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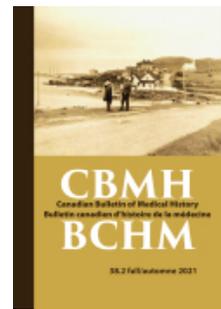
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for a Better Lobotomy

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Two Solitudes: Wilder Penfield, Ewen Cameron, and the Search for a Better Lobotomy

Yvan Prkachin

Abstract. In the 1940s, Wilder Penfield carried out a series of experimental psychosurgeries with the psychiatrist D. Ewen Cameron. This article explores Penfield's brief foray into psychosurgery and uses this episode to re-examine the emergence of his surgical enterprise. Penfield's greatest achievement – the surgical treatment of epilepsy – grew from the same roots as psychosurgery, and the histories of these treatments overlap in surprising ways. Within the contexts of Rockefeller-funded neuropsychiatry and Adolf Meyer's psychobiology, Penfield's frontal lobe operations (including a key operation on his sister) played a crucial role in the development of lobotomy in the 1930s. The combination of ambiguous data and the desire to collaborate with a psychiatrist encouraged Penfield to try to develop a superior operation. However, unlike his collaboration with psychiatrists, Penfield's productive working relationship with psychologists encouraged him to abandon the experimental "gyrectomy" procedure. The story of Penfield's attempt to find a better lobotomy can help us to examine different forms of interdisciplinarity within biomedicine.

Keywords. Wilder Penfield, neurosurgery, epilepsy, lobotomy, Ewen Cameron, psychosurgery, interdisciplinarity, neuroscience

Résumé. Dans les années 1940, Wilder Penfield effectua une série de psychochirurgies expérimentales en collaboration avec le psychiatre Ewen Cameron. Cet article explore la brève incursion de Penfield dans la psychochirurgie et utilise cet épisode pour réexaminer l'émergence de son initiative chirurgicale. La plus grande réussite de Penfield - le traitement chirurgical de l'épilepsie - a en fait les mêmes racines que la psychochirurgie et les histoires de ces deux traitements se recoupent de manière surprenante. À l'ère de la neuropsychiatrie financée par la fondation Rockefeller et de la psychobiologie d'Adolf Meyer,

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les opérations du lobe frontal effectuées par Penfield (dont l'une est réalisée sur sa sœur) jouèrent un rôle crucial dans le développement de la lobotomie au cours des années 1930. La combinaison de données ambiguës et le désir de collaborer avec un psychiatre encouragèrent Penfield à tenter de développer une opération plus efficace. Cependant, contrairement à sa collaboration avec les psychiatres, les rapports fructueux de Penfield avec les psychologues l'amènèrent à abandonner la procédure expérimentale de la « gyrectomie ». Les efforts déployés par Penfield pour raffiner la lobotomie nous invitent ainsi à examiner les pratiques interdisciplinaires en biomédecine.

Mots-clés. Wilder Penfield, neurochirurgie, épilepsie, lobotomie, Ewen Cameron, psychochirurgie, interdisciplinarité, neurosciences

In November of 1963, Herbert Jasper, then perhaps the most respected neurophysiologist and electroencephalographer in the world, gave an address at Montreal's Allan Memorial Institute of Psychiatry (AMI). Located on the slopes of Mount Royal, the Allan, as it was known to locals, also went by another macabre moniker: Ravenscrag. Built in 1863 by the shipping magnate and financier Sir Hugh Allan and donated to McGill University in 1940, Ravenscrag became home to Canada's first dedicated university department of psychiatry in 1943. Headed by the Scottish-born psychiatrist Donald Ewen Cameron, the AMI was a short seven-minute walk from Jasper's home base, the Montreal Neurological Institute (MNI).

Jasper's address was entitled "Neurology and Psychiatry: Two Solitudes?" and began on a positive note. The MNI had followed the development of its "younger, sister Institute" with a "sense of family pride." Indeed, the MNI's founder, the neurosurgeon Wilder Graves Penfield, had helped select Cameron to direct the AMI. However, Jasper's tone quickly turned mournful: "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. . . . Neurology and Psychiatry have been developing, to a large degree, as 'two solitudes.'"¹ Jasper's pointed allusion to Hugh McLennan's 1945 novel *Two Solitudes* would not have been lost on his Montreal audience; since its publication, the struggles of its central character to navigate his French and English identities in the bilingual city of Montreal had become a shorthand for a nation divided by language.² It was with this pregnant metaphor that Jasper described the relationship between neurology and psychiatry, and between the AMI and MNI.

The schism Jasper described might seem to fit a broader pattern in 20th-century medicine: as psychoanalysis came to dominate American psychiatry after World War II, neurology and psychiatry became more and more estranged.³ Yet, as this article will show, the falling-out between the AMI and the MNI actually stemmed more from a local set of circumstances surrounding the brief attempt by Penfield and Cameron to collaborate on a series of experimental psychosurgeries in the 1940s. I will use the history of this brief collaboration, both to explore the nature of interdisciplinarity in medicine and to reframe our understanding of a Canadian medical icon.

Penfield's biographers have frequently emphasized his vehement opposition to lobotomy, a stance that Richard Leblanc has recently described as "swimming against the current."⁴ My goal here is not to contradict these scholars. Indeed, the story of Penfield's brief foray into psychosurgery reinforces his image as a remarkably thoughtful surgeon. However, a closer examination of Penfield's history with psychosurgery also reveals the remarkable extent to which his own surgical enterprise overlapped with the historical origins of psychosurgery itself. Penfield's greatest achievement – the surgical treatment of epilepsy – grew from the same roots as psychosurgery, both institutionally and intellectually, and the careers of Penfield and lobotomy overlapped in crucial and surprising ways. As will be shown, this overlap was the product of the growing trend toward interdisciplinarity that was at the heart of the Rockefeller Foundation's mid-century program in neuropsychiatry and produced both Penfield's epilepsy surgery *and* lobotomy. However, not all brain operations, or interdisciplinary collaborations, were created equally. While the same scientific and social forces fuelled the creation of epilepsy surgery *and* lobotomy, Penfield's deep interdisciplinary style was incompatible with the shallow interdisciplinarity of his collaborator, Ewen Cameron.

Since Cameron's involvement in the CIA's MK-Ultra "mind control" program was revealed in the 1970s, many have retroactively distanced Penfield from him. Retrospectively painted as either a mad scientist or a Cold War monster, Cameron might seem the perfect foil to Penfield's saintly image.⁵ Yet the emphasis on Cameron's role in later brainwashing experiments obscures as much as it reveals. By re-examining Penfield and Cameron's brief experiments with psychosurgery, we can gain a greater appreciation for the differences between the two men, and a deeper understanding for how

interdisciplinary trading zones might grow, thrive, or fail in the biomedical sciences. For much as the “two solitudes” metaphor suggests, a shared commitment to collaboration was insufficient if the players involved refused to speak the same disciplinary language.

Borderlands

Penfield’s longest-lasting contribution to medicine – the surgical treatment of epilepsy – grew from parallel developments in the early 20th century. The first was the growth of neurosurgery itself. By combining the localization paradigm of early clinical neurology (in which lesions were located within the brain through careful testing) with the surgical innovations of Harvey Cushing (who transformed neurosurgery into a relatively safe procedure), neurosurgeons offered effective therapies that made them a growing threat to their neurological colleagues.⁶ By offering heroic treatment in place of therapeutic nihilism, neurosurgery seemed poised to swallow up the patient population being served by neurologists (whose treatment of the milder neuroses within private practice settings was itself being threatened by the growth of psychiatry). With Penfield as one of its brightest young stars, neurosurgery continued to expand its horizons in the 1920s, on the lookout for new disorders amenable to surgery.

The second development was the growing influence of the Rockefeller Foundation’s program in psychiatry. In the 1920s, the Rockefeller Foundation (RF) sought to reorient American psychiatry away from its previous home in the asylum and toward a more expansive role that would transform psychiatrists into experts not only on the intractably insane but also on those disorders that sat on the “borderlands” of psychiatry: the milder neuroses, alcoholism, and epilepsy. Here, the RF drew scientific justification from the Swiss émigré psychiatrist Adolf Meyer, whose philosophy of “psychobiology” was key. Psychobiology, a monist philosophy that drew upon American pragmatism and new developments in the neurological sciences, argued that mind and body were inseparable and that mental disorders lay on a continuum from the severe to the mild. Psychiatry’s purview was no longer discrete disease entities, but rather the “maladjustment” of a patient to her surroundings. Emphasizing the biological plasticity of the brain, this new neuropsychiatry would engage the patient’s social world as much as it would her neuroanatomy and would utilize a pluralistic approach to treatment; a medical approach (including surgery) might exist

alongside work therapy or habit training. At the same time, Meyer's psychobiological vision also demanded interdisciplinary collaboration between the clinic and the laboratory; the neuropsychiatrist was to be as versed in cutting-edge neurophysiology as he was in taking Kraepelinian case histories.⁷ Under this expansive rubric, the RF's director of medical research, Alan Gregg, began to fund an emerging generation of neuropsychiatric research projects connected to different North American universities. Penfield's MNI, the first and most lavishly funded of the RF's initiatives in psychiatry, remained until Gregg's death his proudest accomplishment.⁸

Epilepsy occupied a particularly notable place within the expansive psychobiological borderlands of Gregg and the RF. The "sacred disease" of antiquity had been partially tamed by 19th-century neurologists, who came to understand focal epilepsy as the result of specific lesions in brain tissue that produced uncontrolled electrical discharge. However, while focal epilepsy yielded to a neurological understanding, the vast majority of epilepsies were idiopathic, could not be easily localized, and existed somewhere between the obviously neurological disorders such as aphasia and the more nebulous "psychoneuroses" that would eventually attract the attention of lobotomists.⁹ Indeed, as Penfield's close friend, the Harvard neuropsychiatrist Stanley Cobb, later put it in a book touching on epilepsy, psychoneuroses, and other "borderland" conditions, "Epilepsy is [a] borderland, claimed and disclaimed by neurologists, psychiatrists and internists."¹⁰

Epilepsy also occupied a social position comparable to that of other psychiatric conditions. Viewed with hostility and suspicion in the late 19th century, epileptics were, alongside the "feeble-minded" and the schizophrenic, thought to pose a eugenic and social threat. Much like the intractably insane who populated America's state mental hospitals, epileptics were frequently removed to large colonies, such as New York's Craig Colony, opened in 1894.¹¹ A diagram in Cobb's *Borderlands of Psychiatry* made the situation clear: an inner circle of hospitalized mental patients was surrounded by an expansive outer circle of psychoneurotics, alcoholics, stammerers, and epileptics (Figure 1).¹²

It was in this context that Penfield's surgical treatment for epilepsy grew. Indeed, in 1922 Penfield nearly joined the faculty of Johns Hopkins, which would have given him a close connection to Meyer's Phipps Clinic; as he later recalled, Meyer had proposed the creation of a "neuropsychiatric clinic with neurology and psychiatry on an equal footing," an appealing prospect to the ambitious young

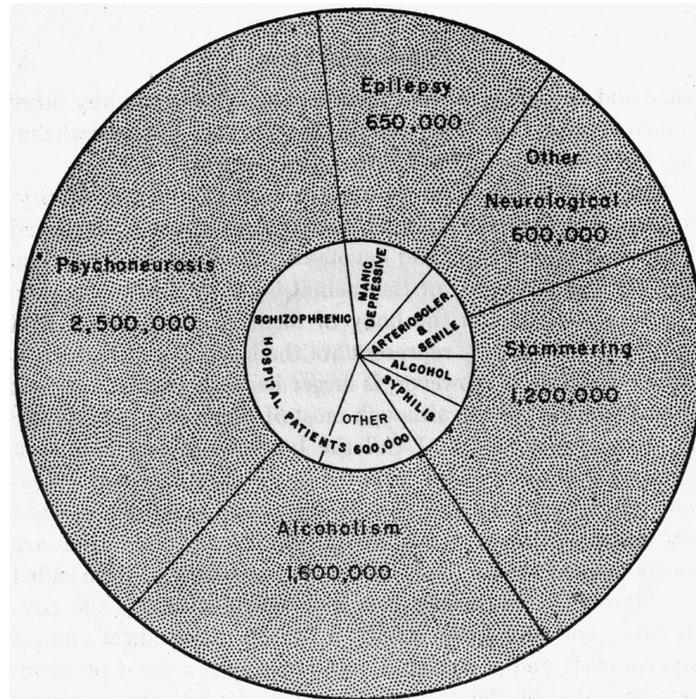


Figure 1: Stanley Cobb's diagram of the "borderlands" of psychiatry. Note the prominent position of epilepsy among these borderland conditions

(Stanley Cobb, *Borderlands of Psychiatry* [Cambridge, MA: Harvard University Press, 1943], xii. Copyright © 1943 by the President and Fellows of Harvard College. Copyright renewed © 1970 by Stanley Cobb. Used by permission. All rights reserved.)

neurosurgeon.¹³ The scheme never came to fruition, but Penfield kept Meyer informed of his plans to apply the advanced neurophysiological knowledge then being generated in the laboratories of Europe to expand the scope of neurosurgery. Trained by the grandmaster of neurophysiology, Charles Sherrington, Penfield was an accomplished laboratory scientist, and in 1924 he took the unusual step of travelling to Madrid using money provided by Isabella Rockefeller to study in the laboratories of Santiago Ramon y Cajal's Spanish school of neurohistologists. Since the turn of the century, Cajal and his cadre of laboratory workers had used new cellular staining techniques to elucidate the microstructure of the nervous system. Penfield hoped to adapt these laboratory techniques to the clinic, with the goal of refining the surgical treatment of neuropsychiatric disorders.¹⁴ Returning to New York in 1924, Penfield proposed

the creation of a neuropathological laboratory at the Presbyterian Hospital that would make use of the new Spanish methods:

There remain the large group of mental and nervous ailments about the causes of which little or nothing is known. It is evident that the complaints of these 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.¹⁵

For Penfield, epilepsy sat on a continuum of neuropsychiatric disorders that might soon yield to his scientific approach and become surgically treatable. As a result, his program was, according to Gregg, “the sort of thing for which we are always searching at the Rockefeller Foundation.”¹⁶

Penfield added a new surgical procedure in 1928, incorporating the German neurologist Otfried Foester’s technique of electrically stimulating the exposed cortex of patients suffering from focal epilepsy, which allowed him to localize the scar tissue or other lesions from which epileptic seizures originated. This “radical treatment” for epilepsy became the centrepiece of his proposal to the RF for a new kind of neurological institute that would combine laboratory investigation and surgical treatment.¹⁷

In 1928, Penfield was approached to take over neurological surgery in Montreal, and the city proved ideal for his ambitions. Penfield would have access to an underserved pool of patients, and Montreal’s location also possessed strategic advantages:

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 centres – might well prove to be the best place in which to be influenced by the work in other centres.¹⁸

Penfield’s neurological institute, then, sat on a number of key borderlands – between the Old World and the New, and between psychiatry and neurology. With Gregg’s endorsement, Penfield obtained funds in 1931, and the RF-backed experiment in psychobiology opened its doors in 1934.

Radical Treatment

Penfield described his approach to epilepsy as “radical,” and in many respects, this was true. Unlike the previous generation of neurosurgeons, who took their lead from Harvey Cushing’s more conservative approach, Penfield was comfortable excising fairly extensive amounts of brain tissue if it could be shown to be diseased or epileptogenic. He was not alone. The emerging second generation of neurosurgeons were increasingly aggressive, combining more invasive diagnostic techniques with larger removals of brain tissue. For instance, Penfield’s contemporary Walter Dandy developed the ventricular pneumogram (injecting an air bubble into the spine to visualize brain ventricles) and also performed the first recorded bilateral frontal lobectomy (removal of both frontal lobes) in 1930. At the same time, evidence accumulating from head wounds during World War I, and the theoretical contributions of neuropsychologists like Kurt Goldstein and Karl Lashley, drew attention to the relative adaptability of the brain to injury.¹⁹ Penfield himself had performed a unilateral right frontal lobectomy on a 12-year-old boy in 1928 and had been astonished by the lack of psychological deficits produced.²⁰

Yet Penfield’s approach was also conservative. It seemed obvious that brain operations must have *some* effect on personality or mental function, a fact most neurosurgeons rationalized as a trade-off between cure and disability. In an infamous description, one of Penfield’s surgical contemporaries, Percival Bailey, unintentionally revealed that such trade-offs often reflected social prejudices:

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.²¹

While Bailey and others acknowledged the possibility of loss from brain operations, they provided few clear guidelines on what areas could be removed. Penfield’s use of electrical stimulation was partly meant to rectify this problem. By using local anesthetics, Penfield could observe the reactions of his conscious patients, allowing him to spare parts of the brain responsible for speech and other functions.

This approach, however, would not work for the frontal lobes. Since 1884, it had been known that electrical stimulation of the

frontal lobes produced no obvious effect; the frontal lobes remained “silent” to electrical probing.²² Into this silence, many read great significance. As Penfield’s New York rival Frederick Tilney melodramatically put it, “It seems reasonable . . . to speak of the entire period of human existence as the Age of the Frontal Lobe.”²³ Yet for all its alleged significance, the functions of the frontal lobes remained unclear in the late 1920s. These issues converged shortly after Penfield arrived in Montreal in 1928, in an incident that would have far-reaching consequences for the development of psychosurgery. The episode began with the remarkable story of Penfield’s operation on his sister, but would not end there.

A Family Affair

Ruth Ingliss had a serious problem. Born in 1885, she had suffered from “fits” since childhood. Owing to the pervasive stigma that surrounded epilepsy in the late 19th century, she had received no real treatment for the condition. By the age of 43, her fits had become increasingly severe, with episodes of vomiting and unbearable headaches. However, Ruth, born Ruth Penfield, had one advantage: her younger brother was rapidly becoming one of the world’s most respected brain surgeons. In 1928, as her seizures became increasingly debilitating, she travelled with her mother to Montreal to see if her brother could intervene. Arriving by train, Penfield conducted a neurological exam that revealed the presence of a massive right frontal brain tumour. As he later recalled, “My knees grew suddenly weak and for a moment I thought I might fall.”²⁴

Two days later, Penfield operated, discovering an enormous tumour in Ruth’s right frontal lobe, which had long since metastasized into the surrounding tissue. Slowly and methodically, he removed the tumour, a multi-hour operation during which Ruth received several blood transfusions. The largest removal he had yet attempted, however, would not be enough. The tumour had grown to encapsulate a number of veins that, if cut, would rapidly lead to death. Penfield was forced to cut his surgery short, well shy of a life-saving operation. The tumour would eventually regrow and kill his sister.

Inglis recovered from her surgery and returned to her home in California, dying on 14 July 1931 (Figure 2).²⁵ A tragic family affair, the operation had a crucial afterlife. For two years following Ruth’s operation, Penfield made careful observations of the subtle changes in his sister’s behaviour and personality. Ironically, this

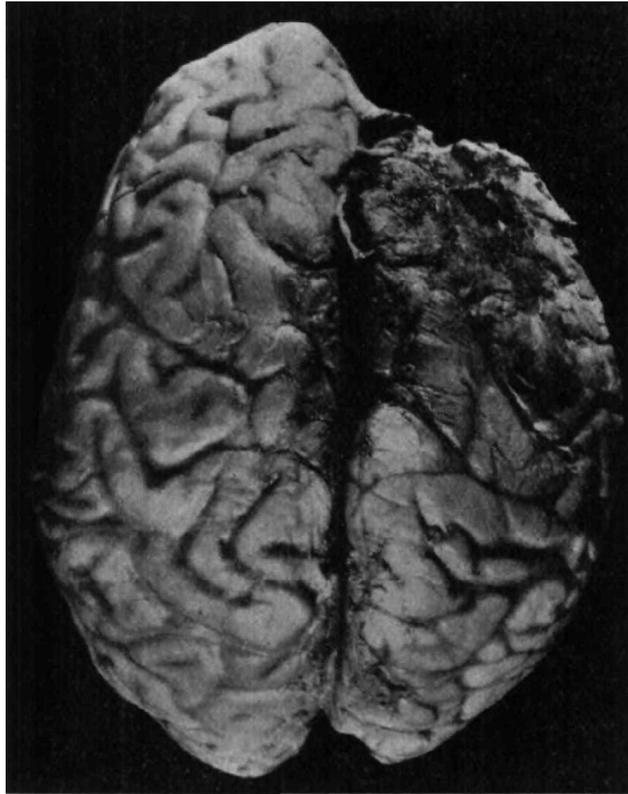


Figure 2: The brain of Ruth Inglis, following her death in 1931
(Wilder Penfield and Joseph Evans, "The Frontal Lobe in Man: A Clinical Study of Maximum Removals," *Brain* 58, no. 1 [1935], 115–33, 118)

study would prove foundational for the emergence of lobotomy, for Penfield's initial skepticism of it, and remarkably, for his brief conversion to the practice.

Immediately following the operation, Penfield and his colleagues tried to determine how Ruth had been affected. Remarkably well, was their initial conclusion. Psychometric tests showed no obvious deficiencies, and Penfield's colleague Colin Russell noted that "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."²⁶ However, Penfield persisted. Between 1929 and 1931, he made a close study of his sister's condition as she readjusted to life in California. His conclusion was that, although much of his sister's personality remained intact, she had lost something that could not easily be measured by psychological tests. A natural setting, however, was more revealing:

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 [the] 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. . . . 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.²⁷

Penfield presented his sister's case, along with two comparable removals, at a meeting of the Association for Research in Nervous and Mental Diseases in 1932.²⁸ His subsequent 1935 paper, "The Frontal Lobe in Man: A Clinical Study of Maximum Removals," noted that psychometric tests failed to register the loss of "those mental processes which are prerequisite to planned initiative,"²⁹ and became one of the most influential papers on the frontal lobes. Writing to Penfield in 1932, the Yale physiologist John F. Fulton referred to the study as "the most crucial case of frontal area extirpation that will probably ever appear in neurological literature."³⁰ Fulton's endorsement was significant; his physiology laboratory at Yale was, after the MNI, the most significant flagship operation in the RF's psychiatric research program (it had also received considerable financial support from Alan Gregg). Working primarily with monkeys, Fulton and the psychologist Carlyle Jacobsen inaugurated a research program that would lead, both directly and indirectly, to the invention and spread of lobotomy.³¹

The origins of lobotomy are typically attributed to events at the Second International Neurological Congress, held in London in 1935. At this congress, Jacobsen presented his research on chimpanzees in which the frontal lobes had been severed from the rest of the cortex in order to determine the role they played in learning and memory. Jacobsen noted, as an aside, that the operations seemed

to have dramatically reduced the emotional outbursts of one particularly troublesome chimpanzee, Becky. The chimp, in Jacobsen's infamous words, seemed to have "joined a happiness cult . . . and . . . placed its burden on the Lord!"³² Sitting in the audience of Jacobsen's talk, the Portuguese neurologist Egas Moniz asked if the reduction in anxiety and agitation observed in the chimp could be replicated in human patients. While no one at the conference dared suggest such a radical procedure, Moniz would attempt just such an experiment later that year with a series of patients suffering from "agitated depression." Psychosurgery was born.³³

In his revisionist history of psychosurgery, Jack Pressman has suggested that the traditional origin story – of Moniz drawing inspiration from Jacobson's work – is largely mythical; Moniz would likely have tried this operation regardless. As Pressman notes, "In the mid-1930s, a broad array of investigators . . . were in hot pursuit of the mystery of frontal lobe function," and Moniz likely drew inspiration from this body of knowledge.³⁴ By 1935, Penfield had contributed significantly to this body of knowledge, and while it is unknown whether Moniz drew inspiration from Penfield's 1932 report, it is clear that one future lobotomist certainly did: the American neurologist Walter Freeman, who would later adapt Moniz's leucotomy procedure into the so-called "standard lobotomy" with the neurosurgeon James Watts. Writing to Penfield in 1936, Freeman stated that

I remember listening to [your] presentation which was the highlight 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 leucotomy] came to me it seemed that there must be something to it.³⁵

Penfield and Freeman, then, took different lessons from this dramatic family affair. For Penfield, his sister's case proved that conventional psychological tests could not capture the deficits produced by frontal lobe operations; those deficits existed, however, and were serious. For Freeman, the same study suggested that frontal lobe operations produced acceptable levels of deficit – so acceptable that, when combined with Moniz's reports, the study of Penfield's sister served as a spur to psychosurgery itself. It would not be the last time that Penfield's frontal lobe studies were creatively misread.

A Change of Mind

By 1936, news of the Moniz psychosurgery operations had spread around the world, and Penfield greeted the news with varying degrees of disbelief and hostility. To Alan Gregg, he remarked in 1936 that he “was much disturbed when he learned of Moniz’ operation on frontal lobes,” although he also commented that “frontal lobe removal for tumours frequently produce[s] placidity and lack of initiative which is often pleasing to the family.”³⁶

By 1937, Penfield was on the offensive. In September of that year, he drafted an editorial with Stanley Cobb to be published anonymously in the flagship journal *Archives of Neurology and Psychiatry*. Entitled “Experimentation in Clinical Medicine,” the editorial lambasted psychosurgery, along with other somatic therapies for psychiatric disorders:

Human experimentation may result in tragedy and can only be justified if it is conducted in a crucial, scientific manner and if it is preceded by thoughtful elaboration of a reasonable hypothesis. Two recent examples of clinical experimentation do not seem to satisfy these conditions fully. The first is “psychosurgery” which after short trial has been described in the public press. This bastard term was used by Watts and Freeman. . . . The bounds of science and wisdom seem to have been overstepped.³⁷

Penfield went on to express concern that the lacerations created by the operation would likely produce epilepsy, and noted that, in his view, lobotomy patients had been insufficiently studied to determine potential long-term effects. This should be done before the operation received widespread acceptance, especially by a public “too eager to accept untried remedies indiscriminately.”³⁸ For unknown reasons, Penfield’s editorial was not published, but his opinions did not change much during the subsequent three years. By May of 1940, he could still remark to F.L. Golla that

in regard to the Moniz and Freeman operation, I may say that the procedure has filled me with astonishment. I have been unwilling to undertake it myself. . . . So far as I have been able to tell, they have applied it to certain cases which were not hopeless. However, I have no right to speak with any authority; I have not investigated it, and of course 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.³⁹

Simultaneously, however, changes afoot at the MNI would lead Penfield to reconsider. The year 1937 saw the arrival of two psychologists, D.O. Hebb and Molly Harrower. The psychologists had two objectives: to study the remarkable effects of electrical stimulation of the cortex during Penfield's operations and to investigate the long-term effects of the operations on his patients. Hebb and Harrower in particular were "to make psychological investigations of patients in whom areas of brain had been removed."⁴⁰ For Penfield, operating on the silent frontal lobes still posed risks, and if he could not say with any certainty what was being sacrificed in each operation, then the value of the elective procedures would be unclear.

In 1937, Penfield operated on a 25-year-old man named K.M., who had suffered from debilitating seizures since he had been struck on the head in a Nova Scotia lumber camp. Penfield removed over one-third of the man's frontal lobes, a procedure even more radical than the unilateral operation on Ruth. "At rare intervals," as Penfield and Hebb later wrote in their co-authored paper, "chance presents to a neurosurgeon a lesion of the brain demanding treatment that would satisfy the exacting requirements of an extirpation experiment."⁴¹ Indeed, the operation on K.M. presented as close to a controlled experiment as Penfield and Hebb had ever encountered. Not one to let the opportunity pass, Hebb performed a battery of preoperative psychological tests on K.M. and repeated them post-operatively.

The results were stunning. K.M.'s IQ had risen from 83 to 94. Moreover, Hebb and Harrower detected none of the typical "frontal lobe signs" that had been identified by previous neuropsychologists like Kurt Goldstein. K.M. seemed perfectly able to sort and categorize objects and possessed "a reasonable degree of capacity for abstract thought." His family also reported that he was now "just like any other person . . . 100 percent improved."⁴²

The strange case of K.M. came as a genuine surprise to Penfield and his collaborators, who knew that the episode required careful interpretation. Penfield, Hebb, and Harrower were convinced that the case *did not* prove that a frontal lobe operation was harmless but struggled for months to determine if tests other than the IQ might show additional deficits. For Penfield, it was not enough that the patient seemed to readjust well to life; a solid metric of loss had to be found. According to Harrower, "[Penfield] 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.”⁴³

This kind of nuance, however, was lost on many onlookers. In 1940, while Penfield and Hebb’s paper was in galley proofs, they were contacted by Walter Freeman, who had heard about the findings and wanted to make use of their research in order to “liven up” his forthcoming book, *Psychosurgery* (1942). Penfield noted to Hebb 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 [*sic*].” Two days later, Hebb dispatched a letter to Freeman containing the agreed-upon excuse, adding in a note that “I think it sounds enough like innocent ignorance that he can’t take offense.”⁴⁴

At the same time, however, the results of the K.M. study were sufficiently confusing that their very ambiguity could feed back into Penfield’s own decision-making process. That same year, Stanley Cobb featured Penfield and Hebb’s study in his annual “Review of Neuropsychiatry” for the *Archives of Internal Medicine*. Cobb summarized the results, noting that “the authors conclude from their psychologic examinations that removing a third of both frontal lobes need not cause gross deterioration.”⁴⁵ This sentence prompted a remarkable response from Penfield:

I wept when I saw your quotation of what we said about [K.M.]. . . . 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. . . . 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. . . . 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.”⁴⁶

For Penfield, the danger of over-interpreting the evidence from K.M. led him to conceal the information from Freeman and rebuke his close friend, with whom he had conspired only three years earlier to subvert the entire psychosurgical enterprise. Then, in a stunning about-face, on the very next page of the letter to Cobb, Penfield added:

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.⁴⁷

Ruth's operation had served to warn Penfield of the possible costs of frontal lobe operations. For Freeman and others, the very same data served to highlight the possibilities of those same operations. In the world of RF-funded neuropsychiatry and Meyerian psychobiology, ambiguous data could easily be repurposed and pull in two different directions simultaneously. Yet the emerging sense of precision created by the K.M. case – that the downsides of his surgeries could be understood and calculated – seemed to spur Penfield to reconsider. Could a more precise operation be devised that might have all of the therapeutic potential and none of the drawbacks? And if so, could Penfield bring another disorder under the scope of neurosurgery? As Pressman has noted, after World War II many neurosurgeons attempted to develop more precise psychosurgical operations that could rationalize the procedure and minimize its downsides.⁴⁸ Far from being removed from this effort, Penfield was, in fact, one of the first to consider such an undertaking. Penfield was positioned on a razor's edge; to undertake psychosurgery was now only a matter of having the right collaborator.

Cameron and Gyrectomy

Montreal needed a new psychiatrist; of this, Penfield was sure. As early as 1937, he had written to a colleague at Johns Hopkins that “we are in a bad way for a psychiatrist.”⁴⁹ By 1941, with the war in Europe in full swing, memos circulated among Penfield, the president of McGill, F. Cyril James, and the dean of medicine at the Royal

Victoria Hospital, J.C. Meakins, about the possibility of establishing a university psychiatry 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 Ravenscrag might be an ideal site for such a facility.⁵⁰

Penfield liked the plan, but with one provision: Ravenscrag should be only a temporary placeholder. In order to appeal to the RF, the new psychiatric institute had to be research-focused, and should have a close relationship with the MNI. Penfield elaborated on plans to share laboratory resources and consult on overlapping cases. “The closest cooperation must depend, in the last analysis, on proximity.”⁵¹

Penfield’s desire to collaborate with a psychiatrist is hardly surprising, given his deep ties to the RF and its program in neuropsychiatry. Moreover, his friendship with Adolf Meyer had continued since their first encounter in 1922, and when McGill contemplated opening the first dedicated university department of psychiatry in Canada, it seemed perfectly sensible for Penfield to consult his old friend on whom to hire. Penfield made it clear that he wanted an independent collaborator, not a subordinate: “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.”⁵² In response, Meyer stressed the importance of getting a “native” Canadian rather than an import: “I wonder about Ewen Cameron who has been in Canada.”⁵³

Cameron certainly had the pedigree 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.⁵⁴ Cameron’s intellectual orientation also fit the bill nicely. Neither an organicist nor a psychoanalyst, he advocated a kind of relentless experimental empiricism, outlined in his 1935 monograph *Objective and Experimental Psychiatry*. As Rebecca Lemov has noted, Cameron’s

perspective combined a behaviouristic orientation with a technophilic interest in automating psychotherapy.⁵⁵ A forward-thinking researcher, interested in experimentation and innovation, Cameron seemed like the perfect candidate. An offer was extended, and Cameron arrived in September of 1943.

Six months later, in March 1944, Penfield and Cameron sat down to discuss their working relationship. No record of the meeting survives, but Penfield sent a letter to Cameron afterward that is remarkable in its candour, and worth quoting at some length. Penfield stated that

I regret your conclusion that you can develop your department best quite independent of ours, and I feel that your simile of the twins "in utero," one of which must be weak because the other uses up the nourishment, does not apply. . . .

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 realize now that you do not care to touch any of these subjects except neurophysiology. . . . 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. 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.⁵⁶

Penfield sent a carbon copy to J.R. Fraser, dean of the McGill Medical Faculty, adding that “I give up . . . 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.”⁵⁷

Could anything overcome Cameron’s reluctance to work with Penfield? A clue can be found in a letter from Penfield to Fraser written 18 months earlier. Discussing possible collaborations between the new psychiatric institute and his own, Penfield noted that

in many institutions certain operations are carried out upon the brain to combat the 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.⁵⁸

After he was rebuffed in his first overture toward Cameron, it was the lure of collaboration itself that finally overcame Penfield’s resistance to psychosurgery. If a project undertaken with Cameron could yield a better lobotomy – one more in tune with Penfield’s cautious approach and scientifically grounded method – so much the better. And if this better lobotomy could unite psychiatry and neurology under the banner of neurosurgery, it would cohere the psychobiological vision that had given birth to the MNI.

Within a year of their disastrous sit-down meeting, Penfield approached Cameron to collaborate on a small series of experimental psychosurgeries, dubbed “gyrectomies” (removal of specific brain gyri). Rather than making blind disconnection through trephined holes (as in a standard lobotomy), this procedure would leave more brain tissue intact, theoretically reducing post-operative intelligence and personality deficits (Figure 3). “I think I would be glad to do a series of these patients,” Penfield confided to a colleague at the nearby Verdun Protestant Hospital. “It might be only a trickle, but this is

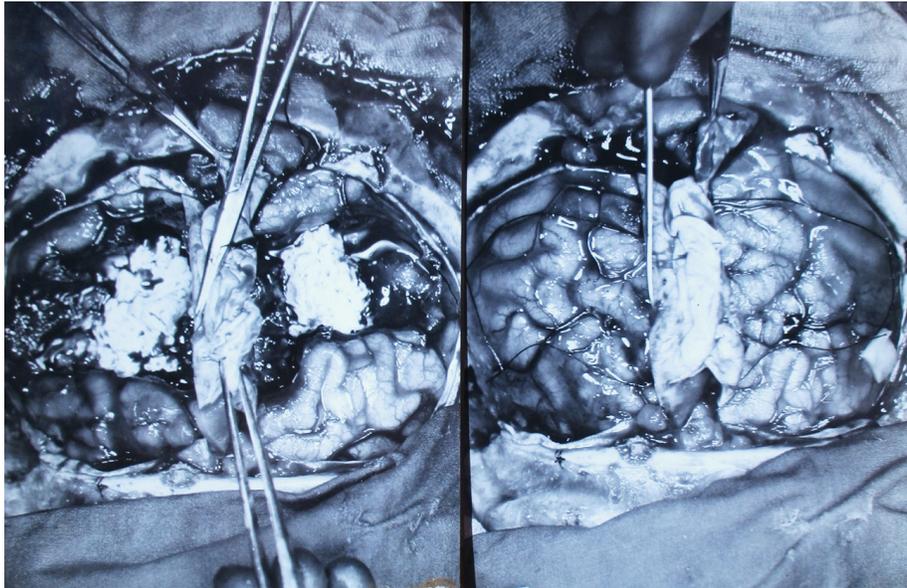


Figure 3: An example of Penfield's gyrectomy procedure

Notes: On the right, the cortex to be removed is dimly outlined in black thread.

On the left, the ablation has been carried out.

(Osler Library for the History of Medicine, Wilder Penfield Fonds, Box 142, W/P 151)

a type of operation that I am very anxious to tackle, and after the war we could perhaps swell the trickle into a larger stream.”⁵⁹ Cameron selected seven candidate patients and handed them over to Penfield for operation. In the spring of 1944, Penfield carried out his first gyrectomy on a 23-year-old man named H.M., who had suffered for years from an “inability to concentrate and to work.”⁶⁰ He had previously received 15 treatments of electroconvulsive 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 tissue in July. After H.M. had recovered from the operation, “his depression . . . disappeared and he [became] president of the local radical political club in which he takes a great interest.”⁶¹

Other patients did not fare as well. A.L., a woman of 33, complained for six years of “listlessness, fear of crowds and of heights.” After several rounds of electroconvulsive and insulin coma treatment, her “chronic anxiety neurosis” remained unchanged. The gyrectomy, performed by Penfield in October of 1944, relieved the patient of fear temporarily, but following a major epileptic seizure

she was readmitted to the AMI and given a standard lobotomy two years later. I.C., a 27-year-old woman, seemed initially relieved of her “obsessive compulsive neurosis” but was later readmitted and lobotomized. All told, Penfield performed the new gyrectomy procedure on seven patients.⁶²

How would the success or failure of the new procedure be evaluated? Both Hebb and Harrower had left the MNI, but Penfield consulted with Hebb from a distance and pestered Cameron to add a psychologist to the team.⁶³ Penfield and Cameron eventually recruited Robert Malmo, then conducting war work at NIH Bethesda. Malmo was selected because of his impressive experimental work with primates, and he arrived to work on the project after four operations had already been performed. “I naturally assumed,” Malmo later recalled, “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 had had no preoperative testing, and that the preoperative testing of the other two . . . was meagre (only Wechsler Intelligence Scales, and incomplete ones at that).”⁶⁴ Nevertheless, Malmo persisted, conducting an extensive battery of psychological tests on the remaining patients, as well as comparing the cases to a series of separate lobotomy patients.⁶⁵ Penfield later informed Hebb that “Malmo is making a study of the gyrectomies and the whole future of operations of this sort now hangs in the balance as we are reviewing the cases already done.”⁶⁶

Here, one can see perhaps the most notable break Penfield made from the Meyerian psychiatric paradigm. As Pressman has noted, psychosurgery was thought to “work” as a therapy largely because success was judged within the framework of Meyer’s concept of “maladjustment.” If lobotomy allowed a patient to return to their social role, it was a success. As Pressman put it, “Patients thus were not cured of a particular disease so much as they were restored as functioning citizens.”⁶⁷ For Penfield, this wouldn’t do. Since his earliest operations on epileptic patients, he had displayed a thirst for information beyond what could be seen with the naked eye; this had motivated his histological work in Spain, his adoption of Foester’s operating techniques, and his collaborations with Hebb, Harrower, and Jasper. If the benefits and losses of gyrectomy could not be measured concretely, then mere clinical impressions would not be enough to recommend the operation.

The gyrectomy project also exposed a deeper difference between Penfield's vision of medico-scientific collaboration and the original RF formulation. Virtually all RF-funded psychiatric initiatives displayed a deep commitment to crossing disciplinary lines, but this interdisciplinarity was mainly theoretical, in keeping with Meyerian psychobiology. Ideas and data crossed disciplinary boundaries easily, but successful collaborative practice was rare.⁶⁸ For Penfield, on the other hand, interdisciplinarity was about *practice*; colleagues worked *together* on shared problems, bringing their independent expertise to bear. In this regard, Cameron could not have been a worse collaborator. Penfield outlined the problem in a letter to a colleague, just as the gyrectomy experiments concluded:

When [psychiatry and neurology] 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. . . . I believe that the rough and tumble of active discussion between those interested in these two fields might well lead to some real advance.⁶⁹

The two solitudes to which Jasper would later allude had begun to grow, and to frustrate the search for a better lobotomy.

Meanwhile, word of Penfield's gyrectomy procedure had got out. Fred Mettler, then in charge of the Columbia-Greystone project in New York – the largest research initiative yet launched on the effectiveness of psychosurgery – contacted Penfield in 1947. Mettler was employing a nearly identical procedure called a “topectomy,” which was derived from early reports of Penfield's own operation.⁷⁰ He explained that, in his study, surgeons were prevented from knowing the results of the operation, while the psychological examiners were prevented from knowing the areas removed, or whether brain tissue had been removed at all (an early example of a double-blind study). Penfield mentioned this “strange sort” of evaluative procedure to Cameron, adding that “from [this] choir of ignorance may emerge a chorus of true tones not influenced by any wishful thinking.” Then, Penfield asked Cameron point blank, “When shall we get together to put our cards on the table about gyrectomy?”⁷¹

Penfield had reason to press Cameron for an answer. The men had agreed to present the gyrectomy cases at the 1947 meeting of the Association for Research in Nervous and Mental Disease

(ARNMD). This meeting was exceptionally important; the years after World War II had seen an explosive growth in the number of lobotomies performed. At the same time, Walter Freeman's invention of the transorbital lobotomy (the infamous "ice pick" procedure) threatened to take control of psychosurgery out of the hands of trained neurosurgeons. John Fulton, initially so pleased to promote lobotomy, now organized the ARNMD meeting to regain some control over the enterprise by refocusing discussion on the physiological underpinnings of psychosurgery, in the hope of developing a better, more precise operation.⁷² The gyrectomy seemed like the ideal candidate.

When Penfield, Cameron, and Malmo presented their findings at the ARNMD meeting, the form of their presentation was revealing: unable to come to a consensus, all three men presented separate papers. Penfield concluded that "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 . . . 'frontal lobotomy.'"⁷³ Malmo, for his part, presented fairly convincing evidence that gyrectomy did, in fact, lead to detectable post-operative intelligence loss.⁷⁴ Cameron, by contrast, interpreted the results within the Meyerian framework of adjustment; success was judged by whether the patients could re-acclimatize to their home and work. He closed on a more hopeful (or perhaps ominous) note: "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."⁷⁵ For Penfield, the failure of the gyrectomy cases was the nail in the coffin for psychosurgery. For Cameron, it was just the beginning.

Two Solitudes

The gyrectomy cases spelled the end of any productive relationship between the MNI and the AMI, and between Penfield and Cameron. Yet this was not for lack of trying. Frequently over the next several years, Cameron and Penfield discussed possible alternative procedures. One possibility involved the injection of the anesthetic procaine into a brain gyrus that Penfield planned to remove from a patient, to observe the effects.⁷⁶ It is unclear if the operation was ever carried out, but in 1952, the pair discussed another

possibility. Cameron had recently learned about the potential “therapeutic uses of ultrasonic sound” from the American psychiatrist Warren McCulloch and suggested using ultrasonic waves as a way of disrupting brain tissue.⁷⁷ Penfield responded that “I do not believe that any technique . . . 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 . . . would fare better.”⁷⁸ Yet Cameron remained sanguine: “I remain convinced that there is gold in this particular hill, but how to come at it is quite a question.”⁷⁹

For both men, the lure of finding a better lobotomy remained strong, despite their growing personal distaste for each other. In both of the above cases, Penfield was not opposed to collaboration on a replacement procedure, but was unwilling to proceed without a firm grounding in physiology. Moreover, for Penfield, collaboration was a give-and-take process; the contributions of all parties ought to be respected. Penfield came close to spelling this out in 1949 in a letter to C. Sidney Burwell, who was considering luring Cameron to Harvard. Summing up his view of Cameron, Penfield stated that he was “a man of considerable force. He is anxious to cooperate with other departments within a university but also always does so in his own way. He has had very little in common with our department.” Penfield added that “I feel rather strongly that . . . advances that come from study of the brain will throw light upon psychiatry. . . . For that reason I feel that a proper academic point of view is that psychiatry, neurology and neurosurgery should have a common bond in laboratory work. The disciplines are at present entirely different as far as psychiatry is concerned, but when the most advances come, psychiatry, neurology and neurosurgery will draw together.”⁸⁰ Far from bringing about the consilience that Penfield wanted, Cameron’s arrival had exacerbated the existing divisions.

Meanwhile, Penfield largely renounced psychosurgery and became an active behind-the-scenes opponent. In 1949, he sent a private note to Henry Alsop Riley, president of the American Neurological Society, asking him not to support Walter Freeman’s proposal for a conference on psychosurgery in Lisbon. 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!”⁸¹

They Hate Psychiatrists

In 1951, Freeman sent Penfield a copy of the second edition of his textbook *Psychosurgery*. Penfield responded: “We must all hope that lobotomy is one step in the direction of more discriminating treatment. The time must come when we can give the patient as much, or more, and ask him to sacrifice less. I am not doing any more work with lobotomy and I am relieved that I do not have to deal with or be pursued by the type of patients in this field.”⁸² Penfield seemed all the more relieved that his dealings with Cameron had also come to an end. In 1954, Penfield confided 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. . . . 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.”⁸³ As Cameron consolidated control over psychiatric practice in Montreal, and psychiatric training in Canada, the two institutes became more and more estranged.⁸⁴ In a 1956 profile of Penfield and the MNI for *Macleans* magazine, an unnamed Montreal psychiatrist told the reporter: “Penfield and his staff are a close-knit bunch – and they hate psychiatrists.” The journalist added 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.”⁸⁵

The division between psychiatry and neurology that began with the failed collaboration between Penfield and Cameron would continue to grow. Despite having a shared intellectual basis for collaboration (Meyerian psychobiology), the men were unable to locally coordinate their efforts, and soon their institutes, only minutes apart, would devolve into two solitudes.

Penfield might not have mourned the separation had he been aware of subsequent developments at the AMI. Cameron’s relentless desire to innovate drove his search not only for lobotomy alternatives but also for other novel treatments. Perhaps the most notorious of these, developed in the years after his collaboration with Penfield,

was “psychic driving.” The therapy was the stuff of nightmares: a patient, typically female and admitted to the AMI with a diagnosis of schizophrenia, was strapped to a bed and forced to listen to a looped recording of her own voice, repeating a key phrase from an earlier psychotherapy session. This was accompanied by a steady drug regimen that included lysergic acid diethylamide (LSD), sodium amytal, and phencyclidine (PCP). Cameron also used frequent electroconvulsive therapy, often resulting in permanent memory loss. The goal was to cause a “disorganization” of thinking that would free the patient from unhealthy schizophrenic thought patterns, remaking them into healthy, well-adjusted citizens. If lobotomy would not get the job done, then perhaps a combination of drugs, technology, and automated psychotherapy would.⁸⁶

When it was revealed in the 1970s that Cameron’s efforts had been partially funded by the CIA as part of their infamous MK-Ultra “mind control” program, his image was remade from a psychiatric pioneer into a caricature – a “mad scientist” unleashed by the forces of Cold War hubris. Yet this *post hoc* vision of Cameron obscures a greater historical understanding. Penfield’s surgical approach to epilepsy, Freeman’s lobotomy, and Cameron’s “psychic driving” grew from the same roots: the RF’s program in psychiatry and the Meyerian paradigm of psychobiology. All three men advocated interdisciplinary medicine and new therapeutic approaches meant to alleviate genuine human suffering. Acknowledging these overlaps, however, also highlights important differences. Penfield’s approach to interdisciplinarity was based on deep collaboration, reciprocal trust, and shared practice; Cameron’s was based on the shallow borrowing of technique, siloed authority, and a division of labour. Penfield listened to others and proceeded with caution; Freeman and Cameron appropriated the work of others to advance their preferred approaches. From the seed planted by Meyer and the RF, two surgical approaches to the brain grew; one survived and remains a part of modern neurosurgery, while the other has largely faded. Knowing something of their shared history can give us a greater understanding of the difficulties of overcoming disciplinary solitude. As Jasper himself put it in 1963, “There is no magic formula in ‘interdisciplinary research.’ Cooperation among scientists, no more than among peoples, can be pre-arranged or organized [and] pre-arranged marriages are seldom successful. However, specialization and isolation in science [also] carries the seeds of its own destruction as it does so often in society.”⁸⁷

None of the above should be seen as tarnishing Penfield's image, or the value of his medical and scientific achievements. Rather, by placing him in context, we can see the true value of his work on the "borderlands" of psychiatry and neurology in the 20th century. Indeed, the story of Penfield's brief engagement with psychosurgery should enhance his status as a hero of Canadian medicine. Given the history recounted here, perhaps the most revealing aspect of Penfield's character was not that he experimented with psychosurgery but that he had the courage to stop.

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