

## **Exhibit P-96**

# The McGill Experience of Robert A. Cleghorn, MD: Recollections of D. Ewen Cameron

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**Note.** In the past decade, the career of Dr. D. Ewen Cameron (1901-67), particularly his work at McGill in the 1950s and early 1960s, has attracted much attention. At least three books have been published about Cameron's controversial practices,<sup>1</sup> now thoroughly discredited, of using various so-called depatterning techniques, including LSD, in investigating schizophrenia and in attempting to treat patients who had that disorder. These techniques are described in detail in the following pages. In 1988, the clandestine role of the American CIA in funding some of this research achieved notoriety when the CIA agreed to make a large cash settlement with several former patients seeking to bring suit against this government agency.

Coincidentally, in 1988 a copy of the unpublished autobiography of Dr. Robert A. Cleghorn came to my attention. Cleghorn, now living in semi-retirement in Toronto, was at McGill and its Allan Memorial Institute throughout the years of the controversial work, and he succeeded Cameron as Chairman of the Department of Psychiatry there. The entire document is most interesting and will, I hope, be published one day. But the portions of the memoirs devoted to Cameron seemed especially relevant, offering as they do the cool reflections and insight of a highly interested bystander who was closely connected with Cameron.

With the permission and cooperation of Dr. Cleghorn, a substantial portion of his memoirs has been excerpted. It is published here as a contribution to our understanding of what is certain to remain an emotional fragment of Canadian medical and ethical history for years to come.

Omissions from the text are indicated in the usual way. Otherwise, the following recollections are as written by Cleghorn. Copies of the entire autobiography, under the title *A Search for Meaning in Hormones and Humans*, are available in several of the Canadian university library systems, including McGill University, McMaster University and the University of Toronto.

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Manuscript accepted 17 January 1990.

CBMH/BCHM / Volume 7: 1990 / p. 53-76

## THE FIRST TEN YEARS

Soon after my arrival in Montreal in August 1946, I set to work in my new setting in the old stone mansion, The Allan Memorial Institute (AMI), high on a promontory of Mount Royal. It was to be the centre for psychiatry at McGill and to become quite the leading psychiatric institution in Canada. By the accident of circumstances and possibly owing to an obscure kind of foresight which caught the trend of the times, I had entered psychiatry as it was about to break with traditional restraints and develop new vistas aided by new techniques. The emergence of this discipline led to a fresh maturity and constituted a new facet on the practice of medicine.

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Several circumstances favored the founding of an institute and the rapid growth of psychiatry at McGill. Among these were the gift of the Allan mansion to the Royal Victoria Hospital and McGill in 1940 and the enterprising efforts of Dean Jonathan Meakins and Dr. Wilder Penfield to establish a Department of Psychiatry, using the building which became known as the Allan Memorial. Then their choice of a director fell fortunately on D. Ewen Cameron, who proved to be creative, resourceful, enterprising, and blessed with administrative skill. His background and training fitted him eminently for this venture. A son of the manse, he was born near Stirling Castle, Scotland. A good athlete and scholar at Glasgow University his well proportioned frame still moved with athletic grace in later life. He received his MB, ChB from Glasgow University in 1924, his DPM from the University of London in 1925, and MD (by thesis) with distinction, from Glasgow in 1936. His psychiatric training began under Professor, later Sir D. K. Henderson, in Glasgow, and continued at the Burgholzi Anstalt in Zurich, which Professor Eugen Bleuler had made famous. He then crossed the Atlantic to come under the influence of Professor Adolf Meyer at the famous Phipps Clinic in Baltimore.... Cameron's attitude to the doyens of his specialty was one of tolerance and scepticism. He absorbed what was of worth and critically rejected psychoanalysis and complicated jargon such as Meyer's nosological neologisms.

At the outset of the depression years in 1929 he moved to Brandon Mental Hospital, where he remained for seven years. In retrospect that might sound like academic entombment, but he turned his experience into a reality of living, organizing mental health clinics, doing research on the measurement of changes in behavior, and writing a textbook entitled *Objective and Experimental Psychiatry*,<sup>2</sup> thus establishing his public stance. In the introduction to this book he wrote succinctly:

Whether we recognize it or not, the influence which most strongly beats upon those of us who passed into psychiatry . . . was the humanitarian. Sympathy, patience, insight, rapport—these were the magic words. They must remain so, but as a means to an end and not as an end in themselves. The more sceptical “tough minded” worker, thrusting out from these enshrouding curtains to the harder realities of active treatment, found himself at once in a sad dilemma, where to find solid ground.

So many of our concepts are mere traditions, so many of our entities were agglomerations held together by immense ramifications of side issues and analogies. We are fatally entrapped by words and logic. An increasing number of us experience a feeling of growing distrust by purely descriptive and intuitive concepts of human behavior and find it more and more difficult to content ourselves with facts or assertions save where they will withstand experimentation and will not fail us on prediction. Time has brought us so great a development of instrumentation and so much larger an understanding of experimental methods that we are now in a fair way to realize our dreams of analyzing human behavior objectively.

This is a fair sample of his incisive clarity of opinion and also of his outstanding literary skills. We see here also the vision which began in his lifetime to find expression in various techniques he utilized, e.g., electronic recording of interviews, tape play-backs, efforts in telemetry, studies of RNA, and memory, but sadly the real burgeoning of scientific inquiry in psychiatry only came towards the end of his life and after his death in 1967. He saw the promised land as few did then and tackled it with fervor but without scientific discrimination. Rescued from the wheatfields of Manitoba in 1936 by a call to the Worcester State Hospital in Massachusetts, he engaged in early studies with insulin in schizophrenia. In 1938 he became professor of Neurology and Psychiatry at Albany Medical School. It was five years later that he came to McGill.

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As a man how was he? He emanated warmth, strength, and often displayed a humorous turn. Anger he rarely showed, exasperation sometimes. He was, nevertheless, a driven man, with too many ideas to express, too many projects to accomplish in too little time. He used his expertise in committee work to divide responsibility, but kept the reins in his hands. He used the same skill to develop his influence in psychiatric organizations. He was perhaps most proud of the organization of postgraduate training in psychiatry.<sup>3</sup> By the year of his retirement there were some 130 graduates in training in the McGill network, which included seven hospital affiliates. How did we, his associates, see him? His arrival at 8 a.m. or shortly after was accompanied by a series of belts dictated several hours earlier. This kept one secretary busy with his correspondence most of the day. Committee meetings in his office, whirlwind rounds on the wards with an entourage of resi-

dents, then more meetings. He lunched at the Royal Victoria Hospital as a rule, and in the afternoon saw private patients, and finally made a round of the wards before going home after 7 p.m.

Although he rather scorned the length and tediousness of protracted psychotherapy, he demonstrated that this he could do successfully. While he rejected formal psychoanalytic parlance, he used some of the basic ideas expressed in his own terms and sought constantly for briefer forms of psychotherapy, using tape play-back and other techniques. With a diplomatic flexibility when he saw good residents leaving for training in psychoanalysis, he introduced outstanding analysts in the early 1950s. . . . Thus his eclecticism succeeded.

Who was his second in command? There wasn't one. He had a series of Assistants to the Director—able men, but in no way seen as Clinical Directors. They were organizational assistants, no more. Who were those close to him? Probably Dr. Baruch Silverman was closest professionally. The rest of us ran our own particular "show" with his support—as he had great facility in raising funds. He was a great innovator and fostered an "open door" policy, a day hospital, and units for gerontological, biochemical, endocrinological, psychological, forensic, and transcultural studies.

What about his warmth, mentioned earlier? It was there, but never allowed to appear as intimacy. His close contacts were all professional organizations or positions which he employed for the furtherance of his laudable schemes, promoting psychiatry with himself in the middle of manifold projects. It was apparent that he was vitally concerned with the well-being of men, regardless of national barriers, race, or religion. He opposed bigotry and entered public debate with ecclesiastical pundits whose views he attacked by spoken and written word, perhaps a bit recklessly. His conceptual capacity and power of description were apparent in his addresses, but nowhere more clearly than in a little known article in *Science* in 1948.<sup>4</sup> This dealt with the emergence of the scientific point of view and its influence on thought and belief.

#### SUMMARY OF THE FIRST TEN YEARS

The ten years from the inception of the new Department of Psychiatry at McGill may be termed the "building years." Patient care began in 1943 at the Allan Memorial with Lloyd Hisey and Cecil Mushatt as residents. They admitted ignorance and only by diligent reading kept up to their exacting if indulgent chief. The senior experienced psychiatrists, Stern and Prados, arrived the next calendar year, and the Allan was official opened. Soon a day hospital was started, a geriatric unit set up, psychological services, an outpatient service was organized under M. Prados, and the endocrinological laboratory under my aegis. The

cessation of the war also saw psychiatric units established at the Montreal General Hospital under A. E. Moll and at the Queen Mary Veterans Hospital under Travis Dancey. The large mental hospital at the neighbouring community of Verdun, under George Reed, took on new life with the closer academic association with McGill. Heinz Lehmann was looming up there as the key figure in this renaissance by virtue of his wide intellectual curiosity, teaching, and research interests.

Over the next four years, the flow of recent graduates from the four-year postgraduate training program supplied new staff. . . . The arrival of Taylor Statten as head of psychiatry at the Montreal Children's Hospital in 1950 opened a new era, and Eric Wittkower's advent shortly after put a stamp of excellence on teaching. During these years teaching initiated by Cameron, Stern, and Prados was supplemented by non-medical McGill faculty members: by K. A. C. Elliott in biochemistry, Oswald Hall in sociology, and various members of the department of anthropology.

The upgrading of postgraduate training, which was an ongoing process, expanded particularly after 1950.<sup>5</sup> Residents were attracted from abroad, so that by 1954 six European, two Asian, and four Latin American countries were represented. This provided a stimulating ethnic mix. The atmosphere in those days and for years to come was one of excitement. This was in part due to the charisma of alert strength emanating from the chief—an aura which was quickly caught and shared by all. Where psychiatry had once been a dull round of custodial care there were now therapies that could be applied diligently, with hope and the expectation of rivaling medicine in supplying an empirical and scientific base. Insulin coma treatment began early and electro-convulsive therapy was used in the four psychiatric centers in the McGill psychiatric network. Research was encouraged and all that was new in the field of psychiatry was expounded. . . . These were stirring times when these natural biological products [cortisone and ACTH] were eliciting a variety of abnormal mental states as well as therapy for medical disorders. These two products seemed to promise a new era but, like many revolutions, the hopeful predictions outran reality which subsided into a period of prolonged and highly technical research before the real revelations came. These Chicago meetings did stimulate me to re-examine the psychological aspects of Addison's disease, on which I had spent so much time in the 1930s, and to communicate this and studies on cortisone psychoses at a Ciba Foundation Conference in London in 1951. . . .

All the while, solid research work was going on which was to mature shortly. Lehmann at Verdun was engaged in his promising study of chlorpromazine, which began to be used in the McGill circuit with

dramatic success. Graham and I showed that ECT had an adrenal cortex stimulating effect, judged by the effect on white blood cells. With Saffran we tried the effect of ACTH in depressive cases, to no avail. Hoffman developed the use of isotopes in the geriatrics lab, while Saffran was devising micro methods for adrenal corticoids. In the early 1950s, Drs. McGrath, Murphy, and Gofton, who worked for me, studied flying fatigue. This led to some seemingly hazardous flights to Japan and to Resolute Bay in the Arctic. There was a general feeling that psychiatry was leaping ahead; hope abounded and McGill was felt to be in the forefront not only in Canada but in the world. There was something of a pressure-cooker atmosphere about the department, especially at the Allan. This was not oppressive; rather, it was stimulating.

There was one potential weakness in all this progress, not then glaringly apparent, but a flaw to emerge later. This concerned finances. While money or bursaries for resident training became available in 1948, by virtue of the new federal Mental Health Grants, funds for clinical staff or research personnel were in short supply. The heads of laboratories had to seek their own sources of support for themselves and for their investigators. One such source was the Medical Research Council, which began to operate more generously; the American foundations were also tapped. Cameron was a good fund raiser, particularly for his own projects. For example, he was able to persuade the parent hospital to finance a new wing for the Allan in 1952. There was insufficient hard money for support of clinical staff or researchers. For the time being those responsible for research, like Malmo and myself, sought funds where we could, but there was an unreal air about financial matters.

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RESEARCH EXPERIENCE OF D. E. CAMERON

Cameron's advocacy of science in psychiatry emerged in a practical way after he moved to Worcester State Hospital in 1936. There the Chief of Research was Roy G. Hoskins, who had a long-standing interest in the relationship between endocrinology and schizophrenia. In the mid-1930s this combination of concerns was innovative, although the endocrine field was still unclear as the chemistry and physiology of the hormones had yet to have their functions definitively established. There was, therefore, precious little basis for the clear application of endocrine secretions to clinical investigation in psychiatry, as the medical and physiological aspects were still being worked out.

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### *Insulin Coma*

Meanwhile, Cameron in association with Hoskins and others made observations on insulin-induced hypoglycemic coma, a technique originated by Sakel of Vienna in 1933 and widely used after 1936 for the treatment of schizophrenia. In a series of papers over the next three years, the group at Worcester reported their observations and conclusions, which fell in line with the studies of the best of other accounts of the new therapy. Mortality they found to be less than 1 percent, but recoveries in their view were considerably less than the 70 percent reported by some European observers. Their papers were conservatively optimistic for the new treatment. They also took records with the electroencephalogram in their schizophrenic patients undergoing the insulin therapy, without revealing significant clues to its effectiveness. It should be noted that there were few defined techniques for recording the experience of the patients undergoing hypoglycemic therapy, so observations were, on the whole, general, in the absence of strict criteria to record changes.<sup>6</sup> It was, after all, an empirical procedure with scant or inappropriate scientific basis supporting it and no obvious way by which to demonstrate the nature of changes in the psychiatric picture except by clinical description of the patient's behavior.

Despite the absence of quantitative efforts to estimate the efficacy of coma therapy, many patients obviously improved. There was little recognition then of the beneficial influence of removal of patients from back wards to a more salubrious environment where increased attention and personal care coincided with the administration of insulin and the recovery from coma which was effected by major injections of glucose. Not until the early 1960s were the control studies done which undercut the belief in the efficacy of hypoglycemia as a specific and indicated the importance of improved social circumstances and tender attention surrounding the therapy, which were then seen in their true light as a large part of the benefit of the therapy.<sup>7</sup> By the mid-1950s the introduction of chlorpromazine began to replace insulin treatment so that it is now little used, though some psychiatrists still maintain that the coma had a specific beneficial effect, at least in some instances. Experience with schizophrenia and the endeavor to study insulin treatment was an advantageous experience for Cameron in this setting, though as research it lacked methodology. The environment at the Worcester State Hospital and the Foundation nearby was, however, stimulating and allowed him to develop this interest, and also the topic of remembering.<sup>8</sup>

### *Anxiety States and Remembering*

In Worcester he also manifested his concern with anxiety states, an interest he carried with him when he went to Albany in 1938 to head

the Department of Psychiatry there. This interest was followed by several studies carried out later at McGill and led more or less directly to trying the effect of injecting adrenalin intramuscularly, or intravenously, in an endeavor to break up what he saw as an autonomous reaction to excessive sympathetic nervous activity in persistent anxiety states. In a series of papers, from 1944 to the most clarifying one in 1946,<sup>9</sup> he reported some degree of relaxation and transitory disappearance of symptoms in such adrenalin-related cases. In these papers there was much documentation of blood pressure and pulse as manifestations of change in reactivity of the cardiovascular system. Unfortunately, little attempt was made to quantify the level of anxiety in the patients by objective assessments by trained personnel using psychological tests, though admirable care was given. The body was observed but not the psyche, except casually. In the course of these experiments, 200 mg of sodium amytal was given to some patients and this obviously complicated the experimental design as it led to relaxation. Nevertheless, of the large number of patients (approximately 50) who were subjected to this therapeutic trial, some manifested clinical improvement. This is hardly surprising when one considers the amount of concern and attention given them by this charismatic physician. Ultimately, Cameron stated that better methods of administration remained to be worked out. . . . There does not seem to have been much response in the literature to the adrenalin researches.

### *Memory*

Another area of interest in which Cameron had long-standing preoccupation concerned impairment of the retention phase of memory,<sup>10</sup> long recognized by novelists and others. These studies at the clinical level of testing were followed in the mid-1950s by his efforts, in association with a biochemist and psychological colleagues, to improve the defective memory of patients suffering from senile and arteriosclerotic behavior disorders by various biological agents such as intrathecal hyaluronidase, nucleic acid, and ribonucleic acid.<sup>11</sup> There were some five papers in the prior 6-year period before 1963. . . . Cameron advanced the belief that RNA might prove to be the substrate of memory; he maintained that the ribonucleic acid, as used intravenously in his work, did reduce memory deficit and he presented data intended to support this contention. A comprehensive review of his work in relationship to these efforts and those of previous investigators appeared in his Maudsley Lecture.<sup>12</sup>

Credit must be given to Cameron for venturing into a field where testing methods were often inadequate, but there does not seem to have been sufficiently controlled work with placebos and, in addition, one of the collaborators has criticized the memory tests on technical

grounds as lacking in validity.<sup>13</sup> A further critical opinion of his experimental studies may be advanced at this point, namely that there appears to be an apparent lack of profound scholarly acquaintance with the field of study. This depends in part on his deficiency of training in a basic science where he might have acquired an attitude of scientific appraisal and caution which could then have been applied as relevant to the area under investigation. This was a shortcoming of the times when psychiatrists were rarely so exposed.

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*Magnetic Tape Recording and Play Back*

Let me turn now to a consideration of the recording and playback of patients' verbal report using magnetic tape. In his book on *Objective and Experimental Psychiatry*, published in 1935<sup>14</sup> Cameron spoke of the great potential he saw in new technologies. Fifteen years later, in his book *General Psychotherapy*,<sup>15</sup> he described his own approach to the practice of this essential skill in psychiatry. He avoided much reference to what other scholars had contributed to this area, so that pundits such as Freud, Jung, Adolf Meyer, and other contemporary contributors were largely ignored. His book, therefore, remained isolated and with little impact on the general field in psychiatry with which he dealt. The book reflected considered attitudes to the subject and was not without judicious opinions but it gave a feeling of superficiality and a failure to come to grips with basic issues in terms that others employed.

In this book, however, he noted that the development of high fidelity magnetic tape recorders provided investigators with a valuable tool for the study of the process of psychotherapy. He proceeded to become a pioneer by putting this device to work, when he went to the Allan Memorial Institute, playing back to a patient some of his or her relevant sentences over and over. This repetition he noted led to discomfort in the patient, which he saw as of ultimate use, and so he devised a series of the patient's own cues in statements and what he termed "autopsychic driving" (as opposed to cues verbalized by other people called "heteropsychic driving"<sup>16</sup>) in which much detail of the recording and playback process is given for a number of cases and several new concepts are elaborated such as the use of the word "shielding," which is essentially denial, and what he called Dynamic Implant. This represented the continuation of an effect after the actual period of driving (playback) was stopped. This original and potentially useful concept was never analyzed in depth or put in relationship to others' theoretical conceptualizations, but the repetition of patients' verbal signals was quickly applied to the therapy of psychoneurotic patients.<sup>17</sup>

These studies were reported with impressionistic charts showing reductions in anxiety, tension, and depression and an elevation of self-confidence, initiative, appropriate affect, and perceptual awareness. The development of new traits, if accepted by the patients' relatives on return home, was helpful, he said. When not supported or opposed, the subjects' symptoms recurred. Brief reference is made to Pavlov and Eysenck, but no other integration with thoughtful and original contributors, while the advantages of the method he had devised are stressed. He saw the possibility of this approach as leading to behavioral reorganization in psychoneurotics, a great advantage over low-efficiency methods available through ordinary and prolonged psychotherapy.

In a final paper in this series, Cameron and associates reported findings on eighteen psychoneurotic patients exposed to repetition of verbal signals on an ambulant basis for at least two years.<sup>18</sup> All had somatic complaints as well. It was noted that changes could be brought about more readily if the repetition of verbal signals followed either prolonged sleep or electric shock therapy (ECT), that is to say, that it looked as if under this kind of treatment, after sleep or ECT, the patients became more receptive to the signals they were given. . . . In reporting the results, Cameron noted that no patients of this series had to be readmitted to hospital. This is an example of undue optimism of psychiatric therapeutic expectations. It implies recovery without verification. Some 44 percent were considered recovered and 39 percent improved only, while 16 percent were not. Furthermore, there was an improvement in secondary or associated symptoms in these individuals. Relapse into older forms of neurotic behavior were often modified by increasing the rate and intensity of exposure to verbal signals during times of stress when the patient's condition was appearing to relapse. There is, in the use of the playback tapes, a close relationship to the development of behavior therapy which, however, was in an incipient stage at this point, and is not mentioned in his papers.

Judgment of change in psychiatric patients was long a vexed question before more exacting criteria were introduced in the past few years, so his observations could frequently be assailed as simply impressionistic. That function still has not reached a satisfactory level of reliability, hence much of the criticism that can be leveled at Cameron's research efforts in the papers just reviewed must be seen in this light. Certainly his results did not suffer from lack of optimism, but really trenchant criticism says he attempted too many things before establishing the previous research project on a more profound basis. The too frequent introduction of neologisms, such as "psychic implant," in the same paper<sup>19</sup> enters the realm of unjustified verbal profligacy. Madison Avenue language sits ill in what proposes to be a

scientific exploration of serious psychic mechanisms. Even if the repetitive signals effected little beneficial change, they did not lead to incarceration of the patient or cutting of their brains by leucotomy or, indeed, imposing the neurological consequences of the neuroleptics.

### *ECT Depatterning*

There remains the most controversial of Cameron's innovative endeavors to be examined. This concerns the use of massive electroshock treatment. ECT was apparently introduced by Cerletti and Bini long before it was recorded in Cerletti's 1950 paper.<sup>20</sup> Contemporary use of repeated ECTs seem to have begun with Milligan in 1946, when he described its use in psychoneuroses.<sup>21</sup> Two years later Kennedy and Ancell applied this method to the treatment of schizophrenia,<sup>22</sup> for which they coined the term "regressive shock therapy," which Cameron deemed misleading as it implied a return to childhood behavior. Hence, he suggested the word "depatterning" as a more appropriate appellation. This was discussed in some detail only in his 1962 paper,<sup>23</sup> though it had been used casually in earlier papers . . . in 1957,<sup>24</sup> and shortly after in Canada.<sup>25</sup> Rather than deal with the inadequate description of depatterning in these early papers I prefer to deal with clarification of the procedures and concepts elaborated in the more detailed paper in 1962.<sup>26</sup> This was published some eight or nine years after he began using the massive shock therapy approach. In this paper he defines the method and rationale as follows:

In its original form, the method consisted essentially of the administration of two to four electroshocks daily to the point where the patient developed an organic brain syndrome with acute confusion, disorientation and interference with his learned habits of eating and bladder and bowel control. While in this condition, his schizophrenic symptoms disappeared. On cessation of electroshock—usually after the patient had been given about thirty treatments—reorganization would set in. The organic symptoms would recede quite rapidly and, in favorable cases, the schizophrenic symptomatology would not reappear.<sup>27</sup>

This method seems to have been used with variable benefit by several other workers in addition to the ones mentioned above. Weil treated 18 cases without lasting benefit.<sup>28</sup> Rothschild et al. claimed mild improvement in 25 percent of 52 schizophrenic cases in 1951.<sup>29</sup> These investigators worked prior to the practice of using succinylcholine which, given just after a short-acting barbiturate, induced profound muscular relaxation so that the rather objectionable sight of a convulsion was obviated for the treatment team. The important central action of the electroshock remained, of course, the muscle involvement being residual, only detectable by eyelid twitching. The latest report of

massive electroshock treatment referred to by Cameron was the work of Glueck,<sup>30</sup> who in 1957 reported on 100 cases given three treatments daily. Of 93 cases sent home, 22 relapsed within 8 months. By then Cameron had several years of experience himself with modifications which he considered to be improvements in the technique.

The improvements he introduced contained certain departures from earlier works cited. First, there was the use of succinylcholine as a muscle relaxant. Secondly, there was the employment of sleep therapy as developed by Azima at the AMI.<sup>31</sup> Part of the logic of using this was the recognition that prolonged sleep (using barbiturates and chlorpromazine) led to a confusional state. Hence it was proposed to expedite the brain syndrome by the imposition of electroshock on the highly sedated patient. The important place of chlorpromazine (Largactil) in the induction, maintenance, and recovery from prolonged ECT therapy must be recognized. This neuroleptic was available at McGill in 1953 by virtue of Lehmann's work, published a year later.<sup>32</sup>

Examination of data as it accumulated persuaded Cameron that the disturbance of memory was the central phenomenon resulting from his depatterning work; not a surprising conclusion in view of his long interest in memory. This was of course difficult to measure after ECT so criteria of disturbance based on a space/time image were used, bringing each patient allegedly to the same desired level of disorganization.

Three stages of such space/time disturbance were described in earlier papers.<sup>33</sup> In the first stage, memory deficit was mild but orientation remained; in the second the patient recognized loss of space/time plus anxiety but still could answer some questions. In the third stage, total loss of space/time image, plus loss of knowledge of marital or other basic status occurred, and he might become doubly incontinent. By then all schizophrenic symptoms would be lost as well as other aspects of memory. These three steps have been termed "depatterning," as noted in Cameron's papers, but neither there nor in other relevant papers have I been able to find a precise definition of the "sketchy" word unless one considers the description of the three stages which I have abstracted above. The recognition of the alleged three stages is less clear in the careful description of procedure in the 1962 paper.<sup>34</sup> This contains in detail the use of three barbiturates and chlorpromazine as the basis for sleep but it also emphasizes the advice given the family regarding treatment procedure, and that a memory blank must be expected to occur after recovery from the course of treatment.

A summary of Cameron's method follows herewith. The Page Russell machine was used, starting after three days of sleep and then twice a day until a third stage of disorganization was reached after 30 to 60 shocks; kept at that level for a week, it was gradually reduced to single

shocks three times a week and later to one. Support and reassurance were supplied and anxiety controlled by 600-1000 mg of Largactil. On occasion the reappearance of psychotic symptoms led to repetition of another course of therapy. It was added that no deep psychotherapy was attempted as it was considered dangerous for reasons not disclosed, although it would have been impossible of course. Amnesia is said to be differential,<sup>35</sup> namely amnesia for manifestations of schizophrenia, while recollection of ordinary life happenings return. This is the hoped-for result and is encouraged by the patient's family helping him build what he called a scaffold of memories of normal events. This pretty word from the building trade is hardly appropriate and explains nothing, and scaffolds do fall with dire consequences. In the two years following the patient's follow-up, the patient goes home and gets one ECT a month and moderate doses of Largactil while the therapist endeavors to keep aware of the patient's condition to foresee any tendency to relapse. The follow-up is carried on for three years.

The results were described as complete recovery in one, social recovery in nine, improved in nineteen and one still in hospital. In discussion of effectiveness, the results seem to have been better than other, earlier workers' results. Then, they did not have Largactil. Compared to his own controls of non-treated patients, readmission rate was 10 percent for this series and 31 percent for controls. On the question of his concept of differential amnesia, it is discussed in the 1960 paper<sup>36</sup> and he declared that there is a greater degree of amnesia for pathological events. This is very similar in import to the 1962 paper.<sup>37</sup> Normal happenings (e.g., positive systems), most elucidating delusional impulsive desires, and catatonic signs disappear. The defect symptoms, however, he says are most difficult to obliterate.

#### *Therapy in Chronic Paranoid Schizophrenia*

It is necessary to retrace one's way in his publications, for the earliest were on the treatment of the chronic paranoid schizophrenic patient, though they did not give the background provided in later papers.<sup>38</sup> In the 1957-58 papers,<sup>39</sup> which are really the same one, he describes success with his technique of depatterning in 26 cases of paranoid schizophrenia. Of the 16 more chronic cases, three had to be readmitted and two refused follow-up, i.e., there was judged to be a relative success of 67 percent. Of 10 others with symptoms of less than 2 years, there were none lost or readmitted. One report ends on a most optimistic note and declares that "it is now possible for schizophrenic patients . . . to be passed through a phase of intensive treatment followed by long term rehabilitation measures . . . to live and in many instances, work outside hospital."<sup>40</sup> There are insufficient follow-up reports to verify the validity of this statement but in all fairness it

should be noted that Cameron saw the long term need for follow-up with multi-modal care. The load he carried can hardly be imagined now, for he had at his disposal no trained post-doctoral fellows such as now abound in medical fields including psychiatry. If he was wrong in his reliance on ECT, it should be granted he was right on the need for long-time care and a wide range of therapeutic agencies in the community. What he badly needed in his therapeutic approach was the idea that a mountain to be climbed needs many foot- and hand-holds. He had none. Scientific research advances step by step, not by gross leaps such as he essayed.

#### COMMENTARY AND CRITIQUE

I would like to try and capture some feeling for the time during much of the above work was undertaken. It was the 20-year or 25-year period from 1935 before the concept of adequate controls had assumed a regular place in medical research (and longer for psychiatry, for it had less involvement than medicine in the basic sciences, and therefore was even more laggardly). This was clearly evident in the failure to utilize proper control studies for insulin coma, which only appears 20 years after the method was introduced.<sup>41</sup> Then it became apparent, with the emergence<sup>42</sup> of an appreciation of social factors, that the move from back wards to more salubrious front ones, plus solicitous and appropriate empathic care, exerted a mighty influence. Another concept, namely the placebo, had not reached the level of common notice till the 1950s. When one looks back and regrets the use of so much electroshock it should be remembered that in those years surgical interference with the frontal lobes was enthusiastically undertaken at many respectable centers in the expectation that it would be of benefit in certain psychotic states. The techniques in vogue carried a limited mortality but a not inconsiderable morbidity.

In a retrospective analysis in 1964 of 1983 prefrontal leucotomies performed by him between 1955 and 1957, the late great Canadian neurosurgeon Kenneth G. McKenzie concluded that this operation did not produce a remission significantly beyond that to be expected without the operation.<sup>43</sup> There has been no public remonstrance against this surgical interference with the psyche as has been voiced against ECT. Why not? In part because the leucotomies were performed under the authorization of the operating room, which carries the aura of a temple's sanction, in this case bi-temporal. Furthermore, it was a procedure in line with the hallowed traditions of medicine. It did evoke seizures in some 10 to 15 percent of cases postoperatively, but nothing so crudely fearsome as a convulsion during treatment, even though this aspect of ECT has not involved the ugly massive

muscle contractions since the early 1950s, when succinylcholine was introduced. Another intrusive procedure practiced on the brains of living schizophrenic patients in the early 1950s was the insertion of depth electrodes for the recording of the electroencephalogram.<sup>44</sup> If voices have been raised at this penetration of the patient's brains, the protest has escaped me.

Finally, we must recognize the despairing lack of effective intervention before the phase of insulin coma and ECT, so their overly enthusiastic acceptance is understandable. Then the neuroleptics, beginning with chlorpromazine, provided real humane control of psychotics. The application of the scientific method in psychiatry had begun. Before the mid 1950s, the challenge in the face of mental illness was monumental. This brings to mind the report of Osmond and Smithies in 1950,<sup>45</sup> in which they say that Mr. Gladstone, with that candor which a great statesman can afford, once said, "Government is a rough business, the results are most unsatisfactory." Unfortunately, they say no great psychiatrist has described the present state of our knowledge of the disorder of schizophrenia in an equally robust phrase. But surely rough business and unsatisfactory results would be a fair summing up? So it had been in psychiatry; Cameron's enthusiastic efforts did bring widespread hope to the Montreal scene which had previously been static.

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EVENTFUL YEAR

The year 1962 turned out to be especially full of conflict, tragedy, travel, and the publication of a book to become a classic. The *conflict* occurred concerning the use of ECT in the so-called depatterning technique in which the use of ECT was the predominant therapeutic weapon. Many of us had doubts about the usefulness of this procedure and were particularly concerned about the memory deficit resulting. . . .

My first intimate contact with the treatment came when I was left in charge of the ward where all this was carried out, while the chief went on a tour to Japan. I was early struck by the zombie-like, repetitious, brief greeting given me daily by a girl known to me as a former classmate of my elder daughter. I can't say I was emotionally distraught nor did I develop a fanatical opposition to the goings-on I had inherited on a short-term basis. My background in wartime, which concerned both civilian as well as military casualties, the former starved to the point of death, doubtless left me armored against an immediate affective reaction. The ultimate result of the impact of the difficulties with these cases was my abolishing the procedures three years later when I arrived at a position to change policies.

Apparently Cameron saw that there were limits to the numbers and types of cases susceptible for depatterning therapy because he appointed me to chair a committee to look into the matter and specifically to put a check on the excessive numbers recommended by his only enthusiastic supporter. When my committee met we dealt with each patient recommended for regressive ECT and found that we could not agree that many such cases fell under the rubric of schizophrenia, which was allegedly the diagnosis qualifying the subject for such treatment. Few cases were allowed through our net. We very much doubted the acumen of this man who in other respects had shown a cavalier attitude to responsibilities. Hence the committee which I chaired cut the cases for ECT drastically.

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#### UNCERTAINTIES AND CAMERON'S RESIGNATION

The rest of that memorable year [1962] with its political troubles was seemingly soon behind us, but there was much going on. First, there was evidence that Dr. Cameron was acting imprudently in recruiting staff. Early in 1964, he hired a former resident who had been doing research in psychotherapy elsewhere. He was brought to Montreal presumably in part because of immigration difficulties and he was promised \$20,000 a year, though the source of these funds was never spelled out. I was to inherit this brilliant but difficult man a year later. Other events included the imminent departure of Dr. Hunter, who had accepted the chair in psychiatry at Queen's University while the incumbent, Bruce Sloane, was leaving [Queen's] for a post in Philadelphia. . . . In April, with no advance warning, Cameron placed his resignation before the University. A considerable period of anxious unrest ensued. Late in May I was on a short trip to Maine with my wife and at one of our stops received an unexpected telephone call from my secretary who suggested that I return early as Principal Robertson wanted to see me. I had a fair guess as to what was going on, although I had not been campaigning to succeed Cameron. My interview with the Principal was simple and clear. He wanted me to take over when Dr. Cameron left. To this I agreed though formal announcement was deferred.

It seemed appropriate then to make some gesture of appreciation prior to Cameron's departure. A fund was raised from the staff and an eminent portrait artist Liliias Torrance Newton agreed to do a portrait. Cameron acceded but seemed very edgy, as well he might, for he had another bout of boils. This did not facilitate his sitting and word trickled back that he was not an easy subject. The proof of this unhappy sitting emerged with the portrait which was not satisfactory

to Cameron, nor to his family, and frankly did not seem up to the high quality of other well known portraits by this recognized artist. It must perhaps be put down to the artist's declining powers, for as a subject, the Chief exhibited features calculated to catch an artist's skills.

Matters changed little as the summer wore on. I have no recollection of significant events until the end of August, when Cameron was about to leave; he asked me to join him as he planned to speak to a dissident staff member in his office. The interview was brief but filled with tension, certainly on the Chief's part. He told the man he could no longer tolerate his presence as leader of the service where his performance was unsatisfactory. So, on the spot, he stripped him of that position. This was the kind of confrontation which he usually avoided by delegating the problem to a committee. Why he felt compelled to deal with the issue on this occasion I don't know, nor do I know why he apparently needed my support in this instance. Within 24 hours the man had filed suit against Cameron for alleged interference with his family affairs. This concerned Cameron's seeing the man's wife some weeks before as she said she had been having trouble with him at home. Apparently Cameron said he'd had trouble too at the Allan. The law at that time implied that the husband should have been contacted too. In any case, a suit was instituted and was dealt with in due course.

I am sorry that the Chief had not earlier sat down with me about this and related department matters. I could have profited by his counsel but that wasn't in his nature. He departed without further ado at the end of August.

FINAL ADJUDICATION AND COMMENTS ON D. EWEN CAMERON

Dr. Ewen Cameron was one of the most impressive men I have ever met. As Chief of Psychiatry at McGill from 1943 to 1964, he established a center that became the focal point for the development of psychiatry in the country. He left of his own volition for a position in Albany, from which he would not have to retire at the statutory age of 65. However, the feeling remained that the political climate in the province had got at him and contributed to his decision to move. Sadly, he had only three years to live, and died suddenly during a mountain-climbing jaunt with his youngest son.

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[Cameron had written a book years before in which]... the key words sympathy, patience, insight stood out like beacons.<sup>46</sup> Unhappily, he was unable to bring these magic words to bear prominently on his dealing with disturbed people. He was seldom impatient but he seemed not to enter into the emotional vicissitudes of his patients, that

https://www.utpjournals.press/doi/pdf/10.3138/cbmh.7.1.53 - Tuesday, March 15, 2022 11:14:49 AM - IP Address:70.80.130.111

is to say, to become a participant observer. He saw the need for new techniques, electronic recording of interviews, tape play back, telemetry, studies of memory, but sadly the real burgeoning of scientific inquiry in psychiatry only developed towards the end of his life when he was too far committed to more empirical therapeutic efforts to profit properly. His genius lay in innovation. At McGill, he had developed an open door policy, a day center for patients which, as one of the first in the world, was a template for similar developments throughout the English speaking world. He also promoted research in geriatrics, in psychology, biochemistry, and endocrinology, and to the chiefs of these units he gave responsibility without interference.

The Allan Memorial Institute with its psychiatric beds constituted a wing of the esteemed adjacent Royal Victoria Hospital. Through his efforts, similar clinical units in general hospitals were ultimately set up by seven other centers in Montreal. This helped bring psychiatry closer to the mainstream of medicine. At the same time he developed graduate and undergraduate teaching at a high level unprecedented in Canada, and so well designed that it set a pattern for export. In international fields he played an important role, bringing meetings to Montreal, spurring research interests by his influence in the American Psychiatric Association and by his participation in several psychiatric societies. His entrepreneurial skills, so apt and successful in these fields, were less appropriate and less rewarding in his own research. On the other hand, much of the research he encouraged in his associates paid off handsomely.

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Cameron was clearly concerned with the well-being of men, regardless of national barriers, race or religion. He opposed bigotry and entered public debate with ecclesiastical pundits whose views he attacked by spoken and written word, perhaps a bit recklessly. He engaged a local Anglican bishop in debate over basic religious tenets, he being an agnostic, if not an atheist. This can be seen as the rebellious boy challenging a clerical father. This same stance was apparent in relationship to the greater Wilder Penfield, who had advised him to become a Canadian citizen and learn French. He did neither and effected little liaison with Penfield's Neurological Institute, a positive avoidance as if he feared the godlike presence of Penfield and had to establish his own autonomy.

An insight into his attitude to powerful father figures emerged casually one day when I asked him if, on an impending holiday to Scotland, he would be playing golf. His answer was, "Hell no, Doc, I had my belly full as a boy. I used to caddy for my father and after 18 holes at 1 p.m. would say, 'Now Ewen, we will go another 9 holes.' And you know what a pre-teenager's stomach is like when hungry,

rubbing up against his spine." This small vignette says something about an inconsiderate, powerful father—to be later avoided at all costs, though having provided a pretty solid ego. A considerate streak for others' feelings emerged on one occasion involving a patient of mine who committed suicide while I was at a meeting in Atlantic City. Cameron called me long distance with reassurances to assuage any sense of guilt I might have in the circumstances. But this kind act was not accompanied by other efforts on his part to socialize. He kept his private life and his professional life distinctly separate. On occasion he held a lunch at the Institute for visiting worthies. One I remember well was arranged for Sir David Henderson, the old Chief from Edinburgh by whom I sat at lunch. By then an old man, he said to me in the loud voice of the hard of hearing, "He's done very well for a man who hasn't overcome his adolescent troubles."

In the field of committees and politicking, Cameron had no peer. He used his expertise in committee work to raise funds and get extensions to his main building. He also had as a device for getting around personality clashes, delegating sticky issues to a subcommittee, thereby squelching confrontation. Alternatively, he delegated dealing with personality problems with his staff whom he wished to avoid, to me. He managed to become President of most psychiatric organizations of any consequence, the last being the World Psychiatric Association, which was an organization largely of his building. Some of the European competition was fairly skillfully sidetracked with the aid of American and English colleagues whom he conned into supporting his candidature.

One eminent man he avoided was Sir Aubrey Lewis, Chief at the Maudsley Hospital in London and the leading academic figure in Britain for 30 years. They had been residents at Johns Hopkins together and had a falling out then. Aubrey was not only erudite in several languages but not above one-upmanship and interjection in psychiatric argument on rounds. Be that as it may, there is a tale that as residents one wanted the other to go halves on a motor boat or motorcycle and the other refused. So the main leaders on two sides of the Atlantic Commonwealth continued not only to differ but to avoid each other. Cameron's students went to Maudsley, Maudsley men came to McGill but made their own arrangements. The personality differences in these two powerful men could not be more clear cut. Lewis was the academic scholar and critical teacher. Cameron was creative in his organization of an institute where teaching and training flourished. He built a useful empire but, no scholar, was lenient with his staff and students. Somewhere, in his avowed scientism, lingered something of the artist in the lucidity and beauty of expression in his writings from time to time. . . . This clarity was not seen in his scientific reporting,

which was neither precise nor sufficiently searching, and had an airy quality that defies description. This aroused in the reader a skepticism as to the quality of the underlying information, hence fostering disbelief. His personal researches are better noted for their variety than for a continuing thread of endeavor. Penfield, at the Montreal Neurological Institute, had a technique for exploring the brain by electrical stimulation which he pursued for years with fruitful results. Hebb, in psychology, spent years in testing for brain defect and then elaborated a widely acclaimed concept in his book, *Organization of Behavior*. Later Cameron exploited novel approaches such as Hebb's sensory isolation technique but had no such personal or fruitful innovative method as did Hebb. It was imperative that the vast field he attempted to tackle be approached piecemeal, and methods for such an attack remained to be developed. He showed impaired judgment by bringing in some oddly-assorted young men to assist in special projects. They proved to be indigestible people who, when the hypomanic flood ran out, were a stranded nuisance. More than one proved to be a psychopathic character for whom he had unhappily a blind eye.

One of his major endeavors, begun in the early 1950s, involved the use of analgesic sleep and repetitious use of ECT. Sir Aubrey Lewis characterized it to me in 1957 at the Zurich psychiatric meeting as "barbaric." The idea was to blot out psychotic behavior and re-educate the patient. The theoretical basis was not only slim but dubious and led ultimately to some unfortunate results: lawsuits for memory loss which went on into the 1980s. It was a sad denouncement to a career which had built so much. A colleague has wondered if the technique was not designed to exorcise the witch-like specter of schizophrenia! If it had been successful, a Nobel Prize was there, as Cameron well knew, and one time he wistfully remarked to me on the number of investigators who had achieved the prize in the field of RNA, which he was also exploring. My cautionary remarks in a 20-year-old paper entitled, "Pitfalls in Thinking Big" are pertinent here.<sup>47</sup> It is tragic that he did not follow his original concept of objective and experimental psychiatry, which he early espoused, with great rigor. His facility with words somehow betrayed him into feeling that his eloquent utterances carried meaning beyond the significance of the work or the occasion. Had he acquired more depth of scholarly and scientific learning, he might have created a synthesis of the scientific views to which he subscribed but this would have meant submitting to the wisdom of the authority of those great pioneers in psychiatry such as Kraepelin, Meyer, and Freud, that is, to father-figures, a thing he could not do. His life was too hurried and diffuse to pause long enough to determine what he was escaping. . . .

Cameron did not decide how he could further capitalize on his considerable clinical abilities, for they were extensive and his estimate

of possible appropriate therapeutic measures was at times uncanny, but these divinations remained outside both his ken and his appreciation. His failure to pause and contemplate deeply several emerging therapeutic issues impoverished his progress. He seemed to miss the full significance of the phenothiazines in 1954, though he employed them enthusiastically, and the antidepressants in 1958, for which he organized one of the first research conferences. He seized on the use of Lithium in 1960 with success in a manic case but let the matter drop, failing to exploit a new field which carried such potential for a therapeutic revolution, which has by now occurred.

He stayed with his "depatterning" program, repressing consciousness out of his patients, trying then to build them up in a fresh image. It was therapy gone wild with scant criteria. The zeal of one acolyte exceeded his own so he formed a committee to limit the number of cases proposed by this man for this specific therapy. The last thing he did before leaving was to realize fully the deficiencies in the activities of this individual in another area, and that he was indubitably a menace. Therefore, he stripped him of his responsibilities. It was left to me to control this dissident's continuing excesses and force him out of the department. One may criticize Cameron's uncritical therapeutic abandon, which left a wake of uncontrolled variables. One may also speculate that there was a flaw in his approach to his own work that he hoped would bring unqualified distinction. By not exercising more critical appraisal in the planning and execution of his experiments he failed to achieve the scientific standards and excellence which would not have left him open to the criticism of more basically oriented investigators such as Hebb, who expressed dismay. In summary, he was inclined to be stubborn, oppositional, and competitive with figures of authority, and was not given to emphasize collaboration. His intelligence, vigor, and drive to reach new horizons left him little time to dally with his peers, and this led him to making some enemies in the political arena. In the aura of practical research, which was not his personal strong point, his own personality became his own worst enemy as he could not tap the scientifically restraining milieu provided by a congenial group of colleagues in evaluating ideas and experiments.

After his departure, the flimsy financing of the department became apparent, e.g., funds for the assistant he had recently acquired had not been adequately provided. Several other individuals who had once been useful had to be removed as they had been allowed to stay on in ineffective roles using valuable funds.

I cannot but feel kindly for the Chief who was a phenomenon, and whose foresight and creativity organized teaching and training which spread many of his sound precepts widely. But he did not become a model for many. He inspired awe and admiration but no affection or

identification in his students. He really had no pupils who followed his hectic path.

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#### TRANSITION

Cameron's leaving in August 1964 did not end depatterning, so I appointed a group to adjudicate. The report by Schwartzman and his committee on the use of drugs and multiple ECT leading to a confusional state had been given at a Canadian Psychiatric Association meeting in Edmonton in October 1965. It consisted of a mild statement but a clear conclusion to the effect that the process of "depatterning" employing multiple ECTs and drugs as used by Dr. Cameron had no advantages over the less dramatic treatment with Chlorpromazine or allied drugs alone.<sup>48</sup> This report supported the impression that many of us had acquired on the basis of clinical observation. It supplied objectively derived data and encouraged me to lay down a rule in November that no more than ten ECT treatments were to be given in any one course of therapy without my explicit approval.

A few months later, in February 1966, I found this edict was being broken and a patient was well on the way to being depatterned. This led to a confrontation and argument with the psychiatrist responsible, but this did not forestall another instance of his departure from the principle of restrained therapy. Trouble continued to simmer, so in August I advised the dissident party that his reappointment in January would not be renewed. This was supported by the hospital authorities but there followed a suit against the hospital and various legal acts before his appeals for reinstatement were turned down by the courts of Canada. Meanwhile, at the meeting of the World Psychiatric Association in Madrid in the summer of 1966, I encountered Dr. Cameron, who said, in an ordinary conversational manner, "I thought you would want to discontinue that method."

There is a principle emanating from our experience with this dissident whose continued variance to authority made regulation of procedures to conform to desirable standards difficult to achieve. Unfortunately, his approach was tied into a system originated by the previous chief to utilize a therapeutic technique which was neither widely accepted nor based on sound experimental or theoretical grounds. In a sense, both teacher and pupil flouted authority. Deviants are sometimes creative, but deviance in thinking and behavior takes many forms in psychiatrists as well as in other ordinary mortals, and even fundamental scientists who subscribe to objectivity are not immune as they strive for recognition. I have dealt with this topic in a relatively light vein some years ago. . . .<sup>49</sup> If I were forced to name a single significant twisted bit of thinking other than outright cheating, I would

have to say that activity associated with a blind desire to make a discovery is the chief element. This is *hubris* and is accompanied by a failure to assess evidence dispassionately and finally to a delusional belief in spurious evidence and a set of assumptions erected to support, justify, or explain the developed hypothesis.

## NOTES

- 1 Don Gillmor, *I Swear by Apollo: Dr. Ewen Cameron and the CIA-Brainwashing Experiments* (Montreal: Eden Press, 1987); Anne Collins, *In the Sleep Room: The Story of the CIA Brainwashing Experiments in Canada* (Toronto: Lester & Orpen Dennys, 1988); Harvey Weinstein, *A Father, A Son and the CIA* (Toronto: James Lorimer, 1988).
- 2 D. Ewen Cameron, *Objective and Experimental Psychiatry* (New York: Macmillan, 1935).
- 3 D. Ewen Cameron, "A University-Centered Postgraduate Training Network," *Canadian Medical Association Journal*, 73 (1955): 112-16.
- 4 D. Ewen Cameron, "The Current Transition in the Conception of Science," *Science*, 107 (1948): 553-58.
- 5 D. Ewen Cameron, "The Psychiatric Training Network of McGill University," *American Journal of Psychiatry*, 120 (1964): 1039-44.
- 6 D. Ewen Cameron, "Further Experiences in the Insulin Hypoglycemia Treatment of Schizophrenia," *Journal of Nervous and Mental Disorders*, 87 (1938): 14-25.
- 7 B. Ackner, A. Harris, A. J. Oldham, "Insulin Treatment of Schizophrenia: A Controlled Study," *Lancet*, 272 (1957): 607; and L. Boling, W. Ryan, and M. Greenblatt, "Insulin Treatment of Psychiatric Patients: Comparative Results of Deep and Light Coma Treatment," *American Journal of Psychiatry*, 113 (1957): 1009-12.
- 8 D. Ewen Cameron, "Impairment of the Retention Phase of Remembering," *Psychiatric Quarterly*, 17 (1943): 395-404.
- 9 D. Ewen Cameron, "Further Studies in the Use of Adrenalin in the Treatment of Persistent Anxiety States," *Psychiatric Quarterly*, 20 (1946): 425-533.
- 10 D. Ewen Cameron, "The Processes of Remembering," *British Journal of Psychiatry*, 109 (1963): 325-40.
- 11 D. Ewen Cameron, "Effects of Ribonucleic Acid on Memory Defect in the Aged," *American Journal of Psychiatry*, 120 (1963): 320-25.
- 12 Cameron, "The Processes of Remembering."
- 13 L. Solyom, personal communication, 1984.
- 14 Cameron, *Objective and Experimental Psychiatry*.
- 15 D. Ewen Cameron, *General Psychotherapy* (New York: Grune & Stratton, 1950).
- 16 D. Ewen Cameron, "Psychic Driving," *American Journal of Psychiatry*, 112 (1956): 507-09.
- 17 D. Ewen Cameron, L. Levy, and L. Rubinstein, "Effects of Repetition of Verbal Signals upon the Behaviour of Chronic Psychoneurotic Patients," *Journal of Mental Science*, 106 (1960): 742-54.
- 18 L. Levy, D. Ewen Cameron, T. Ban, and L. Rubinstein, "The Effect of Long-term Repetition of Verbal Signals," *Canadian Psychiatric Association Journal*, 10 (1965): 265-70.
- 19 Cameron, "Psychic Driving."
- 20 U. Cerletti, "Old and New Information about Electroshock," *American Journal of Psychiatry*, 107 (1950): 87-94.
- 21 W. L. Milligan, "Psychoneuroses Treated with Electrical Convulsions," *Lancet*, 251 (1946): 516-20.
- 22 C. J. C. Kennedy, and D. Ancell, "Regressive Shock Therapy in Schizophrenia Refractory to Other Therapies," *Psychiatric Quarterly*, 22 (1948): 317-20.
- 23 D. Ewen Cameron, J. G. Lorenz, and K. A. Handcock, "The Depatterning Treatment of Schizophrenia," *Comprehensive Psychiatry*, 3 (1962): 65-76.

- 24 D. Ewen Cameron and S. K. Pande, "Treatment of the Chronic Paranoid Schizophrenic Patient," *Second World Congress of Psychiatry, Zurich* (1957), Vol. 1, p. 95-99.
- 25 D. Ewen Cameron and S. K. Pande, "Treatment of the Chronic Paranoid Schizophrenic Patient," *Canadian Medical Association Journal*, 78 (1958): 92-96.
- 26 Cameron et al., "The Depatterning Treatment."
- 27 Cameron et al., "The Depatterning Treatment."
- 28 P. W. Weil, "Regressive Electropexy Shock Therapy," *Journal of Mental Science*, 96 (1950): 514-20.
- 29 D. Rothschild, D. J. Gordon, and A. Varjabedian, "Regressive Shock Therapy in Schizophrenia," *Disorders of the Nervous System*, 12 (1951): 147-50.
- 30 B. J. Glueck, H. Reiss, and L. E. Bernard, "Regressive Electric Shock Therapies," *Psychiatric Quarterly*, 31 (1957): 117-36.
- 31 H. Azima, "Prolonged Sleep Treatment in Mental Disorders: New Psychopharmacological Considerations," *Journal of Mental Science*, 101 (1955): 593-603.
- 32 H. E. Lehmann, and G. E. Hanrahan, "Chlorpromazine: New Inhibiting Agent for Psychomotor Excitement and Manic State," *Archeology, Neurology, Psychiatry*, 71 (1954): 227-37.
- 33 Cameron and Pande, "Treatment of the Chronic Paranoid," *Zurich*, p. 95-99; Cameron and Pande, "Treatment of the Chronic Paranoid" (1958), 92-96; Cameron et al., "The Depatterning Treatment"; and D. Ewen Cameron, "Production of Differential Amnesia as a Factor in the Treatment of Schizophrenia," *Comprehensive Psychiatry*, 5 (1960): 26-34.
- 34 Cameron, et al., "The Depatterning Treatment."
- 35 Cameron, "Production of Differential Amnesia."
- 36 Cameron, "Production of Differential Amnesia."
- 37 Cameron, et al., "The Depatterning Treatment."
- 38 Cameron, et al., "The Depatterning Treatment"; and Cameron, "Production of Differential Amnesia."
- 39 Cameron and Pande, "Treatment of the Chronic Paranoid," *Zurich*, p. 95-99; Cameron and Pande, "Treatment of the Chronic Paranoid" (1958), 92-96.
- 40 Cameron and Pande, "Treatment of the Chronic Paranoid," *Zurich*, p. 95-99.
- 41 Ackner, Harris, and Oldham, "Insulin Treatment of Schizophrenia," 607; Boling, Ryan, and Greenblatt, "Insulin Treatment of Psychiatric Patients," 1009-12.
- 42 Robert A. Cleghorn, John M. Cleghorn, and Fred D. Lowy, "Contributions on Behavioural Sciences to Health Care," *Millbank Memorial Fund Quarterly*, 49 (1971): 158-74.
- 43 Kenneth G. McKenzie, and G. Kaczanowski, "Prefrontal Lobotomy: A Five-year Controlled Study," *Canadian Medical Association Journal*, 91 (1965): 1193-96.
- 44 E. D. Wittkower and Raymond Prince, "A Review of Transcultural Psychiatry," in Silvano Arieti, ed., *American Handbook of Psychiatry* (New York: Basic Books, 1974), Vol. 2, p. 535-50.
- 45 Humphrey Osmond and J. Smythies, "Schizophrenia: A New Approach," *Journal of Mental Science*, 98 (1952): 309.
- 46 Cameron, *Objective and Experimental Psychiatry*.
- 47 Robert A. Cleghorn, "Pitfalls in Thinking Big—Megalomania," *Psychiatry Quarterly*, 38 (1964): 607-18.
- 48 A. E. Schwartzmann and P. E. Termansen, "Intensive Electroconvulsive Therapy: A Follow-up Study," *Canadian Psychiatric Association Journal*, 12 (1969): 539-51.
- 49 Cleghorn, "Pitfalls."