animal research and the relatively limited combination of stereotaxic surgery and the EEG, Penfield and Jasper’s was the result of a collaborative effort that involved thousands of patients and at least three separate specialties.85 Indeed, in discussion of the similarities between the two systems, Magoun that:

Dr. Penfield, in private discussion yesterday, used the simile that his work was like a hand reaching down from the cortex to grasp another hand reaching up from the brain stem….It seems to me that this simile could be broadened to a hand reaching down from clinical studies to grasp another reaching up from the experimental laboratory. The idea of the centrencephalic system reaching up into the cortex arose in Montreal and now the hand reaching down from the cortex to the centrencephalic system is beginning to emerge from studies in Dr. Penfield’s Institute and elsewhere.86

Jasper and Penfield’s conception of the centrencephalic system strove to make sense of both clinical and laboratory work, and human and animal data. The very conception of the brain as a series of interacting systems, rather than an equipotential mass or series of localized functions, was a quintessential example of the Montreal school itself. Moreover, Penfield and Jasper could take credit for introducing this perspective on brain research, and predating the discovery of Moruzzi and Magoun by more than a decade. Indeed, in one exchange during the Laurentian conference, Jasper took the opportunity to undercut the originality of Moruzzi and Magoun’s approach, noting:

85 Jasper and Bremer, Brain Mechanisms and Consciousness, 510.
86 Jasper and Bremer, 307.
In Montreal we have been emphasizing for years the fine structure of the brain system so that I am glad to hear Dr. Moruzzi re-emphasize the importance of studies of detailed topographical organization of the system rather than its mass activity... \(^8^7\)

![Figure 3.1 - On the left, Moruzzi and Magoun’s 'reticular activating system'. On the right, Penfield and Jasper's 'centrencephalic system.' From Brain Mechanisms and Consciousness, 13, and Epilepsy and Cerebral Localization, 475, respectively.](image)

Yet while the centrencephalic and reticular systems were the stars of the 1953 Laurentian conference, Jasper had already embarked on a new avenue of research that would be crucial to bringing the perspective of the MNI and the Montreal school to a larger stage. By the early 1950s, Jasper was the master of the EEG, with dozens of research publications under his belt, and a key role in organizing the professional development of electroencephalography worldwide. Yet Jasper had already begun to experiment with a different technology that would shift his research interests in a new direction. Beginning in the early 1950s, Jasper had begun to experiment with a new technology – the microelectrode – that might clarify the relationship between gross surface EEG phenomena, and the individual discharges of single nerve cells. In attempting to tunnel down into deeper levels of functional organization within the brain, Jasper enlisted the aid of the Chinese MNI fellow Choh-Lu Li, who had arrived in 1950. Li had introduced the technique of glass microelectrode recording from the cortex of cats, and Jasper and Li used this new technique to

\(^8^7\) Jasper and Bremer, 510.
record from deep cortical structures in the brainstem. At the same time, Jasper’s student and technician, David Hubel, had begun a residency at the Walter Reed Medical Research Laboratories, and began to instruct his mentor in the use of the tungsten micro electrodes (more will be said about Hubel in the Conclusion). This technology allowed for so-called ‘single unit’ recording of neural activity - in essence, it allowed for the recording of the electrical activity of a single neuron, pierced by a remarkably thin micro-glass pipette or metal needle. Despite his initial opposition to a reductive mode of thinking about neural organization, Jasper embraced the micro-electrode not as a replacement for the EEG, but rather as way of enhancing his existing studies. Beginning with studies of the thalamocortical system of anesthetized cats, Jasper and Li began to clarify the relationship between the observed slow-wave oscillations of the large-scale EEG recordings and the actions of single cells within the brain.  

The relation of the EEG to the microelectrode opened up another avenue of research for Jasper that was even more crucial. Jasper had long maintained an interest in the classical psychological concept of learning and the ‘conditioned response.’ His acquaintance and collaboration with Donald Hebb in the early days of the MNI (see Chapter 2) had made Jasper familiar with not only classical notions of the conditioned reflex, as demonstrated by Pavlov, but with the more sophisticated theories of conditioning as elaborated in Hebb’s own theory of cell assemblies. Beginning in 1941, Jasper began a series of studies with graduate students from Hebb’s department of psychology, beginning with attempts to condition the occipital alpha rhythm, and leading to more complex studies of single-unit recording of learning processes using micro-

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electrodes implanted in monkeys.\(^8^9\) (See Figure 3.3) These were, in effect, the first studies that used the microelectrode to study behavior. As Jasper and Hebb’s departments began to trade theory and technique, data began to mount that expanded classical theories of learning by suggesting that conditioned responses, while observable with microelectrode methods, could be enhanced, inhibited

\(^8^9\) Charles Shagass and Herbert H. Jasper, “Conditioning of the Occipital Alpha Rhythm in Man.,” *Journal of Experimental Psychology* 28, no. 5 (1941): 373. Shagass’ interest in the problem of conditioning was related to a number of experiments conducted by Hebb and others on the issue of sensory deprivation, itself a sordid story connected to concerns about mind control following the Korean war. In a 1955 report to the Canadian National Research Council, Hebb commented positively on the new collaboration with Jasper, alongside a series of additional research projects on everything from animal behavior to bilingualism. D.O. Hebb Fonds, Folder 0000-2364.01.205.
or otherwise altered based on additional stimuli that affected the experimental animal's attention. “We had a beautiful picture of what the cells do in the cortex during conditioned reflexes,” Jasper later recalled.

What is crucial to recognize about these conditioning studies is that, in addition to representing the logical conclusion of Jasper's mission to reconcile different levels of analysis of the nervous system, they also made the reticular system ideologically palatable to Russian physiologists. As will be detailed below, by focusing on conditioned responses, as opposed to localized function or genetic predisposition, the learning studies created a bridge to an entirely foreign culture of neurophysiology, separated from the West by the imposing Ural Mountains, and the even more imposing Iron Curtain.

**The Moscow Symposium and the IBRO**

The initial Laurentian conference on Brain Mechanisms and Consciousness, in addition to triumphing the interdisciplinary approach of the MNI, was also an event of international significance. Along with representatives from dozens of countries, a delegation of Soviet physiologists attended the conference – a development that was more likely in Canada, given that the United States was then at the height of McCarthyist anti-communism. While Canada had experienced its own anti-communist hysteria during the early years of the Cold War, by 1953, and partially as a result of the debacle of the Korean war, Canadian political culture had begun to pull away from the staunch anti-communism of the United States. A more welcoming atmosphere led

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90 Gloor, “HH Jasper, Neuroscientist of Our Century,” 7–8; Li and Jasper, “Microelectrode Studies of the Electrical Activity of the Cerebral Cortex in the Cat.”

91 American Association of Neurological Surgeons, *Herbert H. Jasper, MD Interviewed by Andre Olivier, MD.*

92 This is, of course, not to imply that scientists were free from surveillance or political suspicion in Canada. The so-called Gouzenko affair of 1945, which revealed that small network of Soviet spies had infiltrated the joint Canadian-British atomic energy project, was arguably one of the most
to participation by Soviet scientists during the 1953 Laurentian conference. Indeed, this more welcoming atmosphere was noted by the Soviet delegates in a report on the Montreal conference, while also noting what they saw as the malevolent influence of American capitalism:

Montreal’s original French aspects have been obliterated by the American way of life, which is being energetically introduced….One has the impression of a lively and rapidly developing economic activity. However, we were able to convince ourselves again that wherever capitalism reigns the results of economic development are of little use to the people.93

The delegates from Moscow also found much to object to in the pernicious influence of American science, despite its technical wizardry:

Although the methods of investigation used by foreign physiologists are technically far advanced, these methods merely permit a more precise determination of facts which are already known and the discovery of individual, detached phenomena. These methods cannot yield important and general results. In them lie the limitations of bourgeois science.94

Despite these limitations, the Russian delegates did identify a number of “progressive foreign scientists” who recognized the ‘limitations of bourgeois science,’ and Jasper and Penfield were clearly amongst them. The Soviets went out of their way to praise Penfield, Jasper and the MNI, not only because of their “many-sided treatment of the most important problems of clinical neurophysiology and neuropathology,” but because of the prominently displayed name of Ivan Pavlov on the Institute’s entrance.95

Important episodes that destroyed trust between the wartime allies, and kick-started the global Cold War. The result of the Gouzenko affair was, according to the historians Reg Whitaker and Gary Marcuse, a state of “scientists under surveillance” in Canada, which included considerable surveillance of the left-leaning Canadian Association of Scientific Workers. However, by 1953 this initial anxiety had dissipated. Moreover, the Liberal government never succumbed to the temptation to pass extreme anti-communist legislation during the height of American McCarthyism. For more on science in the United States and Canada during the Cold War, see J. Wang, American Science in an Age of Anxiety: Scientists, Anticomunism, and the Cold War (Chapel Hill: University of North Carolina Press, 2000); R. Whitaker and G. Marcuse, Cold War Canada: The Making of a National Insecurity State, 1945-1957 (Toronto: University of Toronto Press, 1994).

95 Voronin.
It was not only good manners that facilitated the eventual meeting between Jasper and the Soviets; Jasper’s work on conditioning made much of western electrophysiology palatable for the Soviet interlopers. With the Bolshevik Revolution and the emergence of the Soviet Union, much of Russian psychology had embraced the reflex physiology of Ivan Pavlov, and the ‘mental reflexes’ of Ivan Sechenov. For Russian psychologists and physiologists, any attempt to posit a central directing element of the cortex that was independent of external stimuli, such as the reticular system or centrencephalic system, was ideologically threatening. As Loren Graham has observed, the reticular and centrencephalic systems were of considerable concern to Soviet physiologists and psychologists, who “saw the reticular formation as a foundation on which physiological idealism was being rebuilt.”96 Initially, this had led Soviet scientists to be quite critical of the theory and its variations. However, Jasper’s conditioning studies showed that the centrencephalic system was compatible with Pavlovian concepts of conditioned reflexes.

Jasper’s studies of conditioning created an ideological bridge to Russian physiologists, and his activities as a coordinator of electroencephalographers worldwide also created more tangible connections to the Soviet Union. As the electroencephalographer Mary Anne Brazier recounted, “[In the United States] we could not succeed in getting visitors from the USSR until Kruschev’s denunciation of Stalin in 1956.”97 By contrast, Jasper, as president of the International Federation of Societies for EEG and Clinical Neurophysiology (IFSECN), was able to facilitate meetings in Canada and Europe that would bring Russian neurophysiologists in contact with those in the West.

96 Graham notes that the reticular formation was objectionable for the Soviets on many fronts. In addition to implying “spontaneous activities (output without corresponding input),” its filtering and shaping influence on sensory processes which made it difficult to maintain a “copy-theory” of knowledge, as most dialectical materialists have done” and because it might function to reinsert religious discourse into physiology, acting as a Cartesian “seat of the soul.” Graham’s is the most extensive discussion I have found of this issue, and also notes that these issues came to a head at the Ste.-Marguerite conference, although he is incorrect that the Soviets were not present. Loren R. Graham, Science and Philosophy in the Soviet Union (New York: Alfred A. Knopf, 1972), 404–7.

prior to 1956 - notably during the Montreal and Laurentian conferences in 1953, and the 1955 IFSECN colloquium in Marseilles. These efforts would eventually lead to the pivotal Moscow symposium of 1958. According to Brazier:

the Moscow colloquium…was the culmination of EEGers’ effort in several countries to make a bridge between East and West....scientists were able to travel to France and it was there at 1955 in Colloque de Marseille that [the Soviets] first joined in plans for an EEG Colloquium in Moscow.98

Fortunately for historians, Jasper kept a diary of his visit to Russia, and later submitted a report on the trip. It is worth examining these documents at some length, as they reveal a great deal about the national differences in neurophysiological research in the middle of the twentieth century - and of Jasper's attempts to bridge those gaps.

The Moscow Colloquium was organized by the Russian Academy of Science, including Georgii Donatovich Smirnov, who had spent time at the MNI as part of an exchange program, along with Dmitry V. Sarkisov, L. L. Voronin and Ivan Solomonovich Beritashvili, one of the leaders of neurophysiology in Georgia and Eastern Europe. Henri Gastaut, the French physiologist with whom Jasper had become acquainted several years earlier, was also intimately involved.

Jasper departed for Russia on 3 October 1958, with bags and pockets heavily laden with lantern slides for his various presentations. Jasper felt an air of trepidation about his voyage: “I seemed to sense that this was an unusual trip and because of premonitions and propaganda that we have about Russia, I did feel a little apprehension, wondering if really I should ever return to my family.”99 Traveling in a DC-7, Jasper flew from Montreal to London, and then on to Paris before finally landing in Prague, noting that from the air one could see an enormous red star, and hammer and sickle on the roof of the main airport building. “On getting off the plane our passports were

98 Ibid, 1.
99 Jasper Fonds, Box 8, File 372, 4.
taken by a very officious, tough looking soldier with a steely glint in his eye. Finally taking off for Moscow itself, Jasper felt awash in a fog of secrecy and foreboding, noting that the airplane stewardesses would occasionally whisper to each other in corners of the plane “as though not wanting to be heard. All of this was very mysterious and I felt that finally I had entered the atmosphere behind the Iron Curtain.”

Jasper was met in Moscow by Smirnov, and a young physiologist, Olga Vinagradova, whose husband worked in the psychology laboratory of Alexander Luria. Over the next two days, Jasper and his colleagues toured various villas and exhibitions in the suburbs of Moscow, including an exhibit of digital and analog computers at the Academy of Science, and a full-scale model of the satellite Sputnik and an operating nuclear reactor - all displays of technical prowess clearly meant to impress the foreign visitors.

The colloquium itself began on 6 October, and was held in a building that was “obviously a residence of a former wealthy Muscovite taken over for this purpose [and was] equipped with simultaneous translation rooms....” Twenty-four representatives from Western nations were present, along with twenty-four representatives from Soviet satellite nations, comprising seventeen countries in all. In addition to the representatives, there was an audience of nearly five hundred young scientists, drawn from a pool of over one thousand applicants, and equipped with headphones for simultaneous translation of presentations. The colloquium was introduced by a former student of Pavlov (and friend of McGill’s own Boris Babkin), “a traditional Pavlovian experimenter, now holding the position, really, of Pavlov in the laboratories in Leningrad. He spoke of the dynamics of excitation and inhibition and used traditional Pavlovian methods.”

100 Ibid, 7.
101 Ibid, 9.
102 Ibid, 10-15
103 Ibid, 15.
104 Ibid, 16-17.
Conspicuously, during a brief review of the history of the EEG, Voronin noted that Berger had “failed to achieve much in line of progress because he did not recognize the principles of Pavlov in studying higher nervous activity.” Vorin then went on to note that “Fessard and Jasper then introduced the use of Pavlovian Principles together with the EEG and much progress was then made.” Jasper’s EEG work on conditioning, then, provided a bridge between Russian and Western scientific cultures.

The papers presented were an intriguing amalgam of research on Pavlovian conditioning experiments and different attempts to square these results with EEG work. Notably, a paper by Kupalov showed that, while secretory reflexes could be studied easily in decorticated animals (animals with most of their brains removed), these conditioned salivary reflexes were diminished or disappeared following removal of certain portions of brain mass. The following day, Jasper presented his own work on conditioning in monkeys, followed by a presentation by Moruzzi and Magoun. On 9 October, Anokin presented a paper on the recording of electrical activity from cortical and sub-cortical structures during conditioning, and according to Jasper, gave “an excellent theoretical treatment of the results, particularly with reference to the function of the brain stem and hippocampus in learning processes,” a topic of considerable interest to all those from the MNI who had been involved in studies of memory and brain structure (see Chapter 2).

While the scientific topics under discussion represented an intriguing combination of Western and Soviet research traditions, the relative harmony of the colloquium was occasionally disturbed by reminders of the Cold War context. Following an evening at a local theater, Jasper emerged to a “deafening roar down the street” caused by a parade of large artillery pieces traveling

105 Ibid.
106 Ibid.
107 Ibid, 18.
108 Ibid, 22.
at high speed down through the center of Moscow on tractors.\textsuperscript{109} Moreover, the technical imperative for the Russians was on display during the presentation by Voronin and colleagues, in which they presented a study of conditioning in man “with polygraph recordings of eye movements, muscles, galvanic skin reflex and respiration and electrocardiogram, together with the EEG in a large multichanneled machine.”\textsuperscript{110} Despite its somewhat ominous character, Jasper described this attempt at reconciling multiple streams of physiological data as “excellent work [from which] they drew some interesting conclusions.”\textsuperscript{111}

Jasper was less positive about the quality of many of the laboratories he visited. While he was impressed by the work being done on hearing and evoked potentials in the brain’s auditory system, he was less enthusiastic about the excessive reliance on Pavlovian models of learning:

I visited the chemistry laboratory where they were doing analysis of cortical tissue at different stages of conditioning. This was being done in rats. The rats were conditioned in a small container with automatic administration of food and stimuli and after a certain period of conditioning was obtained the floor would suddenly disappear and the animal would drop into a tank containing liquid nitrogen. He would be suddenly frozen and the brain analyzed for chemical constituents, hoping to find a different concentration of substances related to inhibition, excitation etc. This did not seem to be a very well planned experiment.\textsuperscript{112}

At another institute, Jasper noted:

We saw conditioning experiments with fish and birds and so much that it was impossible to take it all in and one wonders really what it was all being done for. In this institute there were 110 scientific workers and about 350 staff, all working on conditioning or related problems of behavior. It was a vast establishment and one wondered why it was not more truly productive. Obviously, here was a great opportunity being missed by lack of inspiration and new ideas, since they were following too much on the beaten track of old Pavlovian methods and ideas.\textsuperscript{113}

\textsuperscript{109} Ibid, 25.
\textsuperscript{110} Ibid, 28.
\textsuperscript{111} Ibid.
\textsuperscript{112} Ibid, 39.
\textsuperscript{113} Ibid, 41-2.
These incidents suggest that, while Jasper’s own experiments on conditioning had facilitated a dialog between himself and the Russian physiologists, the methods of Pavlov formed a relatively rigid worldview for his Soviet colleagues.

At a final closed-door session of the symposium, Jasper proposed the formation of an international brain research year, similar to the International Geophysical Year which had just ended in 1957. Notably, Jasper and his colleagues initially hoped to organize this year through the IFSECN, rather than UNESCO, suggesting the importance of the EEG organization to the project. Over the next several days of laboratory visits, the plan transformed into one for an international organization for the coordination of brain research amongst the various countries and disciplines represented at the symposium. Despite his initial misgivings, following the end of the symposium, Jasper stopped in Paris on his way home to Montreal to meet with UNESCO officials in order to begin the initial steps towards creating the new research organization. The International Brain Research Organization was incorporated by a Canadian Act of Parliament in Ottawa in September of 1961, although its operations would be run by Jasper in Paris during a year-long leave of absence from the MNI.\(^{114}\)

A number of salient details about the creation of the International Brain Research Organization (IBRO) are worth noting here. First, the importance of the international EEG community, led by Jasper, was paramount. As Brazier later noted, “It was from...strong roots in the IFSECN that IBRO was eventually to flower.” Moreover, because subsequent secretaries general of the IBRO were not electroencephalographers, the roots of the organization in the EEG community were later obscured from view.\(^ {115}\)

\(^{115}\) Brazier would lament to Jasper in a confidential letter: “It is unfortunate that the Secretary-Generals of IBRO that followed you...were none of them associated with IFSECN (or with the meeting in Moscow) and let the Moscow decision drop from sight. So much so, that the 1958 resolution was ignored and the date (1960) of UNESCO’s agreement to support our cause was
Second, the IBRO was explicitly modeled on the approach to interdisciplinarity developed at the MNI. While it would be more difficult for the various clinics and laboratories involved to share materials around the world, the overall relationship between disciplines was to be similar, with no particular theoretical orientation or experimental approach predominating. Indeed, Jasper noted in a number of instances that the conception for the IBRO was “developed with Wilder Penfield and the MNI” and that “My inspiration came at first from working with Dr. Penfield and his team at the Montreal Neurological Institute which was a very good early example of the neurosciences at work, including many of the basic disciplines together with clinical sciences. The term ‘neurochemistry’ was first used here.” For Jasper, the MNI, with its incorporation of basic and clinical research, was in microcosm what the IBRO would become in macrocosm. Indeed, while the organization came to be called the International Brain Research Organization, it was initially to be called the Interdisciplinary Brain Research Organization, a name change that came about because of the involvement of UNESCO.

**A Tale of Two Neurosciences: Schmitt, the NRP and the RNA Memory Theory**

At the same time that Jasper’s organizational efforts were beginning to bear fruit, a parallel development was taking place in the United States, and it is worth pausing here to compare the development of the IBRO with F.O. Schmitt’s Neuroscience Research Program (NRP) in order to highlight the differences between the two approaches. Much of the history of the NRP has been laid out by Schmitt’s close associate George Adelman in a series of articles and books beginning in the 1970s, and it is worth reviewing some of that story here, as Adelman has been quite vocal in always erroneously published as the date of the ‘Founding of IBRO’ as though we did not exist until then.” Jasper Fonds, Box 5, Folder 240, “Notes on the History of the IBRO.”

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116 Jasper Fonds, Box 9, Folder 395.
117 Herbert Jasper to Rudolfo Llinas, 7 October 1993, Jasper Fonds, Box 3, Folder 163.
118 Jasper Fonds, Box 4, Folder 202, 5.
pronouncing Schmitt the ‘father’ of modern neuroscience, and identifying the NRP as its founding organization.119

In many ways, Schmitt and Jasper’s biographies share a number of similarities. Both men were born to minister fathers, and began their scientific training in the 1920s, a period during the history of American education when scientific training sat uneasily next to the more traditional religious orientation of American universities (for instance, Schmitt’s first lab instructor was also a practicing Quaker). However, whereas Jasper had come to physiology and neurology by way of theology, philosophy and experimental psychology, Schmitt truly started from a different direction. Beginning with his training at Washington University in St. Louis in 1920, Schmitt spent much of his early years investigating the phenomena of life from the lowest level of functional organization - the cell and its constituent parts. Initially oriented towards a career in medicine, Schmitt would spend an increasing amount of time, particularly following his postdoctoral studies at the Kaiser Wilhelm Institute in Berlin, immersed in laboratory work that aimed to explore the properties of nerve cells, including their metabolism and the actions of their membranes. A lifelong association with Herbert Spencer Gasser - the same Gasser who had warned Jasper away from the EEG - hints at the direction that Schmitt’s own research would take - one that emphasized the cellular, the molecular, and the biochemical.120

Schmitt’s professional aspirations were given a significant boost in 1941 when he was drafted by K.T. Compton, then president of MIT, to head their department of biology. From this perch, Schmitt spent much of the next decade as a pioneer and organizer of the emerging field of biophysics. This hybrid discipline emerged from two sources: first, the molecular biology of the

1930s that had been largely sponsored by the Rockefeller Foundation, and second, the post-war flood of physicists and mathematicians who entered the world of biology hoping to apply their tools and methods to the study of life (cybernetics was one obvious example of this trend). Years later, Schmitt would characterize biophysics thusly:

Because biophysics, in its maturation to a full-fledged community of scientists in the late 1950s, was firmly rooted in physical chemistry and in general physiology it would naturally tend to relate closely to chemistry in the investigation of cellular constituents (e.g. molecular assemblies, organelles, and partial systems)....biophysicists tend to be reductionistic, to employ the bottom-to-top approach, hoping to find clues concerning mechanisms of the functioning of whole cells, tissues, and eventually the organism itself.

For Schmitt, then, a reductionist or ‘bottom up’ approach to studying organisms and their constituent parts was a fundamental component of his own contribution to the creation of the hybrid discipline of biophysics. Schmitt would later bring this reductionist perspective to his own efforts to unite the neurological and psychological sciences.

Beginning in the 1950s, Schmitt began to organize what was called the “Mind-Brain” group, a regular meeting of about 30 associates, at MIT. Schmitt and some of the members of the Mind-Brain group had connections to Jasper and the MNI (for example, the neuroanatomist Walle Nauta had participated at the Laurentian conference, while the psychologist Robert Galambos had attended the Moscow symposium), while others, such as the fast-reaction physicist Manfred Eigen or the zoologist Theodore Bullock, had little connection with the brain and mind sciences at all. The ‘Mind-Brain’ group eventually expanded to form the Neurosciences Research Program in 1962.


123 Adelman, “The Neurosciences Research Program at MIT and the Beginning of the Modern Field of Neuroscience.”
A number of contrasts between Schmitt and Jasper’s approaches are immediately apparent. Jasper’s approach, as illustrated in this chapter, was holistic on a number of levels - instrumentally (the EEG), philosophically (emphasizing a medium-level of investigation, that of brain systems), and epistemologically (reconciling evidence and results across multiple disciplines and perspectives). By contrast, Schmitt was an unapologetic reductionist, who insisted not only on proceeding from the ‘bottom up,’ but upon the centrality of molecular processes for any understanding of higher-order brain functions.

Important, but subtle, theoretical differences informed the perspectives taken by the MIT and MNI circles. For instance, Karl Lashley’s famous 1950 paper “In Search of the Engram” laid the foundation for much of Schmitt’s understanding of the problem of memory. According to Schmitt:

Lashley, in dealing with this problem [of memory]...had suggested that a particular type of substance, which he called an engram, might represent the substantive basis for the encoding of experiential information, i.e., of long-term memory...Lashley never did discover the nature of his ‘engram’....However, it caused some of us to inquire into the chemical principles by which molecules may encode particular kinds of long-term ‘memory.’

This creative misreading of Lashley’s principal of equipotentiality (all parts of the brain are equally important in forming intelligence, and the memory engram is not encoded in any one brain structure), allowed Schmitt to combine his interest in psychological function with the major developments in molecular biology of the 1950s, notably the discovery of the structure of DNA by James Watson and Francis Crick, and the advances made in understanding the so-called ‘chemical memory’ of the body’s immune system. The emerging research direction of the Mind-Brain group, and that of the NRP more generally, was oriented towards discovering the chemical nature of a so-called ‘memory molecule,’ initially identified as some variant of RNA.

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125 Adelman, “The Neurosciences Research Program at MIT and the Beginning of the Modern Field of Neuroscience.”
The RNA memory hypothesis formed the crux of the early organizing efforts of Schmitt, and defined the theoretical orientation of the NRP. Simultaneously, the work of Brenda Milner in delineating the brain structures of memory formation was conspicuously ignored. This was made all the easier because of Schmitt’s near total ignorance of the activities of Jasper and the IBRO. In a remarkable interview conducted in 1985, ironically as part of the Francis O. Schmitt Oral History of Neuroscience Project, Jasper laid out the history of his association with Schmitt, and his own feelings about the NRP. Given the revealing nature of this interview, and the contrasts drawn in it between his own approach and Schmitt’s, it seems wise to examine carefully.

Jasper discussed his association with Schmitt, including their early encounters at Washington University in the 1930s. The two scientists met semi-regularly after 1933, occasionally sharing tables at dinner parties at Harvard, and at meetings at Woods Hole and Cold Spring Harbor. According to Jasper:

I’d come from a different origin - instead of biophysics, I’d come from philosophy. I soon discovered that Frank and I really originally had the same objectives. He was also the son of a minister, as I was, which stimulated us to be interested in the mind and the spirit of man....Schmitt then became enamored of the molecular approach to an understanding of nerve membranes. Although we differed in our approach, Frank was convinced that we can’t go directly to the more complex processes in the brain. We had the same original interest, but he believed that in order to lay a groundwork for an understanding of brain function, you had to start at the bottom and learn about nerve membranes, how ions were transported across nerve membranes, how the action potential was produced by these ionic flexes, and the importance of the molecular structure. Until we could understand these basic mechanisms, it was futile to try to understand the complicated processes of the brain.126

The pair later diverged not just intellectually, as Jasper embraced the EEG and the clinical approach to examining brain function, but also physically, as he departed for his travels in Europe and resettlement in Montreal. Indeed, Schmitt and Jasper would not meet again until the 1960s. Because of the importance of this reconnection, it seems wise to quote Jasper’s description of it at some length:

126 Jasper Fonds, Box 4, Folder 202, 2
The reason for our meeting was that in 1958 and 1960 I was involved in organizing the International Brain Research (IBRO). This organization developed out of meetings in Russia and Europe in which we looked at the future of brain research, needing the interaction of the various disciplines working together and more international collaboration. We had to bring all these disciplines to bear on the single problem of brain function. That's the reason we organized the International Brain Research Organization, which was the first formal organization which combined all the disciplines concerned with the brain into one scientific activity and one scientific organization. Anatomists, physiologists, chemists, behavioral psychologists, biophysicists, communication engineers all joining together in one organization for research on the brain. In those days, however, it was not called “neuroscience” yet. It was interdisciplinary brain research. That's the original definition of IBRO as [the] “inter disciplinary brain research organization”, but it got changed by UNESCO later into [the] “international brain research organization.” At this same time, and quite independently, Frank...was getting this same idea. He wasn’t in on the formation of IBRO, he wasn’t aware of what was going on over there at the time. He became aware soon afterwards.\textsuperscript{127}

Schmitt’s complete ignorance of the formation of the IBRO is difficult to explain, beyond the fact that the Cold-War context of the early 1950s may have made his view myopic, or because of his own immersion in the molecular world of American biophysics. In either case, it seems clear that Schmitt and the Neuroscience Research Program were latecomers to a party that had started over two decades earlier, and 300 miles to the north.

If Schmitt’s view of neuroscience outside of America was myopic, then Jasper’s view of Schmitt’s neuroscience was rather dim. Following their re-acquaintance in 1960, Jasper occasionally attended NRP workshops, and his evaluation of them was far from positive:

> It was a curious hodgepodge of people [Schmitt] brought to these workshops. They were sometimes not very successful because they couldn’t understand each other’s language. If you wanted to talk to a physicist about the functions of the hippocampus, first you had to tell him what the hippocampus is, what it does, where it’s connected and so forth. We spent a lot of time, wasted a lot of time, some of us thought, teaching physicists about neuroanatomy and neurophysiology instead of getting on with our discussion.\textsuperscript{128}

Jasper’s lament about the inability of the NRP participants to understand each other’s language is all the more poignant given his invocation of the hippocampus as an example - the very structure

\textsuperscript{127} Jasper Fonds, Box 4, Folder 202, 5.
\textsuperscript{128} Jasper Fonds, Box 4, Folder 202, 5.
whose importance had been so vividly demonstrated by his MNI colleague Brenda Milner (see Chapter 2). Elsewhere, Jasper heaped scorn on Schmitt’s attempts to discover the basis of memory in the actions of single molecules:

The basis of [Schmitt’s] neuroscience research program in the early days was the molecular approach to brain function and this was stimulated by the dramatic developments in molecular biology and genetics. There was a surge of rather misguided interest at the time, may I say, in relations between memory and molecular information that might store memories as to [how] codes are stored in the macromolecules of DNA....Frank, I think, was open minded, but he favored the idea of molecular memory for a number of years until he, himself, with his objectivity, saw that this wasn’t the answer....In his usual very stubborn way...he was determined to continue the development of the molecular theory of memory and information processing in the brain. Regardless of my and others’ criticism of this approach, and the failure to come up with really solid experimental evidence on learning and memory, he persisted. He continued to write articles and edit books on the molecular basis of the function of the brain.129

While Jasper acknowledged that the discovery of neuropeptides in the 1970s lent a small amount of credibility to Schmitt’s perspective, he was adamant that the RNA memory theory, as proffered in the 1960s, was scientifically baseless. In 1968 Jasper and Benjamin Doane, his co-investigator on the original conditioning studies that led to the Moscow colloquium, concluded that “there is no convincing evidence that there is a molecular storage of encoded ‘information’ in memory storage by analogy with the genetic code.”130 Indeed, the so-called RNA memory theory made almost no headway whatsoever within Montreal, with one notable exception: the psychiatrist Ewen Cameron, the head of the Allan Memorial Institute psychiatric hospital, whose troubled relationship with the MNI will be examined in Chapter 4.

The Neuroscience Research Program and the International Brain Research Organization, then, developed in parallel and in relative isolation from one another, with their own separate approaches to the fundamental problem of memory; for Jasper’s IBRO, memory was best

129 Jasper Fonds, Box 4, Folder 202, 7-8.
understood using the combined tools of the neurosurgical case study, the EEG, the micro-electrode, and theories of learning borrowed from contemporary psychology; for Schmitt’s NRP, the same problem was a starting ground for speculation about the relationship between molecular processes and information theory. Moreover, Jasper’s vision of neuroscience was one in which interdisciplinarity and internationality were enmeshed in a scientific ethos that valued cosmopolitanism and collaboration. By contrast, Schmitt’s NRP was remarkably inward looking, asserting its right to start from scratch with an explicitly molecular focus. In an ironic twist, when the American Society for Neuroscience, the first professional organization for neuroscientists, was formed in 1969, much of the data for its initial organization came from the IBRO. As Jasper outlined:

[The] organization of neuroscience and the starting of the American Society for Neuroscience came from two sources. One was that IBRO [which] made surveys of neuroscience in all the countries belonging to IBRO and published volumes outlining the work of all the laboratories of neuroscience in different countries. The survey in the United States was a great big volume which we did with the Academy of Sciences, and it opened their eyes about the possibilities that nobody had ever seen, i.e. the combination of all these different disciplines into one neuroscience…. Out of this IBRO survey with the Academy was born the Commission on Brain Research of the Academy of Sciences, and from the Commission on Brain Research was developed the Society for Neuroscience. Now that was the formal way it was done, but I’m sure that Frank’s Neurosciences Research Program certainly had some influence on the thinking of people in the organization of the American Society of Neuroscience.

The Society for Neuroscience, then, benefited directly from Jasper’s organizing efforts, and his nearly three decades of experience in organizing the world’s electroencephalographers and neurophysiologists. If the MNI had been rooted in a cosmopolitan meeting of scientific cultures, then Jasper had succeeded in extending that vision beyond the city of Montreal. If ‘modern’ neuroscience can be said to have a birth moment, it was certainly in this globalization of MNI

131 Schmitt and the NRP make brief appearances in Kay’s examination of information theory and the cracking of the genetic code, although her focus is not on neurological or psychological issues per se. Kay, Who Wrote the Book of Life?: A History of the Genetic Code.
132 Jasper Fonds, Box 4, Folder 202, 6.
interdisciplinarity, and not in the more reductionist program of the NRP. Schmitt may have had the distinction of giving the mature field of ‘neuroscience’ its name, but that seems to have come despite having had little to do with the field’s creation.\textsuperscript{133}

In many ways, the role of Jasper and the IBRO in creating modern neuroscience was obscured and minimized by a certain historical myth-making initiated by Schmitt himself, and perpetuated by his student and colleague George Adelman.\textsuperscript{134} This historical revisionism appears not to have been malicious or ill-motivated, but rather the product of a genuine ignorance about the extent of Jasper and Penfield’s achievement. Writing to Jasper in 1991 following a conference at the MNI, Adelman, who more than anyone propagated the notion that neuroscience was a product of MIT, confessed that his own understanding of the development of neuroscience had been profoundly shaken by the presentations he saw, and that he would have to revise his historical sense of the importance of clinical research. It seems wise to quote Adelman’s confession at some length:

\begin{quote}
the talks [at the MNI conference] gave me a new perspective on Dr. Penfield’s and your accomplishments in developing the Montreal Neurological Institute. I am beginning to realize much more clearly that you and he, in a very real sense, were primary founders of the field of neuroscience. Given my “basic” and NRP bias, my mind set/impression was that the problem of “how the brain works” was attacked originally by basic scientists from a primarily philosophical-psychological base. It should have been more obvious to me that clinicians who, closely involved with the faulty brain and wanting to know how to fix it, would have been among the first to want to know how it worked. At any rate, the “obvious” was not obvious and in my own thinking I stayed on the basic-science side...[You] further reminded me that I must broaden my views and pay more than lip service to the importance
\end{quote}

\textsuperscript{133} Even this distinction for Schmitt is open to question. The coining of the word ‘neuroscience’ is unclear, but even George Adelman, who propagated the notion that Schmitt was the field’s founder, concedes that the name was likely coined by Ralph Gerard sometime in the 1950s. Adelman argues that, while Schmitt might not have coined the word, he was intimately responsible for the field’s orientation and organization. The information presented in this chapter should hopefully make such an interpretation untenable. Adelman, “The Neurosciences Research Program at MIT and the Beginning of the Modern Field of Neuroscience,” 15.

\textsuperscript{134} Adelman, “The Neurosciences Research Program at MIT and the Beginning of the Modern Field of Neuroscience.”
of the feedback from clinical neuroscience to basic neuroscience for a true understanding of
the mechanisms of brain and mind.\textsuperscript{135}

\textbf{Denouement: Science, Separatism and the Université de Montreal}

For Herbert Jasper, interdisciplinarity and internationality were mutually reinforcing scientific
virtues, and the setting of Montreal itself provided a rich political metaphor for his understanding
of scientific exchange and cooperation. His dismissal of the NRPs approach to interdisciplinarity -
as one in which the participants weren't even speaking the same language - is all the more poignant
given the MNI's setting in a bilingual city that attracted scientists from a wealth of national
backgrounds. Yet Penfield and Jasper's idealized vision of Montreal as a harmonious cosmopolitan
hub often contrasted with the political realities of mid-century Quebec. Indeed, in the next chapter,
we will see how Jasper drew a parallel between the increasing isolation of French and English-
speakers in Montreal, and the lack of cooperation that had developed between the MNI and the
city's psychiatrists. However, the notion of a frictionless interdisciplinary exchange within the MNI
itself began to encounter difficulties, in parallel with the turbulent decades of Quebec's \textit{Révolution
tranquille} (Quiet Revolution) and the rise of French-Canadian separatism. While it would be facile to
suggest that the ultimate decline of the MNI was the result of linguistic separatism, the issue
certainly informed Jasper's ultimate departure from the institute and his move to the Université de
Montreal, the city's premier French-language university on the opposite side of Mount Royal.

The decline of the MNI's dominance of neuroscience and Jasper's departure will be
examined in the Epilogue to this dissertation, but briefly, Jasper departed the Institute in 1965 in
order to establish an independent neuroscience research institute at the Université de Montreal that
would operate almost exclusively in French, and focus on combining electrophysiological

\textsuperscript{135} Given that Adelman wrote an article in 2010 again claiming that neuroscience was a product of
MIT, it is unclear whether he took his own advice. George Adelman to Herbert Jasper, 25 October
1991, Jasper Fonds, Box 3, Folder 151.
investigation and neurochemical analysis. Jasper stated publicly that the reason for his departure was the reduced emphasis on laboratory investigation at the MNI, and his increasing difficulty in obtaining funding for his more basic laboratory research. Privately, however, Jasper drew an explicit connection between his departure and the roiling cauldron of political discontent in French Canada. In 1971, just a year after the dramatic October Crisis that saw the murder of Pierre Laporte by the Front de libération du Québec (FLQ) and the invocation of the War Measures Act by Prime Minister Pierre Elliot Trudeau, Jasper wrote to his friend and colleague Tamas Frigyesi of Columbia University that “one of my main objectives in leaving McGill University was to try to de-fuse the explosive political situation here in our Province.”

By the 1990s, Jasper described the reorientation of the MNI toward molecular research as a kind of political separatism. In a speech for the 50th anniversary celebrations of the MNI, Jasper lamented that he was “saddened and concerned by the failures of the Institute to follow some of the main goals establish[ed] by the founders...” Jasper went on to note that:

> the rapid growth of neuroscience in the past 20 years, especially in the field of molecular biology and brain chemistry, has made the problem of continuing the tradition of multidisciplinary research much more difficult today than it was 50 years ago - but I don’t believe it to be impossible. It is my impression that the Institute has been infected by a virulent virus and needs radical treatment to recover from the deadly disease of separatism. (perhaps acquired from the political environment of Quebec!)

Indeed, in a final insult, the ‘political disease’ of molecular neuroscience came to swamp even the evaluation of Jasper’s career by his fellow scientists. In 1972, in a volume of essays in honor of F.O.

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136 Penfield confided to a colleague that Jasper’s departure was because he believed that too much of the funding that came from the Provincial and Federal governments was going to support the salaries of neurosurgeons, at the expense of laboratory investigation. Privately, Jasper wrote that the ‘Block Grants’ given to the MNI by Canada’s Medical Research Council were given to the Institute for his research, but that he never received the money because it was instead used to pay for the salaries of neurosurgeons. William Feindel and Richard Leblanc, *The Wounded Brain Healed: The Golden Age of the Montreal Neurological Institute, 1934-1993* (Montreal: McGill-Queen’s University Press, 2016), 361–62.

137 Herbert Jasper to Tamas L. Frigyesi, 5 November 1971, Jasper Fonds, Box 1, Folder 55.

138 Jasper Fonds, Box 5, Folder 257.
Schmitt, Jasper’s own contribution was entitled “Philosophy of Physics - Mind or Molecules,” a dissenting opinion against what he saw as Schmitt’s overly reductionist perspective on the scientific field he helped to invent. However, by 1986, a similar collection of essays in honor of Jasper himself carried the title Neurotransmitters and Cortical Function: From Molecules to Mind, inverting the perspective that he had maintained for much of his professional life. Explaining the change, his MNI colleague Pierre Gloor stated: “Why, one may ask, have we reversed the direction of this path in describing it as one ‘from molecules to mind? I think Herb Jasper will understand....The higher organizational principles which emerge when these [molecular] mechanisms act in a complex information-handling system such as the brain are yet to be studied.”

Yet much of Jasper’s career had been dedicated to studying these ‘higher organizational principles’ across multiple sites of scientific and medical work, and in so doing he left an indelible impression on the development of neuroscientific thought. In a private letter lamenting his inability to attend a 1970 conference in Jasper’s honor, the noted neurologist Paul MacLean relayed a fitting tribute that poignantly linked Jasper to his adopted country: “I regret that this weekend I will be in Calgary, Alberta and too far away to join in the celebration of your remarkable contributions to the knowledge of the brain. In Calgary I shall not be far away from Jasper National Park, and I am thinking that it must be a great satisfaction to you that there will always be a worldwide Jasper Cerebral Park in the minds of neurological scientists.”

Jasper’s career, both in the MNI and as a leader in the emerging community of electroencephalographers, illustrates a number of crucial themes in this dissertation. First, Jasper’s career is a prime illustration of the importance of connected assemblies of historical actors, and

139 Jasper, “Philosophy or Physics—Mind or Molecules.”
141 Paul Maclean to Herbert Jasper, 29 September 1970, Jasper Fonds, Box 1, Folder 48.
their role in transferring developments from laboratories around the world to specific historical sites where they could be reconfigured. Jasper, Penfield and other MNI actors formed strong assemblies as they coordinated their activities around clinical and laboratory problems (as in the case of their work on cerebral localization and epilepsy, their work with GABA, or their studies of conditioning), but also benefited from their weak ties to other areas of research and other national scientific cultures (as can be seen in Jasper’s connection to the physiologists of Russia, as enabled by his conditioning studies). Second, the story of Jasper’s efforts to organize the world’s brain researchers along the model provided by the MNI strongly counters the MIT origin story for ‘neuroscience,’ and establishes a plausible alternative. Finally, Jasper’s career highlights the alternative styles of brain research that were developing in Montreal and Cambridge after World War II; one more holistic, clinically oriented, and interdisciplinary, the other more reductionistic, philosophical and transdisciplinary. Jasper’s biography illustrates how following the interacting assemblies of actors in Montreal can provide both a new understanding of the history of neuroscience, and deeper historical understanding of the development of interdisciplinary science more generally.

In Chapter 4, we will observe an instance where an interdisciplinary community failed to form in Montreal, and why interacting assemblies of actors failed to successfully wire themselves together.
Chapter 4 – Two Solitudes: Psychosurgery and the Troubled Relationship between Wilder Penfield and Ewen Cameron

Prologue: Two Solitudes

In November of 1963 Hebert Jasper gave an address at the Allan Memorial Institute of Psychiatry for the opening of its newest research laboratory. The Allan Memorial Institute (AMI), known locally as simply “The Allan,” also carried a different moniker that, in retrospect, might seem to allude to its later macabre infamy - Ravenscrag. The mansion, located on the slopes of Mount Royal, a seven-minute walk from the Montreal Neurological Institute (MNI) - past the ornate elegance of the Royal Victoria Hospital and the more utilitarian Donner buildings of McGill University - had its own impressive history as Canada’s single most expensive private home. Built in 1863 by the Scottish shipping magnate and financier Sir Hugh Allan, the 72-room mansion reflected much of the economic history of Montreal itself. By the time of his death in 1882, Allan had built the largest private shipping empire in the world, and sat on the board of the Bank of Montreal, the capital of which had financed much of the Canada’s development during the nineteenth century. In 1940 Allan’s heir, Sir Montague Allan, donated Ravenscrag to the Royal Victoria Hospital, and in 1943 the building formed the institutional home for Canada’s first dedicated department of psychiatry, headed by the Scottish-born psychiatrist D. Ewen Cameron.

In its own way, Jasper’s address also reflected the history of Montreal, albeit in a very different respect; his talk was entitled “Neurology and Psychiatry: Two Solitudes?” The reference to Hugh McLennan’s 1945 novel Two Solitudes would certainly not have been lost on his audience. Often referred to as the great Canadian novel, Two Solitudes detailed the struggles of its protagonist, Paul Tallard, to navigate his twin identities as a French and English speaker in the bilingual (but linguistically segregated) city of Montreal. Ironically, while McLennan’s novel was meant to extol the

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1 Herbert Jasper Fonds, Box 1, Folder 11.
virtues of bilingualism and to create a sense of unity in Canadian culture, following the novel’s publication the phrase ‘two solitudes’ became a shorthand expression for the dysfunction of Canadian society; a nation divided by language, and incapable of communication and mutual understanding. It was with this pregnant metaphor, and against the backdrop of the political upheaval of Quebec’s Quiet Revolution, that Jasper chose to describe the relationship between neurology and psychiatry, and between the MNI and AMI.²

Jasper’s address began on a positive note, remarking that he and others at the MNI had followed the accomplishments of their “younger sister Institute” with a “sense of family pride.”³

Indeed, it was partly due to the efforts of the MNI founder Wilder Penfield that the Allan had been

² To non-Canadian readers, the Quiet Revolution, which has been mentioned briefly in other chapters, may require some explanation. The Quiet Revolution is generally thought to have begun following the death of Maurice Duplessis, the conservative Union Nationale premier of Quebec, in 1959. Duplessis’ death was followed by the Liberal government of Jean Lesage who, along with other modernizers, spent much of the 1960s shaking off what they saw as the backwardness of Quebec culture, characterized by the stronghold of the Catholic Church on the one hand, and foreign investors on the other. Lesage’s party campaigned on the slogan Maîtres chez nous (“Masters of our own house.”) The reforms of the Quiet Revolution represented perhaps the largest shift in social organization in Quebec since the conquest of New France by the English in 1759, and one of the most significant social transformations in Canadian history generally. Education was slowly secularized, and much of the province’s natural resources were nationalized, most notably the hydropower of the province’s hinterland. At the same time, while it repudiated much of the Catholic dogmatism that had been a cornerstone of French-Canadian culture for centuries, the Quiet Revolution emboldened a strain of linguistic nationalism that sought to assert its dominance over the direction of the province’s culture. French-language nationalists became increasingly vocal and assertive; this was nowhere more poignantly felt than in Montreal, which had always managed an uneasy truce between its English and French speakers. Emboldened by the visit of the French General Charles de Gaulle to Montreal for Expo 67, French separatists became increasingly aggressive. The separatist Parti Québécois formed in 1968. In 1970 a group of Marxist French sovereignists, the Front de liberation du Québec, kidnapped the British diplomat James Cross and murdered the Labour Minister Pierre Laporte, precipitating the so-called October Crisis, during which time Prime Minister Pierre Elliot Trudeau invoked the War Measures Act in order to declare a state of martial law in Montreal. The October Crisis is widely seen to have punctuated the close of the Quiet Revolution, which left the province of Quebec, and the city of Montreal, increasingly inward-looking and cut-off from external influences in Europe and North America. P. A. Linteau et al., Quebec Since 1930, trans. R. Chodos and E. Garmaise (Toronto: James Lorimer Limited, 1991), 307–599.

³ Ibid.
established, and that Cameron had been installed as its director. However, Jasper’s tone quickly
turned mournful:

In spite of our common interests in the higher functions of the brain….the development of the Allan Memorial Institute has been largely independent of the parallel growth of the Montreal Neurological Institute….At this time, over 20 years ago, there was a close association between the practice of neurology and psychiatry in Montreal. There were many who combined neurology and psychiatry in their practice and many psychiatrists were active members of the Montreal Neurological Society, attending its meetings regularly. Gradually over the years it has become rare indeed to see a psychiatrist attending the Montreal Neurological Society meetings. The growth of neurology and neurosurgery together has become separate from the phenomenal development of psychiatry as a medical specialty with its supporting scientific activities.4

After bemoaning the estrangement of the city’s psychiatrists and neurologists, Jasper continued, relating this broken relationship not only to broader trends in science and medicine, but also to political trends within Quebec itself:

During the past quarter-century the profession of neuropsychiatry has practically disappeared in all of the leading centers of the world. The Archives of Neurology and Psychiatry has disappeared, to be established as two separate publications, an Archives of Neurology and an Archives of Psychiatry. Thus the relative independence of the Allan Memorial Institute of Psychiatry is part of a larger picture of specialization in all branches of science and medicine, a trend which characterized the past quarter century. Neurology and Psychiatry have been developing, to a large degree, as “two solitudes.” However, during the past few years and, I would predict, in the next quarter century, this trend is to be reversed as there is a healthy scientific revolution taking place in our studies of the nervous system in relation to the mind and behaviour, just as there is a local revolution occurring in the relationships between French and English cultures in our Province.5

Indeed, just as the Quiet Revolution had begun to transform Quebec society, breaking the age-old hold of the Catholic Church and the Duplessis political machine on the province’s culture and politics, so too did Jasper see his previous decades of collaborative research at the MNI as signaling a renewed working relationship between the mind and brain sciences. Jasper went on to extoll the work of his MNI colleague Brenda Milner in investigating the role of different brain structures in

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4 Ibid.
5 Ibid.
memory (see Chapter 2), and suggested that a number of discoveries emerging from Donald Hebb’s psychology department at McGill University might have important psychiatric implications.  

Indeed, Jasper was generally positive about the relationship between neurophysiology and “neuropsychology, an area where traditional barriers between neuroanatomist, physiologist and psychologist have already become broken down by the convergence of specialists working on common problems.” Jasper attempted to find common ground with psychiatry, noting that the EEG, the tool with which he had built his professional reputation, had initially been developed to investigate the brain activity of psychiatric patients. As discussed in Chapter 3, Jasper had his own

6 The work to which Jasper alluded was the discovery of self-reinforcing brain stimulation by Hebb’s students, Peter Milner and James Olds. This work deserves a brief digression. Milner had been given a copy of Hebb’s *The Organization of Behavior* by his wife, Brenda Milner (see Chapter 2), and was inspired to leave his work at the Chalk River nuclear facility and retrain as a psychologist with Hebb. In 1953 Peter Milner joined forces with another Hebb student, James Olds. The pair adapted a form of electrical brain stimulation that had been developed by Jose Delgado, and used it experimentally in rats as part of their investigation of the brain stem reticular activating system. The pair noticed that, when a rat was stimulated by the implanted electrode, the rat would habitually return to the place where it had been during the stimulation. Using a series of Skinner box-based tests, the pair confirmed that the stimulation produced a reinforcing effect, and after sacrificing the animal, discovered that the electrode had not been placed in the structures of the reticular system, but had in fact been in the subcortical structures of the rhinencephalon and the hypothalamus and midline thalamus. This discovery was repeated and confirmed, and was publicized as the discovery of a ‘pleasure’ or ‘reward’ center in the brain. While Milner and Olds’ own evaluation of the discovery was more modest, the study received a great deal of press attention, and is often considered foundational to the study of addictive behavior. Jasper was likely alluding to this study because it suggested a strong link between experimental study of brain function and psychiatric disorders such as addiction and obsessive behavior. For more on this, see Peter M. Milner, “Peter M. Milner,” in *The History of Neuroscience in Autobiography*, ed. Larry R. Squire and Society for Neuroscience, vol. 8 (Washington DC; Society for Neuroscience, 2012); N. D. Campbell, *Discovering Addiction: The Science and Politics of Substance Abuse Research* (University of Michigan Press, 2007), 241n12; Henry J. de Haan, “Origins and Import of Reinforcing Self-Stimulation of the Brain,” *Journal of the History of the Neurosciences* 19, no. 1 (January 15, 2010): 24–32; James Olds and Peter Milner, “Positive Reinforcement Produced by Electrical Stimulation of Septal Area and Other Regions of Rat Brain,” *Journal of Comparative and Physiological Psychology* 47, no. 6 (1954): 419; James Olds, “Self-Stimulation of the Brain,” *Science*, New Series, 127, no. 3294 (February 14, 1958): 315–24.

7 Ibid.

8 Jasper even made mention of his MIT rival, F.O. Schmitt, whose recently established Neuroscience Research Program had announced that it would investigate issues related to the molecular coding of memory. Jasper observed that “psychiatrists are notably lacking in his team.” He was also dismissive of a number of other developments in the brain sciences, including the work of the Australian
reasons for hoping that the EEG would eventually lead to a new understanding of the brain in health and disease. While he concluded on a positive note, arguing that “we could all benefit by some interaction at high levels, though taking care to preserve our precious solitude to continue more intensive work on specialized problems,” the tone of Jasper’s address pointed to a division between neurology and psychiatry that was, in its own way, as deep and consequential as Montreal’s French-English divide.

We have seen in the previous chapters how Wilder Penfield’s plan for a multidisciplinary neurological institute was partially the result of his own biography, and partially the result of his interaction with a number of other historical developments, notably the Rockefeller Foundation’s program for psychobiology, and his interaction with the Spanish school of neurohistology (see Chapter 1). We have also seen how the unique circumstances of the MNI, including its position at the crossroads of a number of national scientific and medical cultures, made it an optimal location for the proliferation of new ‘neuro’ disciplines, including neuropsychology and neurochemistry; the MNI became a unique site that allowed for the trading of techniques and technologies, such as pathological anatomy, psychological testing, and the EEG (see Chapters 1, 2 and 3 respectively). Over the exposed brain of the surgical patient, a multidisciplinary vision of neuroscience developed that was different in content and style from other projects to integrate the mind and brain sciences in the twentieth century. Yet psychiatry was notably absent from the growing team of disciplines at the MNI. Why?

The absence of psychiatry is all the more striking when one considers the context in which the MNI was founded. As detailed in Chapter 1, the MNI was partially the product of the Rockefeller Foundation’s growing interest in ‘psychobiology.’ Historians have noted that neurophysiologist John Eccles, whose work on activities of synapses Jasper admired, but which he felt would be of limited applicability.
'psychobiology' was a largely optimistic turn in the brain and mind sciences that stressed the improvability of the human mind, and the treatability of insanity and mental disorder. Primarily associated with the work of the Swiss-American psychiatrist Adolf Meyer, psychobiology brought about a revolution in American psychiatry and the mind sciences. In a move that echoed the spirit of the Progressive Era, American psychiatrists abandoned the therapeutic pessimism of the nineteenth century, and embraced the potential of both somatic treatments for mental disorders (such as insulin coma therapy and electroconvulsive therapy) and the emerging ecological turn in psychiatry encapsulated by the mental hygiene movement (which sought the origins of mental health problems in the ‘maladjustment’ of an individual to his social surroundings). This embrace not only restored a certain faith in psychiatry’s professional mission, but also led to a search for new cures and treatments - notably psychosurgery. Indeed, by 1921 Meyer could report that ‘psychiatry today has largely found itself.’ Psychiatry’s cultural prestige was growing, thanks to the robust theoretical foundations that psychobiology had provided.

As Jack Pressman has shown, one of the crucial outgrowths of the rebirth of American psychiatry, enabled by psychobiology, was the development of psychosurgery and lobotomy. Alan Gregg, the Rockefeller Foundation officer who had financed the development of the MNI, and who was a great devotee of Meyer’s notion of psychobiology, also financed the development of John Fulton’s neurophysiology laboratory at Yale University, the lab from which much of the early experimental justification for lobotomy emerged (Fulton also became one of the procedure’s most


vocal advocates). Fulton’s laboratory at Yale was similar to Penfield’s initiative in many ways; like Penfield’s institute, it shared a “commitment to the collapse of disciplinary borders,” and Fulton’s laboratory of primate physiology also attracted international research fellows. Indeed, Fulton and Penfield’s efforts represented two of the most prominent spokes of the program enacted by Gregg in his efforts to embrace the psychobiological vision of Meyer.

Thus, the MNI was a product of the very same individuals and institutions that brought psychiatry into the medical mainstream, and psychosurgery into the heart of psychiatric practice. Moreover, the possibility of treating mental disorders surgically would likely have appealed to Penfield; given his expansive vision of the therapeutic value of neurosurgery, and his belief that radical surgery was a valuable treatment for certain neurological disorders such as epilepsy, psychiatry and psychosurgery ought to have been an area of great interest for the MNI. Penfield was in many ways a product of the very same trends in medicine that gave birth to psychosurgery; a friend of Adolf Meyer, a friend and protégé of Alan Gregg, a believer in the therapeutic potential of neurosurgery, and a believer in the importance of interdisciplinary collaboration. Penfield (and his institute) looked like the perfect candidate to undertake psychosurgery.

The story of the failed collaboration between Penfield and Cameron, and the subsequent breakdown in relations between the MNI and AMI, and between psychiatry and neurology in Montreal, reveals a great deal about the nature of successful interdisciplinary science and medicine. The preceding chapters have attempted to demonstrate how the MNI constituted a fertile site where different medical and laboratory disciplines could trade techniques, tools and theories to forge interdisciplinary alliances, and create new scientific subcultures under specific historical circumstances. In many ways, the MNI was an instantiation of Peter Galison’s notion of a ‘trading

zone,’ a place where different scientific cultures could become locally coordinated. Further, I have suggested that the assemblies of actors at this site became ‘wired together’ because of their synchronized, coordinated activity over time; they became a close-knit unit that was still able to incorporate productive new perspectives because of their weaker links to other communities and traditions. While this meant that the emergence of neuroscience was localized at the MNI, it was a loosened localization, akin to Hebb’s notion of a cell-assembly; able to form close associations through local coordination, yet productively connected to other areas.

Galison’s model has proven a productive way of understanding successful collaboration, and the forging of new scientific languages and subcultures. Yet, Galison’s model addresses only one half of the interdisciplinary equation; what can we learn from histories of failed collaboration? The story of the failed attempt to incorporate psychiatry into the MNI program, particularly in the area of psychosurgery, suggests that, just as there was no monocausal reason for successful collaboration, there were also a number of complex interacting historical factors that brought about the estrangement of neurology and psychiatry in Montreal - from the physical separation of their sites to the personalities of those involved. Most importantly, Cameron and his AMI failed to develop his initial weak tie to the MNI into a strong assembly; he failed to ‘fire in sync’ with Penfield and his collaborators, allowing them to coordinate their activity, and reach any kind of agreement. Cameron spurned the kind of deep collaboration and coordinated activity that Penfield embraced. A closer look at the relationship between Penfield, Cameron and their colleagues suggests that the lure of collaboration brought the main players back to the meeting table a great deal more often than has been commonly assumed. Finally, the estrangement of neurology and psychiatry in Montreal demonstrates the potential perils of Jasper’s ‘precious solitude’; estranged completely from the MNI following their failed collaboration, Cameron turned instead to his own attempts to innovate in psychiatry. These innovations would borrow from his Montreal colleagues, often in unsanctioned
ways. The fallout from these transdisciplinary borrowings would unleash one of the most profound scandals in the history of Montreal medicine.

**Meyer, Penfield and Psychobiology**

Penfield’s understanding of psychiatry was shaped most importantly by his association with Adolf Meyer, which dated back to his early medical education at Johns Hopkins. As early as 1916, a course in psychiatry led by Meyer prompted Penfield to write a letter to his mother which was meant to evaluate her overall influence on his life (“Certainly it has been the biggest factor.”)\(^{13}\) In his autobiography, Penfield spoke enthusiastically of an official meeting with Meyer in 1922 during a visit to Johns Hopkins: “Perhaps the most exciting event of the visit was my discussion of neurology and psychiatry with Adolf Meyer….His opinion at that time is interesting. There should be, he said, ‘a neuropsychiatric clinic with neurology and psychiatry on an equal footing.’”\(^ {14}\) Ten years later, Penfield also spoke positively of Meyer in connection to his own interdisciplinary vision, stating that:

I believed that psychiatry should be separate from neurology and neurosurgery in hospital practice for some years to come, but that all three should be joined together somehow in thought and plan, in the laboratory for scientific study, and in teaching - recalling my conversation with Adolf Meyer at Johns Hopkins ten years back.\(^ {15}\)

Moreover, Meyer’s early training as a pathologist likely endeared the older man to Penfield, given his own emphasis on the importance of the pathological laboratory.\(^ {16}\) Indeed, the visit to Johns Hopkins


\(^{15}\) Penfield, 296.

\(^{16}\) Meyer had initially studied under August Forel in Zurich, and trained in pathology and neurology. It was his training as a pathologist that brought Meyer to the United States in 1892, where he worked at Illinois’ Kankakee Hospital. It was not until 1895, following an episode of depression suffered by his mother, that Meyer became interested in psychiatry. Despite this turn to psychiatry, Meyer still conducted major neurological research throughout the 1910s. Elliot S. Valenstein, *Great and Desperate Cures: The Rise and Decline of Psychsurgery and Other Radical Treatments for Mental Illness* (New York: Basic Books, 1986), 15; S. D. Lamb, *Pathologist of the Mind: Adolf Meyer and the Origins of American Psychiatry* (Baltimore: Johns Hopkins University Press, 2014).
in 1922 had been prompted by a job offer for Penfield to become an associate of the neurology department, so that he might carry on the basic research work with animals that he had begun in Charles’ Sherrington’s laboratory in England. According to Penfield, the plan that Meyer and Lewis Weed unfolded before him was “the very thing I had dreamed of during my years in England,” and “[Weed] had pathological material that could, I realized, be converted into a gold mine of research.”

It was only the lack of cooperation with the department of neurosurgery, headed by Walter Dandy, that stopped Penfield from taking the job in Baltimore (Dandy had not wanted to “cut across departmental lines and thus create a new enlarged department”). Reflecting on his second encounter with Meyer, Penfield added in his memoirs that:

I had been his student and had discovered that his own approach to psychiatry had included studies of the anatomy and pathology of the brain. I, myself, had yet to complete that approach. Would I ever meet a psychiatrist like him when I was able to do my part? I wondered.

Beyond their admiration for each other, Meyer and Penfield’s relationship developed at a propitious time. Meyer’s leadership had begun to transform American psychiatry. Meyer’s position of leadership gave great credence to his later blessing of psychosurgery and lobotomy as a promising therapy for the nation’s mentally ill. Simultaneously, the embrace of Meyer’s psychobiological perspective by the Rockefeller Foundation, and particularly its director of Medical Sciences, Allan Gregg, provided the connective tissue between Meyer’s project and Penfield’s establishment of the MNI.

Perhaps most importantly, Meyer’s growing prestige, and his openness to somatic forms of therapy, meant that Penfield valued his opinion very highly on all matters psychiatric.

In a bad way for a psychiatrist...

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18 Penfield, 75.
19 Penfield, 75.
Penfield had spoken positively of the possible association of psychiatry and neurosurgery in his 1928 report to the Rockefeller Foundation (see Chapter 1 and Appendix 1), noting that, “In the ideal neurological clinic which could include surgery and adequate histopathology, what shall be the boundary line between it and psychiatry? Obviously, there can be no hard and fast line. The two specialties must overlap on the milder functional cases and they must both interest themselves in the study of the brain.” Moreover, Penfield felt that the pathological laboratory would be the most common point of overlap between the two specialties, a position that would likely have appealed to Meyer, whose initial training had been in neuropathology.

Penfield extended an invitation to Meyer to attend the MNI’s opening in 1934, noting that “you understand the aims and ideals which have led to the opening of this Institute, and we would like to have your blessing at this time.” The pair were clearly capable of discussing brain surgery and its psychological consequences at some depth; for example, Meyer sought Penfield’s opinion about a patient with unexplained shoulder shrugging, and Penfield offered his opinions on the difficulty of testing for the psychological consequences of certain brain removals.

Meanwhile, the development of a department of psychiatry at McGill had begun, very much with Penfield’s assistance and approval. By 1941, as the war in Europe was in full swing, internal

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21 Penfield went on to say: “Give to psychiatry neurohistology and the means of mental therapy and to neurology neuropathology and complete facility for surgical therapy. Let them be as closely associated with medicine and surgery, as is possible in hospital life, but allow them complete facility for experimental and therapeutic study of their own problems. Then we can hope for solution of some of the riddles presented by the sufferers from nervous and mental diseases.” Wilder Penfield, “Impressions of Neurology, Neurosurgery, and Neurohistology in Central Europe.” W/U 17, Box 155a, Wilder Penfield Fonds.

22 Lamb, Pathologist of the Mind, 26–58.

23 Wilder Penfield Fonds, Box 24, Folder C/D 2-1. Penfield to Meyer, 23 July 1934. Meyer was unable to attend, but responded that “you have more than my blessing. It is a great satisfaction that you are enabled to create this unique center for basic work.” Meyer to Penfield, 20 Aug 1934.

24 Meyer later noted that Penfield’s excisions of certain parietal ‘association areas’ called for greater discussion within a psychobiological framework, while Penfield responded that it was at present too difficult to assess the effect of such excision on the personality of the patient. Penfield Fonds, Box 24, Folder C/D 2-1. Meyer to Penfield 2 January 1933, Penfield to Meyer 14 January 1935, Meyer to Penfield 16 January 1935.
memoranda began to circulate between Penfield, the president of McGill F. Cyril James, and J. C. Meakins, the dean of medicine at the Royal Victoria Hospital, about the possibility of establishing a new department. “This war has increased the need for trained psychiatrists as well as for treatment facilities in the field,” wrote James in an internal memorandum, and added that Ravescrag might be an ideal site for such a facility, since it was situated in such a way as to allow for easy interaction with the rest of McGill. One possible area of conflict, however, might be over the distribution of patients between the new department of psychiatry, and Montreal’s existing mental hospital, the Verdun Protestant Hospital. Founded in 1881 as the Protestant Hospital for the Insane, the renamed Verdun hospital (later renamed again as the Douglas Hospital in 1965), had handled most of Montreal’s psychiatric patients since its inception, and sat then on the city’s outskirts, unaffiliated with McGill. Concern about a possible turf war led James to suggest that the new psychiatry institute might conduct evaluations and treatments, while Verdun would be reserved for incurable and chronic cases of psychosis - a custodial institution to contrast with the cutting-edge institute he hoped to build.25

For his part, Penfield followed this discussion closely, and was adamant that any new psychiatric institute should be “immediately adjacent” to his neurological institute, adding that:

In my opinion the cause of many of the psychoses will be discovered along biochemical lines. When these advances are made there will develop a gradual identity between neurology and psychiatry inasmuch as psychiatric conditions will then be recognized as diseases of the brain which may be treated directly. When that time comes, neurology and neurosurgery may well be fused with psychiatry.26

Penfield regularly repeated the argument that psychiatry would best advance by a close relationship with neurology, and that the best way to ensure this relationship would be to use the laboratory as a common meeting ground. In addition to creating an interdisciplinary environment, this would have

26 Penfield to J.C. Meakins, 12 June 1940. WP Fonds, Box 4, Folder A-M 10-1 McGill Misc 1941-44 Psychiatry.
the added benefit of helping to foot the bill; Penfield noted that the Rockefeller Foundation would be unlikely to provide funding for a mere clinical establishment such as the Verdun Hospital, but might be willing to provide funding on a large scale if they could be convinced that the project would have value for research.27 Again and again, in letters and internal memoranda, Penfield stressed the importance of proximity to the neurological institute as being crucial for the success of any psychiatric undertaking. In a 1942 letter to J.R. Fraser (McGill’s Dean of Medicine), Penfield argued that development of a facility for psychiatry at Ravenscrag would be desirable “only if it is considered to be a temporary scheme,” a placeholder until a “complete modern psychiatric facility can be built,” preferably on University Avenue, above the MNI (even the seven-minute walk to Ravenscrag was too much). Such proximity would allow for collaboration on interesting cases, and the ability to share laboratory resources. Notably, Penfield suggested that, while he could not necessarily make his own EEG lab available, Herbert Jasper was more than willing to help construct a separate EEG lab. Penfield also noted that, in addition to laboratory resources, the MNI might be able to assist in “rare cases” where surgery was necessary, adding parenthetically that:

(In many institutions certain operations are carried out upon the brain to combat mental changes and this has been called psychosurgery. I, for one, am not thoroughly convinced that this has a permanent place, but there is definitely an occasional case who should have an operation upon his frontal lobe. Such operations could only be carried out in the Neurological Institute.)28

Penfield closed this letter to Fraser noting that “The closest cooperation must depend, in the last analysis, on proximity,”29 and the archival record on the establishment of McGill’s psychiatry department clearly indicates that Penfield hoped for a continued relationship with the new institution.

27 Penfield to Meakins, 15 November 1940.
28 Penfield to Fraser, 5 November 1942.
29 Penfield to Fraser, 5 November 1942.
Penfield was closely involved in selecting the man who would run this new institution, although here his opinions were less adamant, and more open to suggestion. Penfield’s hunt for a psychiatrist for McGill began at least as early as 1937, when he wrote to his colleague at Johns Hopkins, the neurosurgeon Eldridge Campbell, that “we are in a bad way for a psychiatrist…. “30 However, the major contribution to the selection of a head of psychiatry for McGill was made by Penfield’s old friend, Adolf Meyer. Indeed, the two men had been in communication about the development of psychiatry at McGill since at least as early as 1938, when a less-than-cordial run-in between Herbert Jasper and Meyer prompted a franker communication from Penfield:

I am writing…in order to clear up a misconception which you evidently have concerning our plans for Psychiatry here at McGill. You spoke to [Jasper] as though we wanted an assistant to neurosurgery. I should like to have you of all people understand my point of view. There has never at any time been any desire to make Psychiatry subordinate to Neurosurgery, nor to Neurology either. I don’t know where you could have gotten the idea that we intend any such thing. No one is more aware of our deficiency in Psychiatry here at McGill. The failure to develop a department has been entirely due to lack of funds. I am assisting Dean Meakins in his plans for the development of such a department….My own desire is to see a Department of Psychiatry developed here, quite independent from a clinical and academic point of view. However, I should like it very much if we can have a Professor of Psychiatry who will cooperate with our Department and who will recognize that we have a common interest in neuropathology and neuroanatomy and who can talk our language as well as his own….You will remember I spoke to you about our desire to secure a good man at McGill two years ago. I wonder if you would be good enough to write to me or to Dean Meakins with any suggestions that you might have for a Professor of Psychiatry. He should be young; he must have some knowledge of basic sciences, either the biochemistry of the nervous system, or the anatomy of the neuropathology.31

For Penfield, the desire to have a competent psychiatrist who could ‘talk the same language’ as the surgeons and neurologists at the MNI was reason enough to seek Meyer’s advice, which he readily gave. Meyer responded that a ‘native’ Canadian psychiatrist would be preferable to an American import, and that he ought to have a good all-round training in psychiatry, rather than an

30 Penfield to Eldridge H Campbell, 3 September 1937. WP Fonds, Box 54, Folder c-G 37 C.
‘overspecialization’ in psychoanalysis. Meyer ended his letter by speculating: “I wonder about Ewen Cameron who has been in Canada.”

Cameron

D. Ewen Cameron certainly had the pedigree to be considered to head a new, experimental psychiatric institute. Born in Stirlingshire, Scotland in 1901, Cameron’s prodigious medical training had taken him from the University of Glasgow to Meyer’s Phipps Clinic at Johns Hopkins, to Switzerland to work with Eugen Bleuler. In 1929 Cameron moved to Canada to serve as the primary psychiatrist for the entire province of Manitoba, where he established 10 clinics. He then moved to Massachusetts in 1936, and New York in 1938, where he became psychiatrist-in-chief at Albany Hospital. In the early 1940s he was approached by McGill about the possibility of heading the new psychiatric enterprise in Montreal. His publication record in clinical research was already impressive, and included a monograph entitled *Objective and Experimental Psychiatry* (1935) that advocated for a relentless experimental empiricism in psychiatric research. Indeed, as Rebecca Lemov has noted, Cameron’s psychiatric perspective combined an emerging behavioral orientation with a ‘technophilic’ search for a way to automate psychotherapy. Cameron had little patience for the subtleties of psychoanalysis, although as will be seen below, he hoped to employ a form of talk therapy. But his intention was not to treat mental disorder through the slow and laborious methods of psychoanalysis; rather, he hoped to cure mental disease through the application of technology to psychiatric practice.

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32 Meyer to Penfield, 8 January 1942. WP Fonds, Box 24, Folder C/D 2-1 Meyer.
Despite Meyer’s endorsement, Cameron was not Penfield’s first choice to head the new institute. A 1942 document listing potential psychiatrists (unsigned, but almost certainly authored by Penfield) notes that Cameron’s publication list was ‘impressive’ and his background ‘excellent,’ but added that he would like to “get Jasper’s opinion on his electroencephalographic work,” along with the opinions of other psychiatrists. Penfield’s preference was for the Yale psychiatrist Edwin Francis Gildea, who fell more in line with his interests: “Here is the real leader in the biochemical approach to psychiatry. This is a man who might build a school. I know him. He is sincere, genuine. Unless he has some weakness I do not know about, this is the man the Rockefeller Foundation might back.”

However, Gildea’s chances were likely torpedoed in a 1943 meeting of interested parties, during which time an MNI colleague and former roommate of Gildea intervened on Cameron’s behalf. At the conclusion of the meeting, it was agreed that Alan Gregg and Meyer should be approached about Cameron, and a formal offer was extended in 1943.

Cameron arrived in Montreal to begin building the Allan into a premier research clinic in September of 1943, but did not have his first formal meeting with Penfield about possible collaboration between their institutes until March of 1944. The meeting occurred in Penfield’s office on the fourth floor of the MNI. Although no record of the meeting itself survives, Penfield felt compelled to follow up the meeting with a letter to Cameron that is remarkable in its candor, and worth quoting at some length. Penfield stated that:

Following our discussion of the interrelationship of your department and mine, I am writing you in the hope of making clear our attitude before you came and now….I regret your conclusion that you can develop your department best quite independent of ours, and I feel

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37 The colleague in this case was the consulting neurologist Norman Peterson. Evidently Petersen, who had been Gildea’s roommate in college, implied that Gildea “is not perhaps as original in his chemical work” as Penfield had been led to believe. Wilder Penfield, “Discussion of the List of Psychiatrists – 22 March 1943.” WP Fonds, Box 4, Folder A-M 10-1 McGill Misc 1941-44 Psychiatry.
that your simile of the twins ‘in utero’, one of which must be weak because the other uses up the nourishment, does not apply. On the contrary, if a satisfactory relationship is established each of the twins should be able to help the other secure nourishment in the form of money and men and add strength in other ways.

It had always been my hope to see McGill develop a strong Department of Psychiatry, and I have refused the suggestion of a combined department with one head. I have always known that our own work would be better if we had cooperation…. I hoped that some members of the new department would discuss with us brain anatomy, neuropathology or biochemistry, and that in some laboratories we would meet and carry out common study and research.

I realize now that you do not care to touch any of these subjects except neurophysiology…. I realize that you feel the neurophysiology you will undertake is quite specialized. Our laboratories on the 7th floor of the M.N.I. are nevertheless open to you while you are developing your own.

You know psychiatry; you are our elected authority here, and we accept your decision to go it largely alone, although I am personally disappointed. I can assure you, however, that any time in the future we will gladly establish the cooperation you consider unwise now.

I am pleased that you have decided to use our department of electroencephalography. Dr. Jasper will report your cases as he does ours so far as his time and space permit, and this may lead to study of some common problems.

Psychiatrists in Montreal, without being organized in a university department, have attended our Wednesday meetings, and contributed a good deal to them, as well as in occasional contact. I hope the current of the new organization will not lessen this. Our neurologists in public service and out-patient department will consider psychoses to lie in the field of your department, in spite of the fact of their previous interest and training in the field. Neuroses they will consider a common problem, and I should hope, as time passes, that the majority of the neurotics will be referred to those men, in either department, who get the best results.

For the sake of record now I would like to express my belief that any Department of Psychiatry which loses contact with those who work primarily with brain lesions is weaker for that lack, and, conversely, any Department of Neurology (in which I include Neurosurgery) which has not close contact with those who work in the field of psychiatry is the weaker for that lack.

The time will come when neurologists will find in the brain the cause for many psychiatric disturbances, and psychiatrists will likewise find in that organ some of the things they are looking for. Thus, looking into the future, their fields must eventually merge to some extent, and far-seeing academic organization should provide for it.

Good luck to you. These things may work themselves out gradually, perhaps after you and I have handed on the professorships to our successors.\(^{38}\)

Cameron had, by Penfield’s account, rebuffed his offer to collaborate, particularly in a common laboratory setting, and had made the decision to ‘go it alone’ at Ravenscrag with relatively little input from the MNI. Penfield’s ecumenical tone regarding psychiatry and neurology provoked no

\(^{38}\) Penfield to D. Ewen Cameron, 20 March 1944, WP Fonds, Box 4, Folder A/M 10-1 McGill Misc 1941-44 Psychiatry.
sympathy, and his offer to share laboratory resources produced only a vague reply from Cameron that cooperation was “desirable, as of course it is with respect to most of the clinical departments…and indeed with respect to those other departments…which are primarily concerned with human behavior.”

Penfield sent a carbon copy of his letter to J.R. Fraser, dean of the McGill Medical Faculty, noting that “I give up, of course, my cherished hope of a Psychiatric Institute built close to the Neurological Institute and the combination of associated work. This may come at McGill, but it will not come in my lifetime.”

Despite all of the contributing factors that should have made collaboration between Cameron and Penfield inevitable, their relationship was dead on arrival; or so it might seem, until the issue of psychosurgery entered the picture.

Psychosurgery at the MNI

Despite Penfield’s disappointment that Cameron would not be joining the MNI’s interdisciplinary team, his offer to assist and collaborate would lead, within the year, to a series of seven psychosurgeries at the MNI - in fact, the only psychosurgeries that Penfield ever performed. Penfield’s acquaintance with psychosurgery, and his anxieties about it, extended to the beginnings of the procedure in 1935. Following his surgery on his sister in 1928, Penfield became increasingly concerned about the possible effects of frontal lobe extirpation on cognitive ability. As detailed in Chapter 2, these concerns ultimately led to the hiring of the psychologists D.O. Hebb and Molly Harrower, who instituted a regime of psychological testing for surgical patients. However, Penfield’s surgery on his sister would have another life, not as a cautionary tale, but as a spur to lobotomy itself. In 1935 Penfield presented his sister’s case, along with several other examples of frontal lobe extirpation, at the Second International Neurological Conference in London, the same conference at

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39 Cameron to Penfield, 27 March 1944, WP Fonds, Box 4, Folder A/M 10-1 McGill Misc 1941-44 Psychiatry.
40 Penfield to Fraser, 30 March 1944, WP Fonds, Box 1, Folder A/M 2/2-4.
which Egas Moniz received his inspiration for the ‘leukotomy’ from the presentation of Carlyle Jacobsen and John Fulton on their experimental neuroses work.\textsuperscript{41} While Walter Freeman, the American neurologist who adapted Moniz’s procedure into the standard and transorbital lobotomy, was inspired by Moniz’s leukotomy procedure, he was also in the audience for Penfield’s presentation, and made a connection between the two. Writing to Penfield in 1936, Freeman noted that:

\begin{quote}
I remember listening to [your] presentation which was the high light to me of the Association meeting that year, and the idea that such extensive removal of the frontal lobe could be accomplished without serious intellectual deficit must have sunk in because when the report of [Egas] Moniz’s work [on leukotomy] came to me it seemed that there must be something to it.\textsuperscript{42}
\end{quote}

It seems that Penfield and Freemen, then, took different lessons from this dramatic family affair. Penfield followed the development of lobotomy from afar, but was highly skeptical about its prospects, noting to a colleague that while he had been “filled with astonishment” regarding the initial reports of the Freeman and Watts operations, he had been unwilling to undertake the procedure himself. Penfield added that he felt that Freeman and Watts would produce epilepsy in at least half of their cases, and that “they have applied it to certain cases which were not hopeless. However, I have no right to speak with any authority…I do know that a large frontal operation sometimes makes people more placid. Perhaps many of us would be better off if we were converted into nit-wits by some such procedure.”\textsuperscript{43}

Penfield’s skepticism about psychosurgery also took a more active form. In September of 1937, Penfield wrote an anonymous editorial for the \textit{Archives of Neurology and Psychiatry}, which he forwarded to his close friend, the Harvard-based neuropsychiatrist Stanley Cobb. The editorial (which appears never to have been published), was entitled “Experimentation in Clinical Medicine,”

\footnotesize
\begin{enumerate}
\item Walter Freeman to Penfield, 7 December 1936. WP Fonds, Box 53, Folder C/G 36 D.
\item Penfield to F. Colla, 6 May 1940. WP Fonds, Box 55, Folder C/G 40 G.
\end{enumerate}
and lambasted psychosurgery as a “bastard term” for a procedure which seemed to overstep the “bounds of science and wisdom.” Penfield went on to note that “lacerating wounds of the brain often produce epilepsy” (a subject he was certainly qualified to pronounce on), and wondered “how many of these patients will become chronic epileptics in the next five years?” Penfield closed by stating that “When mental disease is to be attacked by such treatments….it seems proper to expect preliminary study of laboratory animals and a reasoned, constructive hypothesis,” and that “patients should be observed long enough to be sure of possible harmful late-effects,” a statement that echoed his own extensive observation of his sister’s operation.44

In the interim period between the discovery by Moniz and the arrival of Cameron to Montreal, Penfield undertook some of his most important studies of patients with extensive cortical removals. These included mapping of the motor and sensory strips of the brain, and publications that made important contributions to physiological psychology. By the late 1930s, Penfield had begun to experiment with removals of portions of the frontal lobes in cases of epilepsy, despite his misgivings about possible cognitive deficits. It was his concern for the possible cognitive deficits of frontal lobe extirpation that led to the crucial study with Hebb of the patient K.M. in 1939/40 (see Chapter 2). The study in question, “Human Behavior After Extensive Bilateral Removal From The Frontal Lobes,” reported on the case of K.M., a sawmill worker who had been struck on the head by a log carrier in 1928, and had developed post-traumatic epilepsy as a result.45 Extensive bilateral removal of K.M.’s frontal lobes produced the peculiar finding of an increase in his IQ scores, rather than a decrease. These findings had several consequences, both theoretical and practical. For Penfield, the study confirmed his idea of ‘nociferous cortex’ - the notion that damaged brain tissue

44 The editorial also expressed skepticism about metrazol convulsion therapy and insulin coma therapy. All quotations from Wilder Penfield, “Experimentation in Clinical Medicine,” WP Fonds, Box 29 C/D 14-6.
might be more detrimental to mental function than absence of tissue. For Hebb, the study with K.M. led to a profound break with his mentor Karl Lashley, and a subsequent reinterpretation of the concept of ‘intelligence’ as measured by existing tests. For both men, the experience with K.M. led to a reinterpretation of the notion of ‘frontal lobe signs,’ as postulated by the German neurologist Kurt Goldstein, and a reconfirmation of the value of pre- and post-operative psychological testing.

The results of the study with K.M. also did not escape the notice of Walter Freeman, then the most vocal proponent and practitioner of lobotomy in America. While preparing their paper on K.M. for publication, Hebb and Penfield received a request from Freeman to make use of their research findings and images, in order to “liven up” his forthcoming Psychosurgery (1942). Hebb consulted with Penfield, who confided that “I am not very keen about Freeman claiming too much about our paper and I should hate to see him use any of our illustrations. On the other hand, we do not want to insult him, but if you could find it possible not to have any spare copies of the illustrations…I should do so.”

Two days later, Hebb dispatched a letter to Freeman containing the agreed-upon excuse, adding in a note to Penfield that “I think it sounds enough like innocent ignorance that he can’t take offense.” Penfield’s attempt to conceal his research findings from...

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47 Hebb; Donald O. Hebb, “Man’s Frontal Lobes: A Critical Review,” Archives of Neurology & Psychiatry 54, no. 1 (1945): 10–24. On the issue of the latter paper, Hebb wrote to Penfield, stating that “Goldstein has recently published a paper on frontal lobes, attacking the psychological part of our paper on Ken Matthews. This is only one of several recent papers on frontal lobe localization which are, I believe, seriously off the beam. Goldstein, in particular, hasn’t a leg to stand on. It seems clearer than ever that your scar extirpation cases give practically the only reliable evidence concerning some points of localization of function.” Hebb to Penfield, 26 May 1944, Folder 0000-2364.01.6, DO Hebb Fonds.
48 Wilder Penfield to Hebb, 16 May 1940, Wilder Penfield Fonds, Box 141, Folder W/P 109.
49 Pressman notes that Penfield sent a congratulatory note to Freeman on the publication of Psychosurgery in 1942, saying “It is beautifully and thoughtfully done. It will prove to be a building stone in a structure of therapy in a field where little therapy has stood the test of time.” Yet the behind-the-scenes story suggests that Penfield’s note was largely a professional courtesy, and his attempts to undercut Freeman behind closed doors suggests that he did what he could, within the
Freeman suggests his trepidation about lobotomy, along with his growing confidence in his collaboration with psychologists in assessing the mental effects of brain surgery.⁵⁰

Despite Penfield’s attempt to prevent his study with Hebb from lending support for psychosurgery, this caution in interpreting their frontal lobe work was not shared by their readers. In late 1940, Penfield received a copy of his friend Stanley Cobb’s review of “Functions of the Frontal Areas of the Human Brain,” for the *Archives of Internal Medicine*, which quoted from Penfield and Hebb’s paper on K.M. According to Penfield, “I wept when I saw your quotation of what we said about the evidence of [intelligence effects of frontal lobe operations].” Penfield went on:

I immediately looked up the manuscript to see if we had really said that and found that we had. The implication, of course, is that we conclude that such frontal removals had no effect upon intelligence or personality. Certainly, taking the sentence out from its context gives that impression. You are quite right to criticize it. We should have said there what we said in the conclusion, namely, “It is concluded that bilateral removal of a third of both frontal lobes uncomplicated by pathological change in the rest of the brain, need have no grossly deteriorating effect.” What I meant to point out was that the methods which we used for psychological study showed no change. What I believe is that these methods are inadequate and it may well be that if [the] Rorschach had been [used] earlier in this case it would have given us a better picture. I also believe that the observations that I was able to make in my sister’s case are more accurate and more to the point than the accepted psychological tests would have been in her case. I believe that in that case, as in the cases of large single frontal removal that Evans and I studied, there was “impairment of those mental processes which are prerequisite to planned initiative.”⁵¹

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⁵⁰ Penfield and Hebb were also cited, separately, by Cameron in his 1941 revision to his *Objective and Experimental Psychiatry*. Of Hebb’s 1939 study of left frontal lobe removals, Cameron concluded that Hebb found no loss of intelligence, and that “intelligence levels were above normal.” Hebb’s actual conclusions were far more subtle, and related mainly to lack of classical ‘frontal lobe signs.’ About Penfield and Evans’s 1934 paper on Penfield’s sister, Cameron noted the possible effect on planned initiative, but qualified their judgment by noting that their continued to be disagreement in the field. Indeed, Cameron seemed most concerned with not concluding from any study that memory or other ‘higher functions’ could be localized. “It is still necessary to mention at least provisionally the concept of mass action even in higher animals.” Cameron, *Objective and Experimental Psychiatry*, 335–36; Wilder Penfield and Joseph Evans, “Functional Defects Produced by Cerebral Lobectomies,” *Proceedings. Association for Research in Nervous and Mental Diseases* 13 (1934): 352–77; D. O. Hebb, “Intelligence in Man after Large Removals of Cerebral Tissue: Report of Four Left Frontal Lobe Cases,” *The Journal of General Psychology* 21, no. 1 (1939): 73–87.

⁵¹ Penfield to Stanley Cobb, 3 October 1940, WP Fonds, C/D 14-6, Box 29.
That Penfield's own findings could be used as a spur to lobotomy was deeply disturbing to him. And yet, on the very next page of the same letter, Penfield seemed to reverse himself:

I liked very much your discussion of the lobotomies. I have had a number of doubts recently, thinking perhaps that I was too conservative, and wondering if there was not some way of making selective frontal removals or frontal incisions that would not give rise to important defects but that would benefit the psychotic patient. I have had in mind the possibility of going down to see Freeman and humble myself before him so as to see his results.52

Put simply, the razor-thin edge for interpreting the psychological consequences of brain operations in 1940 led Penfield to lose faith in his doubts. While he might abhor the blind cutting and brain mutilation that went along with the standard lobotomy (the transorbital, or ‘ice pick’ lobotomy had not yet been invented), the possibility of finding a rational replacement for the operation, one that might be of value to the patient with minimal loss, clearly appealed to Penfield’s own desire to expand the scope of neurosurgery through rational, scientific intervention. Thus, by the time of Cameron’s arrival in Montreal, Penfield and his colleagues had assumed a guarded stance towards psychosurgery. Penfield’s statement to Fraser in 1942, that he was not convinced of a place for psychosurgery, but that under certain conditions he might be willing to undertake it in collaboration with the as-yet-unnamed head of the new psychiatric institute, takes on a new significance. For Penfield, the desire to collaborate and forge new ground for neurosurgery, combined with the ambivalence of the psychological results, overcame his initial objections. Writing to George Reed at the Verdun Hospital in October of 1944, Penfield noted that he could see no reason why some lobotomies should not be carried out there “on the bad cases for which it seems to be sometimes quite suitable.” Penfield added that he had begun a series of psychosurgeries with Cameron, noting that “I would be glad to do a series of these patients; it might be only a trickle, but this is a type of operation that I am very anxious to tackle, and after the war we could perhaps swell the trickle into a

52 Ibid.
larger stream.” Despite their initial frost interaction, Penfield’s interest in collaboration, and his waffling on psychosurgery, kept the door open for Cameron.

The archival record is unclear as to who made the first approach concerning the AMI/MNI collaboration on psychosurgery, but within a year of Cameron’s arrival, the project had taken form: Cameron selected seven patients who were candidates for lobotomy and conducted the psychiatric evaluation. Penfield performed an operation he called a bilateral frontal gyrectomy (removal of gyri), which he hoped would “have the same therapeutic effect as [a lobotomy], and yet preserve the function of more of the frontal cortex.” Notably, Penfield planned the operations so as to minimize the possible appearance of post-operative epilepsy, a concern he had voiced about lobotomy from its inception. In the Spring of 1944, Penfield carried out his first gyrectomy on a 23-year-old man named H.M. (not the same as the H.M. – Henry Molaison – from Chapter 2) who had suffered for years from an “inability to concentrate and to work.” He had previously received 15 treatments of electro-convulsive therapy without benefit, and had attempted suicide at least once. The diagnosis was “chronic anxiety neurosis with feelings of unreality.” Penfield performed the operation in May, and later extended his removal of frontal lobe material in July. After H.M. recovered from the operation, “his depression…disappeared and he [became] president of the local radical political club in which he takes a great interest.” The rest of the patients, who had similar diagnoses, did not fare as well. Penfield, Cameron and their colleague Robert Malmo (who conducted post-operative

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53 Penfield to George E. Reed, 4 October 1944, WP Fonds, Fox 58, C/G 44 R.
54 Henry Molaison had yet to be operated upon in Connecticut by William Scoville.
55 Specifically, "Gyri were ablated on both sides along fissure lines under local anesthesia....The removed area began just above the roof of the orbit and extended back along the midline 7 cm. on either side. The removal was carried down to, but did not include, the cingulate gyrus in the median longitudinal fissure. The attempt was made to ablate all of area 10, according to Brodmann, completely on both sides." In the second operation, "The removal was extended laterally across each hemisphere to the frontal operculum, stopping just short of what could be called speech area on either side.'
Penfield concluded that “Bilateral frontal gyrectomy is a difficult, long, and somewhat dangerous procedure. The therapeutic results have been variable. This operation is not proposed at the present time as an acceptable substitute for ‘leucotomy’ or ‘frontal lobotomy.’” Cameron, for his part, came to a slightly different, and more hopeful conclusion: “this operation clearly is one which has no greater value than the lobotomy…However, we may just as clearly state that it is reasonable to explore this new field of surgery and psychiatry through further modifications of this and other operations.”

Penfield and Cameron published their results as two separate papers, and events behind the scenes suggest a profound breakdown of the already limited communication between the MNI and AMI teams. Penfield had been clamoring to add a psychologist to the gyrectomy project in order to carry on the tradition of pre- and post-operative intelligence tests inaugurated by Hebb and Harrower. Indeed, Penfield consulted Hebb on the possible psychological ramifications of psychosurgery from a distance, as Hebb had left Canada to join the Yerkes Primate Lab in 1942 (and Harrower had left the MNI in 1941). After consultation with a number of people at Yale, Penfield

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59 Cameron and Prados, “Bilateral Frontal Gyrectomy; Psychiatric Results.,” 537.
60 Penfield wrote to Cameron in October of 1944 that “The study of the clinical aspects of these patients certainly lies in your field, and I would be delighted if you would establish a routine pre- and post-operative examination.” Penfield to Cameron, 15 October 1944, WP Fonds, Folder C/G 44 C.
61 Penfield had even suggested the possibility of bringing Hebb back to the MNI in order to consult on the gyrectomy operations. In the summer of 1944, Penfield suggested this arrangement to Hebb, who only demurred because of the difficulty of travel. Penfield later agreed, noting that “It seems to me, however, that the work on them should be done by a psychologist here on the ground who is able to follow them along at different intervals and make both pre- and post-operative studies.” Hebb followed the gyrectomy cases from a distance, requesting Jasper’s EEG recordings of the at least one patient, and noting that the combination of careful EEG and psychological study of the
and Cameron selected Robert Malmo, a recently graduated psychologist who was engaged in war research at the NIH laboratory at Bethesda. Malmo accepted an invitation in February 1945 to present his research on the frontal lobes of monkeys at the AMI before formally joining the AMI staff. According to Malmo:

> On this brief visit, I had no time to find out in detail what had been done on the gyrectomy project, although I was told that several gyrectomies had already been performed. I naturally assumed that they would not have proceeded without thorough preoperative psychological testing. You can imagine my astonishment and deep disappointment in finding on arrival for work in July 1945 that 4 of 6 gyrectomies already operated [upon] had had no preoperative testing, and that the preoperative testing of the other two…by Mrs. B. Wickett of the Mental Hygiene Institute was meagre (only Wechsler Intelligence Scales, and incomplete ones at that).

62 Malmo’s shock that no pre-operative testing had been done on the gyrectomy candidates did not stop him from adding his own contribution to the report of Penfield and Cameron’s results in 1948, but Malmo insisted on clarifying his role nearly fifty years later:

> Despite the handicaps that I inherited when I took over this study (flawed by their failure to take preoperative data) I believe that I presented enough positive findings [against gyrectomy]…to raise strong doubts about the wisdom of prescribing psychosurgery. I believe that our findings, along with the poor clinical results, were instrumental in terminating psychosurgery for AMI patients.63

Meanwhile, Penfield attempted to interpret the results of the gyrectomy surgeries in advance of presenting them at the 1948 meeting of the Association for Research on Nervous and Mental Disease (ARNMD). Privately, he confided to John Fulton that he was having difficulty: “I couldn’t seem to send [the paper] off because of the feeling of responsibility for the interpretation of the results….The cases have made me do a certain amount of thinking, and in my case thinking takes time!”64 At least part of Penfield’s thinking involved a concern for the patient’s own appraisal of the operation. While Cameron seemed content with an evaluation of the patient’s ‘social adjustment,’

patients might answer a number of questions neglected by Freeman and Watts. See Penfield to Hebb, 26 May – 16 October 1944, C/G 44 H-I-J, Box 57, WP Fonds.
63 Ibid.
64 Penfield to Fulton, 23 January 1948, Wilder Penfield Fonds, Box 142, Folder W/P 151.
Penfield hoped to “secure an estimate of the effect of the operation and whether or not it was worthwhile from the patient, from the nearest relative, and from the clinician in charge….This will give us a more clinical estimate and leave the more scientific study to follow.”

In that connection, Malmo enlisted the help of Margaret Burns, a psychiatric social worker, to compare the gyrectomy cases with a series of lobotomy cases carried out at the Verdun hospital. The results, as Malmo indicated half-a-century later, were not encouraging.

Penfield’s ultimate evaluation of the operations can be gauged by comparing an early handwritten draft of his paper to the finished product. The paper begins:

It was with considerable reluctance that I was forced to admit that the loss of function produced by frontal lobotomy was justified by the improvement in symptoms and by the simplification of the care of certain patients in the field of psychiatry. Consequently, with the arrival at McGill of Dr. D. Ewen Cameron to create a separate department of psychiatry we agreed to carry out the procedure under the direction of his department. But it also seemed justifiable to seek a procedure that would produce less mental deficit and still be therapeutically successful.

This paragraph did not appear in the published version of the paper, and suggests that Penfield’s reluctance was justified. What little communication there was Cameron and Penfield became increasingly strained. In a 1945 letter to a former colleague serving in England during the war, about one year after the gyrectomy cases, Penfield discussed the relationship between neurology, neurosurgery, psychiatry and psychology, noting that:

when [these fields] are completely isolated we, for our part, miss very much all contact with psychiatry and psychology. At present the Allan Memorial Institute seems to be so far away that it takes a long time to get a consultation and it is impossible to have spontaneous discussions on mutual interesting subjects, not impossible, but so difficult that they occur too rarely. The results must of necessity be that we will have to have a psychologist on our

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65 Penfield to Cameron, 7 January 1946, Folder W/P 151, Box 142, WP Fonds.
66 It is unclear how many lobotomies were performed at the Verdun asylum by 1944, although there was evidently enough for comparison. This number may have increased after World War II, when the invention of the transorbital, or ice-pick, lobotomy was advanced for use on chronic schizophrenics, who were likely held in high numbers at the curatorial institution. It is also unclear who performed the lobotomies at Verdun, although as will become clear below, it could very likely have been surgeons from the MNI, although it was unlikely to have been Penfield himself.
67 WP Fonds, Box 142, Folder W/P 151.
own staff to help us with interesting problems. I believe that the rough and tumble of active
discussion between those interested in these two fields might well lead to some real
advance.⁶⁸

For Penfield, the AMI (including Malmo, their psychologist) was too far way to collaborate with
effectively. Penfield's desire to have his own psychologist on staff came at a propitious time. In 1946,
C.E. Kellog, then head of McGill's psychology department, was attempting to lure Donald Hebb
away from the Yerkes Primate Laboratory. Hebb was initially unsure, but noted that the possibility
of renewing his work at the MNI was an attraction. Penfield confided to Hebb that he was sure that
this would be possible, and noted that Malmo was in the process of evaluating the gyrectomy cases:
"the whole future of operations of this sort now hangs in the balance. There is a good deal of
interesting psychological material of various types, however, which Dr. Malmo at present does not
find time to study, and I am sure that he and Dr. Cameron would like your cooperation and help up
here, just as I would."⁶⁹ Penfield's tone suggested that he hoped Hebb would act as a bridge between
the two departments, given the continued isolation of Cameron and Malmo at the AMI.

Finally, in 1947, with the deadline for their presentation of their gyrectomy cases to the
ARMND looming, Penfield had had enough. While the gyrectomy project in Montreal had stalled,
south of the border in New York state, the Columbia-Greystone lobotomy project, the largest
scientific study of lobotomy yet undertaken, was being overseen by Fred Mettler.⁷⁰ During a brief
visit to Montreal, Mettler discussed with Penfield the strange testing procedure used to evaluate their
own surgical alternative to lobotomy, which they called areaectomies.⁷¹ In a letter to Cameron,
Penfield noted that:

⁶⁸ Penfield to W. S. Ross, 15 May 1945. WP Fonds, Box 58, Folder C/G 45 R.
⁶⁹ Penfield to Hebb, 28 January 1946, Folder C/G 46 H-I-J, Box 58, WP Fonds.
⁷⁰ For more on the Columbia-Greystone project, see Pressman, Last Resort, 147–93, 362–400;
⁷¹ This difference in nomenclature alludes to a subtle, but interesting difference between surgical
procedure at the MNI, and other surgical clinics and physiological laboratories in the North
America. Gyrectomies were named after the anatomical brain structure – the clearly visible gyri – to
[Mettler] told me when he was here in Montreal that there had been a large number of
gyrectomies, which they call areaectomies, done there, carrying it out in a strange sort of
way. The surgeon is prevented from knowing what the results are and the examiners of the
patient are prevented from knowing what the surgeon has removed or if he has removed
anything; so that from a choir of ignorance may emerge a chorus of true tones not
influenced by any wishful thinking.\footnote{72}

The tone of Penfield’s letter suggests that he had grown weary of second-hand reports on the
patients from Malmo. Penfield chose to employ a poker metaphor, asking Cameron “When shall we
get together to put our cards on the table in regard to gyrectomies?”\footnote{73} If Penfield felt that it was
time to ‘call’ in the high stakes game of psychosurgery, then it had been the lure of interdisciplinary
collaboration that had brought the players again and again to the table.

For his part, Cameron’s enthusiasm was not dampened. In April of 1947, Cameron wrote to
Penfield about possibly continuing traditional lobotomies (as opposed to gyrectomies) on a limited
basis for psychoneurotics, asking “Could you let me know when you wish to start?”\footnote{74} He received no
reply. Three months later, Penfield responded to a letter from Freeman and Watts asking if he would
support a conference on psychosurgery in Lisbon. Penfield responded that he would not, and sent a

be removed. The gyri constitutes the most obvious, anatomically discreet and visible feature of the
surface of the brain, and would make an obvious target for a seasoned brain surgeon, given its clear
presence. By contrast, the ‘area’ in ‘areaectomy’ referred to the designated Broadman areas that had
become popular in American neuroanatomy. As discussed in Chapter 1, these areas were meant to
designate different zones in the brain that were marked by different cellular structures that were
identified by histological examination. Although Penfield expressed great admiration for Oscar and
Cecil Vogt, the German neuroanatomists who developed the concept of cytoarchitectonics, which
later became the Broadman areas, he remained deeply skeptical of their value, particularly for
surgery. Again and again in discussion with other surgeons, Penfield disparaged this form of micro-
localization, which he felt was useless for surgery (one could not see Broadman areas with the naked
eye), and did not possess the functional specificity that was typically claimed for them. Pointedly,
when the American geneticist H.J. Muller wrote to Penfield about the possibility of nominating
Oscar Vogt for the Nobel Prize in 1948, Penfield responded that “I would not be willing to support
Vogt….His work and that of Brodman and others upon the cytoarchitecture of the cortex has been
much criticized recently by Lashley, Percival Bailey and others, and I am afraid that no very good
case could be made of for his candidacy. Also, I think that in the English-speaking world his
candidacy might not be very popular because he is, after all, a German of the Germans.” Penfield to
H.J. Muller, 22 January 1948, WP Fonds, Box 60, C/G 48 M.

\footnote{72} Penfield to Cameron, 2 October 1947, WP Fonds, Box 59, C/G 47 M.
\footnote{73} Ibid.
\footnote{74} Cameron to Penfield, 7 April 1948, Wilder Penfield Fonds, Box 59, Folder C/G 48 C.
private note to Henry Alsop Riley, president of the American Neurological Society, asking him not to support Freeman’s proposal. For his part, Herbert Jasper concurred, noting that he was in “wholehearted agreement with your reaction to Freeman’s proposal. How easy it is to lose one’s perspective!” While Penfield carried on cordial written communication with Cameron, his private asides were more candid, and reflected his disappointment in their relationship. In a response to a letter from Leon Bernstein of the VA hospital in Topeka, which espoused exactly the kind of interdisciplinarity that Penfield championed, especially in relation to the “surgical treatment for intractable mental illness,” Penfield responded “it is so much easier to combine all the different fields on paper than it is actually. Eventually the test that must be applied to the use of psychologists, psychoanalysts and psychiatrists...is - what is best for the patient?”. That same year, Penfield responded to a question about Cameron from Sidney Burwell of Harvard Medical School. Penfield reported that Cameron had confided to him that he was planning on leaving McGill. Then, scorn practically dripping from the page, Penfield added in an aside:

Cameron is a good teacher, an industrious worker, and a man of considerable force. He is anxious to cooperate with other departments within the university but also always does so in his own way. He has had very little in common with our department, but has, I think, done an excellent job in regard to the University as a whole and I suppose the students have found him an excellent teacher.

Penfield's growing disdain for Cameron and psychosurgery produced more and more curt responses; he declined to contribute to an NIMH study of psychosurgery, as well as to take part in the 1950 International Congress of Psychiatry in Paris, at which he had been asked to discuss lobotomy and topectomy (another variation of psychosurgery). He even felt compelled to chastise

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75 Freeman to Penfield, 7 July 1947; Penfield to Freeman, 14 July 1947; Penfield to Riley, 14 July 1947; Penfield to Jasper, 14 July 1947; Freeman to Penfield, 21 July 1947, WP Fonds, Box 59, Folder C/G 47 E-F.
76 Penfield to Bernstein, 9 February 1949, Folder C/G 49 B, Box 60, WP Fonds.
77 My emphasis. It is unclear as to why Cameron did not take the job at HMS, but in this letter, Penfield goes on to tell Burwell to consult his friend Stanley Cobb at Harvard, and it is possible that Cobb may have relayed a more negative opinion of Cameron to the hiring committee. Penfield to Burwell, 7 May 1949, Folder C/G 49 B, Box 60, WP Fonds.
his MNI colleague Hebb, who had taken up a position in the McGill psychology department, and who had proposed replacing lobotomy with implanted electrodes which might produce the same effect (“It is a rather radical suggestion.”)  

For his part, Cameron's suggestions for replacing lobotomy became more experimental and fanciful. In 1952 he wrote to Penfield about the possibility of using ultrasonic waves to disrupt brain tissue, a technique then being developed by the cybernetic psychiatrist Warren McCullough at the Manteno State Hospital in Chicago. Penfield responded with a touch of sarcasm, stating that:

> it seems to me that ultrasonic technique might be quite useful if the cortex of the brain were laid out smoothly on a curving surface instead of following innumerable cracks which extend into the substance of the brain. I do not believe that any technique, however well worked out...could ever give anything like the localization of destruction that actual gyrectomy produces, and inasmuch as none of the gyrectomy patterns that I was able to work out seemed to produce a result that you felt was useful, I cannot imagine that any diffuse process of that sort would fare better...the research approach does not seem to me to be a very good one.

Cameron responded with a lengthy statement that a surgeon might be able 'feel one's way' through the brain tissue after several attempts, and then invoked the possibility of using controlled hypoxia as an alternative therapy, adding that “I remain convinced that there is gold in this particular hill, but how to come at it is quite a question.”

Again, Cameron received no reply. For his part, by 1954, Penfield could confide to a colleague that:

> I have carried out no further gyrectomies, and, aside from a very few lobotomies carried out for a year or two following that, I have given up such procedures altogether. Dr. Ewen Cameron....and I have decided not to do any lobotomies or gyrrectomies for the time being. Other members of my staff carry out the procedure of lobotomy when requested to do so, on chronic patients in psychiatric hospitals, but the number of such operations is comparatively small.

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78 Penfield to Hebb, 18 September 1950, Folder C/G 50, Box 60, WP Fonds.
79 Penfield to Cameron, 23 April 1952, Folder C/G 52, Box 61, WP Fonds.
80 Cameron to Penfield, 2 May 1952, Wilder Penfield Fonds, Box 61, Folder C/G 52 C.
81 Penfield to Carl-Eugen Erdmann 20 December 1954, Wilder Penfield Fonds, Box 62, Folder C/G 54 E-F.
What are we to make of this limited collaboration between the AMI and the MNI, and of the relationship between Penfield and Cameron? What does this episode tell us about the history of psychosurgery and its relationship to the broader development of neuroscience? And finally, what can this episode tell us about the nature of the interdisciplinary neuroscience that had developed at the MNI, along with alternative versions of interdisciplinarity?

In the 1970s, during heated arguments over the resurrection of psychosurgery, the practice drew almost universal condemnation, both by critics of psychiatry, and by psychiatric insiders. Those involved in this debate typically portrayed lobotomy as a marginal practice, championed by careless or ambitious doctors, and carried out at the fringes of respectable medicine. This narrative received a much-needed correction by historians such as Elliot Valenstein and Jack Pressman, who in different ways pointed out that lobotomy and psychosurgery were not, in fact, fringe practices, but rather sat at the heart of much of mainstream psychiatry after World War II.82 Simultaneously, Pressman has noted that the professional divisions between psychiatry, neurology and neurosurgery, rather than bringing increased scrutiny on the practice of psychosurgery, actually led to ‘turf wars’ over who was properly qualified to carry out the procedure, and who could best rationalize and improve it. After World War II, and Freeman’s invention of the transorbital, or ‘ice-pick’ lobotomy, many elite clinicians and scientists stepped in with ‘rational’ alternatives, supposedly derived from laboratory practice.83 Penfield’s second thoughts about psychosurgery, and his initial efforts to find a ‘rational’ lobotomy in the form of gyrectomy, predate Pressman’s account of the same phenomenon in the post-war environment, and show just how easily miscommunications, and a desire to expand the therapeutic purview of neurosurgery, could combine to lead even a thoughtful surgeon like Penfield to ‘experimentation in clinical medicine.’

82 Pressman, Last Resort; Valenstein, Great and Desperate Cures.
83 Pressman, Last Resort, 318–61.
At the same time, Penfield’s unwillingness to continue the operations, at a time when psychosurgery was rapidly accelerating, complicates Pressman’s interpretation of this period. Pressman has argued that the overwhelming tendency, when the detrimental effects of lobotomy were first observed, was not to abandon the procedure, but to search for a better, more precise operation. Innovators like Lawrence Pool and others raced to find a better lobotomy, confident that laboratory-based science (particularly with primates) would help to refine the procedure.84 By contrast, with the help of his psychologist colleagues, Penfield had turned his operating room into a psychological laboratory, and this scientific endeavor stayed his scalpel. The interdisciplinary success of his early collaboration with psychologists (Hebb and Harrower) led him to value their input on the issue of loss of function following surgery, and his desire to add a psychologist to the gyrectomy project indicates that he trusted this discipline to act as a bridge to the psychiatric establishment of Cameron, who seemed uninterested in the kind of deep study of patients that had been developed at the MNI.

This was not the kind of collaboration that Cameron had in mind. In addition to hoping to build a psychiatric empire, Cameron had a fundamentally different view of interdisciplinary collaboration. For Penfield, interdisciplinarity was an outgrowth of his own inability to master every aspect of neurological science and medicine. In order to expand his surgical effectiveness, Penfield needed to enlist the aid of others who could ‘speak’ the professional ‘language’ of the neurosurgeon/neurologist. This kind of interdisciplinarity demanded deep collaboration on individual patients, often for extended periods of time - in short, it was time consuming, expensive, and made intensive use of human resources. Moreover, the participants needed to learn each other’s scientific and professional language; Hebb knew enough physiology to convince the surgeons that he was worth listening to, and Harrower demonstrated her effectiveness in a surgical setting by

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84 Pressman, 319–47.
making accurate diagnoses. Jasper not only spoke the language of surgery, but could speak the language of psychology, physiology and chemistry. This kind of synchronized activity was at the heart of the MNI’s approach to interdisciplinarity, and it was an approach that was totally anathema to Cameron, whose professional ambitions for psychiatry led him to approach interdisciplinarity in the same way that one might approach a smorgasbord - take from any adjacent discipline as needed for experimentation. As others have noted, Cameron’s attitude towards psychiatric therapy was not only one of relentless experimentation, but also of a restless desire to automate the psychiatric process - to push more and more patients through the revolving doors of the psychiatric hospital. As early as 1946 Cameron had attempted to reform the clinical practice of psychiatry in Montreal by transforming the AMI into a ‘day hospital’ that might return patients to their homes more quickly, and avoid the expense of long-term hospitalization. Cameron had run into a roadblock on his mission to speed up psychiatry - Penfield wanted to collaborate too closely, rather than act as the mere technician for Cameron’s efforts. For Cameron, psychiatry was to be the master discipline that called the shots, and if Penfield didn’t like it, then he would ‘go it alone.’ Indeed, reflecting on his time in Montreal, Cameron noted that:

> when I came here in 1943 McGill was universally regarded as a very difficult place for a psychiatrist because of strong organic biases represented by Pathology, by the Neurological Institute and indeed by the whole curriculum, yet, in actuality, there were powerful forces in favor of a strong psychiatric development….the psychiatric tide was running strongly and hence the obvious thing to do was to attempt to establish psychiatry everywhere that it could take hold and flourish and the wrong thing to do would have been to centralize it as neuropsychiatry was centralized at the MNI.

In effect, the professional ambitions of Cameron and Penfield prevented the establishment of a meaningful assembly between the two men; they simply could not fire in synch with each other.

Penfield’s desire to collaborate on equal terms would have interfered with Cameron’s relentless

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85 See, for instance, papers by Cameron on day hospitalization in Ewen Cameron Fonds, MG 1098, Acc No 387 Ref 38-220-1-2 and 1-3.
desire to experiment, and to speed up the psychiatric process. Learning the language of another professional would always be too slow for Cameron.

**Sensory Deprivation and the Turn to Psychic Driving**

The failure of the gyrectomy project severed the one connection that the AMI had to other medical specialties in Montreal, and effectively closed the door both to collaboration, and oversight. Cameron proceeded to investigate other forms of innovation in psychiatry. The main line of investigation taken by Cameron was a technique he developed for treating schizophrenia that he referred to as ‘depattern’ or ‘psychic driving’.

If Cameron was unable to speed up the treatment of psychiatric patients by borrowing from the field of neurosurgery, might he be able to automate the field of psychotherapy instead?

Cameron had hoped to use somatic therapies to achieve psychotherapeutic results since at least his time in New York, when he had tried to desensitize patients to anxiety with large doses of adrenaline. Cameron had been unhappy with the results, but by the early 1950s, his attention had shifted from anxiety to schizophrenia. Now that the door to the MNI was more-or-less closed, Cameron decided to pursue this new line of research with a vengeance.

Cameron’s shift to schizophrenia research occurred at propitious time. Donald Hebb had returned to McGill University to take over leadership of the psychology department in 1948 following his time at the Yerkes Primate Lab, and Hebb’s experiments in sensory deprivation provide

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88 A. Collins, _In the Sleep Room: The Story of the CIA Brainwashing Experiments in Canada_ (Key Porter Books, 1997), 119.
an example of how Cameron was now free to appropriate ideas from the MNI group, shorn of their context.

As discussed in Chapter 2, Hebb's collaboration with Penfield in the late 1930s had fundamentally altered his own theories about human intelligence, and his time at the Yerkes lab had made him increasingly skeptical of some of the theoretical presuppositions of behaviorism. Indeed, as Hebb later recalled:

There were…opportunities [at Yerkes] to enjoy the dilemma of those hard-boiled visiting “learning theorists” [behaviorists] who could not help recognizing behavior in the chimpanzee that they were already familiar with in man, but who could not talk about it without embarrassment—because it would require “mentalistic” language.⁸⁹

Hebb wanted to investigate the operations of the mind using the tools of behaviorism, and frequently called for a “behavioristic or learning-theory analysis of the thought process.”⁹⁰ The classic concepts of psychology – intelligence, memory, imagery, attention – could be analyzed in a more sophisticated way if one employed the tools of behaviorism, but not its overreliance on the stimulus-response formula.

Hebb’s theories of intelligence, developed first with Penfield and then expanded in his 1949 monograph The Organization of Behavior, suggested that intelligence was ultimately a product of biological inheritance and experience. At the same time, Hebb felt that the problem of perception was not adequately handled by either the Gestalt school of psychology, with its emphasis on holistic perception, or by behavioristic learning theories, which largely ignored the issue. Hebb developed a tentative solution to both of these issues after being exposed to an obscure German study of blind patients who had their sight restored following a surgical operation.⁹¹ According to Hebb’s theory, the ability to perceive whole forms might involve a simple learning process that occurred at the level

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of the brain cell. Through modification of the activity of the synapse - the microscopic space
between nerve cells - perception of forms might be gradually ‘learned’ as an animal grew.
Simultaneously, this learning process might explain differences in human intelligence, and the
perplexing results of his earlier collaboration with Penfield on frontal lobe removals.  

A work of some theoretical brilliance, Hebb’s theories also implied an obvious line of
experimental research: if perception and intelligence were the result of experience and sensory
input, then what might be the result if the input was shut off? Hebb had attempted to test his ideas
with provisional experiments in which he reared rats in total darkness to determine if this had an
effect on their intelligence or visual perception.  

While these early studies were intriguing, Hebb hoped to expand his line of experimental attack on the problem of intelligence, and he considered
Montreal to be the best place to do it. In a 1951 letter to the Rockefeller Foundation requesting
funding for what would later become his experiments in sensory deprivation, Hebb noted that:
“Because some of the ideas involved were somewhat heretical, the past four years in the animal
laboratory at McGill have been mainly devoted to getting evidence that would show that the
theoretical approach would pay off in new experimental results.”  

In the same letter Hebb made his opinion clear that his research would be of the ‘basic’ variety, and that he did not think much of the
applied research that was currently being conducted in the field of psychiatry: “The need is for the
development of theory, not for more ad hoc research on the specific problems of mental illness.”  

In another letter to the same Rockefeller official, Hebb noted that:

I could easily get money for the study of experimental neurosis, or of schizophrenia, or of
some aspect of social problems here in this bilingual, bicultural city; but I have found it very
difficult indeed to get money simply for the purpose of trying to learn a little more about

93 Donald O. Hebb, “The Effects of Early Experience on Problem Solving at Maturity,” American
94 Hebb to Robert S. Morison, 14 March 1951. DO Hebb Fonds, 0000-2364.01.36.
95 Ibid.
how behavior is determined. To get what we have now I have had to perjure myself to some extent, by letting on to be optimistic about possible practical applications.96

Hebb might have thought that there were no obvious psychiatric implications for his experiments, but that did not that there were no practical lessons to be learned. In the same year that Hebb was in talks with the Rockefeller Foundation regarding research funding, he also participated in a brief conference with the Canadian Defense Research Board on the issue of Soviet mind control and brainwashing. The conference, held at the Ritz Carlton Hotel in Montreal in June of 1951, included representatives of the British and Canadian military establishment, as well as Hebb and two Montreal psychiatrists, J.S. Tyhurst and T.E. Dancey. At that conference, Hebb was asked about the possible mechanisms of Soviet mind control, and particularly the disturbing ease with which American POWs had been seemingly ‘brainwashed’ to spout communist propaganda.97 As Hebb later recalled:

I don’t really know what…basis there was for my being invited to that conference except that it was here in Montreal….they were worried about the apparent changes of attitude on the part of the Russian prisoners who confessed to all sorts of crimes, apparently without physical coercion. They were worried about the possibility that the Russians had discovered some method of changing attitudes radically and fundamentally. It occurred to me that possibly one source of such changes might be isolation that had the effect of radically preventing normal perceptual information from the environment on the thought process, since The Organization of Behaviour’s theory had implied that this was essential to the normal operation. So I wrote to [the DRB] and told [them] that if they could provide me with $10,000 a year I thought it was quite possible that I could get them some information as to what was going on in this.98

Given Hebb’s candid statements regarding his ‘fibbing’ about the practical applications of his work, it seems fair to speculate that he was at least partially using the DRB as a source of funding for work that might have been regarded as too abstract to qualify for more mainstream sources of support.

96 Hebb to Robert S. Morison, 17 July 1951. DO Hebb Fonds, 0000-2364.01.36.
98 Brown, 211.
Hebb received extensive support from the DRB, and undertook his sensory deprivation research from 1951 to 1955. Student volunteers were asked to lie in a sealed room or small box, given blindfolds and ear muffs, and padded gloves and footwear that would cut off tactile sensations. Participants could stop at any time, and were permitted to leave for meals and bathroom breaks. The studies were meant to investigate hallucinations and intellectual impairments, tolerance for isolation and boredom, and most importantly, attitude change, which would determine whether the technique could be employed for the inculcation of propaganda. If a participant made it past the required 24 hours of isolation, they were read a short piece of pseudo-propaganda that a young college student would be likely to resist, typically about the existence of ghosts, or an anti-evolutionary tract, or a discourse on the possibility of extra-sensory perception.99

The effects of the sensory deprivation experiment ran in several directions. Subjects who continued with the procedure (many refused or stopped short of the 24-hour mark) began to experience difficulty thinking, disturbances of body image, and finally vivid visual and auditory hallucinations. Emerging from the isolation, subjects reported perceptual disturbances, and their IQ and reaction-time tests were briefly lowered. The experimental ‘brainwashing’ produced mediocre results. Two weeks after the experiment, some subjects reported that they had checked out books on ESP, or experienced spontaneous fear of ghosts, or tried to “use ESP in card-playing.” While the propaganda effects of the experiment were underwhelming, Hebb reported being deeply disturbed at how quickly the isolation seemed to affect the subjects emotionally. “It scared the hell out of us to

see how completely dependent the mind is on a close connection with the ordinary sensory environment, and how disorganizing to be cut off from that support.”

A desire to publicize the results of the sensory deprivation studies - no doubt to create some academic buzz for the experimental support for his theories of brain function - led Hebb to argue that the studies should be declassified. The DRB agreed to a partial declassification of the results in 1952, but insisted that the components of the study on attitude change be kept secret. Hebb and a number of graduate students published an early communication of the results in the *Canadian Journal of Psychology* in 1954.

Meanwhile, Cameron had begun to experiment with a new form of therapy in 1953, just as his relationship with the MNI completely deteriorated. He initially called the technique ‘psychic driving’ or ‘depatterning,’ and it constituted his main attempt to cure schizophrenia (for which he expected to win the Nobel Prize). The goal was, in effect, to ‘brainwash’ a person suffering from schizophrenia out of their delusions and disruptive behavior patterns, typically through some kind of automated means. In practice, a patient would be given a brief psychotherapy session which was recorded on a primitive cassette tape player. A particularly significant statement from the therapy session would then be looped to play over and over. The patient would then be forced to listen to this tape for periods of time ranging from several hours to several straight weeks. The goal, apparently, was “the penetration of defenses, the elicitation of hitherto inaccessible material and the setting up of a dynamic implant.”

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101 Bexton, Heron, and Scott, “Effects of Decreased Variation in the Sensory Environment.”

Needless to say, some patients resisted the ‘treatment,’ and Cameron employed a number of techniques to overcome resistance. Patients were often confined to a so-called ‘sleep room,’ where they would be placed on a steady diet of psychoactive drugs, including Desoxyn, sodium amytal, PCP, barbiturates, and lysergic acid diethylamide (LSD), along with regular and severe electroconvulsive therapy (ECT). In addition to these methods, Cameron employed a variation of Hebb’s sensory deprivation technique for the same purpose. In addition to being prevented from “touching his body – thus interfering with his self-image,” unspecified means were used to “cut down on his expressive outflow.”\textsuperscript{103} Put another way, in contrast to Hebb’s well-paid college students, patients at the Allan were not permitted to stop the experiment.

Finally, a therapeutic ‘disorganization’ took place, in which the patient might experience partial or total amnesia, incontinence, and would become helplessly dependent on the AMI nursing staff.\textsuperscript{104} Between 1953, when the procedure was developed, and 1964, when Cameron left the AMI, over 100 patients endured the psychic driving ‘treatment,’ which often left them with severe amnesia. When it was revealed in 1977 that much of Cameron’s work had been funded by the American Central Intelligence Agency (CIA) as part of its Cold-War era MK-ULTRA mind control research, scandal ensued. The disclosure of thousands of classified documents to the American journalist Johnathan Marks revealed a vast government attempt to funnel money and resources to scientists who might be able to provide methods of resisting Soviet ‘brainwashing,’ creating sleeper agents for espionage, or breaking foreign operatives through coercive interrogation - a veritable ‘Manhattan Project of the mind.’\textsuperscript{105} Cameron’s research had been identified as promising by CIA officials, and was funded through a front organization known as the Society for the Investigation of Human Ecology. In 1980, Velma Orlikow, a former patient of Cameron’s and wife of a Canadian member

\textsuperscript{103} Cameron, “Psychic Driving,” 504.
\textsuperscript{104} Ibid.
\textsuperscript{105} Collins, \textit{In the Sleep Room: The Story of the CIA Brainwashing Experiments in Canada}; J. D. Marks, \textit{The Search for the “Manchurian Candidate”: The CIA and Mind Control} (New York: Times Books, 1979), Ch 8.
of parliament, successfully sued the CIA for the distress that she endured at the AMI. This lawsuit prompted the Canadian Minister of Justice, John Crosbie, to investigate the role of Canadian military funding in Cameron's work. The combination of Marks’ expose and Crosbie's investigation brought Cameron’s research to public attention, and rewrote much of Cameron’s public image into that of a sort of psychiatric ‘mad scientist’ who experimented on hapless patients as part of a cold-war techno-scientific conspiracy.106

The macabre specter of mind-control in journalistic writing on Cameron has overshadowed an important point about his development of ‘psychic driving’ – it was, in many ways, an interdisciplinary venture that was a mirror-image of the MNI community’s approach. Cameron’s appropriation of Hebb’s sensory deprivation work occurred at almost the exact same time that Penfield finally shut the door to more psychosurgeries at the MNI (approximately 1953). Cut off from one interdisciplinary scientific community, Cameron was both isolated, and free to appropriate tools and techniques as he saw fit, with no oversight from their originators, and no input as to their use. The cavalier-ness with which Cameron approached his own experimentation was totally anathema to Penfield-and-company’s cautious and grounded approach.

Despite their proximity, Hebb and Cameron corresponded almost not at all, and it appears that Hebb was largely unaware of the extent to which Cameron was making use of his techniques, until all was revealed in the 1970s. Asked by John Marks about Cameron’s experiments in 1976, Hebb remarked that:

That was an awful set of ideas Cameron was working with. It called for no intellectual respect. If you actually look at what he was doing and what he wrote, it would make you laugh. If I had a graduate student who talked like that, I’d throw him out….Look, Cameron was no good as a researcher… He was eminent because of politics.107

106 Collins, In the Sleep Room: The Story of the CIA Brainwashing Experiments in Canada; Lemov, “Brainwashing’s Avatar.”
107 The ‘politics’ referred to here was likely Cameron’s growing consolidation of control of psychiatric training not just in Montreal, but for Canada as a whole. Anne Collins discusses this in her book on Cameron, In the Sleep Room. Quotation is from Marks, The Search for the ‘Manchurian
Indeed, Hebb complained about Cameron at the time, although he made an effort to keep the lines of dialog open. Since his arrival in Montreal, Cameron had ambitions of building a psychiatric empire, with the Allan at its center. When a system of federal-provincial mental health grants was inaugurated in 1948, Cameron pounced on the opportunity to fund his vision. Hebb too hoped for some of the federal-provincial money to fund his research, but found out that Cameron had actively subverted his attempts to get grants. In a 1955 letter of complaint to the Principal of McGill, Hebb laid out at great length his view on the relationship between his own department and the AMI.

However, parochial department infighting soon gave way to a disquisition on the nature of collaboration itself. “Psychology should be to psychiatry as physiology is to medicine or physics to engineering,” Hebb proposed. Hebb went on to protest the fact that, to the extent that Cameron wanted any psychologists to work at the MNI, it was in a subordinate role, even when it came to the planning of research. “If [there] is a collaborative undertaking, between psychology and psychiatry, the principle [should] also be maintained that its guidance is collaborative.”

Given that these concessions were unlikely to occur in any collaboration with Cameron, Hebb chose to withdraw from any future collaborative efforts with the AMI. Psychic driving would go from an experiment to a standard procedure in the coming years as the final weak ties between the AMI and the Montreal neuroscience community were severed.

**RNA, Worms and Memory**

One final example will serve to illustrate how the split between the AMI and MNI played out. In 1956, while the psychic driving experiments were in full swing, Cameron embarked upon a new line of research for a different disorder – senility. The memory loss associated with ageing had

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*Candidate*: The CIA and Mind Control, 146; Collins, *In the Sleep Room: The Story of the CIA Brainwashing Experiments in Canada*, 66, 118.

108 Hebb to James, 29 November 1955, Folder 0000-2039.01.3, DO Hebb Fonds.
been a preoccupation of Cameron’s since the 1940s, but with his now-consolidated position at the Allan, he felt that the time was ripe for a new attack on the problem. Cameron had recently read of the research of Holger Hydén, the Swedish scientist who had attempted to demonstrate experimentally that RNA composed the physical substrate of memory. Cameron combined this with his reading of the curious research of J.V. McConnell, the American psychologist who had attempted to demonstrate hereditary transmission of memories in the planarian flatworm; McConnell did this by training the worms in a number of maze-based and Pavlovian conditioning tasks; he then ground up the trained planarian and fed them to a new generation of worms in order to see if they would inherit the ‘memories’ of the previous generation. If the ‘memories’ had been inherited, the worms would presumably complete the tasks more efficiently, which is exactly what appeared to happen.\(^{109}\) McConnell’s widely publicized positive results were later disputed,\(^ {110}\) but they appeared convincing enough to Cameron.

Cameron went on to conduct a series of studies at the AMI that were meant to investigate a possible treatment for memory loss in the elderly. If RNA were the substrate of memory, could RNA supplementation be used as a form of therapy for memory loss? Cameron and his colleagues at the AMI attempted to test this hypothesis with a mechanism that was at least elegant in its simplicity, if not very sophisticated or well thought-through; he simply gave a group of 20 senile, pre-senile and arteriosclerotic patients RNA supplementation, both orally and intravenously. While


\(^{110}\) McConnell’s experiments later became a common trope for some historians of science looking to discuss the nature of ambivalent results and wishful thinking in laboratory research. That being said, it is worth noting that, while McConnell’s findings were largely discredited, the procedures he developed for testing flatworm reflexes were highly productive, and still in use. By establishing that planarian could in fact be trained reliable, he established the reality of invertebrate learning. H. M. Collins and T. Pinch, *The Golem: What You Should Know About Science* (Cambridge: Cambridge University Press, 2012), 5–25; Mark Rilling, “The Mystery of the Vanished Citations: James McConnell’s Forgotten 1960s Quest for Planarian Learning, a Biochemical Engram, and Celebrity.,” *American Psychologist* 51, no. 6 (1996): 5–26.
earlier experiments with DNA supplementation produced no results, Cameron was encouraged by some limited success when he turned to RNA, although “the earlier intravenous solutions were so apt to produce severe shock-like reactions, that they were stopped.” Negative side effects appeared frequently in the RNA memory study, including drops in blood pressure, hyperventilation, and stomach upset and cramping. Given Cameron’s evident tolerance for the side effects of his psychic driving treatment, one wonders how severe these side effects must have been to warrant his inclusion of them. By 1961, a new laboratory technician had evidently improved the intravenous RNA solution, ending the shock-like side effects (the nausea remained, and the blood pressure changes were compensated for with aramine), and the intravenous therapy resumed. Cameron attempted to demonstrate success in his RNA-based therapy through the use of Wechsler intelligence tests, and claimed that EEG records of the patients demonstrated an ill-defined “reduction in pathology.” All told, approximately 30 patients underwent some variation of Cameron’s RNA memory therapy.

Despite its veneer of theoretical legitimacy, much like the psychic driving experiments, the RNA therapy simply made no sense. The study contained numerous flaws that, even if the theory were plausible, would have made the results invalid. There was no mention of effects of digestion on the oral administration of RNA, or of the blood-brain barrier. The intelligence and memory tests were carefully administered, but there was no reference to the ‘practice effect,’ or any attempt to use the tests in a sophisticated way. And the EEG recordings were included with almost no careful interpretation. In all respects, the study was an absurd hash.

Yet the most striking part of the RNA memory study, given the context, was Cameron’s total embrace of RNA memory theory. As detailed in Chapters 2 and 3, the MNI group became increasingly distinguished for their work on memory, and particularly for a theory of memory.

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111 Cameron, “The Processes of Remembering,” 329.
112 Cameron, 330.
formation that relied on the psychological theories of Hebb, the testing work of Milner, and the structural localization of Penfield and Jasper. This theory of memory was distinguished from the perspective of F.O. Schmitt and his colleagues at MIT, who emphasized the possibility that memory might be encoded in macromolecules such as RNA, much in the same way that these molecules retained and transmitted hereditary information. As detailed in Chapter 3, most of the major players in the Montreal group were skeptical, if not outright hostile, toward the RNA memory theory, and found little evidence to support it. In self-imposed exile from the main current of research in Montreal, Cameron felt compelled to take a position on the controversy.

In his 1962 Maudsley Lecture, “The Processes of Remembering,” in which he detailed the results of his RNA memory experiment, Cameron took the opportunity to take a swipe at the MNI and his Montreal colleagues. Discussing the work of Penfield and Milner on memory localization, Cameron added that “it is probably premature,” although he gave no indication as to why. Cameron then added that he was skeptical about some of Penfield's results because of the graduated amnesias that could be produced with ECT. While he did not specify why this made him skeptical, he did state that “we may go on to postulate that those memory traces with numerous, long-established inter-connections are much less easily obliterated than those of recent origin.” Given that ECT was one of the most prominent components of psychic driving, and that many of his patients emerged with the capacity for memory almost completely destroyed, his claim had a certain plausibility.

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114 Cameron, “The Processes of Remembering,” 327.
115 Cameron, 328.
Cameron and the AMI, in the years after the abortive collaboration on psychosurgery, present us with a sort of dark, mirror-image of Penfield and the interdisciplinary community at the MNI. The contrasts are striking. While Penfield and his growing team participated in deep collaboration, learning to speak each other's disciplinary ‘language,’ Cameron feigned interdisciplinarity by appropriating the tools and theories of others with insufficient understanding of their strengths, weaknesses, and tacit knowledge. While Penfield could be autocratic in his personal demeanor, his collaborations with others occurred on an equal playing field – no discipline was higher than another. For Cameron, psychiatry was the master discipline that called the shots – all others were technicians. And above all, while the MNI could transform the operating room into a psychological laboratory, it never did so at the expense of the patient’s well-being. When mistakes led to memory impairment or loss of function, they were corrected for. For Cameron, patients were material on which to experiment, and memory loss was the goal. As Rebecca Lemov has noted, the connection of Cameron's work to cold-war intrigue has tended to obscure it from more serious scholarly inquiry, and restricted it to sensationalistic accounts. In that vein, a closer examination of the relationship between Penfield and Cameron, and the subsequent estrangement of neurology and psychiatry in Montreal –Jasper's ‘two solitudes’ – can serve as a case study of contrasting philosophies of interdisciplinarity.

Epilogue: They hate psychiatrists...

The feud between neurology and psychiatry briefly entered the public arena in a 1956 profile of Penfield and the MNI. A journalist for Macleans magazine reported the comments of an unnamed Montreal psychiatrist: “Penfield and his staff are a close-knit bunch - and they hate psychiatrists.” The journalist went on to note that:

Penfield smiled when he heard that, and admitted it was half true. “We certainly don't hate psychiatrists,” he said. “It's true that we are concerned chiefly with treating ills of the brain
and nervous system by surgery and medicine; but psychiatry and neurology are closely related - and someone may suddenly make a discovery that will bring us all together.\textsuperscript{116}

Given Penfield’s initial dream of uniting all medical disciplines concerned with the nervous system under one roof - a dream born in his relationship with Adolf Meyer and nurtured by the cosmopolitan interdisciplinarity of the MNI - there was a note of melancholy in Penfield’s statement, a melancholy that likely echoed his own disappointment about Cameron and the AMI.

Penfield’s prophecy about the eventual reconciliation of neurology and psychiatry was only partially fulfilled, and in a manner that was highly ironic. At almost the exact same time that Cameron was attempting to ‘cure’ schizophrenia (mid-1950s), one of the most important developments in the actual treatment of schizophrenia was taking place only a few miles away. At the Verdun Protestant hospital - the very same psychiatric hospital that had been meant to house the refuse patients of the newly created Allan Memorial Institute - the German-born psychiatrist Heinz Lehmann was conducting a series of studies on the drug Largactil (chlorpromazine), recently marketed in France by the pharmaceutical giant Rhone Poulenc for the possible treatment of schizophrenia. In a story that would have likely warmed Penfield’s heart, Lehmann was able to read the French-language journals that described chlorpromazine primarily because he had learned French in Montreal, following his marriage to a French-Canadian woman.\textsuperscript{117} Lehmann’s 1954 article, “Selective Inhibition of Affective Drive by Pharmacological Means,” is widely credited which introducing chlorpromazine to the American market, and launching the modern field of psychopharmacology. Notably, in his 1954 article, Lehmann cited Hebb as providing theoretical justification for the action of chlorpromazine, although he used Hebb’s theories cautiously.\textsuperscript{118}

\textsuperscript{116} Eric Hutton, “Penfield,” McLean’s Magazine 18 February 1956.
\textsuperscript{118} Lehmann connected Hebb’s theories of cell-assembly and phase sequence to more traditional notions of inhibition, which he thought were the mechanism of action for chlorpromazine. H. E.
However, Hebb, Penfield and Lehman carried on no extensive correspondence; Cameron’s consolidation of power at the AMI had so thoroughly poisoned the well between psychiatrists and neurologists in Montreal that the introduction of chlorpromazine took place almost completely without Penfield or Hebb’s knowledge (a development rendered all the more ironic given Penfield’s belief that the most likely way forward in psychiatry would be through biochemistry).

To conclude, the story of the troubled relationship between neurology and psychiatry in Montreal presents us with a disturbing counterpoint to the relatively successful story of interdisciplinary collaboration embodied in the history of the MNI, and complicates the legacy of Penfield as a staunch opponent of psychosurgery. Penfield and Cameron’s brief collaboration on psychosurgery, and Cameron’s later development of psychic driving, presents a darker historical portrait of interdisciplinarity. Divorced from context, and the kind of tacit supervision that comes from collaboration, Cameron was free to pick and choose the bits of technique and theory that most fit his agenda, and combined them in ways that would have been unrecognizable to their originators. This is, of course, not to suggest that there was any one ‘true’ way of employing a technique or scientific theory. As has been seen in previous chapters, the members of the Montreal group were often able to collaborate despite strong theoretical disagreements; yet at its peak, the MNI collaborators were knowledgeable enough about each other’s disciplines that they could effectively speak each other’s languages in a way that was mutually intelligible. No such condition existed between the city’s neurologists and psychiatrists, and Jasper’s comparison to McLennan’s *Two Solitudes* was sadly apt. If Montreal’s bilingualism provided a convenient metaphor for scientific collaboration, then the rising tide of linguistic separatism in Quebec provided an equally useful counterpoint. Returning to the 1963 talk with which this chapter began, Jasper’s words are loaded with meaning:

I would not like to give the impression that I believe that solitude has been, or is, necessarily something to be avoided. In science, as in medicine, specialization has become and will continue to be a practical necessity, and solitude a rare and highly cherished privilege. There is no magic formula in “interdisciplinary research.” Cooperation among scientists, no more than among peoples, can be pre-arranged or organized. It must come out of natural desires and needs. Pre-arranged marriages are seldom successful. However, specialization and isolation in science carries the seeds of its own destruction as it does so often in society.\(^\text{119}\)

Ewen Cameron left Ravenscrag shortly after Jasper’s talk in 1964. He had been offered a position at laboratory funded by the American Department of Veterans Affairs conducting research on ageing, where he hoped to continue his RNA research. He died two years later while hiking in the mountains near Albany, New York. It is unclear as to whether he had any response to Jasper’s talk at the AMI. Given the events that occurred under his tenure, one would like to think that he was at least present for it.

The troubled relationship between Penfield and Cameron, between the MNI and the AMI, and between neurology and psychiatry in Montreal is illustrative both of the MNI’s style of interdisciplinary work – dependent on deep collaboration over clinical problems – and of an alternative style of interdisciplinarity advanced by Cameron – shallow, opportunistic, and dependent on the appropriation of ideas and tools without any meaningful ‘weak’ tie to their source. In many ways, Cameron and Penfield’s abortive collaboration over psychosurgery set the stage for a complete alienation of their respective assemblies; they could not ‘wire together’ because they could not ‘fire in sync’ over the issue of psychosurgery. In the Conclusion to this dissertation, I would like to return to the notion of ‘wired together’ assemblies of historical actors, in order to explore a number of issues raise by the preceding chapters.

\(^{119}\) Herbert Jasper Fonds, Box 1, Folder 11.
Conclusion – Wired Together: Reassembling Montreal Neuroscience

Epilogue: The Decline of the MNI and the Rise of MIT Neuroscience

In 1967, F.O. Schmitt and the Neurosciences Research Program published the first of their collected volumes, *The Neurosciences: A Study Program*.¹ The product of a month-long ‘intensive study program’ held in Colorado in 1966, the book was meant to act as a survey of the field of ‘neuroscience,’ and championed the role of Schmitt’s Neuroscience Research Program in bringing the field to fruition. It is instructive to examine Schmitt’s description of the NRP, and the activities of the Colorado Intensive Study Program, as Schmitt’s perspective on neuroscience was on full display.

Schmitt made a great show of the interdisciplinarity of the NRP, which had a dozen Associates from a “wide array of disciplines, ranging from mathematics and physics, through biochemistry and biology to neurology and psychology.”² These associates “garner, glean, and share the best obtainable facts and ideas about the nervous system, its cells, their organelles and molecules, as well as mathematical models and behavior and mental processes.”³ Pointedly, Schmitt noted that the NRP was “not an institute, but a kind of investigative center.”⁴ Although this was probably not meant as an explicit comparison between the NRP and the MNI, it is nevertheless instructive to consider the differences between Schmitt’s ‘investigative center’ and Penfield’s institute. The NRP started from a molecular perspective - working from the bottom up - but its form of interdisciplinary collaboration was the opposite - from the top down. Instead of collaborating on specific problems, the NRP skimmed the cream off of the most recent research in different areas.

² Quarton et al., v.
³ Quarton et al., vi.
⁴ Quarton et al., v.
and tried to relate them to each other using the tools of molecular biology. Notably, during the Colorado Intensive Study Program, the lectures on different topics were supplemented with four intensive “tutorial lectures on brain structure” given by the neuroanatomists Walle Nauta, Stanford Palay, and others.\textsuperscript{5} Schmitt noted that, “the biophysicists, physical chemists, and biochemists, who arrived with a tendency to regard the neurosciences as being in their infancy and lacking powerful simplifying explanations, left with an awareness of the great complexity of brain structure.”\textsuperscript{6} Notably, none of the major characters from the Montreal Neurological Institute were present for the Colorado study program, but it is doubtful that the brain’s complexity would have come as a great surprise to any of them. And no one from the MNI would have needed a tutorial on brain anatomy.

At the same time that the NRP was in ascendancy, the MNI was already in the midst of decline. Penfield’s retirement in 1960, and Herbert Jasper’s departure for the Université de Montreal in 1964, deprived the institute of its most powerful leaders. Simultaneously, Quebec itself began to fracture. The emergence of French-Canadian separatism made Montreal a hotbed of political discontent. In the same year that the NRP published its inaugural study program, the French President Charles de Gaulle made his infamous “Vive le Quebec Libre” speech from the balcony of Montreal’s City Hall during his visit for the Montreal Expo. De Gaulle’s use of the phrase, long a slogan of Quebec separatists, enflamed tensions that had been building in the province since the beginnings of the Quiet Revolution. Penfield, so long an advocate of harmonious co-existence between Montreal’s English and French speakers, was so incensed by the statement that he sent a harshly worded telegram declining to dine with the French president. “As a Canadian citizen and a worker for Quebec’s maturing strength, cultural, economic and intellectual, within the

\textsuperscript{5} Quarton et al., vii.
\textsuperscript{6} Quarton et al., vii.
confederation, I would find it difficult to meet him at this moment.” As has been seen in the previous chapters, Penfield often invoked linguistic differences as a way of explaining interdisciplinary work, and frequently compared the working environment of the MNI to what he thought were the harmonious linguistic relations of Montreal. This idealized version of Quebec was never entirely true, but it had functioned effectively as a metaphor up to that point; now, in the turbulent decade of the 1960s, this fiction could no longer be maintained.

More prosaically, changes to the financing of healthcare in Quebec forced the MNI to speed up its throughput of patients, meaning that less time could be spent on each one. Simultaneously, the Ottawa-based Medical Research Council began supporting the MNI through a block-grant system, which meant that sums of money from the federal government were used primarily to pay the salaries of neurosurgeons, with the leftovers devoted to laboratory research. Consequently, laboratory-based research that dealt with more basic science began to evaporate. The interdisciplinary environment of the MNI became more and more balkanized. Reflecting on the situation shortly before his death, Herbert Jasper noted of the MNI:

I’m a little disappointed in some parts of its development. I miss the close teamwork we had in the early days, where we had Penfield, who was interested in all the clinical and research laboratories, and he brought us all together. And our ward rounds included research labs as well as the neurologists and neurosurgeons and the psychologists. That spirit of teamwork, I don’t see today at the Neurological Institute. And that disappoints me.

Jasper’s departure for the Université de Montreal was indicative of a broader shift in Montreal’s science and technology landscape in the 1960s. The Quiet Revolution had brought a greater emphasis on the funding of science in the province, but often with the caveat that French-language

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Science was to be of paramount importance. Science became a first-priority of the provincial government, with the establishment of additional French-language universities such as the University of Quebec at Montreal (UQAM). Jasper’s move to the French-speaking Université de Montreal thus fit with a broader pattern. As Montreal became a modern ‘city of knowledge,’ it also became more linguistically segregated. While Jasper continued to do creative scientific work at the Université de Montreal, the most productive working relationship of his life gone. In a move of some irony, he even came to accept Schmitt’s term, ‘neuroscience,’ when naming his new laboratory the Centre de recherche en sciences neurologiques, although he pointedly insisted on a Francophone translation.

Despite its decline in prominence, the Montreal Neurological Institute remains at the foot of Mount Royal, still in operation. It has, in fact, outlasted the hospital that sponsored it; the Royal Victoria Hospital shuttered its doors in 2014, with its operations moved up the road to the new McGill University Heath Center - a so-called ‘super hospital.’ Brenda Milner, a fixture of the Institute since she joined in the 1950s, has only recently retired from active work, and at the time of writing, still lectures occasionally at the age of ninety-nine. She is, in many ways, a potent symbol of a time in the history of science when whole new disciplinary identities could be forged through improvisation at the boundaries of medical and scientific specialties. In 2013, she was profiled in the New York Times, under the headline “Still Charting Memory’s Depths.” One wonders what memories Milner contemplates as she leaves the MNI in the evenings and walks down University Street toward her townhouse in the city, or if she ever catches a glimpse of the St. Lawrence River.

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I have argued in this dissertation that, contra the existing historiography, the group of scientists and doctors who amassed around the MNI in the middle decades of the twentieth century developed an integrated, interdisciplinary ‘neuroscience’ considerably before the alleged creation of modern neuroscience at MIT in the 1950s and 1960s. Furthermore, I have attempted to show that what distinguished the MNI approach from the MIT approach was the particular form of interdisciplinary community it created, one which was grounded in the pragmatic concerns of the neurosurgical clinic, in the life experiences of its members, and in the relationships among them. With Montreal serving as a cosmopolitan center of contact for individuals from different national scientific and intellectual traditions, the group that developed around the MNI was capable of forging deep interdisciplinary alliances, while preserving their ability to disagree, act independently, and link to other groups. Montreal neuroscience was improvised in the crucible of the operating theater - with the help of the pathological laboratory, the psychological test, and the electroencephalogram - and it was this experience that forged links between the clinic and lab, between basic and applied science, and between the study of the brain and the study of the mind. This can be seen most clearly in the development of the Montreal method of temporal lobe operations (Chapter 2), which relied on an improvised alliance between surgery, psychology and electrophysiology, and in the interdisciplinary alliance developed between Penfield and Herbert Jasper (Chapter 3), and later extended by Jasper to encompass new disciplines, tools and techniques.

By contrast, the MIT group was more insular, disconnected from external influences, and committed to a molecular reductionist perspective. Theirs was an interdisciplinary born out of biophysics, and modelled on the unifying, reductionist perspective of molecular biology and cybernetics. MIT neuroscience privileged the molecular and eschewed the clinic; Montreal neuroscience embraced the clinic and privileged only the pragmatic.
Further, I have demonstrated that the MNI's brand of neuroscience (interdisciplinary brain research) became (outside of the United States) the leading framework for neuroscience world-wide, most notably through Herbert Jasper's organizing efforts, first with the EEG community, and later through the International Brain Research Organization. Jasper’s perspective not only served as a bridge from Montreal to the rest of the world’s brain researchers, but also fundamentally contrasted with the approach developed at MIT; Jasper’s interdisciplinarity (a product both of his own biography, and his work at the MNI) was less reductionistic than the molecular approach later advanced by his old acquaintance, F.O. Schmitt. The contrast between the Montreal and MIT styles of neuroscience can be seen most clearly in their different approaches to the fundamental question of memory. While the MIT group championed the search for a memory molecule similar to RNA (a search that ultimately proved fruitless), the Montreal group, with its multiple levels of analysis and differing perspectives, made major strides towards unravelling the brain systems responsible for memory, while still preserving their own disciplinary preoccupations and perspectives.

The effective interdisciplinary community built up around the MNI can be seen in even greater relief when it is contrasted with that of Ewen Cameron and his psychiatric circle at the Allen Memorial Institute (AMI). Penfield was initially enthusiastic about incorporating psychiatry into his interdisciplinary institute. His failed collaboration with Cameron over psychosurgery demonstrates both the willingness of Penfield to reach out to other disciplines, and the necessity for deep, reciprocal collaboration in order to cross disciplinary divides. By contrast, Cameron, who saw psychiatry as a master discipline, advanced a form of interdisciplinarity that was rhetorical and shallow; he hoped to treat researchers from other disciplines as mere technicians who could enact his own agenda, and he refused to be transformed by collaboration with them. If modern neuroscience was born in the crucible of Penfield’s institute, then the troubled relationship between
the MNI and the AMI could be seen as foreshadowing the complex and incomplete connection between psychiatry and neuroscience in the second half of the twentieth century.

Finally, it is important to note that, while Montreal neuroscience was crucial in inaugurating this new scientific field, by the late 1960s it was already becoming clear that the MIT perspective was going to become dominant, largely for reasons of scale. An anonymous editorial in the Canadian Medical Association Journal in 1969 described the pivotal work of the IBRO in conducting surveys of manpower and facilities in the brain sciences, before noting that, even adjusting for differences in population, Canada still lagged well behind the United States in terms of funding for neuroscience. 12 Once awakened, Schmitt’s NRP, and the Society for Neuroscience that followed it, would bend the direction of neurological research towards the molecular, and away from the multiperspectival, clinical form of neuroscience pioneered at the MNI. Joelle Abi-Rached and Nikolas Rose have argued that the ascendency of the NRP in the 1960s marked the “birth of the neuromolecular gaze,” an epistemic orientation or ‘thought style’ that came to characterize the modern neurosciences more generally. According to Abi-Rached and Rose, “Although the neurosciences were characterized by their founders [Schmitt et al, in this case] as an integrated multilevel approach to the brain and the nervous system, it is the molecular underpinning that prevailed…marking both the success and challenges of these new sciences of the brain.” 13 While this may be true, it is important to note that there were alternative formulations of neuroscience present at its alleged ‘birth’ in the 1960s. If Penfield and his institute gave birth to a new scientific field, as that field matured, the stamp of its origin would soon be obscured and eclipsed.

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Wired Together: Strong Assemblies, Weak Ties and Interdisciplinarity

As discussed in the introduction, I see an analogy between the activities of the MNI and its members, and the neurons of Donald Hebb’s cell assemblies.\(^1\) The members of the MNI were able to form strong assemblies of actors when their activities were locally coordinated. They became ‘wired together’ when they ‘fired’ in synchronous activity with each other, and their science and identities were altered by these interactions; they learned from, and were altered by, their work with each other. This is seen most clearly in the development of the temporal lobe research project, and in the interdisciplinary brain research of Jasper, Penfield, and their associates. By contrast, the assembly emerging at the MNI failed to fire in sync with that of Cameron and the AMI.

In the remainder of this conclusion, I would like to reflect on two interrelated issues that the history of the MNI and its members raise for other areas in the history of science more broadly, and for our contemporary discussions of interdisciplinarity and science. The first is why the MNI became such a productive site for early neuroscience, yet paradoxically, has remained so poorly understood in larger histories of modern science. How can we think about, and understand as

\(^{14}\) I am, of course, far from the first to use the actions of brain cells as a metaphor for the social interaction of individuals. Such metaphors extend back to the foundations of modern cell theory, and Santiago Ramon y Cajal, who provided the definitive scientific evidence that nerve cells conformed to cell theory, also enjoyed speculating on the similarities between cells and people. In an ironic twist, Mark Granovetter noted in his 1973 “Strength of Weak Ties” article that many of the modeling tools for early social network analysis were borrowed from biophysical attempts to model neural networks. Notably, these early network models were based on the cybernetically-inflected models of Nicolas Rashevsky, himself highly influential for Warren McCulloch and his cybernetic neural net models. If one were to pursue the analogy further, my own comparison between cells and people is closer to in spirit to Hebb’s more organic neural model, sensitive as it is to the ways in which one cell (or person) can modify the behavior of another over time. Laura Otis, Membranes: Metaphors of Invasion in Nineteenth-Century Literature, Science, and Politics (Baltimore: Johns Hopkins University Press, 2000), 8–36, 64–89; Mark S. Granovetter, “The Strength of Weak Ties,” American Journal of Sociology, 1973, 1361; Anatol Rapoport, “Mathematical Models of Social Interaction,” in Handbook of Mathematical Psychology, ed. Robert Duncan Luce, Robert R. Bush, and Eugene Galanter, vol. 2 (New York: John Wiley and Sons, Inc, 1963); Nicolas Rashevsky, Mathematical Biology of Social Behavior. (Chicago: University of Chicago Press, 1951); Nicolas Rashevsky, Mathematical Biophysics: Physico-Mathematical Foundations of Biology (Chicago: University of Chicago Press, 1960).
historians, the influence of the microhistorical phenomenon of individual actors and small research schools, and the macrohistorical emergence of a new scientific field such as neuroscience? The second issue is what the history of the MNI may tell us about interdisciplinary science, both in the past and in the present. In doing so, I would like to explore a comparison between my own notion of ‘wired together’ assemblies of historical actors, and the sociological study of networks - particularly the sociologist Mark Granovetter’s notion of the ‘strength of weak ties.’

In the mid-1970s, Granovetter noticed a paradox in human social relations. Communities that consisted of groups that were made up of ‘strong ties’ – familial relations, close friendships, and insular workplaces – were surprisingly poor at organizing for purposeful social action, adapting to changing circumstances, and influencing other groups. Granovetter explained that the failure of sociology to understand this phenomenon was because sociologists had spent much of their time examining the ‘strong ties’ between individuals, at the expense of understanding the ‘weak ties’ that linked individuals and groups. Strong ties bound up families and close groups, but could never act as bridges to other communities; ideas and innovations seldom diffused from insular, strongly associated groups to the broader social world by way of strong connections. Rather, it was by way of weak ties – casual friendships, workplace acquaintanceships, and temporary employment and apprenticeships – that ideas and innovations diffused from one strongly tied group to another. Granovetter also observed that it was by way of weak ties that organizing efforts in one strongly tied group could link to another, mobilizing entire communities for large-scale social action. By emphasizing the importance of weak ties between groups, Granovetter hoped to link the small-scale study of groups (as conducted in social psychology and anthropology) to the study of large-scale social phenomena (such as the economy and the emergence of social movements). It was through
weak ties that “small groups aggregate to form large-scale patterns.” In Granovetter’s estimation, it was actually the ‘weak ties’ between groups of actors that made them vibrant and capable of affecting large-scale social change. For the historian, tracing both the strong and weak ties of a group of actors – how they wire together to connect different worlds and perspectives – can be a powerful tool for understanding how the microhistorical world of the research school can connect to the macrohistorical development of a new scientific field. For Granovetter, the study of weak ties led to a paradox. In his words:

The major implication…is that the personal experience of individuals is closely bound up with larger-scale aspects of social structure, well beyond the purview or control of particular individuals. Linkage of micro and macro levels is thus no luxury but of central importance to the development of sociological theory. Such linkage generates paradoxes: weak ties, often denounced as generative of alienation are here seen as indispensable to individuals' opportunities and to their integration into communities; strong ties, breeding local cohesion, lead to overall fragmentation.16

Yet for Granovetter, these paradoxes were “a welcome antidote to theories which explain everything all too neatly.”17 These paradoxes should be welcomed by the historian of science as well, as they provide an analytical tool for linking the microscale case study to the macrolevel historical change of science.

Clearly, the participants of the MNI formed strong ties through their close collaboration. At their strongest, Penfield, Jasper, Milner, Harrower, Hebb, and many others could synchronize their activities, and ‘fire together.’ This created strong ties between members and altered each other’s scientific identities in turn. Penfield and the surgeons learned to respect the knowledge and tools of the psychologists. The surgeons learned the value of laboratory work, and Jasper and his electrophysiologists and chemists learned that the operating theater provided tractable problems to attack in their laboratories. If Penfield’s goal had been to use the laboratory to form a new kind of

15 Granovetter, “The Strength of Weak Ties,” 1360.
16 Granovetter, 1374.
17 Granovetter, 1378.
hybrid profession – the neurological surgeon – then by the 1950s, the synchronized work of those in his institute had produced a new scientific field – neuroscience.

Yet at the same time, the MNI was a vibrant place to work because it never became too insular or committed to a single perspective; it preserved its weak ties. If the origins of modern neuroscience could be ‘localized’ to Montreal, then it was the loosened localization of Hebb’s cell-assembly theory; weak connections to other parts of the world, and other scientific schools and traditions, could enrich its work. Examples abound. Hebb’s understanding of the multifaceted nature of intelligence, gained during his training in the United States, provided a way of understanding the effect of Penfield’s frontal lobe operations. Milner’s separate training with Hebb, and with Oliver Zangwill and Frederick Bartlett in England, allowed her to unravel the multimodal nature of memory. Jasper’s precision understanding of the electrical nature of the nervous system, gained during his time in Lapicque’s laboratory in France, made him a more sophisticated user of the electroencephalograph, and altered Penfield’s entire surgical approach to epilepsy. And of course, Jasper’s numerous weak ties to psychology, and to the electroencephalographic community, allowed him to act as a bridge through which the MNI’s perspective could diffuse to the world’s brain researchers.

Yet despite its vibrancy and historical importance, the story of the MNI and its diverse participants has remained relatively unexamined by historians. This too may be a product of its weak ties, which extended deeply into the scientific world of the mid-twentieth century, yet are more difficult to identify and trace than the more overt historical unit of the laboratory, the journal or the individual. Reflecting on his own study of the Cambridge school of physiology led by Michael Foster, Gerald Geison noted that “the nature of [Foster’s] influence becomes harder to specify the
further it extends beyond its institutional base in the Cambridge School.” Research schools and their national styles were relatively easy to identify historically; their long-term influences, less so. In a similar vein, the influence of the MNI is best traced by examining its weak ties and loose assemblies. To do so comprehensively would demand a number of additional chapters, but one final story from the history of the MNI may suffice to make the point, and it is with this instructive story that I will close this dissertation.

In 1950, David Hubel, a young McGill medical student with a passion for electronics decided to ascend University Street to meet Wilder Penfield. Penfield was, by this point, engaged in some of the most intellectually difficult work of his career – investigating the effects of his temporal lobe operations – but he found time to meet the intriguing young medical student. Hubel, for his part, was more than a bit intimidated by Penfield, and had nervously locked his keys in his running car before the meeting.19

Upon learning that Hubel had been a physics student as an undergraduate, Penfield insisted that he meet Herbert Jasper. Jasper was also intrigued by the young medical student and quizzed him on various aspects of neurophysiology before offering him a job doing electronics work in the EEG department. Hubel began to learn electroencephalography from Jasper, and soon became his primary assistant, “attending all the Penfield temporal lobe excisions.”20 As part of a seminar series in neurophysiology, Jasper instructed Hubel to investigate the visual system of the brain, kickstarting an interest that would last for the rest of his life. In 1954, Hubel met Charles Luttrell, a young

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20 Hubel, 300.
doctor from Johns Hopkins who had arrived at the MNI to learn EEG from Jasper. Luttrell and Jasper later conspired to land Hubel a job at Johns Hopkins University.\footnote{Hubel, “David H. Hubel.”}

Hubel’s work at Hopkins eventually led to a new position as part of the neuropsychiatry unit at the U.S. Army Walter Reed Medical Center. At the same time, Hubel was intrigued by continuing some of the research that he had begun in Montreal. “Single-unit recording [using a microelectrode to investigate the activities of individual nerve cells] had only barely begun in the labs of Herbert Jasper and Cho-Luh Li in Montreal…and we hoped that these new methods would soon help us understand consciousness.”\footnote{Hubel, 303.} However, it was not consciousness that Hubel came to investigate with the techniques of Jasper, but vision.

First, however, Hubel would need the right tools. Jasper had been working with glass microelectrodes, with limited success. Walter Reed, with access to more advanced materials through its Army connections, provided Hubel with refined tungsten and an insulating material called Formvar. Hubel, with an electronics background, understood how to sharpen the tungsten in an electrolytic bath of sodium nitrite. “The results were spectacular; within days I was able to make a pointed wire that looked ideal and was strong enough to pierce, with a little care, my thumbnail.”\footnote{Hubel, 303.} At the same time, “Jasper had got wind of the electrode and came down from Montreal to see it for himself and to learn how to make it. It turned out that his group was also working on a system for chronic single-unit recording….,” According to Hubel, “the Montreal group, with the help of my electrode, got there first.”\footnote{Hubel, 304.}

However, while Jasper and his group spent time investigating consciousness and learning with their electrode, Hubel continued to investigate the visual system of the cat, in accordance with

\footnote{Hubel, “David H. Hubel.”}
\footnote{Hubel, 303.}
\footnote{Hubel, 303.}
\footnote{Hubel, 304.}
the preoccupations of his American hosts, Stephen Kuffler and Vernon Mountcastle. Hubel teamed up with one of Kuffler’s postdoctoral students, the Swedish neurophysiologist Torsten Wiesel. Together, Hubel and Wiesel extended the single-cell investigation of vision from the retina and optic nerve into the striate cortex of the cat. Their results were astonishing. Careful instrumental technique, ingenious experimental practice, and a little luck soon revealed that different cells in the visual system responded selectively to different forms of visual stimuli – cells seemed, in effect, to ‘see’ different aspects of the visual world.\(^{25}\)

Hubel and Wiesel’s discovery of specific receptor cells for visual features such as angle, color, edge and form was an event of considerable importance. Indeed, as Anne Harrington has suggested, it constituted one of the most crucial neuroscientific discoveries of the 1950s and 1960s, a period in which the high technology of the Cold War began to drive the research agenda of a nascent ‘neuroscience.’\(^{26}\) Yet an examination of the history of the Hubel and Wiesel studies reveals that it had its genesis not in the high-tech world of Cold War materials and electronics, but in the clinical investigations of the Montreal school. As a member of the strongly tied Montreal group, Hubel was able to also act as weak tie to the American schools of Mountcastle and Kuffler, and could spread the Montreal tradition southward, while importing the new American tools northward. The end result of this assembly of actors - strongly and weakly wired together - was a Nobel Prize in 1981 for Hubel and Wiesel, and one of the most foundational and fascinating discoveries of the modern brain sciences. It therefore seems fitting, then, that Hubel returned to the MNI in 1981 after

\(^{25}\) Hubel, “David H. Hubel.”

he won the Nobel Prize to give the annual Hughlings Jackson lecture, entitled “Eye, Brain and Vision.” It was a homecoming, in more ways than one.

These stories are more than just stories. They are a way of investigating the links between individuals, and how these links combine into historical phenomena that are truly greater than the sum of their parts. If we take seriously, as a generation of science scholarship has argued, that science is a profoundly human activity, then our histories of science must attempt to capture those elements that are quintessentially human, and that are revealed in the intersecting biographies of scientists, and the way that their lives become wired together. Tracing these connections is a painstaking process, but I can think of no more important task for the historian of science, and of no subject more worthy of investigation than the sciences that touch so closely on human self-knowledge.

Simultaneously, the stories of the Montreal Neurological Institute pose a serious question, not only for our understanding of neuroscience, but for our understanding of interdisciplinarity more generally, and how different branches of knowledge ought to be integrated. Much like ‘neuroscience,’ ‘interdisciplinarity’ has become a buzzword in the modern world. To call a research project ‘interdisciplinary’ is to axiomatically bestow a compliment. Yet if the history of the MNI and its many lives suggests one thing for our current discussion of science and its place in the world, it may be that when we invoke the notion of interdisciplinarity, we should reflect upon what we mean. Do we hope, as Penfield did, to collaborate deeply, bringing knowledge and practices from different areas to bear upon discrete problems? Or do we merely, as Ewen Cameron did, invoke interdisciplinarity as rhetoric, or as a shallow practice, borrowing from different fields without

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properly understanding what we are doing? Do we seek to look beyond our national and professional boundaries for the tools we need, to explore those weak ties that may enrich our science, or are we blinded by provincial concerns and myopic perspectives? Do we seek, as Penfield did, to engage in respectful collaboration with different ways of knowing, or do we instead hope to transcend our different fields of knowledge, as F.O. Schmitt did, with a single unifying perspective (one that may not be appropriate for all questions)?

While the history of the Montreal Neurological Institute, and of the men and women who gave it life, certainly cannot supply all the answers to these questions, I hope that this study can provide a useful model for how one might pose such questions in other areas. The twentieth century saw a proliferation of hybrid scientific disciplines – biophysics, nanoscience, information science, physical chemistry, and many others – and these fields have assumed increasing importance in our technoscientific world. These hybrid disciplines were not inevitable, but rather the historical product of specific assemblies of men and women – sometimes working together smoothly, sometimes messily, but always under unique historical conditions. Understanding how these men and women became wired together may help us to understand how these assemblies of individuals have created (and occasionally failed to create) expansive new fields of knowledge.

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Appendix 1 – Wilder Penfield’s 1928 Report to the Rockefeller Foundation “Impression of Neurology, Neurosurgery, and Neurohistology in Central Europe.”

[The text below is a transcription of Wilder Penfield’s 1928 report to the Rockefeller Foundation on the neurological clinics of Europe. The original is contained in W/U 17, Box 155a, Wilder Penfield Fonds. Osler Library for the History of Medicine.]

Impression of Neurology, Neurosurgery, and Neurohistology in Central Europe.
Wilder Penfield - 1928

At the present moment there is no other branch of medicine in which clinic organization is of a greater importance than the study and treatment of nervous and mental diseases. And no other group of diseases is there a greater need for specialize study since nowhere else lies so many unsolved problems and so great a field for possible advance.

The report which falls must of necessity be incomplete and possibly a times unfairly individuals, based as it is upon interviews with her occasionally hurried and upon often incomplete familiarity with the publications of these of the visuals. Certain striking virtues and defects in the clinics visited are, however, self evident. If I presumed to criticize them it is against the background of my familiarity with English Neurology American Neurosurgery and Spanish Neurohistology, and not from any personal feeling of superiority. Professors Bumpke in Munich, Jacob in Hamburg, and Kappers in Amsterdam were not seen and there are of course other regrettable omissions.

The neurologist, neurohistologist, the neurosurgeons will be discussed in separate groups.
Foerster, who is both surgeon and neurologist and Brouwer, who is both neurologist and anatomical histologist appear each and two groups.

Neurology.

F.H. Levy of Berlin is a Professor Ordinarius in the second University Medical Service at the Charite Krankenhaus. He has a small ward devoted to neurological cases. The majority of the patients in his beds were of the museum variety, whose neurological symptoms were interesting but
not curable by any means yet available. There was however one case of cerebellar tumor with a high grade papilloedema. In this case Levy proposed to do a ventriculogram himself (a procedure which most American neurosurgeons would consider unwise and unnecessary under the circumstances) and then he proposed to call in the surgeon, Dr. Heimans, to operate. Dr Levy is a clever man much interested in neurophysiology. The spinal fluid is carefully studied in his clinic but the effective therapy seems to be confined to antisyphilitic procedures, early serum treatment of poliomyelitis and the usual supportive medical measures.

Bonhoeffer is Professor of Psychiatry in the University of Berlin and, according to rumor, oppose the creation of a chair of Neurology, considering this subject to fall within his proper sphere. At the same time he trains no neurologists. Oppenheim of Berlin, who was the foremost German neurological clinician before his death, had no clinic whatever and though he has trained many pupils, he left no successor. In Berlin every internist considers himself a neurologist and most surgeons are eager to operate upon the few neurological cases that are correctly diagnosed by those internists. It is impossible to accurately estimate the results but successful neurological operations are not numerous.

In Vienna a new Professor of Neurology and Psychiatry has just been elected to succeed Wagner-Yaueregg [Wagner-Jauregg]. The latter is responsible for the malaria treatment of general paralysis. His work has evidently been of great value but he is obviously more interested in Psychiatry than Neurology.

Vienna is so flooded with foreign students that the professors in general seem to have little time for anything but didactic lectures. All foreign medical men tend to be classed by them with the individuals who come to listen to their pronouncements. A professorship carries with it complete assurance of infallibility, an infallibility which has not always a firm foundation in fact. Such an attitude is particularly well illustrated in Professor Marbug. In spite of this the postgraduate teaching
I saw was well done and it is obviously splendidly organized, due in large part to the local American Medical Association.

**Arthur Schuller** of Vienna holds some type of professorship of Neurology in the University. Although interested in Neurology, he actually confines himself to Roentgenography [x-ray] of the head. His knowledge and experiences in such radiography is great and his enthusiasm unfailing. If he had adequate facility to work with encephalography and ventriculography as well he would have no equal as an X-ray diagnostician of cerebral conditions.

**Professor Redlich** of Vienna, an old assistant of Wagner-Yauregg, has a splendid service of neurological beds at Maria Theresa Schlüssel. He has 110 beds containing both organic and functional cases and with a well organized polyclinic in the same building. He is beginning to no Neuro-histology in his laboratory, somewhat as an amateur. He is an enthusiastic diagnostician and seems to have organized his assistants effectively. Denk is called in for any neurosurgery that is done.

**Viktor von Weizsäcken** [Weizsäcker?] holds the chair of Neurology at Heidelberg. There are only three such chairs in Germany, according to him.¹ The other two are those of Nonne at Hamburg and Goerster in Breslau. Weizsäcken’s department is a part of Internal Medicine. Psychiatry, on the other hand, is an independent department. His cases in need of surgical treatment are sent to the Surgical Department, and done there by various men. He has 60 beds on his service in the Akademisches Krankenhaus and three assistants. He has no interest in nor opportunity to study Neurohistology. His laboratory contains physiological apparatus capable of making various types of measurement and his assistant, Dr. Stein, does most of this sort of work. There seems to be

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¹ Dr. Goldsteiner of Frankfurt also holds a chair of Neurology, I believe.
little enthusiasm and no original points of view in this clinic. The mere creation of a department of Neurology does not seem to have improved upon the condition of affairs found in Berlin.

With regard to Neurology in Paris I have no right to venture an opinion. It is obvious that the successors of Marie and Dejerine (Guillain and Crouzon) are not the teachers that their masters were. Babinski has practically retired and it will be difficult indeed to find a successor of the same merit. Clovis Vincent, who is in Babinski’s service at La Pitie, is making a real effort to carry out neurological therapy by a close and sympathetic association with the surgeon, De Martel.

Professor M. Nonne at Hamburg is head of the University Neurological Department and has a service of 260 beds at the city hospital, Eppendorfer Krankenhaus. He has a splendid assortment of material at his disposal here, both organic and functional, with some psychiatric cases as well. Nonne is a kindly though obviously an autocratic chief. He is doubles an extremely good clinician. He has a histological laboratory where his first assistant, Dr. Pette, works on disseminated sclerosis and Dr. Schaltonbrand does some experimental pathological work. There is no routine examination of pathological specimens, however.

Professor Nonne will not permit intraspinal antiluetic therapy on his service and, in fact, permits lumbar puncture only in special cases, as in small children. Otherwise, spinal fluid is routinely obtained by the suboccipital puncture of Ayer with a degree of temerity that would make Ayer tremble. [Nevertheless], Nonne believes there have been 10,000 such punctures on his service with only one death, that being from hemorrhage, but with 10 to 12 cases where there was evidence of the spinal cord having been struck. The advantage seems to be absence of post puncture headache.

With all this wealth of material there seems to be very little therapy. Malaria treatment is employed for tabetic pains as well as general paresis. The surgical therapy is carried out in the Surgical Department of the hospital and one of Noone’s aids acts as assistant. There seems to be no
particular development of Neurosurgery. In general the clinic is obviously a good one but rather at a “stand-still.”

Professor Jacob who has his pathological laboratory in connection with the department of Psychiatry is unfortunately in South America.

In many ways the clinic of Professor Brouwer in Amsterdam has the most complete organization of any in Europe. Here ward and laboratory are interrelated in such a way that all assistants have some activity in both places. Furthermore, the plans of future clinic development include Neurosurgery as an integral part in it.

Professor Brouwer has a service of 120 beds in the Binnen-Gasthuiss. The Clinical Laboratory is situated between male and female wards and the Anatomical Research Laboratory is not far removed. From conversation with other neurologist I had understood that Brouwer, though a splendid anatomist, was deficient as a clinician. However, the clinical work seemed to be extremely well done. Whatever his ability as a diagnostician may be, there is more evidence of a well standardized neurological therapy here than in most clinics.

His assistants are given much responsibility. The chief of the Polyclinic and the chief of the ward service have each their own practice. The other three assistants are resident. They are also voluntary workers in the research laboratory. The patients are largely organic rather than functional. Syphilitic cases are put through the usual routine, including intravenous but not intraspinal salvarsan. Traumatic head injuries are kept rigorously in bed for three weeks in all cases, though lumbar puncture is never done. The results are generally good. It is noticeable that traumatic head cases are not seen in most neurological service. Brouwer keeps patients suitable for teaching purposes as long as he wishes. One such patient has been fifteen years on the war and another twenty five. Curiously enough, the latter patient, a case of multiple sclerosis, now has an hysterical fit whenever the
students appear! Lipiodol and encephalography are used as in other clinics. Casual observation seems to indicate that the nursing is of a higher order here than in Germany or France.

The laboratory is an anatomical rather than a pathological one. Autopsy material is utilized but it is studied largely from the anatomical point of view. It may be that pathology will make its appearance with neurosurgery.

Brouwer’s own research has been remarkably consistent and farsighted. He has followed the visual pathway backward from the retina by a succession of concise experiments and has been able to set others to productive pieces of work along the way. The technic in his laboratory is not to be excelled for its kind. The sections are masterpieces.

In the new Hospital Clinic Neurosurgery will be embraced within the Department of Neurology. The surgeon, Dr. Oleyneck, is completing two years at Dr. Cushing’s clinic. He will be given 5000 gilder yearly and allowed to send private patients into the service. Ward patients will be admitted to him only through the neurological service. After study here, however, he will be given, according to Dr. Brouwer, a free hand to examine and operate upon the appropriate cases as he thinks best. The incorporation of Neurosurgery with Neurology met with much opposition from other departments of the University, particularly Surgery. Dr. Brouwer told the authorities that Neurosurgery as done in the United States was much better as he had seen it with his own eyes and he must refuse to accept the French and German views on the subject of organization.

Dr. Brouwer really restricts himself to consultation in his private work. For treatment, his private patients are sent to one of his assistants. He is enabled to limit his work thus by a yearly salary of 10,000 gilder.

The distinctive features of Brouwer’s clinic is that stimulating scientific work is united with clinical work in a well organized unit. If the neurosurgeon should prove capable of realizing his opportunity, adequate therapy will make the clinic truly complete.
To describe the clinic of Professor Foerster in Breslau as briefly as the other clinics requires a good deal of inhibition, as I have spent so much time here. Foerster has recently made Ordentlicher professor of Neurology at Breslau, following an invitation to the chair of Neurology in Heidelberg. This offer he refused because he would have been forbidden to practice Neurosurgery. The creation of a chair at Breslau, however, did not carry with it a university clinic. There was already here an Ordentlicher Professorship of Neurology and Psychiatry occupied by R. Wollenberg, and with this chair a Clinic for nervous and mental cases.

Foerster has charge of a service in a city hospital, the Wenzl Hancke Krankenhaus, without University support. The 110 beds in this service are devoted to organic neurological cases, functional cases for the most part being sent elsewhere. There are four resident assistants in charge of the wards, one somewhat older Oberarzt. Dr. Schwab. a resident histopathologist who has begun to develop this much needed part of Neurology in a small laboratory. and a research assistant, Dr. Altenburger, who is interested in Neuro-physiology. None of these assistants has a practice. Foerster's private patients are in another hospital, the Charlitas Heim, situated far from his clinic and particularly badly organized in that there is no resident staff and he must bring the assistants from the clinic for operations and must carry out the daily routine care himself.

Foerster's clinic is above all a clinic in which therapy takes first place. Syphilis of the central nervous system is treated energetically by the Swift-Ellie endolumbar method in addition to the other usual procedures. Intracarotid injections of salvarsanized serum are likewise freely used. Physio- and hydrotherapy are carried on vigorously in the special rooms which are well equipped for that purpose. The wards are pleasant but the nursing is not of the highest order and decubitus is too frequently seen.

Diagnosis is thorough. Encephalography is very frequently used and in the 1500 cases of spinal injection of air there seem to have been very few bad reactions. Direct ventriculography is
also frequently used as well as lipiodal and the ventricular injection of dyes. Physiological diagnostic procedures also find a place here in a remarkably well equipped laboratory for chronaxie and other electrical measurements. These examinations are carried out by Dr. Alienburgar who is also studying the pituitary extract of cerebro-spinal fluid by biological methods.

The work of Foerster in Neurological Surgery will be taken up below under that heading. The scientific advances for which he is personally responsible are largely due to the opportunity afforded a neurologist in surgery. Practically all of his operating has been done under local anaesthesia. Thus he has used the patient as a witness to pain localization, has outlined areas of skin innervation and has determined the movements of the body which follow electrical stimulation of various areas of the cerebral cortex. This analysis of the cortical areas has made possible an intelligent advance in the treatment of epilepsy. His study of pain paths has made it possible to relieve certain types of pain more intelligently.

The members of this clinic are familiar with foreign literature as well as the local traditions. Foerster is the principal editor of the “Zeitsohrift für die gesampte Neurologie und Psychiatrie.” Above all, here Neurology is accompanied by therapy.

**Neurohistology.**

Professor E. Spielmeyer of Munich is head of the Forschungsanstalt fur Psychiatrie und Neurologie in Schwabing, a suburb of Munich. This Institute is largely devoted to neuropathology and histology. The splendid building and quiet surroundings as well as the pleasing personality of the director make this a most agreeable spot for study. Spielmeyer is primarily interested in the pathology of the neurone and concerns himself little with pathological alterations in neuroglia. Because of its isolation there seems to be little direct connection with clinical problems and perhaps
clinical points of view. Spielmeyer’s textbook of Neuropathology is a thoroughgoing attempt to cover the subject and the views of the German School are well set forth.

There could be no better testimony to the stimulating atmosphere of Spielmeyer’s Laboratory than the fact that Dr. H. Spatz of Munich developed there. The latter is now head of the Laboratory in the University Psychiatric Clinic in Munich. He is young and enthusiastic and, like Spielmeyer, seems to be open minded. His primary interest has been neuroglia and he differs somewhat with certain of the Spanish histologists and with some of my own views on neuroglia. Work in his laboratory is sure to be fundamentally productive, although he is at present a little too much bound by the traditions of Alzheimer.

Spatz is apparently already the accepted authority in Germany on neuroglia and he seems likely to succeed to the role played by Alzheimer in this respect, a role made easier by the German habit of recognizing accepted authorities. The work from his laboratory has a aura to be of value although he is at present somewhat limited by the tradition of his school.

Professor G. Marburg of Vienna is a satisfactory teacher of neuropathology to beginners in the subject, satisfactory to the beginners because he speaks as the final authority and presents subjects clearly. He is in charge of the Neurological institute which is made up of large rooms on two floors and contains the beginnings of a good library. He has almost no financial support and little assistance. All work must be done by himself and his voluntary assistants. He is interested in clinical work and gives outpatient consultations on the service of professor von Eiseleberg.

Marburg must be an indefatigable worker to judge from his literary production, and deserves much praise for his perseverance in the face of post bellum difficulties. Nevertheless, it must be said that he is not open-minded and does not understand some of the recent work in his field. His communications are full of assertions and crystallized opinions. His technique, so far as one can
judge, is not very good. His work is profound without being critical, a contrast with the attitude in Munich. His is not a laboratory to be recommended for research.

Professor Oberling of Strassburg has been lately called to Paris. He is an histologist but not really a neurohistologist. He has described certain tumors of the meninges without careful study of the embryology.

Professor G. Rousey of Paris has a charming personality and a large and well-equipped laboratory. Like many French histologists, he is interested in description of pathological cases, without ever having made a patient fundamental study of the embryology and nature of the cell types concerned. This attitude applies evidently to others who have worked in his laboratory, to judge from their publications.

Professor J. Nageotte of the College de France holds the chair of Comparative Histology. His work has been largely upon peripheral nerves, however. Old, very deaf and somewhat infirm, he is nevertheless fired with enthusiasm in his work and is mentally alert, still pursuing the subject of his earlier discussions with Cajal upon the peripheral nerve. There is much to be learned in his laboratory in this particular field. He does not go into pathology.

Professor M. Hovelacque in the Laboratoire d’Anatomie, Ecole de Medicine, is not a histologist but a gross anatomist of a type that is now rather rare. He has a profound knowledge of the gross anatomy of the peripheral and sympathetic nervous system as evidenced by his splendid book on that subject. He is an enthusiast in the dissecting room and I know no laboratory where the gross anatomy of the nerves could be so well and pleasantly studied.

The work of Professor Brouwer in Amsterdam has already been mentioned. His work has been analogical histology splendidly done and closely related to human physiological problems.

Professor Phillip Stöhr of the University of Bonn is, like Nageotte, an histological anatomist with no clinical or pathological interest. His work upon the microscopical anatomy of the
sympathetic nervous system and the innervation of the pia mater and blood vessels is important. He is an excellent technician, young, enthusiastic and critical. He would be an excellent man to work with, though for the moment he is studying the development of the vascular system.

Professor M, Bielschowsky of Berlin has long been in Germany the accepted authority on the finer structure of nerve cells, thanks to the use of his own silver method for staining these cells. He has a small laboratory with one technician in the top floor of the Kaiser Wilhelm Institute fur Hirnforschung of which Oscar Vogt is the chief. He carries on his own private practice in neurology but has no clinic affiliations. His work has always thorough and original. He is primarily a cytologist but has worked long upon such pathological specimens as are sent to him. His relationship to Vogt has for many years been that of symbiosis with tolerable friction. Bielschowsky, though he has always worked quietly, is too much aware of his international standing to play 'second fiddle' gracefully.

He cannot be said to be either an experienced neurologist nor a well-rounded neuropathologist, but he stands in the front rank of neuro-histologists and is a splendid technician. His laboratory is a good place for a serious worker to study the finer structure of nerve cells.

Professor Oscar Vogt is a unique figure in neurohistology. A man of profound learning, very critical of all investigation including his own, demanding perfection in technique and unfaltering diligence from all associated with him, he has created a remarkable institute for the study of cerebral architectonic. The fourteen assistants and technicians have each their own special field. Vogt is entymologist as well at neurohistologist and has three assistants mounting various types of insects of which he has already over one million specimens arranged in rowe, companies, and regiment, each limb and feeler posed in identical attitudes so that a glance at those glistening backs shows one the various changes of color and shape which an individual species presents in different areas of this and other continents. This collection is not confined to the laboratory but has found its way into two
rooms of his apartment, and some of the finest specimens are to be found behind the wooden doors of the tall cupboards that shoulder each other about his living room.

However, etymology is for Vogt only a hobby. He is Director of the Kaiser Wilhelm Institute für Hirnforschung. A new institute will be ready for occupancy in a few months. He will then have associated with the laboratory a limited number of beds for neurological cases of any type that it is desired to study. He also expects to have further assistants, particularly one well trained in physiology.

Cerebral architectonic, which Vogt is studying, includes the structural relationships of the nerve cells in all parts of the brain. Brodmann, a former assistant of his, has written a book upon this subject. Professor von Economo has also published exhaustive charts of brain areas. But Vogt will never be satisfied until he has mapped out the pattern of nerve cells in every area of the brain and made an attempt to name the function to which these different patterns are devoted.

Two incidents will illustrate the direction of his thought. He drew up an outline of the cell pattern areas as he conceived them on the surface of the brain and sent it in a letter to Professor Foerster, asking him whether or not these areas corresponded to the areas which Foerter was outlining upon the human cerebella cortex by electrical stimulation. Curiously enough, Foerter was at the same time writing a letter to Vogt with a similar request. On Foerter's brain outline were sketched the areas of the so-called motor cortex, but also the areas on the rest of the cortex from which stimulation produced well coordinated movements of the patient's body. The two letters crossed in the mail and the two diagrams corresponded to an astonishing extent. Thus Vogt had more names for function to attach to his patterns.

The second incident has to do with the brain of Lenine. Vogt was summoned by the Russian Government to examine the brain of the dead leader. Before he returned to Berlin with the specimen he was asked what he expected to find. He said that there would probably be a very well developed
third cortical nerve cell layer. Sections of this brain do show a remarkable development of this layer, and sections of criminals with low grade mentality in Vogt’s collection seem to show under-development of the same layer.

To prepare one brain for such study requires the exclusive work of one technician for one year and costs about 14,000 marks in material and labor. Sections are cut through the whole brain and every fifth section it kept, and so large a photographic print is made that the cells stained by Nisell's method are easily studied without reference to the slides. Three of the technicians have been with Vogt for twenty years. Two assistants do nothing but photograph sections. The preparations are literally crowding them out and into the new Institute.

Dr. Cecile Vogt, the wife of Otto Vogt, is a French woman who also works in the Institute. She has done work of great importance. Their publications have usually appeared jointly at from 0. and C. Vogt. Their work on diseases involving the basal ganglia is of great importance in clinical neurology and neurophysiology. She has maintained independence of thought in the presence of so compelling a personality as her husband. The daughter, an only child, has likewise begun to work on cerebral architectonic. Both Otto and Cecile Vogt have a private practice and do just enough to supply their financial necessities. Dr. M. Rose is first assistant in the Institute and manages the publication of the Journal fur Psychologie und Neurologie, of which the two Vogts are the remaining editors.

The Institute under Otto Vogt is a most remarkable example of well organized scientific investigation. To copy such an organization would, however, be futile for the personality of the Vogts could not be transplanted and without it their organization would be sterile.

Neuro-Surgeons.
If the term neurosurgeon were taken here to describe those men who limit their surgical work to neurology the list of neurosurgeons on the continent of Europe would be reduced, I believe, to two men, Foerster and Pussepp, both of whom were primarily neurologists.

The first man in Europe to venture into neurosurgery were Krause of Berlin and von Eiseleberg of Vienna. They began their work under the pioneering stimulus of Horsley in London and Mac Ewen in Glasgow. Krause has now retired. Von Eiselsberg is still Professor of Surgery at Vienna.

When I asked Professor von Eiselsberg if I might see him do some neurosurgery, he waved his hand and said, "You had better go to Cushing…His results are better than mine because he makes his own diagnosis…I never do neurological cases when I can avoid them. Come and see me remove a stomach tomorrow at eight.” One of von Eiselsberg's assistants spent a year with Dr. Cushing. Nevertheless, most of the neurosurgery in Vienna is now done by a former assistant of von Eiselsberg, Dr. Denk.

Professor W. Denk does general surgery but is called by various neurologists to operate upon “their neurosurgical cases.” He is a rapid, rough, precise operator, with little or no interest in neurology.

More stimulating neurosurgical work in Vienna is being done by a rhinologist, Professor Oscar Hirsch, who takes the pituitary into his field. He was the first to operate upon the pituitary through the nose, though Cushing has subsequently taken it up. Hirsch has continued his work with much success but little or no publicity. I saw him remove a tumor of the pituitary, thus saving the eye-sight of his patient, in one hour. The patient sat in a chair, under local anesthesia, and held a basin for the operator. She then walked back to the ward and went to bed. This was rather a startling contrast to the elaborate ritual that attends Cushing’s use of the method under general anesthesia.
Hirsch publishes rarely. His mortality is, however, very low and his results uniformly good. He applies radium through the nose to the tumors after operation. Those cases of pituitary tumor which present above the sella he is of course unable to treat, not being trained for intracranial approach to this region. The surgery of the sella obviously belongs in the field of neurosurgery but too many neurosurgeons elect the more dangerous intracranial approach because of their lack of familiarity with the nasal cavity. In the general indecision between neurologists, rhinologists and neurosurgeons, efficient treatment of pituitary tumors has often been impossible, even when so good a rhinologist as Dr. Hirsh is available.

Two pioneer in neurosurgery in France is Dr. T. de Martel who has reproduced neurosurgical technique there without giving up his broad general surgical activity. He is technically very clever and gives a dramatic operative clinic. He operates in his own hospital at 219 rue Vereingetorix without University affiliation. He is closely associated in his neurosurgical work now with the neurologist Clovis-Vincent, who also acts as his operative assistant.

De Martel, in his address before the Paris Neurological Society this year, described his earliest operations on brain cases as rapid and brilliant but invariably followed by death. He consulted Horsley who told him such operations must never be rapid. Thereupon Dr Martel operated less brilliantly and his patients began to live. Now however, he said, he has gone as far as a general surgeon can go with neurosurgery. Now must Clovis-Vincent himself begin to operate.

De Martel is interested in devising new instruments but seems to lack a grasp of the problem important to the individual patient. Most of the operations I have seen him perform have been well executed but have missed their goal. Nevertheless, he deserves great credit for being able, single-handed to achieve success in neurosurgery without institutional or University backing. He has had to pay for the time consumed and the expensive equipment involved in neurosurgery from the proceeds of simpler forms of surgery.
Rene Leriche, professor Surgery at Strassbourg, may perhaps also be called a neurosurgeon. He recognizes no limitation to the field of general surgery and considers the central nervous system part of his proper province; but his real field of activity has been the sympathetic nervous system and vascular surgery.

He is chiefly known because of the operation of periarterial sympathectomy, which he devised, a procedure which is occasionally of unquestionable value. Its sphere of usefulness is less, however, than was at first supposed and he himself is much more conservative in its application than he was at the time of my visit to him in Lyons in 1924.

He is concerned with the anatomy of the sympathetic and he spins its physiology from clinical findings. He has little knowledge of pathology. It must be admitted that at times his reasoning is paradoxical and even self contradictory, but he is a brilliant opportunist and empiricist, a good surgeon and a keen observer and evidently stimulating to the group of young men gathered about him.

He has discovered that section of the cervical sympathetic, producing as it does a ptosis, is of great help in paralysis of the facial never where the eye cannot be otherwise voluntarily closed. He has concluded that reflex vaso-dilation such as occurs, he feels, in traumatic decalcification can be remedied by sympathectomy just the same as the opposite condition of reflex vasoconstriction.

The experimental physiology being done by Fontaine and other assistants in his Laboratory is in need of critical analysis which he does not seem to be able to provide. He himself feels that the future of the surgery of the sympathetic nervous system depends upon its coordination with neurology in general and that it will form a very large part of neurological surgery.

Dr. Paul Martin, a surgeon in Brussels, has brought to that city experience learned in Cushing’s clinic and a determination to do good neurological surgery. There is here no chair of Neurology. L. van Bogaert is a neurologist attached to medicine. Lay, Professor of Psychiatry, has
little interest in Neurology. Martin states that practitioners of medicine do not hesitate to call him in to consult upon neurological cases although to call in van Bogeart or Bremer or another “medical” neurologist would seem to them admission of their self evident neurological ignorance. Martin does not consider himself a neurologist.

Dr. G. Brunning of Berlin is a surgeon interested in surgery of the sympathetic nervous system. He lacks the brilliance of Leriche and is only a tolerable operator. I must confess not to have read his book upon the subject.

Professor O. Foerster of Breslau has already been spoken of at some length under Neurology. For years he acted as assistant to the surgeon whom he called in to operate upon his own cases. At length he determined to dispense with the operator and do it all himself. This he did shortly before the war. With the coming of the war he was put in charge of a large neurological hospital and did all of his own operating. In taking over surgical therapy he began to carry out all procedures needed by the neurological patients, i.e., such orthopedic procedures as have to do with tendon surgery in addition to what is usually called neurological surgery.

His operative techniques at present differs greatly, as might be expected, from that of American neurosurgeons. It resembles more the manner of operating of Horsley and Sargent in England. He is a slow painstaking operator who uses no bone wax, no silver clips for blood vessels, no suction in the operative field, makes no osteoplastic cranial flaps and closes the scalp in a manner not sanctioned by the school of Cushing. He tolerates little help from his assistants and his nurse stands behind him handing what is needed awkwardly over his shoulder. Nevertheless, his surgery is good. He shows the greatest respect for tissue in general and the brain in particular. He is radical, even brilliant, in the removal of tumors and daring when occasion demands. Wound closure is meticulous and wound healing on the whole good.
There are certain operations which are better done elsewhere. There are numerous technical
devices which Foerster could use to the advantage of both himself and the patient. The craniotomy
patients later bear large cranial defects and the scars may not be well hidden. Moreover, in his almost
universal use of local anesthesia, the patients sometimes suffer acutely, although it must be admitted
that the mortality is there probably lowered. In general the diagnosis is accurate, the treatment
radical and the results good. In focal epilepsy his fearless radical excisions of the scarred brain area
marks a step forward in the treatment of this scourge. So far as my experience goes neurosurgery in
this clinic is unequaled outside of the United States and such a well balanced combination of sound
neurology and neurosurgery is to be found nowhere. The complete lack of neuropathology in this
clinic is its greatest drawback.

Discussion.

From the care of the neurologist have been taken away many of those ailments which are
now susceptible of cure by modern methods. Thus, treatment of epidemic cerebro-spinal meningitis
is carried out in internal medicine, where there is familiarity with serum treatment. Poliomyelitis also
is to some extent come the province of the serologist and even syphilis of the central nervous
system. Orthopedic surgery has largely taken over the treatment of spastic paralysis. It is chiefly
those disease for which adequate therapy is lacking which are left undisputed in the province of
Neurology.

Tumors of the nervous system, because their diagnosis requires much specialized knowledge,
have continued to belong to the neurologist, but with the increased efficiency of neurosurgery in the
United States, the neurologist has already sometimes been set aside even for those cases.

For the so-called functional neurological cases there has also been developed a therapy
sometimes adequate and often inadequate, but nevertheless therapy. It includes psycho-analysis,
child welfare work, hypnosis, gland therapy, etc. Many neurologists have entered this field and for the most part lost touch with organic neurology. Many psychiatrists have come into the field of neurosis but have brought little interest in organic neurology. In this presence of this partial dismemberment of neurology during the past thirty years it is perhaps surprising to find so few centers of neurological thought in Europe. The German Universities have, for the most part, created chairs of Neurology and Psychiatry but the occupants have proved to be interested in psychiatry rather than organic neurology. At present one searches in vain for great teachers of neurology such as Erb and Oppenheim in German and Charcot, Marie and Dejerine in France.

Of the neurological clinics which I have visited, three stand out for special consideration – those of Nonne in Hamburg, Brouwer in Amsterdam and Foerster in Breslau.

The clinic of Nonne is well housed and sufficient in regard to size and personnel. It is essentially a diagnostic clinic. There is here little or no therapy. Neurohistology, although a laboratory exists, forms no vital part of the clinic. Thus the clinic, though admirable for the study of clinical signs and syndromes, accomplishes little except to give rest to the weary and is scientifically nearly sterile.

The clinic of Brouwer is also well situated and well manned. Here however there is a splendid laboratory of neurohistology, the chief understands both the microscopical and clinical aspects of neurology and the assistants work in both fields. There is in this clinic scientific stimulus to new thought. Surgical therapy is still lacking although plans for such therapy within the clinic have been made.

The clinic of Foerster is not so well housed. The neurology is however unexcelled, diagnosis is accurate and verified by modern methods. Neurosurgery has reached a state of development not
to be equaled in any other European clinic. Neurohistology is sadly lacking, although a small start has been made.

Neuropathology is being very well done in the Laboratories of Spielmeyer and Spatz at Munich and apparently Jacob at Hamburg and neurohistology brilliantly done in the Laboratories of Vogt and Bielschowsky at Berlin, neurohistology of a somewhat different type in the Laboratories of Kapers and of Brouwer in Amsterdam. Nowhere has general pathology developed work worth while in this field. These men are all essentially neurologists but the microscopy of the brain must belong to both psychiatry and neurology.

The neurological clinic of Brower is stimulating because an approach to neurological problems is made through adequate histology. The neurological clinic of Foerster is dynamic because it has appropriated surgical therapy. That neurology and neurosurgery can be well combined is proven by this Breslau clinic and here the combination is a real one, the chief and his assistants being neurologist and surgeon at the same time. This gives to neurology a therapy of its own. The diagnosis and care of the organic neurological cases which are not and those which are susceptible of surgical attack requires the same clinical and pathological training. Those neurosurgeons in the United States who have taken up this specialty without neurological training are gradually becoming neurologists, thanks to the fact that they are enabled by the circumstances of professional and university life in the country to restrict their work to neurological cases.

The growth of neurosurgery in the United States during the past twenty years, the lack of development of neurology during the period and its failure to become established as a well recognized unit in university teaching both point toward the fact that neurology and neurosurgery should combine, a fact that is already theoretically obvious, as neurosurgery is blind without neurology and can hope to make no advance, and neurology ineffective without surgery. The preliminary training for the personnel of such clinics must be in neurology, surgery, and
neuropathology. Neurosurgery obviously cannot be well done by a surgeon not devoting his entire
time to that specialty. That seems well demonstrate by the condition of neurosurgery in Europe at
present, and even in such large centers of population as Berlin and Vienna where Krause and von
Eiselsberg have attempted it.

In the ideal neurological clinic which could include surgery and adequate histopathology,
what shall be the boundary line between it and psychiatry? Obviously, there can be no hard and fast
line. The two specialties must overlap on the milder functional cases and they must both interest
themselves in the study of the brain. Foerster, whose preliminary training would have fitted him as
well for psychiatry as neurology sends away the purely mental cases and confines himself largely to
the demonstrably organic. The same is true also in Brouwer’s clinic.

My personal conclusion is that the division, which can never be hard and fast, must
eventually fall between organic and functional. The mental and functional cases then would be
treated by men skilled in psychotherapy and conversant with existing social conditions. It may be of
course that some of these cases will be found to be organic some day, which is the strongest
argument for complete facility for neurohistology in psychiatry departments.

The combined neurologist and neurosurgeon, because he is capable of administering the
therapy will also naturally have the organic neurological cases under his care. There may be two
neurologists only one of whom operates but there should never be a neurologist and a surgeon
ignorant of neurology. The relationship must depend on personalities. The argument that neurology
must be part of medicine and that neurosurgery must be an integral part of surgery, if adhered to,
will eventually end in the development of neurosurgery and the disappearance of neurology on the
principle that therapy determines the care of patients. Medicine will then call in two consultants, the
psychiatrist and the neurosurgeon who, paradoxically, will have become neurologist as well. The
combination of neurology and psychiatry without surgery has tended to result in the disappearance of interest in organic neurology, at least in Germany and Austria.

Certainly nowhere are there more problems remaining to be solved than are presented by the maladies that affect the nervous system and the mind. No arrangement will ever be adequate to solve these questions, which does not ignore the artificial partitions of Medicine, Surgery and Pathology. The Pathological Department which takes to itself the autopsy material, and the Surgical Department which bars the neurologist, put almost insurmountable difficulties in the way of the solution of these problems. The neurologist must be familiar with medicine and he must have general surgical training. The preparation is long and there are no short cuts.

Give to psychiatry neurohistology and the means of mental therapy and to neurology neuropathology and complete facility for surgical therapy. Let them be as closely associated with medicine and surgery, as is possible in hospital life, but allow them complete facility for experimental and therapeutic study of their own problems. Then we can hope for solution of some of the riddles presented by the sufferers from nervous and mental diseases.
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