THE CAREER OF GORDON MURRAY

Patterns of Change in Mid-Twentieth Century Medicine in Canada

“A Thesis submitted in conformity with the requirements for the Degree of Doctor of Philosophy, Graduate Department of History, in the University of Toronto”

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This thesis describes some aspects of mid-twentieth century developments in Canadian medicine through the exceptional career of Gordon Murray (1894-1976). His Toronto surgical career spanned over fifty years during which significant changes in medical education and training, surgery, clinical investigation, experimentation and treatment, and medical reporting occurred. Murray conducted his surgical operations and clinical investigations skillfully and independently with remarkable successes during the first thirty years of his career. His boldness and individualism were both his strengths and weaknesses, awarding him both success and failure. In the latter years of his career, Murray made several bad professional choices, misreading the future and misjudging his own abilities. It became clear that he was unable and unwilling to adapt to the new postwar medical culture and its increasingly specialized, team-oriented practices. His lone practices and experimental approach to medical problems, which had been earlier strengths, became liabilities in this new research world. He began exploring new and complex research questions independently and ineptly, and an impressive career ended tragically as a result.
Murray was a talented surgeon with a fertile, creative and restless mind, who thought broadly and confidently about tackling complex medical problems. His diverse and impressive list of contributions reflects this range of interests. He investigated the clinical application of heparin, expanded the possibilities of vascular surgery, performed the first congenital heart or “blue baby” operations in Canada, pioneered heart valve and renal transplant procedures, and built the first North American artificial kidney machine. Later in his career, he pursued research on cancer sera and vaccines as well as spinal cord regeneration, controversial work that ultimately damaged his reputation. His boldness and innovation contributed to both his success and downfall as a surgeon and investigator. Confident and ambitious, Murray spent a lifetime searching for answers to some of medicine’s greatest challenges during a period of tremendous surgical advances and research changes that eventually overcame him.
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Introduction

Tall and handsome, Gordon Murray walked briskly through the ward. He was a lean, well-groomed middle-aged man with short, graying hair and a clean-shaven face. Weekly tennis matches and other outdoor activities kept him trim and agile. A hospital coat covered his conservative suit and tie, which he would exchange later for surgical garb. Medical students scurried behind him, scribbling notes, trying to anticipate their teacher's questions on the next case.

Murray enjoyed his role as teacher, but at that moment his thoughts were focused on his next patient, a young girl admitted for heart surgery. The group turned the corner and entered the semi-private room. The child lying in bed was blue in colour, and her frightened eyes watched Murray cross the room. Anxious parents made way for the doctor as he walked towards the bedside. They looked pleadingly at him to fix their little girl's heart. Murray smiled, and in a soft voice, reassured the family that surgery would correct the problem. He possessed a quiet but commanding presence. The chiseled features of his face gave Murray a look of unquestioned authority, and his paternalistic
manner exuded confidence to even the most fearful patients. They willingly placed their lives in his hands, and with good reason.

The man who was to save this little girl was by the 1940s one of Toronto's most sought after medical practitioners. Murray was a prominent, well-respected surgeon, with an impressive record according to both professional and public discourse. From appendectomies to thymectomies, heart operations to orthopedic procedures, bowel resections to mastectomies, he performed a range of procedures with great success. He was clever and creative, dedicated and devoted. His ingenuity and long hours in the laboratory produced a number of innovations from cardiac procedures to artificial kidney machines, and he received public praise and professional honours. His skill and style impressed colleagues, nurses, students and patients, and it was Murray they called when in trouble. He was bold, decisive and confident, and operated with absolute precision and technical proficiency. His surgical dexterity and mastery of anatomy mesmerized his surgical team. He loved to operate, and students, junior colleagues and visiting medical men loved watching him. He embodied the contemporary surgical spirit -- surgery as science and craft -- working in the laboratory and the hospital, conducting clinical research and introducing new treatments to often-desperate patients.

This thesis studies the career of this surgeon and by doing so also describes some aspects of mid-twentieth century developments in Canadian medicine. Few such contextual accounts of surgical lives have been written. Equally lacking, the existing

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literature on the history of surgery and medical research offers little more than a listing of surgical firsts and discoveries in the laboratory. Murray's life is a story of triumph and tragedy, a personal drama of clashing personalities, individualism and ambition, spanning more than fifty years during which significant changes in medical education and training, surgery, clinical investigation, experimentation and treatment, and medical reporting occurred. He actively participated in many of these changes, making contributions that advanced the profession's abilities to treat many disorders. His list of achievements of Dwight Harken set within the larger context of the American cardiac surgery community; Thomas Thompson's Hearts (1971), a journalistic account of the daily struggles of heart surgeons Michael DeBakey and Denton Cooley of Houston, Texas; W. Nolen's The Making of a Surgeon (1968), a somewhat dated but still useful autobiographical account of what makes a good surgeon; and Jürgen Thorwald's The Dismissal: The Last Days of Ferdinand Sauerbruch (1962), a remarkable surgeon struggling with cerebral sclerosis but who refused to acknowledge his illness and its effects despite horrible surgical errors.

2 The history of surgery is predominantly characterized by detailed accounts of surgical "firsts" and their innovators, usually written by surgeons swayed by technological determinism in their interpretation of surgery as a progressive march forward "from empiric craft to scientific discipline". L. Zimmerman, Great Ideas in the History of Surgery (1961), Allen O. Whipple, The Evolution of Surgery in the United States (1963), Frederick Cartwright, The Development of Modern Surgery (1967), and R. Meade, An Introduction to the History of General Surgery (1968) provide such accounts. O. Wangensteen and S. Wangensteen's study, The Rise of Surgery (1978), is considered the standard work in the field, followed by R. Richardson, Surgery: Old and New Frontiers (1967). These are two of the better books on the history of surgery, however they are dated. There are no recent general surgical histories. For the most part, historians of surgery in the 1970s and 1980s turned to writing histories of surgical specialties, such as S. Johnson, A History of Cardiac Surgery (1970), S. Friedman, A History of Vascular Surgery (1989), and H. Shumacker, The Evolution of Cardiac Surgery (1992). These are quite limited in scope. Less interested in providing a list of surgical innovations and innovators, medical historians Gert Brieger, "A Portrait of Surgery" (1987) and Christopher Lawrence, Medical Theory, Surgical Practice (1992) ask different questions and focus on the surgical concept of disease and the relation of surgical ideas to practice. These works should prompt scholars to re-visit and perhaps re-interpret the history of surgery. Brieger and Lawrence argue that surgeons redefined treatment (as in the case of the heart) and reject the argument that technical innovations were responsible for the "advance" of surgery. Their emphasis on the shifting of surgical thought in the twentieth century shall be considered in this study of Murray's career, specifically how operative treatment of heart disease became acceptable after such strong denouncements by surgeons T. Billroth and S. Paget. The history of medical research, particularly clinical investigation, in Canada is spotty and incomplete. M. Thistle, The Inner Ring (1966), W. Eggleton, National Research in Canada (1978) and R.A. Jarrell and Y. Gingras, Building Canadian Science (1991) examine the role of the National Research Council of Canada and the nature of scientific research in general in Canada. Sandra McRae's study, "The Scientific Spirit in Medicine at the University of Toronto, 1880-1910", (1987) and Michael Bliss' books, The Discovery of Insulin (1982) and Banting (1984) discuss the emergence of Toronto as a reputable medical research centre in the early twentieth century. Several accounts of Canadian innovations and innovators exist such as W. Bigelow's Mysterious Heparin (1990) and Cold Hearts (1984), M. Dunlop, Bill Mustard: Surgical Pioneer (1989) and J. Lewis, Something Hidden: A Biography of Wilder Penfield (1981). Devoid of context, these last works are limited in their ability to present the significance of these surgical innovations and innovators in the larger setting of medical history and clinical research in Canada.
include the clinical application of heparin, vascular and orthopedic surgical procedures, blue baby operations, heart valve and renal transplantations. Moreover, his professional life straddled two different medical cultures, and offers insight into the contrasting nature of these periods and the difficulties that this practitioner, and probably many others, faced in adjusting to these changes. In this study, the shifting nature of medical education and training, surgery, clinical investigation, experimentation and treatment, and the role of medical reporting will be addressed in relation to this man’s career.

Murray was a general surgeon, traditionally trained during the post-Flexner medical era (1910s to 1940s) to work proficiently in all aspects of surgical practice. He learned to be a surgeon by seeing surgery practised and then increasingly participating in surgery during an extended apprenticeship in Canada, England and the United States. He learned how to conduct clinical research in much the same way. He followed the example of his surgical mentors, not research scientists. As a clinician, he dabbled in research, experimenting with new techniques when presented with patient cases for which he could offer no satisfactory procedures. It was a medical culture in which part-time clinical investigation was encouraged, crude facilities were made available, and individualism and boldness often resulted in new surgical advancements and research contributions. Experimental procedures moved quickly from the laboratory to the bedside when beneficial results seemed promising.

In addition to his training, Murray’s personality befitted the post-Flexner medical culture. He was a curious man, with a fertile, creative and restless mind, a ‘Victorian-minded’ man who thought broadly and confidently about tackling complex medical
problems. In line with his Victorian sense of knowledge, Murray's interests and ingenuity extended beyond medicine, to such fields as nature, agriculture, and home repair. He was brilliant and innovative, an inventor and amateur scientist, eccentric and explorative in his pursuits. He preferred to work alone, and sought absolute control and autonomy over his projects.

Murray's rise and success in the post-Flexner medical era is presented in chapters one through four of this thesis. He trained and triumphed in this framework, his individualism and boldness forging surgical advancements and research contributions, which in turn brought him significant professional recognition and public praise. In the first chapter, his education and training as a surgeon is described. Individual and cultural influences had as much to do with his success as did his aptitude and personality. Changes in medical and surgical education had occurred not long before he enrolled at the University of Toronto, and Murray emerged a product of his 1910s and 1920s training. More than his predecessors, he was inclined to spend time in the laboratory as well as in the operating room. Chapter Two outlines his research on the clinical application of heparin in the 1930s, which brought him substantial professional recognition and assured him a place in medical history. It offers a window into how research was conducted in Toronto in the years after the discovery of insulin, and how Murray's role in this cooperative research venture early in his career was indicative of later behaviour. Chapter Three describes the advances and glory of heart surgery at mid-century, an emerging specialty that illustrates the rapid changes then taking place in

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3Numerous historians have argued the existence of a distinct Victorian mind with respect to both science and knowledge generally. Gertrude Himmelfarb states, "It is the faith of the self-educated man that nothing is beyond his means, that all knowledge must submit to a firm will and good sense. This is also the creed of the amateur." Gertrude Himmelfarb, Victorian Minds (Gloucester, Mass: P. Smith, 1968), p.211-12.
medicine. Murray became known as Canada’s “blue baby doctor” because of his surgical success with congenital heart conditions. The media declared him a hero and miracle maker. The role of medical reporting and image-making is discussed as well as Murray’s increased individualism and egoism as a result of public and professional acclaim.

Chapter Four examines more of Murray’s research achievements during this period: his artificial kidney machine and his early kidney transplantation operations. At the same time that he was performing his heart operations, he was also exploring new treatments for acute and chronic kidney disease, with some success. The range of Murray’s abilities appeared to have no boundaries.

The same characteristics that brought Murray success in his medical career, however, also created difficulties for him. Alongside his individualism and boldness, he was determined, impatient, egotistical, and cantankerous. He never worked well with his Toronto peers or senior medical authorities. This friction plagued almost all of his daily working relationships, beginning early in his career at the Toronto General Hospital and slowly escalating to professional confrontation and chastisement. His demands upon and expectations of the Toronto medical community increased over time, and for the most part, senior medical authorities tolerated Murray’s behaviour. Individualism and eccentricity were often characteristics of a brilliant mind, and most medical men recognized this in Murray. Often it was Murray whom less competent surgeons called to consult or to complete difficult operations. Within the post-Flexner medical culture, the profession awarded Murray respect and leniency because of his surgical skill and past medical contributions. There was less tolerance in the postmodern medical culture, which frustrated and embittered Murray.
The postmodern medical culture emerged after World War II and is distinctive for its expansion in medical specialism, technology and team orientation.\textsuperscript{4} Surgeons began gaining expertise in, and limiting themselves to, newly developing specialties of heart, chest, orthopedic, and neuro-surgery, among others. General surgeons were relegated to relatively minor procedures unclaimed by surgical specialists. The conduct of medical research was also transformed in this period. It moved beyond the dabbling of part-time investigators to the careers of full-time researchers. Trained in more objective, rigourous research methodology and techniques, applying new experimental models such as double-blind randomized clinical trials, they worked as part of interdisciplinary teams within large, well-funded, technologically sophisticated laboratories.

Murray was unwilling and unable to adjust to these changes. His lone practices and experimental approach to medical problems, which had been earlier strengths, became liabilities in this new research world. Furthermore, his work was evaluated less on his reputation and more on its validity as a research project. Murray made critical errors in judgement during this period, continuing to operate broadly on various surgical conditions and choosing to explore new and complex research questions independently and ineptly, neither understanding nor exploiting the new medical culture.

The last four chapters of this thesis present the second half of Murray’s career, when he was practising within this postmodern medical culture. Chapter Five describes the transformation of medical research in Canada in the mid-twentieth century and Murray’s decision to became Director of the W.P. Caven Memorial Research Foundation

\textsuperscript{4}Edward Shorter defines postmodern medicine as beginning in the late 1940s as the result of a revolution in diagnosis and drug therapy. The concept of modern and postmodern medical cultures is taken from Edward Shorter, \textit{Doctors and Their Patients: A Social History} New Brunswick, New Jersey: Transaction Publishers, 1991, pp. 75, 179.
(later the Gardiner Memorial Research Foundation). What might have been a successful Canadian alternative to the university research structure failed because Murray was unable and unwilling to adjust to the many changes occurring in medical research funding, facilities and conduct in the postmodern period. In Chapter Six, his work on anticancer sera and vaccines is studied. He did not deliver a cure for cancer, despite the hope of Murray and the public for that much-wanted breakthrough. Instead his cancer research showed that Murray was an innovative but aging clinical investigator working beyond his capabilities. Chapter Seven further demonstrates this point. His new surgical procedure, based on his work with spinal cord regeneration, intended to make paraplegics walk again. Public displays of previously paralyzed individuals standing, even walking, created great excitement and publicity, and the discovery of spinal cord regeneration promised to be a glorious finale to Murray’s extraordinary career. But he overstated his research success and misled the medical community – serious transgressions in the postmodern research world – and he retired amid allegations of fraud and scientific dishonesty. It was a public and professional fall from grace, and a tragic ending to an impressive career. The final chapter offers some concluding remarks on Murray’s professional life, his career choices, and the changing medical cultures in which he worked.

Murray spent a lifetime searching for answers to some of medicine’s greatest challenges. In positioning his professional activities within the shifting mid-twentieth century medical cultures, his success and failures can be better understood. Moreover understanding the changes occurring in the medical culture offers context to the nature of his achievements and struggles. Overall, Murray led an exceptional career — celebrated
surgeon, creative inventor, controversial researcher, an ambitious and determined individual; he was a leader in his time, but changes in medical and research practices, and subsequent personal choices, eventually overcame him.
Chapter 1

Becoming a Surgeon: Education and Training

Donald Walter Gordon Murray was born May 29, 1894 near Stratford, Ontario. He was the fifth of seven children born to John and Elizabeth Murray. His father was a Scottish immigrant and skilled stonemason who came to Canada to farm. His mother was Canadian-born and grew up on a prosperous 500-acre farm in Oxford County, not far from where she later raised her own family. Soon after their marriage, John and Elizabeth purchased a 434-acre farm. They managed crops and livestock and operated a sugar bush. All of their children, including Gordon, at an early age contributed to the running of the family farm -- feeding the chickens, milking cows, splitting wood, or collecting sap.¹

When young Gordon was not helping out on the Murray farm, he was off with friends to swim in the Thames River or to participate in other adventures. He loved the outdoors and never tired of examining insects, frogs, trees, flora and other living structures. His mother stirred his interest in nature at an early age, and he listened

¹Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.
with fascination as she explained the numerous changes taking place around them. The shedding of a snake's skin, the fertilization of hen's eggs, and the sprouting of bean seeds were some of his first scientific observations. Walks along the stream or through the fields introduced young Gordon to the subjects of biology and physiology.²

Gordon attended the nearby country school, and later Stratford High School, almost eight miles from home. Like other children in the area, he biked or walked to school. According to family lore, during the severe winters Gordon and his brothers jumped from fence post to fence post to stay out of the deep snow. On Sundays, the Murray family attended services at the Presbyterian Church in Harrington. Evenings were spent reading as a family, most often from the Bible.³

Elizabeth and John were strong advocates of education and self-improvement, and all of their children were encouraged to pursue professional careers.⁴ Gordon's older brother William went off to the University of Toronto and graduated as an engineer. It was expected that Gordon would follow in his footsteps, if not in engineering then in some other profession. Gordon decided to study medicine.⁵ He


³Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; Personal correspondence, Mrs. Hilton (Rosina) Morris to author, August 1996; Personal correspondence, Mrs. Jack (Ann) Scholefield to author, 7 October 1996.

⁴John and Elizabeth's seven children in descending order: William (engineer), Jack (farmer), Fanny (died as child), Sara (nurse), Gordon (surgeon), Allan (died in WWI), Charlie (remained on family farm). Family information provided by the Murray family.

⁵According to family lore, older brother William had wanted to study medicine but the program was full. Ironically, Murray had intended to enrol in the engineering program but it was full so he went into medicine. Personal correspondence, Mrs. Jack (Ann) Scholefield to author, 7 October 1996.
looked forward to studying medical ailments and disorders, to exploring the human body, and to learning diagnostic and therapeutic methods of fighting disease.

Murray was entering a profession that had undergone significant change in the last few decades. The length and quality of medical education and training have shifted over time, and different countries have placed varying emphasis on the components within that schooling. To become a physician or surgeon required a longer period of preparation than almost any other profession. Years of study and clinical experience were required to transform bumbling students into competent and skilled practitioners. Numerous "training tales" have described the long hospital hours and difficult professional examinations demanded of men and women pursuing medical careers – stories that celebrate this training as a necessary rite of passage for each doctor who, according to these “tales”, inevitably emerges as hero and somehow unique.6

Murray's training, first as a physician and later as a surgeon, illustrates the context of practising medicine and surgery in the early twentieth century as well as the ideals behind being a good medical practitioner. More importantly, it becomes clear that during Murray’s training, individual and cultural influences had as much to do with his success as a medical practitioner as his aptitude and personality. His extensive training in Canada, Britain and the United States exposed him to different cultural attitudes and approaches towards surgery and medicine, which influenced his

outlook and style. For Murray, these were formative years that shaped the type of surgeon he was to become.

A shift in the education of physicians had begun in the late nineteenth century. Scientific advances, notably the rise of bacteriology, forced medical educators to consider inclusion of the new laboratory sciences in the medical curriculum. They debated the appropriate balance between academic study and clinical experience. Which served as the better training ground for the medical student — the university or the hospital? What role did the new laboratory science play in the traditional clinical training of the physician? Historian Thomas Bonner suggests that Johns Hopkins University and hospital, created between 1889 and 1893, became the model for the new scientific medicine in America. It emphasized scientific theory and laboratory instruction followed by clinical experience. In Canada, as R.Gidney and W.Millar point out, medical educators readily adopted laboratory science into the curriculum, thereby shifting control away from proprietary schools to the university.

Great Britain, France, Germany, the United States and Canada all came to realize the importance of scientific understanding to medicine. By the early 1900s, the debate was not if scientific preparation should be included but how much science was needed and how it should be taught. The ideal was "a unity of systematic academic study, especially in the sciences, with hands-on experience to create a


physician who thinks critically, can solve problems, possesses a wide knowledge of underlying disease processes, and is skilled at applying what has been learned to real-life situations.\textsuperscript{9} As Bonner argues, medical education during this period was in part culturally defined -- different countries, influenced by different political, economic and social circumstances, altered the training of their physicians accordingly. German universities emphasized scientific theory and research, and the French retained the centrality of the clinic. British universities began to include more of the new sciences into their medical training programs but were slow to alter the powerful position of the hospital over the university. In North America, selected universities, led by prominent educators and supported by substantial private funds, forged a judicious balance between laboratory science and clinical experience.\textsuperscript{10} No university adopted the new medical curriculum more wholeheartedly than Toronto.

The University of Toronto came to boast one of the best medical programs in North America during this period. In 1908, the University of Toronto Faculty of Medicine raised its admission standards and extended its medical program from four to five years to incorporate extra study in both the basic medical sciences and clinical years.\textsuperscript{11} Students spent the first part of their program studying the basic sciences in university laboratories and finished their program with clinical instruction on the wards of the teaching hospital. Toronto students studied basic research theory as well as gained clinical experience, combining the science with the art of medicine. In

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\textsuperscript{11}University of Toronto Archives, A83-0036/001 University of Toronto. University Historian. “Professional Education 1906-32,” [unpaginated typescript].
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1910, Abraham Flexner praised Toronto for its superb facilities and the increasing cooperation between the university and hospital. University plans for a new medical building were underway, including research laboratories with the latest equipment. The newly rebuilt Toronto General Hospital adjacent to the University of Toronto was opened in 1913, an impressive modern hospital facility for patients and physicians. This movement to modernize the medical program brought medical education reform, structural change, and overall public enthusiasm for Toronto's confirmed stature as the province's elite university.

In 1914, Murray enrolled in medicine at the University of Toronto. He had worked hard to arrive at university, academically and financially. He had taken his studies very seriously in Stratford, and had successfully passed his matriculation examinations. He spent his summers labouring on the farm to help earn his tuition. In following his brother to Toronto, Murray would receive an excellent medical education. No doubt he anticipated this, impressed by the recent program reforms and by the prominence of his Toronto instructors. Professors Alexander McPhedran, J.M. MacCallum, J.J. Mackenzie, Velyien Henderson and other prominent doctors gave course lectures at the university; Professors C.L. Starr, F.N.G. Starr, H.A. Bruce and others shared their clinical experience by instructing students on the hospital wards. If not overawed by his teachers, then Murray may have been overwhelmed by the

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medical knowledge he was expected to learn, understand and apply. The field of medicine had grown rapidly since the turn of the century expanding from its traditional base of anatomy, pathology, and physiology to include chemistry, physics and biology. His classmates were predominantly white men, roughly his age, and most were from urban professional and middle class families.¹⁴ Only a small percentage of medical students came from rural backgrounds, at times making Murray self-conscious about his simple Oxford County upbringing.¹⁵

In 1914 Canada went to war. After only one year in medical school, Murray left university to enlist in the Twenty-Sixth Field Artillery Battery of the Canadian Armed Forces. Nearly one-third of his classmates also interrupted their medical studies and awarded leave for military service. In 1915, Murray left Canada and, for the next two years, fought in the trenches of Europe. Despite his mother's attempts to get Murray posted to hospital duty, Murray remained on the front line. His experience was horrific -- he was buried alive, trapped in enemy territory, and sustained a serious leg wound that almost required amputation. Casualties were high at the front, and at times Murray pitched in to treat the wounded. Most devastating, his younger brother Allan was killed in battle. Like other soldiers who fought in World War I, Murray later kept most of his war experiences to himself. He had considered himself unsuited for the nightmare overseas and faced a difficult readjustment period when he returned

¹⁴ In Murray’s graduating class of 131 students, there were no more than twelve women and no visible minorities. Less than one-fourth of the class came from rural backgrounds, and approximately 43 of his classmates were servicemen. “Class of 1921” [Composite graduating class photo] Faculty of Medicine, University of Toronto; “Register of Students, 1920-21,” Faculty of Medicine, University of Toronto Calendar, for the year 1921-22, University of Toronto Archives, Box P78-0023(02), pp.35-36.

¹⁵ University of Toronto Calendars, Years 1914 to 1921, University of Toronto Archives, Toronto. R.D. Gidney and W.P.J. Millar, “Medical Students at the University of Toronto, 1910-40: A Profile,” Canadian Bulletin of Medical History 13 (1996): 34, 42.
home. He refused to talk about the war with family. He also had little respect for, even some resentment against those men who chose not to fight, to continue their medical training, or worse, profited during the war.16

Upon return from Europe, Murray immediately resumed his medical studies. He was a serious student, earning top grades, and in 1919 was elected to the Alpha Omega Alpha Honour Fraternity, a fourth and fifth years Medical Honour Society based entirely upon scholarship. He joined the “Daffydil” Committee, which organized “Daffydil Night”, an annual evening of entertainment, usually satirical skits, put on by the graduating medical students. With his witty sense of humour, Murray would certainly have enjoyed the tongue-in-cheek event. He also dated during these years, notably Helen Tough, a student at the Toronto Conservatory of Music. She was a thin, attractive woman with bobbed, brown hair and large, dark eyes, and was seven years younger than Murray. Born a minister’s daughter near Hornby, Ontario, she was a proper, modest woman. In 1921, Murray graduated from medicine at the age of 27. With medical degree in hand, Murray left Toronto and returned to Stratford as assistant to Dr. Lorne Robertson.17

The next eighteen months served as an apprenticeship for Murray. It was nice to be home, familiar and comforting, assisting Robertson as he had done the previous summers. Murray was exposed to the practical side of rural medicine, which his training in Toronto did not provide. From Robertson, Murray learned a great deal about the doctor-patient relationship as well as creative medical solutions. "Dr.

16Gordon Murray, Medicine in the Making (Toronto: Ryerson Press, 1960), 13-15; Information also provided by the Murray family.

17University of Toronto Archives, Torontonensis 1914 to 1921, University of Toronto Calendars 1914 to 1921; Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.
Lorne", as his patients affectionately called him, was a middle-aged, genial doctor and surgeon who served the residents of Perth County. It was a practice Robertson inherited from his father. He saw patients in his Stratford office as well as made house calls. On occasion, he performed surgery on kitchen tables by lamplight.

Murray assisted Robertson in his operations and adopted his chief's attitude of "fix it up" — doing what was necessary, using ingenuity and often only rudimentary tools. Moreover, Murray observed the elder doctor's interaction with his patients. Known for his compassion and general therapeutic success, Robertson held the confidence of his patients. What Murray learned from Robertson he carried with him throughout his career.

After little more than a year in Stratford, Murray decided to leave Canada to pursue post-graduate surgical training. Robertson, who had gone overseas for post-graduate training many years earlier, had no intention of holding him back. Murray wanted to be more than a rural physician; he wanted to be a full-time surgeon. To be a surgeon was to be a ‘cutter’, to take an aggressive and interventionist approach to treating patients. Surgery demanded dexterity, precision and skill, and it appealed to Murray’s bold and eager nature. His medical undergraduate education however had

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20 Surgeons were different from physicians. According to William Nolen, "Surgery attracts a different sort of person than does medicine. The guy that goes into surgery is the fellow who doesn’t want to sit around waiting for results. He wants the quick cure of a scalpel, not the slow cure of a pill." Richard Selzer suggests that, "the surgeon, armed to the teeth, seeks to overwhelm and control the body; the medical man strives with pills and potions to cooperate with that body." Taken from William A. Nolen, The Making of a Surgeon (New York: Random House, 1968), p.219; Richard Selzer, Letters to a Young Doctor, 2nd Edition, (New York: Harcourt Brace and Company, 1996), p.40.
provided him with scant surgical training. Murray needed to learn the craft of the
surgeon – the technical proficiency, good judgment, and sound knowledge of
disorders and disease.21

The next several years were thus a self-imposed program of study under
distinguished physicians and surgeons in an attempt to learn these surgical skills.
Apprenticeships under the masters were still endorsed as the best way to train future
surgeons, and hospitals offered resident positions for this purpose. Post-graduate
sojourns abroad or to the United States were expected of those men hoping to practice
and instruct in the larger, prestigious teaching hospitals. At this time, the profession
had not formalized the length or breadth of this post-graduate surgical training. Nor
had the profession identified the centres qualified to offer this training. Individuals
like Murray were left to make these choices themselves, to decide which hospitals and
universities would provide them with the best training to learn and keep pace with the
many surgical procedures being performed at that time.

By the 1920s, surgery had advanced beyond simple wound management of the
nineteenth century to daring abdominal and chest procedures. Historically, surgeons
were challenged by three main problems — pain, infection and bleeding — and their
reputations were based on skill, speed and operative success.22 Most mid-nineteenth

21 As quoted from Joan Cassell, a good surgeon needs to know “when to operate, when to stop
cutting, and how to take care of what you did.” Joan Cassell, Expected Miracles: Surgeons at Work

22 The identification of pain, infection and bleeding as the major difficulties faced by surgeons taken
from Robert G. Richardson, Surgery: Old and New Frontiers (New York: Charles Scribner’s Sons,
1968), p.5. This is supported by Owen and Sarah Wangensteen who refer to the three As of surgery —
anesthesia, antisepsis and antibiotics — as the three key innovations responsible for the advancement of
century surgeons were poorly educated but, as craftsmen, they often possessed great manual ability and technical skill. When combined with ingenuity and boldness, they were even successful at times. Overall, however, major surgery in the mid-nineteenth century was risky, painful and often resulted in death. Patients feared surgery, and only consulted surgeons as a measure of last resort. As a result, the surgeon’s practice remained limited to mostly external wound treatment, bandaging and bleeding.

The modern era of surgery began with the success of anaesthesia in the 1840s and the adoption of antisepic, then aseptic, techniques in the 1870s and 1880s respectively. Anaesthesia gave patients relief from their pain, once considered inseparable from surgery, and also awarded the surgeon more time. The practice of antisepsis and later asepsis provided a sterile operating field, lowering the risk of infection and making surgery much safer. The new sciences of bacteriology, physiology and microbiology gave legitimacy to antisepsis and asepsis, and allowed surgery to ground itself in science.

Basic surgical procedures were improved and new operations introduced. Surgeons were now performing a range of procedures from gastrectomies to appendectomies in addition to older operations such as colostomies and ovariotomies. Abdominal surgery became the new challenging field for modern surgeons. They cut into their patients to relieve intestinal obstructions and to remove gastric cancers, inflamed ulcers, painful bladder stones, and diseased appendices. As more and more successful abdominal operations were performed and reported, patients gained greater confidence in accepting surgical treatments. Surgeons then shifted their attention to

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chest surgery, particularly procedures to treat tuberculosis sufferers. Early twentieth century therapy for tuberculosis consisted of rest, sunshine and a dry climate; physicians could offer little more. For those patients with pulmonary tuberculosis, surgeons introduced several new procedures. They began performing thoracoplasties (removal of portions of the ribs to collapse diseased areas of the lung), phrenicotomies (cutting of the phrenic nerve to produce immobilization of a lung by inducing paralysis of one side), and pulmonary resections (removal of portions of the lung) with promising results.\(^4\)

It was an exciting time to be a surgeon. In 1923 Murray traveled to the Mayo Clinic in Minnesota. He spent several months there as a junior assistant pathologist, studying the pathology of disease and current therapy. The reputation of the clinic in surgery and innovative treatments dated back to the 1880s when William and Charles Mayo began specializing in surgery. By the 1920s, the Mayo Clinic had grown in its number of surgeons and was a leader in research and surgery. It had developed into a centre of graduate medical education and attracted many doctors to come to observe their new surgical techniques.\(^5\)

The Mayo Clinic and its reputation for embracing new surgical techniques appealed to Murray, but he had a stronger urge to go overseas to train in the older,

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\(^4\)A thoracoplasty is the surgical removal of parts of the ribs to allow the chest wall to fall in and collapse the affected lung. A phrenicotomy is the surgical removal of a section of the phrenic nerve which paralyses the diaphragm, a procedure used as a means of resting a lung infected with tuberculosis. In the early 1920s, English surgeon Tudor Edwards pioneered pulmonary resections – the surgical removal of diseased portions of the lungs. These definitions from the Oxford Medical Dictionary, 4\(^{th}\) edition (Oxford University Press, 1994).

traditional medical centres. In the fall of 1923, he sailed for London, England with letters of introduction from his Canadian instructors at the University of Toronto. The Professor of Surgery, C.L. Starr, and the Professor of Medicine, Duncan Graham, had written letters on his behalf, as well as Professors B.P. Watson, W.E. Gallie, F.N.G. Starr and Dr. Lorne Robertson.

In London, Murray secured the position of Resident Medical Officer at the West End Hospital and during the next three years shifted into a variety of postings at several of the city’s notable hospitals. In addition to the West End Hospital, Murray also worked at St. John’s Clinic in Leicester Square in the Department of Dermatology. He then spent six months as House Surgeon at the Hampstead General Hospital and later House Surgeon and Clinical Assistant at All Saints’ Hospital. Following these posts, he became Temporary Registrar at the London Hospital, Demonstrator of Anatomy at St. Mary’s Hospital, and Demonstrator of Anatomy and Physics at the University of London. In 1926, he became House Surgeon at the National Orthopedic Hospital and St. Bartholomew’s Hospital. These appointments allowed Murray to work with many skilled medical practitioners, and to complete rotations in general medicine, dermatology, genito-urology and surgery. He gained experience treating a wide range of diseases and conditions – such as disorders of the kidneys, bladder and genital organs, chest problems, skin afflictions, and inflamed


tonsils and adenoids — and he participated in a variety of procedures. In total, Murray performed over 200 major operations during his various rotations.\(^{30}\)

Murray greatly admired the medical men who surrounded him — general physicians Laming Evans and Harry Campbell, prominent bone surgeons Milne, Rowntree and Lambrinudi, world-renowned general surgeons Bathe-Rawling and Sir Charles Ballance. But it was Professor Ernest Frazer, the anatomist, who had the greatest influence on Murray during his London training. Frazer was a genial, soft-spoken Scotsman who turned the "dry bones of anatomy" into captivating "living structures" for Murray.\(^{31}\) He lectured on the importance of anatomy for every medical practitioner, particularly the surgeon. He drilled anatomy into Murray, and in turn Murray realized that he became a better surgeon because of it. His respect for anatomy and his determination to master it came directly from Frazer. Murray never forgot that lesson, and in later years would repeat this pattern with his own students.

After nearly three years of hospital experience and additional university study, Murray felt prepared to sit the demanding Fellowship examination of the Royal College of Surgeons. He passed on his first attempt in 1926.\(^{32}\) This accomplishment landed him the position of Demonstrator of Anatomy at the London Hospital and University of London. He was now also House Surgeon at St. Bartholomew's Hospital and his seniors praised his surgical skills. Murray was a good surgical

\(^{30}\)NAC, Murray Papers, MG 30, B 110, Vol. 28, File 5.


assistant with excellent operative technique and judgment. He was congenial to work with as well as popular and tactful with patients. One surgeon commented favourably on his delightful sense of humour.\(^{33}\)

Murray enjoyed these years in London. His British training was influencing his formation as a surgeon, clinical investigator and teacher. Murray believed that the method and quality of British medical training was superior to North American schooling. The British schools provided a broader classical education and better foundation for future investigation and practice. According to Murray, "the teaching was logical, analytical, based on current observations and those recorded in literature by the great men of past generations, and all founded on a profound and extensive knowledge of the basic sciences involved, including anatomy, physiology, chemistry, pathology and histology."\(^{34}\) Anatomy was particularly important to the surgeon; Professor Frazer had convinced Murray of this. He saw how American surgeons, unsure of their anatomy, wasted time and lacked confidence. In contrast, Murray's mastery of anatomy and his extensive operating experience in London hospitals made him a skilful and confident surgeon.

In England, Murray also dabbled in clinical investigation. On occasion, he was asked to participate in the administration of experimental treatments to his ward patients. Between the wars, there were many patients suffering from illnesses or disorders for which there were no adequate cures or treatments. His British mentors encouraged Murray to experiment, both in the laboratory and on the ward. No established research procedures or protocols existed in British hospitals at this time.

\(^{33}\)NAC, Murray Papers, MG 30, B 110, Vol. 28, File 5.

Experimental treatments that seemed promising in the laboratory were administered to patients with hopes for encouraging results. There was more emphasis on clinical results than scientific theory and process in Britain because the hospital, rather than the university, stood as the more powerful and influential institution. Worldwide, clinical medical investigation, in terms of methodology and protocol, was in its infancy and only after World War II were medical research standards introduced. Murray was not trained as a researcher, but his exposure to clinical investigation in British hospitals did infuse a life-long interest in research and emphasis on clinical results over scientific theory and methodology.

Murray's three-year sojourn in England left a cultural imprint on his character. An older cousin, Gladstone Murray, and his wife took Gordon under their wing to introduce him to British society. Gladstone had come to Oxford as a Rhodes Scholar in 1913 and married an English girl in 1923. He worked for the BBC, holding a variety of public relations positions, at the beginning of a long career in broadcasting. Gladstone was a big, burly man who was outgoing, sociable and well connected in London society. He introduced the less gregarious Gordon to the British forms of play and recreation -- cricket, tennis, and golf -- as well as schooled him in the customs and manners of the Englishman. Gordon wanted to learn these British customs of social etiquette and decorum. His “Canadian manner of speaking” embarrassed him, and his rural, colonial upbringing made him self-conscious in large gatherings. He came to appreciate certain aspects of British society --- politeness,

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reserve and formality. After three years in London, Gordon had become an anglophile.  

In 1926 Murray decided to leave London. Despite his high regard for British medicine and the potential employment opportunities of staying in London, it was time to go home. Murray had accomplished what he had set out to do and he missed Helen, who was waiting patiently for him in Toronto. She had faithfully corresponded with Gordon throughout their long-distance courtship. After three years away, Murray began making enquiries about surgical positions back home. He soon received a letter from Professor Starr offering him the post of Resident Surgeon, a one-year residency, at the Toronto General Hospital. The Professor of Surgery also hinted that a permanent staff position would be forthcoming. With this offer in hand, Murray returned home in the fall of 1926.

But he did not remain in Toronto long. Murray had almost eight months before his surgical residency began at Toronto General Hospital, so he traveled to New York where he became House Surgeon at the New York Hospital as well as the Hospital for Ruptured and Crippled Children. He arrived for his new appointment at the American hospital in London attire — bowler hat, black overcoat, spats, gloves and cane — much to the amusement of hospital staff. If Murray was posturing or trying


37 Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.

38 National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, File 5, "Correspondence Gordon Murray to C.L. Starr, 1 October 1926", "Correspondence F.N.G. Starr to Lorne Robertson, 19 October 1926".

to impress someone, he succeeded only in embarrassing himself. New York was not London and he soon sorted out the differences. There was neither the academic thoroughness nor the rigid and formal British structure with which he had become accustomed. What did he have to learn here in the New World when he had spent three full years in the Old?

Murray did learn many things, notably from Dr. Eugene Pool, Chief of Surgery at New York Hospital. First, Pool had devised a system of sign language that he used in the operating room to reduce speech and thus the spreading of germs into the atmosphere. Murray later adopted this method of signing in communicating with his operating room nurses. Secondly, Murray admired Pool's surgical delicacy while operating. Pool handled all body tissues with gentle respect and this prevented further inflammation or haemorrhages. Murray learned to appreciate the remarkable capabilities of the body to react and heal itself. Thirdly, Pool had done a great deal of clinical investigation and had published his work. Murray felt this made him an inspiring teacher as well as a gifted surgeon. American enthusiasm for experimentation and innovation delighted Murray.

In July 1927, Murray arrived at the Toronto General Hospital as Surgical Resident. After training with prominent medical men in North America and England, Murray had returned to his alma mater. He was happy to be home and at one of Canada's elite medical centres. What he may or may not have realized was that the Toronto hospital and university had changed while he was abroad.

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40Personal interviews by author.

After the First World War, the Toronto medical programme underwent further reform. In 1919, University of Toronto educators outlined a ten-year plan for changes in all of its professional programmes. In the Faculty of Medicine, these changes were gradual and cumulative and resulted in the steady increase of research activity in its departments through the hiring of key medical men and the construction of new buildings.42 Most significantly, Duncan Graham was appointed the university’s first full-time Professor of Medicine in 1919. Charged with reorganizing the Department of Medicine and its teaching curriculum, Graham discharged staff, appointed new professors, added courses, and increased student clinical hours.43 The changes in Toronto reflected the contemporary ideals of medical reform, articulated in Flexner’s 1910 report and modeled after the Johns Hopkins medical programme and its ideal of a full-time medical faculty. Graham’s success in reorganizing the Department of Medicine soon led to the reorganization of the Department of Surgery.

In 1921, a Chair of Surgery was established, funded by money from the Rockefeller Foundation. C.L. Starr accepted the full-time position of Professor of Surgery and Surgeon-in-Chief, the first such surgical appointment in Canada. Starr, like Graham two years before him, was committed to reorganizing the Department of Surgery and to setting a new direction in the teaching of surgery. He followed suit by

42 University of Toronto Archives, A83-0036/001 University of Toronto. University Historian. “Professional Education 1906-32,” [unpaginated typescript].

43 The Eaton Chair of Medicine, the first full-time clinical position at the University of Toronto, was financed by the generous gift of $25,000 donated by Sir John and Lady Eaton. Graham earned a salary of $10,000, became Physician-In-Chief at the Toronto General Hospital, and was mandated to reorganize undergraduate medical teaching. He had a small private practice but his primary concern now was to mould an efficient Department of Medicine, emphasizing excellence in educating and training future physicians. Thomas Fisher Library, University of Toronto, Academy of Medicine Collection, Biographical Files, 0597 Duncan Graham; W.G. Cosbie, The Toronto General Hospital 1819-1965: A Chronicle (Toronto: Macmillan of Canada, 1975), 167-169; Robert Kerr and Douglas Waugh, Duncan Graham: Medical Reformer and Educator (Toronto: Dundurn Press, 1989).
cleaning house, revoking all university positions and re-appointing only a handful of the most distinguished surgeons. He reorganized all surgical services to fall under his authority. In keeping with the new attitudes of balancing clinical experience with greater scientific understanding, Starr arranged for research equipment, animal accommodation, animal operating rooms and other amenities necessary for clinical investigation. Both the Professor of Medicine and the Professor of Surgery, due to the nature of their position and power within the university and hospital, were able to wield tremendous personal influence over the changing medical curriculum and structure.\(^4^4\)

Murray's Toronto education and overseas training had put him in good standing with Starr. His British and American sojourns served as excellent surgical apprenticeships as well as provided him with experience in teaching and clinical investigation, which corresponded well with the faculty's growing orientation towards medical science.\(^4^5\) Furthermore, Starr had personally chosen Murray over other

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\(^{4^4}\)Professor of Medicine Duncan Graham held his post for 28 years, training and appointing those men who shared his views on medical education and training. Starr died unexpectedly in 1928. In his seven years as Professor of Surgery, Starr reorganized the department and made some substantial curriculum changes. His successor was W.E. Galie who remained in this position for the next 18 years. Galie continued his predecessor's pursuit of excellence in re-building the department. He made new appointments of outstanding medical men to the department as well as inaugurated a new post-graduate training course in Surgery at the University of Toronto. In 1931, the "Galie Course", as it became known, accepted senior interns for a period of three years. They received training in pathology, general surgery and two of the three specialties, urology, neurosurgery or pediatric surgery. The course expanded to include newly emerging specialties in surgery and, after 1939, was considered suitable preparation for the fellowship examination of the Royal College of Surgeons of Canada. (In 1939, the Royal College of Surgeons (Canada) was established, and medical men passing their examinations could obtain fellowships in this country. The Royal College in England still held greater status within medical circles for many years thereafter. After WWII, this distinction between the two Royal Colleges for Canadian medical practitioners had dissipated considerably.) Thomas Fisher Library, University of Toronto, Academy of Medicine Collection, Biographical Files, 1620 C.L. Starr; W.G. Cosbie, The Toronto General Hospital 1819-1965: A Chronicle (Toronto: Macmillan of Canada, 1975), 169-175; Herbert A. Bruce, Varied Operations (Toronto: Longmans, Greens and Company, 1958), 127-132. William Edward Galie: Surgeon, Seeker, Teacher, Friend (Toronto, 1978), 1-17.

\(^{4^5}\)During the interwar period, Duncan Graham and Clarence Starr attempted to replace the local private practitioner dabbling in "common-sense experimentation" (as exemplified by Frederick
qualified applicants for the surgical position, so a friendly reception was assured. Murray found the Professor of Surgery pleasant and enthusiastic, and he soon found a mentor in the older surgeon. Murray was impressed with Starr’s postgraduate training abroad, his ten-year tenure as Surgeon-in-Chief at the Hospital for Sick Children in Toronto, and his military service as Senior Surgeon in a Canadian hospital in England during World War I. Moreover, he was a skilled surgeon and inspiring teacher, admired and respected by colleagues and students. His reputation was outstanding.

Likewise, Starr was happy with his new Surgical Resident because he was a skilful surgeon who fit in with the new philosophy and direction of the department. Murray had acquired the technical skills and dexterity required of a surgeon, and operated with decisiveness, control, and confidence. By this point, he had also developed his own style of operating. He was bold and aggressive in his surgical approach, but never proceeded carelessly or unnecessarily. He was methodical and exact, but swift and purposeful with his scalpel. His mastery of anatomy, ingrained in him by Frazer in London, was one of his strongest attributes as a surgeon, and allowed him to manoeuvre seemingly effortlessly within the cavities of the body.

Murray was precise, quick and clean; nurses remarked on the conciseness and

Banting) with professional research medical men. The practice of medicine “demanded scientific rigour, not amateur enthusiasm.” Thus Graham and Starr began appointing younger medical men who alongside their private practices were “oriented towards serious scientific research” and clinical instruction, and they removed older, ‘scientifically-untrained’ physicians from their departments. A.B. McKillop, Matters of Mind: The University in Ontario 1791-1951 (Toronto: University of Toronto Press, 1994): 349-352.


47Not all surgeons are alike. Richard Selzer comments that “there are those who are slow and methodical, obsessive beyond all reason … then there are the swashbucklers who crash through the underbrush waving a machete … then there are the rest of us who are neither too timid nor too brash. We are just right.” Richard Selzer, Letters to a Young Doctor, 2nd edition, (New York: Harcourt Brace and Company, 1996), p.47-48.
minimal blood loss of his operations. Students loved to attend his operations, and to observe him in action.48

Murray appeared to be well suited for the competitive, and at this time exclusively male, specialty of surgery. He was aggressive and decisive in the operating room, and clearly in charge. He was demanding and brisk with colleagues and hospital staff, and confident, if not arrogant, with patients. This was the type of surgeon that patients and their families wanted. Who would choose a surgeon lacking in decisiveness and confidence to perform a complicated operation? In most ways, his style reflected his personality – forthright, demanding, precise and pragmatic.

Undoubtedly, he also adopted aspects of the surgical styles he had trained under in London and New York. Regardless, Murray’s operative success and dedication to treating public ward cases pleased both patients and hospital administrators.

In 1928, as Murray had hoped, Starr offered him a staff appointment in the Department of Surgery. Murray was happy to accept.49 It allowed him to continue working with Starr. He assumed more of Starr’s teaching duties, as the Professor of Surgery’s health was slowly declining. Starr also directed more surgical cases to the younger surgeon. The Toronto staff position offered Murray all that he wanted -- excellence in surgery, hospital privileges, teaching responsibilities, clinical laboratories, and prestige.

48 Personal interviews by author with nurses and former students who worked with Murray.

49 Gordon Murray, Medicine in the Making (Toronto: Ryerson Press, 1960), 55-56, 64.
A few months later, he married Helen in a small, quiet ceremony at a friend’s home in Toronto. After a brief honeymoon in Northern Ontario, the young couple moved to a lower duplex at 29 Classic Avenue. Their friends Norah and Roland Michener, he a future Governor-General of Canada, lived in the upper duplex. Murray was 34 years old, and he was no longer a surgeon-in-training. He was now a staff surgeon at a leading hospital, and his future surgical practice appeared to be secured.

Murray’s success in becoming a good surgeon and receiving a prestigious hospital appointment was due, in part, to his ability to adapt to the particular demands of the specialty, but more importantly, to his elite education and surgical training in Canada, Britain and the United States. Murray was not unique in travelling overseas for his post-graduate education, but his seven-year surgical training was unusual in its length and its range of assignments. He worked as a junior practitioner in rural Canada, as medical officer and house surgeon in notable British and American hospitals, and as surgical resident at the Toronto General Hospital. Murray acquired the surgical knowledge and experience to practise skillfully and expertly. Patients felt secure in his capable hands, and for good reasons. Murray was a talented surgeon -- bold, decisive and confident -- how he loved to operate! Moreover, he had landed an appointment at one of Canada’s elite medical institutions – an impressive start to

50 "Murray Tough", *Toronto Globe* 20 August 1928; University of Toronto Archives, D.W.G. Murray file, A73-0026/343(68).

anyone’s medical career. The farm boy from Oxford County now took his place among Toronto’s most prominent surgeons, and looked forward to a promising surgical career.
Chapter 2

Surgical Breakthrough: The Clinical Application of Heparin

In 1928, after completing his surgical residency, Gordon Murray received his expected appointment to the Toronto General Hospital surgical staff. He was assigned to Norman Shenstone’s general surgical service on Ward C. Another junior surgeon, Robert Janes, was also on the service, having been appointed six years earlier. All three general surgeons performed a range of chest, abdominal and orthopedic procedures. Each had his assigned operating time, usually one or two mornings a week, in the O.R. on Ward C, and the nurses soon learned their preferred surgical instruments and routine. Shenstone and Janes developed a collaborative working relationship on thoracic cases, most notably devising the Shenstone-Janes tourniquet. ¹ Murray, however, worked alone and never shared a close camaraderie with either of these surgeons.

¹The Shenstone-Janes tourniquet, devised in 1929, was placed around the root of the lung, pinching the blood vessels and the bronchus. The loop of the tourniquet was about the thickness of a shoelace, and the operator could manipulate the loop into position and tighten it. The surgeon could remove the lung with relatively bloodless field, after which he could then tie off the ends of the arteries and veins (with silk) as well as oversew the bronchus (with catgut). W.G. Cosbie, The Toronto General Hospital 1819-1965: A Chronicle (Toronto: Macmillan of Canada, 1975), 195-6; University of Toronto Fisher 34
In addition to his ward duties, Murray was cross-appointed to the University of Toronto as a clinical instructor in the Department of Surgery. He enjoyed lecturing and conducting hospital rounds with medical students, and he soon became known as a demanding, yet stimulating teacher. This position also entitled him access to university research laboratories as well as a small office in the Medical Arts building across campus from the hospital. In the afternoons, he kept private patient appointments in this office.

On Christmas Day 1928, the Professor of Surgery and Surgeon-in-Chief, C.L. Starr, died unexpectedly. He had been battling ill health for some time, but his death had been sudden nonetheless. For Murray, it meant the loss of his mentor, and he contemplated leaving Toronto. Soon thereafter, W.E. Gallie, a skillful surgeon with a comparable reputation, was appointed Starr's successor. Murray decided to stay because he liked Gallie's commitment to clinical research and his record of surgical accomplishments, notably his work on "living sutures" or transplantation of fascia (a fibrous membrane covering, supporting, and separating muscles) and tendons (fibrous connective tissue that attaches muscles to bones and other parts). Lab space was designated for the Department of Surgery on the fifth floor of the new Banting Institute, then under construction, directly across from the Toronto General Hospital on College Street. Gallie strongly supported research and encouraged his staff and

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students to use the facilities. This new medical laboratory and Gallie's attitude toward departmental research pleased Murray. In his view, Toronto still offered him a promising career in both surgery and clinical investigation.

Murray took advantage of the university laboratories and Gallie's research support to explore new surgical procedures. His first investigations focused on the knee joint and tendons in the leg, and became the subject of his first publication. He then experimented with the pancreas and found a decrease in insulin content in dogs suffering from acute infection. He also reported on improvements in treatments for patients suffering from carcinoma of the oesophagus and the rectum. But the most innovative contribution made by Murray during this early period was his bone-graft procedure for a fractured, non-united scaphoid (one of the eight carpal bones of the wrist). A non-united scaphoid may lead to avascular necrosis, inflammation, severe pain and ultimately disability. Murray drilled a hole in the wrist cutting out a large portion of the fracture surface. In this hole he placed a piece of bone from the tibia (shin) which acted as a bone graft and splint. Within weeks, union of the bone occurred and patients regained full mobility and range of motion. These results were


published in more prominent journals than Murray’s previous reports, and soon other surgeons were performing the procedure with the same success.

Murray enjoyed tinkering in the lab, experimenting with new surgical techniques. He was focused on clinical application rather than being research-oriented, and he was delighted when he was able to offer improved treatment to his patients.\(^8\) But these early investigations, regardless of how they may have helped particular individuals at the time, made little impact on medical science. They were small improvements on diverse conditions, and did not cause a great stir among research scientists or practitioners.

Later that year, Murray began work on his first long-term research project, experimenting with heparin, an anticoagulant. He spent the next several years investigating its use in treating thrombosis and embolism, and then its application in repairing arteries and veins for improved circulation. His laboratory and clinical work demanded surgical skill, repetition and controls. Murray was thorough and precise in methodology and technique, and it resulted in the best research of his career. He developed the clinical application of heparin and made a key breakthrough in surgery. It was a substantial contribution to medicine, and marked Murray’s debut into the public and professional spotlight.

The question of blood coagulation had interested many nineteenth and twentieth century medical men. "Why does blood remain fluid in the vessels of the living body and yet clot when shed?" Blood clotting (or coagulation) is the process whereby blood is converted from a liquid to a solid state, and is an essential mechanism for haemostasis (the arrest of bleeding). It is a series of complex steps that involve one of the plasma proteins, fibrinogen, changing into fibrin, an insoluble protein. Threads of fibrin enmesh the blood cells to form a semi-solid mass, during which fluid is squeezed out, and a firm clot is left. Clotting outside of the body is good, whereas clotting inside the body generally is not. Surgeons were concerned with internal clotting, and it developed in the following way. Blood clots form inside the body when vessels (veins or arteries) have been injured or traumatized. Blood clots consist of predominantly red blood cells, and when bleeding occurs outside the vessel, a clot is formed which stops the bleeding. A clot and thrombus can often occur together, but there is a difference in their composition. A thrombus consists of both white and red cells. When the lining of a vessel is injured, the body reacts by sending platelet cells or tiny white cells that stick together and adhere to the injured area in order to fill any gaps. Other cells of the blood are also attracted to the area and they are held together by strands of fibrin. This is a thrombus. Clots and thrombus are dangerous because they can enlarge to occlude or block the flow of blood in a

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vessel or they may become dislodged and move through the vessels that lead to the lung resulting in pulmonary embolism.\textsuperscript{10}

Medical men of the nineteenth century had observed that the blood's clotting ability is diminished when mixed with hirudin (a secretion of the buccal glands) from leeches. Many of them were convinced that humans (as well as leeches) possess a "natural anticoagulant" but had no idea of its chemical properties or how to isolate it.\textsuperscript{11} By the 1930s, surgeons had found ways to manage pain and infection but still grappled with the problems of patient bleeding. The physiology of the cardiovascular system and the chemical components of the blood had yet to be fully described and thus understood.

The substance that they searched for was heparin. Small amounts of heparin are normally present in the blood. It is a polysaccharide and is produced by many different cells in the body, particularly in the tissue surrounding the lungs and liver. It has both an antithrombotic (to prevent formation of thrombus) and an anticoagulant (to prevent formation of clot) effect.\textsuperscript{12} In many ways, heparin continues to remain a "mystery" even today, according to W.G. Bigelow. Medical scientists are still unable to identify all of its properties and thus are unable to synthesize the drug.\textsuperscript{13}


\textsuperscript{11}Ronald J. Baird, "Give us the tools ... The story of heparin -- as told by sketches from the lives of William Howell, Jay McLean, Charles Best and Gordon Murray, Journal of Vascular Surgery Vol 11, No 1 (January 1990), 5.


\textsuperscript{13}University of Toronto Fisher Library, Academy of Medicine Collection, Biographical files, 0118 W.G. Bigelow, "An Interview with Dr. W.G. Bigelow," no date.
Heparin was discovered in 1916 by Jay McLean while he was conducting research on the action of coagulants, specifically cephalin, at Johns Hopkins University, under Physiology Professor William Henry Howell. Continuing McLean’s research, Howell investigated blood coagulation more closely, and described the properties of the newly found anticoagulant, naming it “heparin”. He purified and improved the potency of this substance. By 1922, Hyson, Westcott and Dunning in Baltimore began to produce heparin commercially according to Howell’s direction. For the next several years, heparin was made available to physiologists for experimental work. But it was crude, toxic and expensive. Soon, frustrated researchers elsewhere, dissatisfied with the commercial product, began to study heparin with hopes of developing a better form of the substance -- less toxic, stronger in potency, and less expensive -- for use in medical research.14

One such researcher was Canadian Charles Best. An effective anticoagulant, like heparin, would greatly assist his histamine research experiments.15 In 1929, Best set up a research team at the University of Toronto to study the chemistry and physiology of heparin, and turned to chemists David Scott and Arthur Charles at Connaught Laboratories. Best asked them to "(1) find a more readily available (and inexpensive) source of heparin, and (2) purify the extract so that it was potent and


suitable for safe use in animals and humans." Early in their research, Scott and Charles found that beef liver and beef lung were better sources of heparin than Howell's dog liver. It was more readily available from local abattoirs, and, through autolysis, greater yields of heparin could be produced. This met the research team's first objective. Now they sought to purify the substance so that it could safely be injected in humans, and this would take substantially more time. After several years of work, Scott and Charles successfully converted heparin from a crystalline barium salt into sodium salt -- a potent, non-toxic form. Samples of the Toronto heparin were sent to interested researchers throughout the world with which to experiment, but a pure commercial form of heparin was not available until 1937.

Initially, Best's research team consisted of only Charles and Scott since the chemical work on the purification of heparin needed to precede physiologic and clinical studies. By 1932, Charles and Scott had found a way to produce heparin in larger quantities and relatively inexpensively, but they were still working on its purification. Best was impatient, perhaps thinking that Charles and Scott were not far off from purifying their preparations, and he decided to begin testing the value of

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19University of Toronto Fisher Library, C.H. Best Papers, MS Coll 241, Box 70 Heparin Correspondence 1930-72, File 70-8 "December 1935", Correspondence Gordon Murray to C.H. Best, 20 December 1935.
heparin clinically. He expanded his research team beyond the Physiology Department and Connaught Labs to include the Department of Surgery, specifically wanting to engage an investigative surgeon to explore the clinical application of heparin. Best approached Gallie and asked him to nominate someone from his department. Gallie endorsed such interdepartmental cooperation and joint research ventures, and suggested Murray because of his skill as a surgeon, his interest in clinical research, and his work with embolism in peripheral arteries at that time. Murray seized the opportunity to be part of Best’s research team. Shortly thereafter, he began experimenting with heparin at the Banting Institute.

Murray had limited time in the laboratory; he was a full-time surgeon but only a part-time clinical investigator. His experiments on heparin had to be fitted in and around his surgical operations and his teaching responsibilities. Best sent over several research assistants, senior students who received some pay, to help Murray with his experiments. Winnifred Chute, L.B. Jaques, T.S. Perrett, and Walter Cowan all worked with Murray at various times. Each contributed in varying amounts to the project and Murray’s research progress. Chute carried out the initial heparin experiments but was soon replaced by Jaques. He worked full time under Murray for one year during a highly productive period, and was succeeded by Perrett who confirmed these results during his two-year research position. Cowan assisted

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throughout the project as the lab technician. Jaques and Perrett played the greatest roles, and were co-authors of Murray's and Best's first heparin publications.

Murray did all the necessary experimental surgery, between 7:45 and 8:45 in the morning, and then left for the hospital to start his clinical operations.\(^\text{23}\) His research assistants administered heparin to the dogs, took blood samples, monitored clotting times and recorded their observations. Murray held fortnightly meetings with his research assistants and over a cup of tea created an informal and relaxing setting in which to discuss their heparin research.\(^\text{24}\) Jaques remembers his relationship with Murray was "that of eager student and helpful, sympathetic teacher".\(^\text{25}\) Murray was supportive of and sensitive to his juniors. Their opinions and views were received and considered with those of other team members. He was an excellent teacher and his enthusiasm and excitement was contagious. For the most part, Murray worked well with his junior assistants and they liked him. But it was clear that Murray was in charge. Every morning he gave explicit directions to each of them as to that day's research tasks.\(^\text{26}\)


\(^\text{24}\)Connaught Laboratories Archives, Heparin Papers, Correspondence L.B. Jaques to J.F.W. Ferguson, 20 February 1986.

\(^\text{25}\)Connaught Laboratories Archives, Heparin Papers, Correspondence L.B. Jaques to J.F.W. Ferguson, 20 February 1986.

\(^\text{26}\)Jaques carefully recorded the heparin data in his lab notebooks and based his master's thesis on that work. Upon completion of his master's degree, Jaques was allotted lab space in the Medical Building. Without pay, he continued his heparin investigations and began a lengthy career studying blood coagulation. Jaques published over 450 articles on the subject. See L.B. Jaques, "Reminiscences on Completing Twenty-five Years of Research in the Blood Coagulation Field," University of Saskatchewan Medical Journal Volume 4 (1960): 5.
The objectives of Murray's first experiments were to examine the effect of heparin in animals and to determine the dosage necessary to get the required effect. It was hoped that what Murray learned from dogs could then be applied to patients to prevent or cure thrombosis and embolism. Once the proper equipment and dogs were arranged, he set to work. His first challenge was to find a method that would consistently produce clotting and thus provide the control for his experiments. It proved to be quite difficult to produce thrombosis in healthy vessels. Blood clots in two to three minutes once removed from the body. Murray decided to place clamps on the dog's vein hoping to cause trauma to the vessel and thus induce clotting. He waited fifteen minutes and then examined the vein. It did not work; there was no clot. He repeated the exercise, waiting thirty and then forty-five minutes before removing the vein segment. Still no clot had formed. A greater mechanical injury to the vein was necessary so Murray crushed the radial vein of dogs, causing substantial damage to the vessel, but it did not consistently produce clotting. He resorted to inserting a thread up the vein and then crushed the vessel with artery forceps. The thread was then removed, stripping the lumen, and the wound closed by silk sutures. The vein was later examined and in all cases, occlusion had occurred. This procedure was Murray's mechanical means to produce injury and thus clotting, and it became his experimental model. Murray then spent the next several years experimenting with

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27 As early as 1931, pathologist Thomas Belt told his Toronto colleagues that pulmonary embolism was a much greater cause of death than previously thought. Pulmonary embolism was evident in 14 per cent (not one per cent) of routine autopsies at Toronto General Hospital. T.H. Belt, "Thrombosis and Pulmonary Embolism," \textit{American Journal of Pathology} Vol 10 (1934): 129-144; W.G. Bigelow, \textit{Mysterious Heparin: The Key to Open Heart Surgery} (Toronto: McGraw-Hill Ryerson, 1990): 62.

the method, dosage and length of time of heparinization and assessing its effectiveness.

Murray conducted five series of experiments on dogs, where he sought to establish the occurrence of clotting and thrombosis after vessel damage and then test the activity of heparin against this injury. In his first series of experiments, Murray injected the area with heparin and then proceeded to crush and damage the vein. The vein was immediately removed before the heparin had worn off and examined. Ten of thirteen veins were clear of thrombosis. In the next series of experiments, Murray injected the animals with a single injection of heparin and crushed the veins as before, but did not remove the veins for several hours. Of the thirteen veins in this experiment, twelve were completely filled with thrombus and clot while one was only partially occluded. In the following series of experiments, Murray repeated his previous techniques of a single injection of heparin and then crushed the veins. Then an infusion of heparin in saline solution was started immediately and maintained for up to 72 hours. Only three of the nine veins were occluded by thrombi while the remaining six remained clear. For Murray's fourth series of experiments, he damaged the vein on one side of the animal and administered no heparin. If this vein became obstructed, the corresponding vein on the other side of the animal was then damaged after an injection of heparin was given. In nine of the ten cases, the heparinized vein remained open and free of thrombus. Murray then repeated this last series of experiments, leaving the damaged control veins (with no heparin) in the body for seven days before heparinizing the corresponding test veins. These veins (with heparin) were examined after six days. Eleven of the fourteen veins in this experiment remained patent (open). Murray was elated with these results. It had been
a tedious process, but he felt he now had irrefutable data on which to present his conclusions. His experiments clearly demonstrated that injections of heparin prevented clot and thrombi formation in the damaged vessels of dogs, during and after trauma.29

Murray also experimented with varying methods of administering heparin to these dogs. Local heparinization was used in his first experiments, where a small dose of heparin was injected directly into the damaged area. He then explored regional heparinization where blood flowing through the limb is affected (and becomes systemic if a large dose is administered), and general heparinization where heparin is injected intravenously and the blood throughout the body is rendered incoagulable. In most cases, Murray favoured general heparinization and intravenous injections (continuous, small dosages), preferable methods for lengthy periods of time (three to four days) and for deterring the formation of clots at site of injury as well as preventing the dislodgement of previously formed clots.30

He also had to find out how much and how long heparin should be administered after trauma to protect against clotting. What was the proper dosage to administer? If too much heparin was administered, the dogs died of massive haemorrhage. If too little was given, it was ineffective against clotting. How long should heparin be administered — a matter of hours or days or weeks following trauma

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or surgery? Murray used a "coagulometer" to determine the onset of clotting. It tested the clotting time of blood carefully extracted from the body, and gave Murray a guide against which to gauge dosage. During his experiments, Murray relied heavily on clotting time to measure the dosage and length of time that was required to produce this effect. For example, "a dog of 11 kilograms was given 2200 units of heparin intravenously. Within ten minutes the clotting time rose from the normal of 2 minutes to 15 minutes. After remaining at that level only a few minutes a gradual return to normal occurred in about one and one-half hours."

The toxicity of heparin influenced all of Murray's experiments. In 1932, Murray was working with Howell's very toxic, five-unit (per milligram) strength heparin. As Charles and Scott worked on the toxicity and potency of the substance, Murray was provided with improved heparin. Nevertheless, many of his dogs had strong reactions, sometimes vomiting or becoming very weak, other times dying. By 1935, Charles and Scott had produced a purified, 500-unit strength heparin. This was dissolved in distilled water or saline and 1 c.c. of this solution usually contained

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33 "The Howell unit of heparin is the amount which will prevent clotting of 1 c.c. of cat's blood for twenty-four hours. As this test is not carefully controlled, it was considered advisable to set up a standard for a unit of heparin. This standard has been set up in the Connaught Laboratories in the University of Toronto. The unit in the preparation provided by these laboratories is the potency of 0.01 mg of the purified crystalline barium salt. This unit has about five times the activity of the original Howell unit." D.W.G. Murray, "Heparin in Thrombosis and Embolism," British Journal of Surgery Vol. 27 (January 1940): 570.
approximately 15,000 units of the anticoagulant. Correspondingly, decreased reactions were recorded with this purified preparation of heparin.\textsuperscript{34}

In 1935, there was a shake-up in the make-up and direction of the heparin research project. Surgical fellow T.S. Perrett replaced Jaques when he left to write his master’s thesis. Perrett had both science and medical degrees, and from 1933 to 1935, had been a Fellow in the Department of Physiology. He was now a Fellow in the Department of Surgery, and was assigned to the heparin project.\textsuperscript{35} At this juncture, changes were made in methodology and supervision within the project. For the most part, before 1935 Murray had enjoyed substantial autonomy, working quietly on the fifth floor of the Banting lab with his various assistants, methodically conducting his experimental operations. There was no obvious friction among the team members; everyone was working on his particular aspect of the project, and their results reinforced each other’s findings. While Murray performed his animal experiments, Best studied the physiological action of heparin microscopically. Best’s research demonstrated that heparin prevented the buildup of platelets on the injured lining of the blood vessel, which corroborated Murray’s findings and his surgical success with heparin.\textsuperscript{36}

But this independent arrangement changed when Perrett joined the project. Best was shuffling the heparin team to prepare for the next stage of clinical trials.


\textsuperscript{35} Connaught Laboratories Archives, Heparin Papers, Correspondence J.F.W. Ferguson to L.B. Jaques, 9 January 1987 and correspondence L.B. Jaques to J.F.W. Ferguson, 17 January 1987; University of Toronto Archives, employment personnel card.

Murray expressed his opposition to Perrett's appointment to the research project. Based on Perrett's recent performance as a surgical fellow, Murray felt that Perrett was a poor surgeon and addition to the team. But Best did not agree with Murray. He felt Perrett was much better suited to the project than Jaques; he preferred to have Perrett who was both scientist and clinician. This way Perrett could then take the heparin work directly into the clinic. Perhaps this was an insight from his insulin experience. As a result of Murray's contention with Best as well as his personal feelings about Perret, it is not surprising that Murray and Perrett did not form a close working relationship. This mattered little since Perrett reported to Best, not Murray, unlike all other preceding arrangements with clinical assistants. This disturbed Murray.

Best and Murray did not share a close working or personal relationship. Easy interchange between participants, informal and formal, is a highly prized part of any research team. It was missing within this cooperative project. While Murray maintained an open, friendly rapport with his junior assistants in his laboratory, he did not work well with others outside of his area and control. Best simply did not want to repeat his current predicament with Jaques, who had left with all heparin data books and slides (Jaques' property) placing the research team in a vulnerable position when

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38 In the end, Murray's assessment of Perrett as a poor addition to the team was correct. Perrett did not work out as Best had hoped. According to Jaques, "Best hoped that Perrett would do the job with heparin that Bob Kerr did later with protamine-insulin; gain a year's experience with the drug in dogs as the basis to become the clinical expert with the drug." What success Perrett did have was in large part due to Campbell Cowan, the technician. Jaques commented that, "I found Perrett much more interested in boasting of his success in getting nurses into bed with him than in research." After the 1937 publication in Surgery, Perrett's name never appeared on another heparin paper. Connaught Laboratories Archives, Heparin Papers, Correspondence L.B. Jaques to J.F.W. Ferguson, 17 January 1987.
it came publication time. Murray, however, interpreted Best’s decision to supervise Perrett directly as an encroachment upon his autonomy as the primary clinical investigator. Best’s unilateral decision-making had been consistent throughout the project. He never consulted Murray, a man junior to him in reputation and position on the team. Best and Murray were also different types of researchers and men; in time, these differences became apparent to everyone on the team and a rift between Best and Murray began to grow. According to Jaques, Murray refused “to gloss over the failures and inabilitys of other people … Best was a remarkable scientist-politician [and] for Murray this attitude was anathema.”

After Perrett joined the research team, Best instructed him to repeat the work on thrombosis carried out by Jaques with a change in technique; clotting would be induced not by trauma or mechanical injury but by a chemical means - soricin. Perrett’s results showed a lower percentage of patent heparinized vessels when injury was produced by chemical means than Murray’s results with injury produced by mechanical means. The difference, however, was inconsequential. Perrett’s work supported the team’s overall findings that the incidence of vessel obstruction by thrombi was decreased when heparin was administered.

In three years, Murray made substantial gains towards a better understanding the effectiveness of heparin. By 1935, he had demonstrated the value of heparin against clotting and thrombosis in dogs. His results were clear: no clot or thrombi

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formation occurred when an appropriate dosage of heparin was injected, and if heparin was continued after trauma there was reduced tendency for occlusion during the healing. Murray’s work displayed good technique and methodology for the times. Generally, researchers defined their experiments, repeated them under various conditions, and reported their observations to others in such a way so that fellow clinical investigators could reproduce their results. Murray’s experiments presented controls, different methods of injections, resulting clotting times, and varying time lapses after trauma and heparinization. It was now time to test heparin on patients.42

Murray decided to administer heparin to two groups of patients. Postoperative patients received heparin to prevent the formation of blood clots and patients who had survived an attack of pulmonary embolism were also given heparin in an attempt to prevent a second embolus. He first administered heparin to patients on April 16, 1935, at the Toronto General Hospital. In his first series, Murray injected nine patients by regional or general heparinization. Clotting time was increased threefold (approximately 15 to 20 minutes) but five patients had a toxic reaction to the heparin. They experienced general weakness, headache and chills so the injections of heparin were discontinued and Murray waited until a less toxic preparation of heparin became available. Six months later, on September 30, 1935, Murray resumed and administered heparin to eight patients with much improved results.43

By mid-1937, Murray had treated seventy-six postoperative cases with heparin by intravenous injection. These patients had undergone a range of operations, such as appendectomies, herniotomies, colon resections, gastrectomies, bone grafting, as well

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as other minor procedures, and they were selected based on their risk of suffering postoperative thrombosis and/or embolism. (There was no control group, other than previous incidence statistics.) On average these patients received heparin for two or three days. Thrombosis did not occur in seventy-five cases. The injections had been measured to increase clotting time to 15 to 20 minutes but in some cases, the clotting time increased to as high as 51 minutes. These clinical cases were successful because they demonstrated "(1) that no deleterious effects were produced in several cases in which regional heparinization was carried out with moderately pure heparin and (2) that using highly purified material to prolong general heparinization would appear to be feasible." Heparin however was still an expensive drug. It cost $20 a day for the typical patient to receive heparin, and treatment usually continued for several days. In some cases, treatments were necessary for as long as three weeks. The price of heparin did not drop until Connaught began to manufacture the drug in greater quantity after 1937.

It was clear that toxicity had been a factor in Murray's first cases, and with pure heparin his results improved dramatically. Indeed Murray had some remarkable success cases. He described the case of one young mother who, two days after the birth of her baby, suddenly collapsed, turned blue and began to gasp for air. Medical

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44 D.W.G. Murray, L.B. Jaques, T.S. Perrett and C.H. Best, "Heparin and Thrombosis of Veins Following Injury," Surgery Vol 2 (August 1937): 175-77; 183-84. Dr. William Mustard remembered how Murray rigged the ward so nurses could monitor the intravenous drip. "He had rigged up in the office one of the drip things and the intravenous would make contact, you see, with the heparin in it, and a light would flash on in the office and he had a panel set up there. So the nurse would know by the count that the IV drips were working and they also knew if it was shut off. This was, of course, the introduction of heparin." Hannah Institute for the History of Medicine Oral History Collection, Volume 33, Dr. W.T. Mustard, page 30.

45 "Doom of Thousands — Blood Clot Declared — Overcome in Toronto," Toronto Star 4 November 1937.
Staff resuscitated her, but everyone feared a second fatal embolism. (At that time, doctors were reconciled to two out of five patients dying of a second embolism within a few days of the first attack.) He administered heparin intravenously to this patient, and after two weeks, she went home with her new baby with no ill effects.\footnote{Gordon Murray, \textit{Medicine in the Making} (Toronto: Ryerson Press, 1960), 89-92.} By 1939, Murray had treated 22 pulmonary embolism cases with heparin. As he stated, "again we were surprised and pleased with the results. Of the 22 cases none have died of embolism even though death seemed imminent for some."\footnote{D.W.G. Murray, "Heparin in Thrombosis and Embolism," \textit{British Journal of Surgery} Vol. 27 (January 1940): 588.} In 1940, Murray reported that 440 patients had been treated with heparin at the Toronto General Hospital without the occurrence of thrombosis and embolism.\footnote{D.W.G. Murray, "Heparin in Surgical Treatment of Blood Vessels," \textit{Archives of Surgery} Volume 40 (February 1940), 325.} Due to the Toronto group’s work on heparin, medical men could now use heparin to treat diseases where thrombosis in blood vessels was a threatening factor, such as pulmonary embolism, phlebitis, mesenteric, venous and coronary thrombosis.

In spite of Murray’s success, the medical community however did not immediately embrace heparin. In 1937, despite the availability of a commerical, purified form of heparin from Connaught Laboratories, it was not being used regularly in patient cases. According to Dr. W.G. Bigelow, practitioners were hesitant for two reasons: first they did not have a clear understanding of clot formation, thrombus and pulmonary embolism, and secondly they were afraid that the patient might haemorrhage.\footnote{W.G. Bigelow, \textit{Mysterious Heparin: The Key to Open Heart Surgery} (Toronto: McGraw-Hill Ryerson, 1990): 56, 87.} Most medical men did not diagnose pulmonary embolism correctly
and, as Thomas Belt and others were showing, there was a higher incidence of embolism than commonly believed.\textsuperscript{50} Pulmonary emboli are clots that form in the leg or pelvis and break free to travel to the lungs thereby obstructing the pulmonary artery or one of its branches. Peripheral emboli form in the heart and pass to the leg or arm arteries thereby leading to ischemia (deficiency of blood supply) and gangrene (tissue death). Embolectomies (the surgical removal of an embolus from a vessel) were rarely performed for pulmonary embolism as most operations ended in fatality. Murray had unique success in operating on peripheral arterial emboli.\textsuperscript{51} Now Murray was advocating the use of heparin to stop clots from forming in the pelvis and legs, thus eliminating the need for embolectomies. According to many practitioners, however, administering heparin was no better a solution in treating peripheral embolism because of the possibility of haemorrhage. Murray, himself, had lost animals to haemorrhage when too much heparin was administered. Murray responded that the prescribed amount of heparin was to be administered two to three hours after an operation, thereby allowing time for haemostasis to occur before introducing the anticoagulant.\textsuperscript{52} Strict adherence to Murray’s guidelines for administering heparin was crucial in these early days of its use.

The whole problem became more complicated. In North America, two methods of treating pulmonary embolism emerged during the 1930s that were preferred by practitioners over embolectomies. One group, including Murray, used


the anticoagulant (heparin) treatment to control the formation of clots, not just in the leg veins, but in the entire body. They were careful to administer the proper dosage to prevent haemorrhage. The second group advocated surgical vein ligation, which would manage the problem of embolism without the threat of haemorrhage. When clotting was suspected, these surgeons tied off the appropriate femoral veins to prevent clots from travelling up into the lungs. Their critics argued that tying off the femoral veins or the vena cava would lead to future leg disability. Often the two sides -- "anticoagulants" verses "vein ligators" --- participated in panel discussions at surgical conferences, adamantly arguing their positions. During the 1940s, Murray's fundamental arguments were finally accepted. After more reported successes with heparin, the use of anticoagulants in treating embolism became the more favoured therapy.53

It seems ironic that Toronto, the site of heparin research, put up substantial resistance and was one of the last medical centres to fully endorse its use in the treatment of embolism. In the Toronto General Hospital, the Professor of Medicine, Duncan Graham, strongly felt that heparin was dangerous and even banned Murray from the medical wards to prevent him for administering it to patients. His cautious stance was not unreasonable; many physicians would have been unqualified to administer the new drug. Graham had also conducted his own small trial of heparin on patients and no one had survived the treatment. As a result, Graham supported more conservative methods of therapy, a combination of nonoperative treatments,

including suction and pressure therapy, antispasmodic drugs and heat to the body.\textsuperscript{54} Consequently, heparin was not used on the medical wards (as opposed to the surgical wards where it was used) of the Toronto General Hospital for almost ten years after Graham's decision. This was true for both pulmonary and peripheral arterial embolism.

In 1950 Dr. William Greenwood finally swayed the medical department into using heparin after reviewing past peripheral arterial embolism cases. He reported that between 1938 and 1948, from a total of 310 cases, "only 13 percent developed gangrene of the leg after surgical embolectomy using heparin, compared with 42 percent of those on medical treatment."\textsuperscript{55} By this time, heparin was also more widely administered throughout the world and more embolism cases in both the lungs and the limbs had been reported that demonstrated its usefulness. It finally became accepted practice on the medical wards at the Toronto hospital.

In 1938 Murray shifted the focus of his heparin investigation. By this time he had already demonstrated the effectiveness of heparin in treating thrombosis and embolism. Murray wanted to explore greater surgical possibilities. He began to experiment with the clinical application of heparin in repairing damaged arteries and veins, and became a pioneer in the field.


Before 1900 little could be done with blood vessels except tying them off to stop bleeding. There was no technique for opening and closing arteries or veins, or for sewing two ends of a blood vessel together. There were many investigators exploring these possibilities, but no one agreed on the correct way to reunite arteries and graft veins. Were blood vessels best repaired directly or with various prostheses? What was the appropriate suture technique to do this work?\(^5\)\(^6\) By 1910, experimental surgeon Alexis Carrel had ended the debate and was christened the father of vascular surgery. Carrel’s triangulation technique --- three sutures placed at equal distances from one another along the circumference of the divided vessels – and his “everted” sutures, which left the inside of the vessel free of thread, reduced trauma on the artery or vein and lessened the chance of blood clotting. On dogs, Carrel had demonstrated the feasibility of suturing together blood vessels and laid the foundations for vascular surgery, heart surgery, and organ transplantation. In 1912, he received the Nobel Prize for Physiology or Medicine for this research.\(^5\)\(^7\) But Carrel was never able to move beyond the laboratory; the risks of clotting and infection were still too high and prevented him from performing any clinical cases. Now that heparin was available, Murray dared to carry Carrel’s work to that next stage, to apply his techniques of arterial anastomosis (surgical connection of vessels) and venous grafts on patients.

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Arterial suturing required the end-to-end joining of arteries to resume blood circulation. Surgeons, as trained, ligated damaged arteries to stop the bleeding and thus prevent haemorrhage. In most cases, ligature is necessary and often is all that is needed. The blood finds alternate routes to supply that area or limb. But in some cases, it is desirable to restore the blood flow through large arteries. If these vessels could be reopened and anastomosed, loss of circulation and ultimately amputation could be prevented. When a segment of the artery was too badly damaged, and an end-to-end arterial suture was not possible, a venous graft could possibly restore circulation. A segment of vein was sutured into the gap left by the damaged artery, which had been excised and prepared to receive the graft. The vein was placed in a reversed position so that its valves would allow the flow of blood to continue in its intended direction. There was also the question of whether the vein graft could withstand the stronger arterial pressure.

Back in the lab, Murray attempted to duplicate Carrel's techniques on dogs. His experiment notebooks record his early failures and frustrations. Murray was working on small vessels and this required great surgical skill. Moreover, he needed to determine the right amount of heparin to administer before and after the surgery. His first experiments were venous grafts. In each dog, Murray excised a segment of the carotid artery, then removed a similar length of the external jugular vein that was anastomosed (put end-to-end) to take the place of the artery.58 At the outset, Murray lost many of his dogs to haemorrhage or pneumonia: April 13, 1938 -- "... died from haemorrhage from 2 small holes adjacent to suture line ...", April 22, 1938 -- "... died suddenly from haemorrhage due to tear at both suture lines ...", May 18, 1938 -- "...

dog died from pneumonia..." By the fall of 1938, Murray was able to keep his dogs alive but his vessels were not patent despite varying heparin dosages and lengths of time that it was administered. He was frustrated and baffled by his lack of results. He plodded on, tediously repeating Carrel’s techniques. Months passed, his days seemed longer, and more dogs died. It was exhausting work. It was not until early in 1939 that Murray finally had some successful grafts. Success at last, thought Murray. He photographed these prized grafts for future publication.59

From 1938 to 1940 Murray performed a variety of vascular procedures on almost 100 dogs. His research notebook records each dog, the experimental procedure, dosage and administration of heparin, clotting times, and autopsy findings.60 Murray’s experiments ranged from venous and arterial grafts to inserting glass cannula between vessels to work on coronary occlusion and kidney grafts.61 His

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60For example, Dog 21 was a female black mongrel, part scotty with hound ears:
"Oct 24/39 - venous graft to right carotid, 500 VH on table, hooked to pump at 12:45 noon, 2:09 pm clotting time 6 minutes, approx 600 cc of saline containing approx 12 VH per cc;
Oct 25/39 - 11 am clotting time 4 min, 800 cc of saline 22 units per cc at 11:05 am;
Oct 26/39 - 10:35 am clotting time 2 hour, 4:00 pm 200 cc normal saline added to flask;
Oct 27/39 - 10:29 am clotting time 50 minutes;
Oct 28/39 - dog drowsy not eating, 9:30 am 700 cc saline containing 10 VH per cc;
Oct 29/39 - found dead at 10 am;
Oct 30/39 - considerable oedema of lower jaw area and neck, no gross evidence of bleeding but tissues blood stained, oedema and hemorrhage extended down into superior mediastinum, catheter was out, graft did not leak when water forced through with syringe, when opened was a large frutile(?) vegetation at the proximal end of the graft, the rest of the graft was filled with a dark red semi-solid material, here was a small thrombus clinging to the wall but not pulling the lumen of the artery just above the superior anasto(?) of the junction, cause of death septicaemia (had been receiving non-sterile intravenous solutions)"


biggest challenge was discovering the right amount of heparin to administer to avoid clot formation and to allow the grafts and cannula to fibrose or attach to the vessel. Once he had determined this, he had substantially more success. Murray reported that "of over 50 experiments in which heparin was used, the arteries remained patent in more than 80 per cent, as compared with 35 per cent of the controls. In a similar group, with improved technic in administering heparin, all the vessels remained patent."  

Murray was conducting these animal experiments in preparation for performing those procedures on patients. He did not wait long before applying these vascular techniques to clinical cases. In October 1940, Murray reported his clinical success to the profession’s elite at the American College of Surgeons conference in Chicago. One of Murray’s first arterial suture cases had been a Toronto man with an aneurysm in the brachial artery. Murray removed the aneurysm, brought the divided ends of the artery together and sutured the ends together in anastomosis. Heparin was injected to fill the vessel. A continuous intravenous injection of heparin was given for the next three days keeping the patient’s clotting time at about 15 minutes. The patient returned to work four weeks after the operation. In another case, Murray replaced two and one-half inches of the femoral artery with a venous graft. "With this three inch piece of vein the gap in the artery was bridged successfully and heparin was injected. When the circulation was restored, excellent pulsation appeared in the vessels at the foot. Heparin was injected continuously for the next eight days and then


stopped. Murray reported many other cases where he had successfully repaired or replaced arteries. Newspapers reported several of these cases, especially sensational occurrences such as how Murray saved the severed hands of a factory worker from almost certain amputation after her heroic 200-mile trek to Toronto.65

By 1940, Murray was publishing his results in surgical journals for other surgeons to repeat. His vascular research, based on Carrel's techniques, showed greater success in blood vessel surgery when heparin was administered in the right dosage for the proper length of time. Heparin allowed vessels to remain patent while healing and his dog experiments showed that some venous grafts in arteries had been kept patent and functioning for almost one year.66 Ultimately, Murray's increasing number of successful clinical cases encouraged others to enter the field. Surgeons all over the world started to experiment with heparin and blood vessel surgery, repeating his procedures and devising new ones. Amputation was no longer the only course of action; surgeons could now repair arteries and veins and restore circulation. (In fact, during World War II, Murray devised a procedure for field surgery: glass cannulas inserted between ruptured blood vessels maintained blood circulation in the limbs of wounded soldiers until they were transported to a surgical unit where the tubes could be replaced by venous grafts. French surgeon Tuffier had suggested a similar operation during World War I, dipping the tubes in liquid paraffin to maintain circulation temporarily however the tubes plugged immediately with thrombus. By


1940, surgeons had heparin to prevent clotting. This capability of repairing damaged blood vessels significantly advanced the infant field of vascular surgery. Years of meticulous research and hard work paid off for Murray and medical science in general.

Together and individually, Best and Murray received praise and congratulations from the media and the medical profession for their work on heparin. There was some but not a lot of newspaper coverage of their innovation. At first, this attention was undoubtedly due to Best's notoriety as a co-discoverer of insulin rather than any realization of the true impact of heparin. Best had announced heparin and its clinical applications as significant contributions to medicine, and the public and profession accepted it. But in contrast to the dramatic impact of insulin on dying diabetic children, the contribution of heparin to medical and surgical problems was less visible and doctors had yet to explore all of its new applications. Moreover for Best, who considered insulin to be his greatest research, heparin was only a side interest. Yet it was his standing and experience from being the co-discoverer of insulin that led the heparin project to its successful end. Best played the key role of identifying the problem, making possible the financing, arranging the necessary facilities, and allowing each group of scientists to do their work. Furthermore, Best used his contacts to promote the acceptance of heparin by the medical community.

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67 Dr. Bill Bigelow learned this procedure before heading overseas, as did others like Dr. Bill Mustard who did perform the "tube" operation. The War Office had been informed of this procedure as well, but it is unknown how many such operations were ever performed overseas. University of Toronto Archives, University of Toronto Annual Reports (1940): 69-70; (1944): 97; and (1945): 108; W.G. Bigelow, Mysterious Heparin: The Key to Open Heart Surgery (Toronto: McGraw-Hill Ryerson, 1990), pp.96-101.

68 In fact, Michael Bliss argues that Best spent much of his career protecting that identity as co-discoverer of insulin. See Michael Bliss, "Rewriting Medical History: Charles Best and the Banting and Best Myth," Journal of the History of Medicine Vol. 48 (1993): 253-274.
Best "by just being there and setting up the project spelt (sic) out the 'can do' — same as insulin," commented Jaques.\textsuperscript{69} The media latched onto this insulin association when reporting on the success of heparin. Headshots of Best and Murray were printed alongside sensational headlines: "Doom of Thousands ... Overcome in Toronto", "Save 1,000 Yearly," and "Heparin Miracle".\textsuperscript{70} Like insulin, heparin was presented as a "miracle drug."\textsuperscript{71}

More than the press, the medical community recognized the significance of and thus awarded greater attention to Best and Murray's heparin research. Their early co-authored publications were timely, purposeful and well written. Their first communication, a two-page article in the \textit{Canadian Medical Association Journal} in 1936, had been to establish precedence in the clinical use of heparin. The next year, a more complete report of their results was presented in \textit{Surgery}, which detailed their laboratory and clinical experiences with heparin. These were well-timed publications. Researchers at the Karolinska Institut in Stockholm, Sweden were conducting the same experiments with similar results, and the Toronto team knew it.

Within months of Best and Murray's publications, Clarence Crafoord, a thoracic surgeon and researcher at the Karolinska Institute, was reporting his work on the clinical application of heparin. His colleague, Erik Jorpes, a research chemist, had

\textsuperscript{69}McMaster University Archives, Oral History Collection, L.B. Jaques Interviews, Box 001, Correspondence L.B. Jaques to M. Bliss, 28 June 1986; W.G. Bigelow Papers, Private Collection, Correspondence L.B. Jaques to W.G. Bigelow, 25 June 1988.

\textsuperscript{70}"Doom of Thousands — Blood Clot Declared — Overcome in Toronto," \textit{Toronto Star} 4 November 1937; "Hopes to Save 1,000 Yearly by Using Heparin in City," \textit{Toronto Star} 19 November 1937; "Heparin Miracle May Save Limbs of Many in War," \textit{Toronto Star} 18 April 1940.

\textsuperscript{71}James Marcum argues that the research projects of insulin and heparin share a similar research strategy including the researchers' roles, research methods and protocols and presentation of their results as 'a miracle drug'. See "Research in the 'Best' Tradition," AAHM Conference abstract, May 1996; Personal correspondence, James Marcum to author, 5 February 1996.
begun purifying heparin soon after his 1929 visit to Connaught Laboratories. With Jorpes' heparin preparations, Crafoord conducted his first clinical cases in August 1935, only four months after Murray, but with poor results. After more successful trials, Crafoord published his work on the clinical application of heparin in 1937. This publication did not usurp credit for heparin, but in fact served to substantiate Murray's early findings.\textsuperscript{72}

The Swedish team was competing with the Toronto team to describe and to demonstrate the clinical application of heparin. There were clearly strained relations between Best and Jorpes as well as Charles and Jorpes.\textsuperscript{73} Their professional disagreements, however, were kept out of print and the nature of their falling out cannot be documented. In Best's mind (and probably Charles' too), Jorpes' interest and methods of purifying heparin had originated with his visit to Connaught Laboratories.\textsuperscript{74} Jorpes was a strong personality, and there were known tensions between him and his students as well as colleagues. Anyone working in Jorpes' laboratory knew that no one "could differ with the Professor." Many were surprised that there was no apparent friction between Jorpes and Crafoord.\textsuperscript{75} Relations between Murray and Crafoord were cordial, but there was almost no communication between

\textsuperscript{72}Clarence Crafoord, "Preliminary report on postoperative treatment with heparin as a preventive of thrombosis," \textit{Acta Chirurgia Scandinavica} Vol 79 (1937): 407-426. In 1941, Crafoord published a second article on heparin but it was clear that Murray, with his vast clinical experience, had emerged the expert in the field. See Clarence Crafoord and Erik Jorpes, "Heparin as a Prophylaxis against Thromboses," \textit{JAMA} Vol 116 (1941): 2831-2835.

\textsuperscript{73}James Marcum also comments that a "tension" existed between the two research groups. See James Marcum, "The Development of Heparin in Toronto," \textit{Journal of the History of Medicine} Volume 52 (July 1997): 323.

\textsuperscript{74}Best makes this very clear in his 1959 article, with little other reference to Jorpes. See C.H. Best, "Preparation of Heparin and Its Use in the First Clinical Case," \textit{Circulation} Vol 19 (January 1959): 83.

\textsuperscript{75}Connaught Laboratories Archives, Heparin Papers, Correspondence L.B. Jaques to J.K.W. Ferguson, 5 May 1986.
these two surgeons. In the end, the Toronto team was first in its discovery and description of the clinical application of heparin. If they had faltered, the Stockholm group would have delivered this miracle drug to the world. Thus, the presence of researchers elsewhere investigating heparin motivated the Toronto team to work harder and ultimately served to corroborate their results.

The Toronto team, notably Best and Murray, emerged as the heparin experts. They received numerous professional honours, and Best and Murray basked in the recognition, if sometimes competing for credit. After their first few co-authored publications, Best and Murray began to publish separate articles and ended their formal collaboration. Best worked the physiology and scientific audiences while Murray kept to the surgical circles, each focusing on their own role in the heparin project. For Murray, his association with Best had been beneficial, boosting his standing and profile as a clinical investigator. Professional recognition of the value of their heparin research was now secured and Murray received invitations to speak at prestigious venues on his work. Between 1938 and 1940, Murray gave papers before the three most prestigious surgical groups in the Anglo-American medical world – the American Surgical Association, the American College of Surgeons, and the Royal College of Surgeons in England.

For Murray the most flattering of these venues was the Royal College of Surgeons in England in 1939. He was asked to deliver the Hunterian Lecture and was only the second Canadian to do so, preceded by W.E. Gallie in 1924. It was a sweet return to London for Murray. His lifetime admiration for British culture and medicine

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deepened his pride in accepting the honour. Dinners and luncheons were arranged upon his arrival, and Murray marveled at the titled medical men attending the party of a ‘colonial’. He liked the attention, the praise and the hospitality of the London medical community. After little more than a decade as a surgeon, Murray was enjoying the professional spotlight – the accolades and prestige that came with medical breakthroughs and their discoverers.

As a member of Best’s research team, Murray demonstrated the clinical application of heparin, advancing the field of vascular surgery as well as opening up other new surgical possibilities. It was a substantial contribution to medicine, and marked his debut into the public and professional spotlight. He was a pioneer in blood vessel surgery, with others like Swede Clarence Crafoord close behind. Murray’s heparin investigation highlighted his skills as a surgeon and competence as a clinical investigator. It also revealed that Murray did not work well within a large interdisciplinary team. He preferred to conduct his research independently, within his own control, and with firm direction to junior assistants.

As a result of these years spent operating on dogs and later humans, Murray emerged as one of the most experienced vascular surgeons in the world. Throughout the 1930s, he had had a virtual monopoly on the clinical use of heparin. Only Crafoord in Stockholm had had a similar supply, yet the number of his cases was

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substantially less. By 1940, Murray had reported on over 400 clinical cases, and he was regarded as one of the leaders in this new field of surgery.⁷⁹

Murray was now encouraged to perform more difficult and previously unsuccessful operations. His success with the clinical application of heparin fed his drive and self-confidence as a researcher and surgeon. He enjoyed confronting various surgical challenges and he experimented with a range of techniques and applications, including a few early kidney transplant operations. At the time, there was little interest in organ transplantation. Instead, the attention of the public and the profession shifted to the heart, one of the most sensitive organs of the body. Historically, practitioners had been unsuccessful in treating the various diseases of the heart. By the 1940s, surgeons boldly began applying their newly learned vascular techniques to the heart – once again, with Murray emerging as one of the leaders in the field.

Chapter 3

Canada's "Blue Baby Doctor": The Glory of Heart Surgery

"Blue babies" are children suffering from congenital cyanotic heart disease, a condition present in 8 of every 1,000 births in North America.¹ These babies are born with certain heart malformations — possibly holes in the interior heart walls, open fetal ducts, transposed heart vessels or obstructed valves — which interfere with the normal flow of blood through the heart and lungs. In these cases, a large proportion of the child’s blood is prevented from properly circulating through the heart and into the lungs to be replenished with oxygen. De-oxygenated blood is re-circulated through the body, causing cyanosis; the child’s skin, lips and fingernails take on a bluish hue. As well, blue babies may suffer unusual murmurs or thrills of the heart, a slow or stunted rate of growth and development, and an alteration in the size, shape and/or position of the heart. In some cases, the heart may enlarge to compensate for its

malformation and abnormal circulation, and the baby’s blueness may not appear for some months after birth.

As blue babies age, they suffer increasing shortness of breath, spells of unconsciousness, and respiratory distress from oxygen deprivation. Depending on the nature of the heart malformation, these infants live with varying degrees of incapacity. Most cyanotic children are not capable of participating in such normal child activities as playing outside or walking to school. Prior to the mid-twentieth century, no improvement in their condition was to be expected and their prognosis was grim: few blue babies lived into adulthood.

Until the 1940s, physicians could offer only palliative medical treatment. Blue babies were treated with rest, oxygen, heart medication, and sometimes diuretics. In 1936, Canadian doctor Maude Abbott of McGill University published an Atlas of Congenital Cardiac Disease based on one thousand cases, and in 1947 American cardiac pediatrician Helen Taussig at Johns Hopkins University wrote the first comprehensive textbook on the subject entitled Congenital Malformations of the Heart. As physicians like Abbott and Taussig were improving the profession’s understanding of and ability to diagnose congenital heart conditions, several surgeons were working towards offering surgical treatment for these patients --- what had never been attempted before: operating on a beating heart.²

Gordon Murray, fresh from his heparin research and establishing new vascular techniques, was among those tackling the challenge of heart surgery. He and other investigative surgeons, including Swede Clarence Crafoord and Americans Robert

Gross and Alfred Blalock, were drawn into the problem of blue babies through an extension of their work with blood vessels. What they now were able to do in vascular surgery seemed applicable to certain heart malformations, and they hoped to treat such disorders surgically despite failed attempts by others in the past. They had several technical and ideological barriers to overcome, and in the end, were able to do so. Their success in treating cyanotic children with palliative surgery led to more complex heart operations and ultimately corrective procedures for both congenital and acquired heart disease. Modern cardiac surgery began with the blue baby operations of the 1940s and 1950s, and expanded rapidly as more researchers and clinicians entered the field and new technologies led to greater surgical possibilities.

At the beginning of this century, heart surgery was limited to simple extracardiac procedures. Surgeons sutured puncture wounds, drained the pericardium (the sac around the heart) to treat an infection or to relieve chest pain, and, in a few exceptional cases, removed foreign bodies lodged in the walls of the heart. Patients, however, rarely recovered from such injuries. This discouraged most surgeons from operating, and the profession thought this wise. In the 1880s, Theodor Billroth of Vienna, one of the world’s most prestigious surgeons, stated that, "Any surgeon who would attempt an operation on the heart should lose the respect of his colleagues." In 1896, Sir Stephen Paget of London declared, "The heart alone of all viscera has reached the limits set by nature to surgery. No new method and no new technique can
overcome the natural obstacles surrounding a wound of the heart." But in the same year as Paget's pronouncement, Ludwig Rehn of Frankfurt successfully operated on a patient with a stab wound to the right ventricle of the heart. Rehn closed the wound with three sutures and the patient made a complete recovery. Subsequent heart operations, however, were not as successful as technical difficulties such as infection, clotting and surgical accuracy continued to plague surgeons. As of 1909, there had been 159 heart operations reported with a sixty per cent mortality rate. Medical practitioners and patients remained properly wary about cardiac surgery.

Historian Christopher Lawrence argues that, in addition to technical difficulties, different ideological understandings of the heart also acted as obstacles in the development of cardiac surgery at this time. He suggests that the rise of the "new cardiology" in Britain at the beginning of the twentieth century displaced the surgeon from treating heart disease. Previously, diseases of the heart were diagnosed as either valvular or nonvalvular, as physicians focused on the anatomy and mechanics of the heart. The "new cardiology" embraced the concept of the "living heart" and shifted the clinician's focus to the physiology and dynamics of the heart; a "failing" heart was now due to muscular properties and valvular concerns were considered much less important. Diseases of the heart were defined as medical, with surgery playing a

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5Christopher Lawrence, "Moderns and Ancients: The "New Cardiology" in Britain 1880-1930," in W.F. Bynum, C. Lawrence and V. Nutton, ed. The Emergence of Modern Cardiology (London: Wellcome Institute for the History of Medicine, 1985), p. 12; see also Christopher Lawrence, "Democratic, divine and heroic: The history and historiography of surgery," in Christopher Lawrence,
lesser role, if any. Chest surgeons, of course, challenged the concepts of the new cardiology, for they regarded the gross lesion, such as diseased valves, as crucial. The new cardiologists responded that valve surgery did not treat what was basically wrong, the functioning of the heart muscle.

It was during this debate in 1925 that Henry Souttar of London performed the first non-fatal and possibly successful heart valve operation. It was a controversial procedure. Souttar exposed the heart and, by inserting his finger through the left atrium of the heart, believed that he had opened an obstructed mitral valve. (The mitral valve consists of two cusps or flaps and allows blood to pass from the left atrium into the left ventricle of the heart but prevents any backward flow. Mitral stenosis or obstruction is the narrowing of the opening of the mitral valve, most often a result of chronic scarring, and is a common heart disease. ) Souttar’s patient survived the operation and showed some probable improvement, but British cardiologists were unwilling to refer any more patients to him for heart surgery. Souttar was never given the chance to repeat his operation. Sir James Mackenzie, the

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9In a letter from Sir Henry Souttar to Dr. Dwight Harken, Souttar wrote, "I did not repeat the operation because I could not get another case. Although my patient made an uninterrupted recovery the Physicians declared that it was all nonsense and in fact the operation was unjustifiable." Taken from Lael Wertenbaker, To Mend the Heart (New York: The Viking Press, 1980), p. 76; See also W.P. Cleland, "The Evolution of Cardiac Surgery in the United Kingdom," Thorax Vol. 38 (1983): 887.
King's personal physician and the acknowledged authority in England on heart disease, commented: "Indeed! The only heart disease I know of is that of the muscles and no operation will correct it." Traditional British physicians did not always endorse the new cardiology, but they were unconvinced of surgical success for heart cases, especially those procedures that involved inserting a probing finger into this sensitive organ.

Medical men elsewhere were also skeptical about operating on the heart. American surgeons Elliott Cutler, Claude Beck, Evarts Graham and Duff Allen tried to repeat Souttar's success but could not. Cutler, Beck, Graham and Allen devised new instruments and techniques to operate on the mitral valve to relieve stenosis, but failed. They faced the same technical difficulties -- inadequate diagnosis, poor anesthesia, blood clotting and infection -- that had frustrated surgeons before them. Little had been gained; heart surgery still almost always resulted in fatality.

By the late 1930s and particularly the 1940s, several vascular surgeons -- including Gordon Murray, Clarence Crafoord of Sweden, Russell Brock of England, and Americans Robert Gross, Dwight Harken, Charles Bailey and Alfred Blalock -- had become interested in heart surgery. Unlike earlier surgical investigators, Murray, Crafoord, Brock and others were operating with heparin, an anticoagulant, now available and performing vascular procedures with success. They now transferred those techniques to the heart. Murray began research in heart valve surgery, picking up where Souttar, Cutler and Beck had left off.


Murray studied the mitral valve and the problem of mitral stenosis and regurgitation. In the past, to correct mitral stenosis Souttar and other surgeons inserted their fingers or finger-knives into the mitral valve to widen the opening. However, regurgitation became a problem. Mitral regurgitation or incompetency occurs when the valve leaflets or flaps fail to stop blood from flowing back into the left atrium from the left ventricle. Little was known about the exact anatomy and function of the valve leaflets at this time. The problem of regurgitation was graver than a stenosed valve so many surgeons left the original condition untreated.\footnote{12}

Murray suggested that after resecting part of the damaged valve, a vein graft be sutured under the opened valve to prevent regurgitation; the vein was flexible, strong and not likely to induce blood clotting. The vein was turned inside out and guided into place below the opened mitral valve. It acted as a sling, allowing the blood to flow through the valve into the ventricle. When the ventricle contracted, the sling was forced back into the orifice to prevent regurgitation. In his animal experiments, eight of his ten dogs died of infection after surgery. The two surviving animals showed no signs of heart failure, suggesting to Murray that his vein graft procedure did work if no infection occurred. These experiments and results were published in the Canadian Medical Association Journal in 1938, but there was little attention given to Murray’s work.\footnote{13}

\footnote{12}{American surgeons Elliott Cutler and Claude Beck experienced failure with this operation in the 1920s, little else was tried, and a successful and universally accepted procedure for mitral stenosis did not emerge until 1948. Harris B. Shumaker Jr., \textit{The Evolution of Cardiac Surgery} (Bloomington: Indiana University Press, 1992), p.107-108; Lael Wertenbaker, \textit{To Mend the Heart} (New York: The Viking Press, 1980), p.69-73.}

Soon thereafter, Murray performed his vein graft procedure on patients. He operated on several young patients who suffered from mitral regurgitation and varying degrees of disability because of it. These patients took the risk of the new heart surgery in hopes of improving their condition, and fortunately, some of them did enjoy better health after the operation. One man who previously had led the life of an invalid was able to return to work, play golf and enjoyed good health for many years after the operation. When Murray presented his alleged clinical success to his Toronto General Hospital colleagues, they were horrified and refused to refer any more cases onto Murray. In their mind, it was clearly an experimental procedure that was not ready to be tried clinically. They were right; Murray had moved too quickly from operating on animals to humans. His laboratory results were not conclusive, two out of ten successful dog operations did not demonstrate that the procedure was safe for patients. Hospital medical authorities had to err on the side of caution, and discouraged Murray from performing any more such operations without further laboratory data. Murray never formally reported on his few clinical cases and stopped doing the operation. In his view, his procedure was successful and he was not interested in simply repeating it in the lab just to accumulate more successful animal cases. He returned to the lab but turned his attention to other vascular and cardiac procedures. He was the only surgeon known to be performing heart valve operations on patients at that time, and as seen by the conservative but professionally necessary

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14 Bill Mustard said, "[T]hey never let him do anything at the Toronto General Hospital ... the medical profession frowned on having anything to do with the valves in people who had diseases of the heart muscle. You see, it's like everything else. It was a misconception." Hannah Institute for the History of Medicine Oral History Collection, Volume 33, Dr. W.T. Mustard, page 133. See also Gordon Murray, Medicine in the Making (Toronto: Ryerson Press, 1960), p.135.

reaction of his Toronto colleagues, the medical world was not ready to receive such bold new procedures. It also typified Murray’s impatient behaviour of moving too quickly from the laboratory to his patients with new therapies, and his lack of follow-up on successful cases to document the alleged improvement and innovation more conclusively.

Elsewhere, there were other surgeons operating on patients for a variety of heart conditions at this time. Robert Gross in Boston, Clarence Crafoord in Stockholm and Alfred Blalock in Baltimore were devising surgical procedures to overcome various malformations of the heart and great vessels. This was the prelude to the celebrated blue baby operations. These surgeons were operating on the great vessels of the heart (arteries leading away from the heart), closing “patent ductus” and removing “coarctations”, and preventing early heart failure. When Blalock surgically redirected un oxygenated blood back into the lungs to relieve cyanosis in children, he was universally acclaimed. After Blalock’s operation, blue babies turned pink and were no longer fatigued after minimal physical exertion. These early cardiac operations, however, were difficult procedures. The surgeon did not have to penetrate the heart, as Murray had in heart valve surgery, but these various procedures did demand surgical skill and the application of new vascular techniques.

In Boston, Gross developed a procedure to correct “patent ductus arteriosus”. (The ductus arteriosus is a blood vessel in the fetus that connects the pulmonary artery directly to the ascending aorta, by-passing the pulmonary circulation. It normally closes after birth, but when it remains open or patent, it allows a significant proportion of the infant’s blood to flow back through the body without being replenished with
In 1938 Gross operated on a baby dying from heart failure due to patent ductus arteriosus. He surgically closed the duct. The child's blood was no longer improperly routed to the lungs. What nature had failed to do, the surgeon was now able to fix. Gross' successful heart procedure was applauded by the profession, and soon other surgeons began performing this operation.\textsuperscript{17}

In 1944, Clarence Crafoord in Stockholm and Gross in Boston independently developed a procedure to correct coarctation of the aorta. (Coarctation of the aorta is a congenital narrowing of a short segment of the aorta, which obstructs blood flow.) In both Crafoord's and Gross' procedures, the narrowed portion of the aorta was excised and the two ends of the vessel sutured together. In cases where the distance was too great for the two ends to meet, an arterial graft was performed. This heart procedure was in fact a form of vascular surgery, employing the techniques of vessel anastomosis and grafting.\textsuperscript{18} Regardless of the techniques used, this operation corrected a heart malformation found in many children. It improved their condition by reducing their high blood pressure and their risk of heart failure.

That same year in Baltimore, surgeon Alfred Blalock and cardiac pediatrician Helen Taussig developed a surgical procedure for blue babies with Tetralogy of Fallot heart malformations. (Tetralogy of Fallot is a complex congenital heart disorder in


\textsuperscript{17}Dudley E. Ross of the Montreal Children's Hospital was the first surgeon known to perform this surgery in Canada. By June 1941, he had operated successfully on three children suffering from patent ductus arteriosus. McGill University Archives, Montreal, Scrapbook for 1941, page 152, "Montreal Doctor Cures Blood Stop," \textit{Montreal Gazette} June 28, 1941; Montreal Children's Hospital, Annual Reports, Report of the Department of Surgery for 1940, p.46.

which there are four distinctive heart abnormalities. Blalock and Taussig developed an operation for this condition whereby a vascular shunt was constructed between the subclavian and pulmonary arteries. It was crude but effective. They rerouted the partially oxygenated blood flowing out the aorta directly back to the lungs to receive oxygen. It was not a cure, but a palliative procedure. The surgeon did not enter the heart to repair the malformations, but operated on the greater vessels outside the heart to create a new route for blood to reach the lungs. This procedure of creating a shunt between vessels outside the heart offered a palliative measure to overcome a multitude of circulatory problems within the organ.

Prior to 1944, a child with a malformed heart had a poor prognosis and surgical treatment was not considered an option. The new blue baby operation changed this. The medical profession reviewed the operations of Gross and Crafoord with interest but also very cautiously. The Blalock-Taussig procedure had a much greater impact on the profession. This blue baby operation excited and won the immediate support of the profession. Why? Reporting in the *Journal of the American Medical Association* in 1945, Blalock and Taussig described their operation in "minute and careful detail and in easy-to-understand English". This went a long way

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20 As mentioned above, the great vessels of the heart are the superior vena cava, the ascending aorta and the pulmonary trunk. The left and right pulmonary arteries branch off the pulmonary trunk. Off the aorta arch (the top of the ascending aorta), the brachiocephalic, carotid and subclavian arteries branch off. The Blalock-Taussig shunt therefore connects the pulmonary artery (of the pulmonary trunk) and the subclavian artery (of the aortic arch). Robert Carola, John P. Harley, Charles R. Noback, *Human Anatomy and Physiology* (Toronto: McGraw-Hill, Inc, Second Edition, 1992), p.586.


22 Blalock had been using his "shunt" inadvertently to study pulmonary high blood pressure. Taussig realized its suitability to relieve the problems of cyanotic children. Personal interviews by author.
towards educating practitioners about new surgical possibilities for the heart.

Surgeons could understand the procedure and some were able to repeat Blalock's success. As well, this operation in which blue babies turned pink was far more dramatic than the relief of high blood pressure and heart failure (coarctations and patent ductus).

Moreover, most children suffering from coarctation of the aorta and patent ductus arteriosus lived to be adults. Tetralogy of Fallot patients did not; only one-fourth of these blue babies reached adolescence and they suffered severe disabling symptoms. Surgeon-historian Dan McNamara wrote, "The palliation of Tetralogy of Fallot offered a new approach to complex anomalies of the heart. While closing a persistent ductus seemed like the obvious and logical thing to do, creating a ductus was a new and ingenious concept of indirectly relieving the problem of diminished pulmonary circulation." With Tetralogy of Fallot as the most common of congenital heart disorders, surgeons swiftly accepted and used the shunt procedure. Along with the closure of ducti and resection of co-arctations, it became one of the first routine heart operations of this century.

Surgeons and physicians flocked to Baltimore to study with Blalock and Taussig, to learn how to diagnose the condition with greater accuracy and how to

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perform the difficult and meticulous procedure.\textsuperscript{26} According to surgeon Dan McNamara, the Blalock-Taussig operation "provided the model and rationale for palliative surgery." It erased previous medical concerns about operating on critically ill heart patients. It demonstrated that these patients could safely receive anesthesia, could endure a two-to-three hour operation, and could tolerate the clamping of their pulmonary arteries for anastomosis to be performed. Furthermore, their arm, from which the new blood flow to the pulmonary artery was now diverted, functioned properly despite the new deprivation of blood supply.\textsuperscript{27} Other surgeons soon developed their own palliative shunt procedures. Willis Potts designed an aorta-to-left pulmonary artery anastomosis, William Glenn developed a superior vena cava-to-right pulmonary artery anastomosis, and Brock performed a pulmonary valvotomy for pulmonary stenosis.\textsuperscript{28}

\textsuperscript{26}Many surgeons returning from the war had tremendous surgical experience — vascular, abdominal and chest — and took up the new exciting field of cardiac surgery. In the case of Toronto surgeons, both W.G. Bigelow and W.T. Mustard spent time at Johns Hopkins, observing Blalock and others, before returning to the Toronto General Hospital and the Hospital of Sick Children respectively and their successful careers in heart surgery. After returning from Hopkins in 1947, Mustard successfully performed many blue baby operations, particularly after all such operations were permanently transferred from Murray's service at the TGH to HSC in the early 1950s. Some people have incorrectly referred to Mustard as the "blue baby doctor". Mustard did perform various blue baby procedures, and in 1964, devised a new operation to save blue babies with transposition of the great vessels -- a congenital heart disorder that needed the technology of open heart surgery in order to treat surgically. See "Blue Baby Here is Sheila Armour, Progressing Well," Port Arthur News Chronicle 23 December 1947 taken from National Archives of Canada, D.W.G. Murray Papers, MG 30, B110, Volume 47, File 44; Marilyn Dunlop, Bill Mustard: Surgical Pioneer (Toronto: Hannah Institute and Dundurn Press, 1989), p.45-48, 65-68; Thomas Fisher Library, University of Toronto, Academy of Medicine Collection, Biographical Files, 1235 W.T. Mustard.

\textsuperscript{27}Dan G. McNamara, "The Blalock-Taussig Operation and Subsequent Progress in Surgical Treatment of Cardiovascular Diseases," \textit{JAMA} Volume 251 (April 27, 1984): 2140.

By this time, Murray had been experimenting with various vascular and cardiac techniques for years in his small lab at the Banting Institute. Routinely, he began his day at the Banting Institute, operating on animals, before walking across the street to the Toronto General Hospital to see patients. Various junior assistants and medical students monitored the recovery of the lab animals thereafter, recorded changes, and then reported back to Murray when they met again the following day. Murray had been following the laboratory and clinical work of Gross and Crafoord, and was duplicating their procedures on dogs. When he read about the new Blalock-Taussig operation, Murray, like so many other surgeons, traveled to Baltimore to observe and learn. He liked and respected Blalock who was a modest, charming and creative man. According to Murray, Blalock "had the type of mind which led him to explore many fields of medicine and surgery. He was an experimenter of the first water, one of the leaders in this type of work in the United States ... He had a very pleasant and amiable personality, a kindly and gentle face ... Everybody was his friend and he was a friend of everybody." Murray was impressed with what he saw Blalock do, and he hurried home to practise the Blalock-Taussig procedure on animals. Within only months after returning from Baltimore, Murray began performing these operations clinically.

In March 1946, Murray was presented with his first blue baby patient -- 13-year old Isabel Douglas of Toronto. Her lips, fingers and toes were quite blue and

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31On March 11, Toronto newspapers announced Murray's operation on Isabel Douglas as the first blue baby operation in Canada. This was incorrect. The next day, a small, three paragraph article appeared in the Toronto Daily Star stating that Dudley Ross, surgeon-in-chief of the Children's Memorial Hospital in Montreal had in fact performed the first blue baby operation (to correct Tetralogy
doctors had told her family that she would not live much beyond her early teens. The Douglas family had read about the new Blalock-Taussig heart operation in the newspaper. Another local blue baby, Gail Mitchell, had traveled to Baltimore to have the operation. Unfortunately Gail’s operation had been unsuccessful and the young girl had died. Nevertheless, the Douglas family wanted the same operation for Isabel, driven by the possibility of restoring their daughter’s health and extending her life. They asked their family doctor about going to Baltimore for the procedure but their doctor recommended Gordon Murray. He knew of Murray’s success on animals and he felt that the operation could be done just as well in Toronto as in Baltimore.

Murray outlined the risks of the new corrective heart procedure to the Douglas family, and they agreed to proceed with Isabel’s operation, in secret, at the Wellesley Hospital. It was his first blue baby operation, and he wanted to avoid attention from his colleagues and the press at this point. He may also have been concerned about the Toronto General Hospital’s policy of referring children to the nearby Hospital for Sick Children. More likely, he wanted to thumb his nose at his Toronto General Hospital seniors with whom he was increasingly battling for more financial and professional support of his work, and the tension between Murray and the hospital increased. At his first blue baby operation, Murray spent four hours operating.

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of Fallot) in Canada in June 1945. At the time of the article, Ross had operated on three blue babies, of which only one survived with “just fair” results. See “Blue Baby Operation in 1945, Doctor Says,” Toronto Daily Star March 12, 1946. By October 1949, Ross had performed 60 blue baby operations, substantially less than Murray, with a higher mortality rate between 15 and 20 per cent. McGill University Archives, RG 38 Faculty of Medicine, Container 17, File 233, Scrapbook, page 241 — “Blue Baby Feels ‘Wonderful’ Doctors Meeting Here Are Told,” [unknown newspaper], 7 October 1949. Murray may not have been the first Canadian surgeon to perform the operation, but he had the greatest volume, success and notoriety in regards to blue baby surgery.

32 Murray wanted privacy. At various times when he was operating, he sometimes booked his operations as routine procedures instead of heart surgery to discourage visitors. He sometimes covered windows with towels to prevent casual observers from watching. Hannah Institute for the History of Medicine Oral History Collection, Volume 46, Dr. D.R. Wilson, page 33; Miss Jean Dodds, Interview with author, 9 April 1996, Toronto.
carefully re-routing the blood stream and building new passages to the lungs to improve Isabel's blood circulation. When he finished the operation, all felt that a miracle had been performed. When the clamps were released, blood rushed through Isabel's heart and lungs. Her colour turned from blue to pink before everyone's eyes. It was an exciting climax to what all believed to be a successful operation. Five hours after leaving the operating room, Isabel suffered a relapse and died almost instantly. An autopsy attributed Isabel's death to cerebral thrombosis -- a blood clot on the brain.33 An upset Murray returned to the lab.34

Three months later, Murray was presented with his second blue baby case. Five-year-old Viola Ireland of Burks Falls, Ontario wanted "a new heart so that she could go to school like the other little girls."35 Viola's parents had been saving money to send their daughter to Baltimore for the blue baby procedure. Instead they decided to ask Gordon Murray to perform the operation. Murray agreed and this time performed the operation at the Toronto General Hospital. After spending two hours in the operating room, Viola's colour changed from blue to pink and the hopes of leading a normal, healthy future were fulfilled. The child's family praised Murray and his surgical skill. The operation was a glorious success and newspaper journalists

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33 Did the operation cause the cerebral thrombosis? Even by today's standards, no one can be certain, however surgery can contribute to the dislodging and/or moving of a clot, and if this clot obstructs pulmonary or cerebral vessels, the patient can die. "Death Follows Canada's First 'Blue' Operation," Toronto Globe and Mail 11 March 1946, page 1 and 2; "Blue Baby," Toronto Globe and Mail 11 March 1946; "Blue Baby's Father Hopes Others Won't Be Deterred," Toronto Star 11 March 1946; "Toronto 'Blue Baby' Dies Post-Operation Relapse Defeats Surgical Success," Toronto Telegram (Evening) 11 March 1946.

34 Miss Jean Dodds, OR nurse for Murray, remembers how upset Murray got when a patient died in the operating room. He found it difficult to see the family when he lost the patient. Jean Dodds, Interview by author, 9 April 1996.

35 "Blue Baby No Longer -- She Asked For Mother," Toronto Star 3 July 1946.
announced Viola as the "first baby to be rescued in Canada from the fate of a 'blue baby'".  

The Toronto General Hospital designated six beds specifically for Murray's blue baby cases. The nurses, notably those caring for the blue babies, were extremely loyal to Murray. He acknowledged their important nursing role "ever in attendance to supervise and provide the oxygen and other treatments necessary to comfort and console the children." Dr. Stephen Evelyn was the anaesthetist for most blue baby operations, whether at the Wellesley or the Toronto General Hospital, for Murray was reluctant to work with anyone else.

But the Blalock-Taussig operation and other modified shunt procedures were not cures or even long-lasting palliative solutions for these children with congenital heart disease. These shunts only partially corrected the problem and blue babies often needed a second operation. This fact stimulated surgeons to find other ways to

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37 This was unusual, since all children were to be operated on at the Hospital of Sick Children next to the Toronto General Hospital. Soon after Murray began performing blue baby operations, W.T. Mustard did also at HSC, though not equal to the number done by Murray. By the 1950s, Murray's "blue baby" beds were removed from TGH and Murray concentrated on performing heart operations on adults, notable acquired heart disease conditions. Mustard then emerged as one of the "blue baby" specialists in Toronto in the 1950s and 1960s. Mustard performed the first successful open-heart blue baby surgery in Canada, correcting a septal defect. By 1958, Mustard was performing closed heart blue baby operations two to three times a week, and open heart blue baby surgery once a week. In 1963, Mustard devised an open-heart procedure for transposition of the great vessels, a congenital heart disease which after tetralogy of fallot, is the second most common blue baby condition. See Marilyn Dunlop, *Bill Mustard: Surgical Pioneer* (Toronto: Hannah Institute and Dundurn Press, 1989), p.45-48, 65-68; Thomas Fisher Library, University of Toronto, Academy of Medicine Collection, Biographical Files, 1235 W.T. Mustard.


repair heart malformations, specifically how to correct these abnormalities directly rather than re-routing blood to compensate for them. One of the first heart malformations that surgeons attempted to correct were septal defects.

The septum divides the two atriums and the two ventricles of the heart, and in some cases, children are born with "a hole in the heart" where the septum has not closed completely. The hole in the septum allows blood to pass improperly between the atria and the ventricles. According to Murray, almost half of his blue baby cases had been diagnosed with this heart malformation -- patent interventricular or interatrial septa. How could he suture close these holes in the heart? At this time, one could not open the heart to suture the edges together or to graft a patch. Murray set to work to develop a "blind" procedure to correct this condition. With the blunt end of a needle, threaded with either a “living suture” (for example, a strip of fascia lata from the leg) or cotton thread, Murray entered the front of the heart and exited from the back, tying both ends in place. The inserted living suture or cotton thread narrowed the opening in the septum, partially blocking the hole. Thrombosis may have occurred around the suture and perhaps closed the opening more completely.

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41In the fetal heart, the foramen ovale serves as a flap between the two atria. This provides an opening, through the atrial septum, for blood to flow from the right atrium to the left atrium, bypassing the lungs. At birth, the foramen ovale spontaneously closes, sealing the atrial septum. In cases of holes in the ventricular septum, this is an embryonic malformation. Robert Carola, John P. Harley, Charles R. Noback, Human Anatomy and Physiology (Toronto: McGraw-Hill, Inc, Second Edition, 1992), p.632.


43When open-heart surgery arrived, this became the acceptable surgical treatment and produced better results due to improved accuracy of locating and completely closing the hole. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 31, File 40, Correspondence ---- to Murray, 1975.


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Murray was proposing a difficult operation — it was a tremendous technical challenge to define the exact location of the hole in the septum and to insert the material without producing arrest of the heart. Alone in his lab at the Banting Institute, sometimes allowing student assistance, he practised locating the position of the septum from the exterior surface of the heart. He tediously repeated inserting a needle through the septum, taking care not to puncture the coronary arteries, the valves or the bundle of His.\textsuperscript{45} Using this procedure, he then operated on four children suffering from either interatrial or interventricular septal defects. Three survived the surgery and they experienced moderate improvement.\textsuperscript{46} It was a surgical first.

Murray described the operation at the 1948 meeting of the American Surgical Association "to an electrified audience of the continent's greatest surgeons."\textsuperscript{47} Blalock commented, "Certainly it never would have occurred to me to place sutures blindly in this manner ... I wish to congratulate Dr. Gordon Murray on his excellent work."\textsuperscript{48}

Murray proved to be an innovative and skilled surgeon, but this operation for interatrial and interventricular septal defects was only one of many daring heart operations. He performed a range of procedures for different congenital heart cases including shunts, grafts, sutures, and vessel anastomoses. The number of blue baby

\textsuperscript{45}Coronary arteries carry blood to nourish the heart muscle. Valves restrict the flow of blood within the heart to one direction only. The bundle of His, or atrioventricular bundle, is a bundle of heart nerve fibers which transmit contraction waves from the atria, via the AV node, to the ventricles. Oxford Medical Dictionary, 4th edition (New York: Oxford University Press, 1994), p. 55-56, 151, 694.


\textsuperscript{47}Roy Greenaway, "Tells How Darning Needle Can Put Sutures in Heart," \textit{Toronto Star} 31 May 1948.

\textsuperscript{48}Gordon Murray, "Closure of Defects in Cardiac Septa," \textit{Annals of Surgery} Vol. 128 (October 1948), 853.
operations that he performed was second only to Blalock, but Murray reported a lower mortality rate. He lost fewer patients than almost any other surgeon operating on the heart. Dr. M.E.J. Stalker stated that, "I don't know of anyone else in Canada who could attempt such an operation. It takes not only great surgical skill but also great courage and an outstanding knowledge of anatomy. It is an operation in which many of the great blood vessels must be severed." A Toronto doctor, John Scott, said, "It was known that he was technically outstanding; and that he would try things and succeed where others would hesitate even to attempt it." Blalock was delighted with Murray’s success and congratulated him on his “creative approach to surgical science.” Leading surgeons came to Toronto to observe Murray perform these delicate operations. Medical and surgical students juggled their schedules to attend his operations and watch him in action. Murray became an expert in blue baby surgery, in large part due to his skill as a surgeon and also the sheer number of operations he was performing successfully each week.

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49 In 1948, Murray reported a mortality rate of 11.3 per cent in a series of 62 blue baby operations. Blalock’s mortality rate was 17 per cent in a series of over 200 blue baby operations. It was an unfair comparison but one of the newspapers picked up on it in order to glorify Murray’s success in Toronto. See “Over 200 Blue Babies Saved in Two Years of Operations,” Toronto Telegram (Evening) 29 November 1946; Alex Henderson, “Success in 90 p.c. in Dr. Murray’s 190 Blue Babies,” Toronto Star 9 February 1949; Gordon Murray, “The Tetralogy of Fallot and Its Surgical Treatment,” British Medical Journal Vol. II (December 6, 1947): 907-8; Gordon Murray, “Surgical Treatment of Congenital Heart Disease (Tetralogy of Fallot), Canadian Medical Association Journal Vol. 58 (January 1948): 11-12.

50 “Surgeon Here Wins Acclaim of Continent,” Toronto Telegram (Evening) 29 October 1946.


52 According to James Maloney, Jr., a surgeon close to Blalock, Blalock “considered Gordon Murray a close personal friend and had very great admiration for Dr. Murray’s creative approach to surgical science.” Personal correspondence, Dr. James V. Maloney, Jr. to author, 12 December 1996; National Archives of Canada, MG 30, B 110, D.W.G. Murray Papers, Vol. 28, File 25, Correspondence Alfred Blalock to D.W.G. Murray, 3 October 1946.

As a medical man and a parent, Murray was sympathetic to the plight of these cyanotic children. (At the age of 42, he had become a father when his wife Helen gave birth to their only child, daughter Rosalind, in 1936.) Murray wrote, “[I am] greatly disturbed with the situation arising in the family with a ‘blue baby’ ... the child so afflicted was so delicate, required so much care and attention and aroused so much sympathy, anxiety, disappointment and fear for the future, in the mother particularly but also in the father, that the other children very often had to stand aside and receive indifferent treatment ... there was an attitude of despair, but very often a saintly resignation and self-sacrifice.”

He knew he could improve their health through surgery, and families willingly surrendered their children to the able surgeon. They liked Murray's confident and compassionate manner and trusted him implicitly.

In the five-year period between 1946 and 1951, Murray performed almost 600 heart operations. He did few other procedures, often performing two blue baby operations a day. He operated on infants, children and adults, ranging from ten months to 43 years in age. He kept a demanding schedule, reluctant to turn away patients, and received great satisfaction in transforming these sick children. Often he stood quietly outside his blue baby ward, watching his young post-operative patients playing and laughing.

Murray could and did demand respect at the Toronto General Hospital. Senior medical men did capitulate and grant him beds in his ward for his blue baby cases. They were not so accommodating however to Murray’s other expectations. He viewed the hospital and university’s lack of recognition of his skills and lack of


enthusiasm to endorse a new cardio-vascular surgical centre as personal affronts. Murray, with his experience and successes, thought he could establish Toronto as the leading medical institution in this field. According to him, he was one of the university's medical leaders as proven by his heparin and thrombosis research, his vascular surgery and now his heart operations. Why was the University of Toronto so reluctant to showcase his skills and contributions, he wondered. Murray became increasingly frustrated by what he perceived to be institutional obstacles and personal jealousies against his continued success. Moreover, he began to look over his shoulder at the new lot of surgeons entering the foray. In the immediate post-war period, younger surgeons who had been removed from the local scene for four years returned from Europe with tremendous surgical experience and ambition. It was a time of great changes for the Department of Surgery.

If Murray was suspicious and unsure of his status within the hospital and university, he could take solace in the media’s celebration of his work. His cardiac surgery generated much more media attention than his heparin and thrombosis work. His previous contributions were relatively unknown to the general public, but his heart operations, splashed across the front pages of major Canadian newspapers, made him a very famous and well-known doctor to Torontonians. In Toronto, medical reporters Ken MacTaggart, Joan Hollobon, Marilyn Dunlop, David Spurgeon and Ron Kenyon followed Murray's success and began referring to him as "Canada's Blue Baby


57 I refer almost exclusively to newspaper coverage of Murray's heart operations at this time. Radio also highlighted his success. For example, Larry Henderson on CJAD 800 in Montreal started a series "Canadians Who Head the Field," for which Murray, the "Blue Baby Doctor", was selected. National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 48, File 5 - Clippings: "New Series," Montreal Monitor 26 October 1950.
Everyone enjoyed a good "medical success" story, and journalists loved to report the sensational results of medical progress. Newspaper photos of Murray and his patients became common as reporters dramatically described his numerous blue baby cases. The headlines were glorious: "Gladys Hie - Dr Murray's Third Triumph", "Blue Baby' For 16 Years -- Doctor's Skill Frees Girl From Wheel-Chair Prison", "Surgeon Plays Santa Claus", "'Blue Baby' Home for Yule -- Murray's 20th Success", "Famed Blue Baby Doctor Reaching One-a-Day Rate", "Once 'Blue', Now Normal".

His operations delivered miracles; blue babies suffering from congenital heart disorders, facing certain death, became pink, robust children.

Murray was a hero, according to the media. Public discourse was flattering and congratulatory. Murray, the divine doctor, made miracles happen and could cheat death by his skill in the operating room. He saved lives, and what was more selfless than saving the life of a child? He corrected malformations dealt by nature, and offered a child a normal life. Even when he lost a patient, the media sympathized with the surgeon who had done all that he could do. To the press, Murray was humble about his abilities, but he was delighted by their glowing words. "Two-Year-Old

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58 Medical reporters had recently emerged, the beginning of specialized journalism, and they strove to present more correct and detailed information to the public. Good medical reports helped to keep the public abreast of medical change, and the readership enjoyed a good "medical success" story. There was a definite increase in the number of medical stories reported in the newspapers after World War II, this being directly related to the emerging specialist reporter and rapid medical changes that brought new procedures and cures. (The most notable case of the 1950s being the polio epidemics and development of a vaccine.) Doctors and hospital administrators, initially reserved, soon began to enjoy the publicity and the celebrity that glowing news reports created. They came to use reporters to generate publicity for fundraising purposes. Often medical reporters had a rapport with various doctors, and once credibility was established, these doctors opened up to reporters. Marilyn Dunlop, Interview by author, 18 January 1997; Joan Hollobon, Interview by author, 18 January 1997; Ron Kenyon, Interview by author, 19 January 1997; David Spurgeon, Interview by author, 18 January 1997.

59 "Gladys Hie, 5, "Blue Baby" - Dr. Murray's Third Triumph," Toronto Star 3 August, 1946; "Blue Baby' For 16 Years - Doctor's Skill Frees Girl From Wheel-Chair Prison," Toronto Telegram (Evening) 8 October 1946; "Surgeon Plays Santa Claus," Hamilton Spectator 27 November 1946; "Blue Baby' Home For Yule - Murray's 20th Success," Toronto Star 26 December 1946; "Famed Blue Baby Doctor
Patty Reniewick Transformed Into Healthy Child By Skill of Dr. Gordon Murray", wrote one journalist, "the operation was a surgical miracle" and "Dr Murray is leading the way." The media praised Murray who was "operating on a blue baby almost every other day, bringing new life and hope to them and to their parents." In the past, the media had dutifully reported Murray's earlier contributions -- his heparin research with Best, his Hunterian Lectureship, his vascular procedures -- but this time they glorified his successes. His blue baby operations contained all those sensational elements sought by the press — life, death, drama, children, heroism, and celebration. Murray's media personae as created by journalists — hero, leader, and miracle-maker -- remained with him until the end of his career. He continued to work with journalists at different times throughout his career, and they deferentially reported his work with praise and admiration.

Blue baby newspaper coverage announced the expanding surgical possibilities now available. The public's previous fears and anxieties about heart surgery subsided with the increasing number of favourable reports. Moved by happy endings, the public celebrated blue baby cases and the daring surgeon who performed the operations. They were not concerned about which procedure Murray used or if he

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60 "Hamilton Blue Baby Healed By Daring Heart Operation," Hamilton Spectator 27 November 1946.

61 "19, Was 'Blue Baby', She Can Sing Again," Toronto Star 13 January 1948.

62 One good example is June Callwood's "A Day In The Operating Room", Maclean's Magazine July 15, 1953, pages 8-10, 54. She reports on the nurses, doctors, surgeons, families and patients at the Hospital of Sick Children, focusing on the blue baby operation performed by W.T. Mustard. It represents the celebratory tone journalists took towards medical advancements and the men and women responsible, as well as an attempt to inform the public on these great operations.
had been the first to perform these operations or not. They celebrated his success at saving the lives of children.

Families and community clubs flew their blue babies to Toronto from all across Canada. Murray operated on children from Calgary, Edmonton, Winnipeg, northern Ontario, Montreal, Halifax, and from as far as New Zealand. These were desperate parents who placed their hope and faith in a surgeon able to improve, if not save, the lives of their children — and they were astonished at the results. Mrs. Psutka, describing her five-year old son, said, "His lips, tongue and even finger nails were as blue as blueberries before the operation, and now his color is just like that of other healthy children." Mrs. Russell, mother of 9-year old Vivian, said that Vivian "had to be pulled to school last year ... in her cart. Now she can walk like other girls and not be as she was, so short of breath that she could not walk more than half a block with great difficulty." Mrs. Ireland, mother of the first transformed blue baby, praised Murray as "a wonderful man and a wonderful surgeon." Mrs. Werezak, mother of second blue baby Mary, commented that "Dr Murray is a wonderful man. He says our


64Roy Greenaway, "U.S. Surgeons Come To See Toronto-Cured 'Blue Babies'" , Toronto Star 16 September 1946.

65"Blue Baby No Longer -- She Asked For Mother," Toronto Star 3 July 1946; "Toronto Doctor Gives Blue Baby 'New Heart'", Toronto Globe and Mail 4 July 1946.
little girl will now be normal again. It seems like a dream."\textsuperscript{66} Indeed Murray delivered miracles. Enjoying his son's improvement, one man wrote, it was "something we never really expected ... death was so close."\textsuperscript{67} Another father wrote, "when I see my little son playing with an energy he never knew before, a layman like myself feels that the medical profession is a godly one and that men like yourself are the glories of it."\textsuperscript{68} There were numerous references by parents to the "godly" powers of Murray. "The work and type of work that you are doing daily is not of this world but of the supernatural ... we fully realize that with men like you to watch over us and our loved ones that our future is safe and our hopes and prayers will be answered."\textsuperscript{69} Parents were extremely grateful to the surgeon who saved the lives of their children, and the public's praise was unending.

Murray also received praise and admiration from the medical profession for his heart surgery success. He had earned professional honours before for his work on heparin. Once again, during these heady days of early cardiac surgery, Murray stood among the leaders in the field. He described various cardiac procedures at the Canadian Medical Association and the Royal College of Physicians and Surgeons of Canada in 1947, the American Surgical Association in 1948, and the Royal College of Surgeons of England as the Lord Moynihan Lecturer in 1949.\textsuperscript{70} All were important

\begin{footnotes}
\item[66]"Doll Mary's Only Concern — 'Blue Baby' Is Now Pink," \textit{Toronto Star} 22 July 1946.
\item[69]National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 31, File 13, Correspondence ---- to Murray, 1951.
\end{footnotes}
venues to which his colleagues flocked to learn of Murray's successes. He basked in this glory.

Professionally, Murray was known worldwide for his contributions. Dr. Donald R. Wilson, his student and later Professor of Surgery at the University of Toronto, commented that "at one stage he was the doctor that was best known in the world from Toronto ... it was really the opening up of cardiovascular surgery and here was a man who was a leader in the field. It was a very stimulating time to be around." Denton Cooley, a Texan and student of Blalock's, remembered, "he [Murray] did some outstanding and unusual surgical operations and was considered one of the most progressive researchers of that era." The surgical profession recognized Murray's leadership in the field by electing him into the prestigious American Surgical Association, which even today has only 260 active members and a handful of Canadians. American surgeon William Longmire, Jr., a student of Blalock's, argued: "From the standpoint of innovations and ideas [Murray] was indeed a leader. But from the standpoint of having his ideas generally accepted and applied at or about the time of presentation, my feeling [was] that he was too much ahead of his time to be considered a leader in the clinical field of surgery." Some of Murray's earlier ideas about heart surgery only took hold after World War II when younger surgeons began working on the heart in increased numbers.

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71 Hannah Institute for the History of Medicine Oral History Collection, Volume 46, Dr. D.R. Wilson, page 36-37.

72 Personal correspondence, Denton A. Cooley to author, 27 November 1996.


74 Personal correspondence, William P. Longmire, Jr. to author, 8 December 1996.
By the late 1940s and increasingly into the 1950s, there was no doubt that heart surgery was the new and exciting field in which to be. Successful blue baby operations gave way to surgical procedures for acquired heart disease conditions, most notably valve dilatations and repairs. Earlier, in 1938, Murray had received little recognition for his work on stenosis and incompetence of valves. In fact cardiologists refused to refer such patients to him. Ten years later, the medical profession celebrated the heart valve operations of British surgeon Russell Brock and American surgeons Horace Smithy, Charles Bailey and Dwight Harken who received joint credit for the first successful pulmonary and mitral valve commissurotomies. Murray was not convinced of the value of the new mitral valve commissurotomy procedure. Returning to his earlier work on valve resection and replacement, he continued to argue that regurgitation, a condition often worse than stenosis, occurred after either division or resection of the hardened valve. In 1949, Murray operated on ten patients, with satisfactory results in eight, relieving their stenosis but also suturing a tendon sling valve beyond the mitral valve to prevent regurgitation.

During the course of his valve surgery, Murray also devised an improved cardioscope. This instrument was to provide the surgeon with direct vision of the

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75 Commenting on the early blue baby operations of 1938, '44 and '45, Bill Mustard stated, this was "the golden era of heart surgery in which I was fortunate to be in at the beginning. So that's how I became a cardiac surgeon. I just fell in love with it, that's all. It was so exciting and the time was so exciting ..." Hannah Institute for the History of Medicine Oral History Collection, Volume 33, Dr. W.T. Mustard, page 73-74.

76 Bailey, Harken and Brock are credited with the first, second and third modern operation inside the heart to relieve mitral stenosis. The mitral valve, which channels the blood from the left atrium and into the left ventricle, is a bi-cuspid valve which can harden close. The surgeon splits the valve open. Lael Wertenbaker, To Mend the Heart (New York: The Viking Press, 1980): 4.

operating field. It was not a new device. It had been used first in 1913 and later in the 1920s by those surgeons who at the time were unsuccessfully operating inside the heart. Murray's cardioscope was a hollow tube with a glass window at the end. It was a 'home-made affair', and to those who inquired about purchasing one, Murray instructed them to make one themselves. According to Murray, "with this, one can inspect the interior of the heart chamber entered; can identify the opening of the mitral valve, and the site for resection can be selected. It can be inspected again following the resection to see if an adequate opening had been made. Then when the new valve is about to be placed, its position can be checked through the cardioscope to make sure it is in the desired position." By the end of 1950, Murray had done 28 mitral valve operations, "with some very good results, others are fair only and a mortality rate of about twenty per cent."

Murray continued to experiment with different procedures for diseased valves. In 1954, American Charles Hufnagel had reported his success in reducing the effect of a leaking aortic valve by placing a mechanical valve in the descending thoracic

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78 Bill Mustard remembers, "I couldn't see a damn thing but he would put in an instrument [cardioscope] and take out a piece of the mitral valve ... he was a way ahead of his time." Hannah Institute for the History of Medicine Oral History Collection, Volume 33, Dr. W.T. Mustard, page 133.


82 National Archives of Canada, MG 30, B 110, D.W.G. Murray Papers, Vol. 42, File 47, Correspondence D.W.G. Murray to D. Hickey, 3 November 1950. Compare this with the fact that in 1990, a 2 per cent mortality rate was considered "excessive" for coronary bypass and most other heart operations. Personal interviews by author.
aorta. His mechanical valve consisted of a lucite cage and a silastic ball, and was implanted in a small series of patients.\textsuperscript{83} Note that Hufnagel inserted the ball valve in the descending aorta, rather than in the exact anatomical position, the aortic ring. It provided only a partial and temporary relief of valve incompetency. Moreover, this first prosthetic valve was noisy as the opening and closing of the valve produced an audible clicking noise.\textsuperscript{84} Surgeons were also having difficulties attaching the rigid metal cage within the delicate aorta. Consequently, Hufnagel had no long-term survivors because the mechanical valve refused to remain in place permanently.

Surgeon Dr. Ronald J. Baird remembers, "In the spring of 1955, as a junior intern on Ward C, I recall watching a patient bleed to death within a minute or two when one of these valves dehisced."\textsuperscript{85}

Dissatisfied with the mechanical valve, Murray began experimenting with homograft (from cadavers) valves in his lab. He transplanted homograft aortic valves into the descending aortas or mitral valve rings of dogs and later patients, successfully replacing diseased valves and eliminating problems of obstruction and regurgitation. He performed his first clinical operation in secret in 1955. Murray's homologous valve, placed in the descending aorta, appeared to overcome the complications incurred with Hufnagel's valve. This was the first evidence that a human heart valve could be successfully transplanted! His Toronto colleagues became aware of Murray's


\textsuperscript{85}Taken from "The Development of Cardiac and Vascular Surgery in Canada — Personal and Anecdotal Reflections on an Era," Ronald J. Baird, Private Collection.
procedure only when he published his results in 1956 in an obscure Italian medical journal, *Angiology.*

Murray's operation started a debate within the profession about the merits of human tissue versus mechanical heart valves. Surgeons became more adept at using the Hufnagel valve, and improved mechanical valves soon became commercially available. The debate continues today: American practitioners tend to favour mechanical valves while British and Canadian surgeons prefer tissue replacements.

While Murray was experimenting with valve transplant experiments, he was also investigating coronary artery disease. A growing number of people were suffering from coronary heart disease by the 1950s, and surgeons applied their new vascular surgical techniques to this previously exclusive medical problem. J.B. Herrick had identified the relativity between angina and obstruction in coronary heart disease forty years earlier, but little had been offered in the way of treatment. At the base of the heart, two coronary arteries emerge from the narrow pedicle to nourish the wall of the heart with blood. Arteriosclerosis causes these arteries to narrow and eventually obstructs the passage of blood. Without this supply, the muscle weakens,

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87 Both Hufnagel's artificial valve and Murray's homograft valve only partially relieved aortic valve insufficiency. In 1962, the sub-coronary Starr-Edwards ball-valve became available; it provided complete correction of aortic insufficiency. In 1964, Bigelow et al. reported on improved long-term functioning of homograft valves when positioned in ascending aorta. This article also addresses the debate of which is better: artificial or natural valves. "We believe that a properly positioned aortic valve homograft has a better chance of long-term function, but the operative risk and the incidence of insufficiency may be higher." W.G. Bigelow, J.K. Yao, H.E. Aldridge, R.O. Heimbecker and G.D.W. Murray, "Clinical Homograft Valve Transplantation," *Journal of Thoracic and Cardiovascular Surgery* Volume 48 (September 1964): 342; R.J. Baird, "The Development of Cardiac and Vascular Surgery in Canada -- Personal and Anecdotal Reflections on an Era," Private Collection.

loses it energy, causes chest pain, and eventually infarcts. Surgeons experimented with ways to correct the narrowed or occluded coronary arteries, and to re-route and attach supplementary arteries to the wall of the heart as a third source of supplying blood to the muscle.

It was natural that Murray, increasingly applying his vascular techniques to other heart problems, also experimented with surgical procedures to correct coronary artery disease. In fact, he had experimented with other possible sources of blood to the heart muscle as early as 1937. He had tried to graft the internal mammary artery into the cardiac muscle as a third source and thus improve coronary circulation. Murray was unsuccessful, but other surgeons continued on, notably Arthur Vineberg in Montreal. By the early 1950s, Vineberg had successfully transplanted the internal mammary artery into the left ventricle wall, linking up with the vascular supply of the coronary artery. Upon learning this, Murray returned to his earlier coronary artery work. He was able to repeat Vineberg’s success, and then operated on three patients suffering from severe angina pectoris. Murray wrote, "One patient was having about twenty-two attacks a day, taking about that number of nitroglycerin tablets to be relieved of pain. He could not get out of bed nor take a few steps, nor move out of a chair without precipitating an attack. Following the operation, the patient’s symptoms were relieved in short order and within a few months he was able to walk about a mile without discomfort. Another patient was having about twelve coronary anginal

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attacks a day, which following the operation were relieved and the patient was able to go about without discomfort. In a third [case], a man who was disabled returned to work as a machinist."

Murray also tried to repair narrowed or occluded coronary arteries. During the 1940s heyday of vessel resection, grafting and repair, Murray applied these techniques to coronary arteries but was unsuccessful. The blood supply was interrupted for too long, and the heart muscle died. A decade later, Murray announced an operation that overcame this problem. He described a procedure by which the distal end of the coronary artery was perfused by way of a plastic tube placed through the branch of the divided internal mammary artery, subclavian or other artery, and thus continued the essential supply of blood. The surgeon repaired the damaged coronary artery and then sutured either end-to-side or end-to-end to the distal segment of the coronary. No local death of muscle or infarct occurred. It was a technically difficult procedure, and Murray lost many of his first lab dogs. In the end, five of the seventeen dogs survived this procedure of anastomosis of a systemic artery to the coronary. There were no clinical cases.

When Murray shifted his attention from blue baby operations to acquired heart disease procedures, the media devotedly reported his surgical success. Murray's mitral valve procedure, which had received little attention in 1938, was seen as "amazing" in 1950 -- "Amazing Operation Eases Heart Condition - Local Woman Benefits From

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Rare Surgery". Roy Greenaway of the Toronto Star interviewed Dr. Francis O'Donnell, a Buffalo heart specialist, who said, "I don't think many surgeons in the United States know how far advanced Dr. Murray is in heart surgery. I'd say he is 10 years ahead of us. He is the only man I know of who is doing actual cardiac surgery." When Murray had success with his experimental coronary artery surgery, the newspapers reported how "one man getting the spare [artery] had been having dozens of painful angina attacks daily, at the slightest exertion. For three years now he's been able to walk a mile daily." Media coverage such as this continued to celebrate Murray as a leader in the field. All this attention and affirmation reinforced Murray's commitment to clinical research and to his lone practices of solving medical problems.

His list of heart investigations and procedures covered an impressive range of corrective cardiac operations. He performed numerous procedures for congenital heart malformations including patent ductus arteriosus, coarctations of the aorta, Tetralogy of Fallot, interatrial and interventricular septal defects. He fixed damaged valves and coronary arteries. If someone needed an operation for a heart condition, Gordon Murray was the man for the job! Unknowingly, Murray was at the height of his surgical career. He had pioneered important heart operations: the closure of holes

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94 Roy Greenaway, "Renews Valve in Heart — Murray 10 Years Ahead — U.S. Specialist Admits," Toronto Star 1 February 1950.

95 "Detouring of Arteries to Supply Ailing HeartOutlined by Toronto MD," Toronto Globe and Mail 4 September 1952; "Blow-Out Patches For Heart, Spare Arteries Described by Doctor," Toronto Star 4 September 1952.

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in the heart, the repair of heart valves and the relief of coronary artery disease. Moreover, the volume and success rates of his operations made him one of the most experienced and well-known cardiac surgeons in North America.

The world in which Murray functioned however was about to change and he would find it difficult to adjust. In the mid-1950s, the field of heart surgery underwent an enormous change. Technological innovations began to change dramatically how surgeons operated on the heart. This was the beginning of open-heart surgery. Surgeons were now able to re-route blood safely away from the heart and lungs, to open the chest and expose a quiet, bloodless heart. Now under direct vision, they began to perform more complex, corrective cardiac operations with greater success.

Toronto surgeon Dr. Wilfred G. Bigelow introduced hypothermia, a surgical technique involving total body cooling, to a skeptical audience at the American Surgical Association in 1950. Bigelow's idea was to "cool the whole body, reduce the oxygen requirements, interrupt the circulation, and open the heart." In 1952, John Lewis successfully operated on a blue baby, suffering from atrial septal defect,

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96 Murray as pioneer of these three operations -- closure of holes in the heart, repair of heart valves, relief of coronary artery disease -- is an evaluation given by one of Murray's junior colleague, Bill Bigelow, and taken directly from W.G. Bigelow, Cold Hearts: The Story of Hypothermia and the Pacemaker in Heart Surgery (Toronto: McClelland and Stewart, 1984), p.114.

97 Being well-known within the medical community, through medical and public press, generated a great number of referrals for Murray. Consider this doctor's comments on DeBakey and his well-known heart centre in Houston: "Pretty soon the guy with the aneurysm in New York would ask his doctor, 'Who's the best surgeon in the field?' and the doctor replies, 'I don't know who's the best but the guy who's done the most is Mike DeBakey.'" Thomas Thompson, Hearts (New York: McCall Publishing Company, 1971), p.48.

98 Bigelow remembers his colleagues' scepticism, he wrote, "It was a blasphemy. This concept completely contradicted currently accepted teaching, which was to avoid any fall in body temperature." W.G. Bigelow, Cold Hearts: The Story of Hypothermia and the Pacemaker in Heart Surgery (Toronto: McClelland and Stewart, 1984), p.51.
using the open-heart hypothermia technique in Minneapolis. This technique allowed surgeons to temporarily cut off blood circulation to a beating heart for eight minutes, providing a bloodless field and direct vision in which to correct heart anomalies. However, this eight-minute window, in which the heart could be stopped without affecting the brain, limited the surgeon to simple cardiac operations.

Several other investigators were experimenting with methods of extracorporeal circulation that would extend the operating time of the surgeon. Most promising were the heart-lung machines being built by Clarence Crafoord in Sweden, Jongbloed in Holland and John Gibbon Jr. in the United States. It was a steep technological challenge to remove the blood from the body, oxygenate it and return it without damaging its properties. Tubes were inserted in the inferior and superior vena cavae, re-directing oxygen-poor blood going into the heart to the machine. The

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101 This technique became a world standard for the next four to five years. Personal interviews by author.

102 Biological oxygenators, not mentioned, were experimented with in addition to mechanical oxygenators or heart-lung machines. In the early 1950s, W.T. Mustard at the Hospital for Sick Children used a Cowan perfusion pump and a monkey lung as an oxygenator in cardiac bypass operations to correct transposition of the great vessels (a complex congenital heart disorder). There were obvious problems with using animals this way, notably deterioration of lung function and pulmonary congestion during the operation. See Stephen L. Johnson, The History of Cardiac Surgery 1896-1955 (Baltimore: Johns Hopkins Press, 1970), p.155; Marilyn Dunlop, Bill Mustard: Surgical Pioneer (Toronto: Hannah Institute and Dundurn Press, 1989); "Monkey's, Infant's Lungs Switched," Toronto Globe and Mail [April 1953], Ken MacTaggart, "Monkey's Lungs Used To Help Save Babies," Toronto Globe and Mail 10 June [1955], "Toronto Heart-Lung Machine Shown U.S. Doctors," [n.d.], "Unique Method Saves Child's Life in Operation at Children's Hospital," [n.d.] — newspaper articles found at Thomas Fisher Library, University of Toronto, Academy of Medicine Collection, Biographical Files, 1235 W.T. Mustard.

103 Problems of filming and bubbling of the blood, the two main principles of the design of the heart-lung machine of oxygenation, were continually being improved. Robert G. Richardson, The Scalpel and the Heart (New York: Charles Scribner's Sons, 1970), p.221-4.
machine then pumped the blood to an oxygenator, replicating the functions of the lungs by removing carbon dioxide and adding oxygen. The blood was then pumped through a filter to remove clots and bubbles before returning to the patient via a tube inserted in the aorta. In 1953, Gibbon successfully operated on a blue baby, suffering from atrial septal defect, using his heart-lung machine. Improvements to the pump needed to be made, but Gibbon had shown that it was possible. It took several more years to refine the machine, and to encourage other surgeons to use the pump.

By the late 1950s, heart surgeons almost everywhere were performing cardiac bypass, combining hypothermia with extracorporeal circulation. Blood was removed from the heart, and ventricular fibrillation induced to allow a "quiet state of the heart." A deeply cooled and bypassed heart could be stopped for up to an hour and started again without suffering damage. Heart operations became more numerous, complex and successful. By the late 1960s, surgeons were performing previously unthinkable coronary bypass and cardiac transplant operations. Michael DeBakey and Denton Cooley had established the Baylor College of Medicine in Houston as the best

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105 Crofords', Jongbloed's, and Gibbon's heart-lung machines all worked on the same operating principle but had different oxygenators that were refined and improved upon. John Kirklin at the Mayo Clinic modified the IBM-Gibbon machine and the inexpensive, easily-assembled DeWall bubble oxygenator (or Lillehei pump-oxygenator named after surgeon Walton Lillehei) became available after that. See Stephen L. Johnson, The History of Cardiac Surgery 1896-1955 (Baltimore: Johns Hopkins Press, 1970), p.145-158; University of Toronto Archives, A89-0030, Department of Surgery, Box 002, Department Chair Files, File 1, "Open Heart Surgery Performed by Extra-Corporeal Circulation Through Pump Oxygenators," written by Dr. R.O. Heimbecker, March 1957.

known cardiac surgical centre in the world, becoming as one journalist stated the
"Lourdes to heart-diseased pilgrims from everywhere."¹⁰⁷

Murray was not an open-heart surgeon. He never performed a heart operation
with a heart-lung machine nor used the technique of hypothermia. American cardiac
surgeon Henry Bahnson suggests that, "Murray probably did not become involved in
open heart surgery because he saw it as such a large field that he thought a younger
person should be involved. Dr. Blalock, born in 1899, was about the same age as
Murray and did not get involved in open heart surgery."¹⁰⁸ Murray was nearing 60
years of age, and many Toronto surgeons believed that he was stepping aside to let
younger men through.¹⁰⁹ According to Dr. Donald R. Wilson, "[Murray] said this is
as far as I am going and if I don't have to, I don't think I should get into that era. So he
never did an open pump case."¹¹⁰ Yet his Stockholm colleague and contemporary,
Clarence Crafoord, in his mid 50s at the time, made the leap, performing the second
successful open-heart operation with the heart-lung pump.¹¹¹

¹⁰⁷ Journalist Lael Wertenbaker wrote, "... for many years, Houston was Lourdes to heart-diseased
pilgrims from everywhere, and even Texas-sized pride was satisfied. High-profile surgeons DeBakey
and Cooley were considered to be "two of the most exhilarating and egotistical heart surgeons in
the world." They were competitive and strong personalities, a mentor and his protege who eventually
became enemies. Their success came from their volume of heart operations, performing in one week
the number of cardiac procedures other medical centres saw in a month. Their leading-edge reputations
were somewhat marred after unsuccessful heart transplant operations in the late 1960s, their previous
surgical success and experience driving these immodest surgeons to attempt the procedure before
immunosuppressive drugs became available to ward off rejection. See Thomas Thompson, Hearts
(New York: McCall Publishing Company, 1971); Lael Wertenbaker, To Mend the Heart (New York:

¹⁰⁸ Personal correspondence, Dr. Henry T. Bahnson to author, 22 November 1996.

¹⁰⁹ Dr. D.R. Wilson, Interview with author, 14 April 1997; Dr. Ron J. Baird, Interview with author, 2
April 1996.

¹¹⁰ Hannah Institute for the History of Medicine Oral History Collection, Volume 46, Dr. D.R.
Wilson, page 37.

¹¹¹ W.G. Bigelow, Cold Hearts: The Story of Hypothermia and the Pacemaker in Heart Surgery
contemporaries, Murray was experimenting and performing numerous heart procedures through these early years of open-heart, and he would have been excited about the possibilities of correcting more difficult heart conditions. He would not have considered himself either too old or too conservative to try new surgical techniques. More likely, his decision not to perform open-heart surgery was due to a combination of local events and changes.

After the war, there was a changing of the institutional guard within the Toronto university and hospital structure. Chief of Medicine, Duncan Graham, Chief of Surgery, W.E. Gallie, and Head of Ward C, Norman Shenstone all retired from the Faculty of Medicine. As dictated by seniority and the University of Toronto Board of Governors, Robert Janes became Chief of Surgery and Murray was promoted to Head of Ward C at the Toronto General Hospital and Associate Professor in the Department of Surgery at the University of Toronto. Returning from overseas, Dr. Wilfred G. Bigelow and later Dr Donald R. Wilson were added to the surgical staff of Ward C as junior surgeons with bright and promising careers ahead of them.112

Murray also became the Research Director of a private medical research laboratory, the W.P. Caven Memorial Research Foundation. He no longer conducted experiments at the Banting Institute, frustrated with the University of Toronto’s lack of research support and endless battling with university medical authorities for space, assistance, money and recognition for his projects. At the Caven Foundation, he continued his cardiovascular research as well as work on his artificial kidney machine, renal transplantation, fat embolism, and malignant tumour growth. Murray was well

112 Dr Joseph Sullivan, aware of departmental tensions, expressed his concern over Murray’s sudden resignation. “I hope you did it of your own accord and that is what you wanted,” he wrote Murray. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 28, File 32
aware of Bigelow's research on hypothermia at the Toronto General Hospital and Mustard's work with monkey lungs at the Hospital for Sick Children as techniques for open-heart surgery. Americans Lewis and Gibbon were reporting early open-heart surgery success using hypothermia and the heart-lung machine respectively.

After lengthy teaching trips to Brazil in 1950 and to Australia and New Zealand in 1951, Murray returned to work on Ward C at the Toronto General Hospital. Competition over cardiac cases, professional jealousy, and personality clashes brought things to a head on the ward. Dr. Wilfred G. Bigelow was the number-two man on Murray's surgical service; he had not been chosen by Murray and the two men did not share a close relationship. Like Murray, Bigelow was a skilled surgeon and devoted to clinical research, notably his hypothermia experiments. But he was a different man than Murray. Bigelow was not as antagonistic with colleagues, built good relationships with cardiologists in the Department of Medicine, and had more hospital allies. Murray was well aware of Bigelow's professional ambitions.

Correspondence Joseph A. Sullivan to Murray, 27 November 1953; Correspondence Murray to J.A. MacFarlane, 15 October 1953; Correspondence Sidney Smith to Murray, 27 November 1953.

Ron Baird suggests that Murray recognized Bigelow and Mustard as the new rising stars in Toronto. Ronald J. Baird, Interview with author, 2 April 1996.


Australian surgeon Rowan Nicks visited Murray in 1951 and 1957, on extended observation trips, during which he noticed the apparent tension between Murray and his colleagues. He wrote, "They [Bigelow and Mustard] appreciated his [Murray's] distinction and ideas but were pursuing their own fields and did not wish to be overshadowed. They were conscious of him and he was conscious of them ... It was not warm ... I think Gordon Murray inspired Bigelow and Mustard and that they were already growing their wings of independence at my first visit ... The environment at Toronto General during my visits were friendly to me but not particularly amongst themselves. There seemed to be a lot of feuding
By 1953, Bigelow was receiving more referrals from the Department of Medicine and performing more heart operations than Murray. According to Bigelow, Murray "saw the writing on the wall"; he was fed up with the TGH staff and administrators and simply retired early.\textsuperscript{116} According to Murray's daughter, the Professor of Surgery planned to direct heart cases away from general surgical wards to a specialized cardiac unit, rumoured to be directed by Bigelow, and thus away from Murray. A phone call from the Professor of Surgery to Murray at his home one Sunday afternoon confirmed this, and he abruptly retired.\textsuperscript{117} Murray felt that he had never received proper recognition from the Toronto establishment; he had been overlooked for Chief of Surgery in 1947, and now passed over for head of the new cardiac unit. Murray left Ward C in November 1953.\textsuperscript{118} His retirement from the university and the public ward service came as a surprise to most TGH staff, but as nurse Rita (Smith) Macnab remembered, "that was Murray -- he didn't tell people ahead of time what he was going to do."\textsuperscript{119}

No longer on the teaching staff at the Toronto General Hospital or the University of Toronto, Murray removed himself from the front line. (He did retain

\textsuperscript{116}W.G. Bigelow, Interview with author, 2 April 1996 and 17 April 1997, Toronto.

\textsuperscript{117}Murray's daughter remembers the phone call that Murray received at home from the Professor of Surgery one Sunday afternoon in 1953. Russell Brock was visiting Murray in his home when Murray was told, by telephone, that Bigelow would perform all cardiac procedures on the ward. Murray was stunned. It was a forced retirement. Rosalind Bradford, Interview with author, 10 July 1996.

\textsuperscript{118}Murray always considered himself a general surgeon with an interest in cardiac cases. He was not in favour of surgical specialization, probably due to pragmatic financial reasons of thus limiting your surgical caseload. When Murray left in 1953, Ward C was changed from a general surgical service to half cardiac and half orthopaedic cases. Bigelow became head of this service, and after much lobbying on the part of Bigelow, the new cardiac unit was realized in 1958.

\textsuperscript{119}Mrs. Reta (Smith) Macnab, Interview with author, 14 May 1996.
operating privileges, and continued performing both general and closed cardiac surgery in the Private Patients Pavilion.) If Murray had remained as head of Ward C, it would have been difficult for him not to adopt the open-heart techniques that were replacing closed-heart operations.\textsuperscript{120} He almost certainly would have battled Bigelow for control of the new cardiac unit. Despite leaving the hospital, he remained interested in the developments surrounding open-heart procedures, but was never quite convinced of Bigelow's hypothermia technique. He received updates from others on the use of extracorporeal circulation being used in early Toronto Hospital cardiac cases. One such source was a former resident of Murray's, who sent him a list of open heart operations to date, writing "I hope you'll be free to see a case soon. It is really remarkable to see a bloodless heart lying there motionless for 40-50 minutes, and then literally "snap" back into activity." On the bottom of this note, Murray scrawled: "pump mortalities -- 6 deaths in 19 cases."\textsuperscript{121} It was a time of advances and he was watching Toronto developments closely. At the Canadian Cardiovascular Society a decade later, Murray acknowledged his obsolescent technique: "a younger generation has taken over and, with modern equipment and plenty of money, are doing things bigger and better."\textsuperscript{122}

\textsuperscript{120}Murray's daughter states that he was not against open-heart surgery but saw it as the next advance or the next stage. He didn't leave the hospital because he wasn't ready for the new wave of open-heart surgery; he left because it was made impossible for him to continue to perform cardiac surgery. Rosalind Bradford, Interview by author, 10 July 1996.


\textsuperscript{122}Murray, invited by the Canadian Cardiovascular Society to give the 1963 Lecture, spoke on "Perspectives in Early Cardiac Surgery". No longer a leader in the field, he reflected on the past, something he had been quite reluctant to do. "I tried to avoid some old sores related to various phases of my efforts in the past," he said. National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 41, File 21, "Perspectives in Early Cardiac Surgery" by D.W.G. Murray, Canadian Cardiovascular Society, 21 November 1963.
Heart surgery in the twentieth century underwent rapid change, moving through three distinct phases -- extracardiac to closed-intracardiac (or closed-heart) to open-heart surgery -- in two generations. Not all of the surgical firsts have been presented, only those that represent key operations in the technical or ideological shift from one phase to another, and Murray's participation in these changes. As Canada's Blue Baby Doctor, Murray was a high-profile participant in the emergence and expansion of modern heart surgery. Blalock's blue baby operation of 1944 marked the beginning of successful closed heart surgery. Surgeons and researchers studied and performed these early palliative heart procedures, devising new techniques and saving countless patients, before corrective open-heart surgery made these operations obsolete.

Without a doubt, Murray was among the surgical leaders of the closed heart period. He was one of numerous surgeons working on the great vessels of the heart as well as directly within the heart. There were only a handful of surgeons who made several key contributions to the field and could perform the new heart procedures with repeated success. These men were Russell Brock and Clarence Crafoord overseas, Murray and later W.G. Bigelow and W.T. Mustard in Canada, and in the United States, Robert Gross, Alfred Blalock, Dwight Harken, Charles Hufnagel, followed by Michael DeBakey and Denton Cooley. It was elite company, and, without realizing it, Murray was at the peak of his surgical career.¹²³

¹²³ Christopher's Textbook of Surgery, dating back to 1936 and printing of several editions, remains the standard surgical textbook in Canadian and American medical education. Senior surgeon Claude Beck wrote the chapter on the heart and pericardium for the first five editions (1936 through to 1949) before being replaced by Charles Hufnagel in the 1956 edition. Noteworthy for this study is the fact that Gordon Murray contributed the chapter on Fractures of the Pelvis in the 1949 edition (as did Alfred Blalock contribute chapters on Abnormalities of the Thymus and Traumatic Shock during the 1940s.)
The public's feelings toward heart surgery changed with the sensational blue baby operations of the 1940s. The surgeons' success in saving the lives of cyanotic children soon led to more successful procedures for other congenital and acquired heart disease conditions. Misgiving gave way to confidence in the skills of the surgeon. Murray and others had demonstrated their ability to treat heart disorders -- confidently cutting into this sensitive organ, offering blue babies a longer life, correcting diseased valves and coronary arteries -- thus deepening society's beliefs and expectations in medicine, surgery and dramatic cures. For Canadians, Murray was at the centre of all this, the media forging an almost unshakeable belief in the public's mind of this man's abilities. Murray's celebrity, among the profession and the public, was due to his heart operations, and it was well deserved.

Murray's role as one of the leaders in heart surgery, arising out of his earlier achievements with the clinical application of heparin, encouraged him to pursue solutions to other medical problems. His success to date suggested that if anyone could do it, Murray could. His heart surgery success fed his ego, his individualism and his pursuit for greater autonomy and status within the Toronto medical community. But as much as Murray tried to repeat his contributions of the 1940s and '50s, he never again enjoyed the professional and public adulation that came with the glory of heart surgery.

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He did not contribute to the next edition. This suggests Murray's prominence among North American surgeons at this time.
Chapter 4

More Research Success: Kidney Machines and Transplantation

As Murray’s surgical career advanced, he became increasingly busy. Despite underlying tensions between him and his superiors at the Toronto General Hospital, he was building a reputation as a skilled although temperamental surgeon. Toronto physicians began referring more patients to him. He operated predominantly at the Toronto General Hospital, both on Ward C and in the Private Patients Pavillion, but also had operating privileges at several other Toronto hospitals including the Wellesley Hospital. He was presented with a wide range of cases, some of which he could easily remedy surgically and others that he could not. He hired a full-time secretary to look after his private practice office and she devoted the next twenty years to his service. Murray could not have managed without her. She answered his phones, booked patient appointments, answered his correspondence, and organized his days. He had a full schedule, operating early in the morning, and conducting hospital rounds before his afternoon office appointments. He still tried to continue his clinical research either at the Banting Institute, or later at the W.P. Caven Memorial Research
Foundation. His day began at seven a.m., and he rarely left the office before six o’clock in the evening.

Throughout the 1940s, Murray was involved in several different and expanding areas. He was performing general, orthopedic and vascular surgery, experimenting with cardiac procedures, and investigating renal therapy all at the same time. He never could contain his interests, intrigued by any problem for which no successful treatment existed. This fertile, restless mind drove him to explore an incredible breadth of interests. While he was devising new surgical techniques for congenital and acquired cardiac defects, he was also investigating ideas of how to save patients suffering from anuria, the failure of kidneys to produce urine. There were many medical challenges, and more often than not, Murray came up with new and successful ways of treating the various conditions he faced in his practice. Diseased kidneys were a field far removed from damaged blood vessels and cardiac surgery. Some medical men argued that a general surgeon would be out of his league in the field of renal therapy. They underestimated Murray. His ingenuity and resolve paid off, first with his artificial kidney machine and later kidney transplantation.

The primary function of the kidneys is the elimination of liquid waste. There are a number of conditions, such as acute toxaemia, acute nephritis, and injury or obstruction to the ureters and kidney, which cause this organ, or pair of organs, to shut down. When they stop functioning, deadly poisons accumulate in the body. The patient lapses into a coma, the body becomes unable to cope with the excess poisonous waste in the blood, and he or she dies. Murray realized that doctors were “helpless” in combating the condition, “desperate” to do something, and they were
largely "ignorant of the disease of uraemia, its cause, how it acts, and its cure."¹ So Murray began investigating the kidney with the prospect of mechanically replicating its functions.

An artificial kidney machine could potentially save the life of a patient suffering from acute kidney failure, providing a mechanism by which the accumulated poisonous wastes could be excreted. If not severely damaged or diseased, the kidneys could occasionally recover completely once the excessive build-up of waste products was removed from the blood stream. "For a long time I had been fascinated by the prospect of producing a dialysing device that might simulate the function of the kidney," said Murray.² He certainly had his work cut out for him. Like all his other research, this project was conceived, directed and controlled by Murray, and he enlisted the help of only junior assistants to carry out activities in his absence. With the assistance of Edmund Delorme, a young surgeon from the University of Edinburgh, and Newell Thomas, an undergraduate chemistry student at the University of Toronto, he began to build his machine in late 1945.³

The artificial kidney is a dialyser, a machine that 'cleans the blood' by removing waste materials or poisons from the blood by filtering the blood through a semi-permeable membrane. By means of extra-corporeal circulation, blood is taken from the patient and pumped through this membrane or tube into a dialysate bath.


Blood cells and proteins remain in the tube, while water, salts, sugar, amino-acids and waste products pass into the dialysate or dialysing solution, a mixture of water and salts. The blood, cleansed of waste products, is returned to the body. Successful vividiffusion experiments on dogs had occurred as early as 1913 by J.J. Abel, L.G. Rowntree and B.B. Turner at Johns Hopkins University. Clinical use of artificial kidney machines however did not regularly occur until after the availability of heparin -- a necessary anti-coagulant for continuous blood circulation outside the body -- in the 1940s. Several artificial kidney prototypes then emerged and were used on patients.

The artificial kidney contained three basic elements: a dialysing membrane or tube, a dialysing solution or dialysate, and a mechanism for circulating blood through the machine. For all three components, Murray had to overcome technical difficulties. First, the dialysing membrane or tube that led the blood through the dialysate had to be a semi-permeable membrane. That is, it had to have "perfect pores" allowing tiny molecules of harmful wastes to pass into the dialysate but retaining the larger protein molecules and blood cells. Murray experimented with various natural and synthetic

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4The Hopkins artificial kidney machine was crude and dependent on hirudin -- an anticoagulant extracted from leeches -- to prevent blood clotting outside the body. Hirudin was difficult to work with and often toxic. World War I interrupted the work of the Hopkins group, and there was little activity on dialysis in the interwar period that followed. One notable exception was German Georg Haas who attempted the first human hemodialysis in 1924 as well as a second clinical case in 1925. His work showed promising but inconclusive results for the clinical use of the artificial kidney machine. Allen B. Weisse, "Turning Bad Luck into Good: The Alchemy of Willem Kolff, the First Successful Artificial Kidney, and the Artificial Heart," Hospital Practice (February 28, 1992): 109-110.

products, such as portions of skin, sheets of peritoneum of animals, rayon, nylon, and wood products. The semi-permeability of the membrane was crucial; the efficacy of dialysis depended on it. In January 1946, Murray connected a sixteen-kilogram uraemic dog to a crude artificial kidney, and circulated the animal's blood for thirty minutes before ending the experiment. He tested for the presence of sugar, urea, albumin, sulphate, and phosphate in the fluid of the rinsing bath, but found none. He concluded that the membrane used was non-permeable. He went back to testing other possible materials. After "exhaustive experimentation", the best semi-permeable membrane proved to be a type of cellophane used for sausage casing. He experimented with the size and length of tubing before settling for the satisfactory size of 1/4 inch in diameter, varying in length from 35 to 150 feet. The tubing was coiled vertically around a wire-mesh cylinder and contained in a large bath jar or drum filled with the dialysate.

Next, Murray turned to his second major challenge: the dialysate. When tap water was used as the dialysate, Murray's animals died. The machine removed substances that were necessary for life, so Murray had to find a dialysate consistent with some of the normal substances of the blood. When Ringer's solution, which was formulated to balance the chlorides, calcium, magnesium, potassium, sodium, sodium,

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8Gordon Murray, Quest in Medicine (Toronto: Ryerson Press, 1963), p.4.

phosphate, carbonate and sugar in the animal's own blood, was used as the dialysate, the animals suffered no ill effects. Murray eventually settled on this solution.¹⁰

The third difficulty encountered by Murray concerned the method of circulating the blood through the machine. The availability of heparin now made extracorporeal circulation a possibility, preventing certain clotting. Initially, Murray extracted blood from an artery, but the artery could only be used once and it did not provide sufficient force to propel the blood through the machine before returning to a vein. He also had problems of hemorrhaging in his animals. Murray decided to work exclusively within the venous system, taking blood from and returning it to a vein, using a pump system. A rubber tambour was inflated and deflated by the action of the piston-syringe, acting as the pump, attached to an electric motor. Intake and outlet valves controlled the blood flow.¹¹

For the most part, Murray's technical difficulties had now been sorted out. His artificial kidney machine was built, and he began running a series of experiments to test its efficacy. According to his assistants Delorme and Thomas, "Murray would slip over to the [Banting] Institute between operations, stay for perhaps two hours studying the results of the previous day's experiments and laying out the work for that day. He was a coldly scientific man ... Everything had to be tested a dozen times and a dozen different ways. When something went wrong he would just stare at it for a moment while he worked out some complicated solution in his head." He continued


his trials with uraemic animals, treating them for hours, even overnight, with relative success.\textsuperscript{12}

Several months later, Murray had his first clinical case. Dr. H.B. Van Wyck, Head of the Department of Obstetrics and Gynaecology at the Toronto General Hospital, presented Murray with a 26-year-old female patient lying in a uraemic coma on Ward F. An unmarried, pregnant woman, she had attempted an abortion using a douche of mustard and water, causing severe toxicity and renal failure. Her body was bloated and her skin like putty. Her injured kidneys could not filter and flush away the poisons that had brought on the coma. On the ninth day, her doctors declared her case hopeless.\textsuperscript{13} At that point, Van Wyck called Murray. Van Wyck was not terribly convinced that the artificial kidney would actually work but he was at a loss as what else to do for his patient.\textsuperscript{14}

Murray quickly arrived on the ward with his odd-looking machine — and what a contraption it was! It was massive and cumbersome and took three men to carry it to the bedside. Murray, his interns and additional nurses rushed around the ward to set up the machine. New dialysing tubing was mounted and the equipment sterilized. Electricians were brought in to provide the correct current on which to run the machine.\textsuperscript{15} Multiple, large wires ran across the hall to various electric outlets.


\textsuperscript{13}As June Callwood correctly notes, "According to medical practice he [Murray] could not experiment on a human being until it had been established that he could do no harm because the patient was going to die in any case. Three sets of specialists certified this." See June Callwood, "The Amazing Mechanical Kidney," Maclean's Magazine (August 15, 1949): 20.


\textsuperscript{15}The motor ran on a 25-cycle, alternating current — what Murray had in the lab. The Toronto General Hospital ran power on direct current, and the motor was not universal. Gordon Murray, Quest in Medicine (Toronto: Ryerson Press, 1963), p.7.
Curious observers took in all the commotion, wondering if this machine would actually work. A surge of excitement swept across the ward.\footnote{Jean Dodds, who became the Director of Nursing at the Toronto General Hospital, was a general duty nurse on Ward F when Murray used his artificial kidney on his first clinical case. She remembers Murray on the ward around the clock and how he had his interns (not Ward F interns) looking after the patient. It received much attention and was very exciting. Miss Jean Dodds, Interview by author, 9 April 1996, Toronto. See also Dr. Murray Enkin, Interview by author, 14 November 1996, Hamilton; Dr. G.G. Caudwell, Interview by author, 13 November 1996, Hamilton; Raymond O. Heimbecker, "Questions and reflections: Geraldine's kidney machine," The Pharos (Spring 1993): 35.}

That evening, after Murray's artificial kidney was set up, the comatose woman was wheeled in. Working swiftly, Murray cut into the large femoral vein on the inside of the patient's left thigh. A long, plastic catheter was inserted into it and connected to the dialyser coils. Then Murray cut into the femoral vein in the right thigh, pushing another catheter up into the vessel until it reached the vena cava just below the patient's heart. Heparin solution was then injected into the patient's bloodstream and into the machine. When the machine was switched on and its pump started moving, dark red venous blood was carried into the cellophane tubing and slowly flowed through the coils in a 15-quart glass jar, containing the dialysate, perched on the bedside table. Bodily wastes escaped through the pores of the cellophane tubing in the fluid bath -- just as they would have been filtered out by normal kidneys and flushed away as urine. The blood then passed through an air trap that removed any bubbles, and returned to the patient's circulatory system. A thermostat control had been built into the machine to maintain the patient's blood temperature outside the body. The noisy machine attracted constant onlookers, curious and perhaps a bit awed at the technology. The patient's condition appeared to
improve, but after one hour, she developed a severe chill. Murray discontinued the treatment immediately.\(^\text{17}\)

The next day, the patient's condition was fair. The day after, she was much worse again and she received eight hours of treatment on the artificial kidney. Her condition improved, however her kidneys still produced little urine. Three days later, the patient again relapsed, and was connected to the artificial kidney for another six hours. The patient was comatose at the beginning of each treatment, but was much revived and alert by the end of the session. It was a trial-and-error approach to regulating the treatments. It was the first time that the artificial kidney had been used and Murray did not know how long the patient's kidneys needed to rest before resuming their function on their own. Would this third treatment prove to be enough?

The day after this third treatment, there was an enormous output of urine -- this was what the doctors had been waiting for. The patient's kidneys had begun to function and residual poisons and excess liquids were soon washed out of her body. She made a steady recovery and was released from hospital 33 days after being admitted.\(^\text{18}\)

It was a celebrated case. Newspapers reported it as yet another life-saving treatment by the "blue baby" doctor. "Artificial Kidney Saves Human Life", "Dr Murray's Machine Restores 'Dead' Girl," "Sausage Casing Used as Kidney Saves


Lives" shared headlines with his sensational heart surgery cases. Murray described his mechanical invention and his success at treating acute kidney failure before his medical colleagues at the meeting of the Central Surgical Association in Chicago in February 1947. Delegates were captivated, calling Murray a "New Trail Blazer."

He spoke on the artificial kidney in London, England as the Alexander Simpson-Smith Lecturer at the Great Ormond Street Children's Hospital on July 11, 1949 -- the same week he presented the Moynihan Lecture on "Surgery of Congenital Heart Disease" before the Royal College of Surgeons of London.

Doctors, hospitals and manufacturing companies wrote to Murray, asking him for specifications of his artificial kidney. Can we send someone to Toronto to observe and learn more about your machine? Where did you acquire your cellophane membrane? How much does it cost to build one? According to Murray, anyone could build an artificial kidney, "it is a very simple arrangement." The most expensive item was the motor and pump component of the machine. He was shocked to learn that a Buffalo maker was

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22 National Archives of Canada, MG 30, B 110, D.W.G. Murray Papers, Volume 42, File 43 Correspondence C.F. Kemper to Murray, 6 January 1949, Correspondence F.J. Wallace (American Cystoscope Makers Inc) to Murray, 26 October 1948; File 37 Correspondence Chas. B. Ripstein to Murray, 27 February 1948; File 28 Correspondence Edwin H. Brown (Allis-Chalmers Manufacturing Company) to Murray, 9 March 1949, Correspondence Solomon Goldenberg to Murray, 20 December 1949.

selling his model for $600. Commenting on this Buffalo unit, Murray said, "it is a very fancy machine with three gears, three speeds, different strokes adjustable in all directions, a plastic chamber with plastic valves ... a very expensive machine compared with the very simple apparatus we were able to build in the laboratory."24

By 1951, the Allis-Chalmers Manufacturing Company in Wisconsin had sold six artificial kidneys at $3600 each.25

Murray's machine had been the first North American model but it was only one of several prototypes in the world.26 Dutch physician Willem J. Kolff invented the artificial kidney in 1943 in occupied Holland. Murray stated that he had not received any information about Kolff's work until after the war, maintaining that "our efforts have been going on apparently simultaneously and independently."27 There were notable design differences in the two machines that suggest Murray was indeed unaware of Kolff's unit. Unlike Murray's vertical cellophane coil in a glass jar, Kolff's artificial kidney had its cellophane membrane or tubing wrapped around a rotating horizontal drum, contained in a 70 to 100 litre tank of dialysate. As well, the


two inventors had decided upon different methods of blood circulation. Kolff took blood from the patient's femoral artery, and depended on gravity as well as arterial pressure to move the blood through the rotating tubing, thus "blood traversed the dialysate in a screw thread-like manner." Murray worked through the patient's venous system exclusively, and relied on a pump to circulate the blood through his machine and back to the patient.

Kolff, who moved to Oxford after the war, had spent three years developing and refining his artificial kidney, dialysing seventeen patients with varying degrees of success, and had more clinical cases and experience than Murray. More importantly, as argued by J.T.H. Connor, Kolff assembled a research/manufacturing team of dialysis experts; "this was one of Kolff's essential contributions for the development of the artificial kidney: a critical mass of technically skilled physicians imbued with the desire to implement and encourage the use of mechanical haemodialysis. These people, and the equipment they built, were instrumental in the successful process of technology transfer from post-war Netherlands." By 1950, Kolff had published a book in English on haemodialysis, had visited and then moved to the United States, establishing closer ties to American manufacturing companies, and had disseminated several Kolff machines around Europe and North America. He had freely offered sketches, designs, and information for others to build their own artificial kidneys,

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further spreading Kolff-based machines around the world.\textsuperscript{30} Surgeons Carl Walter, George Thorn, J.P. Merrill and H. Swan at Peter Bent Brigham Hospital in Boston recognized the potential of the apparatus and improved upon Kolff's machine during this period.\textsuperscript{31} Numerous other doctors in various American centres and elsewhere also expressed interest in the artificial kidney – Kolff and Murray models – and they asked for more information, even design sketches, so they too could build their own machines.

These artificial kidneys, however, were still experimental and offered only a short-term, intermittent treatment to patients suffering from acute renal failure. There remained problems of sustaining patients on the machine for lengthy periods of time and of exhausting usable arteries and veins of patients for treatment. Many practitioners were reserved, if not reluctant to latch onto the device. British medical men outrightly rejected the new technology. Success rates in its use were not convincing. Some patients who received treatment by the artificial kidney regained adequate renal function, perhaps because their own kidneys were allowed a "rest" and their body had been "detoxified". For many patients, this did not happen.\textsuperscript{32}

Consequently, opponents of this therapy viewed the artificial kidney as unsafe, and


they cited occurrences of patient haemorrhaging, dehydration or water overload. "They [artificial kidneys] have no place in the treatment of acute renal failure," stated one British practitioner.33

Comparatively, North Americans were more enthusiastic about the artificial machine. The media reported the "life-saving" capability of these machines, and showcased the various homemade machines built by local doctors. "Artificial Kidney Is Developed Here", "Second in Canada — Artificial Kidney in Use at VGH Saving City Man's Life," "Life-Saving Artificial Kidney Built Here."34 Essentially, American and Canadian prototypes were all based on Kolff-like designs, perhaps modified by their clinician-inventors, each claiming improvements or advantages.35 It was Kolff's model that became the standard from which other medical men used to build their machines and manufacturers based their commercial models. Kolff's work and artificial kidney had been better presented professionally and commercially than Murray's. Few people outside of Toronto seemed to even be aware of Murray's


machine. Montreal and Vancouver papers incorrectly reported that they had the first and second artificial kidneys in Canada respectively.\(^{36}\)

Murray stated that his "interest in the artificial kidney came only as a side-line to his work being done with heparin."\(^{37}\) He had built the machine, proved its efficacy in treating acute renal failure, but it was used comparatively less at the Toronto General Hospital than elsewhere. By 1949, Murray had treated only 11 patients with the artificial kidney, of whom five survived.\(^{38}\) This number increased to sixteen by 1952, many of them public ward cases treated without charge, according to his secretary's records.\(^{39}\) But as Dr William Clarke observed, "one of the major problems faced by Murray was a shortage of trained, knowledgeable personnel, which meant that he often had to be involved throughout the dialysis. The procedure usually had to be carried out overnight, with a full day's work scheduled for the next day."\(^{40}\) Dr G.G. Caudwell, a resident of Murray's, remembers how Murray's interns "dreaded the call" for attending a patient on the artificial kidney because it required "24 hours of continuous monitoring."\(^{41}\) Murray's time was increasingly being spent performing heart surgery; his patient load consisted of more congenital and diseased heart cases than individuals suffering acute renal failure. Murray's machine was then moved to


\(^{41}\)Dr. G.G. Caudwell, Interview by author, 13 November 1996, Hamilton.
the basement of the Toronto General Hospital, and seldom used after 1949. Murray did not abandon his artificial kidney work entirely, but he was frustrated with the hospital's lack of support and interest in renal therapy. Nevertheless, his kidney work remained a secondary line of investigation for Murray at this time.

Three years later, he returned to his work on the artificial kidney machine when his cardiac operations lessened. (Open-heart surgery was on the horizon, and Murray was contemplating the change.) By this time, he had moved his research from the Banting Institute to the W.P. Caven Memorial Foundation due to increased conflict with his Toronto General Hospital superiors over research facilities and funding. Murray also had a new assistant working with him on the project, Dr Walter Roschlau. The young Roschlau was a German immigrant with limited English, trained in pharmacology at the University at Heidelberg. He was hired first as a lab technician, and graduated to Murray's research assistant. They shared a friendly, collegial relationship, but Murray was clearly in charge, and directed all work. Together they developed an improved model.  

Murray thought Kolff's machine was "large and cumbersome ... quite expensive and had certain drawbacks on that account." He wanted to offer "a small working model which will be very compact and not very expensive." So Murray and Roschlau built a smaller artificial kidney. It featured a parallel plate dialyser

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instead of the vertical coil dialyser, making it much more compact. The fluid storage container was also designed to be tucked out of sight under the table, showing "less" machine at the bedside. Its operation had been simplified and its efficiency improved; it was easier to handle, clean and less "frightening". They reported that the results of their dog experiments were "encouraging although not all were satisfactory." Further work on the machine was set-aside during 1953, but Murray and Roschlaau returned to their investigations in 1954. Twenty-seven experiments, involving ten dogs, were conducted to test the performance and reliability of their new machine. The results were satisfactory, but Murray decided to wait for human trials before presenting his work on this second artificial kidney to the medical profession. In November 1955, the new model was used for 12 hours on an uraemic patient who then died ten hours after the completion of treatment. Murray reported that "no flaws in the design or the proper functioning could be observed."

Before Murray could report on this work, one of his engineers, Erwin Halstrup, returned to Germany with the designs of the improved artificial kidney, passing it off as his invention. Halstrup, who had recently arrived in Canada, had

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49 National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 42, File 43 Artificial Kidney, Newspaper clipping: "Artificial Kidney" in Freilbourg Clinic, Suedkurier 15 February 1954, English translation. It is unknown when Murray read this clipping. Also National Archives of Canada,
approached Roschlau, a fellow German immigrant, for a job. At the time, Murray and Roschlau were struggling with the increasing technological complexities of their new machine. They realized that they needed the assistance of an engineer. Halstrup was thus hired, and he helped them develop a new pump and change the prototype from plexiglass to metal. Halstrup left Canada soon after the work was done. Shortly thereafter Murray and Roschlau received letters from two German medical schools asking them for their experience with the Halstrup-Bowman artificial kidney. Roschlau was devastated and Murray outraged! With the help of German businessman Bowman, Halstrup was peddling the machine as his invention, selling the artificial kidney to medical clinics and hospitals in Germany. After that, Murray totally lost interest in the artificial kidney. He had lost control over his own machine, and he walked away from any further involvement with it. Understandably, the incident made Murray more secretive and suspicious of fellow researchers.50

Murray's interest in renal therapy extended beyond the artificial kidney to transplantation during the 1950s. The kidney machine offered treatment for acute renal failure only. How might he treat chronic kidney disease? Again, his restless mind was looking for new frontiers. Murray returned to his early kidney transplant experiments. In the early 1930s, he had removed the kidneys of dogs and successfully

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transplanted them to veins in the dogs' necks.\textsuperscript{51} (This was not a new procedure; Emerich Ullmann in Vienna had successfully performed this experimental operation, also on dogs, in 1902.\textsuperscript{52})

In the postwar period, there emerged a renewed interest in clinical kidney transplantation, not only by Murray in Toronto, but also by Paris surgeons Jean Hamburger, Rene Kuss and Marcel Legrain, as well as American surgeon David Hume at Peter Bent Brigham Hospital in Boston.\textsuperscript{53} Heart, kidney and other organ transplant operations seemed ever more possible, but as yet remained elusive to these investigators. Dr. Jacob Markowitz, research associate in physiology at the University of Toronto argued that the difficulties of transplantation would be overcome by the work of a physiologist, not a surgeon. It is "the physiologist ... [who] has to find a way to keep the heart, kidney or other organ alive and make it grow into the place to which the surgeon has transplanted it. It may be that a serum will be found to make them live."\textsuperscript{54} Murray was not deterred.

During the late 1940s and early 1950s, Murray ran a series of transplant experiments and was able to report on the results from over 60 experimental operations, including four clinical cases. He experimented with autotransplantation,


\textsuperscript{54}Dr Jacob Markowitz was the first doctor to transplant successfully the heart of one warm-blooded animal to another. He and Dr Frank Mann, of the Mayo Clinic, grafted new hearts into the necks of dogs and rabbits in 1931. Markowitz and others anticipated the need of immunosuppressive drugs in organ transplants. "Easy to Transplant Heart - Keeping It Alive Problem," \textit{Toronto Star} 1 February 1946; Ross Harkness, "Heart, Other Organs to be Transplanted," 1 February 1946; "Sees Early Transplanting of Human Hearts, Kidneys," \textit{Toronto Evening Telegram} 1 February 1946.
grafting an organ from one part of the body to another, and homotransplantation, removing and implanting organs between individuals of the same species. He ran many series of experiments, almost always unsuccessful transplants, but they helped him to develop the ideal methods by which to protect the kidney. Murray reported that if the organ was cooled, it could be deprived of its blood supply longer without damage. After its removal, it was irrigated thoroughly; this decreased chances of rejection and prolonged its viability. But he was stumped by his transplant failures; complete necrosis (tissue death) of the kidneys eventually occurred in almost all his animals. He never considered that the host’s immune system was rejecting the organ, theorizing that it had to do with inappropriate blood supply. "The vital factor in transplantation of such organs lay in the careful management of the vascular tree," wrote Murray. The organ’s blood supply had to be maintained, and thrombosis prevented. He continued with additional series of experiments, but these organ transplants were also unsuccessful.55

Murray refused to abandon his experiments. "Despite our own results as well as the failure of others to obtain survival of transplanted kidneys in animals, this did not prove with certainty that such would be the case in human beings," he wrote.56 Murray had successfully attached a kidney to the circulation of a sick person, via the arm, which functioned temporarily to clear the blood stream, much like the artificial kidney.57 In some of these cases, the kidney transplants to the arm resulted in a fair secretion of urine but it was believed that the vessels of the arm hampered the free


flow of blood to the kidney due to their small size. This success with kidney transplants to the arm encouraged Murray to try permanent transplants in humans.\textsuperscript{58} This was highly unusual: failed laboratory research and related but limited clinical success would not have been viewed as encouraging to most scientists. Murray however was convinced and boldly pressed on.

In 1951 and 1952 he attempted kidney transplants on four patients, all suffering from late stages of uraemia. Three of these patients did not survive, despite the fact that the transplanted kidney had appeared to function and had improved the condition of the poisoned blood.\textsuperscript{59} The other patient was 26-year-old Dorothy Pezze, swollen with Bright's disease, fifty pounds heavier than her normal body weight. She had suffered from fifteen years of chronic kidney disease, and she had no chance of survival without a new kidney. Pezze wanted the experimental surgery, was admitted to the Toronto General Hospital and waited for a donor kidney to become available. On May 2, 1952, Pezze underwent the transplant operation.\textsuperscript{60}

The transplant surgery was reminiscent of Murray's first blue baby operations. It was exciting and new, risky but potentially life saving. The O.R. team had been selected well in advance: Jean Dodds and Reta Smith were his preferred O.R. nurses and Dr. Steven Evelyn was the anaesthetist.\textsuperscript{61} They were called in at two o'clock in


\textsuperscript{59}Young Fred Moffat was training at the Toronto General Hospital at this time, and remembers witnessing one of these unsuccessful transplant operations, as well as transplanted kidneys to vessels in the patient's arm. Personal Correspondence, Dr Fred Moffat to author, 21 January 1997; Gordon Murray and Richard Holden, "Transplantation of Kidneys, Experimentally and in Human Cases," \textit{American Journal of Surgery} Volume 87 (1954): 513.

\textsuperscript{60}"Transplanted Kidney Saves Dying Woman," \textit{Toronto Telegram} 2 July 1952.

\textsuperscript{61}The O.R. team had known for months about these operations, and their tendency to be performed in the early hours of the morning depending on the availability of donor kidneys. Mrs. Reta (Smith)
the morning, had assembled promptly, and now stood in position next to the
anaesthetized patient in the O.R. Murray began the operation. He decided to leave
Pezze's two diseased kidneys undisturbed, and made room to add the donor organ as a
third kidney. He made an incision into the abdomen, exposed her internal organs and
shifted them to the left side of the cavity without much difficulty. The donor kidney
had been irrigated with heparin and Ringer's solution; it was almost white in colour
and had shrunk to two-thirds its normal size. Murray retrieved the kidney from its
large basin filled with a cold, saline solution and placed it in the patient's right iliac
fossa (the space above the right groin). He now had to attach its vessels to those of
the patient so that circulation would be restored and necrosis prevented. He carefully
sutured the kidney's vessels to the external iliac vessels of the host in an end-to-side
anastomosis. The ureter was transplanted through the wall of the bladder. Now
Murray tentatively removed the clamps and willed the new organ to begin functioning.
It had been two-and-a-half hours since the kidney had been removed from the donor.
Blood began to circulate through the kidney, and it changed from greyish-white to
bluish-red in colour and finally to a bright pinkish-red hue. Within a few minutes,
urine was trickling from the end of ureter. Within four minutes, urine was squirting a
distance of eight inches beyond the end of the ureter. The anastomoses had held and
the blood was circulating; the kidney was functioning. Murray could not have been
more pleased -- the operation was a success!  

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The kidney transplant made Toronto headlines, another celebrated case of Murray's. "Transplanted Kidney Saves Dying Woman" -- it was a sensational story.\(^6^3\) "Your pioneer work in so many fields has been an inspiration to countless young surgeons, including myself," wrote one former student.\(^6^4\) "Congratulations in your most recent triumph," wrote his cousin Gladstone Murray. "You have cultivated a steady rhythm of progress moving forward from one pinnacle to the next with calm precision. The multitude of your admirers increases in number and enthusiasm; the envious few are distraught with frustration."\(^6^5\) Dorothy Pezze became known as the "woman with three kidneys" and she lived another 35 years.

But there was little if any assessment of the cause of her longevity. Was it the transplanted kidney or her original two kidneys that saved her? When Murray reported the operation in 1954, he refrained from offering any conclusions. "The best proof," he said, "is that a chronic invalid with alleged irreversible nephritis, high blood pressure, persisting edema and a low concentrating power has now returned to normal health and vigour."\(^6^6\) It was a critical oversight by Murray to not follow her recovery over any length of time. This lack of follow-up with patients was a consistent pattern in Murray's career, and consequently a repeated criticism of his


research. So while the operation had been a technical success, no one knew how long the transplanted kidney actually worked. Many medical men suspected that Pezze’s own kidneys had recovered and questioned the long-term functioning of the transplanted kidney. For Murray, he had clinical proof – Mrs. Pezze was alive and well – and that was enough for him. He was not interested in collecting further data on Mrs. Pezze or on other patients to prove that his procedure worked. In his view, his research on kidney transplantation was successful, and he moved on to the next medical challenge. There is no record of any further kidney transplant operations performed by Murray, nor further follow-up to his one success case. Consequently his work received little recognition from the profession.

In 1954, two years after Murray’s operation on Mrs. Pezze, Dr. Joseph E. Murray at Peter Bent Brigham Hospital in Boston successfully transplanted a kidney from one identical twin to another. He later performed other successful kidney transplants on fraternal twins as well as unrelated donors and recipients due to his incorporation of immunosuppressive therapies – first, whole body irradiation, then immunosuppressive drugs. His clinical transplant operation, based on years of perfecting the procedure on animals and drawing upon techniques pioneered by surgeons Emerich Ullmann, Alexis Carrel and other earlier investigators, became the

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67Dr Joseph Murray of Peter Bent Brigham Hospital received the Nobel Prize in 1990 for his work in the field of kidney transplantation, his success due to the use of immunosuppressive drugs. Dr. Raymond Heimbecker, Interview by author, 1 November 1996, Toronto; Personal Correspondence, Dr. Lloyd D. MacLean to author, 25 November 1996; Dr C.B. Mueller, Interview by author, 15 November 1996, Hamilton.

68Researchers at Peter Bent Brigham Hospital in Boston followed Murray’s four transplant operations closely, however, since there was no postoperative studies of renal function or biopsies of Mrs. Pezze’s third kidney, it was impossible to know if the transplanted kidney was alive and functioning well. Renee C. Fox and Judith P. Swazey, The Courage to Fail: A Social View of Organ Transplants and Dialysis. 2nd edition (Chicago: The University of Chicago Press, 1978), p.70.
model for subsequent transplant operations. Greater understanding of tissue typing and immunosuppression has reduced organ rejection and led to greater success. In 1990, Dr Joseph E. Murray received the Nobel Prize in Medicine and Physiology for the first successful transplants of vascularized human organs.69

The range of Murray's abilities appeared to have no boundaries. He developed new operations and inventions that saved lives in whatever field he focused. Reporters glorified Murray's success and thankful patients praised his talents -- he was a "genius", a "miracle-maker", and a "challenger of death".70 Yet despite these contributions, Murray felt that he received little if any support from his colleagues at the Toronto General Hospital. According to Murray, his artificial kidney and the lack of enthusiasm for its use came to exemplify Toronto resistance and jealousy of his work. Dr. F.B. Bowman supported this view and wrote to the Toronto Globe and Mail. "It seems about time that some endowment and provision be made for a department for research in surgery, and in Toronto there is a surgeon [Murray] who has brought more attention to the University of Toronto than anyone since Sir Frederick Banting, the discoverer of insulin. One surmises that he is getting about as

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70National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 31, File 9 Correspondence — to Murray, 1944; File 12 Correspondence — to Murray, 1947; File 13 Correspondence — to Murray, 1948; File 16 Correspondence — to Murray, 1951; File 17 Correspondence — to Murray, 1952.
much help from the university, perhaps even less, than young Banting got when he brought his first proposition to those in authority.”

Murray began complaining openly about the limited funding and facilities offered him by the Department of Surgery in Toronto. At the bottom of some of his medical publications, he pointed out that: "Some facilities were provided by the Banting Institute. Otherwise this work was carried on with private funds only and without other assistance." He stated that the artificial kidney had been built through his own efforts and at his own expense, costing more than $10,000 for materials, laboratory animals and paid assistants. These statements only served to alienate Murray further from his Toronto colleagues. They too struggled, doing research that needed greater funding and better facilities. "I think Dr Murray's point of view is that he has tried to interest Professor Gallie in his research projects from time to time with hope of getting a grant, but that he received no help at all -- and does not want to be given anything now," wrote one surgeon.

71Bowman's editorial included comments on Murray's selfless, personal expenditure of $10,000 for inventing the artificial kidney, the undue criticism he received from his colleagues over the blue baby publicity, and the obvious "pangs of envy" apparent within the Toronto medical community. It is very critical towards the university authorities. F.B. Bowman, "Aiding Research in Surgery," Toronto Globe and Mail 26 September 1949.


74University of Toronto Archives, Department of Surgery, A89-0030/001, File (05) Estimates (194x), Note to file signed T.M., 3 March 1947.
Added to Murray's aggravation over insufficient research funds was his frustration with his Toronto colleagues' apparent disinterest in new procedures or technology. In his view his artificial kidney was the perfect example of a new invention launched into such a non-supportive medical environment. There was simply a lack of interest by others in its use on the wards. What he did not understand was that many doctors, including the Chief of Medicine Duncan Graham, were concerned with protecting their patients from ill-conceived experiments. Murray would have resented this inference. Nevertheless, they were wary of machines, including his artificial kidney, and took a conservative stance towards the new technology being brought into the hospital. Perhaps it was because it was Murray's project that the response against it was so negative.75 The friction between Murray and his Toronto colleagues was well known. One visiting surgeon commented that, "Somehow he seemed to arise (sic) the jealousy of his colleagues so that the atmosphere of the hospital was like that of the feuding Scots. They [colleagues] appreciated his distinction and ideas but were pursuing their own fields and did not wish to be overshadowed ... It was not very warm. Gordon Murray did not communicate with his colleagues and they did not discuss him."76


Murray demanded much from those around him, and he had no time or tolerance for incompetence. He viewed most of his Toronto colleagues as extremely parochial and conservative, lacking important post-graduate training and study outside of Canada, and he criticized them for their disinterest in clinical research. Despite his own harsh comments about his colleagues, Murray was constantly irritated and bothered by what his colleagues in turn said about him. Murray was thin-skinned and extremely sensitive to professional comments and banter that caused him to brood over even the most meaningless of remarks.\footnote{Dr. J.K.W. Ferguson, Interview by author, 18 March 1996, Toronto; Dr. W.G. Bigelow, Interview by author, 26 March and 2 April 1996, 17 April 1997, Toronto; Dr. D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto; Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; Dr. H. Hoyle Campbell, Interview by author, 28 August 1996, Inglewood.} He often told people that he regretted his decision to come to Toronto, yet despite later offers, Murray never did leave the medical community which so frustrated him.\footnote{Over the years, Murray received various offers to set up surgical units, ie Australia, New Zealand, New York, but only one was tempting -- McGill University after World War II. According to his daughter, McGill officials were considering Murray to be their new (and first full-time) Professor of Surgery, however W.E. Gallie would not recommend him and refused to tell Murray why. McGill Dean J.C. Meakins had asked Gallie to suggest someone for the position, saying they were looking for someone "under forty-five and preferably under forty ... he should have the reputation of solely being a good surgeon, but should have shown evidence of a capacity to be a good teacher and direct research within his department, as well as carry out such himself." Of the nine men eventually shortlisted for the post, Murray's name was absent. In January 1946, Dr. Gavin Miller was appointed Professor of Surgery at McGill University and Surgeon-In-Chief at the Royal Victoria Hospital. Interestingly, Gallie felt that while "surgery in McGill was [not] beyond redemption", it did need reorganization along Toronto lines. In letters to Dr Fred Kergin, who had been shortlisted by the McGill Selection Committee, Gallie wrote that he expected great resistance by McGill staff and "frankly I mistrust Meakins." In the end, due to Gallie's advice against taking the job, Kergin turned down the position. In an effort to entice Kergin back to Toronto, Gallie described the exciting new surgery taking place at TGH under Janes, who "is almost in a class by himself in chest surgery", Harold Wookey and Roscoe Graham. Gallie did not mention Murray. Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; University of Toronto Archives, Private Papers, F.G. Kergin, B 81-0016/003 Correspondence W.E. Gallie to F.G. Kergin, 10 May 1945, 17 June 1945, and n.d. [1946]; McGill University Archives, RG 38 Faculty of medicine, Container 7, File 169, Correspondence Dean J.C. Meakins to W.E. Gallie, 5 October 1944, Correspondence Dean J.C. Meakins to F.G. Kergin, 29 July 1945, Selection Committee on the Chair of Surgery, Summary of Minutes of Meetings held to date, 7 September 1944 and 1 May 1945; McGill University Annual Report 1945-45, "Principal's Report," p.28.} He had a successful private practice, one at which he had worked hard to build, and he was reluctant to start over...
elsewhere, despite his reputation.\textsuperscript{79} So he continued to hope for change in the department and in his current research facilities and funding arrangement. Change was coming, and eventually Murray was forced to make some important career-altering decisions.

\textsuperscript{79}Economic security was important to Murray, and he had invested his time and money (property) in Toronto. According to his daughter, he had always been concerned about his livelihood, worrying that he would have enough to provide for his family, his research (since little funding), and old age. He felt he had no family or other backing on which to fall on, and so took building a successful practice very seriously. In fact, he was very critical of young people who did not put all their time into building their new practice. Murray did not take long holidays — perhaps ten days each summer for family camping. In the end, Murray became financially well off, investing in land, but also due to his ability to save and live frugally. His daughter remembers the family growing their own fruits and vegetables, and doing without the frills of daily living such as magazine subscriptions. Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.
Chapter 5

On His Own: The W.P. Caven Memorial Research Foundation

By the late 1940s, Murray’s surgical career was thriving. He was recognized as one of the leaders in vascular and cardiac surgery, and was now expanding the possibilities of renal therapy, saving patients with failed kidneys. Some medical men felt that there had been too much media attention to Murray, particularly to his blue baby operations and now his artificial kidney machine. Murray dismissed it as Toronto resistance and jealousy. Friction, tension and verbal altercations were increasing between Murray and his hospital superiors. His supporters argued that he had brought more attention to the university than anyone since Frederick Banting, and even struggled as the young Banting had to acquire adequate funding and facilities for his work.¹ Glowing statements from reporters and patients encouraged Murray to continue, but his medical colleagues found him to be increasingly more difficult to work with.

With his growing reputation, Murray began to make greater demands on the Department of Surgery at the Toronto General Hospital. He was frustrated by the lack of research funds, inadequate facilities and negligible recognition he received for his projects. With finances from an outside benefactor, the Department of Surgery attempted to remedy this by setting Murray up with his own research division. It seemed a workable solution, certainly a fair compromise to satisfy both Murray’s complaints and the need for university controls. The endeavour failed because of Murray’s non-negotiable demand for full autonomy from the Department. Shortly thereafter, the W.P. Caven Memorial Research Foundation was established, and Murray was appointed Research Director. He was now out on his own – director of an independent research facility and surgeon with a large private practice – distancing himself from the university and hospital staff men that held such prestigious and influential positions in the Toronto medical community. It was a time of transition for Murray -- a change in affiliation and physical surroundings as well as an opportunity to direct and expand his research interests.

W. E. Gallie, Professor of Surgery at the University of Toronto and Surgeon-in-Chief at the Toronto General Hospital, distributed the central funds of the department at his discretion. In the past, Gallie had supported many of Murray’s research projects, including his work with heparin, yet the two men rarely agreed on the terms of such departmental support. Over the years, they developed an antagonistic relationship. Gallie recognized Murray’s abilities as a surgeon and innovator, "circling around him with care", and provided him with what he thought he
could. Gallie was a promoter of research, and despite personality differences, Gallie tried to find research facilities for Murray. It was always inadequate, according to Murray, and the two men constantly bucked heads. In 1947, Murray fired off a heated three-page letter to the Chairman of the Board of Governors of the University of Toronto complaining of Gallie's, as well as others', overt efforts to stifle Murray's research efforts:

"My object in writing [this letter] is to bring to your attention the difficulties under which my attempts in research have been carried out ... during the past year, three subjects have reached the stage in development where they have attracted widespread attention to this school. First is the blue baby work -- next to Blalock, I have the largest series of successful operations and the lowest mortality rate on this continent ... Secondly, the artificial kidney was developed single handed. Following a paper in Chicago in February 1947, there has been shown great interest by many American Surgeons ... They all return to laboratories where they have many full time assistants to continue developments, and what chance have I single handed of competing with them? ... Thirdly, my paper, and the only paper from Canada, given at the American Surgical Association, on the Pathophysicsiology of the cause of death from Coronary Thrombosis, excited great interest ... Many of the great surgeons of the great schools were much interested ... The work so far on many of these subjects has merely opened the door, and the great developments are to follow. As well, I have several other original investigations that I would like to proceed with. However the difficulties have been too great. I have tried repeatedly to approach the head of the department of Surgery to obtain physical, financial and moral support in struggling on with this research, but my efforts have fallen on stony ground ... about one year ago I waited in his [Gallie's] office at the Banting Institute and the resulting interview was short. I tried to discuss the problems and asked if he thought I was justified in asking for some assistance. The answer was emphatic and I quote the words of the head of the department, after jumping to his feet and pounding the desk with both fists he said, "I don't give a G.D. what you do." That seemed to convey to me the amount of support that I could expect ... I continue to pay my own assistants. I have done [chemical] research in odd corners and by anyone who did not object, namely the laboratory in the Private Patients Pavilion ... I installed a chemist on my pay and provided the materials and the expenses myself ... The question arises whether the board is aware of the attitude toward research, or it may be the attitude

toward me personally ... There is no school on this continent or elsewhere which has produced as much original work in recent years as the Toronto School and that has been done mostly by one person ... Therefore, sir, I am writing to ask your opinion whether this is the atmosphere in which you wish research and investigation to be done in the University of Toronto?"³

Murray challenged his Toronto medical seniors constantly, often openly, his antagonistic approach never endearing him and his current research efforts to men of authority. According to one surgeon, Murray was the "burr under the saddle of the surgical establishment."⁴

In 1947, W.E. Gallie retired after an eighteen-year tenure. With Gallie's endorsement, the University of Toronto Board of Governors selected Robert Janes as the next Professor of Surgery and Surgeon-in-Chief.⁵ Murray was upset with the appointment, and in his eyes it was another unjust action taken against him by the "Family Compact" (influential Toronto medical men).⁶ Deep down, he had wanted the job. He would have liked the professional recognition and university standing that came with the position, perhaps enticed by the control over research funding and staffing it included. The Board of Governors had considered him for the position. Murray was better known internationally and had great breadth as a surgeon. But Janes was the better candidate for the job. He had five years seniority on Murray and

³National Archives of Canada, MG 30, B 110, Volume 28, File 28 (page 1) and Volume 30, File 17 (page 2 and 3), Correspondence Murray to W.E. Phillips, 8 May 1947.

⁴Dr. Robert Harris, Interview by author, 19 April 1996, Toronto.

⁵Gallie's successor was debated behind closed doors by the Board of Governors. Gallie handpicked Janes while Herbert Bruce and J.S. McLean, a strong voice due to his generous donations to the university, lobbied hard for Murray to be appointed to the position. Unfortunately for Murray, Bruce and McLean were "outsiders" while Janes' supporters held prestigious positions at TGH and the university. Dr. W.G. Bigelow, Interview by author, 26 March and 2 April 1996, 17 April 1997, Toronto; Dr. D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto.

held professional recognition for his own contributions to thoracic surgery, notably the Shenstone-Janes lung tourniquet. More importantly, Janes was better liked among his colleagues in the department, and interacted well with them. Murray simply did not have the personality to manage a department full of surgeons with difficult egos. Janes was the better administrator for the many post-war challenges that lay ahead, such as returning young veterans seeking surgical training, research facilities and funding for a department in need of expansion, and initiation of a new hospital building programme -- all of which Janes sought to play a key role. Nonetheless, Murray felt that he had been unfairly overlooked.

Soon after Janes became the Professor of Surgery, Murray requested several changes to the department so that he could continue his research. Janes was unable to appease Murray; he could not give Murray money the department did not have, nor increase his operating time on the ward, nor provide special facilities for his artificial kidney. He was also unwilling to relieve Murray of his teaching duties or make Murray's service autonomous within the department. And so continued Murray's antagonistic relationship with his superiors as he demanded greater facilities, funding and privileges -- working from within the department to try to effect change. Herbert

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8"There was no way such a 'maverick' could have been allowed to hold the chair." Personal correspondence, Hugh R. Gallie to author, n.d. [May 1996]. The friction and competitiveness surrounding the choice of Gallie's successor, and the personalities involved, taken from: Dr. J.K.W. Ferguson, Interview by author, 18 March 1996, Toronto; Dr. W.G. Bigelow, Interview by author, 26 March and 2 April 1996, 17 April 1997, Toronto; Dr. D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto; Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; Dr. H. Hoyle Campbell, Interview by author, 28 August 1996, Inglewood.

Bruce and J.S. McLean, Murray's staunch supporters on the Board of Governors, set about to find a solution to Murray's departmental conflicts.

Bruce and McLean worked through the Board of Governors in an effort to establish a new position for Murray -- one they thought better suited his talents. As early as June 1947, McLean announced that he was prepared to offer $15,000 a year for five years to the Department of Surgery for research under Dr Gordon Murray. He wanted a Chair of Research in Surgery established specifically for Murray. The Board of Governors was interested; they wanted nothing more than to find "some satisfactory solution of the personnel problem" with Murray and the department. Moreover, McLean was offering $75,000 at a time when funds were very much needed by the university for research. The Board of Governors asked Janes to submit a plan to set up a Division of Experimental Surgical Research within the Department of Surgery.

In October 1947, Janes submitted his plan to the Board of Governors with the endorsement of Dean J.A. MacFarlan and President Sidney Smith. He proposed that the Division of Experimental Surgical Research include a Coordinator to direct the work of the division, two fellowship positions, a chemist, an assistant technician and one secretary, all with appropriate salary allocations. They were still looking for suitable space -- aware that the present departmental research quarters at the Banting Institute could not handle additional researchers. The division would be under the supervision of the Professor of Surgery, and he would select and approve personnel to

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10 University of Toronto Archives, Board of Governors, Minutes, A73-0024, Microfilm roll #15, 26 June 1947, p.155; 11 September 1947, p.164; Executive Committee Minutes, A79-0038, Box 2, File 1, 17 October 1947.

11 University of Toronto Archives, Board of Governors, Minutes, A73-0024, Microfilm roll #15, 23 October 1947, p.224.
work in the new division.\textsuperscript{12} Dean MacFarlane approved of Janes' arrangements; "It will fill a need long felt in that Department. Not only have there been lacking in the past the funds, equipment and facilities for those more senior members of the Department of Surgery to carry out important work, but there will be from time to time the accession to the Department of younger, well trained men who desire to give their whole time to teaching and research for a period of one, two or three years."\textsuperscript{13} The Board of Governors endorsed the views and suggestions as presented by the department's report, and left it to Smith, MacFarlane and Janes to approach Murray.\textsuperscript{14}

Murray was excited about the prospects of a research division under his control, with greater funding, facilities and assistants to pursue his projects. But he was not happy with Janes' plan for the division. They haggled over some minor points --- Murray would be the 'Director', not the 'Coordinator' and the chemist would be working full-time for the division rather than being shared with other departments -- to which Janes agreed. But Murray wanted more than Janes could ultimately give him. Murray wanted two full-time men assisting him in the division and at least two rooms at the Banting Institute made available to him immediately. Most importantly, he wanted total independence "so that work can be carried on under my own planning."\textsuperscript{15} Murray wanted full administrative and financial autonomy. Perhaps he

\textsuperscript{12}University of Toronto Archives, Board of Governors, A73-0025/007 File 27 Appropriations by Faculties and Divisions 1947-1953, minutes abstract, Correspondence R.M. Janes to J.A. MacFarlane, 20 October 1947.

\textsuperscript{13}University of Toronto Archives, Board of Governors, A73-0025/007 File 27 Appropriations by Faculties and Divisions 1947-1953, minutes abstract, Correspondence J.A. MacFarlane to Sidney Smith, 20 October 1947.

\textsuperscript{14}University of Toronto Archives, Board of Governors, Minutes, A73-0024, Microfilm roll #15, 23 October 1947, p.224.

\textsuperscript{15}University of Toronto Archives, Office of the President (Sidney Smith), Correspondence (subject files) A68-0007/036 File 2 Medicine July 1947 to June 1948, Correspondence D.W.Gordon Murray to Sidney Smith, 24 November 1947.
was using the Banting and Best Research Department, which was autonomous from the Department of Medicine, as his model. In any case, he could not persuade university administrators that this was the way to go. Janes and MacFarlane flatly refused: the division will be "part of the University Department of Surgery and not a separate department ... [and] there shall be one budget." They were willing to consider allocating more money to the division in order to hire two full-time men for Murray but they could not give him complete autonomy from the Department of Surgery. Janes and MacFarlane were structuring the new division based on principles of administration, not personalities, in contrast to Bruce, McLean and Murray who were catering to the concerns of the involved individuals.

The two sides dickered back and forth. On November 12, Smith heard from Janes and MacFarlane that Murray would accept the position. Twelve days later, Smith received a letter from Murray stating that he could not accept the post unless it was established as a separate department. Murray refused to be restricted by the Department of Surgery any longer; he wanted his own research department and control of the projects and allocated monies therein. Ultimately the Board of

16 Michael Bliss, Banting: A Biography (Toronto: McClelland and Stewart, 1984), pp. 120, 182.

17 University of Toronto Archives, Office of the President (Sidney Smith), Correspondence (subject files) A68-0007/036 File 2 Medicine July 1947 to June 1948, Correspondence J.A. MacFarlane to Sidney Smith, 27 November 1947.

18 University of Toronto Archives, Office of the President (Sidney Smith), Correspondence (subject files) A68-0007/036 File 2 Medicine July 1947 to June 1948, handwritten notes.

19 Writing in third person, Murray gave his account of these events: "Pressure was brought to bear on the young surgeon [Murray] to accept a research set-up under the supervision and control of the head of the department. The young surgeon, however, having had previous bitter experiences and now nine negatives to his request in this direction, together with the attitude of the Dean, he was well aware how little he would be free to develop his ideas. Once under the control of the Head of the Department, who had neither experience nor interest in research and was highly prejudiced, it was obvious that his days as an investigator and contributor were numbered. Having considered it all very carefully, and having decided that added to this removal by legislation of his privileges, he would not add further to his own sorrow by selling the mess of pottage. He, therefore, at his peril resisted the trifold pressure [from the
Governors, on the advice of Janes, MacFarlane and Smith, decided that there could not be a separate Department of Experimental Surgical Research, and thus they could not accept McLean's generous funding offer. ²⁰ The matter was closed, and Murray lost his opportunity for improving his research situation.

Murray returned to his small lab room at the Banting Institute and resumed his work. This failed attempt to provide adequate research facilities and stature for Murray only increased his belief in the university's determination to undermine his work. It was a repressive environment; jealousy and lack of vision surrounded him, he thought. It became a daily challenge for Murray to withstand the departmental power struggles. He was not well liked by his peers; his difficult personality had caused lasting antagonism. In the end, Murray felt a victim of the establishment and it was best just to get out. He refused to leave Toronto, to go into exile as he saw it, but he did make plans to leave the Toronto university research structure. ²¹ Herbert Bruce and J.S. McLean made this possible.

President of the University, the Dean of the Faculty of Medicine, and the Head of the Department of Surgery] and decided to keep under his own control the future developments which he had in mind.”

National Archives of Canada, MG 30 B 110, Volume 44, File 13 Medicine in the Making - drafts, pp.516A and 516B.

²⁰University of Toronto Archives, Board of Governors, Minutes, A73-0024, Microfilm roll #15, 11 December 1947, p.268.

²¹Not long before this, McGill University conducted a search for a Chair of the Department of Surgery and Surgeon-in-Chief at the Royal Victoria Hospital. They were looking for someone under forty-five, closer to forty, years of age. Murray was now over fifty years old, but according to him, his chance at this position was eliminated by Prof. Gallie who told McGill Dean of Medicine Meakins that Murray would be an unsuitable candidate. For Murray, this was yet another example of the Toronto medical men working against him. There is no documentation to suggest that Murray was ever seriously considered for the McGill position. National Archives of Canada, MG 30 B 110, Volume 44, File 13 Medicine in the Making – drafts, p.518; McGill University Archives, RG 38 Faculty of Medicine, Container 7, File 169, Correspondence Dean Meakins to W.E. Gallie, 5 October 1944, Minutes of the Selection Committee on Chair of Surgery, 7 September 1944 and 1 May 1945. See also National Archives of Canada, MG 30 B 110, Volume 45, File 2 Medicine in the Making - drafts, notes, undated; Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; Gordon Murray, Quest in Medicine (Toronto: The Ryerson Press, 1963), p.98-99.

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Dr. Herbert Bruce was a respected surgeon and founder of the Wellesley Hospital in Toronto. The same year that he was appointed Lieutenant Governor of Ontario (1932), he joined the Board of Governors of the University of Toronto. Years ago, he had fought his own battles with the Toronto medical community. Bruce belonged to an earlier generation of surgeons, and Murray liked and admired the older practitioner. Their relationship had been friendly and collegial. It had been in Bruce’s hospital that Murray performed his blue baby operation one year earlier. J.S. McLean, President of Canada Packers Ltd., was a good friend of Murray’s, and the two families socialized regularly. McLean was also on the Board of Governors of the University of Toronto, and a generous benefactor to medical research. Both Bruce and McLean were strong supporters of Murray, and they were able to realize Murray’s plans for a research unit.

After the university's failed attempt to appease Murray in 1947, Bruce and McLean took on the task of finding proper research facilities and funding for Murray. Less than two years later, they announced the opening of the W.P. Caven Memorial Research Foundation with Murray as Director. It was a private research laboratory that had been set up completely independently of the University of Toronto. The bequest of $100,000 by the widow of the late Dr William Proudfoot Caven made this possible. Caven belonged to a well-known Toronto family, was a well-liked physician, and a close friend of Bruce's. In fact, he had been one of the founders of the Wellesley Hospital.²² Bruce was one of the trustees of this bequest and he could

²²"He [Caven] was almost worshipped by his patients," said Bruce. "His gentle sympathetic attitude and kindly manner gave patients immediate confidence in his ability to help them." Caven's elder brother, Dr John Caven, was the first professor of Pathology at the University of Toronto and Caven's younger brother, Dr James G. Caven, also practised medicine in Toronto. Caven's father, Rev Dr William Caven, was Principal of Knox College. Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," Toronto Star 24 November 1949; University of Toronto, Thomas Fisher Library, Academy of Medicine Collection, Biographical Files, 0279 W.P. Caven; H.A. Bruce,
think of no better way of honouring Caven's memory than to establish a research institute under Murray's guidance. Once again, McLean announced his offer of $75,000 over five years as funding for Murray's research projects, this time to the Caven Foundation. Percy Gardiner and Joseph West also made generous donations to the new research institute. Murray had supporters, and Bruce was an influential force among Toronto philanthropists. The University of Toronto attempted to secure these monies for their centralized research accounts -- the Wellesley was now a division of the Toronto General Hospital -- but was unsuccessful. No doubt, Bruce would have set up a research department for Murray within the Wellesley Hospital if it had not amalgamated with the Toronto General Hospital. Their recent union however did not make this possible.23

The Caven Foundation was located at 73 Homewood Avenue, not far from the Wellesley Hospital. It was a large red brick house, purchased at the reduced rate of $17,000 from Joseph West.24 Sections of the house were renovated for laboratory work and a modest amount of research equipment was purchased. Animals were kept in the basement, the first floor "exhibited" specimens in jars, and most work, such as experimental surgery, was done on the second floor.25 Besides Murray, staff included Dr Richard Holden, a 1945 medical graduate from the University of Toronto, and Miss M.S. deWolfe, who recently graduated with a bachelor's degree in biochemistry


23 Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," Toronto Star 24 November 1949; "Medical Research 'Completely Free' Spurred by Bequest," Toronto Telegram 24 November 1949.

24 Wellesley Hospital Archives, W.P. Caven Memorial Research Foundation file, Notes of Miss Arnot, archivist, from discussion with Miss Jean West including pictures, 27 October 1978.
from the University of Toronto. They were hired as Murray's full-time research assistant and lab technician respectively.  

A caretaker lived in the apartment on the third floor. Ralph Pearce, the institute's first caretaker, looked after the building and the animals and assisted with some experimental surgery when Murray needed him. The Caven Foundation was a centre devoted to research, "particularly of the artificial kidney and vascular surgery for which Dr Murray is so famed," said Bruce. Murray's vision was broader; he told others that he was open to any research that investigated the cause and prevention of disease. "Who knows the cause of rheumatism, kidney disease, appendicitis, cancer, varicose veins?" he asked.

Most importantly, the Caven Foundation enabled Murray to work "independent of outside pressures," and he told the press that he was relieved to escape the "prejudices and jealousies" of the university. Bruce reiterated this fact: "research will be carried on with complete freedom, the investigator being responsible only to the Caven trustees." The Caven Board of Trustees was made up of three men: Dr. R.J. MacMillan - a Wellesley doctor, Hugh McLaughlin - a solicitor, and Dr. Herbert Bruce. For years, these men had been sympathetic to Murray's crusade

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25 Mrs. Paul Phelan, Interview by author, 18 November 1996, Toronto; Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.


27 "Medical Research 'Completely Free' Spurred by Bequest," *Toronto Telegram* 24 November 1949; Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," *Toronto Star* 24 November 1949.


29 Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," *Toronto Star* 24 November 1949.
against the university for proper support and recognition of his work. "It is our duty and privilege to provide adequate facilities for a man so endowed, who is willing to devote his time to research work," said the trustees.  

Lord Webb-Johnson, past president of the Royal College of Surgeons, was in Toronto to officially open the Caven Foundation in November 1949. He described Murray as "a young man of genius, a distinguished Hunterian fellow and a Moynihan lecturer, so honored in England." Under his direction, the centre promised new discoveries, future expansion and recognition as one of several leading research institutes in the world. Webb-Johnson, Bruce, McLean, Gardiner and others maintained an unwavering belief in Murray and his research abilities. Murray, now able to work without interference and distraction at the Caven Foundation, "will be able to forge new weapons against disease and death". There was enthusiastic applause from the small group of Toronto business and medical leaders who had assembled for the opening. Among those present were University of Toronto President Sidney Smith, Dean J.A. MacFarlane, and Dr. W.E. Gallie. Murray's anticipated future research success in his new laboratory appeared to signal an end to their personal battles with Murray for research funding and facilities.  

The opening of the Caven Foundation was an occasion not missed by the media. Journalists had celebrated his successes in the past, and, in turn, they heralded his directorship of this independent research institute as a triumph for the individual researcher at odds with the medical establishment. They saw it as a deserving honour.

30 "Medical Research 'Completely Free' Spurred by Bequest," Toronto Telegram 24 November 1949; Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," Toronto Star 24 November 1949.

31 Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," Toronto Star 24 November 1949.
for the famed surgeon, who boldly operated on the hearts of blue babies and saved comatose patients in renal failure with his kidney machine. Headlines announced, "Victory for Dr Murray Seen as New Lab Opens [and] Ends Centralization," "Medical Research 'Completely Free' Spurred by Bequest," and "Noted Doctor is Foundation Research Head." They emphasized the need for more money and research in medicine and pointed out Murray's past battles with the University of Toronto. According to the media, he had been "compelled from the start to pay for his research work himself ... and labored two years in his cellar to perfect his artificial kidney."

The opening of the Caven Foundation, "completely independent of the faculty of medicine at the University of Toronto marked a victory for a prominent section of the medical profession against those in official circles who had refused Dr Murray access to the Banting Laboratories." While the media did not always get it quite right, it made for a good story: it appeared that David had beaten Goliath.

Murray received many letters of congratulations from those aware of his conflicts with the university. Abe Brodey, Murray's former Pharmacology Professor, wrote: "my heartiest good wishes on the success of the Caven Foundation ... I can well imagine some of the organizational difficulties involved in your present set-up but at least you will not have to kowtow to higher authorities whose motives are sometimes

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33Roy Greenaway, "Victory for Dr Murray Seen as New Lab Opens Ends Centralization," Toronto Star 24 November 1949.
not above reproach." His cousin Gladstone Murray said, "the recognition is singularly belated so I hope there are compensating considerations apart from heavy additional responsibilities and anxieties." Others, such as Dr Joseph A. Sullivan, wrote to Murray to offer their support for his decision to leave the university: "I understand exactly your condition ... many years ago when I was attempting to do experimental work at the Banting Institute, I was rebuffed and frustrated on every side ..." Murray was "overwhelmed" by this support. In a letter to Dr R.D. Defies of Connaught Laboratories, Murray revealed, "having run the gauntlet before not too kindly eyes, I found the ploughshares very hot and painful, and at times have wondered whether one's efforts at forward groping were worthwhile. Such expressions of kindness by yourself, as well as by your collaborators in your department are most encouraging and very gratefully received."

Murray was recognized as a competent clinical investigator among his peers, but it was his surgical ingenuity, not his research skills in particular, that impressed them. He had entered the profession at a time when "the best practitioners, not content with being skilled diagnosticians, were trying more than in the past to understand the diseases from which their patients suffered." When Murray


37 Connaught Archives, Robert Davies Defies Papers, 83-07, Correspondence Murray to R.D. Defies, 5 December 1949 and Correspondence R.D. Defies to Murray, 24 November 1949.

38 Years earlier at Johns Hopkins, Halsted had "identified promising young surgeons who had the curiosity and originality to apply the methods of science to the study of the problems of surgery." Would Halsted have selected Murray? A. McGehee Harvey, Science at the Bedside: Clinical Research in American Medicine, 1905-1945. (Baltimore: Johns Hopkins University Press, 1981): 73, 98.
underwent his medical training, clinical research was a relatively new field but most medical men agreed that an improved understanding of disease would lead to better therapeutic practice. For the clinician, doing research meant conducting "a more or less systematic investigation" of the disease with the intention "of improving methods of diagnosis and treatment." Clinicians of Murray's generation had been exposed to basic scientific procedures in medical school, and they combined the work of the clinic and the lab. Older clinicians relied on observation, analysis and deduction in their studies of disease. Traditional clinical research meant the compilation of hundreds of cases and reflection on one's past and broad experiences. By the 1920s, a new, modern style of clinical research emerged, one that incorporated lab analysis to produce clinical data and biopathological concepts. Research became less observational and more experimental. The process of modern clinical research, using techniques and facilities of laboratory scientists, included: first, formulating a problem; secondly, devising an experiment to answer specific questions pertaining to the problem; and thirdly, observing, recording, studying, comparing and analyzing the


40 Keith Wailoo stresses the tensions between the lab worker and the physician, the lab and the clinic. In the case of sickle cell anemia (SCA), Wailoo argues that lab workers studying the chemical environment of the blood cell, the membrane itself and the constituents of the cell could diagnose SCA without ever seeing the patient. Thus the lab was able to transcend clinical thinking and to produce new knowledge; a definite threat to the authority and controlling role of the physician. Clinicians incorporated lab analysis as a subordinate diagnostic tool and as a standardized and routine method for producing clinical data and biopathological concepts. Murray was part of this new, modern style of clinical research, as seen by his heparin investigations. He would never subordinate the clinic to the lab however. See Keith Wailoo, "A Disease Sui Generis: The Origin of Sickle Cell Anemia and the Emergence of Modern Clinical Research, 1904-1924," Bulletin of the History of Medicine Volume 65 (1991): 185-208; Keith Wailoo, Drawing Blood: Technology and Disease Identity in Twentieth-Century America, (Baltimore and London: The Johns Hopkins University Press, 1997).
outcome. As Doctor A. McGehee Harvey wrote, clinical investigation required "imagination, insight, ingenuity as well as clear and consecutive thought."\footnote{A. McGehee Harvey, Science at the Bedside: Clinical Research in American Medicine, 1905-1945. (Baltimore: Johns Hopkins University Press, 1981): xviii.}

During the first half of this century, most clinical investigators were practitioners who did research part-time. They were practising physicians and surgeons who adopted "the experimenter's critical and provisional approach toward therapeutic knowledge."\footnote{Harry M. Marks, The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990. (Cambridge: Cambridge University Press, 1997), p.29.} Their various research interests usually arose out of one of their cases, and piqued by curiosity, they began to experiment. Early clinical work possessed little research methodology and was results-oriented.\footnote{Dr. Ronald J. Baird, Interview by author, 2 April 1996 and 16 April 1997, Toronto; Dr. Edward Mullens, Interview by author, 20 January 1997, Toronto; Personal correspondence, Alison Li to author, 5 February 1997; J.K.W. Ferguson, "Canadian Milestones in Medical Research," Bulletin of the Medical Library Association Volume 48, No 1 (January 1960): 24; University of Toronto Archives, A89-0030/002, File 2: W.G. Bigelow, "Medical Research: A Triple Responsibility," An Address to the Canadian Heart Foundation, reprinted in the Varsity Graduate Volume 12, No 3 (Spring 1966).} In some early studies, physicians simply reported interesting cases and their success in treating the patient with a different approach. As Harry Marks argues, early clinical work revolved around the integrity and expertise of individual researchers to produce reliable, untainted knowledge about disease and the effects of new treatment. By the 1930s and 1940s, university-based medical men began "cooperative investigations" which transcended "the methodological vulnerability of individually conducted research."\footnote{Harry M. Marks, The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990. (Cambridge: Cambridge University Press, 1997), p.9.} Murray followed this trend -- working quite independently during the 1920s to devise improved ways to treat immediate ward cases, and then joining
Charles Best’s heparin research team in 1932 to study the clinical application of this anticoagulant.

The success of wartime medical research, notably the development and clinical investigation of penicillin, created an explosion in the field.\textsuperscript{45} In the ten years after World War II, the number of physicians seeking training in clinical investigation rose tenfold.\textsuperscript{46} With changes in the medical curriculum, undergraduate medical students were now receiving some training in research techniques. At the same time, there was a massive increase in research funding, largely a result of government support, which used and strengthened the existing Canadian research infrastructure. The Banting Research Foundation, established in 1926, had rarely received more than $35,000 a year in funding in the prewar period. By 1958, government and private funding for medical research totaled more than six million dollars, and was distributed exclusively to Canadian university laboratories. Despite these increases, medical research funding was still lacking. In 1959, the Farquharson Committee recommended that 37 million dollars be made available by the government for "the construction of urgently needed medical research facilities in Canadian universities and hospitals". The committee’s report also drew attention to the fact that "50 per cent of the established clinical investigators in Canada lack security of income and tenure". In 1965, a second report entitled "Medical Research in Canada: An Analysis of Immediate and Future Needs" was commissioned by a group of medical scientists to be submitted to the federal...
government in a plea for increasing funding. The government responded with only incremental increases in medical research funding.\textsuperscript{47}

Despite ongoing financial challenges, the field of clinical research continued to grow.\textsuperscript{48} A greater number of clinical investigators were securing full-time appointments in academic settings, and being relieved of teaching and private practice responsibilities. Clinical research methods became more lab-oriented and experimental with improved techniques and instruments. According to Harvey, the full-time clinical investigator had become "the middleman of medical science ... who formed the bridge between the basic scientist on the one side and the practitioner of medicine on the other."\textsuperscript{49} These men acquired training in both the practice of medicine and in the basic sciences, developing research projects relevant to their particular clinical interest.\textsuperscript{50} Postwar clinical investigation, with its "well-controlled" experiments and later double-blind, randomized clinical trials, special facilities,


\textsuperscript{48}The professionalization of clinical science has its roots in the 1920s with the appointment of the first full-time professorships in clinical departments and the emergence of societies and journals, such as The Journal of Clinical Investigation. World War II experiences with medical research resulted in a stronger commitment to research by government, the medical profession, and society, and thus established clinical investigation as a valid field of pursuit for many medical men and further "professionalization" occurred through adaptation of more rigorous standards of investigation, even the development of its own standard procedures such as double-blind, randomized controlled trials. I thank Alison Li for her insights on the changes occurring in the basic and clinical sciences during this time. Personal correspondence, Alison Li to author, 5 February 1997. See also Alison Li, "J.B. Collip and the Making of Medical Research in Canada," Ph.D. dissertation, University of Toronto, 1992.


equipment and expertise, was soon beyond the means of most medical practitioners who "dabbled" in it part-time.\textsuperscript{51} For Murray and other practitioners wanting to pursue clinical research part-time, their greatest challenge was not necessarily their limited training but securing proper facilities and funding.

The Caven Foundation was one of Canada's first independent research facilities.\textsuperscript{52} Until then, serious experimental work had taken place exclusively in research centres affiliated to, or part of, Canadian universities and hospitals. Well-equipped laboratories were only to be found in academic settings, and departments were allocated money from general university funds for research.\textsuperscript{53} There was never enough money for research, and department chairs made tough decisions in distributing funds to some projects over others. Murray certainly had been unsuccessful in receiving enough money from Gallie in the Department of Surgery to


\textsuperscript{52}Dr. C.B. Stewart’s survey of 1948 revealed that medical research in Canada was done mainly in medical schools. However, there was also some work being done in laboratories maintained by pharmaceutical houses, in some hospitals apart from medical schools, in provincial and federal health laboratories, and in special establishments maintained by the Department of National Defence. Stewart reiterated that, “the most significant work is done in the medical schools.” It was also noted that the main aim of medical research projects in 1948 seemed to be the discovery of fundamental knowledge. The Caven Foundation was mistakenly believed to be part of the University of Toronto as all other institutes in Canada at that time were affiliated with a university and/or teaching hospital. For example, research facilities affiliated with McGill University were the Allan Memorial Institute, the Montreal Neurological Institute, the Donner Building; research facilities affiliated with the University of Toronto were the Connaught Medical Research Laboratories, the Banting Institute of Medical Research, the Banting and Best Department of Medical Research; and research facilities affiliated with the University of Western Ontario in London was the Collip Medical Research Laboratory. In a 1974 list of Registered Research Facilities in Ontario, the Gardiner Medical Research Foundation was listed as one of only a handful of private medical research institutes. National Research Council of Canada, Annual Review 1949, p.135-6; Royal Commission on National Development in the Arts, Letters, and Sciences 1949-51, “Medical Research” by G.H. Ettinger, p.318; National Archives of Canada, National Research Council of Canada, Division of Medical Research, RG 77, Series A-1, Volume 279, File 40-1-2, Correspondence E.W.R. Steacie to R.J. Brearley, 16 April 1953; Correspondence P.T.O. to M.E. Parker, 10 April 1953; D.W.G. Murray Papers, MG 30, B 110, Volume 30, File 12, 1974 List of Registered Research Facilities in Ontario, 15 July 1974 produced by the Ministry of Agriculture and Food.

\textsuperscript{53}General university funds consisted of private donations from wealthy alumni, businessmen, philanthropic agencies as well as public monies from the provincial government.
fund his research projects.\textsuperscript{54} In the late 1930s, the Canadian government became involved in and adopted an extramural system of funding medical research, and supported investigations carried out in existing university facilities.\textsuperscript{55} (Britain and the United States supported mixed extramural and intramural systems; they provided funds for research at medical schools and universities as well as created central laboratories such as the National Institute for Medical Research at Mill Hill and the National Institute of Health in Bethesda, Maryland respectively.) The Associate Committee on Medical Research of the National Research Council of Canada, chaired by Sir Frederick Banting, was established in 1938. Eight years later, it became the Division of Medical Research and was chaired by J.B. Collip. It finally emerged independent from the National Research Council of Canada as the Medical Research Council in 1960. In its various forms, the Medical Research Council coordinated and

\textsuperscript{54}During Murray's heparin research in the 1930s, Gallie used departmental funds to pay for Murray's dogs and in 1940 Murray received funding from the Banting Research Foundation for his expenses. Overall, these costs were not high. There was no record of department funding for Murray after 1940. During and after the war, the University of Toronto was one of the more successful institutions in soliciting and receiving federal as well as agency funding for research. University of Toronto Archives, Department of Surgery, A89-0030/001, File (07) Untitled, Correspondence W.E. Gallie to Frederick Banting, 5 October 1936; Correspondence S. Gairns to W.E. Gallie, 1 March 1937; Memo to Department of Medical Research from Department of Surgery, 20 October 1938; Correspondence S. Gairns to W.E. Gallie, 30 December 1938; Correspondence W.E. Gallie to Frederick Banting, n.d. [July 1940]; Notice of Payment, 31 May 1940; "Best Asks More Funds for Postwar Research," Toronto Globe and Mail, 6 January 1943; University of Toronto Archives, Office of the Dean, Correspondence A76-0044/095, File Research (Advisory Committee for Medical Research), Faculty of Medicine Research Funds 1950-51 and Faculty of Medicine Research Grants and Fellowships 1953-54; Toronto General Hospital Archives, W.G. Bigelow papers, Container 39, File 0094-11-0-51, Government of Canada Support for Cardiovascular Research: An Interim Study, n.d. [c.1958].

\textsuperscript{55}Alison Li argues that the National Research Council did not have much choice but to support an extramural programme due to financial constraints and the availability of personnel. There was not a critical mass of researchers and they were widely dispersed, as described by Banting in his 1939 survey of research centres in Canada. Personal correspondence, Alison Li to author, 5 February 1997. See also National Archives of Canada, National Research Council of Canada, Division of Medical Research, RG 77, Series A-1, Volume 279, File 40-1-1, Year End Review 1946 and File 40-1-2 Correspondence J.B. Collip to Thomas V. Grant, 24 July 1947; Archives of Ontario, RG 10, Ontario Ministry of Health, File RG 10-6-0-1529 and File RG 10-6-0-1530 Medical Research Projects in Canada 1949-1963. (Check out: Banting, Frederick and C.B. Stewart. Survey of Facilities for Medical Research in Canada. Ottawa: National Research Council, 1939, 1948, 1958. — three reports)
provided funding for experimental work, stimulating and institutionalizing medical research in Canada.\textsuperscript{56}

In some instances, special research departments or institutes within universities were set up in an attempt to provide proper facilities and funding for medical research.\textsuperscript{57} After the discovery of insulin, the University of Toronto created a Research Chair for Frederick Banting, which later became the Banting and Best Department of Medical Research. Soon after, the Banting Research Foundation was established in Toronto to provide support for future medical breakthroughs. Money raised by the Foundation was intended "to give Banting further support in his research, and also to support future Bantings, at Toronto and other Canadian universities, who had good ideas but no money."\textsuperscript{58} The Montreal Neurological Institute (MNI) was opened in 1934 under Wilder Penfield, the famed neurosurgeon who had developed the "Montreal Procedure" to relieve epilepsy. The MNI was a hospital and research facility, affiliated with McGill University, and built with money from the Rockefeller Foundation.\textsuperscript{59} J.B. Collip, co-discoverer of insulin, was appointed Director of the Research Institute of Endocrinology at McGill University in


\textsuperscript{57}The establishment of special research departments or institutes within universities was a common practice in nineteenth century Germany, and later other European and North American universities.

\textsuperscript{58}Michael Bliss, Banting: A Biography (Toronto: McClelland and Stewart Ltd., 1984), p.152-3.

Also during the 1940s, a beautifully equipped Department of Experimental Medicine and Surgery, similar in programme to the Department of Medical Research at other universities, was established at the University of Montreal under the directorship of Professor Hans Selye. Banting, Best, Penfield, Collip and Selye were able to secure research funding and facilities to continue their work, to a large part due to their reputations, and enjoyed the stability and recognition that came with being directors of medical institutes. This was what Murray wanted.

Murray sought designated space and funding for his clinical investigations, and the Caven Foundation offered that at a time when the university would not. In Murray's view, the university had missed its chance to build a world-class cardiovascular centre or heart institute, and he would not compromise on his demands for autonomy as head of a research division. In actuality, it was Murray who failed to realize and seize a golden opportunity when he turned down the Surgical Research Director position under Janes. After that failed attempt, no other academic research post would be forthcoming at the University of Toronto and he realized that he would have to go elsewhere. The Caven Foundation, a small private laboratory, held the promise of something grander for Murray. The Ontario Minister of Health made overtures of support that, although never amounting to anything, aroused thoughts of a new modern research institute in Murray's mind. Architectural designs of an eight to

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63 Mrs. Paul Phelan, Interview by author, 18 November 1996, Toronto; Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.
ten storey facility were drawn, and Murray and his staff spoke often of the future centre. He planned to bring in the best people, of international calibre, as well as younger men that had trained under him in Toronto.

In the meantime, Murray accepted his lot as it was in 1949 and got on with the task of conducting research. In the first few years, Murray and Holden did a great deal of experimental surgery, mostly transplantation of the kidney. They also began investigating the growth of bone, and these modest results were eventually published in the American Journal of Surgery. Miss deWolfe studied cancerous tissues and attempts were made to grow human tumours in the yolk sac of chicks, and later mice, for the purpose of study. Initially, DeWolfe spent little time with Murray, studying with Dr A.W. Ham at the Banting Institute before transferring the research to the Caven Foundation. Murray could only pursue his research work at the Caven Foundation part-time. He was still juggling his time between 73 Homewood Avenue, Ward C at the Toronto General Hospital, and his private surgical practice.

Less than two years after the opening of the Caven Foundation, the trustees -- Bruce, MacMillan and McLaughlin -- expressed their concern for the future of the

64John T. Phair, Ontario Deputy Minister of Health, had been present at the Caven Foundation opening in November 1949, but was not able to commit government funds to the centre. Mackinnon Phillips, Ontario Minister of Health, hinted to Murray about an electron microscope (approximately $1 million) and talked about the prospect of building a "Gordon Murray Institute". By 1960, Walter Roschlau was through waiting, Murray had been "stringing him along" for years saying that the Gordon Murray Institute would not happen just quite yet. Dr. Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.


67National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 32 W.P. Caven Memorial Research Foundation, Minutes and Correspondence, 12 July 1951; File 31 W.P. Caven Memorial Research Foundation, Minutes 1951; Gordon Murray, "The Clinical Results of Serum
institute. Holden had resigned and Murray had accepted an offer to embark on a two-month teaching trip to Australia and New Zealand. They began to question Murray’s dedication to the research centre. There had been "no great discovery", and Murray had yet to present any research contributions to the medical community under the auspices of the Caven Foundation. Clearly this had been the intention of establishing a private laboratory for Murray. Bruce, MacMillan and McLaughlin inquired whether the facilities were being used to its fullest. More directly, they asked Murray whether or not he had the time to devote to research at the Caven Foundation. Murray replied that, "it would be impossible for him, and that it was not his intention, to devote more time to the research being carried on at the Caven Foundation than he had devoted in recent months". But "if there was dissatisfaction which results in the winding up of the Foundation he would himself carry on research some place else." Several months previous, Murray had asked for additional senior research workers but had not received any. Again, Murray was quarrelling with those above him. In the end, the trustees did not terminate the foundation and they left Murray to replace Holden and carry on with the research in progress.68

In the years that followed that strained meeting, Murray became more adept at playing the game. That is, he became more oriented towards displaying the centre’s productivity when need be, which pleased the trustees, and he made sure to acknowledge the support of the institute and its workers, which raised the profile of the Caven Foundation. Additional staff joined the centre -- Dr Walter Roschlau became Murray’s new research assistant, a position he held for nine years. A

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biochemist was hired, who with the lab technician, focused on tissue chemistry and the study of cancer antigens in malignant tumours.

For all his hopes of attracting high-calibre researchers to the Caven Foundation, Murray could not entice them with a high salary or compete with the stature and resources of working in a university setting. Nevertheless, he believed leading researchers would come anyway, telling the Caven Foundation Board of Trustess: “men seemed willing to make research their career at modest salaries.” Roschlau, a recent German immigrant, was hired after responding to an advertisement in the Canadian Medical Association Journal. He received an initial wage of $150 a month (equivalent to the monthly rent of his Rosedale apartment), which was increased to $300 a month the following year.69 Comparative to university research positions, salaries were certainly modest at the Caven Foundation.

Then, in 1953, Murray suddenly resigned from the Toronto General Hospital as head of service for Ward C.70 More than any other factor, this event shifted the direction and increased the pace of research at the Caven Foundation. Murray spent more time at the centre, and began to pursue new research projects with greater interest and commitment. He also became more involved and dictatorial in the daily running of the Foundation, and all work was driven by Murray’s current project.


70National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 28, File 32 Correspondence Murray to J.A. MacFarlane, 15 October 1953; Correspondence Sidney Smith to Murray, 27 November 1953; Correspondence Joseph A. Sullivan to Murray, 27 November 1953.
Overall, a substantial amount of research work was being done at the institute by a relatively small number of researchers. Full staff included Murray, a research assistant, a lab technician, a biochemist and the caretaker.71 At times, visiting researchers and students became involved in different projects.72 Naturally, some investigations were given higher priority than other research. Murray's interest during the early 1950s was directed towards new cardio-vascular procedures. His work in this area resulted in substantial contributions to the field, notably coronary artery anastomosis and transplantation of the aortic valve.73 At the same time, a new model of Murray's artificial kidney machine was also developed, largely due to Roschlau's full-time efforts on the project in 1952 and 1954.74 Sideline projects for Murray

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71 In some years, there were vacancies due to lack of funds or inability to find a suitable candidate to fill the position. The following individuals were employed by the Caven Foundation during Murray's tenure: Dr. Richard Holden, Dr. Walter Roschlau and Dr. Eileen Graves as Research Assistants; Miss M de Wolfe, Mrs Inge Wolf, and Miss Eliane De Cock as Lab Technicians; Mrs. Beverly Reichert and Robert J. Oliver as Biochemists; Ralph Pearce, Nicholas Korponay and Cecil Wilding as Caretakers. Murray did not have a problem employing women, single or married, for any position. None of these positions paid particularly well though, and perhaps this explains the high turnover of staff and the strong representation of women. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 31 W.P. Caven Memorial Research Foundation, Annual Report 1951, 1952, 1953, 1954, 1955, 1956, 1958.

72 Few doctors ever visited the Caven Foundation. Only a few students of Murray's, such as Dr Ray Heimbecker, were welcomed and most of Murray's medical colleagues knew little about his research. Those who visited the private lab were mostly financial men and the first floor was set up to showcase various examples of their research (ie. specimen jars). Dr. Raymond Heimbecker, Interview by author, 1 November 1996, Toronto; Mrs. Paul Phelan, Interview by author, 18 November 1996, Toronto.


included experiments on the new growth of bone in cavities\textsuperscript{75} and investigations into fat embolism.\textsuperscript{76} Those individuals working on biochemical experiments with Murray were involved in a range of investigations including cholesterol analyses, anticancer serum, tumour transplantation in mice, tumour induction in mice, glycogen analyses of dog hearts, blood glucose and lactic acid levels in rabbit blood, and non-protein nitrogen determinations.\textsuperscript{77} These "sideline projects", notably Murray's cancer research, gained greater priority by the mid-1950s.

In 1954, J.S. McLean died. That same year, the Caven Foundation received the last instalment of McLean's five-year funding commitment to the institute. Now that this support was no longer forthcoming, the trustees -- Bruce, MacMillan and McLaughlin -- decided to terminate the work of the Foundation at 73 Homewood Avenue.\textsuperscript{78} According to Bruce, they wanted to preserve their $100,000 capital investment. They decided to discontinue the laboratory in favour of using the money to provide fellowships in research, under the control of the Medical Faculty at the University of Toronto, in the newly established Cancer Research Centre attached to the Wellesley Hospital.\textsuperscript{79}


\textsuperscript{78}National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 28, File 33, Correspondence Hugh J. McLaughlin to Murray, 21 May 1954.

\textsuperscript{79}Herbert A. Bruce, Varied Operations (Toronto: Longmans, Green and Company, 1958): 178.
Money had been a constant issue, and the Caven Foundation had relied exclusively on private donations to cover its operating expenses. Annual donations included McLean's $15,000 contribution and were topped up by other supporters, reaching as high as $27,500 in one year. In reality, this was a modest amount to run a laboratory, almost half of it going to pay salaries. (Murray did not receive a salary.) The institute's capital and operating expenses almost always exceeded its income. Murray had difficulties in managing money -- there was never enough of it --- and he often dipped into his own pockets to cover expenses. Moreover, Bruce was having other problems with Murray. The Board of Trustees still found Murray uncooperative, secretive, and "too hard to guide and control": "Murray would never confide what he was going to say or do." These complaints were not new; Murray had been criticized for this behaviour throughout his career. The institute had simply not developed as the trustees had envisioned back in 1949, and it was time to pull out.

It was a difficult summer for everyone who worked at the Caven Foundation. There was no five-year anniversary celebration since "the fate of the Institute was very undecided". Nor was there any feeling of "encouragement" that the laboratory would continue to exist. Financier Percy Gardiner eventually rescued the foundation from certain closure. Gardiner was one of Toronto's well-known philanthropists, making

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80 For the Year 1949: Income = $105,000 (Caven Bequest); Expenses = $44,286;  
For the Year 1950: Income = $19,500; Expenses = $23,889;  
For the Year 1951: Income = $17,500; Expenses = $15,772;  
For the Year 1952: Income = $27,500 ($10,000 not until 1953); Expenses - $15,745.  

81 Ironically, Murray and his difficult personality, who had been Gallie's and Janes' administrative problem and for whom Bruce has lobbied in support of years earlier, was now Bruce's vexation.  
Hannah Institute for the History of Medicine Oral History Collection, Volume 5, Mrs. Herbert Bruce, p.81-82.

large contributions to a variety of charities and causes from the arts to sports. In the past, he had made sizeable donations to the city hospitals, the university and medical research --- including Murray's earlier work. The Caven Foundation became the Gardiner Medical Research Foundation on August 1st, 1955, but there was little fanfare over the change. Murray was pleased that his laboratory would remain open, and he found Gardiner to be pleasant and supportive of his work.

Percy Gardiner supported the Foundation for another ten years, although at times reluctantly. In 1955, he had been intrigued by Murray's ideas of a cancer serum and Murray's initial success with a few "hopeless" cases. In the beginning, Murray had said that he needed "to carry on for a couple of years at least, either to prove or disprove the efficacy of a horse serum produced under his direction for cancer in humans." Gardiner bought the house from the Caven trustees and agreed to donate $12,500 a year for operating expenses in order to allow Murray to conclude his cancer experiments over the next few years. Gardiner's support allowed the institute to remain open, but it was less than ideal. It was not enough money to run a "cutting-edge" research laboratory. Gardiner himself "felt frustrated on account of our lack of equipment and our inability to hire associates or technicians experienced in the field.

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84 At a dinner in Gardiner's honour in 1963, Murray said, "Personally I am greatly indebted and in addition have the warmest feeling toward him. [Gardiner] His kindness and understanding are beyond words to express. As well he has been most generous in supporting my research for what it is worth." National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 41, File 18 Toast made by Murray to Gardiner, 12 February 1963. See also "Medical Research Foundation Set Up," Toronto Telegram 30 November 1955; National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 33 Gardiner Medical Research Foundation, Annual Report 1955; Gordon Murray, Quest in Medicine (Toronto: The Ryerson Press, 1963): 99.
of virology due to our limited financial resources. By 1961 Gardiner wanted out; he had been supporting the laboratory longer than he had originally intended, but did not want to abandon Murray and his work at a crucial juncture. He asked Murray to complete his current investigations within the next six months, but if this was not possible, added that he would extend his support. It became a yearly dialogue between Gardiner and Murray; "we originally intended to discontinue activities at the Laboratory this year but inasmuch as some interesting work remained unfinished I was both willing and pleased to extend my financial support [for yet another year]," wrote Gardiner.

In 1965, Percy Gardiner died. Fortunately for Murray, Gardiner had made arrangements for Murray to use the house on Homewood Avenue for as long as he wished. Still Murray needed funding for operating expenses. Granting agencies such as the Medical Research Council, the Heart and Stroke Foundation, the National Cancer Institute, the Canadian Arthritis and Rheumatism Society, and other philanthropic foundations required applicants to complete detailed research proposals and reports. Murray was unwilling to do this. He refused to divulge his research plans so someone else, younger and better financed, could take his ideas and conduct

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88 National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 26, Correspondence Percy Gardiner to Murray, 6 April 1964; Volume 30, File 8, Correspondence Murray to L.V. Kilmury, 16 March 1970.
the work. His experience with his stolen artificial kidney designs still haunted him. He was extremely frustrated by these new rules of grantmanship. He felt that funding should be awarded to him based on his reputation and past research accomplishments. Murray was part of the past tradition in which money was directed towards talented individuals, such had been the case with Banting, in hopes that more life-saving discoveries would be made. Now grants were awarded based on the merits of the research proposal. There was also a growing trend towards collaborative research efforts; the research team was more successful in securing funding and producing results than the individual investigator who was quickly becoming obsolete.

After Gardiner's death, Murray's work was supported predominantly by the J.P. Bickell Foundation as well as private donations. Friends such as the Micheners were constantly trying to find other possible granting agencies for Murray. By 1970, operating expenditures had been reduced to a lean $10,000 a year, due to the

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90 Recently, Gerald Freund called for a return to this approach: "foundations should take as their central task the discovery and support of exceptionally talented individuals ... gifted administrators, like Ford's McNeil Lowry and Rockefeller's Warren Weaver, roamed the country aided by a network of unpaid talent scouts to identify gifted young scientists and scholars who could blossom with the aid of generous grants and achieve great eminence in their fields." Taken from Derek Bok, Review of Narcissism and Philanthropy: Ideas and Talents Denied (New York: Viking Press, 1996) in New York Times Book Review, 1996. See also Alison Li, "J.B. Collip and the Making of Medical Research in Canada," Ph.D. Dissertation, University of Toronto, 1992, p. 201.

91 The J.P. Bickell Foundation was established in 1951, with an endowment of thirteen million dollars, upon mining executive J.P. Bickell's (1884-1951) death. As of 1996, the Bickell Foundation had distributed more than 75 million dollars, mostly in the fields of health and education. "Ontario arts get big boost [Bickell Foundation]", Toronto Globe and Mail 26 June 1996; National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 49, File 11 Correspondence Murray to Rolland Michener, 8 January 1965; Volume 47, File 29 J.P. Bickell Foundation Report 1965 and 1967 (Murray received $12,000 each year.); Volume 30, File 7 Correspondence P.J. Sewell to Murray, 1 April 1969 (Murray received $4,000 that year); Volume 47, File 25, Correspondence Murray to S.E. Bernier, 12 February 1970.
Bickell Foundation's decision to reduce their funding. It was becoming more and more difficult to meet his costs. Murray hated trying to eke out enough money each year in order to do his research. Members of the business community, aware of Murray's chronic lack of funding, made small personal donations but they saw this as only a temporary, piecemeal solution to a much larger problem. They recommended that Murray retain someone to sort out his financial records, outline a budget and make plans on how to secure the appropriate amount of money. "This will relieve you of a great deal of the burden of ways and means and leave you free to pursue the more important objectives of the foundation," urged one supporter. Murray ignored the advice.

The medical community had expressed concern over Murray's age, isolation and field of research, and this was enough to make philanthropists shy away from investing money in his various projects. In the early 1970s, there was a glimmer of renewed interest. Norah Michener had approached Floyd Chalmers who in turn was interested in donating up to $30,000 to the Gardiner Foundation. He had a few


93Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.

94National Archives of Canada, MG 30, B 110, Volume 52, File 9 Correspondence L.M. Nichols to Murray, 8 September 1970 ($5,000); Correspondence H.R. Jackman to Murray, 20 October 1970 ($2,500); Correspondence Mrs. Pauline Harris to Murray, 18 December 1970 ($500); Correspondence H.C. Hiltz to Murray, 12 January 1971 ($500); Correspondence H.R. Jackman to Murray, 11 March 1971 ($7,000 Bickell Foundation); Correspondence H.R. Jackman to Murray, 24 December 1971 ($2,000); Correspondence K.B. Andras to Murray, 28 December 1971 ($2,000); Correspondence H.R. Jackman to Murray, 12 December 1972 ($3,500); Correspondence K.B. Andras to Murray, 27 December 1972 ($2,000); Correspondence H.R. Jackman to Murray, 27 December 1973 ($2,500); Correspondence K.B. Andras to Murray, 27 December 1973 ($1,700); Correspondence H.R. Jackman to Murray, 28 December 1973 ($2,500); Correspondence Mrs. G.H. Forster to Murray, 12 June 1974 ($100).

conditions which he wrote out to Murray: hire "... some younger researcher in the
field of immunology to work with you ... [hire] a part-time secretary to keep the
records in order ... [and] there must be closer association between your work and that
which is being done by other doctors in the other fields of immunology." Murray
wanted to hire a younger researcher and, the year before, had tried to interest a former
employee, Eileen Graves, to return to work with him. Murray wrote, "there is the
possibility of expanding the lab and facilities. With the world wide interest in
immunity in cancer, we might lead the way to make this a centre to diffuse
information and also a great centre for treatment of great numbers of patients. The
nihilistic attitude that stopped the project in the past seems to have subsided and there
is a general search in this direction ... would you consider returning to 73 Homewood
to take part in what I hope might become a great institution?" Graves, having now
completed her medical training, did not accept Murray's offer.

Nothing came of it all, and increasingly Murray began using his own money to
pay for his research expenses. By this time, the Gardiner Foundation was little more
than a run-down house, with outdated research equipment, and no staff under the
direction of a seventy-year old surgeon. Any hope that Murray had had for a thriving,
independent institute, which supported leading research work by numerous talented
research scientists and clinicians, as his legacy, was long gone. Due to "high taxation

96 National Archives of Canada, MG 30 B 110, Volume 30, File 11 Correspondence Norah Michener

97 National Archives of Canada, MG 30 B 110, Volume 30, File 10 Correspondence Murray to

98 National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 8,
Correspondence Murray to L.V. Kilmury, 16 March 1970; Volume 47, File 25, Correspondence
Murray to S.E. Bernier, 12 February 1970.
and low income", the Gardiner Medical Research Foundation was closed on June 30, 1974.

As Director of the W.P. Caven Memorial Research Foundation, later the Gardiner Medical Research Foundation, Murray worked outside of the traditional academic structure. What might have been a successful research alternative failed because Murray was unable to adjust to the many changes occurring in medical research funding, facilities and conduct in the post-war period. The university remained the dominant setting for basic and clinical medical research in Canada, an extramural funding strategy chosen by the Associate Committee on Medical Research of the National Research Council, later the Medical Research Council. As increased public and private funding was directed towards medical research, a new process of detailed research proposals and reports was put into place. Murray chose not to participate in this new system of grantmanship; he belonged to the older generation of researchers who expected support based on their professional standing and past accomplishments. He was envious of the "untold amounts of money" awarded to research in the United States, and disliked having to depend on the generosity of businessmen to fund his work each year. 99 After leaving the Toronto hospital structure, the concept of individual autonomy and control was again muddled.

99To a colleague in Norway, Murray wrote, "I had limited facilities at the University of Toronto ... [and] little and practically no financial support ... Later on jealousies in the surgical department suggested that I might be more free to work in a laboratory outside of the University jurisdiction ... I had a small laboratory set up just in a private house on a very ordinary street in Toronto. In this tiny institute with very limited financial support and only a few assistants, we have struggled ... I have been much impressed with the state of affairs in research in the United States ... the Americans have untold amounts of money available, assistants in profusion and technicians of all sorts in abundance ..." National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 29, File 3 Correspondence Murray to Carl Semb, 13 December 1960.
Murray was never a full-time clinical investigator. Nor did he ever acquire the increasingly specialized research techniques and methodological training that postwar clinical investigators received. Yet he had a researcher's outlook. "Research is fundamentally a state of mind involving continual reexamination of the doctrines and axioms upon which current thought and action are based. It is, therefore, critical of existing practices," wrote Theobald Smith in 1933. Murray's vision of surgical possibilities inspired much of his research work. He was unusual, experimenting when clinical investigation was in its infancy during the 1920s. As defined by the research community of the 1920s and even the 1930s, Murray conducted well controlled experiments, which Harry Marks describes as: "ones in which a knowledgeable and experienced investigator had anticipated the multitude of factors that might affect the outcome: patient selection, dosage, laboratory technique, and the natural history of the disease." As well Murray performed experimental operations on animals, the model of the well-controlled experiment, before attempting the new procedure on patients. At times he pushed the limits, in some cases operating on only a few animals, sometimes unsuccessfully, before trying it out on the patient. He was an original, but undisciplined researcher. For Murray, the ultimate test for any new procedure or discovery was to take place in the clinic, not the laboratory. Murray was results-oriented; he was not concerned with compiling statistics as became the trend in the 1950s. Changes in clinical research were taking place; the subjective


102 Murray's heparin research developed through numerous series of experiments — methodical research conducted over many years. Some of Murray's later research was less rigorous: consider his early heart operations of the 1940s and his kidney transplantation in 1952.
judgment of individual investigators was being replaced by a new impersonal and objective standard of scientific integrity — the double-blind randomized, controlled clinical trial. According to Marks, "Considered as an attitude, experimentalism was potentially accessible to all. Considered as a technology, however, experiments were the domain of the few --- experts with the training, intelligence, and resources to produce and interpret their findings." In the end, Murray was more clinician than scientist. He continued his old style of clinical research and never fully adapted to the new postwar standards of investigation.

Moreover, medical research was being transformed from being "an activity pursued by a few, lone investigators with little financial aid and institutional support to a profession, carried out by full-time workers in large teams, with systematic governmental funding and established institutional roles." Murray was not necessarily resisting this transition -- in fact he had been trying to "team-build". He would have preferred to stay in a university setting, but with greater financial and research autonomy. Murray wanted absolute control of his projects. When this was not possible at the Toronto General Hospital, he hoped to build a large and successful institute, independent of the university, in which stable funding, facilities and staff would allow him to pursue his various research projects. The modest lab on 73 Homewood Avenue did not become that institute.


104 Dr Ronald J. Baird, Interview by author, 2 April 1996 and 16 April 1997, Toronto; Dr. Edward Mullens, Interview by author, 20 January 1997, Toronto; Dr. William Geisler, Interview by author, 17 April 1997, Toronto; Dr. D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto; Personal correspondence, Dr. N.T. McPhedran to author, 23 January 1997.

For 25 years, Murray conducted experiments at the Gardiner Medical Research Foundation before it eventually closed. In the end, it was little more than a third-rate institute, substantially smaller and inferior compared to university laboratories, and represented a desperate attempt to continue his research.\(^{106}\) It offered him freedom to conduct various research projects, but in the end, further alienated and isolated him from the changes occurring in clinical research. He became the 'lone investigator', working with limited equipment and expanding into new areas for which he was not trained. He became increasingly secretive about his investigations, and did not make any attempts to coordinate his work with other researchers elsewhere.\(^{107}\)

Murray had always maintained a wide range of research interests, stimulated by the variety of disorders presented to him by his patients; however medical specialism had begun to stake out professional territories.\(^{108}\) Murray was impervious to these increasingly rigid boundaries, trespassing without apology. "Perhaps one's pursuit for the cause and prevention of disease which might make inroads on the type of practice to which the profession has settled down in this country does not make this a popular theme. It may seem ridiculous that a mere surgeon would have the audacity to revel in such ideas," he wrote.\(^{109}\) His boldness manifested itself in his later research projects, which in turn led to tremendous controversy ... beginning with his cure for cancer.

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\(^{106}\) Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto; Dr William Geisler, Interview by author, 17 April 1997, Toronto.

\(^{107}\) Dr. E. McCulloch said, "I know that he [Murray] did a lot of research and I know that much of it didn't have the impact it should have." Hannah Institute for the History of Medicine Oral History Collection, Volume 30, Dr. Ernest Armstrong McCulloch, p.16.


\(^{109}\) Connaught Laboratories Archives, Robert Davies Defries Papers, 83-07, Correspondence Murray to R.D. Defries, 5 December 1949.
Chapter 6

A Cure for Cancer? Sera, Vaccines and the Theory of Immunity

Cancer was claiming an alarming number of Canadian lives by the mid-twentieth century. In the 1930s, heart disease and cancer had replaced influenza and tuberculosis as the country’s top two fatal diseases and cancer had become one of society’s most feared illnesses. It was the “dread disease” that brought painful and lengthy infirmity, harsh treatments and almost certain death.1 Historically, many scientists and medical men have studied the disease and unsuccessfully worked towards finding a cure. Lay groups and physicians formed various cancer organizations during the 1930s, but made few inroads on the disease.2

1See James Patterson, The Dread Disease: Cancer and Modern American Culture (Cambridge: Harvard University Press, 1987).

2The following attempts were made to organize and coordinate lay and medical efforts towards the problem of cancer in Canada during the 1930s: the appointment of the CMA of a Study Committee on Cancer in 1931; the inauguration of the King George V Silver Jubilee Cancer Fund in 1935; the formation of the Canadian Cancer Society in 1938; the formation of the Department of Cancer Control of the CMA in 1938; the Ontario legislature passed the Cancer Remedy Act in 1938 and the Commission for the Investigation of Cancer Remedies was set up that same year. The war diverted attention away from these organizations. National Archives of Canada, MG 28 I 224 National Cancer Institute of Canada, Minutes of the Third Annual Meeting, 16 May 1949; Barbara Clow, "The Problem of Cancer: Negotiating Disease in Ontario, 1925-1945," PhD Dissertation, University of Toronto, 1994.
During the 1940s, a greater effort was made to coordinate cancer research, funding and education at a national level.\(^3\) Dating back to the First World War, the language of war was carried over into peacetime efforts against medical problems, including cancer.\(^4\) According to politicians and medical professionals, the 'battle against cancer' was underway, and they began to mobilize against the disease.

National agencies and conferences were set up to coordinate 'the fight' against the second leading cause of death in Canada. In 1947, the Minister of National Health and Welfare, Paul Martin, was behind this 'national plan of attack'. He promoted two phases of the programme: improvement in diagnosis and treatment as well as research into the cause of the disease with "the ultimate objective of wiping it out."\(^5\)

The National Cancer Institute of Canada (NCI), later the Canadian Cancer Institute, was formed in 1947 and the resources of the King George V Cancer Fund

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\(^3\)In 1947, the Honourable W.G. Blair told the House of Commons that, "Every year, cancer kills approximately 14,000 Canadians ... Canadian citizens are demanding action ... The problem will have to be met in the same way and with the same energy as research workers discovered the secrets of radar and atomic energy." House of Commons Debates, 4 March 1947.

Under the auspices of the National Research Council, the Associate Committee for Medical Research, chaired by Frederick Banting, was established in 1938. After World War II, national coordination and funding of various projects and causes by the federal government increased. As argued by Barbara Clow, "Investment in the battle against cancer increased as did the institutionalization of cancer care, and the opinions of the scientific and medical communities came to dominate the management of the disease." Barbara Clow, "The Problem of Cancer: Negotiating Disease in Ontario, 1925-45," Ph.D. dissertation, University of Toronto, 1994, p.10. For more on medical research in general, see Alison Li, "Expansion and Consolidation: The Associate Committee and the Division of Medical Research of the NRC, 1938-1959," Building Canadian Science: The Role of the National Research Council edited by Richard A. Jarrell and Yves Gingras. Special Edition of *Scientia Canadensis* Volume 15, Number 2 (1991): 89-103.

\(^4\)Deborah Lupton argues that the language of warfare is common in modern medical and public health discourses, and can be traced back to World War I and campaigns against syphilis. In regards to cancer, military metaphors worked well because of the disease's "severity, mystery and evasion of medical solution" — it appealed to the public's sense of fear against the threat of cancer. Deborah Lupton, *Medicine as Culture: Illness, Disease and the Body in Western Societies*, (London: SAGE Publications, Ltd, 1994), pp. 61-66.

were put at the Institute's disposal.6 The Ontario Cancer Treatment and Research Foundation (OCTRF), formed in 1943, opened its first clinic in Kingston in 1947, and later other centres throughout the province.7 At times, the work of the two organizations overlapped in Ontario. The NCI tried to coordinate and fund basic research, while the OCTRF focused on the clinical aspects of diagnosis, treatment, and investigation.8 By the end of 1947, Martin boasted that research on 28 separate cancer projects was being carried out in medical colleges and clinics across Canada, "in order that a cure for this dread disease may be discovered."9

Cancer research in Canada had begun long before the 1940s. One such researcher interested in the disease had been Frederick Banting. Who better than the discoverer of insulin to find the cure for cancer, thought the public? As early as 1922, Banting began thinking about cancer after a trip to England and a visit to the laboratory of cancer researcher William Gye. In 1926, he began experiments to create cancer, by transmitting Rous sarcoma, and then directed his attention to finding a way of stopping its creation by discovering some agent -- vaccine, serum, antitoxin,

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6The policy of the Institute was: "the promotion, stimulation and fostering of fundamental research and personnel training in the field of cancer with a view to assisting in the reduction of cancer morbidity and mortality." The Board of Directors agreed that it was "not a matter of just receiving applications and granting money, but the Institute should initiate new projects and contact research men in regard to them." [my emphasis] Later, they became more "neutral" and did not approach researchers to offer funding. Interestingly, they also agreed that a major emphasis should be laid upon cancer of the breast due to increasing number of women dying from the disease. National Archives of Canada, MG 28 I 224 National Cancer Institute of Canada, Minutes of the First Annual Meeting, National Research Council, Ottawa, 30 September 1947.

7University of Toronto Archives, A81-0047/004, Ontario Cancer Institute 1951-65, Brief of the Ontario Cancer Treatment and Research Foundation to the Royal Commission on Health Services, 16 May 1962.


something. Between 1928 and 1934, 1,768 birds were used in Banting's experiments. Of these, one bird in 164 became resistant to Rous sarcoma. Banting preferred to use the term "resistance" rather than "immunity" where cancer was concerned, and he never produced a "cure."10 Similarly, Murray became interested in cancer while in England when doing his post-graduate training in the 1920s, although he never worked with Gye.

Like Banting, Murray was intrigued by Rous sarcoma, a strain of cancer caused by infective agents, later known as viruses. In London, he studied the reaction of lymph tissues before and after invasion by malignant cells. He did not complete these experiments, yet "the ideas were kept active" and he planned on investigating the problem of malignancy at a later date.11 Murray returned to this research question in 1949, no doubt influenced by the national and provincial campaigns against the disease at this time. It became his longest research project, spanning the entire period that the Caven and Gardiner Foundations were in operation.

Murray approached the problem of cancer as a clinician, wanting to provide cancer sufferers with a viable treatment. His early research convinced him of the possibility of host immunity (protection against contracting the disease through antibodies) and resistance (diminished tumour growth through antibodies) to cancer.12

10By the 1930s, more and more researchers were trying to make the same discovery, but as Michael Bliss states, they "were finding the enigma of cancer in general, and these strange virus-induced cancers in particular, overwhelmingly complex." Michael Bliss, Banting: A Biography (Toronto: McClelland and Stewart, 1984), p.157, 211; National Archives of Canada, RG 29 Department of Health and Welfare, Central Registry Subject Files Series, Volume 188, File 311-C1-1 Epidemiology - Disease - Cancer - General, "Progress Is Seen In Experimental Cancer Research," Toronto Globe and Mail 2 November 1938.


So he began experimenting with anti-cancer sera and vaccines. This work represented a complete departure from his past investigations. It took him outside the domain of surgery and into the area of immunology. Despite criticisms from his colleagues for 'dabbling' in a field outside his expertise, Murray reported some sensational clinical results, extending patients' lives by months and years. Public excitement and praise for what appeared to be yet another medical triumph for Murray prompted government and philanthropic support of his research. Had Murray discovered a cure for cancer? As one journalist reported in 1955, "Murray only dares hope that his discovery of the cancer serum will be the crown of his career."\(^{13}\)

Murray had treated cancer surgically for years.\(^ {14}\) He believed cancer was an "incurable and highly fatal disease", and that conventional treatment, useful to a point, cured very few. He did not understand why so many researchers and doctors continued to advocate "such forms of treatment that have no hope of offering a cure."

Now that he was Director of the Caven Foundation, Murray had the facilities and money to "embark in a new direction" in cancer research.\(^ {15}\) In 1949, Murray and his

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\(^{14}\)During the early part of Murray's surgical career, the medical profession endorsed only two treatments for cancer, surgery and radiotherapy, despite Frederick Banting's (as well as other's) belief in the possibility of resistance to cancer due to ongoing research with animals. A 1938 newspaper article stated: "Many cures, some reputable, many frankly experimental, and mostly "quack" have been publicized. But conservatively speaking, there are only two cancer 'cures' — the knife and radiation. Neither of them is set forth as a cure by any reputable member of the medical profession." National Archives of Canada, RG 29 Department of Health and Welfare, Central Registry Subject Files Series, Volume 188, File 311-C1-1 Epidemiology - Disease - Cancer - General, "Progress Is Seen In Experimental Cancer Research," *Toronto Globe and Mail* 2 November 1938.

staff began to investigate tissue malignancy as one of the Institute's several secondary research projects (after the artificial kidney machine and cardiac procedures).

Murray approached cancer as if it were an infectious disease. This theory or approach to the causation of cancer was not new. During the nineteenth century, the theory of the parasitic origin of cancer gained credence after the identification of specific agents in various infectious diseases, such as malaria, diphtheria, pneumonia, cholera and typhoid fever. However, by the 1910s researchers such as Peyton Rous and Paul Ehrlich were unsuccessful and extremely discouraged in their attempts to isolate a virus as the causation of human cancers. Cancer immunity was proving to be no more than immunity against the transplantation of a strange tissue, whether normal or cancerous.\(^{16}\) By the 1950s, many researchers had turned their attention to the cellular and molecular biology of cancer. Murray's research was based on the viral theory of cancer at a time when it was not a mainstream approach. He strongly argued that a toxin or poison was present in the malignant tissue, invading and overcoming healthy cells, and theorized that "there is a possibility that an anti-body might be made which would neutralize or overcome the toxin or poison." The development of vaccines and anti-sera against various toxoids and diseases, such as diphtheria, had proven effective in the past. Therefore, surmised Murray, "I decided to try to produce an antibody against this substance [toxin] coming off the cancerous area, an antibody against the cancer itself."\(^{17}\)

Murray's plan was to inoculate horses with human cancer, to allow the horse to build immunity to the disease by producing antibodies, then to bleed the horse for the


\(^{17}\)Gordon Murray, Quest in Medicine (Toronto: Ryerson Press, 1963), p.99-100.
necessary gamma globulin fraction containing the antibodies (the immunoglobulin). On advice from Connaught labmen, he bought a suitable horse and secured its accommodation at a stable in north Toronto.\textsuperscript{18} It was a system of trial and error for Murray and a delicate task for the assistant handling the horse. Dr Richard Holden, Murray's first assistant, had little experience with horses and he and Lily, the six-year-old mare selected for the job, were equally nervous around each other.\textsuperscript{19} At the stables, a box stall was available for experimental animals and a retaining plank was built so the horse could be handled safely. Holden's job was to inject the horse daily with the cancer antigen material prepared at the Caven Foundation.

Murray initially focused on breast cancer and collected malignant tissues from his breast cancer patients to inject into the horse. A few Toronto colleagues also supplied him with tumour material for his experimental serum, sending it over to the Caven Foundation in aseptic containers. Murray and his staff then prepared the cancerous material for injection. The tumour tissue was minced with scissors and blended with equal amounts of saline. Once reduced to a pulp, the tumour tissue was strained numerous times through layers of surgical gauze. What remained was the antigen solution, ready for injection, and from which the horse would produce antibodies.\textsuperscript{20}

\textsuperscript{18}National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47 File 31 W. P. Caven Memorial Research Foundation, Annual Report for 1953; File 32 Minutes of Caven Foundation Board Members, 6 July 1951.

\textsuperscript{19}Murray wrote, "Still in the car, he [Holden] would start his high-pitched voice to make what he thought were the necessary soothing remarks. When he stepped from the car, the pitch of his voice rose until he was crooning an affectionate tune to soothe the ragged nerves of the pretty but resentful beast. As he crossed the yard and entered the stable door, his crooning pitch increased until, on his appearance at the threshold, Lily was in a complete state of jitters ... Without Harry's [stable man] assistance, the young assistant would have been hurled from the stable in a state of terror and in a hundred spattered fragments." Gordon Murray, \textit{Quest in Medicine} (Toronto: Ryerson Press, 1963), p.102.

\textsuperscript{20}Gordon Murray, "The Clinical Results of Serum Treatment of Carcinoma of Breast," \textit{Bulletin of the Academy of Medicine, Toronto} Volume 28, No 9 (June 1955); National Archives of Canada, 185
At first, Holden injected the horse daily, with increasing amounts of the antigen preparation. Samples of the horse's blood were taken before and during the period of injections. After three months, not knowing exactly how long to continue the injections, Murray felt that the horse had been sufficiently immunized. Lily was positioned safely in the experimental stall, and an incision was made over her jugular vein. Two gallons of the horse's blood were taken and sent back to the Caven Foundation for the separation process.

The task now was to fractionate the blood taken from the horse to isolate the immunoglobulin. Again, after seeking advice from Connaught Laboratories, first the blood cells, then the proteins and finally the immunoglobulin allegedly containing the antibodies were removed from the horse's blood.21 Murray and his team followed the methods of separation and purification similar to those used in the commercial production of antitoxins, although they did make alterations. In the end, the valuable immunoglobulin was less than one-tenth of the original volume taken from the horse. To Murray, the serum did appear to contain the antibodies for it was a light greenish colour and had a mild opalescence characteristic of other antitoxin sera. The biochemist then tested the serum for sterility to prevent contamination.22

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21 National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47 File 31 W. P. Caven Memorial Research Foundation, Annual Report for 1952. Murray's cancer research was also described in earlier annual reports.

A series of experiments was conducted, under the microscope, recording reaction of cancerous cells to the serum. Malignant cells mixed with saline solution showed no change, as did malignant cells mixed with untreated horse serum. When the immunized horse serum was added to the malignant cells, the cells appeared to undergo changes very quickly. Murray reported, "within a matter of an hour the cells looked sick in all respects. They had lost their regular outline and contour. The nuclei were beginning to show changes, and within a few hours they were completely broken up." It was exciting for all involved, but Murray was careful to note that all that could be said for certain was that there had been a "very distinct effect on malignant cells as a result of the ... antibody-filled horse serum."\(^{23}\)

Next, Murray wanted to test the efficacy of the anti-toxin serum against tumour growth in animals. The biochemist on staff at the Caven Foundation had been experimenting with the development of tumours in eggs and mice for years. This work failed, however. It was difficult to grow human tumours in eggs; only half were successful but then faded out after the fifth generation. Likewise, they were unsuccessful in getting tumours to grow in mice.\(^{24}\) Similar to his work on kidney transplantation, Murray plunged into clinical cases with relative little supporting laboratory data. He was eager to move ahead with what had always been his original idea: to administer his anti-cancer serum to human sufferers.

Only those patients in the advanced stages of breast cancer, who had undergone all modes of conventional treatment, were considered as possible

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candidates for Murray's anti-cancer serum treatment. Patients selected had to be able to walk around on their own, feed themselves, and have a life expectancy of at least three months. In some cases, these conditions were overlooked due to family pressure or for compassionate reasons. Some women were therefore included who should not have been treated. All of these cancer sufferers knew that this serum was experimental, and no promises of a cure were made. His first patient was treated on February 9, 1950, in hospital and under the watchful eye of Murray and his assistants. The serum was injected intravenously, increasing the dosage from a small amount to about 20 c.c.'s. The serum was administered daily for about three to four weeks until the total dosage of 300 c.c.'s had been given to the patient. Again, it was trial and error. How much serum was enough? Many of the women experienced reactions to the horse serum: chills, fevers, backache, and general discomfort, and in some cases their treatment was stopped. Others continued after their reactions subsided -- their last attempt to ward off death.

In most of these early cases, there seemed to be "mild improvement" in these breast cancer patients; ulcers healed and tumour growth slowed. A few of Murray's early patients had been "utterly hopeless from the beginning" and died within weeks of his serum treatment. Still others experienced lengthy periods of health, returning

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This was stated in Murray's article, "Experiments in Immunity in Cancer," CMAJ Volume 79 (August 1958): 253, and seems quite probable. Yet there are no surviving lab books to confirm this date. In fact, Murray contradicts himself in his book Quest in Medicine saying his first patient was treated in early 1946. This date however seems too early, since the Caven Foundation was not opened until three years later.

home or to work, for months after the treatment. These results were enough to encourage Murray to continue his research and he set out to produce a more potent serum. But his greatest problem was the limited supply of his serum. Murray had difficulty collecting enough fresh cancer tumours and he had only one horse producing the antibody serum. When one of his patients returned for a second treatment, he was unable to offer her any serum, and she died soon thereafter.28

Many Toronto physicians knew that Murray was experimenting with some form of cancer treatment. Some were helpful, providing him with malignant tissue, while others were critical and uncooperative, dismissing his work as flawed, impossibly naïve, almost quackery.29 Murray's anti-cancer serum did not have the profile or draw the attention that his other work did at this time, such as his heart operations or kidney transplant surgery. His cancer research was secondary to several other projects at the Caven Foundation, and there were only sporadic periods of cancer research activity during the early 1950s. When Murray left for teaching trips to Australia and New Zealand in mid-1951, the Foundation disposed of his horse that was used for creating the cancer antibodies. This research did not resume until November 1952, and two patients were treated that year. In 1953, only one patient was treated. In 1954, nine "runs" of serum were processed, and four patients received treatment. This was hardly the volume of clinical trials of a priority research activity.30 But in 1955, this changed after Murray decided to present his anti-cancer


sem work to the medical community, based on only fourteen cases. It was a planned 'debut', after which cancer research became the primary focus of Murray's Institute.

By early 1955, enough medical practitioners in Toronto knew of Murray's controversial serum and his results that a formal announcement of his work was almost forced upon Murray. He had been reluctant to comment on his various cases before 1955, but patients "at death's door" had given their stories to the press, praising the miracle drug. The newspapers had given "startling" reports of the potential cancer cure; medical practitioners wanted to know more about this serum before patients arrived on their doorsteps demanding the new treatment.

On March 8th, 1955, Murray presented his cancer serum research and clinical results to the Toronto Academy of Medicine. It was a small and local venue yet its membership included prominent medical physicians and surgeons. On this day, the room was filled to capacity with practitioners motivated by curiosity and suspicion of this new cancer treatment. They also knew that Murray, known for his theatrics and flair as a raconteur, would be anything but dull. It was a meeting not to be missed, and Murray did not disappoint his audience. He described his technique of producing and injecting the antigen substance, and showed coloured slides of the reaction of the serum at the cellular level.

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32 "Given up for Dead Toronto Wife Well After Cancer Serum," Toronto Star, 1 March 1955; Ron Kenyon, "Toronto MD Develops New Cancer Treatment," Toronto Telegram 28 February 1955.

33 "New Serum Treatment Tried in Cancer Cases," Toronto Globe and Mail 1 March 1955; Ron Kenyon, "City MD's Cancer Serum Seen Key to Other Types," Toronto Telegram 1 March 1955; Ron Kenyon, "Science Report [Editorial]," Toronto Telegram 2 March 1955.

34 J.E. Belliveau, "Murray Serum Discovery Starts Controversy On Germ As Cancer Cause," Toronto Star 12 March 1955; Dr Bruce Charles, Interview by author, 9 February 1997, Toronto; Dr D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto; Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.
In dramatic fashion, Murray then presented a vial of the greenish-tinged anti-cancer serum, holding it high in the air for everyone to see. This, he argued, was what had arrested cancer growth within patients described as "hopeless" by his Toronto colleagues. Only those women who had exhausted all conventional remedies had been treated with this anti-cancer serum, and free of charge, said Murray, trying to reassure his colleagues that professionalism had been upheld. He then paraded two patients onto the stage. Here was the proof! Both women had breast cancer and now had secondary cancers in the chest and abdomen. The first woman walked slowly to the front of the room with the assistance of nurses. She was still receiving Murray's serum treatment and was living months longer than doctors had predicted. The second woman had received treatment some time since while on her "deathbed", and she told the audience how she was now "enjoying a normal life."  

Medical practitioners reacted with "cautious optimism", and they withheld comment on Murray's serum. At the time, researchers were exploring systemic as well as local methods of controlling cancer, and this included new chemical drugs (chemotherapy was in its infancy) and research on immunity in cancer. Murray's serum was received with cynicism, even laughed at by some practitioners who knew that cancer was a much more complex disease. Others hoped that he was onto something. Well aware of their scepticism and resistance to unconventional

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35 "Given up for Dead Toronto Wife Well After Cancer Serum," Toronto Star, 1 March 1955; "New Serum Treatment Tried in Cancer Cases," Toronto Globe and Mail 1 March 1955; Ron Kenyon, "City MD's Cancer Serum Seen Key to Other Types," Toronto Telegram 1 March 1955.


37 Dr. Raymond Heimbecker, Interview by author, 1 November 1996, Toronto; Dr. D.L. MacIntosh, Telephone interview by author, 29 March 1997, Mrs. Rita (nee Smith) Macnab, Interview by author,
treatments, Murray was careful not to suggest that his serum was a cure for cancer.\(^{38}\)

Also, he wanted to prevent the media from printing any inaccurate information that would increase the professional controversy surrounding his treatment, so Murray prepared a modest statement summarizing his findings for the press.\(^{39}\)

More than simply updating his colleagues on his cancer research, Murray had other reasons for presenting his discovery of an anti-cancer serum to the profession and the public in 1955. Murray's research future at the Caven Foundation was in jeopardy. Months earlier, the Board of Directors had informed him that they were discontinuing their support of the private laboratory due to lack of operating funds, and redirecting their investment to the University of Toronto.\(^{40}\) Murray needed new backers for his research institute. He wanted to present his promising results, suggesting that with more research, a viable treatment for cancer might be at hand. This, he hoped, would raise the necessary funding to proceed. In addition, he hoped to receive more patients on which his serum could be tested.\(^{41}\)

14 May 1996, Toronto; Mrs. Paul Phelan, Interview by author, 18 November 1996, Toronto; Dr. D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto.


\(^{39}\)National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 43, File 11, Correspondence Ian Macdonald to Murray, 4 March 1955; Correspondence Murray to Ian Macdonald, 7 March 1955.

\(^{40}\)J.S. Maclean's death in 1954 resulted in the termination of support from the Caven Foundation's main financial backer. Ironically, the Board of Directors (which included Herbert Bruce) decided to redirect their initial investment, the Caven bequest, to the University of Toronto for the purposes of cancer research. Based on a report by University officials, the Ontario Government decided in 1952 to build the new Ontario Cancer Institute beside the Wellesley Hospital; it was named The Princess Margaret Hospital and was officially opened in 1958 by Princess Margaret. See Herbert Bruce, \textit{Varied Operations} (Toronto: Longmans, Green and Company, 1958), p.178; Archives of Ontario, RG 10-6-0-429 Cancer: The Ontario Cancer Institute, brochure celebrating the opening of The Princess Margaret Hospital, c. 1958.

\(^{41}\)Dr. Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.
The media took their cue. Reporter Ron Kenyon wrote, "Is This the Break-Through On Cancer?" Kate Aitken announced on her radio broadcast that "Dr Murray can bring back hope and life to otherwise hopeless cases ... here's a man whose stature will be as great as that of Banting and Best, Lister and Pasteur."

Journalists reported Murray's serum as an exciting new advance in cancer research. More importantly for Murray, his financial difficulties became well known, and public pressure was put on the provincial and federal governments for support. In the House of Commons, Health Minister Paul Martin was asked: "Is the government giving Dr Murray all possible help in connection with this very important work?" Statements were quickly issued to the press in an effort to demonstrate government support; politicians certainly did not want to appear indifferent or obstructive to research that might result in a cancer cure. Martin said that the government would certainly consider funding this serum research under the Cancer Control Grant and he urged Murray to make an application for support. Likewise, the provincial Health Minister Mackinnon Phillips jumped on the bandwagon, announcing his government's intention to give Murray "sufficient funds to maintain three horses" as well as possible assistance from government virologists. Murray was very encouraged by this show

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42 Ron Kenyon, "Is This The Break-Through On Cancer?", Toronto Telegram 9 March 1955.


of support, and it was his hope that the Ontario government would fund his research, ending his financial worries.\textsuperscript{46}

Donations began to arrive from private benefactors in Toronto as well as from elsewhere in Canada and the United States. Most often, family members of those who had been treated by Murray's serum were the first to offer large sums of money towards further research. Some people wanted to donate horses or offered their stables for Murray's use.\textsuperscript{47} Most significant was the contribution of Percy Gardiner, a Toronto financier and long-time supporter of medical research, who ultimately rescued the Caven Foundation from certain closure.\textsuperscript{48} Murray's cancer investigations became the institute's primary research project, and all employees became directly involved in the production of serum.

Murray's 1955 address on his serum treatments achieved what he had hoped: he had found a way to keep his research institute open. His work was published in a four-page article in the \textit{Bulletin of the Academy of Medicine, Toronto}, which had only a local and limited circulation.\textsuperscript{49} Murray sent revised versions of this paper to \textit{Cancer

\textsuperscript{46}National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 52, File 7, Correspondence Murray to Arthur C. Sanderson, 11 April 1955 and Correspondence Murray to E.W. Tyrrell, 4 May 1955.

\textsuperscript{47}National Archives of Canada, D.W.G. Murray Papers, MG 30, B 110, Volume 52, File 7, Correspondence W.C. Hoyt to E.W. Tyrrell, 24 March 1955, Correspondence E.W. Tyrrell to Murray, 28 March 1955, Correspondence Boyd J. Bell to Murray, 31 March 1955, Correspondence Murray to Boyd J. Bell, 11 April 1945, Correspondence Murray to E.W. Tyrrell, 4 May 1955; "Yank Gives $5,000 To Toronto Doctor," \textit{London Free Press} 1 April 1955.

\textsuperscript{48}Percy Gardiner had been a financial supporter of Murray's research (and the Toronto General Hospital) well before he bought the Caven Foundation in 1955. As described in the preceding chapter, the Caven Foundation was renamed the Gardiner Medical Research Foundation, and Gardiner supported Murray's cancer work for the next ten years until his death in 1965. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 23 Annual Report of the Gardiner Medical Research Foundation for the Year 1955.

\textsuperscript{49}Gordon Murray, "The Clinical Results of Serum Treatment of Carcinoma of Breast," \textit{Bulletin of the Academy of Medicine, Toronto} Volume 28, No. 9 (June 1955).
Research and Cancer for publication, but these were rejected due to lack of detail. He had received invitations from larger medical societies in Canada and the United States at which to present his serum research, but he had turned them down. Most likely, he realized that his work was not ready to stand the scrutiny of the profession. He became reluctant to make further pronouncements about his cancer research, despite several notes of encouragement and interest from medical colleagues. Murray wrote, "everything I have said so far has been only in the form of a preliminary report. The experiment is only in the early stages and we are continuing as we have for some years, collecting further evidence." Murray made the right decision to withhold making any more announcements; it would have been premature and damaging to his reputation. His laboratory work was rudimentary, and even if he did not realize it, amateurish, and he could not continue to parade clinical cases as proof without becoming ridiculous. It was another three years before he offered a more complete report on his serum treatments.

Murray's research contributed to the old debate on the cause of cancer. Was cancer caused by a virus? By the 1950s, most researchers in the field described cancer as a cellular disorder. The viral theory and an immunological approach to its prevention challenged their cellular disorder theory and Murray found himself in the midst of another controversy. The idea of a cancer virus was not new; anti-cancer

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serum was first produced in dogs and donkeys as early as 1895. But the theory was dropped in favour of the cell disorder concept when researchers had failed to produce tumors in animals from human malignant tissue to test their virus hypothesis. By the later 1950s, the pendulum began to swing back to the virus thesis.

Viruses are minute particles of complex proteins, too small to be seen by a light microscope or to be trapped by filters. They can easily penetrate cells, and force them to manufacture virus material instead of cell material. The body's immune system prevents the virus from taking over by producing immunoglobulin, a blood protein containing antibodies specifically matched to fight the antigens of the invading virus. Murray's anti-cancer serum, which allegedly contained the cancer antibodies of the horse, was designed to offer passive or temporary immunity for his cancer patients who had been unable to produce enough antibodies on their own.

Virologists in Canada and the United States had experimented with various types of animal cancer in which a virus origin had been proven, such as leukemia in mice, and they announced the possibility of a vaccine. Vaccines are non-virulent, antigenic disease material injected into patients to stimulate active immunity. The patient's own body is able to produce antibodies to counter the invading antigens.

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Researchers hoped they were onto something; as one researcher stated in 1962, "immunology is the only hot lead we have as a direct route to possibilities of cancer control." They regarded a vaccine as "science's best hope for a breakthrough against the disease." But the problem was that no scientist had been able to isolate the cancer virus. As one journalist reported, the virus theory was "all based on circumstantial evidence." Whether the evidence was circumstantial or otherwise, the public embraced the idea of a cancer vaccine. Vaccines against diseases such as smallpox were viewed as lifesaving discoveries. Scientists' success over polio in 1953 with the Salk vaccine reinforced society's belief in a forthcoming cure for cancer — and what better than a similar vaccine. It was an appealing, simple solution to what the public failed to acknowledge as a complex disease. The same resources that had been used to "fight" polio were now redirected to the "war on cancer".

Fundraising campaigns helped amass large sums of money for cancer research. In April 1957, when the Canadian Cancer Society launched its annual drive for funds, "leading Toronto cancer specialists" were asked: "How long do you think it will be before a cure for cancer is discovered?" The answers ranged from ten years to "not within this lifetime". Understandably, researchers were reluctant to give an exact

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56 "Vaccine Called Best Hope For Control of Cancer," Toronto Star 14 June 1962.
57 "Vaccine Called Best Hope for Control of Cancer," Toronto Star 14 June 1962.
58 Ken MacTaggart, "This Virus Could Lead to the Missing Link: We May Rid the World of Cancer," Toronto Telegram 29 September 1962.
60 Phyllis Griffiths, "10-Year Target Set To Find A Cure For Cancer," Toronto Telegram 1 April 1957.
date, but were optimistic about the "progress" being made. Postwar faith in the power of research to solve many of the world's problems included the belief that if enough money and effort were directed towards finding the cure for cancer, it would soon be forthcoming.  

Murray was among the seven men identified as "leading Toronto cancer specialists", his photo sharing the front page with headshots of researchers and directors from the Ontario Cancer Institute, the Ontario Cancer Treatment and Research Foundation, the Hospital of Sick Children and the Toronto General Hospital. These other cancer researchers headed large research teams that were well funded by the government. Their research ranged from studying the basic biological processes of cell growth and reproduction to experimenting with new forms of radiotherapy. Only Murray was experimenting with an anti-cancer serum. A lesser-known surgeon injecting his patients with such a serum might have been publicly denounced as a "quack", but as one reporter stated "... anything from Dr Murray deserves consideration and cancer scientists everywhere are watching his work

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61 Dr. J.A. MacFarlane, Dean of Medicine at the University of Toronto, reiterated this view in 1956 to the Canadian Cancer Society: "They [Canadians] were accustomed to wartime methods, where money was no object, and if enough good scientists could be got together it seemed that any problem could be solved." Records of the Canadian Cancer Society, Ontario Division, Toronto, Minutes of the Annual Meeting, 24 February 1956, Appendix A.

62 Accompanying Murray in this elite category of "leading Toronto cancer specialists" were:

- Dr. Clifford L. Ash, director of the Toronto General Hospital's Cancer Clinic and director of the new Ontario Cancer Institute;
- Dr. R.M. Taylor, executive director of the Canadian Cancer Society and the National Cancer Institute;
- Dr. William L. Donohue, director of pathology at the Hospital for Sick Children;
- Dr. W. Gerald Cosbie, medical director of the Ontario Cancer Treatment and Research Foundation;
- Dr. Arthur W. Ham, director of Biological Research at the Ontario Cancer Institute; and
- Dr. Harold E. Johns, head of the Physics Division of the Ontario Cancer Institute and designer of the Cobalt bomb.

See Phyllis Griffiths, "10-Year Target Set To Find A Cure For Cancer," Toronto Telegram 1 April 1957.
Murray was well aware of professional scepticism and mutterings of "quackery" in response to his cancer research. Thus he was careful to avoid the word "cure" and to ensure that his patients had exhausted conventional therapies before undergoing his serum treatments.

Over the years, new remedies and cures for cancer were constantly presented, but as was often the case, after a matter of time, the original claims were found groundless. In the post-war period, there were various "cancer cures" promoted, some more believable than others. Dr Hendry Connell's Ensol, developed during the 1930s and available well into the mid-1940s, was an enzyme solution that used the theory of immunity to explain its curative action. During the 1940s (and dating back to the 1920s), Dr Thomas Joseph Glover was treating patients with an anticancer serum produced by inoculating horses with the pleomorphic microorganism, which he believed to be the cause of cancer. Somewhat differently, Dr. John Hett believed cancer was due to a virus working in conjunction with hormone imbalance and activated by chronic irritation; he believed cancer to be infectious but not hereditary. Hett had developed an immune serum, its basic ingredients being B. coli and streptococcus, and was treating cancer sufferers in southwestern Ontario from the 1930s to the 1950s. Also at this time, a London physician, Dr. David Arnott, was

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promoting the Koch treatment. Dr William Koch, from Detroit, administered
injections of solutions, usually glyloylidae, malonide and benzoquinone, high enemas
and strict diets to his cancer patients.67

During the 1950s, the Hoxsey Treatment, developed by American naturopath
Harry M. Hoxsey, was advertised in Canada and the United States. It consisted of a
fluid or pill for 'internal' cancer and an ointment paste or powder for 'external' or skin
cancer.68 Also making the headlines as a new cure for cancer was krebiozen, a drug
manufactured from the blood serum of horses inoculated with actinomycyes bovis, a
mold. It enjoyed "an aura of high scientific prestige" due to its sponsorship by Dr
Andrew C. Ivy of the University of Illinois. Chemists analyzing one sample of
Krebiozen discovered it was a common amino acid, creatin monohydrate; others
found their Krebiozen to be little more than mineral oil.69 By the late 1950s, the
Ontario Commission for the Investigation of Cancer Remedies had investigated at

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67 Warren E. Schaller and Charles R. Carroll, Health, Quackery and the

68 Archives of Ontario, RG 10-6-0-428 Box 39 File: Hoxsey Treatment for Cancer, 1957-58,
Correspondence Mackinnon Phillips to L.M. Frost, 27 March 1958; Report of a Committee of Faculty
Members of the University of British Columbia concerning the Hoxsey Treatment for Cancer, 1957.

69 Warren E. Schaller and Charles R. Carroll, Health, Quackery and the Consumer (Toronto: W.B.
Saunders Co., 1976), p.297, 306-7; James Harvey Young, American Health Quackery (New Jersey:
Quackery — The Past in the Present," Cancer Quackery Volume 6, Number 4 (December 1979): 530-
31; Dennis Connaughton, "Krebiozen — The Scandal That Would Not Die," Warren Cole, MD and the
Ascent of Scientific Surgery, pp.131-167; Patricia Spain Ward, "Who Will Bell the Cat? Andrew C.
Stoddard, Krebiozen: The Great Cancer Mystery (Boston: Beacon Press, 1955); Herbert Bailey, K.
least sixty claims of cancer cures in Ontario.\textsuperscript{70} The number of drugs, tonics, herbal extracts, sera, vaccines and energy treatments, all claiming to cure cancer, seemed to increase with time -- Mucchoricin, Sanders treatment, Millrue, Orgone energy, The Rand Vaccine, Anti-Cancer Hormones, Laetrile and others.\textsuperscript{71}

No doubt Murray was aware of these treatments and probably, like his colleagues, dismissed most of them as quackery. He certainly would not have placed his serum in the same category.\textsuperscript{72} He still advocated surgery, notably radical

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\item Medical Director, D.E. Cannell, reported that "in the majority of these there is a fairly constant pattern of lack of evidence of malignancy, inadequate information to validate or disprove many 'cures' and finally in many instances refusal on the part of the discoverers to reveal the ingredients, method of preparation, proof of sterility, etc." Archives of Ontario, RG 10-6-0 Deputy Minister's Files, Series: Legislation, Box 100, File 1024 Cancer 1938-1965, The Cancer Remedy Act, 1938; Deputy Minister's General Correspondence, Series: Disease, Box 39, File 10-6-0-431 Cancer Treatments, Correspondence D.E. Cannell to M.B. Dymond, 13 July 1966.
\item The American Cancer Society summarized the characteristics of the "quack" verses the "reputable doctor" as follows:

"The practitioner is a QUACK if — he offers a cancer treatment available only from himself; the treatment bears his own name, or is offered in the name of his private research organization whose other members are not listed; he claims he is being persecuted by the "medical trust" or sabotaged by the medical profession; he uses testimonials or letters from patients in support of his treatment; his "cured" patients and greatest supporters have only his word for it that they had cancer when they came to him; he refuses or discourages consultations with specialists in the medical profession.

The practitioner is a REPUTABLE DOCTOR if — he does not offer "exclusive" treatments when approved treatments are widely available in many hospitals; he does not "patent" a treatment or cure that could help all mankind, and his membership in medical organizations is always a matter of public record; he belongs to medical organizations devoted to spreading - not blocking - the latest health knowledge; he does not divulge case histories to the public; he insists upon laboratory proof of cancer in making his final diagnosis, and before treatment is given; he always welcomes a consultation with colleagues in the medical profession."

Murray would be placed in the latter group. These characteristics taken directly from Warren E. Schallier and Charles R. Carroll, Health, Quackery and the Consumer (Toronto: W.B. Saunders Co., 201
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mastectomies for breast cancer of which he performed many, as the most effective way to contain the disease. He accepted cancer patients only after they had exhausted all conventional therapies, and never sought to profit from selling his serum.

Moreover, Murray still commanded respect from his medical peers, and was seen by others outside of the establishment as being part of that conventional order. Note the example of Ernest Frederick, a Toronto doctor who advertised his own alternative cancer therapy during the 1950s. Frederick advocated a cancer treatment based on vitamins and hormones, a carefully planned diet, as well as faith, hope, courage, determination and the will to live. He argued: “This simple, harmless treatment is foundational to every other type of treatment considered advisable, such as surgery, radiation or sera like Dr Murray’s.” Murray’s serum was neither whole-heartedly accepted by the medical profession nor did they outwardly dismiss it as quackery.

Yet an event concerning one of the current unconventional therapies may have hit close to home for Murray. In 1952 Dr. John Hett was charged with infamous and disgraceful conduct and barred from practising medicine in Ontario because he "professed to have a serum, which will cure cancer, arthritis, diabetes and peptic ulcers but refused consistently to divulge the formula." This action may have

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73 National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 52, File 7, Announcement of private clinic practice treating malignant disease, Ernest Frederick, MD, Toronto (c. 1955); Archives of Ontario, RG 10-106 Ministry of Health, Public Health Central Files (1916-1970), Box 17, File 17.8 "Cancer Cures: Frederick, Dr E.W. 1955-60," Correspondence Dr. E.W. Frederick to Dr McKinnon Phillips, 11 August 1958, Correspondence Dr. E.W. Frederick to Dr M.B. Dymond, 30 December 1958, Report of the Commission for the Investigation of Cancer Remedies on Dr. E.V. Frederick, 10 February 1956.

tempered Murray's remarks regarding his own cancer research, and made him more cautious about his pronouncements. Murray was not afraid of controversy – he had a history of challenging orthodox medical theory and treatments – but he would not risk losing his medical license. He stayed within the bounds of acceptable medical conduct, and at no time did the College of Physicians and Surgeons of Ontario investigate his treatment of patients. No doubt, his standing as a respected cardiovascular surgeon and his efforts at presenting his cancer work in the discourse of experimental research through professional venues shielded him from public labelling as a "quack", despite now working outside the traditional university research structure.

After his 1955 address, Murray concentrated his efforts on improving the serum, administering it to more patients, and producing more clinical data, rather than only patient testimony, to support his theory of immunity in cancer. He focused on increasing the potency of his serum for breast cancer (SB) as well as experimenting with a new serum -- a "polyvalent serum" (SP) -- for chest and abdominal cancers. This SP serum was produced from malignant tumours other than cancerous breast tissue with the expectation that the horse would generate antibodies specific to these

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75It was rumoured that steps were taken to bring Murray before the College due to one particularly bad death allegedly resulting from his serum treatments. I could find no written documents to substantiate this story. Hannah Institute for the History of Medicine Oral History Collection, Volume 35, Dr. Arthur Evan Parks, p.228; College of Physicians and Surgeons of Ontario Archives, Annual Reports 1950-1976.

76Barbara Clow argues that professional standing and conduct influenced acceptance of unconventional treatments by the medical community. This was the case with Dr Hendry Connell's Ensol in comparison with Hett's serum or Rene Caisse's essiac tea. She states, "Connell's methods may have been flawed but his ideas were communicated in the language both familiar and acceptable to the scientific and medical communities ... combined with his medical qualifications and his professional connections, these factors won for Connell a measure of tolerance and support." I would argue that, to the same extent, this argument is applicable to Murray's situation. See Barbara Clow, "The Problem of Cancer: Negotiating Disease in Ontario 1925-45," PhD Dissertation, University of Toronto, 1994, p.77-78.
antigens. Murray's operation now included three horses, courtesy of the Ontario government; one horse was inoculated against breast cancer only, the second horse inoculated against gastric carcinoma only, and the third horse inoculated against all other forms of cancer available from the operating rooms. When all three horses were in serum production, it was enough to administer treatment to twelve patients at a time. Murray began injecting SP serum into patients suffering from a variety of different cancers in 1956, but he found that the SP serum caused severe reactions such as fevers and infections in almost all his patients.77

One of Murray's greatest challenges was to collect enough malignant tissue to produce serum. He could not secure enough tissue material from his own patients, or from the few colleagues who assisted him, to inject the horses daily. He began experimenting with the transplantation of tumours in rats and hamsters treated with cortisone in an attempt to create his own source of malignant tissue. His experiments failed.78 Mackinnon Phillips, Ontario Minister of Health, became aware of this malignant tissue shortage directly from Murray as well as from letters from cancer patients turned away due to lack of serum.79 Phillips offered to help Murray by

77 The immunized horse used by Murray in the production of the SP serum was destroyed and two younger horses, a mare and gelding, were used to produce S2P serum and S3P serum respectively. To avoid confusion, these sera have been lumped together simply as SP serum. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47 File 23 Gardiner Medical Research Foundation Annual Report for 1956. See also Roy Greenaway, "Toronto Doctor Reveals Second Cancer Serum," Toronto Star 1 December 1955; "Treat Intestinal Cancer With New Serum Produced At New Centre," Peterborough Examiner 7 December 1955; Roy Greenaway, "Cancer Patients Say Toronto Serum Gives New Lease On Life," Toronto Star 5 December 1956; Gordon Murray, "Experiments in Immunity in Cancer," CMAJ Volume 79 (August 1958): 253; Ben Rose, "Murray Tells Government — Need Research, Cash for Cancer Vaccine," Toronto Star 5 February 1959.


organizing a system of collection from hospitals within a 100-mile range of Toronto. In October 1957 at the Ontario Pathologists Association conference, Phillips solicited the participation of pathologists for the "Gordon Murray Research Project". He outlined Murray's work in brief and the necessity of securing enough cancer specimens to either prove or disprove the anti-cancer serum experiments. Phillips also made it clear that Murray and his associates were working without remuneration. He rallied the pathologists to the greater research cause: "I can assure you gentlemen that this research project is being done to allay the fear of cancer, not only in this generation, but in generations to come ... The end result could be the answer to the cause, prevention and cure of cancer, and I am sure that you would agree with me that it would warm every person's heart here to know that he had played a small part in bringing about the answer to this long awaited question: -- What is the cause of cancer? How can it be prevented? and How can it be cured?" Sterile containers and specimen information forms were distributed to hospitals, and pathologists received instructions as to type, size and temperature at which to keep the requested material. The specimens needed most were identified as gastro-intestinal tract, ovary, lung, brain, gall bladder, liver and breast tissues. Samples would be picked up within three

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80 Archives of Ontario, RG 10-106 Public Health Central Files (1916-70) Box 19 Cancer Cures (1937-61), File 19.23 "Cancer General Correspondence 1957-65", Correspondence Mackinnon Phillips to Murray, 22 July 1957; Correspondence Mackinnon Phillips to Murray, 31 October 1957; Correspondence Murray to Mackinnon Phillips, 1 November 1957.

to five hours of notification by special courier car and rushed to the Gardiner Medical Research Foundation.  

In the ten-year period from 1952 to 1962, Murray recorded receipt of approximately 584 malignant tissue specimens. Until April 1955, most specimens had come from Murray’s own patients. After Murray’s cancer research was presented formally in 1955 and Phillips established a system of collection in 1957, Murray’s specimen numbers increased substantially. Malignant tissue of the breast, colon, rectum, stomach, liver, bowel, thyroid, ovary and kidney removed from both men and women, ranging from 30 to 70 years of age, was collected and sent to Murray.  

Medical practitioners may have been cautious, even suspicious of Murray’s serum, but patients were not. It was their last chance to beat cancer or at least delay death. Murray exuded extreme confidence to his patients. He was able to dismiss all their doubts and in turn they placed their faith in him and his serum. Moreover the continuing number of reported success stories fuelled their belief in these treatments.

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83 In 1958, Murray reported that only twelve tumour specimens had been received from pathologists working in hospitals outside of Toronto. The major contributors were the Toronto General Hospital, the Toronto East General Hospital, St. Joseph's Hospital and Women's College Hospital. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 52 Gardiner Medical Research Foundation Annual Report for 1958; Volume 35 File 22 Specimen Book 1 — 1952 to 1957; File 23 Specimen Book 2 — 1957 to 1962.  

84 Throughout his career, Murray had this talent for securing absolute confidence from his patients in his ability to treat them. In the case of cancer, the fear of incurability and death prompted most patients to accept Murray’s serum treatments without questions since Murray himself believed in its physiological action against the disease. I agree with Barbara Clow who argues that "fear — of the disease, of treatment and death — figured prominently in the narratives of cancer sufferers but it did not necessarily paralyze them." Those patients who regained their health after Murray's serum treatments were his greatest fans, engaging the media to rally support for him throughout his cancer research. Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto; Barbara Clow, "The Problem of Cancer: Negotiating Disease in Ontario, 1925-45," PhD Dissertation, University of Toronto, 1994, p.151. See also Warren E. Schaller and Charles R. Carroll, Health, Quackery and the Consumer (Toronto: W.B. Saunders Co., 1976), p.286.
A young Toronto businessman, suffering from stomach cancer, underwent an operation in October 1955 and was told he had only a few weeks to live. The surgeon sent him to Dr. Murray who administered serum injections. "Today," reported Roy Greenaway in December 1956, "he is back at work, leading a strenuous, normal existence." A 37-year-old woman from Florida, battling breast cancer, developed a secondary growth in the brain that caused one of her eyes to bulge from its socket. Greenaway wrote, "fifteen months ago, she had her first injection [from Murray]. Her eye went rapidly back to normal and today she seems normal." There were numerous other accounts. "A young Toronto woman with breast cancer was rescued from her death bed 10 months ago" and an elderly woman from Winnipeg told others that "a miracle had been performed" on her.85 Families sent gifts, cards and other expressions of gratitude to Murray.86 The serum was not a cure, but it relieved pain. In some cases, it appeared to put cancer into remission and a few were lucky enough to return to a 'normal' life temporarily. Others had severe reactions, and became very ill.87 Most died within weeks of treatment. In the end, no one overcame the disease.

Nevertheless, more and more patients arrived at Murray's office for treatment, sometimes returning for a second series of injections. It was a matter of timing for most. Were they well enough to withstand treatment? Was there enough serum on hand? Was a bed available at the nearby nursing homes? By the late 1950s, Murray was producing twelve to fourteen litres of SB serum (breast) and three to five litres of

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86 National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 31, File 41, Correspondence n.d.

87 Dr. Edward Mullens, Interview by author, 20 January 1997, Toronto; Dr Neil Watters, Interview by author, 23 May 1996, Toronto; Dr. Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.
SP serum (polyvalent) annually. The anti-cancer sera were not an accepted treatment, and while Murray's first patients had been treated in hospital, now most cancer sufferers were treated in one of three nursing homes in Toronto. Murray's serum therapy lasted three to four weeks, a lengthy occupation of hospital beds by "hopeless" patients. These individuals were not charged for their treatments, as Murray repeatedly made known, but they did assume the cost of their stay in nursing homes. Hospital insurance did not cover this care. Many cancer sufferers were frustrated with the limited number of beds and the amount of serum available. Leslie McKee, in a letter to his M.P.P, wrote,

We waited nearly an hour in Dr Murray's waiting room, and could not help hearing the telephone calls coming into his office from different parts of the province, asking for appointments and treatments. Should the Government wait until a 100 per cent cure is discovered, or should they support this treatment that is already achieving good results in many cases? It would give hope to hundreds of apparently hopeless cases ... Whether or not the Government accepts this treatment at this time as worthy of support, we cancer patients have accepted it and only ask that it be made available as we have not the time to wait until the serum is 100 per cent perfected.

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88Each time a horse was bled, about eight litres of blood was taken. This was taken back to the Gardiner Foundation where the biochemist prepared the anti-cancer serum, a process that took about two weeks. This was enough for two or three patients, depending on whether the full treatment of 300 c.c's (or millilitres) was administered. In 1956, 54 patients received treatment whereas in 1958, 77 patients received treatment. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 47, File 23, Gardiner Medical Research Foundation Annual Report for 1956; Volume 52 Gardiner Medical Research Foundation Annual Report for 1958; Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.

89In the beginning, cancer patients were hospitalized at the Private Patients Pavillion at the Toronto General Hospital where Dr Walter Roschlau administered their treatments or they were hospitalized at the Wellesley Hospital where Dr Neil Watters treated them with Murray's serum. Later, patients were sent to the Woodlands Private Hospital, the Roycroft Nursing Home and the Bel Air Nursing Home where respective doctors administered the serum treatments under Murray's directions. During the 1970s, there are records that Murray was administering his vaccine to a few of his cancer patients admitted to the Private Patient's Pavilion. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 52 Gardiner Medical Research Foundation Annual Report for 1958; Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto; Dr Neil Watters, Interview by author, 23 May 1996, Toronto.

Understandably, cancer sufferers were impatient. Murray was more cautious, yet pleased by public and government support, stating, "We are inching along slowly ... many patients are surviving for a longer period ... What a day it would be if we should eventually hit something that would wipe out cancer!"\(^{91}\)

In 1958, Murray published "Experiments in Immunity in Cancer," in the *Canadian Medical Association Journal*. It was a more detailed and complete report of his research than his first article, and had a widespread national, but limited international, circulation. Murray discussed serum production, selection of patients, treatment techniques and his results. He described his difficulties in immunizing the horses -- the mare produced serum better than the male gelding -- as well as his problem of diseases that affected the horses. He outlined his selection process of patients: only those in "the late stage of massive recurrence of the disease, after having received benefit from the above [conventional] methods of treatment" were considered. Early in his work, Murray had discovered that metastases or damage to the liver interfered with his therapy. The liver played an important role in detoxifying the serum, therefore all his cancer patients had to have healthy livers. He explained the optimum daily dosage, injection procedures, and the length of the full treatment, including the fact that some patients had strong reactions to the serum and were unable to complete treatment.\(^{92}\)

By 1958, Murray had also developed a 'vaccine' treatment in an attempt to increase "active immunity" within the patient. Cancerous tissue excised from the

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patient was emulsified and then injected into that same patient. Murray hoped that he was stimulating this patient's own antibody production, so that in addition to serum treatments, chances of resistance to the growing cancer were increased. He reported that "there has been some evidence in some patients of a possible increased antibody effect," but it was difficult to isolate the action of the vaccine. In all cases, these patients had a recurrence of the disease and died. Still, Murray found it "the most alluring of prospects ... the development of a vaccine with which the whole population might be injected in childhood to prevent malignant tumours, as is done in vaccination for smallpox or in Salk vaccination for poliomyelitis."93

Murray revealed that a total of 233 individuals had been administered the anti-cancer serum in the past eight years, but only 91 had received what was considered the full dosage, 250 to 300 c.c. of serum. Of these 62 were breast cancer patients. The majority of these women died within one year of the treatment. Fourteen remained well after one year, 7 were still well after two years, and one woman lived almost three years. There were no control groups.94 The research community found his results meaningless; there was no evidence, only Murray's convictions, that his serum had prolonged the lives of any of these women.

In this report, Murray described at length the various types of cancers treated and the moderate improvements resulting from the serum. In the end, he had ambiguous results. Those patients who received the polyvalent serum (SP) and who

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suffered from cancer of the brain, stomach, bone, colon and/or ovaries did not fare as well as his breast cancer patients. A small percentage did live more than a year after treatments, most did not. (The polyvalent serum (SP) continued to cause adverse reactions in patients, and Murray discontinued production of it in June 1958.) His serum was certainly not a cure, Murray stated, but "the relief of pain alone has justified it as a palliative measure." Compared with other forms of palliation available for patients with such advanced carcinoma, it gave better results. Many sufferers enjoyed relatively good health without symptoms or pain after their serum treatments. Some were even able to return to their usual routines. These were patients in the terminal stages of cancer, and they were grateful if the treatment did prolong their lives by months or days. The fact that his serum could apparently arrest the cancer in some of these advanced cases excited Murray about the potential of this treatment if administered earlier. He asked: "What would happen if an early case of cancer were treated with the immune serum? The answer to that is, we do not know." Murray admitted that this treatment was "still in its infancy", but he clearly felt that his experiments supported the view that "cancer is caused by something against which, either actively or passively, antibodies may be made and host resistance to cancer improved." 

In his report, Murray referenced several recent articles in Cancer Research by other investigators also conducting experiments on immunity or host resistance to cancer. The virus theory was being tested, but only Murray's work included clinical

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results. Researchers, such as Dr A.W. Ham, had obtained new knowledge of the structure of normal and malignant cells by using electron microscopes, validating Murray's observations of cellular reaction and activity.\(^{98}\) In fact, the Ontario Cancer Institute, which carried out most of the cancer research in the province, was devoting much of its efforts to the virus theory. Ham told reporters, "We are spending more time on virus than anything else."\(^{99}\) The fact that other respected cancer researchers were also working on a similar approach to the disease gave Murray's serum greater consideration, even legitimacy, than it would have earned otherwise from the medical profession.

Based on Murray's CMAJ article, the Canadian Medical Association released a press statement on the 18th of August 1958 announcing that "cancer was caused by something against which anti-bodies may be made and the resistance to cancer improved." This created a great deal of excitement. Such an encouraging announcement in the 'battle against cancer' had never been released from the Canadian Medical Association. Almost 200 medical researchers and practitioners from all over the world requested reprints of Murray's article from his office.\(^{100}\) Newspapers printed sensational headlines: "Cancer Serum Adds To Life," "City Cancer Discovery -- Serum Prolongs Life," "Canadian Serum in Cancer Fight Impresses U.K."\(^{101}\) Reporters described the serum as "surprising and promising"; they highlighted the


success cases and hinted at the possibilities of a vaccine or even a future cure. As they had done in the past, reporters created a strong public discourse which accepted and valued Murray’s work.

Murray’s office was immediately inundated with telegrams, telephone calls, and visits from medical practitioners and cancer sufferers. At the time, Murray was at his cottage at Pointe-au-Baril on Georgian Bay, writing his autobiography. He had not anticipated the great reaction to the CMA’s press release or his recently published second article. His secretary worked long hours responding to the barrage of requests for information, appointments, and serum, and sent daily reports off to Murray in response to the great wave of public and professional attention. The switchboard was jammed with calls, the mailman began delivering bags of correspondence directly to Murray’s office, and the waiting room overflowed with people wanting an appointment. Reporters pestered Murray’s secretary to tell them where the doctor could be reached; she refused to tell them, inventing a story about his lecturing in Scotland. People were desperate to know more, families of cancer sufferers wanted to get their hands on this serum, seeing it as a “miracle cure” which irritated their doctors who knew better. Murray offered to return to Toronto immediately but his secretary insisted that he remain at his cottage. Murray did not return for several weeks.

The greatest number of letters and telephone calls were from families begging for Murray’s serum to treat loved ones suffering from cancer. One American woman

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wrote, "... our little girl, our only child, has cancer of the eyes ... the doctors can do no
more ... please, please I beg you, won't you please have mercy." From New York, one
man wrote, "Is there any hope for my father?" From Ghana, another pleaded: "Dr
Gordon, for the love of God, can you help? Please, please, if there is a chance ... we
will fly to Toronto." Many were desperate calls on behalf of individuals who were
days within dying. Several telegrams simply read, "Rush anticancer serum" and listed
an address in New York or elsewhere. People camped out in the halls of the Medical
Arts Building, hoping to see Murray. They arrived without appointment and without
telephoning in advance. They refused to believe that Murray was not in his office.106
His secretary gave appointments to as many suitable patients as possible, overbooking
Murray's schedule, filled up beds in the nursing homes and then began a waiting list.
By September, people had become "irritated" by the delays in getting appointments
and undergoing the serum treatments. "Why haven't I received an answer to my letter"
or "What do you mean there is no serum to send out?" were common responses.107
Most did not understand that the treatment lasted four weeks with daily injections,
administered by Murray, his assistants or nursing home doctors. Murray simply could
not see all of these cancer patients. He did not have enough serum or bed space to
accommodate all these requests. His secretary balanced these appointments, which

105. "All World Pleads for Cancer Vaccine MD Asks Gov'ts Aid," Toronto Telegram 5 February
1959.

Correspondence —— to Murray, 27 August 1958.

Correspondence —— to Murray, 9 September 1958.

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were not billed, with regular surgical consultations and operating times at the hospital.  

Murray's cancer work and serum treatment were controversial. His belief that cancer was caused by a virus was supported by some doctors, yet disputed by others. Critics of Murray's serum treatments felt his report was superficial: "Why did it cover only 91 cases though he had begun treatment on 233? ... Had all patients been in the hopeless state that he asserted? ... Could their improvement not be due to chance or delayed-action benefits from previous treatments?" The most obvious criticism was that Murray, despite his standing as a leading surgeon, had little background in immunology. Dr Arthur Ham, head of the Ontario Cancer Treatment and Research Foundation's biological research, had urged Murray to seek advice and/or assistance in immunology at various points in his research program, but Murray had refused. Some immunologists said bluntly that his method would not work.

Murray began to speak more openly about his cancer research, probably because he had more clinical experience and because an increasing number of other researchers were conducting similar immunological investigations towards controlling cancer. He wanted to make sure that his work was known and recognized for professional and financial purposes. In January, 1959, Murray spoke at the American

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110 According to R.M. Taylor, Murray had responded that, "this was not necessary and proceeded to develop a serum and to publish one or two reports on this material." National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 7 Correspondence R.M. Taylor to M.J. Grimes, 14 August 1969.

111 Dr Walter Roschlau described Murray's methodology and criteria as crude, and felt that he [Roschlau] was not qualified to do the research that Murray was asking. Immunologists had much stricter criteria. Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto. See also "Serum Against Cancer?" Time Magazine [Canadian Edition] 8 September 1958, p. 48-9.
Academy of Orthopedic Surgeons in Chicago, a strange venue to present cancer research. He downplayed any talk of a "cure" and focused on the possibility of immunity against cancer. "The important thing to determine," he said, "is whether the serum caused the improvement or whether it resulted from some still-undetermined factor." Though his words were guarded, Murray felt that his experiments did provide evidence of host resistance to cancer.

Murray was growing impatient. He was convinced that he was on to something and believed he was close to finding a cure to cancer. It was time now for the federal and provincial governments "to step in to quicken and expand the pace of research". With only three horses, a small staff and himself, his research was under-funded, slow and even amateurish. "The more people who work on this, the faster will a vaccine be found, if one can be found," pleaded Murray. "It is time now for an intensive government-supported effort to pursue every angle on whether cancer is caused by a virus or looking toward a vaccine with which people could be inoculated against the disease." He wanted nothing less than a nationally supported research program to investigate immunity in cancer. He criticized the Canadian Cancer Institute and the Ontario Cancer Treatment and Research Foundation (granting agencies of the federal and provincial governments) for spending too much money on trying to find a cancer cure through orthodox methods, particularly through radiation.

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In thirty years of experiments, argued Murray, radiation had failed to offer a cure. Orthodox treatments left too many cancer patients to die, and an expanded "drive against the disease" was needed. "This cry of humanity cannot be ignored," said Murray. 114

Murray had delivered harsh criticisms of conventional cancer therapies. Out of medical conscience and integrity, practitioners attacked Murray’s comments to offset a public panic that orthodox medicine offered little hope in fighting cancer and to denounce his serum as extremely experimental and unproven. Defending conventional therapy, Dr. R.M. Taylor, executive director of the Canadian Cancer Institute, argued that radiation or surgery or a combination of the two had proven effective in controlling cancer for many patients. Dr. Arthur Ham claimed that there was a great deal of research being done on the virus theory as a cause of cancer, but not to the exclusion of other experimental research. "I don't think we are shutting our eyes to any new field," commented one radiologist. Other researchers complained that Murray would not provide them with serum to do their own investigations because the serum was in too short supply. More proof of Murray’s success was needed. "It should be proven or disproven," argued Dr George Culnan. "Statistically, his researches don't mean anything yet." 115

In the House of Commons, Health Minister J.W. Monteith stated that the federal government had spent nearly $1,100,000 towards


cancer under health grants in the past ten years.\textsuperscript{116} They were following Murray's work closely, and once proof was established of its benefit, the government would "spare no effort" to provide Canadians with this treatment.\textsuperscript{117} It was confirmed that no formal request for money had ever been received from Murray.\textsuperscript{118}

In the end, Murray's outburst against the cancer research community had been 'unwise'. It further isolated him and his work from others.\textsuperscript{119} In 1960, Dr Walter Roschlau, Murray's research assistant for the past nine years, left and took a position at Connaught Laboratories. He had become disillusioned, realizing that the Gordon Murray Institute would never be built, so he left to advance his career elsewhere.\textsuperscript{120} His new research facilities were a sharp contrast to the working environment of the Gardiner Foundation. The large, team-oriented, professional laboratory at Connaught was more collegial, possessed greater resources, and exercised more rigorous research conduct. Roschlau came to realize how naïve he had been in his research in regards to statistical methodology and evidence. He found the Connaught laboratories more intellectually rewarding in comparison to the Gardiner Foundation where all

\textsuperscript{116}House of Commons Debates, 11 February 1959, p. 908-909; "Anti-Cancer Serum Work Being Watched by Ottawa," Toronto Globe and Mail, 12 February 1959. For a further breakdown of annual allocations of the Cancer Control Grants by province, see House of Commons Debates, 18 February 1954, p.2201.


\textsuperscript{118}"Asks Government Aid Battle Against Cancer," Toronto Globe and Mail, 6 February 1959.

\textsuperscript{119}Dr Arthur Park commented, "What seemed so strange was that he was so accurate and so dead on anything of cardiovascular surgery. He was a way ahead of his time. He was a way ahead of time in the artificial kidney, but he got the idea into his head that he was an immunologist and when you get into that area, boy, that's pretty deep stuff ... he wasn't willing to work with other people, so in the end that came to a bad end ... he had to do all this work alone in his own private laboratories." Hannah Institute for the History of Medicine Oral History Collection, Volume 35, Dr. Arthur Evan Parks, p. 228.

\textsuperscript{120}Dr Walter Roschlau, Interview by author, 17 April and 24 July 1996, Toronto.
research had been limited to Murray’s field of interests. He had begun to question Murray’s research, and to resent playing second fiddle to him for so long. Undeterred by Roschlau’s departure, Murray continued to produce his sera and vaccines and to treat as many patients as he could handle, while others at the Gardiner Foundation carried on with their related experiments, all hoping for some kind of breakthrough.  

By the 1960s chemotherapy had proven to have more demonstrated effects than immune sera, and resources were redirected accordingly. Chemotherapy is the treatment of cancer by drugs, mainly chemical compounds called cytotoxic (or cell-poisoning) drugs which have the ability to destroy living cells. The treatment is unpleasant but results in the regression of tumours, and sometimes their disappearance. When first introduced, chemotherapy was used only for advanced and recurrent cancers that had failed all other therapies, but its success soon started a trend toward using chemotherapy earlier in the course of patient therapy. Cancer clinics and treatment centres began to endorse chemotherapy in combination with surgery and/or radiation treatment. Surgery and radiation are local mechanical therapies,

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121 In October 1958, Murray had begun a new line of investigation: kidney perfusion and inoculation. Perfused kidneys were inoculated with fresh tumour suspensions and maintained by perfusion for up to eleven days. Murray hoped to grow and maintain the cancer virus in kidney tissue, just as the poliomyelitis virus can be grown in monkey tissue culture. This experiment was obviously based on the cancer virus theory, and the ultimate objective was to try to demonstrate and isolate a virus-like body by means of the techniques described. The experiments were not successful and were discontinued in May 1960. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 52 Gardiner Medical Research Foundation Annual Report for 1958; Volume 36 File 12 Kidney Perfusion, October 1958 to May 1959; File 13 Kidney Perfusion, May 1959 to May 1960.

122 Chemotherapy destroys cancer cells by interfering in some way with their ability to reproduce themselves. The drawback is that cytotoxic drugs are not selective enough and do not discriminate between cancer and non-cancer cells. But once the tumour is destroyed, the damaged normal cells can recover. Some chemotherapy drugs are administered orally, others by injection or intravenous drip. The side-effects of these drugs include: hair loss, nausea, vomiting, tiredness, depression, diarrhoea, skin rashes, general soreness, and possible loss of fertility. All information on chemotherapy taken directly from Understanding Cancer (Consumers’ Association, June 1986), p.78-82.

applied to particular areas of the body, whereas chemotherapy is systemic, its effects circulated throughout the body.

Nevertheless, Murray continued to treat cancer patients during the 1960s, never wavering from his belief in a virus theory. But his work on cancer immunity received less assistance from the government and his medical colleagues. Once again, the malignant material for his sera and vaccines came from his own patients. By now, the public was confused about whether talk of an anticancer vaccine was bold theory, a clinical possibility, or quackery. In 1963, journalist Barbara Moon of Maclean's Magazine asked "Who is Dr Murray? [and] How seriously does medicine now take cancer-serum experiments?" She reported on the "distressing outbreak" of bogus anticancer drugs offered to the public as treatments or cures. In Toronto earlier that fall, two doctors were under investigation for the harmful treatment of

124 Dr. A. Cameron Wallace, Director of the Cancer Research Laboratory at the University of Western Ontario in London, had this to say about immunity in cancer. Was he referring indirectly to Murray? "... the possibility exists that eventually it may be possible to produce immunity to tumors ... this has been the dream of cancer workers for more than a half century -- to be able to immunize people against cancer as against diphtheria, tetanus or typhoid. Countless attempts to produce an antiserum have failed, in spite of much initial optimism. Again, the central trouble seems to be that cancer cells, being derived from own cells, are so much like them that our body will not recognize them as an enemy, as it does with germs and viruses. It is still common to read in the paper of someone producing an antiserum which it is claimed will cure cancer, then gradually the excitement wears off and everyone wonders what has become of it." Records of the Canadian Cancer Society, Ontario Division, Toronto, Minutes of the Annual Meeting, 22 November 1961, Appendix A.

125 In several cases, Murray excised cancerous tissue, rushed it to his private lab for production into a vaccine, and within days, returned to the Toronto General Hospital to administer the treatment to his patient. Malignant tissue was homogenized aseptically with sterile saline, filtered three times through six layers of fine mesh cheese cloth, diluted roughly 1:10 with sterile saline, bottled and heated at 56 degrees Celsius for twenty minutes. The material was labelled for the respective patient, many admitted to beds on the 6th floor at the Private Patients Pavilion at Toronto General Hospital. National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 35, File 21 Laboratory Record Book 8 - February 1963 to June 1964.

126 Barbara Moon, "Who is Dr Murray? How seriously does medicine now take cancer-serum experiments?" Maclean's Magazine (November 16, 1963): 27.
patients with anticancer injections. How was Murray's serum any different? Moon did not answer the question. There was no insinuation that Murray was a quack, but some specialists expressed strong "reservations" about his clinical success. The shift in emphasis in cancer research (towards the virus factor) that had led to the "outbreak" of false cures and remedies also gave legitimacy, albeit lessening, to Murray's serum. It appeared that the public still revered its "daring" blue baby doctor, "world-famous as a surgeon of creative brilliance." No doubt they wanted to believe the possibility of his work providing a cure for cancer. Moon's article prefaced an excerpt from Murray's forthcoming autobiography (volume 2) in which he described "his lengthy and continuing research" into cancer immunity. It was hardly a critical article, and she ironically closed with an extremely optimistic and naïve quote from Murray:

"Undoubtedly the facts are accumulating rapidly, and it is only a matter of a short time before the cancer problem is in the bag."

In 1965, Murray published a two-page article entitled "Experiments in Host Resistance to Cancer (in Human Subjects)," in the American Journal of Surgery. It was brief, incomplete and offered no more insight into his serum research than his 1958 report. Two hundred and forty-eight patients had been treated, experiencing "favourable temporary responses", but none were cured. Murray offered little information to qualify his work: the time period during which these treatments took

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127 Dr Leo V Roy was accused "of treating one patient for breast cancer with health foods, an electrical machine resembling a short-wave apparatus, and injections every week or oftener, including one injection directly into the breast." The other Toronto doctor, Dr T.J. Glover, was "alleged to have treated another recent cancer victim with injections of horse serum he prepared himself from horses he kept at nearby Shanty Bay." It was thought that Glover, investigated years earlier, was inactive, but "his reappearance seems significant", according to Barbara Moon. Was everyone trying to jump on the immunity bandwagon? Barbara Moon, "Who is Dr Murray? How seriously does medicine now take cancer-serum experiments?" Maclean's Magazine (November 16, 1963): 27.
place as well as details on patient selection, malignancy and dosage were notably absent. He acknowledged the recent success of chemotherapy, which seemed to provide a "cure in a few", but argued, "It still remains to find a universal cure for any and all the remaining groups. Until this happens, every prospect should be explored, for out of the least promising sometimes the unexpected happens, for example, penicillin." In closing, he reiterated his stance about the possibility of a future cancer vaccine.

The AJS article did not generate the fanfare that his earlier article had in 1958. In fact, it aroused professional criticism, even threats, according to Murray. Angry family members of women who had died while taking Murray's cancer serum may have sought to bring action against Murray in an effort to revoke his medical license, although no such case records exist at the College of Physicians and Surgeons. Murray and his cancer serum treatment received increased criticisms of "quackery" from the medical profession and some members of the public. Aside from a few cases in which women with cancer lived months longer than predicted by other doctors, Murray had not reduced the morbidity and mortality of this disease. It was a discouraging time for him, and a low point in his research. Opposition to his cancer serum finally overwhelmed him. He "thought it discreet to discontinue," and set his cancer investigations aside temporarily.130

128 Barbara Moon, "Who is Dr Murray? How seriously does medicine now take cancer-serum experiments?" Maclean's Magazine (November 16, 1963): 27. Murray quote taken from Gordon Murray, Quest in Medicine (Toronto: Ryerson, 1963), p. 120.


130 Personal interviews by author; Personal Correspondence, Dr Fred Moffat to author, 21 January 1997; National Archives of Canada, D.W.G. Murray, MG 30 B 110, Volume 43 File 30 Correspondence Murray to P.G. Scholefield, 10 April 1972.
In 1968, Murray returned to his cancer research. He refused to believe he was wrong, and began a series of experiments on rats in an effort to provide proof that it was possible and practical to increase immunity against cancer. A control group of rats was implanted with Walker 256 sarcoma and all died. Another group of rats was implanted with the same cancer in similar fashion, and when tumour growth reached 2.5 to 3 cm in diameter, they received 1 c.c. of immune serum daily for five days. According to Murray’s records, in every rat tumour growth stopped and the animal lived with no recurrence of cancer. Another group of rats was 'vaccinated' against cancer for twelve weeks, and then implanted with cancer. Seventeen of 26 rats did not develop tumours. Rats immunized for periods less than three months did not fare as well. One group of rats, which had developed cancer, been treated and recovered, was bled. Their blood was given to rats newly injected with tumour material to test if blood from immune rats could be used. It worked; tumour growth receded and the rats lived. Another group of rats with advanced cancer was given the immune serum. Tumour growth was slowed but there were no survivors. Many experiments were repeated with hamsters with similar results. From this lab work, Murray concluded that if serum and vaccine treatments were administered before the development of late stages of cancer, the animals survived the cancer.

These convoluted animal experiments were conducted between 1968 and 1974, and by this time Murray’s credibility as a researcher had been seriously

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undermined if not destroyed. In late 1967, Murray had announced a cure for paraplegia, specifically a spinal cord regeneration operation for individuals with spinal cord injuries. He paraded his most successful clinical case in front of physicians, philanthropists, reporters and the public. Soon thereafter, medical authorities at the Toronto General Hospital discovered that Murray had not performed the celebrated breakthrough surgery. Murray was accused of scientific dishonesty and “broomcloset” research. The spinal cord controversy of 1967 (which is examined in more detail in the next chapter) damaged his reputation, and effectively ended his surgical career. Thereafter, both the public and the profession became more skeptical of Murray’s research, notably his cancer serum.

Nonetheless, Murray was convinced that his rat and hamster experiments in addition to his clinical cases did represent indisputable evidence supporting his theory of immunity in cancer. He treated several cancer patients during the early 1970s, and received similar results to his earlier clinical cases. One of his last 'success' cases was Mrs. G, age 47, who suffered a recurrence of cancer three years after a radical breast mastectomy. She did not live in Toronto, so her local physician administered the serum treatment, as advised by Murray by phone, beginning in June 1974. Her therapy lasted well over a year. She experienced healthy, pain-free periods as well as bouts of chills, headaches, and reactions to the serum. Overall her pain was reduced

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and any further tumour growth was stopped temporarily.\textsuperscript{135} But by 1975, Murray had difficulties providing Mrs. G with a continuous supply of his serum. He was no longer producing it.\textsuperscript{136} Moreover any serum he had in stock had become contaminated when it was moved to a refrigerator in his garage after the closing of the Gardiner Medical Research Foundation in 1974.\textsuperscript{137} Mrs. G. died while still receiving sporadic serum treatments.

No one in the medical community was interested either in pursuing further research on Murray's anti-cancer sera and vaccines or in publishing any further articles on the subject.\textsuperscript{138} At various times during the 1970s, Murray wrote up his experimental and clinical results. The \textit{American Journal of Surgery}, \textit{Cancer Research},

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\textsuperscript{136} During the 1970s, when Murray no longer had staff (other than Cecil Wilding, the caretaker) at the Gardiner Foundation, he contracted out the work. Boarding fees as well as an hourly wage were paid to Mr. Patterson to inject and bleed the horse. Arrangements were made with Connaught Laboratories to prepare the serum. According to his daughter, after the Gardiner Foundation was closed in 1974, Murray made arrangements to do some research work at Connaught Laboratories for two years or so. How much time and what type of research Murray did at Connaught in these years is uncertain. It is highly unlikely he was manufacturing serum for patients. He was probably carrying on with his rat experiments, testing his sera and vaccines, which was the subject of his posthumously published paper, "Immunity in Cancer". National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 39, File 21 Memorandum from M.J. Walcroft to Murray, 15 September 1972; Volume 30 File 11 Correspondence R.J. Wilson to Murray, 9 August 1973, Correspondence Murray to R.J. Wilson, 17 August 1973; Volume 39, File 25 Correspondence G.M. Healey to Murray, 10 June 1974.

\textsuperscript{137} Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.

\textsuperscript{138} To attract research funding in the early 1970s, Murray knew he had to have a younger researcher, preferably an immunologist, working with him on the cancer sera and vaccines. He tried to interest a former employee, Eileen Graves, in returning to the Gardiner Medical Research Foundation. Murray wrote, "there is the possibility of expanding the lab and facilities. With the world wide interest in immunity in cancer, we might lead the way to make this a centre to diffuse information and also a great centre for treatment of great numbers of patients. The nihilistic attitude that stopped the project in the past seems to have subsided and there is a general search in this direction .... would you consider returning to 73 Homewood to take part in what I hope might become a great institution?" Graves, having now completed her medical training, did not accept Murray's offer. National Archives of Canada, MG 30 B 110, Volume 30, File 10 Correspondence Murray to Eileen Graves, 25 January 1972.
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Cancer, International Journal of Cancer and the Canadian Medical Association Journal all rejected his article, entitled "Immunity in Cancer", in one form or another. Reviewers commented that Murray's investigations were described in the most general of terms, with no particulars of the methodology, thus making it almost impossible for other researchers to repeat his experiments in order to confirm or refute his conclusions. They found fault with Murray's standards, form, controls and evaluation as a researcher.\textsuperscript{139} In the end, his results were made known only after an obscure Italian medical journal agreed to publish his work in 1977.\textsuperscript{140}

The 1970s were also a time of financial difficulties for Murray and his cancer research program. If not for the generous gifts of private benefactors, secured mostly by the efforts of Murray's friends, the Gardiner Foundation would have closed many years earlier. Murray's relationship with the Canadian Cancer Institute and the Ontario Cancer Treatment and Research Foundation had been problematic due to personality clashes and professional criticisms exchanged over the years. Murray was hurt that these established cancer research agencies had never funded his serum work, especially given the encouraging actions of the federal and provincial governments in the mid-1950s. Yet he had never submitted a funding application.

In 1972, the Governor-General of Canada, Roland Michener, who was a close friend of Murray's, tried to remedy this. He arranged a meeting between the Executive Director of the Cancer Institute, R.M. Taylor, Murray and himself to discuss Murray's


research program and the possible role of the Cancer Institute.\(^\text{141}\) The meeting went well, and within weeks Murray had submitted his first grant application to the CCI for $52,000 for his research. The application was returned to Murray with a letter asking for more research program details and a further breakdown of costs, given that, in the past, the average size of a grant awarded to an individual investigator had been only $14,000.\(^\text{142}\) He responded tersely in a brief letter, citing his articles and reiterating much the same information as on the application.\(^\text{143}\) The Cancer Institute sent another letter to Murray, politely asking for more details. P.G. Scholefield wrote, "I think you have amplified in your recent letter the nature of your objectives but what we really need is a complete statement of how you propose to achieve these objectives."\(^\text{144}\) Scholefield also did something quite "unusual"; he sent Murray a copy of a competing grant application requesting approximately the same amount of funding. In contrast to Murray's submission, this investigator had submitted a detailed application. "I hope this material will give you a much clearer idea of the amount of detail which is required concerning any proposed research program and I must repeat that a similar detailed analysis of the proposed expenditure is also needed," wrote Scholefield.\(^\text{145}\) This show of preferential treatment was unfair, if not unethical.\(^\text{146}\) It was done not


\(^\text{143}\)National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 43, File 30 Correspondence Murray to P.G. Scholefield, 10 April 1972.

\(^\text{144}\)National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 10 Correspondence P.G. Scholefield to Murray, 13 April 1972.

because the Cancer Institute 'liked' Murray or favoured his serum research over other projects; they were indulging Roland Michener who wanted Murray to get this funding. Within days, Murray mailed off a short letter that expanded on the purpose of his research and included an itemized listing of his costs.\footnote{147}

Normally, the Cancer Institute submitted its grant applications to a group of ten scientists gathered together in Toronto for the purpose of evaluating these research proposals. The Executive Director, R.M. Taylor, agreed to consider Murray's application immediately, not adhering to usual deadlines or procedures.\footnote{148} His application was sent to four outside referees in Sweden, the United Kingdom, the United States, and Canada who were considered to be experts in the field of immunology.\footnote{149} These reviewers ripped Murray's research proposal apart. They were extremely sceptical about Murray's clinical results. Extensive work of the same kind had been done by others, better trained in the field, without significant success, they wrote, so what made Murray think he would be different? He was out of his league. One reviewer wrote that Murray lacked "a good understanding of basic immunological mechanisms" and "there existed grave deficiencies in the project".\footnote{150} Another reviewer reported, "Dr Murray states that he is now curing rats and hamsters

\footnote{146}{It is unknown whether the investigator of the detailed grant application knew that his submission had been copied and sent to Murray.}

\footnote{147}{National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 10 Correspondence Murray to P.G. Scholefield, 19 April 1972, Correspondence P.G. Scholefield to Murray, 3 May 1972.}

\footnote{148}{National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 43, File 30 Correspondence P.G. Scholefield to Murray, 13 March 1972.}

\footnote{149}{National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 11 Correspondence R.M. Taylor to Murray, 7 July 1972.}

\footnote{150}{National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 11 Correspondence R.M. Taylor to Murray, 7 July 1972.}
of cancer by treatment with immune sera, and is preventing cancer in these species by vaccination. These are indeed exciting statements but no substantiating data are presented, and since other investigators have had very little success with either immunotherapy of established tumours or prevention of autochthonous tumours, one must be sceptical until the data are presented."\textsuperscript{151} Based on the damming comments of his reviewers, the Cancer Institute had no choice but to reject Murray's application for funding.

Murray's funding problem was further compounded by his secrecy; he simply refused to divulge the details of his proposed experiments. "He has felt in years past that to do so is to reveal plans which he would prefer remained known only to himself," wrote R.M Taylor.\textsuperscript{152} Therefore, Murray became dependent on support from individuals and smaller foundations willing to donate money because of Murray's status and their belief in his ability to produce a cure for cancer. They did not bother him with wanting to know the details of his research. This support from individuals helped fund his research for several years, until finally, in 1974, the Gardiner Medical Research Foundation was closed due to financial difficulties. Murray was now 80 years old. He moved any serum he had in stock to his home in north Toronto, and settled into semi-retirement.

\textsuperscript{151}National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 11 Correspondence R.M. Taylor to Murray, 7 July 1972.

\textsuperscript{152}National Archives of Canada, D.W.G. Murray Papers, MG 30 B 110, Volume 30, File 7 Correspondence R.M. Taylor to M.J. Grimes, 14 August 1969.
What had seemed possible twenty years earlier had not come to fruition. Murray's anti-cancer sera and vaccines did not become "the crown of his career." It had been a lofty goal: to discover a cure for cancer. Perhaps it attracted Murray's attention because it would have been such a magnificent discovery. Many believed that Frederick Banting might have been capable of such a discovery, and now others thought it possible by the pioneering surgeon and researcher, Gordon Murray. At one time, Murray had been presented as one of Canada's leading cancer researchers, with some level of government, professional and public support. He had been one of many investigators exploring the possibility of immunity in cancer during the 1950s. But his anti-cancer serum remained an experimental and unsanctioned treatment. When researchers shifted their attention to other experimental means to control cancer, Murray did not. Yet he was unable to offer evidence to substantiate continued research on sera and vaccines. Clinical investigation had become more rigorous in methodology, controls and standards. Within the new research model, Murray's experiments were flawed. After 1967 and the spinal cord controversy, his credibility as a researcher was seriously questioned by the profession and the public. He did not learn from that debacle, and resumed his faulty research practices. He was unable to secure funding from larger granting agencies, and no amount of demands or pleading

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154 "Regardless of the complexity of the cancer problem, the public believed that some "genius" researcher would discover the cause and cure for the disease. Sir William Mulock, described as a genuine hero-worshipper by Michael Bliss, wrote, "Banting in the matter of research has the research instinct. In part is I think almost a genius in matters of research work. Those who know him so describe him, a simple, unaffected, shy and thoughtful man, contemplate his solving the cancer problem. Is it not worth taking the chance of enabling him to devote himself to the work?" As the discoverer of insulin, Banting were revered as a hero, capable of unravelling many other medical mysteries. Did Murray see himself as the next "Banting"? After years of working under the Toronto shadow of Banting, was Murray desperate to make a final mark, a permanent legacy? Banting's work on cancer taken from Michael Bliss, Banting: A Biography (Toronto: McClelland and Stewart, 1984), p.121-122.
to the government or elsewhere served his purpose; it only alienated him from the established research structure.

Murray's investigations on immunity in cancer constituted the longest ongoing project of his research career. For twenty-five years, he experimented with sera and vaccines, testing his theory of immunity on animals and humans. In the end, despite the hope of many Canadians that Murray's serum would be that much-wanted cancer breakthrough, his research ended in failure. A cure for cancer was not discovered nor was immunity against cancer proven or disproven. Murray however convinced himself that his cancer research had been a success. He was pleased with the dramatic improvements he had seen in his patients because of his serum, and continued his investigations until his death.\(^{155}\)

Over the course of his cancer research, it became clear that Murray was an antiquated clinical investigator. His approach to cancer was naïve, and his research methods and results were pathetic. He was working outside of his acknowledged expertise, in a research world that had become highly specialized, without adopting modern investigative methods. Scientists and colleagues told Murray that his cancer research was wrong. He refused to believe them. In the past, Murray’s convictions had often placed him in opposition to the medical establishment. His work had always pushed the limits --- vascular surgery, heart operations, renal therapy --- and eventually his ideas had been accepted. But this was not the case now. Murray did not have a cure for cancer, and after the spinal cord controversy of 1967, the public and the profession seriously doubted his abilities to do so. It became evident that Murray was working beyond credible research parameters and was unwilling to alter

\(^{155}\text{Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.}\)

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his practices. His spinal cord regeneration work highlights the extent to which his lone research practices had removed him from, and he believed he was above, acceptable postmodern research conduct. Once again, the senior surgeon took on the medical establishment, and destroyed what remained of his reputation.
Chapter 7
Making Paraplegics Walk Again: The Spinal Cord Controversy

In November 1967, Murray stunned the medical community, the press and the public with his announcement of a cure for paraplegia. Paraplegics and quadriplegics would now be able to walk again! Murray had devised and performed a new operation to trigger regeneration of the spinal cord and to restore motor and sensory function in persons with spinal cord injuries. He claimed that his animal experiments and eight clinical cases proved that he was onto something. It was a remarkable breakthrough for a medical condition that had stymied neurological researchers for years.

At the age of 73, Murray savoured the celebration of his spinal cord operation. It promised to be a glorious finale to a long and impressive surgical career. Despite what all the medical books and experts had proclaimed -- that the spinal cord could not regenerate -- Murray had proven them wrong. Other researchers and medical men doubted Murray’s hypothesis from the beginning, and refused to become involved in
his research. So Murray had worked alone in his small, sparse private laboratory. He hoped his discovery would now attract funding and stimulate interest.

The press praised Murray; it was a "medical miracle" for individuals with spinal cord injuries who otherwise were left in "helpless hopeless despair." "We can stack all the wheelchairs in a lump [now]," said Murray. He had done it again, and the public and press never questioned otherwise. Just like his kidney transplant and cancer research, Murray was presenting spectacular clinical results based on thin laboratory data. It was of little concern to the media, but the medical community was less certain. Most practitioners were guarded in their comments to the press on the impact of this new procedure. Professional skepticism and demands for scientific proof prolonged media coverage and forced Murray to disclose and defend his work publicly. All was not what it seemed to be. No one anticipated the impending drama or the widespread repercussions of Murray’s announcement.

The spinal cord is part of the central nervous system, a large nerve trunk encased within the vertebral column, which consists of nerve cells and bundles of nerves that connect all parts of the body with the brain. Sensory and motor function messages travel from peripheral sites through the spinal cord to the brain and vice versa. When the spinal cord becomes injured, these messages are disrupted and function is affected. Medical science can repair most injuries to the body but not in the case of the central nervous system. Damage to the spinal cord is irreversible.²

¹Don Dutton, "Toronto surgeon's medical miracle brings paralyzed man to his feet," Toronto Star 15 November 1967.
Spinal cord injury is one of the most serious injuries that a person can survive. Individuals with spinal cord injuries suffer either paraplegia or quadriplegia. Paraplegia is paralysis of the lower part of the body and both legs are affected. Quadriplegia is paralysis affecting all four limbs, legs and arms. The level at which the injury occurs and the completeness of the lesion directly influence the outcome of the injury. A greater loss of function occurs in higher injuries in the spinal cord, and some sensation and movement is retained if the lesion is incomplete. Urinary, bowel and sexual functions are almost always impaired in spinal cord injuries. The degree of paralysis (without negating personal resolve) determines the level of independence, self-care and medical management of individuals with spinal cord injuries.

Historically, individuals with spinal cord injuries have had poor life expectancies. Until the mid-1940s, the mortality rate was 80 per cent. The most common cause of death was urinary sepsis. Post-World War II advances in the management of spinal cord injury reduced this high mortality rate to below ten per cent. Spinal units and centers were built and specialized medical practitioners and nurses trained. New programs for treatment and rehabilitation were established. Previously, individuals with spinal cord damage were bedridden and died in hospital within months of their injuries. Postwar medical and social changes, such as

2In cases of peripheral nerve damage, only the axon or nerve fibre is cut or damaged and thus it can grow back. In cases of central nerve damage, the cell out of which the axon grows dies and it cannot come back to life. The task is then to remove dead cells (that act as barriers) and entice living cells to connect again through axons to continue communication routes through the spinal cord. Lynn Phillips, et al., Spinal Cord Injury: A Guide for Patient and Family (New York: Raven Press, 1987), p.10-12; M. Oliver et al., Walking into Darkness: The Experience of Spinal Cord Injury (London: MacMillan Press, 1988), p.ix. I thank Dr. William Geisler for helping me understand the science of spinal cord regeneration. Dr. William Geisler. Interview by author, 17 April 1997, Toronto.


improved medical care, special education and training programs, and greater wheelchair accessibility within the community, offered a more promising future for men and women with spinal cord injuries. Some paraplegics managed to return to the workforce and to their homes instead of remaining in hospital awaiting an early death.\(^5\)

Still, individuals with spinal cord injuries clung to the hope of walking again. Paraplegics participated in exercise and strengthening programs directed by physiotherapists and nurses and learned how to walk with braces, calipers (crutches), and canes. Advances in medical treatment and rehabilitation prolonged their lives, provided them with greater mobility, and improved their overall quality of life, but still paraplegics wanted a cure. Social and physical barriers in the community continued to challenge their re-integration. It raised a debate between cure and care -- should not time and funding be spent looking for a cure, argued many medical practitioners and paraplegics.\(^6\) Murray agreed, and like his approach to cancer, he argued that more money should be directed to finding a cure for spinal cord injuries.\(^7\)


\(^6\)Despite advances in wheelchairs and other assistance devices, the ultimate goal of treatment was still walking. See Geoffrey Hewelcke, "They Walk ... Though Paralyzed," *Maclean's Magazine* February 1, 1946, pp.21-22, 37; Mary Tremblay, "Going Back to Civvy Street: A historical account of the impact of the Everest and Jennings wheelchair for Canadian World War II veterans with spinal cord injury," *Disability and Society* Volume 11, No 2 (1996): 158.

In 1960, Murray shifted his attention from cancer to paraplegia and began experiments in animals "in an attempt to help the incurable state of quadriplegics and paraplegics." Fractures, skin grafts, tendons and incisions heal when treated and placed in close contact. Why should the same principle not apply to the spinal cord, thought Murray. He began operating on rabbits, exposing the spinal cord at various levels, dividing and sectioning the cord in different places. In 27 rabbits, the spinal cord was cut on one side or hemisected, and although not severed completely, varying degrees of paralysis resulted. Two weeks after the operation, function began to return and after ten weeks, paralysis had almost disappeared in each rabbit. Under the microscope, Murray saw that axons or nerve fibers had grown and crossed over the site of injury. He grew excited -- he had proof that regeneration in the spinal cord was possible. "If one motor tract could recover why not all tracts, provided the sectioned surfaces of the cord could be brought as accurately into apposition as were the individual tracts when we made these limited sections?" thought Murray. In his experiments, the intact portion of the spinal cord acted as a splint, therefore the ends of the divided section remained in ideal apposition. "What then would happen if in a completely transected cord, the ends could be brought into neat and complete apposition?" Murray asked.

From 1960 to 1966, Murray experimented on hundreds of animals. In numerous groups of rabbits he nicked the spinal cord in various locations --

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hemisections on the right, left and center — and noted the immediate paralysis and eventual function recovery. Some rabbits died of infection or causes unknown. Other rabbits recovered with almost no sign of impairment. These ‘success cases’ were sacrificed and their spinal cords removed for examination. Slides of the growth of axons or nerve fibers across the line of hemisection were made, and axon patterns studied. The spinal cords were then placed in glass jars of saline and became part of Murray’s collection of specimens on display at the Gardiner Medical Research Foundation.

In another series of animal operations, Murray experimented with shortening the vertebral column. The spinal column stabilises and protects the spinal cord. He practised resecting a complete circumference of the bone without damaging the cord as well as fixating the bone to restore stability. It was delicate work and demanded great surgical dexterity. After numerous operations, Murray began to see encouraging results. He was now prepared to attempt a full transection of the spinal cord.10

Complete transection of the spinal cord is the severing of the nerve trunk leading to certain paralysis. Would the nerve axons, without the guidance of intact tracts, be able to regenerate and realign themselves? In a series of rabbit operations, Murray divided the spinal cord completely at about the twelfth dorsal vertebra with scissors. When he cut the cord, it retracted like an elastic band forcing him to shorten the vertebral column to allow the cord to be placed in apposition. It was imperative that the cord was snugly in apposition to prevent scar tissue from forming. Nerve fibers could not penetrate scar tissue. The gelatinous consistency of the cord made it

impossible to sew the ends in place. Still, Murray applied one fine silk suture. He fixated the spinal column and hoped that this would suffice as a splint. Most rabbits did not survive the operation, dying of shock, infection or irregular bone growth due to poor fixation within weeks of the procedure. In a formal report, Murray summarized the results as follows: "[the operation] completely paralyzed the animal posterior to that level [twelfth dorsal vertebrae]. They had no movement or sensation in the hindquarters and no sphincter control. After twelve to fifteen days there were signs of recovery of motor function which progressed, until at four or five weeks the animal [referring to all surviving rabbits] could pull the hind feet under his body and make some uncoordinated efforts at hopping. Of the twenty-six [of seventy rabbits] that survived, at about twelve weeks the animals had regained control of sphincters and were able to hop normally, not with complete coordination but with fairly good power and moderate coordination."11 These were encouraging results after years of experimental surgery – twenty-six of seventy paralysed rabbits had made a full recovery after undergoing Murray’s spinal cord operation. Surely others would be as excited as Murray about these findings.

In 1965, Murray's rabbit experiments and observations of spinal cord regeneration were published in the American Journal of Surgery. In the article, Murray directly challenged the accepted medical belief that the spinal cord cannot

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regenerate. He outlined his experiments, his results and his conclusion that this
accepted medical belief was wrong. "There is experimental evidence that the
transected spinal cord in rabbits can regenerate with concomitant return of motor,
sensory and sphincter function when adequate apposition is obtained," wrote
Murray. Later that year, he presented his findings to the Toronto Memorial Society.
It was a low-key affair and Murray was guarded with his statements. It might well be
possible to re-assemble human spinal cords effectively, stated Murray, "it's a matter of
experience, accident and good luck." He refused to elaborate on his hope for clinical
application when he realized that reporters were in the audience. To the media he
made it clear that he was not seeking publicity and his experiments were complicated
and not yet complete. The Toronto newspapers obliged Murray, printing only
minimal reports of the address the following day. Despite Murray's bold work,
there was little fanfare from either the medical profession or the public over his rabbit
experiments.

This was due in part to the fact that Murray was not alone in his work on
spinal cord regeneration. Numerous investigators, dating back as early as 1873, had
explored regeneration within the central nervous system on a range of animals and had
also questioned the long-held belief that spinal cord regeneration was simply not
possible. During the mid-1960s, researchers in the field seemed particularly hopeful

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that they were closer to understanding axon growth in the central nervous system. For example, in 1966, English neuropathologist Lionel Wolman reported evidence of well-developed axon regeneration in 12 of 76 human cases of traumatic paraplegia, citing Murray's 1965 article as well as the experimental results of other investigators in defense of his findings. Researchers in the field seemed to acknowledge that axon growth was possible, however they were not convinced that functional recovery, either motor or sensory, could be restored. Neurospecialists and other scientists were still grappling to understand the condition physiologically. In contrast, Murray had already accepted that axon growth and restored function was possible, and was working towards devising a corrective treatment.

Over the next twelve months, Murray started a new series of experiments on dogs. He was not as successful with dogs as he had been with rabbits, and it became a frustrating period for Murray. One dog fell out of the sling and woke the caretaker up in the middle of the night. In another dog, the bone splints punctured the skin and it had to be sacrificed. Efforts were made to encourage dogs to stand, to urinate, and to wag their tail as some measurement of restored function. Some motor and sensory function seemed to return in some of the dogs but no objective measurements were ever taken. The dog "seems fine", "seems to be well and happy", "standing up and quite perky", wrote Murray's assistants in the experiment notebook. In the end, none of the 23 dogs survived, all dying of infection or shock. Post-mortem examinations

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showed that none of the spinal cords and columns had remained in position. Yet Murray was not to be deterred; six years of rabbit experiments persuaded him otherwise.

In mid-1966, Paraplegic News re-printed Murray's 1965 spinal cord article from the American Journal of Surgery. After reading this, one woman wrote directly to Murray on behalf of her quadriplegic son. She inquired if he had operated on humans yet, implying that her son was interested in the procedure. Murray sympathized with her situation. "I am sure no one needs to remind you of the pathetic despair involved both in the patient and in the family of paraplegics," he wrote. To date, he had not operated on any human patients, but he was hoping to do so soon. It pleased Murray to receive this encouragement for his research.

In 1967, Murray dared to perform his new spinal operation on humans. Between January and May Murray operated on six individuals with spinal cord injuries. All of his patients were male, had been injured several years earlier and had undergone conventional treatment. His first patient, D.C., was a 22 year old quadriplegic. In the operating room, Murray removed the bone from an earlier fusion operation and exposed the spinal cord. Dense scar tissue had built up around the site of injury and this was cut away. A bluish cyst was then found and removed from the center of the cord. Murray did nothing else, hoping that this was enough for improvement. It was not. Over the next few months, D.C.'s paralytic condition and general health remained as before.

Murray's second patient, P.D., was a 21 year old paraplegic. Like the first operation, Murray removed the bone from an earlier fusion operation and exposed the spinal cord. He severed the cord in two at the point of injury and removed scarred tissue at each end for biopsy until living axons were found. When he was finished, there was a 5 1/2 centimetre gap between the viable cord. Murray then removed a corresponding amount of the spinal column (6 cm dorsally and 3 cm anteriorly) and the ends of the cord were placed snugly together. He carefully passed a fine silk suture through each end of the cord and tied it lightly. The spinal column was stabilized with heavy wire. After the operation, the patient was placed in plaster shells that were strapped firmly to prevent movement. P.D. remained in hospital for 86 days. Upon release from hospital, some improvement such as voluntary movement of both toes was noted, but he was lost to further follow-up when he returned home to the United States. Nevertheless, Murray felt that there was positive evidence of a return of communication across the site of the divided cord.\(^\text{19}\)

Murray's third and fourth patients were both paraplegics in their early thirties. Based on a myelogram, both men were suitable candidates for the surgery but in the operating room it proved otherwise. The spinal cord injuries were too extensive for Murray's cord resection operation to have any effect. Murray's fifth patient, A.D., was a twenty-year-old quadriplegic. Bone from a previous fusion operation was removed and extensive scar tissue was cut away. A biopsy taken from the remaining intact cord at the site of injury showed fifty per cent of living axons present. Murray did not proceed further and hoped that he had done enough for improvement. Within six

weeks, A.D. had made "worthwhile recovery" with increasing movements and strength in his arms, hands, legs and feet, and was released from hospital.²⁰

Murray's sixth patient, B.P., was a 23 year old quadriplegic. Again, the fusion bone from an earlier operation was removed and extremely dense scar tissue was cut away from the cord at the site of injury. A projecting bone located near scar tissue was also removed. A biopsy of this scarred area showed fifty per cent living axons present so a radical transection was not done. Post-operatively, B.P. demonstrated improvement in arm and hand function and beginning voluntary power in both quadriceps.²¹

Murray described his clinical experiences in an unpublished article entitled "Encouragement in Spinal Cord Regeneration" written in the summer of 1967. He outlined his procedure in detail and summarized his findings. In two cases, A.D. and B.P., improvement was recorded due to partial excisions of the spinal cord and, in another case, P.D., a complete transection of the spinal cord was performed, resulting in an apparent return of muscular control and vague sensory functions. Further improvements might develop in time. Murray wrote that two years were required to see the end result of peripheral nerve repair, so who knows how long the cord might need? Murray, now age 73, wanted these findings published "as a stimulus and encouragement for others to press on with this project."²² Instead, The Journal of the American Medical Association politely rejected the article. "We do not believe that it adds enough definitive information," wrote the senior editor. "We are glad that


encouraging progress is being made, but this, in itself, is not enough to justify publication in our journal."23 It frustrated Murray that his medical colleagues could not recognize the extent to which his spinal cord operation might aid thousands of paraplegics in North America.

Murray continued to perform his spinal cord procedure. In October 1967, he operated on another two male quadriplegics. His seventh patient was L.R., age 22. He removed the bone fusion from an earlier operation and exposed the spinal cord. Murray attempted to relieve compression on the cord by removing fragments of bone and dense scar tissue at the site of the injury. A biopsy showed that no living axons were present, but due to "problems with the anaesthetic and general conditions", he did not proceed with a complete transection. Murray's eighth patient was 18 year old S.K. As in other cases, Murray removed the bone fusion and exposed the cord. A cyst was removed and scar tissue cut away from the site of injury. A biopsy showed no living axons present, but again, due to "problems with the anaesthetic and the poor condition of the patient," nothing further was done. In both these last two cases, if no improvement occurred, Murray planned to proceed with the complete cord transection operation in a few weeks time.24

Shortly after performing these last two operations, Murray was scheduled to give an after-dinner speech at the Toronto East General and Orthopedic Hospital Research Foundation fundraising dinner. The Foundation was entering its third year; its research studies included barbiturate overdosage, the Rh factor, grease-gun


injuries, factors in traffic accidents, and cervical spine studies. Tickets for the Foundation dinner and dance were sold at $50 a person or $75 a couple. Invitations were sent to medical practitioners, businessmen and Toronto society members, and the press planned appropriate coverage of the event. Murray was well known within Toronto circles, and his billing as guest speaker gave the Foundation Dinner added prominence.25

The Foundation event was held on November 14, 1967 in the Centennial Ballroom at the Inn-On-The-Park in Toronto. A crowd of nearly 2,000 medical and non-medical people attended the event.26 After a multi-course dinner, the guests waited patiently for Murray to begin his talk. No speech title or topic was printed in the program but they knew that Murray, known for his theatrics and flair as a raconteur, would be an entertaining speaker regardless of subject. The audience sat contently, sipping after-dinner drinks and coffee, and shifted their attention to the stage.

After receiving a kind introduction, Murray rose and took his place at the podium. He faced the audience and smiled. He thanked the Foundation for inviting him to speak and praised them for their research efforts. To further encourage and validate such needed research efforts, Murray told the audience that he planned to share his current research with them.27 He then made his announcement: he had had


27 "My purpose was to help the hospital in its research programme, and I hoped I could encourage and inspire the research spirit, which usually is so feeble or non existent in most Canadian hospitals," wrote
success with a new surgical technique that made spinal cord regeneration possible for the first time. In laymen's terms, Murray described his animal experiments and presented slides of rabbits first paralyzed and then hopping. Severed spinal cords were rejoined by removing some of the bone of the spinal vertebrae. Nerve fibers crossed the site of injury, restoring function to areas before paralyzed. Where others had failed, Murray had succeeded. The secret was bringing the ends of the spinal cord in apposition. The audience listened politely but was unmoved; animal experiments and modest gains in the laboratory seemed to be routinely reported.

At that moment, the audience was distracted by noise at the back of the ballroom. The large doors had been opened and a hospital bed was being wheeled through the audience and onto the stage. Murray introduced B.P., a 24-year-old French-Canadian accident victim who had been paralyzed from the shoulders down for more than three years. B.P. had no leg or arm movement; he was unable to feed himself or control his bladder and bowel movements. Murray announced that he had recently operated on B.P., (he was the sixth patient in the series of eight clinical cases), as well as seven other individuals with spinal cord injuries. "Now," said Murray with a flair for the dramatic, "would you like to see him sit up?" The audience jolted to attention. Six months ago, B.P.'s arms were useless but now he used them to lift his body by means of handstraps. The crowd applauded while B.P. rested and smiled. Then, as three nurses steadied him, B.P. slid to the floor and stood briefly in a walker. He smiled again and waved. The crowd clapped louder. B.P. returned to the bed, and from a sitting position, he held a glass of water in one hand before lifting it to
his lips for a drink. "This is a marvel," said Murray. "It's never happened on earth before ... this fellow's going to walk." Murray praised the work of the nurses and others in caring and providing rehabilitation for these spinal cord patients after surgery. Continued exercise and strengthening programs promised further improvement. Exiting amid deafening applause, B.P. was then hurried back to the Toronto General Hospital and Murray returned to his dinner seat. The crowd was dumbfounded by what they had witnessed. "Trust old Murray to bring it out like this," remarked one observer. Physicians and the press quickly gathered around Murray to ask questions about his spinal cord regeneration work.28

The next day front page headlines glorified Murray's newest "medical miracle" --- "New Technique Permits Paraplegic to Sit, Stand," "Quadriplegic Stands Up ... Waves," and "Toronto surgeon's medical miracle brings paralyzed man to his feet." Pictures of Murray and B.P. accompanied congratulatory articles in all the Toronto newspapers, and the story was sent out on the Canadian and American newswires. Reporters commended Murray for his eight years of research on spinal cord regeneration and his compassion for individuals with spinal cord injuries. "He [Murray] had become fascinated and horrified by the plight of paraplegics and quadriplegics doomed to helplessness ... [wondering] 'surely something could be done for them,'" wrote Ken MacTaggart in the Toronto Telegram. Another journalist reported, "he was driven by the knowledge that many of these people were so

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miserably unhappy that 27 per cent commit suicide." Most articles listed Murray's earlier medical contributions including his pioneering techniques in heart surgery, his "blue babies", his mechanical kidney, and his kidney transplant operation. "Doing the impossible is something that Dr Murray has been doing for 30 years," wrote Don Dutton in the Toronto Star. "The undeniable fact about Dr Murray is that you can't write him off. Despite his bravado, his achievements are impressive," wrote David Spurgeon in the Toronto Globe and Mail. As they had in the past, the press declared Murray a hero and a miracle maker.29

The day after the Foundation fundraising dinner, Murray repeated his performance at St. Joseph's Hospital. It was an audience of doctors, well aware of what Murray was going to talk about, and they had many questions. Again, Murray described his animal research, showed slides of paralyzed rabbits that had regained nerve function, and paraded B.P. as clinical application and proof. Still the doctors wanted to know more; they asked: "What would hold the ends of the cord together if it is cut right across?" Murray explained that bone was removed from the spinal column to bring the ends of the cord together. "Did he need special instruments?" Murray made his own instrument that looked like a wire from a fence to curve deep into the spinal column. "How did he determine how much of the spinal cord was deadened and must be removed?" Murray replied that bits of the cord were sliced off and tested to see if they were alive and active. Murray admitted that he did not know how long recovery would take, especially for paraplegics who had been bedridden for

Furthermore, all of his patients were American, except B.P., and they were lost to any follow-up when they returned to the United States. He faced many limitations including the fact that he was doing almost all the research himself "in a lousy little lab mostly at my own expense" and he could not interest other Toronto medical men in his work. Reporters loved it! These comments positioned Murray as the selfless lone researcher working towards a cure for others. The media refused to see Murray any other way, and on that day, apparently so did the doctors. They gave Murray a standing ovation, saluting him as a "great surgeon, a great human and a great Canadian."  

No one praised Murray more than B.P. With his limited English, he told reporters, "It's wonderful. It's nearly a miracle." Months ago, B.P. had learned of Murray's animal experiments from a television report. He then wrote directly to Murray and volunteered for the procedure. And it had paid off for B.P. He could now feed himself, write letters, and smoke cigarettes. He could stand and lift one foot at a time from the floor. Six months ago, he could do none of this and he held high hopes for further improvement. One reporter asked him, would he be able to walk someday? B.P. replied, "Of course I will."

S.K., Murray's eighth patient, was lying in plaster casts at the Toronto General Hospital waiting "for a miracle of his own." He had complete confidence in Murray and B.P.'s progress reassured him of his own future. "I think Dr Murray is a brave man," said S.K. "I'm sure the operation has helped me. My breathing has improved.

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If I get better – even if I can just walk on crutches, it would be wonderful." He would have to wait and see.

Other paraplegics and quadriplegics were equally excited about Murray's spinal cord procedure. "It means a new life for us," said James Strachan, a 38-year-old paraplegic. "He [Murray] should get the Nobel Prize." At Lyndhurst Lodge, a treatment center in Toronto for individuals with spinal cord injuries, patients were demanding the operation. One man told his wife, "Don't give up hope. The doctor put one guy together, and now I got a chance to walk again." Another patient told reporters, "It would put me in life again. If anyone would come and ask me to go I would volunteer right away." As James Strachan explained, "You'll never realize what it's like to have to rely on others for everything. To take you places. To get you into bed. That's the worst part of it." Still, some individuals guarded their optimism. Twenty-four-year-old Betty Higgins said: "If it [Murray's operation] works, I want it. But I want to be sure first. I don't want to build up my hopes too high. I can't afford a setback."33

The public was made aware of the potential impact of Murray's spinal cord procedure through daily newspaper, radio and television reports. "How fortunate we are to have the likes of Dr Murray in our country," wrote one woman.34 The media continued to report sensational updates and photos of an improving and smiling B.P. as well as the sad stories of other paraplegics and quadriplegics. Murray had performed a miracle, wrote one man. "It consists in the power of man to rise above

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barriers, to move what seems immovable in the faith and confidence that all things are possible if man cherishes the dream and exercises the will."35

The medical profession was less accepting of the "miracle." Reactions to Murray's announcement ranged from cheers of congratulations to serious reservations. Dr John Mullan, chief of neurosurgery at the University of Chicago, called Murray's operation "a major and dramatic breakthrough." Dr Al Jousse, medical director of Lyndhurst Lodge, said, "Everything he [Murray] has attempted in the past has succeeded, and I cheer him now." Dr J.G. Love, senior neurological consultant at the Mayo Clinic commented, "If Dr Murray has actually successfully rejoined a severed spinal cord, he has done something very wonderful." Neurosurgeon Arthur Ward asked directly, "Was this patient an actual quadriplegic?" All of B.P.'s Quebec doctors confirmed that he was. Other medical men were just not convinced. British paraplegic specialist Dr Ludwig Gutterman said, "a newspaper report describing Dr Murray's operation does not sound feasible." Dr J. Lawrence Pool of the Columbia Presbyterian Medical Center told reporters: "It sounds very exciting, but I'm inclined to be skeptical at this stage until this is verified by neurosurgeons of other clinics. Certainly it deserves investigation."36 Most specialists wanted more information before commenting further. Toronto medical men were sceptical but unwilling to criticise Murray publicly without knowing more details.37

Murray was inundated with requests from medical professionals, paraplegics, their families, and the media for more details about his procedure. 


and television reporters phoned his secretary, staked out his office, and camped at the gates of his home in North York. For the most part, Murray repeated how he had experimented with rabbits and talked about the continued improvements in B.P. A lengthy segment on the spinal cord surgery was broadcast on CBC's The Way It Is on the 19th of November and included separate interviews with Murray, B.P., and Dr William Drucker, Surgeon-In-Chief of the Toronto General Hospital. Murray came off well on the program. He was relaxed and friendly to the reporter, and seemed happy to answer all questions. When asked if other doctors would start developing his new spinal cord procedure, Murray responded: "I hope so -- I'm old, I'm obsolete and I'm trying to get the younger group going on this project." CBC Reporter Barbara Amiel commented that it was hard to understand why Murray was not as famous as William Osler or Wilder Penfield.38

Drucker and other Toronto General Hospital medical men were still reeling from Murray's announcement. The news came as a complete surprise to them. The ensuing onslaught of inquiries from around the world made it a public relations nightmare for the Hospital. Surgeons, physicians, reporters and individuals with spinal cord injuries were telephoning and wiring the Hospital wanting more information about this "breakthrough" spinal cord procedure. Hospital authorities needed to gain control of the frenzied media situation that had erupted. First and foremost, they needed to find out more about Murray's operation -- was it indeed a breakthrough? Murray had performed these operations in the Private Patients Pavilion unbeknown to his Hospital superiors. They were caught in an embarrassing

37Due to his past contributions, Murray still commanded respect among his colleagues, despite their obvious disbelief in his spinal cord regeneration announcement. Personal interviews by author, Toronto.
position of ignorance and a Hospital investigation into the matter was quickly organized. 39

Hospital Superintendent Dr J.D. Wallace sent a stiff two-page letter to Murray explaining the Press Code that was to be used when the name of the Hospital was involved in statements to the press. "It is particularly unfortunate that the name of this Hospital was included in this publicity [spinal cord announcement] and that we were not able to defend our reputation through having the proper information in advance." Wallace asked Murray "to correct some of the overly enthusiastic impressions that were created by press stories" and to work through the proper channels to "avoid this type of publicity" in the future. 40 Wallace then turned to the Hospital Medical Director and the Head of Nursing: what was the policy regarding the provision of passes permitting patients to leave hospital? "Passes" as such did not exist as a document, but a patient could leave hospital on a written order by his doctor. 41 More importantly, why were senior hospital men not aware of Murray's spinal cord operations? "There are far too many loop holes in our present regulations to satisfy

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39 Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence W.R. Drucker to J.C. McCulloch, 20 November 1967; J.C. McCulloch to W. Anderson, 29 November 1967; Correspondence J.D. Wallace to John S. Young, 18 December 1967.
40 Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Murray, 20 November 1967.
41 Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Dr H.S. Doyle and Miss M.J. Dodds, 20 November 1967; Correspondence Miss M.J. Dodds to J.D. Wallace, 21 November 1967; Correspondence Dr H.S. Doyle to J.D. Wallace, 28 November 1967; Correspondence J.D. Wallace to H.S. Doyle, 1 December 1967.
my desire for quality care and the protection of patients," wrote Wallace. Tighter controls needed to be enforced.

Wallace then met privately with Murray and Drucker. They agreed that the publicity was premature, but if this was a breakthrough, the Hospital and university should support Murray's research in regards to financing, personnel and project organization. Wallace recorded that "this was accepted with enthusiasm by Dr Murray." They agreed to hold a press conference "to announce the start of a hospital-organized and supported assessment of results to assist further in Dr Murray's work."43

The press conference was held on November 21st at the Toronto General Hospital.44 It was intended to ease media attention. Drucker saw this as an opportunity to set the record straight on the "unfortunate publicity" and to protect the reputation of the Hospital; Murray wanted journalists "off his back".45 Wallace, Drucker, Murray, Dr T.P. Morley, Associate Professor of Surgery at the University of Toronto and Chairman of Neurosurgery at the Toronto General Hospital, and Dr Al Jousse, Medical Director of Lyndhurst Lodge constituted the panel. They sat uneasily in the Hospital boardroom, bracing themselves for the glare of television lights and

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42Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to J.C. McCulloch, 29 November 1967.
43Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Members of the Board of Trustees, 29 November 1967; National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 39, File 29 Statement written by Murray, n.d..
44National Archives of Canada, Audio-visual materials, VI 9601-0016, ISN 178361, CBC News Film pack, 22 November 1967.
the barrage of questions from reporters. The five doctors looked "tense, nervous and rather grim."46

Drucker was appointed spokesperson and did most of the talking during the hour-long press conference. Murray sat slumped in his chair, his arms folded, with a disdainful look on his face. Drucker began by stating that Murray's work was preliminary and its announcement premature. The surgery was unproven, and Murray had not produced scientific evidence to back his claims.47 Drucker told reporters, "We have absolutely no unassailable scientific evidence either for or against Dr Murray's findings and observations. This could be a very real breakthrough: all of us sincerely hope so. But we cannot document it scientifically and to allow it to be propagated throughout the world is doing harm."48 He turned to Murray and commented, "I have the greatest respect for this man ... but I don't believe him in this particular instance because I can't get the data that says this patient really improved."49 Drucker focused on Hospital protocol and the necessity of scientific rigour. It amounted to a public professional chastisement of Murray for his "unsubstantiated,

46Joan Hollobon, "Hospital surgeon emphasizes proof lacking for spinal cord operation," Toronto Globe and Mail 22 November 1967.


49Joan Hollobon, "Hospital surgeon emphasizes proof lacking for spinal cord operation," Toronto Globe and Mail 22 November 1967.
unscientific" announcement, and as The Medical Post reported, "it highlighted the medical predilection for scientific method over 'broomcloset' research."\(^{50}\)

Despite an earlier agreement to work with Hospital authorities to abate the intense press attention, Murray decided that he could no longer keep quiet. As one reporter whispered, "the script went out the window." Murray spoke out angrily and defensively. "I'm not in the least interested in any of these opinions here, including you Dr Drucker, with the greatest respect ... I don't give a damn what you think. I know it's true," blasted Murray.\(^{51}\) Striking back at the skeptics, Murray said the reason why they did not have documentation was that the Toronto medical community had been uncooperative. "Now everyone criticizes me for being a lone wolf ... I tried to get cooperation in this but I didn't get it so I've done it alone. I'm satisfied. I'm quite certain there is regeneration."\(^{52}\) Murray told his colleagues: "You can drop it if you like; I've carried on individually up to this stage and I can do it again. I'm going to continue to do others -- if I get support here I'd welcome it, I'd love it -- I love the cooperation going on now, but I'm going to go on with it regardless."\(^{53}\)

The press conference disclosed the obvious disagreement between Murray and his Toronto colleagues. "It revealed strong undercurrents of personality conflicts," wrote David Spurgeon. "It also illustrated the conflict between the lone wolf and the organization men of modern medicine and between the intuitive approach of the brilliant, old-style surgeon and the rigourous, impersonal demands of modern medical science."\(^{54}\)

\(^{50}\)"Error in interpretation," The Medical Post (Toronto) 5 December 1967.

\(^{51}\)Joan Hollobon, "Hospital surgeon emphasizes proof lacking for spinal cord operation," Toronto Globe and Mail 22 November 1967.

\(^{52}\)Marilyn Dunlop, "Dr Murray Clashes With Hospital Over Effect of Spinal Surgery," Toronto Star 22 November 1967.

\(^{53}\)Joan Hollobon, "Hospital surgeon emphasizes proof lacking for spinal cord operation," Toronto Globe and Mail 22 November 1967.
science."54 In the end, both sides could claim victory. Drucker agreed to set up a panel of leading doctors to study the procedure, but it would be at least two years before even a tentative conclusion would be reached. He made it clear that much more work would have to be done before any more claims could be made.55 Murray smiled as he listened to Drucker commit financial, medical and moral support to his research.56 "The profession, I agree, must be conservative," said Murray. "There are all kinds of hare-brained ideas proposed, and they must be examined. We're all earnestly trying ... however as he [Drucker] says, it will be two years or so before we can produce the data that will prove it."57

The press conference did not go as Wallace had wanted, not according to their earlier meeting, and the Hospital came off looking bad once again. "Dr Murray took this opportunity to reverse his field and give the impression that the 'Medical School and Hospital Establishment' was attempting to minimize his discovery," he wrote. "The Press, as usual, tended to support the underdog."58 However, Hospital authorities and Murray did agree not to issue further statements to the press without mutual consent.

Toronto medical men were under enormous pressure from the profession to clarify the sensational reports arising from Murray's spinal cord research. Medical

54David Spurgeon, "Lone wolf at odds with modern men of medicine," Toronto Globe and Mail 22 November 1967.
58Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D.Wallace to Members of the Board of Trustees, 29 November 1967.
specialists and members of international paraplegia associations expressed grave concern about any publicity claiming such extraordinary results. Ludwig Guttmann, President of the International Medical Society of Paraplegia and Founder and former Director of the National Spinal Injuries Center at Stoke Mandeville Hospital, wrote Dr A.L. Chute, Dean of Medicine at University of Toronto, and Dr A. Jousse, Director of Lyndhurst Lodge in Toronto, terse letters urging action. "I feel very strongly that an international commission of expert specialists in the field of paraplegia and tetraplegia should be set up to investigate these 7 cases by Dr. Murray. Having regard to the universal furor which these reports have aroused and in particular the hopes which they have already given to paraplegics and tetraplegics, their families and friends, no stone should be left unturned to investigate and verify or refute the extraordinary claims made by Dr. Murray."59 Dr Alain Rossier, Vice Chairman of the World Committee of Spinal Paraplegia, was "deeply disturbed" by the publicity generated from Murray's announcement.60 Dr. A. Tricot, specialist in the treatment of paraplegics at Brugmann University Hospital in Brussels, wrote "I think it is my duty, as a member of the Executive Committee of the International Medical Society of Paraplegia to express my total disapproval about the very regrettable publicity conducted by the press concerning works of Dr Gordon Murray. Those reports have given false hopes to several paraplegics."61 Chute assured Guttmann, Rossier, Tricot

59 University of Toronto Archives, Faculty of Medicine, Office of the Dean, A76-0044/Box 231 (Correspondence), File 21 Dr Gordon Murray, Correspondence Ludwig Guttmann to A.L. Chute, 22 November 1967.

60 University of Toronto Archives, Faculty of Medicine, Office of the Dean, A76-0044/Box 231 (Correspondence), File 21 Dr Gordon Murray, Correspondence Alain Rossier to A.L. Chute, 30 November 1967.
and others that he shared their concerns and that Murray’s work was currently being investigated.  

On November 24th, three days after the press conference, Murray was in Ottawa as a houseguest of the Governor General, long-time friend Roland Michener. Murray was one of 35 Canadians to be invested as Companions in the Order of Canada. The investiture had been arranged months beforehand, and Murray was delighted to be so highly honoured. The Order of Canada medal was a symbol of service and achievement. It was a new honour, its creation announced by Prime Minister Lester Pearson only months earlier. Its first recipients were an elite group and included Vincent Massey, first Canadian-born governor-general, former Prime Minister Louis St. Laurent, novelists Hugh MacLennan and Gabrielle Roy, neurosurgeon Dr Wilder Penfield, historian Donald Creighton, Murray, and others. All of these individuals "have served their country and their fellows with singular accomplishment and they deserve a full measure of recognition from Canada and its people," stated Governor General Roland Michener.

The November 24th Order of Canada ceremony was timely. It allowed Murray and his family to escape Toronto and unrelenting media attention. They enjoyed visiting the Micheners and the protection of being inside the walls of Government House. The awards ceremony was a formal and intimate affair at

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61 University of Toronto Archives, Faculty of Medicine, Office of the Dean, A76-0044/Box 231 (Correspondence), File 21 Dr Gordon Murray, Correspondence A. Tricot to A.L. Chute, 27 December 1967.
62 University of Toronto Archives, Faculty of Medicine, Office of the Dean, A76-0044/Box 231 (Correspondence), File 21 Dr Gordon Murray, Correspondence A.L. Chute to Ludwig Guttmann, 1 December 1967; Correspondence A.L. Chute to Alain Rossier, 6 December 1967; Correspondence A.L. Chute to A. Tricot, 8 January 1968.
Rideau Hall. Murray sat patiently, in his white tie and tails, appreciating the stately atmosphere of the evening. When his name was announced, Murray rose and walked to the platform. The Governor General placed the Companion of the Order of Canada medal around his friend's neck; he smiled at Murray, shook his hand, and spoke to him for a few minutes. There was applause from the audience and Murray returned to his seat, a broad smile across his face. The recognition had been long awaited and was now savoured.65

While Murray was receiving his national award, Toronto reporters were phoning Toronto General Hospital authorities, armed with the tip that Murray had not rejoined the spinal cord as he had earlier stated. Someone had leaked the results of the Hospital review committee, and Drucker had no choice but to address reporters.66 The timing could not have been worse for Murray. While he was out of town accepting a prestigious national honour, Drucker dropped the bombshell. "On review of the operative record and x-rays, it is evident that no operation was performed on B.P. to shorten the cervical spine nor was the spinal cord transected and rejoined," announced Drucker.67 The operation that had been hailed as a medical miracle ten

65 Ethel Kerr wrote, "I should really be writing to congratulate 'the powers that be' for at least giving you some recognition for all the many contributions you have made. I am very glad you received this honour and just hope that many more such will be coming your way." National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 30, File 3 Correspondence Ethel Kerr to Murray, 7 July 1967. See also Volume 49, File 5 Order of Canada Investiture Program and Peter Sypnowich and Janice Tyrwhitt, "Dr Murray's "Miracle" — The Whole Story," The Star Weekly Magazine 16 December 1967, p.6.


days ago never took place, according to Hospital investigators. Media coverage of the story intensified dramatically. Bold headlines were printed: "Spinal Cord Not Joined," "Surgeon Discounts Spinal 'Miracle'," and "Dr Murray Controversy Still Boiling." After reviewing B.P.'s hospital records and several patient examinations, Hospital investigators reported that B.P. had undergone a decompression of the spinal cord, a 30-year-old procedure. Part of one vertebra had been operated on and a piece removed, relieving pressure on B.P.'s pinched spinal cord. Rumours were circulating about the report and Drucker felt compelled to make a public statement. This decision was supported by Wallace who felt that the "public good took first priority in this situation." Drucker told reporters that he had not been successful in contacting Murray in Ottawa.

Murray was stunned by the newspaper headlines the next day. He refused to talk to reporters while in Ottawa, and drove back to Toronto on November 25th. Murray was angry that the Hospital had breached their agreement on issuing press statements. (Drucker told reporters that there was no such agreement.) When Murray and his family arrived home, two reporters met them at the gates. "Don't let them in," said Murray. "I'm not talking to the press!" His wife and daughter bantered a bit with the reporters before they left. An hour later, a CBC television crew arrived. They eventually made their way into the house, and Warren Davis interviewed

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68Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Members of the Board of Trustees, 29 November 1967.


Murray. Davis asked Murray to set the record straight. "We operated on a man and several of them, with improving recovery," began Murray. "We operated on the cord. We did what was necessary. In this man we didn't take out a vertebra as Drucker said and I never said we did. Now that's the truth. I've operated on a man's cord. He's moving, he's doing well. Isn't that enough?" What about Drucker's statement, asked Davis. "Well he's off the beat. He doesn't know what he's talking about," replied Murray. "I didn't claim anything that I didn't do. I operated on the man's cord and he's recovering now. That's good enough." Davis asked, "Did you cut the cord?"

Murray's temper flared: "I'll cut your goddam head off if you don't shut up. I'm not going to have any of this bunk around here. None of it." Emotions were high. When Davis asked Murray again about the procedure, Murray exploded: "You get the hell outa here." There was a brief scuffle between Murray and the television crew before Mrs. Murray and her daughter ushered them out of the house.71

The question every reporter wanted answered was: "did Murray do what dozens of doctors thought he said he did?" Everyone thought that Murray had transected the spinal cord, removed part of the spinal column to place the cord in apposition, and that, for the first time, the spinal cord had regenerated. Hospital authorities announced that this had not been done on B.P. and they were now investigating Murray's other surgical cases. It was all a misunderstanding, argued

71Peter Sypnowich and Janice Tyrwhitt, "Dr Murray's "Miracle" -- The Whole Story," The Star Weekly Magazine 16 December 1967, p.4 [transcript of interview]. See also earlier CBC broadcast: National Archives of Canada, Audio-visual materials, VI 8402-0035, ISBN 16510, "The Way It Is," 19 November 1967. Murray later scribbled out an angry letter to Judy LeMarsh, head of CBC television, to lay a complaint against Davis in particular. Murray wrote, "We [were] invaded by at least a dozen men and one woman. They had cameras, lights, tape recorders. They were on private property without permission and took tape recordings without permission. This they presented to the public over T.V. and the news without permission. It was like a spy system of the gestapo." It is unknown whether this letter was ever typed and mailed to CBC. National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 39, File 29 Correspondence Murray to Judy LeMarsh, n.d.
Murray. At no time did he claim to have shortened B.P.'s spine. He had been misinterpreted from the outset.\textsuperscript{73} "Some assumed that in all patients I resected both cord and vertebral column, confusing this with the statements made about the experimental animals," explained Murray. "In the eight patients I have operated upon I removed varying amounts of bone in each depending on the requirements."\textsuperscript{74} Nevertheless, as one reporter pointed out, Murray had ample opportunity to correct the impression that he had performed eight spinal transections.\textsuperscript{75}

It would be unfair and inaccurate to attribute the whole controversy to misquotation by journalists. "It was very wrong of Murray to imply he did something he did not do, but don't let this stop us from being humane," pleaded K.S. Edey, Information Officer at the University of Toronto. "I am among the fortunate ones who have not been involved in any Faculty-Murray confrontations over the years. Therefore I could argue, 'Here's an old man who wants one more achievement. When you and I are 73 we may want to score another goal too.'"\textsuperscript{76} Murray's age was a factor raised by several Toronto medical men. "Dr Murray is seventy-three and this should

\textsuperscript{72}Mack Laing, "Dr Murray Controversy Still Boiling," \textit{Toronto Telegram} 27 November 1967.

\textsuperscript{73}"Spinal regeneration: the uproar grows," and "Error in Interpretation," \textit{The Medical Post} (Toronto) 5 December 1967.

\textsuperscript{74}National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 39, File 29 Statement written by Murray, n.d.


\textsuperscript{76}University of Toronto Archives, Office of the Dean, Correspondence A76-0044/231, File 21 Dr Gordon Murray, Correspondence K.S. Edey to A.L. Chute, 29 November 1967.

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perhaps be taken into consideration," wrote Chute.⁷⁷ "He is getting older and he may be reaching for one last success," commented Jousse.⁷⁸ These charitable comments were offered amidst Toronto gossip that the 73-year-old's mental capacities were slipping.⁷⁹ These medical men had seen these types of cases before: doctors who continued to practise beyond the limits of age or fitness because they could not bring themselves to give up the occupation that has been their life.⁸⁰ Age was used to explain Murray's unprofessional actions, and Hospital authorities scrambled to neutralize the situation created by the grandstanding, pathetic old man.

Still, Murray had many loyal supporters. One woman, in a letter to the Toronto Hospital, wrote, "I think Dr Murray should be treated better than some quack on his past record alone, and the money made available to him to go ahead with his research and train younger men. These people live (or should I say exist) a miserable life and anything you could do to help this along would be appreciated."⁸¹ But possible advances in treating paraplegia soon became secondary to exposing past and present rivalries between Murray and Hospital authorities. "They [Toronto medical men] love picking on him," Roland Michener told reporters.⁸² Angry letters were

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⁷⁷University of Toronto Archives, Office of the Dean, Correspondence A76-0044/231, File 21 Dr Gordon Murray, Correspondence A.L. Chute to A. Tricot, 8 January 1968.

⁷⁸University of Toronto Archives, Office of the Dean, Correspondence A76-0044/231, File 21 Dr Gordon Murray, Correspondence A.T. Jousse to Ludwig Guttmann, 29 November 1967.

⁷⁹Many medical men believed that Murray had ruined his reputation by staying too long, and not retiring earlier. Yet one resident commented on Murray's remarkable surgical dexterity "even at that age". Personal interviews by author, Toronto.

⁸⁰As Jurgen Thorwald says, "Doctors, like other mortals, do grow old, do lose the full possession of their faculties and make professional mistakes because of this." See Jurgen Thorwald, The Dismissal: The Last Days of Ferdinand Sauerbruch (New York: Pantheon Books, 1962).

⁸¹Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence Mrs E. Reynolds to Thomas Bell, 16 January 1968.


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printed in Toronto newspapers: "I cannot understand why a man of Dr Gordon Murray's stature should be subject to such criticism from the medical profession," wrote Louis Galperin.\(^{83}\) "... This is indicative of the terrific politics that goes on behind the scenes among the top brass in hospitals," wrote Harold Fine. "So let us give Dr Murray a vote of confidence and the moral support such as he has deserved, and not received, during all the years he has served humanity, limited by lack of support and assistance."\(^{84}\) Another Murray supporter criticized the media: "To discourage such a brilliant mind [Murray] with disgraceful publicity is indeed shortsighted. It was particularly mean to publicize his spontaneous outburst when making statements in the midst of turmoil. This was a striking example of an interviewer's disregard for decency in the desire to produce the 'sensational'."\(^ {85}\)

Other supporters wrote encouraging notes to Murray directly. One family wrote, "We want you to know how deeply concerned and heartsick we are for you and your very devoted family to be caught in this bizarre nightmare. The injustice is incredible that such a wonderful doctor and human being as you, who has dedicated his life to help mankind could be treated so shabbily. We are appalled and shocked to think that your colleagues would question your integrity. Jealousy would have to be their only motive and it is very disillusioning that civilized, educated men would


resort to such means." Floyd Chalmers wrote, "... it [Order of Canada medal] reminded people of your magnificent work over a long term of years and got some perspective into this 'dispute' which is the product of long-standing jealousies which I would have hoped to see disappear with the passing from the scene of some of the old TGH clique ... Their conduct is most unethical and non-professional ... [You] should pay no attention to any statements or comments, many of which have already been based upon obvious pettiness and malice and plough ahead." 87

In early December, Murray agreed not to perform any more spinal cord operations, and he discharged all his waiting paralyzed patients. 88 A form letter was sent out in to those who requested the operation, saying "... there will be some delay ... we have you on a list for call-in ..." 89 Until the Hospital finished its investigation, there was to be no further public statements made by either Murray or the Hospital. Their sudden silence left the press and public with many unanswered questions. Most importantly, did Murray perform a medical miracle or not? 90 An editorial in the Medical Post outlined the confusion: "What are we to believe? Are we to be


90 Peter Syponowich and Janice Tyrwhitt, "Dr Murray's "Miracle" -- The Whole Story," The Star Weekly Magazine 16 December 1967; Marilyn Dunlop, "Doctor watchdogs 'too busy to check',"
stampeded by the public press into picturing Dr Murray struggling single-handed against the malice and indifference of the department of surgery for which he works? Or are we to take the view of the department of surgery at the Toronto General Hospital that Dr Murray's evidence of spinal cord regeneration is unscientific and in one case even suspect?" Regardless, it was bad for the profession. Sidney Katz agreed, calling the dispute "a tragic comedy of errors ... characterized by indiscretion, concealed truths, the violation of scientific principles and unanswered questions." No one involved in the controversy could be absolved completely.92

Behind the scenes, Hospital authorities were working towards finding more complete answers. Murray agreed to meet with senior members of the department of surgery on December 13th in an effort to work together to decide how to proceed with his spinal cord research.93 Hospital medical men wanted to set up a "scientifically acceptable experiment to assess Murray's claim".94 Early in the meeting, Drucker stated that Murray would receive Toronto support for his research but only if he cooperated. Hospital protocol had to be followed; experimental operations had to be approved through proper channels in advance. Murray said he would work with a Hospital team only "if the Hospital lifted restrictions on his spinal cord operations"

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91 "Editorial Comment: Dr Gordon Murray," The Medical Post (Toronto) 5 December 1967.


and "if they corrected every statement in press under his name". Murray mentioned specific incidents in which he had been slandered: Drucker's statements at the press conference had implied that his research was doubtful and "made him out to be an amateur or charlatan." Worse yet, Drucker's announcement that Murray had not transected B.P.'s spine made for an "embarrassing situation in Government House." Drucker repeated that there was "only one way to do research" and that was the "scientific way" with adherence to protocol. The other doctors present at the meeting jumped in at various times trying to gain more information on the nature of Murray's work. How many operations had Murray performed? Eight, responded Murray. Had he shortened the cord of any of these patients? In one, answered Murray, but he refused to elaborate. Murray doubted that they were interested in supporting his work.

Drucker's only intentions were "to smear my name, to damage my reputation and to limit my hospital privileges." The Hospital was trying to "cut him off" and, as had happened in the past, he "would be completely left out, having introduced and developed it." He dredged up past offences when he had "been burnt" or "blackballed" by senior Toronto medical men. Murray jeered that the meeting was nothing more than a "Kangaroo court." In the end, the meeting allowed Murray to express his anger face-to-face with his colleagues, but settled nothing. It served only as a forum for Murray to vent his long frustration with the Toronto medical establishment.


97Two days later, Murray attended the annual awards presentation ceremonies where he was presented with a City of Toronto medal of merit. The furor over his spinal cord procedure was taking its toll on Murray. "I don't deserve it ... I've made nothing but horror, desperation and dissatisfaction,"
Hospital authorities did not want to issue any more public statements until the Hospital investigation was complete and a full report was represented to the Medical Advisory Board. Still, a Hospital statement was prepared and kept on hand for release if necessary. Wallace told Hospital staff, "I sincerely hope that it won't be necessary and that we can allow the heat to dissipate before an official statement is released." 58

The long-awaited Hospital report on Murray's spinal cord operations was finally presented on December 21st to the Medical Advisory Board. Dr. William Anderson, Chief of Surgical Pathology at the Toronto General Hospital, reported that "there were no improper procedures undertaken by Dr Murray but that, with the exception of one case, the procedures could only be termed exploratory laminectomies. In one case it was recorded that the 5.5 centimetres of the spinal cord had been excised as well as 2 1/2 vertebrae ... no subsequent information as to the clinical course of that particular patient was available ... In the case of [B.P.], the committee did not find that shortening of the body spine or excision of the spinal cord had been recorded or undertaken." 99 The Hospital released an official statement to the lay and medical press (the same statement that had been prepared before the report was written). No evidence of a major advance in the treatment of paraplegia had been made in Toronto, and the Hospital apologized for raising the hopes of paraplegics around the world. 100

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58Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Miss M.J.P. Lundie, 19 December 1967.

99Toronto General Hospital Archives, Minutes of the Medical Advisory Board, 21 December 1967.

100Toronto General Hospital Archives, Public Relations Department, Press Statement from the Toronto General Hospital on the Treatment of Spinal Cord Injuries; Minutes of the Medical Advisory Board, 21 December 1967.

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Newspaper headlines read "No Spinal Miracles by Murray" and "Dr Murray's Spinal 'Cure' Denied." No "miracle operation" was performed on B.P. — it had been routine surgery. Only one of Murray's eight patients received the complete spinal operation, and it had failed. P.D., of Convoy, Ohio, was still paralyzed from the waist down, still dependent on wheelchairs and crutches and still unable to control his bladder and bowel movements. He was also shorter than he was before. P.D. told reporters "the operation didn't help me -- didn't hurt me" but he was firmly convinced that Murray was on the right track. Murray commented that P.D.'s "condition might be due to lack of proper physiotherapy following the operation," and it was unfortunate that he was lost to follow-up. Reporter Ken MacTaggart called P.D. a pawn in the medical controversy. Legal and ethical restrictions prevented the Toronto Hospital from reporting further on P.D.'s case. If P.D. was a successful case, why was Murray not parading him in front of his medical colleagues? Murray replied that he could see no benefit in producing P.D. now.

Hospital authorities were anxious to end the controversy and the negative press and were trying to move quickly to do so. Murray was enraged with how he was being treated. In a letter to Thomas Bell, Chairman of the Board of Trustees, Murray wrote,


103 Ken MacTaggart, "Murray's Spinal Patient Called a Pawn in Medical Controversy," Toronto Telegram 17 January 1968.
The recent world approval and the spectacular results of the heart transplantation by Dr Christian Barnard in South Africa emphasizes the contrast in the response in the Toronto General Hospital ... All my patients are alive, none are worse and several are much improved, whereas Dr Barnard's single patient is dead. Why then is there the contrast in the medical assessment? ... The accusations are made by those who know nothing about this investigation and refused to cooperate when I requested such, on several occasions. Without justification they have brought the work to a halt and have denied possible improvement and hope to those in utter despair. Is this a desirable attitude of the Hospital? ... This is submitted to express my impressions on what I am sure must give you some concern, whether some of the medical staff on the Toronto General Hospital have the proper perspective. 104

Bell's reply, prepared by Wallace's office, endorsed the Hospital's position. 105

As a courtesy, Wallace sent Murray copies of the Hospital's press statements before their release. He made it clear that their findings were planned for release with or without Murray's backing. Soon thereafter, this Hospital statement was published verbatim in Paraplegia and the Journal of Neurosurgery “in order that the Profession be made aware of the true situation.” 106 Murray responded with a multi-page, heated letter.

Enclosed is a copy of a statement sent to the board and to several other parties involved. This is a statement of facts about which none of you have taken the trouble or interest to enquire. You all have treated this as a whimsical fancy and have treated me as an inexperienced upstart without scientific background. As already stated little do any of you know the immense amount of work that was done over several years with carefully studied results and proof both under the microscope and

104 Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence D.W.G. Murray to Thomas J. Beel, 4 January 1968.

105 Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence Thomas Bell to J.D. Wallace, 5 January 1968; Correspondence Thomas Bell to Murray, 16 January 1968.

with recovery of function. Moreover the operations on none of the patients was the routine exploration decompression and laminectomy ...

As this work has received wide acclaim in four continents and is of great importance to so many, it obviously will be developed further, elsewhere than under the vindictive community of Toronto General Hospital. The reaction here is so typical of events on former occasions; [and he lists numerous past procedures] ...

If your committee had been in charge it would all have been stopped -- as you now intend to do with the spinal cord work. This effort on your part has already damaged the Toronto General Hospital in public opinion, as evidenced by many hundreds of letters ...

As proof are more than 1000 letters many from doctors all of whom without exception have commented on yours and the Toronto General Hospital's committee's stupidity and all have expressed confidence and faith in me and my work. It would do you good to read a few hundred of these. Having obstructed the most important advance in the care of paraplegics and quads ... the interesting question arises, what is your committee's motive? In brief it is obvious your efforts are to annihilate me. All Dr Drucker's statements have cast doubt on my experience and integrity. He has evaded the essential point that the patients are recovering. Dr Drucker's insidious effort to influence City Hall, with the purpose which it accomplished of damaging me, was a further set back to this vital work in which I am engaged. In scurrilous fashion he has played the man and not the ball, which I doubt if either of you know, isn't cricket ... If both you and Dr Drucker contribute to the Toronto General Hospital's reputation one tenth as much as I have you will have to improve your record.107

In Murray's mind, the shabby treatment that he had received from Hospital authorities over his spinal cord research topped a career of frustration and obstruction from the same medical community.

On January 22nd, Hospital authorities made one final announcement. The Toronto General Hospital would not be supporting any further studies of Murray's controversial spinal cord surgery. Murray had declined to participate in a hospital

committee to establish an acceptable protocol towards continuing this research.\textsuperscript{108} The matter was thus closed for the Toronto General Hospital. Murray refused to offer a statement to the press; he had been beaten.\textsuperscript{109} Relative to past media attention, this final announcement by the Hospital was anticlimactic. After ten weeks of media attention, the spinal cord controversy was finally over. The public had been deceived -- Murray could not make paraplegics walk again. "It is too bad that the publicity caused us all some bad moments," wrote Wallace.\textsuperscript{110} The dispute had been ugly and damaging to all involved.

Murray did not abandon his spinal cord research, despite forced retirement from active practice at the Hospital on July 1, 1968.\textsuperscript{111} Over the next few years, several of his spinal patients wrote letters to Murray, keeping him abreast of their improvements and encouraging him to continue with his research.\textsuperscript{112} Murray did not


\textsuperscript{109}After weeks of media interference and frustration with Hospital authorities, Murray decided to end it and thus to "lose". It had been ugly and damaging, increasing Murray's bitterness against the Toronto medical community. Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July and 10 July 1996, Toronto.

\textsuperscript{110}Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Murray, 30 January 1968.

\textsuperscript{111}In 1963, the Board of Trustees approved a change in the by-laws that required Consultants to retire from active practice in the Hospital at age 70. Five years of grace were allowed before the change became effective on July 1, 1968. Murray was thus grandfathered and retained his operating privileges until 1968. Had this not been the case, no doubt Hospital authorities would have taken action to restrict his Hospital privileges. Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Members of the Board, 29 November 1967.

give up trying to get his research findings on spinal cord regeneration published. The
Journal of the American Medical Association, the American Journal of Surgery and
the Journal of Trauma all rejected his paper. One anonymous reviewer wrote, "The
report is unscientific and not capable of contributing to the problem which the paper
purports to discuss." Another reviewer said, "on the basis of the information reported,
the lack of complete objective preoperative findings, and the scanty incomplete
postoperative findings, one must conclude that there was no evidence of spinal cord
regeneration in this case, or if there were, it is not presented in this paper." Murray's paper was finally accepted by Panminerva Medica, an obscure Italian
medical journal, and published in September 1972. It provoked no reaction from
either the medical or public press. Moreover, neither this article nor his 1965 article
was ever cited thereafter by others working in the field.

The spinal cord controversy was "a tragic comedy of errors and needless
humiliation." Journalist Sidney Katz as well as others following the dispute were
saddened and embarrassed by these events. What might have been yet another
medical feat for Murray and a splendid finale to an exceptional surgical career instead
resulted in a public fall from grace. More lamentable was the false hope given to
thousands of paraplegics who believed they would be able to walk again. Murray,

113National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 43, File 26
Correspondence Charles G. Roland to Murray, 31 January 1968; File 29 Correspondence Murray to
American Journal of Surgery, 28 July 1970; Correspondence Journal of Trauma to Murray, 9
November 1970.
hospital authorities, medical professionals and reporters involved in the spinal cord controversy all contributed to the ugly escalation and mishandling of the dispute.\textsuperscript{114}

Most obviously, the Toronto East General and Orthopedic Hospital Research Foundation fundraising dinner, on November 14th at Inn-On-The-Park, was an inappropriate occasion at which to announce the discovery of any "medical breakthrough". Murray misjudged the venue. At 73 years of age, he had become impatient and frustrated with protocol, and was upset over the recent journal rejection of his paper on the subject. He was driven to get others interested, if not directly involved, in his spinal cord research. He was too old to carry his ideas to a successful conclusion and he wanted younger Toronto men to take up his work. A recent visit by an enthusiastic American neurospecialist, Dr. E. Shannon Stauffer from the prominent spinal cord centre Rancho Los Amigos Hospital in California, probably also contributed to Murray’s impatience and decision to announce his breakthrough surgery.\textsuperscript{115} (There is no published record that confirms Murray’s belief that soon after his visit Stauffer performed the Toronto procedure successfully in California or that he altered his research to incorporate Murray’s spinal cord regeneration theory.\textsuperscript{116})

The day after the dinner, Murray stated that he had been unaware that reporters were present at the event. Not true, said several reporters who had talked with him before


\textsuperscript{115}Peter Synowich and Janice Tyrwhitt, "Dr Murray's "Miracle" -- The Whole Story," \textit{The Star Weekly Magazine} 16 December 1967, p.3.

\textsuperscript{116}Dr. E. Shannon Stauffer was working with other neurospecialists at the Rancho Los Amigos Hospital in California studying incomplete spinal cord injury lesions (ruling out complete lesions) and return patterns of gross functional recovery from 1956 through to the 1970s. See Albert Bosch, E. Shannon Stauffer and Vernon L. Nickel, “Incomplete Traumatic Quadriplegia: A Ten-Year Review,” \textit{Journal of the American Medical Association} Volume 216 (April 19, 1971): 473-8.
he made his presentation. Murray did not want to appear as a doctor who courted publicity, as he had been criticized by other medical men in the past. The press had shown minimal interest in his spinal cord research on earlier occasions, so what did he have to worry about? Naive or cunning, he did not anticipate such overwhelming public and professional reactions to his after-dinner speech.

Murray's defense of "misinterpretation", when it became evident that he had not performed the spinal transection on B.P., was weak. There had been numerous occasions on which Murray might have clarified the issue; he allowed reporters and the world to believe that B.P. was clinical proof of his transection surgery. For Murray, the end justified the means; he was onto something and, with greater resources, his research on spinal cord regeneration would be recognised as pioneering. He believed that his surgery would work, and time would confirm it. In his mind, he had made an important discovery. It is possible that there was no intended fraud; he had deceived himself as well as others in his ability to perform miracles. "All I have tried to do is help the sick and maimed," wrote Murray. He reacted angrily to accusations of 'experimenting' on humans. These spinal cord operations were not...

117Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Members of the Board of Trustees, 29 November 1967. Murray later stated that he understood the Foundation dinner to be a meeting of the hospital staff and some lay supporters of their research projects, and thus not open to the public. When he alerted the organizers to the presence of reporters, they assured him that "it would be under control." National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 39, File 29 Statement written by Murray, n.d.; Personal interviews by author, Toronto. "Nuts," said Marilyn Dunlop, medical writer for The Toronto Daily Star. "I phoned him before the dinner and he told me he was bringing along a quadriplegic he'd operated on, so we covered it." Peter Sypnowich and Janice Tyrwhitt, "Dr Murray's "Miracle" — The Whole Story," The Star Weekly Magazine 16 December 1967, p.5.

118Scientists accused of fraud commonly cite the excuse "the ends justified the means". See Norman Swan, "Baron Munchausen at the lab bench?" in Fraud and Misconduct in Medical Research edited by Stephen Lock and Frank Wells. Second Edition. (BMJ Publishing Group, 1996), p.140.

experiments but treatments for sick patients, marking Murray as practitioner rather than scientist.\textsuperscript{120} He was less interested in protocol and scientific measurements than in clinical application and results. The fact that several of his patients had improved because of the surgery was enough justification for him to continue.

Murray belonged to an earlier generation of clinical investigators. Like his work on cancer, his spinal cord research lacked objective standards, clinical trials, and strict methodology in a time of expanding research specialism and sophistication. On various occasions, Murray refused to collaborate with neuropathologists and neurophysiologists or to share his research data with other specialists in the field.\textsuperscript{121} His secretive nature worked against his greater goal of enticing others to carry on his work. It also damaged his reputation and left him defenceless against accusations of scientific dishonesty and “broomcloset” research.

Murray’s older style of conducting research was unacceptable to the Toronto medical community, and rightly so. Unfortunately, spokesmen for the Toronto General Hospital mishandled Murray’s announcement. Whether Murray’s research was good or bad science became secondary to what the public perceived as unfair treatment of Murray and his work by the powerful Medical Establishment and jealous individuals. "In our society one man standing out against the medical establishment, or any other establishment, has large sections of the public on his side. Fair or not, that's the way it works," said Information Officer K.S. Edey. In public relations terms, 


\textsuperscript{121} Murray declined to participate with a Toronto General Hospital research team as well as previous suggestions by various funding agencies to team up with neuropathologists. Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to Murray, 16 January 1968; Personal interviews by author, Toronto.
Edey warned Hospital authorities that, "Dr Murray, it should be admitted, had the status of hero to begin with. He could not be dismissed as a charlatan or a quack."122

Leading the Hospital response was Dr. William Drucker. Drucker was not well liked by his medical colleagues,123 nor did he receive particularly good press during the dispute. Murray took Drucker's public expressions of skepticism personally, later extending his anger and bitterness to include all senior Hospital medical men. "Nothing but treason and vindictiveness, "'[they are] trying to annihilate me," wrote Murray privately. "The struggle between good and evil is just as vivid today as it was years ago."124 Drucker sought to protect the reputation of the Hospital, and was bound to do so, but still some individuals thought he was perhaps too zealous to prove Murray wrong. "By all means, give the public all the facts about the new spinal operation. But why was Dr Drucker in such haste that he had to make his announcement during the brief period that Dr Murray was out of town? ... Why, through the lack of tact, should a distinguished medical scientist -- whose life has been devoted to the sick -- be subject to unnecessary humiliation?" argued one reporter in defense of Murray.125

122University of Toronto Archives, Office of the Dean, Correspondence A76-0044/231, File 21 Dr Gordon Murray, Correspondence K.S. Edey to A.L. Chute, 29 November 1967.

123William R. Drucker was an American, newly installed as Professor of Surgery and Surgeon-in-Chief in Toronto in 1966. Drucker was hired to re-organize the department of surgery, specifically to apply new ideas about medical education and research that had worked at his previous university, Western Reserve in Cleveland, and there was obvious reluctance from staff. His informality and ideas of democratizing the department ran contrary to past Toronto practices of professor formality, seniority and rites of succession. Moreover, Toronto professional admiration and respect came grudgingly because Drucker was an “outsider” within a strong Toronto old boys network. Drucker left in 1972 when offered the position of Dean of the School of Medicine at the University of Virginia. Personal interviews by author, Toronto.

Drucker found himself in a tough position, swarmed by media people anxious to keep the controversy alive and flooded by inquiries from the medical profession for clarification of the procedure. Never did he attack Murray personally, aware of his fame among the public and Toronto medical men. Drucker focused on the fraudulence of the procedure. At times, Hospital and university authorities were noticeably silent and he alone had to respond to accusations from the media or Murray. As Hospital and university authorities saw it, "he [Drucker] was up to his neck in the affair anyway."126 Drucker played an unpopular but necessary role in revealing the details of Murray's spinal cord operations.

For ten weeks, the press released dramatic stories. Journalists reported the miraculous surgery without checking its validity with the Hospital; they as well as the public congratulated Murray without suspecting otherwise. The media was pro-Murray and anti-Establishment, and they only relented when the facts could no longer be ignored. Then the story was dropped altogether. They had been hoodwinked. These medical reporters had few qualifications and little understanding of science and of the involved ethical issues to report medical procedures and alleged cures critically and analytically. It resulted in a great disservice to the public, and especially to individuals suffering from paraplegia. It fell upon their doctors to explain medically why the publicized claims were untrue.127 Public beliefs and expectations in dramatic medical cures began to wane, and their cynicism grew.

126University of Toronto Archives, Faculty of Medicine, Office of the Dean, Correspondence A76-0044/231, File 21 Dr Gordon Murray, Correspondence K.S. Edey to A.L. Chute, 29 November 1967; Toronto General Hospital Archives, Public Relations Department, Spinal Cord Controversy, Correspondence J.D. Wallace to W.R. Drucker, 28 November 1967; Correspondence W.R. Drucker to J.D. Wallace, 4 December 1967.
The spinal cord controversy ended Murray’s career. The dispute had damaged his reputation publicly and professionally. Murray had deceived himself, his patients, and the public into believing he had made an important discovery. He was convinced that his spinal procedure restored lost function in individuals with spinal cord injury, and he confused his clinical cases with his laboratory experiences, consciously or not, as proof of its success. Like his kidney transplant patient and his cancer sufferers, he balked at those who demanded scientific data despite witnessing B.P., previously a quadriplegic, stand, sit, smoke, and wave. He was driven to solve these medical challenges, and convinced that he was on the right track if more help and money were forthcoming. When the offer was made by Drucker to do this, Murray refused to surrender control of his research. At age 73, he was too much a lone investigator. It was a characteristic inconsistency in his career: he wanted assistance but was never able to relinquish data or direction of his work to other, more qualified researchers.

In the end, he had mistakenly overstated his research and misled the medical community, and this was inexcusable and unforgivable. Hospital medical authorities had no choice but to expose the truth. Many Toronto medical men had questioned the rigours of Murray’s research, dating back to his renal therapy and cancer investigations. Friends and colleagues were saddened by the outcome of the controversy: allegations of fraud and scientific dishonesty seemed unfitting for this surgical pioneer. It was a harsh ending to an extraordinary career.128

Even more unfortunate, Murray’s work on spinal cord regeneration had little impact on the field of spinal cord injuries. Several years after the spinal cord

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*Association* Volume XXIII, No 1 (Spring 1968): 11-12.
controversy, Dr William Windle of New York University called together a small group of neuroscientists to launch a new research effort into spinal cord regeneration. No promises were made. Historically, scientists dismissed research in this field as a waste of time. But new advances, such as the recently acquired understanding of cellular behaviour, now increased their interest in spinal cord regeneration.\(^{129}\) In 1980, the First International Symposium on Spinal Cord Reconstruction was held “to review again the problems of spinal cord regeneration.” New research was presented, including experiments similar to Murray’s transection procedure, but no encouraging results were produced. Despite increased understanding of nerve regeneration and central nervous system processes, “functional restitution after spinal cord transection remains an unrealised dream,” concluded conference participants.\(^{130}\) Researchers continued to study the problem but experiments conducted as recently as 1997 offered negligible gains towards the goal of repairing severed spinal cords.\(^{131}\) Three decades after Murray’s premature announcement, medical scientists have still not found a cure for paraplegia.


Chapter 8

Aftermath: The End of a Career

After the spinal cord controversy, Murray eased into retirement. He spent the next several years tinkering at the Gardiner Foundation, attempting to finish his work on various problems and to publish his findings from his cancer and spinal cord research. But he spent fewer and fewer hours in the lab. He also stopped operating, closing his private practice when he was no longer granted surgical privileges at the Toronto General Hospital in July 1968. After forty years of balancing a busy private practice, operating, teaching and doing clinical research, Murray now had time on his hands. He took numerous long lunches at the Hospital, catching up with former residents, colleagues, even Professor of Surgery, W.R. Drucker. He chatted with the librarians over cups of tea at the Academy of Medicine. He indulged in his love of the outdoors and ornithology, and even tried his hand at writing children’s books.¹

¹Murray wrote a children’s book entitled Sandy about a stray collie adopted by a family. The manuscript was never published, rejected by several Canadian publishing companies. National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 41, File 26 and 27, “Sandy” draft manuscript, n.d.;
During the early 1970s, several people approached him to write his biography.² He was cooperative, but nothing came of it. Murray's celebrated career also attracted the attention of the Dominion Archivist who wrote to Murray to inquire about his personal papers. An agreement was reached and Murray's will was amended to include the deposit of his records in the Public Archives of Canada after his death. Undoubtedly this pleased Murray. His prominent role in Canadian medicine was recognized and would now be preserved.³

On June 30, 1974, the Gardiner Medical Research Foundation was closed. The house was sold, all debts were cleared, and the remaining funds deposited in the Foundation research account.⁴ Later that year, Murray terminated his medical practice insurance policy.⁵ He did not continue his research at home, but turned to other activities. He puttered around the house, tended to his apple trees, did more woodworking and spent time with his wife Helen.⁶

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⁶Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.
In December 1975, Murray and his wife drove to Aylmer, Quebec to spend Christmas with their daughter Rosalind and her family. He helped his son-in-law chop wood and put in a new oak porch floor. He visited with Rosalind and played with his grandchildren. It was a delightful family holiday, and it would be Murray's last. Shortly after his return home, Murray suffered a massive stroke while having a cup of tea in his favourite easy chair. He was paralysed on one side, and almost totally blind. He was taken to Sunnybrook Hospital but later moved to Toronto General Hospital. There was little the doctors could do for him. Against resuscitation, Murray died January 7th, 1976, at the age of 81.7

The Murray family opted for a private funeral at Bloor Street United Church in Toronto, perhaps due to their unpleasant experiences with the media in the recent past. Many residents and colleagues attended the funeral and followed the procession to Paris, Ontario where Murray was buried with his wife's family, the Tough's. Helen and Rosalind received numerous notes of condolences from family, friends and medical practitioners around the world.8 Kind obituaries were printed in newspapers and medical journals, all praising a Canadian surgical pioneer. In all of these accounts, Murray's


8National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 52, File 16 Correspondence Elton Lent to Mrs. Murray, 9 January 1976; Correspondence Cam Tolton to Mrs. Murray, 11 January 1976; Correspondence William E. Ortved to Mrs. Murray, 19 January 1976; Correspondence A. Lawrence Abel to Mrs. Murray, 22 January 1976; Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July, 10 July 1996, Toronto.
controversial cancer serum and fraudulent spinal cord procedure were downplayed or omitted.9

In 1978, the Murray family donated the remaining Gardiner Foundation research funds to the University of Toronto.10 As a result, the Gordon Murray Lecture was established and has become an important part of Gallie Day, an annual clinical meeting arranged by the Professor of Surgery. The Gordon Murray Lectureship is intended as a venue for young researchers with new ideas, struggling or otherwise, to share their work with others.11 It continues to showcase leading medical scientists each year, and serves to honour one of their own surgical leaders. Ironically, after a lifetime of contention Murray's final tribute came from the University of Toronto and the Toronto General Hospital.

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10Before his death, Murray donated funds in 1975 to establish the following scholarships: The Helen Murray scholarship to Royal Conservatory of Music of Toronto from a $10,000 donation; The Rosalind Murray Bradford scholarship at the University of Toronto from a $10,000 donation; and a scholarship at the Oxford County Board of Education (his early education) from a $10,000 donation. As part of this trend, his wife and daughter donated a total of $55,000 to the University of Toronto in 1978 and 1979. National Archives of Canada, MG 30 B 110, D.W.G. Murray Papers, Volume 52 File 11, Medical Research Expenditures.

11Mrs. Rosalind Bradford, Interview by author, 26 June, 3 July and 10 July 1996, Toronto; Dr D.R. Wilson, Interview by author, 27 February 1996 and 14 April 1997, Toronto.
Murray's career is a story of triumph and tragedy – a personal drama of clashing personalities, individualism and ambition that took place within the changing contexts of medical education and training, surgery, clinical research and treatment, and medical reporting. He was a skilled surgeon and an innovative clinical investigator who received substantial recognition and honours for his many medical contributions during the better part of his life, but tragically ended his career an antiquated, professionally chastised, lone practitioner. He had not intended to become a “lone wolf”, as both the public and profession came to see him, but wanted to rise within the medical establishment within which he trained. His individualism and boldness, which had been earlier strengths, collided disparately with postmodern practices of specialism, collaboration, and research rigour. Inevitably he clashed with those medical men who promoted and upheld this new medical order. The “old-style” surgeon, who was more practitioner than scientist, stood out of place in the new research world.

In Murray’s case, personality and personal choices in relation to changes in the medical culture explain the unfortunate ending to his career. His early successes were a result of his surgical skill, medical knowledge, curiosity and commitment to medicine. Moreover he worked within the model in which he had been trained and excelled. From the 1920s to the 1940s, like many of his contemporaries, he practised as a general surgeon, who conducted clinical research part time, and made significant contributions. His interests led him to an expertise in vascular and cardiac surgery, although he resisted specialization. By the 1950s, by anyone interested in participating in these new, increasingly sophisticated fields sought postgraduate training in these areas as well as in research methodology and theory. Murray continued to tinker alone in the laboratory.
Moreover medical research now required significant funding and facilities, and was growing beyond the dabbling of inquisitive clinicians.¹²

Murray had difficulty adapting to these changes and the new medical culture. He also made critical errors in judgement. His decision not to continue into the new field of open heart surgery, to leave cardiac surgery altogether, and to work outside the university research structure show how he misread the future and misjudged his own abilities. His boldness and individualism were both his strength and weakness, and these characteristics brought him both success and failure. He had always conducted research alone and he saw no reason to change. He had investigated the clinical application of heparin apart from Best’s physiological experiments; he had performed his cardiac surgery lab work alone in the Banting Institute; and his artificial kidney machine was built without any hospital assistance. When he moved to the Caven Foundation, he further isolated himself physically and intellectually from Toronto researchers. This shift from the university to a private research institute marked a key turning point in his perception of his abilities and the obstacles he faced from the Toronto medical community. By this time in the 1950s, his work was driven by his ego, the need to please research sponsors, and his desire to repeat Frederick Banting’s famed career and to achieve final vindication. He came to believe in his own press, and it became his undoing.

An expansion in medical specialism, technology and team orientation was taking place, and Murray resisted. Surgeons were gaining expertise in, and limiting themselves to, newly developing specialties of the heart, chest, orthopedic and neuro-surgery, among

others. General surgeons were relegated to relatively minor procedures unclaimed by surgical specialists. The conduct of medical research was also transformed in this period. It moved beyond the dabbling of part-time investigators to full-time researchers trained in more objective, rigorous research methodology and techniques, applying new experimental models such as double-blind randomized clinical trials and working as part of interdisciplinary teams within larger, well-funded, technologically sophisticated laboratories. Murray entered areas of research in which he was not trained, and field specialists criticized his work as “amateurish”. He refused to work with research or clinical specialists, fearing loss of control and credit, when collaborative research was becoming the trend. Murray wanted to direct a leading research team within an autonomous unit or medical institute, but secrecy, antagonism, and later hubris vied with his surgical skill, research drive, commitment and intelligence. The Caven (later Gardiner) Foundation was never more than a third rate research facility due to Murray’s limited leadership and inability to fit into the new postmodern research world.

Murray was simply unable and unwilling to adapt to many of these changes occurring in the medical culture at mid-century. He remained a surgeon and a product of the modern era of surgery. His lone practices and experimental approach to medical problems, which had been earlier strengths, became liabilities in this new research world. Furthermore, his work was evaluated less on his reputation and more on its validity as a research project (and more often than not found lacking by colleagues). In the latter years of his career, Murray continued to operate broadly on various surgical conditions and chose to explore new and complex research questions independently and ineptly, neither understanding nor exploiting the new medical culture.
In addition, the public’s belief and expectations in dramatic cures grew during Murray’s lifetime, perhaps further fueling his research dreams. Society had witnessed and benefitted from the remarkable changes and improvement in medical care. The discovery of salvarsan and insulin marked the beginning of sensational twentieth century treatments. Heparin, vascular surgery, heart operations, artificial kidney machines, sulfa drugs, the Salk and Sabin vaccines, organ transplantation and other new procedures reinforced the public’s faith and hope in their medical researchers. The press celebrated these medical advances and the individuals responsible. Reporters sought sensational success stories, at times acting irresponsibly in their enthusiasm to announce another cure. Yet their conduct was understandable. At this time, journalists had no specialized scientific training as medical reporters and acted as little more than passive conveyors of scientific and technical information, celebrating and “selling science”. Researchers, including Murray, welcomed favourable news coverage, seeing it as a means to enhance research support, often prepared press releases. Murray enjoyed favourable media reporting throughout most of his life, from his early work on heparin to respectful obituary notices. The press and public admired his dedication to medicine.

It was the challenge of understanding and treating desperate cases that intrigued Murray throughout his career. The rewards, fame and admiration that accompanied his successes reinforced his drive and commitment, and he tackled greater and more complex medical problems. His mind was too restless to stay with one project, especially after he had achieved success and he moved on to the next medical question almost immediately. From vascular surgery to heart operations, from artificial kidney machines to organ transplantation...
transplantation, from finding the cure to cancer to making paraplegics walk again,
Murray explored a range of medical problems and possible treatments. He judged people
by their contributions, and so demanded much of himself. But he was also human, his
foibles at times unseemly but present nonetheless. His career is a story of individualism,
and to a certain extent, the individualism of the surgical craft. Confident and ambitious,
Murray spent a lifetime searching for answers to some of medicine’s greatest challenges
during a period of tremendous surgical advances and research changes that eventually
overcame him.
Note on Sources

The most important source for this biography was the D.W.G. Murray Papers in the National Archives of Canada in Ottawa. This 52 volume collection (10 metres of material) holds patient files, personal and office correspondence, research notes, published and unpublished work, professional societies and associations material, newspaper clippings, and photographs. Deposited by his estate in 1978, these records cover his entire surgical and research career. Murray retained the services of a clipping service throughout his career, which accounts for a near complete set of newspaper clippings in the collection. Records of the Caven and later Gardiner Foundation, plus his extensive laboratory and experiment books, were useful to trace the progress of his various research projects. It is suspected that the family, if not perhaps Murray himself, destroyed sensitive letters. There are, however, drafts of his autobiography, other unpublished writings, and private scribblings that candidly present his views of Toronto opposition. Further to this, his two-volume autobiography, _Medicine in the Making_ (1960) and _Quest in Medicine_ (1963) were analysed as primary documents. He also published 78 articles in his lifetime; various drafts and related correspondence are in the collection. Unfortunately, only fifty
percent of the Murray collection is open to researchers. The other half of the
collection, which is predominantly patient medical files and some patient letters, is
closed until after the year 2008. I was granted restricted access to some of these files,
which allowed me to glean a certain insight into the breadth of Murray’s surgical
practice.

The more important secondary collections used in this study were records
from the University of Toronto Archives, The Toronto Hospital Archives, and the
Academy of Medicine Collection held at the Fisher Library in Toronto. Simply put,
these records presented the other side to Murray’s story. There are key letters, memos
and minutes of meetings between senior medical and administrative men at the
university and the hospital discussing Murray and his endeavours at various points in
time. Information from this material provided context to Murray’s account of events
as presented in his personal papers.

Many other collections of papers, published primary and secondary sources,
and interviews were used to increase and substantiate my research on Murray and his
career. A complete list is provided in the bibliography. My interviews and personal
correspondence with anyone that knew or worked with Murray were a vital part of my
research. With the exception of one interview, I did not use a tape recorder but took
detailed notes. Any quotations were taken from letters written to me in response to
direct questions. I hope that those who participated in these interviews and written
exchanges will find that I have interpreted these conversations in good faith. Most
individuals were enthusiastic and helpful; some provided me with additional personal
papers and photos that I would not have had access to otherwise. It was my first
experience with oral history, and one of the more enjoyable components of my
research.
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Dr. H. Hoyle Campbell, Surgeon - Orthopedic/Reconstructive
Dr. G.G. Caudwell, Surgeon
Dr. Bruce Charles, Physician, Internal Medicine
Dr. W.T.W. Clarke, Physician - Internal Medicine
Dr. Denton A. Cooley, Heart Surgeon
Miss Jean Dodds, Nurse on Ward C
Dr. William R. Drucker, Surgeon
Ms. Marilyn Dunlop, Medical Reporter, Toronto Star
Dr. Murray Enkin, Surgeon
Dr. James Kenneth Wallace Ferguson, Pharmacology, Director of Connaught Laboratories
Dr. Hugh R. Gallie, Surgeon
Dr. Bill Geisler, Physician
Dr. W.H. (Bill) Harris, Surgeon - Obstetrics and Gynecology
Dr. Robert Harris, Orthopedic Surgeon
Dr. Raymond Heimbecker, Surgeon
Ms. Joan Hollobon, Medical Reporter, Toronto Globe and Mail
Dr. Robert W. Jackson, Surgeon
Dr. James V. Maloney, Jr., Surgeon
Dr. T.P. Morley, Surgeon
Dr. Lloyd D. Maclean, Surgeon
Mrs. Reta (Smith) Macnab, Nurse on Ward C
Dr. N.T. McPhedran, Surgeon
Mrs. Alex (Margaret) Millar, Niece of Helen (Tough) Murray
Dr. John Moffat, Surgeon
Dr. Fred Moffat, Surgeon
Dr. T.P. Morley, Surgeon
Mrs. Rosina (Mrs. Hilton) Morris, Friend of Murray family
Dr. C.B. Mueller, Surgeon
Dr. Edward Mullens, Surgeon
Dr. Rowan Nicks, Surgeon
Mrs. Paul Phelan, Daughter of Mr. Percy Gardiner
Dr. J.C. Portnuff, Surgeon
Mrs. Marion Reid, Nurse TGH
Dr. Walter Roschlaub, Researcher Caven/Gardiner Institute
Dr. David C. Sabiston, Jr., Surgeon
Mrs. Ann Scholefield, Niece
Dr. John Scott, EEG (electroencephalography)
Dr. Harris B. Shumacker, Jr., Cardiac Surgeon
Mr. David Spurgeon, Medical Reporter, Toronto Globe and Mail
Mrs. Douglas (Mary McLean) Stewart, Family Friend
Mr. and Mrs. Clair and Amy (nee McLean) Stewart, Family Friends
Dr. Robert Stone, Surgeon
Mrs. Sheila Swanson, Librarian, Academy of Medicine
Dr. Neil Watters, Surgeon
Ms. Rosalind Waxman, Lab Technician, Banting Institute
Dr. D. R. Wilson, Cardiovascular Surgeon
III. Published Work of D.W.G. Murray

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"Surgical Treatment of Congenital Heart Disease (Tetralogy of Fallot)" CMAJ Vol. 58 (January 1948): 10-12.


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**Medicine in the Making.** Toronto: The Ryerson Press, 1960 (Volume one of autobiography).


**Quest in Medicine.** Toronto: The Ryerson Press, 1963 (Volume two of autobiography).


**IV. Articles, Books, Unpublished Accounts**


Moore, Francis D. “A Nobel Award to Joseph E. Murray, MD: Some Historical Perspectives,” *Archives of Surgery* Volume 127 (May 1992); 627-632.


