BUILDING A "CROSS-ROADS DISCIPLINE" AT MCGILL UNIVERSITY: A HISTORY OF EARLY EXPERIMENTAL PSYCHOLOGY IN POSTWAR CANADA

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Abstract

This dissertation presents an account of the development of psychology at McGill University from the late nineteenth century through to the early 1960s. The department of psychology at McGill represents an alternative to the traditional American-centered narrative of the cognitive revolution and later emergence of the neurosciences. In the years following World War II, a series of psychological experiments established McGill as among the foremost departments of psychology in North America. This thesis is an institutional history that reconstructs the origins, evolution, and dramatic rise of McGill as a major center for psychological research. The experiments conducted in the early 1950s, in the areas of sensory restriction, motivation, and pain psychology, were transformative in their scope and reach. Central to this story is Donald O. Hebb, author of *The Organization of Behavior* (1949), who arrived at McGill in 1947 to find the charred remains of a department. I argue that the kind of psychology Hebb established at McGill was different from most departments in North America; this is developed through a number of interwoven storylines focused on the understanding of a particular character of McGill psychology - a distinctive "psychological style" - and its broader historical importance for Canadian psychology, for North American psychology, and for psychology across the globe. This psychological style was an amalgam, embracing both the experimentalism associated with behaviorists and attention to subjective and emotional states associated with psychoanalytic and Gestalt theory. It contributed to the development of cognitive (neuro)psychology, but through avenues that lay somewhat outside the main scientific developments commonly noted in existing historical studies, which tend to neglect the role of emotion and embodied experience. This dissertation provides an account of the complex interplay of factors that affected the trajectory of psychology at McGill with attention to key individuals, department structures, and priorities; it examines how research institutions in Canada were built after the war; how various tensions and relationships shaped these early projects; and investigates the development of key concepts, theoretical views, research practices, and commitment to interdisciplinarity.

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Introduction

"The focal problem of psychology is found in the patterns of behavior shown by the whole animal of a higher species in adjusting to his environment over appreciable periods of time; to say the same in a different way, it is found in the mental processes of the higher animal" (Hebb, 1958, p. 8).

Just after his retirement in 1976, Canadian psychologist Donald Olding Hebb (1904-1985) reached out to then Minister of State for Science and Technology, Hugh Faulkner (1933-2016), to urge additional financial support for experimental psychology from the Trudeau government (Hebb, 1976). During the 1950s Hebb had overseen the Department of Psychology at McGill University. From his time as Chair, a number of important psychological discoveries can be traced. These include the mechanism of reward in the brain, the peculiar effects of prolonged sensory isolation, and insights that mark the early development of a psychology of pain in the 1950s. Twenty years later, in his letter to Faulkner, Hebb remarked that, "Any one of these by itself might have been a landmark in the study of man [...] all took place within a short period and may be seen as parts of the same spurt in the knowledge of behaviour, and they all took place in Canada" (Hebb, 1976). While it is unknown whether these examples convinced Faulkner to provide the requested funding, it is clear Hebb believed Canadian psychology had proven that it was continuous with biological science, as "a hard-headed scientific discipline" (Hebb, 1976). This physiological and neurological approach demarcates Canadian psychology in the mid-twentieth century from the American behaviorist tradition. A special issue of the *Canadian Journal of Psychology* started, "If there is any brand of psychology which is peculiarly Canadian, it is the field of brain and behavior, and especially that area of it which has become known as neuropsychology" (Kimura, 1983, p. 1). But why? This dissertation examines the early history of experimental psychology in Canada (specifically at McGill University) in order to address this question.

The history of psychology in Canada, compared to the development of the discipline in other parts of the work such as the United States and Great Britain, has been scarcely examined. In part this is due to the smaller size of Canadian psychology departments and research output. It might come as a surprise to readers that there was much history to be written from this period. Indeed, before World War II there was little in terms of experimental or academic psychology

outside a handful of universities (Wright & Myers, 1982). After the war, however, Canadian psychology underwent a surge of interest in psychological knowledge and expertise; and with funding came tremendous growth at Canadian universities (Frost, 1984; Wright & Myers, 1982). It is said that Canadian psychology 'came of age' in the 1960s (Wright, 1969). While this may be true for the nation as a whole, the decade that came before laid the groundwork for the organization, direction, and growth of experimental psychology that would follow.

To the extent that there is a traditional origin story of the emergence of psychology in Canada, much emphasis has been placed on the first experimental laboratory at the University of Toronto (e.g., Hoff, 1992; Wright, 2002; Green, 2004). However, much groundbreaking Canadian psychological research is attributed to the contributions of psychologists associated with the Department of Psychology at McGill University in the 1950s. It was during this postwar period that Donald Hebb extended his neuropsychological theories into the nature of learning and motivation while constructing early neural network theory, and his students, such as Brenda Milner, Ronald Melzack, and James Olds redefined the areas of memory, pain, and reward (respectively). Psychology at McGill and in Montreal during this period signaled the beginnings of radical psychological experiments today recognized as belonging to the fields of cognitive neuropsychology and neuroscience. Since the separation of the Faculty of Arts and Science in 1971 (Frost, 1984), the psychology department has remained firmly situated as part of the Faculty of Science, as a program of study within the Biological, Biomedical & Life Sciences. The faculty at McGill draws heavily from backgrounds in cognitive neuroscience, clinical neuropsychology, and biological psychology. Students interested in applying to the graduate program are reminded that it "does not offer programs in industrial, managerial, consumer psychology, or in counseling or educational psychology," only experimental and clinical tracks. This dissertation is the story of how this department, institution, and the city became the locus of activity for an internationally renowned program for research and innovation that remains to this day, the "city of neuroscience" (Tourisme Québec, 2017).

This project relied primarily on archival documents processed and oral history interviews conducted over the last half of the twentieth century. I visited the archives of McGill University and the Osler Library of the History of Medicine in Montreal in the mid-2010s. The account presented draws primarily from the materials I examined. I reviewed the Donald Olding Hebb papers and administrative material of the Department of Psychology within the archives of the

Faculty of Arts & Science. I also investigated department records related to McGill psychology from the Faculty of Graduate Studies and Research and the Faculty of Medicine, including the Wilder Penfield Archive and Digital Collections. I also drew upon oral history interviews from the Canadian Psychological Association collection held at the Library & Archives Canada in Ottawa and the John C. Liebeskind History of Pain Collection of the Louise M. Darling Biomedical Library at the University of California, Los Angeles. The archives at McGill were challenging to work with, records were difficult to navigate, and material was sparse and bureaucratic. The documents available offer limited representation with gaps and inconsistencies in the record. This approach was limited by what had been written, what I was able to find in the archives, and what was selected for preservation by past collectors and archivists at McGill and elsewhere.

The archival material provides details of the department, in order to understand the historical emergence of a new kind of interdisciplinary psychology (i.e., neuropsychology), we need to examine the lives of some of its creators, and the institutions and settings that brought these actors together. The primary method of this dissertation is a combination of group biography and institutional history. It examines how participants of McGill's psychological community developed in their separate scientific and intellectual milieux, and how they combined disciplinary approaches at McGill University. By tracing the interaction of these actos, we not only gain a deeper understanding of the historical forces that brought modern experimental psychology into existence in Canada, but we also gain a new perspective on the development of interdisciplinary science in general.

I take an explicitly transdisciplinary approach to the history of psychology in Canada. Most discussions of postwar behavioral science have been structured along disciplinary lines, oriented around intradisciplinary debates (Heyck, 2015). One reason for this disciplinary focus is that most histories of postwar behavioral science have been written by psychologists themselves, usually with the reform of their own discipline in mind. While this has led to some astute analyses of intellectual movements within disciplines, it has generally not led to studies that look at similarities across disciplines. An investigation of the many relations and interests of psychologists in Canada is necessary for a broad understanding of the development and intellectual trajectory of the department of psychology at McGill. Therefore, while this dissertation remains focused on the discipline of psychology, it explores shifting relations with

health, medicine, education, and industry through the departments shifting relations with mental hygiene practitioners, school teachers, medical students, psychiatrists, and the Canadian military. This dissertation seeks to understand how psychologists, such as Hebb, positioned themselves and the discipline of psychology within broader historical debates related to applied psychology, the role of psychology in medicine, and the future of Canadian psychology.

Three important concepts inform my historical investigations. First, it can be said that Canadian psychologists, as scientists, exhibit certain styles of scientific thinking. In my dissertation I make use of the concept of a 'psychological style' and refer to a McGill style of psychology. I use the term style broadly to indicate a relevant departure from the kinds of research priorities, institutional arrangements, and methodologies uniquely available to a group of psychologists at a particular time and place (e.g., postwar McGill). I mean to capture with this term the sense of multiple overlapping research programs cobbled together from the fields upon which they drew (i.e., neurology, physiology, and ethology). While drawing loosely from both Ludwik Fleck's concept of a 'thought collective' (1979) and Ian Hacking's 'styles of reasoning' (1992), McGill's 'psychological style' is represented mainly by an examination of research focus and disciplinary positioning. My understanding of psychological style, as a tool for thinking about local historical conditions, informs my account of a history of Canadian psychology.

Second, I find it useful to draw on the insights of sociologist Mark Granovetter about 'weak ties' (1973). Granovetter argued that communities that were strongly linked together were paradoxically less able to connect with other groups in order to diffuse their innovations. By contrast, communities with extensive weak ties were more able to interact with other groups, which both prevented them from becoming too insular and inward looking while simultaneously allowing their innovations to affect larger macrosocial developments (Granovetter, 1973). Psychology at McGill before and after World War II demonstrates a shift from a small, strongly linked community of applied psychologists into a carefully negotiated network of biologically-oriented psychologists with weak ties to Montreal's hospitals and neurological institutions (e.g., the Montreal Neurological Institute and the Allan Memorial Institute), as well as other scientific cultures (mainly European neurophysiology).

The third concept I've used to inform this research is historian of science Peter Galison's notion of "trading zones." By analyzing the historical circumstances that allowed for the trading of tools, techniques, and theories across the diversity of actors within 'big science,' Galison has

supplied a useful model for how to think about the creation of new scientific fields, and about how they relate to one another (Galison, 1997). Borrowing from the field of linguistic anthropology, Galison suggests that, by collaborating on projects in specific 'trading zones,' scientists from different disciplines can hammer out coordinated trading languages that allow for local cooperation, even when they disagree about the global meaning of their work. In time, these 'trading languages' can become robust enough to blossom into entirely new fields of inquiry (Collins, Evans, & Gorman, 2007). Galison's emphasis on the importance of material culture and locally coordinated activity has the advantage of making his enterprise thoroughly historical; the analysis of trading between different subcultures takes place under particular circumstances, and at a particular moment of time. Hebb's department of psychology in the 1950s represents such a trading zone and his neuropsychological theory, inculcated and advocated by his students and colleagues, facilitated exchanges across the disciplinary boundaries of psychology and physiology in the interest of a more "a hard-headed scientific discipline" (Hebb, 1976).

While this dissertation makes use of Galison's concept of a trading zone, the narrative approach to this study is in the form of group biography or institutional history. The goal is not to merely shift the history of Canadian psychology from a 'great man' approach to one that emphasizes 'great people' or a 'great institution.' By tracking the lives of the different actors that created the department and passed through its halls, we can gain an appreciation for how their scientific biographies brought them into a position to trade and develop a style in the first place.

My dissertation begins with the dissemination of academic and laboratory psychology to Canada from the United States and Great Britain (Scotland) at the turn of the century. The roots of Canadian mental philosophy and the creation of an independent department in the early 1920s is discussed in the context of debates on division from philosophy, scientific status, and utility. I will demonstrate how a relatively small group of academic psychologists, mainly in Toronto and Montreal, established their scientific legitimacy and social utility during the interwar period. This was done, I will argue, through early psychologists' involvement with the mental hygiene movement in Canada and the United States through the 1920s and early 1930s. I will demonstrate, through archival correspondence and meeting notes, the relationship between academic psychology and the medicine in Montreal was discordant, largely unproductive, and marred by interpersonal conflict in the first half of the twentieth century. The first chapter sets

the stage for understanding the roots of disciplinary struggles that recur throughout the events of this story. These include the tentative relationship between basic and applied researchers, funding for psychological research, and the dynamics of discipline-building. The response and positioning of psychologists and psychological expertise during periods of social change remains a theme throughout this dissertation.

Chapter Two examines the role of psychology and Canadian psychologists during World War II. The story of a fledgling psychology department at McGill now focuses on the many effects and implications of a world thrown into conflict and uncertainty. This chapter recounts postwar reconstruction efforts and attempts to rebuild the department. Here I introduce some of the most important historical actors for understanding the postwar psychology at McGill: these men are Robert B. MacLeod (1907-1972), Donald O. Hebb (1904-1985), and the lesser known Nelson W. Morton (n. d.). Together these three men laid the groundwork for experimental psychology and the discipline's vital connection to government. Their biographies contribute to a better understanding of the factors and conditions that shaped the kind of research that was possible at McGill's department in the following decade and beyond.

Psychology at McGill changed dramatically after World War II. The source of greatest change, without a doubt, was the appointment of Hebb as chairperson of the department in 1948. At that time he was in the final stages of publishing *The Organization of Behavior: A Neuropsychological Theory* (1949); in it he offered a conceptual and disciplinary vision for experimental psychology. This book and its author exerted a significant influence on the scope and direction of psychological research and practice, nowhere more so than at McGill University in the 1950s. Chapter Three delves into Hebb's background and intellectual influences discussed in the context of the broader theoretical commitments of his book (1949). Particular emphasis is placed on the periods of time Hebb spent working with psychobiologist Karl Lashley and neurosurgeon Wilder Penfield in the 1930s. Together Hebb and Penfield collaborated on a series of studies that documented the effects on personality and intelligence of frontal lobe operations. Hebb incorporated the results of his work with Penfield into a neuropsychological theory of learning and memory that formed one of the first challenges to the post-war dominance of behaviorism in American psychology. This chapter offers an analysis of major themes of Hebb's neuropsychological vision and its influence on postwar research.

Chapters Four describes and analyzes some of the major changes to academic psychology at McGill and in Canada during the immediate postwar period. As chairman of the largest Canadian experimental psychology department, Hebb had to contend with a number of pressing concerns that bore directly on the future of the discipline. These concerns related to availability of funding for research activities, rising tensions between academic and professional psychologists, and the lack of standardized training. While these concerns are not necessarily novel, this chapter describes the historical conditions that exacerbated them, forcing them to the surface with renewed urgency after the war. How Hebb responded to these challenges is telling, and I will argue, decisions made during this period affected the trajectory of academic psychology in Canada.

In the fifth and final chapter, I explore some specific examples of postwar psychological research at McGill. I use three examples of overlapping research programs that emerged at McGill during the "same spurt in the knowledge of behaviour" (Hebb, 1976) in the 1950s: Hebb's infamous studies into the effects of prolonged sensory deprivation (e.g., Hebb, 1961), the discovery of the reward centre in the brain by Peter Milner (1919-2018) and James Olds (1922-1976), and the early psychological studies of pain experiences by Ronald Melzack (1929-2019) and others. Through an historical analysis of these experiments, I argue they share more than a family resemblance to the kind of neuropsychological approach to experimentation that align closely with Hebb's (1949). The McGill style of psychology that emerged in the 1950s was qualitatively different from behaviorist trends becoming hegemonic in the United States at the time. The psychology that Hebb embodied was more holistic than reductionist, and more medical than philosophical. Concepts of "intervening variables" and "central processes" speak to his adjacency to the cybernetics moment. The dissertation ends with a consideration of the surprisingly lively and embodied approach to psychology Hebb encouraged among the postwar Canadian psychological community: a neuro-psychology of mind.

This dissertation, in part, is an account of how psychology in Canada changed in response to changing institutions and novel theoretical considerations It is a story of some of the driving forces shaping a discipline and sheds light on historical narratives regarding the early development of psychology in Canada and neuropsychology at McGill University.

Chapter One: Psychology at McGill in the Early Twentieth Century

Disciplinary psychology was established in different ways and at different times. Though few in number, histories of Canadian psychology exist, each telling part of a story which begins in the early twentieth century. This chapter describes the establishment of psychology as an independent academic discipline at McGill University. McGill has one of the oldest departments of psychology in Canada and the development of psychology in Canada has not received the same degree of analysis as it has in other parts of the world, for example, the United States and Europe (Brock, 2013). Prior to the publication of Mary Wright and C. Roger Myers' book (1982), the only published records of psychology's past in Canada were scattered throughout the pages of *Canadian Psychologist* and the newsletters of provincial psychological associations (Dzinas, 2000). Unfortunately, not much has changed. There is a tendency to lump the history of psychology in Canada with that of North America in general, without closely attending to local differences that shaped the discipline (Berry, 1974; Adair et al., 1996; Brock, 2013). While some departmental histories exist (e.g., Wright & Myers, 1982) they tend to be institutional hagiographies with minimal consideration to deeper analysis of the cultural, institutional, and interpersonal factors that shaped local practices, priorities, and outcomes.

An important part of the story of early psychology's struggle to establish its disciplinary identity was its relationship to other disciplines, such as medicine and public health (Capshew, 1999; Burnham, 1988; Pickren, 1995). In Canada, this relationship, in large part, is shaped by the organizational efforts of the Canadian mental hygiene movement. Mental hygienists and early psychologists become allies in the struggle for recognition, expertise, and disciplinary authority. Outside histories of the University of Toronto (e.g., Pols, 1999; 2002), the role of the mental hygiene movement in the development of academic and professional psychology in Canada remains relatively unexamined (Gleason, 1999; Richardson, 1989). This chapter investigates some of the ways this movement has influenced, and was influenced by, early psychologists at McGill University.

This chapter is divided chronologically around three major periods of development in the histories of both academic psychology at McGill University and the Canadian National Committee for Mental Hygiene in Montreal. It links the co-development of these institutions and the ways they mutually reinforced one another throughout their development. The first section

concerns the late nineteenth century and first decade of the new century. This is the period in which both psychological topics are first introduced into the Canadian university curriculum and when the mental hygiene movement emerged in the United States. The second section concerns continues to the mid-1920s.This was a period of expansion in academic psychology at McGill: the establishment of a psychological laboratory in 1910 and culminated in the establishment of a separate Department of Psychology in 1924. During this same period, foundations for a Canadian National Committee for Mental Hygiene were established in Montreal and was officially formed at the end of World War I. The third section details the period of organizational growth which followed upon the expansion of mental hygiene activities in Montreal in the 1920s. The chapter concludes with the consideration of factors associated with fragmentation of disciplinary psychology and the decline of support from the National Committee by the late 1930s. I close the chapter with analysis of some of the consequences of this period of modest disciplinary growth for the collapse and subsequent rejuvenation of the department in the post-World War II period.

1. Early Development of Academic Psychology at McGill

The roots of academic psychology can be found in the direction of philosophical thinking at McGill University at the turn of the century. The Department of Philosophy at McGill, like the university itself, was heavily influenced by various long-standing Scottish schools of thought (Frost, 1984; Wright & Myers, 1982). Scottish Realist and Empirical thought was particularly influential in the department; the philosophy of Thomas Reid (1710-1796), Dugald Stewart (1753-1828), William Hamilton (1788-1856), and Alexander Bain (1818-1903), in particular, held sway. The influence of these philosophers on teaching at McGill can be traced to the historical founding of McGill upon the nineteenth-century Scottish university system. The ideas of these philosophical traditions tended to reach North American through nineteenth century German experimentalist thought.

Long before the establishment of a department of psychology at McGill, courses on topics we would recognize today as ‘psychological’ -- such as instincts, consciousness, and perception -- were offered by the Department of Philosophy. McGill was among the first to teach university courses on psychological topics (Frost, 1984). Philosophy departments across Canada followed McGill in offering these kinds of courses (Wright & Myers, 1982). Psychological

topics were taught in courses on Moral Philosophy and Mental Science (sometimes referred to as 'intellectual' or 'faculty' philosophy) which the Department of Philosophy began offering shortly after the Faculty of Arts and Science was established in 1843. By 1850, William Turnbull Leach (1805-1886), an Anglican Archdeacon from Edinburgh who had joined McGill as both Professor of Classical Literature and Vice Principal in 1846, began teaching mental science (Macmillan, 1921). An early course description read as follows:

> Mental science: Mental phenomena, different classifications of mental phenomena, unity of the human mind, volition, consciousness, the senses and sensation, perception, understanding, reason, instincts, passions, affections, moral sentiments, reflect sentiments, sentiments of beauty, sublimity, and religion (as quoted by Ferguson, 1982, p. 35).

In 1871, two new courses with 'Psychology' explicitly in their titles were scheduled to be offered by a newly hired Assistant Professor, George Forbes of Edinburgh (McGill College, 1871). These new Psychology courses would come to replace those previously offered under Mental Science over the next decade (Ferguson, 1982).

1. a. Moral and Mental Philosophy and Early Psychology at McGill

Psychology developed as its own distinct discipline much earlier at McGill than at other universities in Canada (Wright & Myers, 1982). My research suggests the support of professors in the domain of mental philosophy, more so than in the United States, was important to the establishment of early psychology in Canada. Prominent Canadian mental philosophers were important for bridging the divide between theological scholarship and scientific debates (i.e., "old" and "new" psychology; Rodkey, 2011). McGill philosophers explicitly incorporated recent psychological research and empirical thought into their lectures and into the curricula of the philosophy department in the late nineteenth and early twentieth centuries. Without such support from prominent philosophers it is unlikely academic psychology would have developed as quickly and would likely not have taken the same experimental focus (i.e., the establishment of the psychological laboratory in 1910). In this section, I illustrate how early psychology benefited from this support during a critical moment in the history of psychology at McGill.

Two professors of mental and moral philosophy were particularly influential in laying the groundwork for the establishment of academic psychology at McGill: John C. Murray (1836-1917) and his predecessor William Caldwell (1863-1942). Both men studied philosophy and

theology in Scotland and later in Germany and were highly receptive to the new psychology emerging from the German experimentalist tradition.

John Murray came to McGill from Queen's College in 1872 when he was offered the appointment of John Frothingham Professor of Mental and Moral Philosophy. Murray's philosophical psychology was initially grounded in the work of William Hamilton and Scottish Common Sense philosophy but gradually shifted towards more diverse thinkers and schools (Terpstra, 1983). Murray taught the course on 'Elementary Psychology' newly offered to second year philosophy students and was familiar with the work of Wilhelm Wundt (1832-1920). Although he had received no formal experimental training, Murray was intrigued by the new laboratory studies emanating from Germany and the United States and chose to include them in his lectures (Ferguson, 1982).

Murray supported the "New Psychology" of Wilhelm Wundt and the adoption of physiological laboratory methods to subject matter of mental philosophy. In 1885, a little more than a decade after his arrival at McGill, he authored a textbook, *A Handbook of Psychology* (Murray, 1885) which makes extensive reference to Wundt's *Grundzüge de physiologischen Psychologie* (1874). *A Handbook of Psychology* was one of the first books on psychology published by a professor at a Canadian university (Ferguson, 1982), and was preceded only by British psychologist James Sully's *Outline of Psychology* (1884). Other texts of the period were those of prominent American psychologists: such as John Dewey's *Psychology* (1886), George Ladd's *Elements of Physiological Psychology* (1887) and William James' *Principles of Psychology* (1890). Murray's textbook was highly successful. After being published in London and later in the United States, a sixth edition with a new title *An Introduction to Psychology* was published in 1904. Support for psychology and 'psychological thinking' at McGill was strong.

As a proponent of the "New Psychology" Murray held that an important co-extensive relationship between the methods and subject matter of psychology and physiology was paramount. In his *Handbook* he notes that, "among the auxiliary studies, to which the psychologist resorts, the highest value must be attached to human anatomy and physiology, in so far as these explain the structure and functions of the different parts of the nervous system" (Murray, 1885, p. 11). According to Murray, therefore, the relevance of psychology to medicine was self-evident (Murray, 1872; 1892). In an address to members of the Medical Society of McGill College in 1892, Murray urged his audience to consider the potential of this relationship

for empirical study. While being mindful not to overgeneralize (and to distance himself from the discredited practice of phrenology), Murray emphasized the utility value of the New Psychology for clinical practice: “if disease may be produced by mental causes, may not the same causes be employed for curative purposes?” (Murray, 1892, p. 894). Since the relationship between mind and body could theoretically be approached from either direction (e.g., mind or body), Murray argued there was an inherent continuity between the domains of academic psychology and clinical practice in medicine. Unbeknownst to Murray, the status of psychology, as a basic and applied science, would continue to play out and define the character of psychology at McGill over the next hundred years.

In the early decades of the twentieth century the role of psychologists in relation to psychiatry and medicine was already being worked out in the United States (e.g., Pickren, 1995). In Canada, given the far fewer total number of psychologists, the development of this relationship emerged over a longer period of time. With the exception of Murray's writings, there are few published accounts prior to the 1930s. Murray's lecture (1892) illustrates the favourable reception of early psychology, its image as an experimental science, and its proposed relevance to the medical community by mental philosophers at McGill. Murray may have been ahead of his time. Lectures for medical students on psychological topics were not introduced into the curriculum for more than a decade later.

By the early twentieth century, the Department of Philosophy at McGill had expanded to include courses on metaphysics, logic, and moral philosophy (which covered ethics and social philosophy). When Murray retired in 1903, Principal William Peterson (1856-1921) chose William Caldwell (1863-1942) to succeed Murray as Professor of Mental and Moral Philosophy. Caldwell, like Murray, had been born in Edinburgh and both graduated and taught philosophy at the University of Edinburgh. In 1891, Caldwell emigrated to the United States to teach logic and metaphysics at Cornell University and later the University of Chicago. Caldwell shared Murray's support for European and American experimental psychology. He recognized psychology as an expanding discipline, was an early member of the American Psychological Association (APA), and worked throughout his career to promote the interests of the New Psychology until his retirement in 1929 (Ferguson, 1982).

As chair of Moral and Mental Philosophy at McGill, Caldwell supported early psychology by expanding the philosophical curriculum to include more courses on psychological

subjects and by bringing in money for the support of experimental psychology. For example, the year after his arrival at McGill, Caldwell and Principal Peterson managed to persuade Sir William MacDonald (1831-1917) to invest in the development of a fund for experimental psychological research. This fund persisted in various forms into the 1980s and proved of much assistance to the Chairman of the Department of Psychology (Frost, 1984; Ferguson, 1982). This cooperation between mental philosophers and early psychologists is notable, rarely was this relationship harmonious (Fuchs, 2000; Rodkey, 2011). Yet, Caldwell had received a fairly traditional nineteenth century philosophical education and was disconnected from broader debates. Robert B. MacLeod (1907-1972), a student and later chairperson of the department, would later publicly allege that Caldwell "charged like a lion into structuralist-functionalist controversy" (MacLeod, 1955, p. 13) to the detriment of the early discipline.

Following the arrival of Caldwell in 1904, much of the teaching of psychology-related subjects in the Department of Philosophy was done by Joseph W. A. Hickson (1872-1957). Unlike Caldwell, Hickson had been trained in the German experimental tradition. He was born in Montreal, graduated from McGill in 1893 with the gold medal in philosophy, and completed a Ph.D in 1900 at the University of Halle in Germany (Frost, 1984, 29; Hall, 1957). Hickson likely worked under Alois Riehl (1844-1924), who had preceded German psychologist Herman Ebbinghaus at Halle in the late 1890s (Ferguson, 1982). Hickson assisted Murray with the teaching of psychology in the Department of Philosophy, first as tutor in 1901 and, after Caldwell's arrival in 1903, he was promoted to Assistant Professor of Psychology and Lecturer in Philosophy. While Hickson was the first to be officially designated professor of psychology at McGill (and possibly all of Canada), he is perhaps better known for his accomplishments in mountaineering which he pursued after his brief career in psychology (Hall, 1957).

Following the Scottish model, the Faculty of Arts offered honours programs for advanced undergraduate study (Frost, 1984; Ferguson, 1982). In 1905 the annual calendar announced that "graduate and others" were invited to specialize in psychology and was listed among departments that offered graduate degrees. The course offerings available to students interested in psychology at McGill in 1904 included a course on 'Introductory Psychology,' 'Introduction to Psychophysics,' and 'General Course in Psychology,' and 'Analytic and Experimental Philosophy' for fourth year students. These developments signaled two important changes to the Department of Philosophy: 1) that by 1904 institutional support and demand for the teaching of

psychology at McGill had growth and been formalized (in courses and faculty selection) such that a program could be offered, and 2) that the administration deemed investment in this nascent discipline was prudent for the standing of the university (e.g., ability to attract students). The first M.A. in psychology was granted in 1907 to Edith M. East (n. d.) on the topic of 'the subconscious' under William Caldwell (East, 1907). This was much earlier than most universities in Canada, many of whom didn't offer advanced degrees of psychology until after World War II (Wright, 1969) with some exceptions (such as Toronto in 1926, Western in 1932, and British Columbia in 1936; Wright & Myers, 1982).

Throughout the 1900s psychology at McGill, under the auspices of mental philosophy, continued to grow. In 1908, two courses, one called 'An Introduction to Experimental Psychology' and another called 'Problems of Comparative Psychology,' which included the first lectures on child psychology (Ferguson, 1982). The inclusion of these courses, taught primarily by Caldwell and Hickson, created space and demand for psychological topics in the philosophy curriculum. The inclusion of recent findings of experimental studies into textbooks, lectures, and their own philosophies, mental philosophers at McGill supported early psychology (Terpstra, 1983). Early experimental psychologists found this department welcoming to empirical methods and contributions from the natural sciences. While there is a long tradition within psychology of disparaging "old psychology" (associated with mental science) and emphasizing its discontinuity with "new psychology" (experimental) as a rhetorical strategy (see Rodkey, 2011), historical evidence suggests that at McGill, the two developed more or less harmoniously into the first decade of the twentieth century.

Experimental psychology was more-or-less compatible with the mental and moral philosophy being taught at McGill in the late nineteenth century. The Scottish influence upon McGill as well as its status as a private institution supported by the anglophone economic elite of Montreal ensured that it was shaped by a strong utilitarian ethos since at least the 1850s (Shore, 1987, p.xiii). This ethos contributed to the early positive reception of empirical and experimental studies as part of the philosophy curriculum at McGill.

In the first decade of the twentieth century, the number of students interested in the burgeoning psychology curriculum, as well as developments in philosophy, justified the appointment of a second full-time faculty member. As the establishment of experimental approaches and laboratories flourished in North America in the late nineteenth and early

twentieth centuries (Garvey, 1929; Benjamin, 2000), the need for faculty specifically trained in these new methods became essential for the department to remain relevant and abreast of international trends. Since the program had first started being offered in 1905 only a single graduate degree had been awarded. This was not due to lack of interest. The Department of Philosophy needed staff trained in the new experimental methods of psychology; therefore, in the spring of 1909, a decision was reached to add an experimentally-trained psychologist to the staff. Upon Edwin Holt's recommendation, Principal William Peterson offered the position of sessional Lecturer in Experimental Psychology to William Dunlop Tait (1879-1944). Tait would come to have the greatest influence on the emergence and formation of academic psychology at McGill in the first half of the twentieth century.

1. b. William Tait and the Psychological Laboratory at McGill, 1909-1923

The period between 1909 and 1923 was pivotal to the development of psychology at McGill. During this period, academic psychology developed from a few mental philosophers who chose to promote the New Psychology in their classrooms and in public lectures to the expansion of courses dedicated to these topics, the establishment of a Psychological Laboratory in 1910, and eventually to the creation of a separate Department of Psychology in 1924. Throughout this entire period and into the early 1940s, no figure influenced the direction of psychology at McGill more strongly than William Tait.

Tait was born in Hopewell, Nova Scotia. In 1902, he graduated from Dalhousie University with high honors in philosophy and subsequently went on to Harvard to work with Hugo Münsterberg where he completed both his Master's and Ph.D. degrees. Münsterberg had been trained by Wundt, among the first to establish psychology as a scientific discipline (Danziger, 1990). In the early 1900s Tait explored psychophysical attitudes and memory at Harvard with Münsterberg and published an article on this work (Tait, 1913). As one of the few Canadians with ties to the original psychological laboratories of Germany, Tait represented a valuable addition to a philosophy department that wanted to maintain its prestige as a bastion of leading European thought. Interestingly, when Caldwell suggested to Principal Peterson that he reach out to Münsterberg, he initially recommended William McDougall (1871-1938) but later changed his mind in favour of Tait (Ferguson, 1982).

Tait's appointment as official Lecturer in Psychology was an important step in the development of the discipline at McGill. Historians Stanley Frost and George Ferguson said of

this appointment that it "was largely to determine the character of the future Department of Psychology" (Frost, 1984, p. 28) and that it "shaped the image and substance of psychology at McGill for one third of a century" (Ferguson, 1982, p. 41). These perspectives, more than anything, are likely based on Tait having established the first psychological laboratory at McGill, a move widely considered among traditional historians of early psychology to be pivotal in moving away from the "old" mental philosophy and towards the "new" kind of experimental practices we associate with scientific psychology today (Fuchs, 2000; Rodkey, 2011). Indeed, much historiography of the early discipline has identified the introduction of the laboratory as the marker of the scientific origins of psychology (Capshew, 1992).

Tait established the first psychological laboratory at McGill in 1910, one year after his arrival. McGill's laboratory was the second in Canada, established after James Mark Baldwin's laboratory at the University of Toronto in 1890, which was among the first in North America. Baldwin and Tait's were the only experimental laboratories in Canada prior to the 1930s (Garvey, 1929). The McGill university calendar for 1910 included the following description of the new laboratory:

> "The Psychological Laboratory occupies rooms in the Arts Building. In the main library are found the chief periodicals and works of reference on all branches of the science. Besides this, there has been added during the past year a considerable amount of apparatus so that the laboratory is now equipped for original research work in experimental psychology, physiological psychology, and applied psychology. The same equipment also serves to train students in the methods of experimental psychology, and furnishes material for demonstrations and lectures" (as quoted in Ferguson, 1982, p. 41).

In later expanded descriptions the laboratory is said to occupy 'some eighteen rooms' in the Arts Building. In 1912 Tait was promoted to Assistant Professor of Psychology and was given the title Director of the Psychological Laboratories which he held for the next thirty-two years at McGill. He later recalled, "I came to McGill in the year 1909 as a Sessional Lecturer on a salary of $1000 and initiated the experimental aspect of Psychology. The effort appears to have been appreciated by the students for within two years it became necessary to double the size of the classroom. The classes in Psychology, although optional, are among the largest in the Faculty of Arts" (Tait, Dec 14, 1920). Indeed, by 1913 Tait and Hickson were teaching six courses in psychology in the Department of Philosophy: Elementary Psychology, Experimental Psychology, Advanced Psychology, Social Psychology, Advanced Experimental Psychology Seminar, and the Psychological Laboratory.

In the 1910s New Psychologists were under considerable pressure to legitimize their discipline as a science (Leary, 1987; Pickren, 2000) and the rejection of the "Old Psychology" allowed these psychologists to define themselves against their antiscience predecessors and in doing so establish scientific credentials and loyalties (Rodkey, 2011). While historian Stanley Frost has asserted that psychology "took on a new direction" (1984, p. 147), my interpretation suggests continuity of thought between mental philosophers and early experimental psychology, rather than sudden departure. Caldwell used his position to help the development of a psychology with "a strong experimental emphasis" (Ferguson, 1982, p. 42). Although tensions would rise within the discipline, my study suggests that no explicit move was made to distance themselves from mental philosophy, as practiced at McGill in the 1910s. This was certainly not universally the case, as the history of American psychology indicates (e.g., see Rodkey, 2011 and Fuchs, 2000). Perhaps both the mental philosophers and early psychologists at McGill had more in common (and more at stake) than at other universities experiencing similar challenges and tensions. In the case of both Tait and Hickson, it is worth noting, it was their disciplinary training and aptitude in philosophy that had initially brought them in contact with the German experimentalist traditions in North America; only later in their academic careers did they adopt the methods and theoretical frameworks of the New Psychologists.

1. c. The Great War and its Aftermath, 1914-1924

Given the relatively few psychologists in Canada in the late 1910s, their role in the war effort, as psychologists, was minimal compared to those in the United States (e.g., the work of Robert Yerkes and Lewis Terman in the development and administration of the Army's Alpha and Beta Intelligence Tests). Academic life at McGill was seriously disrupted by World War I (Ferguson, 1982). Not a single graduate degree was awarded in psychology at McGill from 1913 to 1925 (McGill, 2018).

The McGill Contingent of the Canadian Officers Training Corps had already been established (1912) when Canada joined the war in August 1914. This assured that the university would play an important role in the national war effort (including the establishment of the first military neurological hospital overseas). Many students and alumni enlisted in the first wave of patriotic fervour that swept the nation in 1914, but by the spring of 1915 — after the first wave of heavy Canadian casualties at Ypres — the Canadian regiment was faced with a desperate shortage of troops. A number of units were organized for overseas service: these included the

McGill General Hospital (made up of McGill graduates as medical staff and nurses from the Royal Victoria and Montreal General Hospitals), two McGill Siege Batteries, the No.7, under Major William D. Tait and the No.10 under Sir Stopford L. Brunton, six companies of Infantry for the Princess Patricia Battalion and several groups of reinforcements. In total, 3,059 McGill men served in the Armed Forces and 363 gave their lives. Many women also served in medical and auxiliary services (McNally, 2007).

The No.7 McGill Siege Battery (later known as "the Majillses") was recruited at McGill with a strength of 150 men. Tait, then 36 years of age, became commanding officer, and Cyrus Macmillan, Associate Professor of English, was second-in-command (Smol, 2017). This unit proceeded to England in the winter of 1915-1916 and was at once detached from the Canadian Forces and placed under Imperial command. After their training had completed in England, they were sent to France as units of the Royal Garrison Artillery and attached to the Canadian Corps Heavy Artillery. The Battery served with distinction at Lens, Hill 70, Passchendale, Arras, Valenciennes and Mons, some of the most severe battles of the war (Frost, 1984).

Tait relinquished his command in 1918 and both he and Macmillan resumed teaching at McGill. Tait's experiences during the war seem to have diminished his interest in theoretical and experimental psychology; anecdotal evidence suggests his wartime experiences had seriously affected his outlook and personality (Ferguson, 1982). Indeed, MacMillan's diary describes a possible mental breakdown Tait experienced at Vimy in early April 1917 (Smol, 2017). In a letter, Caldwell notes how upon returning to McGill Tait had become, "a very difficult man to get along with, brusque, headstrong, domineering, unreasoning ready to take offence…" (Caldwell, ca. Jun, 1921). It is unknown whether Tait maintained a relationship with Macmillan after their time together during the war; Caldwell remarked that "he [Tait] and his intimate friend Macmillan had a bitter quarrel when returning to McGill" (Caldwell, ca. Jun, 1921). Macmillan was promoted to the Dean of the Faculty of Arts and Science in 1940.

Tait resumed his appointment at McGill as assistant professor of psychology in the department of philosophy after the war. Given his postwar activities both in the department and outside (Tait, Dec 14, 1920), it would be fair to say the newfound enthusiasm for applied psychological expertise (Capshew, 1999) existed in Canada as well. An important development that has not been adequately considered in the historical literature was the role of the Canadian

mental hygiene movement in the shaping of psychology at McGill and in Montreal during this period.

2. The Mental Hygiene Movement in Canada and at McGill

To understand the development of psychology in Canada it is important to consider the role of the mental hygiene movement. While few histories of Canadian psychology have been written, fewer still have examined the critical role of mental hygiene to its early development. This section examines the adoption of the mental hygiene movement from the United States to Canada in the late 1910s and describes the early activities of the Canadian National Committee for Mental Hygiene in Montreal. It emphasizes how Montreal and McGill University were perceived by the National Committee leadership as key sites for the development of interdisciplinary partnerships that would contribute to the strengthening of the movement in Canada. This understanding was based, I argue, on the success of the collaborations between psychologists and psychiatrists that developed at the University of Toronto in the 1920s (Pols, 1999; 2002). Similar partnerships were attempted at McGill in the 1920s. For Tait, alignment with the Canadian National Committee bolstered his claim that psychology deserved its own department (like that at Harvard) and he used the popular support and philanthropic backing of this committee to help fashion a disciplinary identity separate from philosophy in the early 1920s.

From the 1890s to the 1920s, the Progressive Era was a period of widespread social activism and political reform across the United States and Canada. One area of considerable reform was the management and treatment of the mentally ill. Perhaps the most influential movement related to this aim in North America was organized around the concept of 'mental hygiene.' Broadly, the notion of mental hygiene refers to the application of what was learned during the public hygiene reforms of the late nineteenth century (especially around tuberculosis and syphilis) to mental and social problems in the early twentieth century. The mental hygiene movement first gained momentum in the United States in the 1910s and later in Canada. The role of psychologists in this movement expanded dramatically after World War I (Pols, 1999). While reformist movements related to the management of the mentally ill became prominent in various parts of the world, mental hygiene as a social movement was most prominent in North America and England, particularly the United States.

Since the turn of the century the 'feebleminded' had become a consuming concern for public health advocates interested in eugenics, immigration policy, and race betterment (MacDougall, 1990; McConnachie, 1987; McLaren, 1990; Simmons, 1982; Sutherland, 1976). The concept of feeble-minded was used from the late nineteenth century to refer to illnesses or deficiencies of the mind which included all degrees of educational and social deficiency as well as criminality, promiscuity, and alcoholism. Within the working understanding of the time there existed a hierarchy, ranging from idiocy, at the most severe end of the scale, to imbecility, at the median point, and to feeble-mindedness at the highest end of functioning. The latter was conceived of as a form of high-grade mental deficiency; which was perceived as particularly pernicious due to its potential for concealment within society. A blend of concerns about prostitution, illegitimacy, and the threat of race suicide, combined with the fear of being flooded with Europe's 'inferior stock' set the backdrop for the movement and the menace of the feebleminded became the rallying cry of Canada's women's and public health movements (Pols, 1999; McLaren, 1990).

The roots of the American mental hygiene movement are attributed to the reform efforts of Clifford W. Beers (1876-1943). In 1903, Beers is said to have suffered a manic-depressive psychosis and was committed to various Connecticut hospitals over the next three years. He wrote a wildly popular book based on these experiences, *A Mind That Found Itself* (1908). This autobiographical account of hospitalization and barbaric treatment went through multiple editions, serving as a cultural catalyst and sparking broad mental health reform in the United States under the banner of the mental hygiene movement (Beers, 1908).

Beers' book attracted the attention of prominent physicians, educators and philanthropists. In 1908 Beers founded the Connecticut Society for Mental Hygiene. In 1909 Beers and a group of socially-minded psychiatrists, such as Adolf Meyer (1866-1950), came together to found the 'National Committee for Mental Hygiene' (now called Mental Health America). The main purpose of the National Committee was the development of 'extra-mural' psychiatry, or the application of psychiatry in service to social problems (Dain, 1980; Richardson, 1989; Shea, 1980). The Committee drew its inspiration from the public health movements in England and the United States, which were guided by the ideal of prevention. The Committee consisted of prominent members of the medical, academic and business communities

and through this organization the activities of the mental hygiene movement in the United States were organized and upon which the later Canadian National Committee was based.

After the war, faith in science was strong and hope was held out that the social sciences could serve the interests of the state, help curb humanity's capacity for self-destruction, and actively shape individuals to better cope with the demands of an increasingly technologically-driven world. The human sciences, which included sociology, anthropology, and the psy-disciplines (McAvoy, 2014), provided new means for the management of "calculable minds" (Rose, 1988) and created new roles for psychologists throughout the interwar period (see Capshew, 1999).

In the area of health, the end of the war presented a number of challenges for which mental hygienists suggested they were uniquely qualified and so aided Canada's postwar reconstruction efforts (MacLennan, 1987; Dowbiggin, 1995). For example, returning soldiers in need of psychiatric care represented a real risk for the possibility of overburdening the few institutions with staff trained in psychiatric care. Concerns emanating from the United States, emphasized links between the mental health of Canadian society and its national economic productivity in the early decades of the twentieth century (e.g., Hincks, 1918). This impetus for social scientific expertise combined with the longstanding concerns of "feeblemindedness" in Canadian society generated sufficient momentum for the creation of its own Canadian National Committee after World War I. With assistance from Beers, two Toronto psychiatrists, Charles K. Clarke (1857-1924) and Clarence M. Hincks (1885-1964), established the Canadian National Committee for Mental Hygiene at a meeting in Ottawa in the spring of 1918. Both men would contribute importantly to the direction of the Committee and its role, as one of the largest funders of early twentieth century social research in Canada, in shaping academic psychology and psychiatry at the University of Toronto and McGill University during the interwar period (Dowbiggin, 1995; MacLennan, 1987).

Hincks was born in St. Mary's, Ontario, in 1885 and was the nephew of T. Wesley Mills, the McGill physiologist (Roland, 1990; Shore, 1987). A pioneer in the field of mental hygiene, Hincks introduced the newly developed Binet-Simon intelligence tests to Ontario schools and was appointed public school medical inspector for the Toronto Department of Health from 1912-1917 (Griffin, 1989). Hincks investigated problem children referred to him by teachers and, in

addition to a myriad of health problems, he found feeblemindedness to be the main reason for school-related problems (Roland, 1990).

In 1915, C. K. Clarke -- Canada's most influential psychiatrist and Dean of the Toronto Medical School -- offered Hincks a position at the recently established Psychiatric Clinic of the medical school, the first of its kind in the country (Pols, 1999). In 1916, Clarke became the Chair of the Psychological Laboratory at the University of Toronto. He brought to early academic psychology the clinical tradition in which he had been trained. He invited Edward "Ned" Bott (1887-1974) and his colleagues in psychology to collaborate with him at the Psychiatric Clinic. Bott and the other psychologists at the University of Toronto welcomed the association with clinical practice as a way of asserting their separation from philosophy (Pols, 1999), as Tait would do when finally given the opportunity after the war.

Through Clarke and Hincks, psychologists at the University of Toronto became acquainted with relevant mental tests and started to experiment with them in the context of the clinic (Pols, 1999, 2002). A steady stream of 'juvenile delinquents' and 'problem children' were referred to the clinic for mental examinations. For Hincks and Clarke, the mental test offered an enticing new technological device to measure the mental level both reliably and scientifically. After all, psychologists involved with intelligence testing of soldiers had demonstrated the value of these instruments; mental measurements were used to buttress existing rhetoric around the menace of the feebleminded with scientifically derived statistics. The findings of the clinic substantiated the assumed connection between feeblemindedness and other social problems (e.g., Clarke, 1918). The clinic was quickly transformed into an extensive testing unit for problem children (Conboy, 1916) and uses for mental tests quickly expanded from individual delinquents to whole school populations (Pols, 1999).

The war had also provided new ways for Toronto psychologists and psychiatrists to cooperate. In 1916, Clarke, Hincks, and Bott worked on developing an experimental rehabilitation centre for wounded and disabled soldiers (see Pols, 1999). It was from the experiences of these fruitful early interdisciplinary collaborations at Toronto (1914-1917), and the prospect of creating similar groups in other major cities in Canada during the postwar reconstruction effort, that Hincks and Clarke proposed to develop a Canadian National Committee. This is how the mental hygiene movement came to Montreal.

Hincks was the driving force of the movement in Canada. When Hincks went about establishing the National Committee he approached prominent members of the medical community not at Toronto, where he was based, but rather at McGill (Roland, 1990; McLaren, 1990). It was early enthusiasm for the vision of the Committee as presented to this McGill group -- which included neurologist Colin K. Russel (1877-1956), Dean of Medicine Charles F. Martin (1868-1953), and Principal Sir William Peterson (1856-1921) -- which provided the critical support for its formation (Roland, 1990). Indeed, convinced of the importance of setting up their own Canadian National Committee, Peterson supplied Hincks with "a list of 20 distinguished Canadians suitable for the Board of Directors" and all but two agreed to become members of the Executive Committee (Roland, 1990, p.43); these members (strongly tied to its founders) would form the basis of necessary support from the English-speaking Montreal business and university communities.

On April 26, 1918 in Ottawa, an organizational meeting was held for the development of a Canadian National Committee for Mental Hygiene (Hincks, Aug 21, 1928). The Canadian committee would be organized after the example of, and in close cooperation with, the US National Committee (Hutchison, 1973). C. K. Clarke was the first Medical Director of the Canadian Committee. Both Sir Robert Falconer (1867-1943), President of the University of Toronto, and Sir William Peterson, Principal of McGill University, were made Vice-Presidents of the Committee. Hincks became the Associate Medical Director and Secretary of the Committee (Hutchison, 1977). The Canadian and American National Committees worked so closely that in later years Hincks became Medical Director of both Committees simultaneously.

The early years of the Canadian National Committee was a period of development and laying of foundations (Hutchison, 1973); the Committee hoped to establish their utility (through surveys), to create a network of allies (by recruiting experts), and to address issues with popular support. Whereas the U.S. National Committee was founded around a program directed toward the improvement of the care of the mentally ill, the Canadian Committee was primarily oriented toward the perceived menace of the feebleminded to society (Pols, 1999). The Committee began by seeking changes to existing immigration laws, "to prevent the entry into Canada of criminals, prostitutes, idiots, the insane, imbeciles and the feeble minded" (Hutchison, 1973, p.10) and developed mental tests to be administered to immigrants.

It next turned towards the development and conduct of surveys of hospitals and schools, developed psychiatric training courses for health care workers, and began to engage in campaigns to educate the public about the need for reforms and about the important role that mental hygiene was to play in Canadian society. There were also efforts to educate various publics through the circulation of press articles and public lectures, including a Mental Hygiene Exhibit in 1923 (Hutchison, 1973).

Along with Toronto, Montreal was necessarily the most important site for the mental hygiene movement and Hincks would even later come to describe it as "one of our chief, if not our chief, citadel in Canada" (Hincks, Feb 1, 1928). Many of the National Committee's most influential supporters resided in Montreal. For example, the first vice-presidents of the Canadian Committee, influential business elites Lord Shaughnessy (head of the Canadian Pacific Railway), Sir Vincent Meredith (President of the Bank of Montreal), and Sir Lomer Gouin (Premier of Québec, 1904-1920). Directors of the Committee included newspaper publishers Hugh Graham (Lord Atholstan; publisher of the Montreal Star), Richard S. White (senator and publisher of the Montreal Gazette), and eminent Montreal-based medical men such as H. G. Birkett and Colin K. Russel (Hutchison, 1977). Moreover, as the largest urban centre in Canada, Montreal faced many of the social issues proponents of the mental hygiene movement purported to address, including, immigration, unemployment, and problems related to schooling (such as delinquency and truancy, issues believed to be caused by maladjustment). Mental hygiene became closely tied with eugenic ideals in the early twentieth century, and McGill, in particular, was home to a number of prominent defenders of eugenics (McLaren, 1990), including prominent and outspoken professors such as geneticist Carrie Derick (1862-1941), pathologist John Adami (1862-1926), and W. B. Tait (e.g., Tait, 1914) (MacKinnon, 2016).

Organizationally, the most important connection the Canadian Committee had in Montreal was through its first president, Charles F. Martin (1868-1953). Martin had studied at McGill and received his B.A. in 1888 and his M.D. in 1892. He held many positions in the Faculty of Medicine and served as Dean of Medicine at McGill from 1923 to 1936 and Governor of the university from 1935-1938. Through Martin's close relationship with Hincks, he would oversee all activities of the Canadian National Committee in Montreal in his position as President of the National Committee and later as Medical Director of the organization (Roland, 1990).

Funding for mental hygiene in Montreal was largely obtained through grants provided by the Rockefeller Foundation, a major American philanthropy. The Foundation funded these activities with the help of the Canadian National Committee as intermediary. Through this arrangement, Martin and Hincks oversaw the allocation of funds granted by the Rockefeller Foundation for mental hygiene work in Montreal throughout the interwar period. One of Hinck's early goals for the National Committee was the strengthening of its Montreal base at McGill (Shea, 1980). Since establishing scientific legitimacy was an important early objective, close ties with academic and medical institutions was identified as key to the movement's success (Pols, 1999). The appointment of two university heads as Vice-Presidents suggests a high degree of cooperation between universities, their affiliated teaching hospitals, and the Canadian National Committee (Hutchison, 1973). This early partnership was unique to the movement in Canada and contributed to its early success (see Pols, 1999).

The divergence of psychology from philosophy in Canada was directly connected to the work of mental hygienists (Pols, 1999; MacLennan, 1987; McLaren, 1990) and when the Canadian National Committee for Mental Hygiene was founded in 1918, it consolidated and solidified the arrangements that had brought psychiatrists and psychologists together during the previous decade at the University of Toronto:Clarke became the first Medical Director, and Bott, the head of the Research Division. The Canadian National Committee was organized to strengthen these sorts of ties and to open up new sources of funding for research and teaching. The Toronto psychologists (under the direction of E. A. Bott and W. E. Blatz) influenced mental hygiene by expanding its interest from the pathological to the abnormal (Pols, 1999, p. 123; Richardson, 1989, p.112). This would mitigate some of the early 'turf battles' between psychologists and psychiatrists that had emerged in the United States (for example, Pickren, 1995).

While the collaborative work of psychologists and mental hygienists in Toronto schools has been investigated (e.g., Pols, 2002; Dowbiggin, 1995), the situation in Montreal and at McGill has remained relatively unexamined. In the next section I will examine some early collaborations with McGill University, discuss how they differed from those at the University of Toronto, and suggest how these relationships shaped the kind of psychology that emerged prior to the establishment of the first department of psychology in Canada.

2. a. Early Collaborations Between Mental Hygiene and Psychology, 1918-1923

While psychology at McGill was relatively slow to develop compared to Toronto prior to the 1920s, the years following World War I were critical to the separation of academic psychology from moral and mental philosophy. From this position philosophers dominated the teaching of psychological subjects at McGill in the early decades of the twentieth century. The activities of Montreal psychologists within and outside academic institutions (in schools, clinics, and classrooms of medicine), though small at first, was driven largely by opportunities made available through partnerships. No partnership was more important to early psychologists in the 1920s than with the Canadian National Committee.

An important difference between the US and Canadian National Committees was that the Canadian Committee had, from the beginning, secured itself solid footing within the much smaller Canadian university system in Toronto and Montreal. As planned, this footing was established through direct involvement of prominent members of the university community in Montreal (such as Martin and Peterson at McGill) (Roland, 1990). While some historians have examined the situation at the University of Toronto (e.g., Pols, 2002; MacLennan, 1987; Richardson, 1989) the influence of the National Committee on early psychology in Montreal has been comparatively less examined. This is likely due, in part, to the significantly fewer academic psychologists in Montreal and the less successful partnerships that were established. Tait was the only trained academic psychologist affiliated with a university in Montreal until the mid-1920s. Even after a department was established, it would continue to demonstrate a tendency to hire internally from its strong ties established within the department, choosing to appoint teaching positions to members from its own ranks.

When Tait returned to his appointment after the war as Assistant Professor of Psychology in the Department of Philosophy, his work expanded considerably. In the first five years of his appointment he had focused primarily on teaching; he published three articles during this time, one on attitudes towards colours and associated feelings (1912), his doctoral research on the influence of feelings on memory (1913), and a eugenics perspective on intelligence (1914). After the war, in line with North American postwar trends (e.g., Capshew, 1999), Tait turned towards more practical concerns. In publications throughout the 1920s and 1930s, Tait expressed his overriding concern for the application of psychology to Canadian society and to various social problems, including education, crime, and public health (e.g., Tait, 1921; 1925; 1926; 1927).

Having begun to gain recognition as an independent discipline—first in the United States, and slowly in Canada—there was pressure to demonstrate that psychology could be used to solve problems of a practical nature. This spurred new considerations about the role of psychology, its place in relation to the applied/helping professions (i.e, those that nurture the growth of or address the problems of a person's physical, psychological, intellectual, and emotional well-being). The creation and widespread use of mental tests during World War I had demonstrated how a psychological technology could serve the interests of governments and the military while contributing to the advancement of applied psychology. Regarding the important role the wars played in the development of psychology, Capshew wrote, "They did more than 'prove' that psychology was of practical use; they brought new opportunities and increased resources, moral as well as financial, to the community. They also helped psychologists to think of themselves not only as professional scientists but also as scientific professionals" (Capshew, 1999, p. 6).

Tait followed in the footsteps of his mentor, Hugo Münsterberg, and embraced the focus on psychology as an applied science in service to the interests of the state (Moskowitz, 1977). Indeed, despite the rhetoric of scientism in the New Psychology he espoused, Tait tended to show little concern for psychology as a scientific discipline in practice. Indeed, while Tait is credited as having established one of the earliest psychology laboratories in Canada and spoke of the discipline as "to-day one of the experimental Sciences" (Tait, Dec 14, 1920), records I consulted suggest he published and supervised little (if anything) in the way of empirical research throughout his long career at McGill. Whether it was the absence of institutional support, the necessary interest, or expertise, or simply a lack of time dedicated to experimental pursuits, we do not know. However, despite later criticism that he had done "nothing to improve the situation" of psychology (Hebb, Sept 23, 1954), Tait's contribution to the development of psychology at McGill was important.

Tait possessed a keen regard for the formation of psychology's disciplinary identity. But instead of an experimental science, records indicate he was interested in developing the horizon of possibilities for a young discipline in relation to various applied fields including education, medicine, business, social work, physical education, and human relations. In a letter to the Secretary and Bursar in 1920, he described his work and associations with the postwar reconstruction effort, social work, and medicine (Tait, Dec 14, 1920). Here Tait expressed his conviction that psychology possessed particular potential for expansion in the areas of education,

industry, and medicine. In relation to education, he wrote: “Educational psychology demands more attention because psychology forms the basis of nearly all educational methods. Courses in genetic psychology, child psychology, and the practical applications in the schoolroom should be available for the teaching profession as also graduate courses for teachers desiring higher degrees” (Hebb, Dec 14, 1920).

Tait was enthusiastic about the contribution of psychology to the community. On this he wrote: “Business Psychology offers an opportunity for considerable extension [...] There is almost unlimited opportunity for bringing McGill in closer touch with the community by means of extension lectures on the psychology of history, art, nationality, business, etc.” (Tait, Dec 14, 1920). Following upon the practical work of mental testing that had “put psychology on the map,” Tait was certainly not alone in his enthusiasm for the applications of the New Psychology. In fact, famed Canadian humorist (and McGill professor) Stephen Leacock (1869-1944), even wise-cracked that the 1920s were witnessing a veritable “outbreak of psychology” (1924, p. 471).

The attitudes Tait expressed in this letter to the Secretary and Bursar on the possibilities for psychology were likely formed, in part, through his ongoing participation with the mental hygiene movement in the early 1920s, both in the areas of educational research and psychological teaching in medical schools. These collaborations were formed as part of the explicit objective of the National Committee to address the perceived gap between modern scientific understanding and institutional practice regarding the menace of the feebleminded (i.e., Pols, 1999): What was needed, the National Committee suggested, was to bring health workers from various disciplines together to address common social concerns and to reform institutions and policies related to mental deficiency. From the beginning of the Committee's organization, psychologists were among those invited to work together towards these objectives.

At the first meeting of the Executive Board of the Committee in 1919, a gift of five thousand dollars from Lady Eaton was announced with the stipulation that part of it be used for work in industrial psychology (Richardson, 1989). This led, in 1920, to the establishment of a sub-committee on Educational and Industrial Psychology, led by James A. Dale (n. d.). Dale was the founder of the McGill School of Physical Education (the first of its kind in Canada) and Professor of Education from 1908 to 1921, when he left for Toronto (Frost, 1984; Shore, 1987). The subcommittee consisted of members associated with the newly established Department of

Social Service at McGill (Richardson, 1989). This department (which later separated into the Department of Sociology and the School of Social Work) was part of an attempt to combine social work with social science at McGill (Frost, 1984; Shore, 1987). While Tait himself was not directly involved in this subcommittee, records of graduate theses indicate that a number of students completed MA degrees on topics related to education and industry and were often co-supervised by faculty from these departments between 1924-1930. Tait's letter to the Bursar in 1920 and the development of the sub-committee this same year indicate the enthusiasm with which Canadian psychologists were seen as important contributors to the postwar reconstruction effort and the betterment of society during this period.

In the early years of the Canadian National Committee academic psychologists at McGill were enthusiastic about opportunities to broaden their relevance to Canadian society, make advances towards professionalization, and about the kind of financial support their own institution failed to provide. While there was little or no money for basic research, there was, however, funds for applied research. These monies came primarily from partnerships between the university and anglophone Montreal philanthropists and were channelled through socially-relevant initiatives directed by leadership in the mental hygiene movement. From these kinds of collaborations, academic psychology developed in close partnership with education and social work at McGill (e.g., the ambitious interdisciplinary McGill Social Science Research Project on unemployment launched in 1930, discussed later).

Montreal psychologists were not as directly involved with the activities of psychiatrists as at the University of Toronto. Instead, early partnerships were formed in relation to mental hygiene initiatives such as the development and conduct of surveys for use in public institutions (like schools and hospitals) and in the teaching of psychological subjects to medical students. These activities are important because they demonstrate the expanded role of psychologists in modern society and contributed to the professionalization of these disciplines (McLennan, 1987).

2. b. The Montreal School Surveys, 1919-1921

It is unknown how and when Tait became involved with the mental hygiene movement in Montreal, but he was certainly sympathetic to the movement's more eugenically-oriented concerns prior to World War I (e.g., Tait, 1914). When Tait returned from the war in 1918, he was invited to conduct a mental survey of the Protestant Schools of Montreal with physician A. G. Morphy under the auspices of the newly created Canadian National Committee (Richardson,

1989). This survey, conducted between 1919-1920, with permission of the Protestant Board of School Commissioners, was part of a broader effort of the Committee to standardize the mental test on Canadian subjects and provided data on the "mentality" of different schools (Clarke & Hincks, 1919; Pratt, 1921a, 1921b). Morphy and Tait conducted a survey of 355 students in eight schools including, "typical schools in the industrial, residential and foreign districts" (Morphy & Tait, 1921). The aim of the survey was to assess the number of "subnormal" students in the public school system, "the percentage of retarded, borderline, feeble-minded, imbeciles and idiots." Murphy and Tait were explicitly interested in "the relations between physical and mental defect, and to ascertain to what extent hearty and home conditions might be responsible for school difficulties" (Morphy & Tait, 1921).

The conduct of original Canadian research on this subject through the development of local knowledge gathered by these surveys, was important. Rather than relying on work conducted elsewhere (which had been the case with the eugenics movement in the 1910s), this work expanded the scope and authority of psychologists as experts of the normal and the needs of Canadian society, which was articulated to government and policy makers through the efforts of the National Committee. One of the most significant features of the surveys conducted in Canada is that they created a new basis for expert knowledge: the conditions existed to produce their own knowledge because 1) these surveys inevitably revealed that mental abnormality was more widespread than was previously assumed and, 2) Montreal and McGill were particularly attractive because of existing relationships between the academic institutions and relevant subject populations, such as delinquent students in the 1920s.

An important difference between the partnerships between psychology and psychiatry at McGill and Toronto was the accessibility of school children due to nineteenth century compulsory schooling laws in Ontario. Access to subjects (i.e., school children) allowed for systematic study and solved the problem of the stability of the object of study — this allowed psychologists at Toronto (i.e., Bott and Blatz) to turn schools into laboratories (Pols, 1999). This was not the case in Québec In Montreal, mental hygienist and physician Gordon S. Mundie (1885-1926) complained about the comparative lack of research done in Québec:

> "The public school should really be the clearing house for mental defectives, but to make it absolutely satisfactory, compulsory education is necessary. Unfortunately, the province of Québec still clings to mediaeval ideas on education, and while this idea lasts, there will be thousands of illiterates and feebleminded roaming our streets. Provided there was a compulsory education law which compelled every child to go to school up to the age of

> fourteen, there should be an efficient medical examination of every child" (Mundie, 1920, p. 69).

This law would not come to pass in Québec for more than two decades (until 1943) and Québec would be the last province to modernize its educational policy (Oreopoulos, 2005).

The surveys Tait and Morphy conducted showed "the unexpected ranges of mental deficiency and laid bare the urgent need for special classes in the English-speaking schools of Montreal" (Cameron & Silverman, 1965, p.189); they also demonstrated the importance of understanding normality – something which psychologists had argued was their expertise (Capshew, 1999). Therefore, the findings of these school surveys marked an innovation in the thinking of the Canadian National Committee. Psychologists who worked on these Canadian surveys helped change the focus of the National Committee from attempting to find solutions to the problems of abnormality to the prevention of maladjustment in the normal child (Richardson, 1989). While this concern for the influence of positive environments on the shaping of desired social behavior in children and the need for preventive measures prior to signs of maladjustment would gradually develop as a major theme in Canadian mental hygiene throughout the 1920s, I disagree with McConnachie (1987) that, unlike the Toronto school studies, university psychologists, not the practicing psychiatrists, dominated in Montreal.

Noting the importance of psychological expertise in the realm of the normal, the sub-committee on Educational and Industrial Psychology would later conclude that,

> "while the accomplishments of the Committee have been considerable in the past, nevertheless activities have been somewhat hampered because of a prevalent opinion that our interests are centered wholly in the feebleminded and insane. While there is no real justification for such an attitude, nevertheless it seems necessary to safeguard our future by laying emphasis on such work as is contemplated by the Department of Education and Industrial Psychology. If proposed activities are put into effect there can be little doubt that the Committee will be performing a much greater national service than has been the case in the past" (Minutes, December 3 1920; quoted in Richardson, 1989, p. 114).

The work referred to was that which dealt directly with the public schools in providing psychometric testing, modernizing methods of promotion and school grading, and initiating vocational guidance as a means to "bridge the gap between school and industry." It was this kind of non-psychiatric social and occupational adjustment which was to dominate Canadian mental hygiene in the years to come (Richardson, 1989). Shortly after the publication of the Montreal surveys, Tait was added to the Executive Committee of the National Committee.

2. c. Teaching Psychology in Medical Schools, 1921-1923

In the early 1920s, the teaching of psychology to medical students and staff was another area into which Tait, through his association with mental hygiene, extended the domains of possibility for early psychology. The situation in terms of psychiatric and psychological training available to medical students during the first decades of the twentieth century at McGill and in Canada has not been adequately examined. There was a significant gap in psychological and psychiatric education and training in medicine in Canada compared to the United States. Tait's appeal to his university for the inclusion of psychology (Tait, 1920) indicated it was not a significant part of formal medical education prior to this time.

It is perhaps surprising that, despite the fact that McGill University's medical school is the oldest in Canada (first degree was awarded in 1833), internationally recognized, and that prominent nineteenth century psychiatrists like Joseph Workman (1805-1894) and Richard Bucke (1837-1902) received their medical education at McGill (Greenland, 1967), no formal training in psychiatry was available until the early 1940s (Cameron & Silverman, 1965). Indeed, similar to many medical schools in North America in the nineteenth century, the role of the mind in health and illness was rarely considered; Canadian physicians interested in the emerging field of neurology and psychiatry focused on this area as part of their broader clinical practices (Murray et al., 2013) with specialization typically attained through further study abroad (e.g., in England, France, or at the Phipps Clinic at Johns Hopkins).

While Catholic religious institutions in Montreal provided some measure of care for psychiatric patients in the nineteenth century (Moran, 2000), facilities for the care of English-speaking patients was severely limited. In response, the anglophone (mostly Protestant) community of Montreal established their own asylum in 1890: the Protestant Hospital for the Insane (later renamed the Verdun Protestant Hospital in 1924 and the Douglas Hospital in 1965). While most French-speaking hospitals were administered by Catholic organizations, English-speaking hospitals like the Protestant Hospital were largely reliant upon wealthy benefactors and volunteers. These volunteers had little, if any, education in current psychiatric knowledge and practices. Indeed, prior to the establishment of Montreal institutions of psychiatric training and research in the 1940s, medical students, nurses, and social workers received education on psychological topics at private hospitals and asylums through partnership with the university. For example, starting around 1900, medical students from the Faculty of Medicine at McGill began

receiving lectures in psychiatry at the Protestant Hospital (The Douglas, 2014). Later, these lectures were given by the medical superintendent, Carlyle A. Porteous who had joined the staff of the institution in 1904 and became superintendent in 1923. Porteous was Professor of Clinical Psychiatry at McGill and Consultant Psychiatrist at several of the hospitals in Montreal (CNCMH, 1937).

When the Canadian National Committee for Mental Hygiene was established in 1918 there was little in the way of psychiatric training opportunities and research in Montreal. Given mounting concern about the growing numbers and inadequate care of psychiatric patients in Canada, the expansion of psychiatric and psychological training for medical students and health care workers was one of the core missions of the early Committee (Hutchison, 1973). As early as 1919, the National Committee had initiated a two-month course in mental hygiene, primarily for nurses specializing in psychiatry, in the Social Service Department of the University of Toronto (MacLennan, 1987). Collaboration between psychologists and psychiatrists in Montreal was much less direct.

The original director of this clinic was Gordon S. Mundie (1885-1926), Professor of Neurology at McGill and editor of the Canadian Journal of Mental Hygiene. In 1919, Mundie had organized a small group to establish a Montreal branch of the National Committee; with the help of the Committee President, the Dean of Medicine (Martin), this group was able to open a part-time Outpatient Clinic only twelve days later. The clinic was held every Wednesday afternoon and served both adults and children. In addition to Mundie the clinic was supervised by Colin Russel and A. G. Morphy, two psychiatrically-oriented physicians and assisted by one full-time and two part-time social workers (Richardson, 1989).

While it is unknown exactly how the association between Tait and the psychiatric outpatient clinic was originally formed (likely through his work with Morphy), by early 1920 he was a member of its Executive Committee of the Canadian National Committee and in the spring of 1920 he had published an article in the *Canadian Journal of Mental Hygiene* enumerating the reasons psychological education should be included into the medical curriculum (Tait, 1920). Following this, he was invited to give a course of weekly lectures to Fourth Year students at the Royal Victoria Hospital (Dec 14, 1920). And, while no psychologists were officially employed by the clinic, it opened with four physicians who had received training by Tait (Richardson, 1989; McConnachie, 1987). It is likely that Tait's involvement with the clinic formed the basis of

his conviction that, “Medical Psychology will soon require further extension [...] It is rapidly becoming of vital importance to all physicians, nurses, social workers, and those specializing in mental disease and mental deficit” (Tait, Dec 14, 1920). He continued to give lectures on psychology to medical students at the outpatient clinic throughout the early 1920s (Ferguson, 1982), at least until James W. Bridges (1885-1980) arrived at McGill in 1924. When Mundie died in 1926 he was succeeded at the clinic by W. T. B. Mitchell, who was brought from Toronto for the purpose (Hutchison, 1973). David Slight of the Maudsley Hospital in London was invited to join the staff as a professor of psychiatry in 1926 (Cameron & Silverman, 1965, p.190; Griffin, 1989, p.47). He was one of the first to introduce the teaching of psychiatry on the wards and as part of the clinic at McGill.

While spanning only a few years, Tait's role in teaching psychological subjects to medical students and staff was important because it signalled the beginning of new disciplinary relations between medicine and academic psychology in the early 1920s. The clinic at the Royal Victoria Hospital, “medical students, nurses, social workers and even physicians attend for instruction” (Tait, Dec 14, 1920), with many of the cases having been “sent up from the Juvenile Court” (Tait, Dec 14, 1920). These connections opened up new possibilities which had not been previously established and would continue well after in various forms through the work of James Bridges in the late 1920s and Donald O. Hebb (1904-1985) after World War II. The association of McGill psychologists with practical concerns, like those of the mental hygiene movement in Montreal, would also contribute increasing tensions and to a disciplinary identity which was at odds with the philosophy department within which early psychologists like Tait worked. The next section examines the tensions that developed within the philosophy department which ultimately led to the separation of psychology from philosophy and the establishment of an independent psychology department in 1924.

3. The Department of Psychology at McGill

3. a. Issues resulting in the establishment of a new department, 1918-1923

Three main sources of tension contributed to the separation of psychology from philosophy at McGill in 1924: 1) increased enrolment (i.e., demand for training in psychology), 2) interpersonal tension between faculty members, and 3) an emerging disciplinary identity for psychology distinct from philosophy. These factors are important to consider because, rather

than resolving these tensions with the creation of the new department, they instead persist as themes
important for understanding the character of psychology at McGill throughout the first half of the twentieth century.

After modest but steady growth from the late nineteenth century, when courses were first introduced at McGill, academic psychology grew dramatically after World War I. In 1920 there were seven psychology courses offered by the Department of Philosophy, including Abnormal and Educational Psychology, taught by Caldwell, Hickson, and Tait. The Department of Philosophy struggled to keep up with undergraduate demand for courses in psychology; enrolment in psychology courses had increased to the point that new classrooms had to be assigned to accommodate the numbers (Tait, 14 Dec 1920), additional staff was necessary to teach these courses (Caldwell, Feb 2, 1921), and an entirely separate department was often suggested to accommodate these changes (Tait, Jan 3, 1922). In 1922, 117 students were registered in the philosophy courses, whereas 253 were registered for the regular psychology courses, plus another 100 in the Extension Department (Tait, Jan 3, 1922). Tait lamented the situation in an appeal for additional teaching staff: "There is twice as much teaching in psychology as philosophy" (Tait, Jan 3, 1922).

Despite the increased demand for psychology and its widely discussed potential to ameliorate Canadian society (e.g., Tait, 1920), correspondence between Tait and the administration suggest psychology at McGill received little support from the university throughout this period. This was not unusual for the time; financial support for the social sciences from private and public sources was practically non-existent in Canada during the interwar years (Shore, 1987). However, Tait believed the situation at McGill was especially dire for psychology. Indeed, while Caldwell and Hickson taught some of the courses, most duties related to the teaching and development of psychology courses were left to Tait. By his own estimate, Tait was teaching approximately 21 hours a week and described himself in 1920 as, essentially, a one-man-department: "It is only stating a fact to say that I am attempting to do in McGill what three psychologists and assistants are doing in Toronto" (Tait, Dec 14, 1920). With the exception of the Macdonald Fund that Caldwell had helped secure, there was no financial support for psychology. Graduate training in psychology had ceased well before the outbreak of World War I and was only resumed once the new department was finally established in 1924.

Between 1905 and 1924 only three Master's degrees were awarded. The late 1920s represent the period in which graduate training seems to have been more active prior to World War II; more degrees were awarded in the late 1920s than the next decade.

Given the lack of support from the university and the desire to articulate an independent disciplinary identity during this period, it is unsurprising that Tait sought support for psychology in association with the growing mental hygiene movement in Montreal. This association dovetailed with the expanding role of psychologists (and the psy-disciplines) as experts of the normal in relation to the institutional demands of bureaucratization which accelerated after World War II (Rose, 1988; Heyck, 2015). Indeed, this association, which would begin as mutually advantageous but become increasingly antagonistic, would come to shape the kinds of psychology possible at McGill, both in the new Department of Psychology and in the Faculty of Medicine throughout the interwar period, and beyond.

3. b. The new Department of Psychology at McGill

Prior to 1924, psychologists and philosophers at McGill were described as having "lived in a strained relationship" (Frost, 1984, p. 147). This was largely due to differences between Caldwell, Hickson, and Tait, but also existed as the growing pains of a discipline that was in the process of re-defining its own boundaries. As the identity of psychologists in North America became increasingly associated with their methods—particularly mental testing—an emphasis emerged around the scientific establishment of normality; the domain of expertise of psychologists was related to the normal mind and the process of normal human development. This was emphasized in terms of psychology's relation to medicine, especially psychiatry.

The challenges that existed in the early 1920s included differences in methodological approaches to psychological subject matter, differences in disciplinary ambitions (i.e., professional and clinical applications), and interpersonal conflict between the three men (i.e., Tait, Caldwell and Hicks). The world that emerged in the aftermath of World War I was no longer accommodating of these differences. Opportunities became available for psychologists for which their connection to the trappings of philosophy and theological thinking was damaging. The detriments of affiliation quickly overcame perceived benefits; differences led to the institutional division of psychology from philosophy and the creation of an independent Department of Psychology headed by Tait in 1924.

The establishment of a separate department meant that psychologists at McGill were independent from philosophy. While never truly constrained—Caldwell appears to have supported the development of psychological subjects in philosophy (Ferguson, 1982)—the separation nevertheless came with important implications for the identity of psychologists at McGill; no longer affiliated with the philosophical traditions from which they emerged, Tait was finally afforded the autonomy to hire new faculty members and expand the psychology curriculum at McGill.

Correspondence indicates the relations between Tait, Caldwell and Hickson were strained throughout early 1920s and that eventually the three had difficulty remaining collegial with each other after Tait's return to the university in 1918. Ferguson characterizes Tait as having been "aggressive, materialistic, irreligious, a believer in determinism" (Ferguson, 1982, p.44) while Caldwell, on the other hand, was "scholarly, contemplative, religious, a believer in free will." Caldwell saw Tait as "difficult to get along with, brusque, headstrong, domineering" (quoted in Ferguson, 1982, p.44). Tait often found it difficult to get along with others, which is reflected in his correspondence. In an interview with R. B. MacLeod (1907-1972), a McGill undergraduate in the early 1920s, he described Tait as having "a very military kind of mind […] he remained an officer in peacetime" and his teaching style was "dogmatic" (Myers, 1974, p. 106). These characterizations of Tait are borne out by correspondence indicating frequent interpersonal conflict with colleagues including Bridges, Hincks, and Martin, among others.

There were tensions between Caldwell and Hickson as well. These frictions came to a head during the 1922-1923 academic year when Caldwell took a leave from the Department to travel to Europe. For some time, things had not been going well for Hickson in terms of his career; he had been teaching for almost two decades, promotions were not forthcoming, and his relationship with Caldwell, his academic superior, were sour. In the spring of 1923, with Caldwell still away, Hickson submitted his resignation to Principal Currie directly (Hickson, Apr 19, 1923) and Caldwell only found out about Hickson's intentions to resign months later. Correspondence during the ensuing months shows that Hickson's departure from McGill was in response to Caldwell's management style and upon his final departure in January of 1924, Hickson noted that he would have looked forward to remaining at McGill, "*had my immediate environment been different*" (Hickson, Jan 31, 1924, emphasis in original).

After World War I, Tait discussed with Principal Currie and university administrators the possibility and importance of establishing a new department of psychology at McGill (Tait, Dec 14, 1920; Tait, May 30, 1921; Tait, Jan 3, 1922). Separate departments of psychology were being established across the United States (Garvey, 1929) and both Tait and Caldwell referred to these trends in their appeals to the university (e.g., Caldwell, ca. June 1921; Tait, May 30, 1921). It is important to note that, as mentioned, Tait was in the process of aligning himself with the aims and practices of the mental hygiene movement; he wrote articles for the *Canadian Journal of Mental Hygiene*, conducted surveys of public schools, and gave lectures on the relevance of mental hygiene to the lives of various publics, such as teachers and medical students (Ferguson, 1982). In these letters Tait noted how, given the activities with which psychologists like him were involved, it was inappropriate for such a prominent Canadian university as McGill to not follow the steps other (mostly American) universities had taken in establishing independent departments. For example, Tait wrote to the Secretary and Bursar of McGill in 1920 to explain that: "There is no more affinity between Psychology and Philosophy than between Biology and Philosophy because Psychology is to-day one of the experimental Sciences. In many universities it is a department by itself and in nearly all catalogues of educational books it is listed with the sciences" (Tait, Dec 14, 1920). Likewise, Caldwell was keenly aware of the divergence taking place between the two subjects. In the summer of 1921, he wrote to President Currie in support of the division of philosophy to create a new and autonomous Psychology Department (Caldwell, ca. June 1921). Caldwell supported the move to separate psychology from philosophy; perhaps as a means to resolve these interpersonal tensions inside the department as well.

Although the seeds had been sown in the years preceding his departure, it was Hickson's resignation that finally set into motion the official separation of psychology from philosophy at McGill. A year after Hickson's initial resignation, the newly named Dean of the Faculty of Arts, Ira MacKay, sent forward a recommendation to the Faculty of Arts that psychology and philosophy be divided into two departments (MacKay, Apr 2, 1924). MacKay emphasized his support for the move in the same letter: "I personally approve of the separation of these subjects. It has, in any case, become customary in all universities for the psychologists to claim their subject has now reached a stage of its development where it is entitled to rank as a separate special science with Physics, Biology and the other natural sciences. In any case I am perfectly sure that the Department of Philosophy and the Department of Psychology will work even more

harmoniously in the future if they are separated than if they remain subject to a single head of the department" (MacKay, Apr 2, 1924). Unfortunately, as I will explain in the next section, the relations between psychology and other disciplines was not "harmonious."

4. Separation and Consolidation: Scientific Psychology between Philosophy and Mental Hygiene

In 1924, Principal Currie authorized the new department and Tait was promoted to full Professor, the first official Chairman of the Department of Psychology. This same year the Canadian National Committee received a substantial amount of funding from the Rockefeller Foundation to expand mental hygiene activities in Montreal. One of the areas that the National Committee had identified as being important for the implementation of its goals was the development of Montreal as a site for teaching and research related to mental hygiene. Despite the fact that the National Committee considered the fledgling discipline of psychology to be among the principal strands of its multidisciplinary health movement (Pols, 1999) and that psychologists like Bott and Blatz at Toronto were already working closely with the Committee, they chose not to support Tait and the activities of the new Department of Psychology. Instead, the leadership of the National Committee in Montreal under C. F. Martin, who was at that time the Dean of the Faculty of Medicine at McGill, decided to hire their own psychologist: James W. Bridges (1885-1980).

Bridges went on to establish an independent, but ultimately short-lived, Department of Abnormal Psychology (1924-1930) in the Faculty of Medicine. The National Committee's decision to support Bridges (by committing resources to provide for his and his wife's appointment and laboratories expenses) and Tait's subsequent refusal to cooperate with Bridges, led to a rift between two kinds of psychology that were emerging simultaneously, one associated with academic and experimental methods headed by Tait in the Department of Psychology and the Psychological Laboratory in the Faculty of Arts and the other, more aligned with the applied methods of clinical and developmental psychology, by Bridges and his wife at the Department of Abnormal Psychology, the Nursery at McGill, and the Mental Hygiene Institute in the Faculty of Medicine. The following outlines what is known about the work of these separate strands, the problems that arose and how efforts were made (unsuccessfully) to bring them together. This section closes with an examination of some of the possible consequences for the development of

academic psychology at McGill prior to World War II as a result of this situation and the psychological landscape going into the war.

4. a. Activities of the new Department of Psychology under William Tait, 1924-1935

The late 1920s and 1930s represent an important but little examined period for the development of psychology in Canada. Histories of Canadian psychology tend to focus on James Mark Baldwin and the development of psychology at the University of Toronto. There are few accounts of the emergence of psychology in what at the time was Canada's largest city: Montreal.

The Department of Psychology at McGill was the first department of its kind at a Canadian university (Wright & Myers, 1982). Despite operating its own *de facto* department decades prior to the establishment of McGill's department, it wasn't until 1926 that the University of Toronto would establish its own (Myers, 1982). The establishment of McGill's department in the early 1920s was early for Canada but not internationally. Canada, compared to the United States, was much slower to develop independent departments of psychology; most of the teaching of psychology in Canada was done in departments of philosophy until the late 1950s with some departments having "suffered a painful birth trauma which effectively retarded their development for several years" (Wright, 1969, p. 239). In these respects, the emergence of academic psychology at McGill was unique. In the years immediately following its establishment, the new department was more active, in terms of courses and graduate research, than it would be at any other time in Tait's long tenure as chairperson.

As Chairman of the new department, one of Tait's first tasks was to appoint an additional professor in psychology. Among the candidates who were considered—including Gordon Allport, Paul Young, and George Humphrey—Chester E. Kellogg (1888-1948) was offered and accepted the position of Associate Professor in 1924, starting at $3300 (Kellogg, May 1, 1924). Kellogg was an American with a PhD from Harvard where he had worked primarily with Robert Yerkes. He served with Yerkes during WWI as a civilian examiner administering mental tests to recruits (National Academy of Sciences, 1921; Kellogg, 1923). Kellogg later co-authored the controversially-titled McGill University Revision of the U.S. Army Beta Examination with N. W. Morton, his student and the first PhD graduate in Psychology at McGill (Frost, 1984). Morton would go on to play a critical role in the development of academic psychology in Canada after World War II (see Chapter Four).

When Robert B. MacLeod, an undergraduate and one of the first graduates of the new department, was interviewed about his experiences in the mid-1920s, he described it as follows: "… [The Department of Psychology] had one little room about 15 feet x 25 feet that was the combined office, lab and library, of the department. Kellogg had a little table about 2 feet x 4 feet at which he sat and we could come in and talk with him. He was always very cordial. Tait had an office somewhere else but nobody ever went voluntarily to see him" (Myers, 1974, p.106). In 1926, "they reconstructed the Arts Building and Tait got a sizable hunk of space in the Molson Wing of the building. There was a fairly big introductory lab — nobody would design a lab like that now—the tables were all screwed to the floor. Each table had an electrical outlet and compressed air. There was a photographic darkroom and a soundproof room. There were two offices: one for Tait and one for Kellogg, and a shop which was also a storage room, and about four other rooms which could be called research rooms, one of which became a miniature library. But no research was done there" (Myers, 1974, p. 107).

With the establishment of his own department Tait was able to expand its scope and define its direction independent from philosophy. Like most psychologists of the time Tait was driven by the need to demonstrate the independence of his discipline from philosophy. Charged with establishing his own department, Tait was committed to the project of advancing psychology as a scientific discipline. The common theme behind many of the ensuing clashes between Tait, the administration, and other faculty members was his vision of the new science of psychology, how it should be represented, and by whom. For example, Tait scolded other psychologists for involving themselves with local Practical Psychology Clubs (Tait, Oct. 10, 1927). These clubs, typically associated with popular psychology magazines, such as *Golden Rule Magazine: The New Psychology* (1919), *Popular Psychology: The Magazine of Straight Thinking* (1920), *Herald of Psychology* (1921), and *Psychological Review of Reviews* (1923), encouraged public engagement with the early applied psychology and grew in popularity during the 1920s (Benjamin, 2012).

Psychology at McGill in the 1920s turned away from philosophy and towards social service. Patterns of co-supervision (between students in psychology and faculty in sociology, education, and English) as well as choice of research topics indicate clear priority for applied problems suitable to social service (e.g., education, public health, unemployment, personnel selection, and vocational guidance). Given Tait's training at Harvard with Münsterberg (an early

pioneer of applied and industrial psychology), the topics of publications, and postwar work with the National Committee for Mental Hygiene, it is unsurprising Tait would take psychology at McGill in this direction. Indeed, through association with other departments, like the newly established Department of Social Service, Tait would go on to fashion psychology in the interwar period as a 'bridging' discipline between the social sciences and the needs of society: described later by R. B. MacLeod as a "cross-roads discipline" (MacLeod, 1955, p. 43).

One of the early initiatives of the department was the establishment of a School Service Bureau in 1925. Before he had joined the department in 1924, C. E. Kellogg had been Professor of Psychology and Education at Acadia University and had a keen interest in psychological testing and test construction (Ferguson, 1982). Tait facilitated the establishment of this Bureau with the explicit aim of serving "at the disposal of superintendents, principals, teachers, parents, and others interested in education," as far as time and equipment permits (Annual Calendar, 1925, p. 180). The description in the Annual Calendar outlines its purpose: "to furnish aid and advice with regard to intelligence tests and measurements, and other psychological aspects of education" (Annual Calendar, 1925, p. 180). The School Service Bureau was an explicit attempt to extend the kind of work Tait had done for the National Committee with the public school surveys, but under his own roof.

Psychology at McGill brought new tools to address social problems and Tait and Kellogg were enthusiastic about showing what the New Psychology was capable of contributing to Canadian social problems. In addition to the School Service Bureau, Kellogg also became involved with the revision of the U.S. Army Beta Examination that he had administered while working with Yerkes during World War I. Kellogg began work on the revision of this test in the early 1930s with N. W. Morton (n. d.). Morton joined the department in 1932, worked with Kellogg on test development and was the first recipient of a Ph.D. degree in psychology at McGill (1933). The revised test was published in 1935 and was widely used by the Canadian military. It is interesting to note, for what it says about the status and public image of the discipline at the time, that there was considerable resistance to naming these tests after the university, first by the Registrar T. H. Matthews in a letter to Principal Currie (Matthews, Dec 11, 1931) and conveyed again by Principal Currie himself to Tait two years later (Currie, Jul 10, 1933).

Throughout the late 1920s and early 1930s undergraduate demand for psychology continued to increase. In response to this demand, Tait expanded course offerings in the department, introducing Business and Industrial Psychology, Child Psychology, Vocational Psychology, and several psychological measurement courses. Based on their own accounts, Tait and Kellogg were overworked and underpaid in these early years and had little time left outside social work for research and writing. The education of academic psychologists through graduate research and training at McGill, like all sciences, was especially important for a young department (only three Master's degrees were awarded for theses in psychology when it was part of philosophy). In 1928 a Ph.D. program was established and five graduate courses were initially offered. The calendar lists the regulations for attainment of the Ph.D.: students were required to have some knowledge of advanced statistical methods and some ability in 'shopwork' (enough at least to show they were capable of designing and constructing simple laboratory apparatus). Comprehensive examinations were required on the history of psychology, principles of psychology, experimental and physiological psychology, statistical method, and contemporary psychology (Ferguson, 1982).

Between 1924 and 1946, about 40 master's degrees in psychology were granted. The first Ph.D., titled "The industrial quality of the unemployed," was supervised by Kellogg and awarded in 1933 to N. W. Morton. Only four such degrees were granted between 1928 and 1939 (Nelson W. Morton, 1933; Edward C. Webster, 1936; Joseph A. Bois, 1936; Kenneth E. Norris, 1939) -- all of whom would later work in the department. While the rate of Master's degrees being earned was greatest in the late 1920s, between 1924 and 1943 there was a stable rate of about two every year. Throughout the 1920s Tait tended to co-supervise MA students with faculty outside psychology. Throughout the 1930s and early 1940s Tait and Kellogg would alternate supervision of students every couple of years. Tait was involved in the supervision of two-thirds of all degrees awarded and Kellogg a third.

Topics throughout the 1920s and 30s were consistently of an applied nature and involved a review of current thought about a particular subject of social interest rather than empirical experiments. Indeed, MacLeod, as a graduate student in the department from 1926-1929, reported that, "Nobody was doing any experiments except these little exercises in the undergraduate lab" (Myers, 1974, p. 108). MacLeod noted that it wasn't until he studied abroad in Germany in the late 1920s that he 'discovered psychological experimentation' (Myers, 1974,

p. 108). The first thesis which included original research was that of Kenneth W. Spence (1907-1967) in 1930. He conducted experiments on complex maze completion with psychology undergraduates at McGill and would be known for his theoretical and experimental contributions to learning theory and motivation (see Chapter Three; Kendler, 1967). Of all the 42 degrees offered by this department prior to 1943, only seven of them were earned by women.

While abroad in Berlin on a Moyse Traveling Scholarship, MacLeod summarized his impressions of "German methods and our own methods" which he sent to Dean MacKay (MacLeod, Mar 6, 1929). In short, MacLeod held that McGill stood up well to comparison, although he suggests "a greater emphasis on systematic investigation, if necessary, at the expense of some of the teaching" would be a good idea (MacLeod, Mar 6, 1929). Correspondences show MacLeod stayed in contact with Currie and the Department while holding positions at Columbia and Swarthmore (MacLeod, Apr 17, 1933), and applied for an Assistant Professorship at McGill in 1931 (MacLeod, Jan 27, 1931). MacLeod would later return to McGill to play an important role in postwar reconstruction of the Department of Psychology (see Chapter Two).

A report prepared by Tait in 1930 described the typical research interests of the time. The list of topics actively under research varied and included: 'relation of reaction time to physiognomy, race and sex'; the study of 'humidity as it affects work' in cooperation with the Department of Industrial Medicine; correlation between Allport A-S Reaction Test and intelligence; psychological survey of Rushbrook school with 'special reference to grading, mental hygiene, and curriculum'; 'psychology of journalism with reference to make-up, headlines, etc.'; the study of 'the fatigue of school children at three levels of intelligence'; the 'psychology of applause'; study of rhythm and time; a 'new type of paper image allowing for visual cues,' and so on (Tait, Apr 22, 1930).

4. b. Activities of the new Department of Abnormal Psychology under Bridges, 1924-1930

Throughout the 1920s and 30s psychology in the Faculty of Arts maintained a steady amount of teaching and graduate training. At the same time, however, another strand of psychology at McGill emerged in the Faculty of Medicine. The Department of Abnormal Psychology -- though lacking formal graduate training -- maintained a fairly regular schedule of research and teaching of clinically-relevant psychological subjects from 1924-1935. Although little is known about specific activities, it represents one of the first official departments of its

kind at a School of Medicine in North America (Ferguson, 1982). Furthermore, the establishment of two departments during a time in which most Canadian universities had none, is significant for what it says about the tensions of disciplinary formation and the institutionalization of academic psychology during this period.

The establishment of the Department of Abnormal Psychology at McGill was possible due to funds made available by the Rockefeller Foundation under the guidance of the Canadian National Committee for Mental Hygiene. The Canadian and the U.S. National Committees developed different relations with the Rockefeller philanthropies (Fisher, 1993; Kohler, 1991). When the U.S. National Committee was receiving much of its early funding, the guiding policy was the mobilization of existing knowledge related to public health and mental deficiency; as this policy was slowly phased out in the 1920s the U.S. Committee received less funds (Pols, 1999). The agenda of the newly formed Canadian National Committee (est. 1918) organized itself around the emerging priorities of its primary funders. The Laura Spelman Rockefeller Memorial fund (LSRM) played a significant role in the funding of early social science research in North America (Bulmer & Bulmer, 1981). The Canadian Committee was one of the first institutions to receive funding according to the policies of the LSRM. The officers experimented with funding university-based research centers with the aim to create a new generation of elite scientist-teachers (Pols, 1999; Bulmer & Bulmer, 1981). The Department of Abnormal Psychology at McGill was one such research center in Canada.

The Rockefeller Foundation was fundamental to the development of medicine at McGill after 1924 (Brison, 2005; Frost, 1984). Following a survey of Canadian universities by the Foundation's Division of Medical Sciences in 1920, the Rockefeller Foundation decided to support a number of medical schools in Canada. Working together in a national system, the aim was to have these schools address regional and cultural considerations as well as medical ones: It was an example of central management not only for medical education, but also for Canadian culture (Brison, 2005). The Rockefeller Foundation hoped to transform specific institutions in strategic locations across Canada in a way similar to what the Carnegie Foundation had done with Johns Hopkins following the results of the Flexner Report in 1910 (Flexner, 1910). McGill University was among a handful selected to become models for less fortunate universities to emulate and received substantial grants in 1923 and 1932 (in addition to the original endowment). Indeed, the 1932 grant was the Foundation's largest in the medical sciences that

year and went towards the establishment of the Montreal Neurological Institute, the construction of laboratory facilities at McGill's Royal Victoria Hospital, and endowed new departments in clinical neurology, neuropathology, neurophysiology, and neurosurgery (Brison, 2005; Gavrus, 2011). McGill and Montreal were designated centers for American and Canadian investment in brain studies in the 1930s.

The Rockefeller Foundation used the National Committees as intermediaries to coordinate the funding of a wide diversity of initiatives in mental hygiene (Richardson, 1989; Pols, 1999). The decision to use intermediaries was designed both to maintain a position of impartiality and independence as well as to be able to rely upon the experts to judge the quality of applications for funding. Like the mental hygiene program in Toronto, the McGill program was mostly funded by the LSRM. Beardsley Ruml (1894-1960), consummate "man of affairs" and the first director of the LSRM in 1922-29, had received a Ph.D. in psychology from the University of Chicago in 1917. Ruml had hopes of addressing real social problems and bridging the cultural lag between the natural and social sciences through institution building and the support of interdisciplinary, problem-oriented research (Bulmer & Bulmer, 1981; Samelson, 1985; Shore, 1987).

Rockefeller Foundation officers were pleased with the psychological research in Toronto and with the Canadian National Committee in general (Pols, 1999). The role of psychologists and other behavioral scientists was important to the Foundation's plan to increase its emphasis on the support of advanced medical teaching and research in order to properly realize the Flexner plan in Canada. The Rockefeller Foundation was therefore keen to foster the kind of productive relations between academic psychologists and members of the Faculty of Medicine at McGill as had been established at the University of Toronto. The drive for the inclusion of more psychological research and training at medical schools corresponded with the concerns of the president of the Foundation, George E. Vincent (1864-1941), himself a Chicago-trained sociologist, that scientific medicine tended to overlook the whole person and therefore supported investigation into the field of psychosomatic medicine (Pressman, 1998).

The Canadian National Committee received a large grant ($75,000) from the LSRM fund and the Lady Byng of Vimy Fund for Mental Hygiene was inaugurated in 1924. Thus, by the mid-1920s, the mental hygiene movement in Canada had received a substantial influx of financial support already (Richardson, 1989). Hincks, who had been intimately involved in the

creation of the original National Committee, maintained a warm relationship with officers from the Rockefeller Foundation and was credited for the procurement of these funds (Richardson, 1989). Support for the activities of the National Committee was negotiated by Hincks on the grounds of an explicit separation of research labour: "two strategic mental hygiene centers specializing in two fundamental branches of work […] Montreal would be concerned primarily with the organic side of psychiatric research, while Toronto work would be concerned chiefly with the psychological approach" (CNCMH, 1924). It was this designation of "two strategic mental hygiene centers" that I argue contributed to the creation of two separate strands of psychology at McGill as the National Committee chose to support the "organic side of psychiatric research" through the Department of Abnormal Psychology in the Faculty of Medicine. Financial support was eventually withdrawn due to a lack of cooperation between two rival departments, and unable to develop ties with the medical community as had been established at the University of Toronto (Pols, 1999).

Much of the Rockefeller funds for mental hygiene supported National Committee activities in Montreal. For example, Baruch Silverman (n. d.) opened the Child Guidance Clinic at the Royal Victoria Hospital in 1925. Here, longitudinal studies focusing on the role of environmental conditions on abnormal behaviour were carried out and the Family Life Education programme was initiated; Silverman gave public lectures on the theme of "Mental Hygiene of Childhood" (Griffin, 1989; Gleason, 1996). Psychologists working at the Faculty of Medicine received salary support from the National Committee (Griffin, 1989; Gleason, 1996): In 1926, the National Committee was granting salaries and honoraria to 22 staff members – 16 of whom were psychiatrists or psychologists (CNCMH, Dec 3, 1926). Silverman was among the first practicing psychiatrists in Montreal (Hutchison, 1973; Cameron & Silverman, 1965).

In 1929, the outpatient clinic at the Royal Victoria Hospital became the Mental Hygiene Institute of Montreal and moved to university buildings adjacent to the medical school and hospital. One of its primary activities was the teaching of psychiatry and mental hygiene to undergraduate medical students, nurses in training, and students in the McGill University School of Social Work. When the Allan Memorial Institute was founded in 1943, the Mental Hygiene Institute reverted to its primary purpose of encouraging mental health in the population at large. B. Silverman guided the activities of the Mental Hygiene Institute as Assistant Director 1929-1941 and succeeded W. T. B. Mitchell (n. d.) as its Director from 1941-1969. The Mental

Hygiene Institute was important for disciplinary expansion; the clinic represents one of the earliest sites for the collaboration of psychologists and psychiatrists at McGill, supported and developed by the National Committee (Hutchison, 1973).

The Rockefeller Foundation grants received by the Canadian National Committee, and overseen by the Dean of Medicine (Martin) allowed for the appointment of a full-time psychologist to the Faculty of Medicine. James W. Bridges (1885-1980) was called in 1924 from the University of Toronto where he had worked with Bott and had met Hincks. Through this connection to Hincks he was selected to head efforts to establish a medically-oriented psychology program in Montreal (Bridges, 1966). Bridges had a background in what would now be considered clinical psychology; he had worked as a psychological intern in the Psychopathic Hospital in Boston in 1913-1917 where he met psychologist Robert Yerkes (1876-1956) with whom he collaborated on test development for the U.S. Army during World War I (Haraway, 1989). In 1924 he was appointed Associate Professor in the Faculty of Medicine and established the Department of Abnormal Psychology. Bridges was among the first psychologists in North America appointed in a Faculty of Medicine (Ferguson, 1982; Bridges, 1966). Bridges' appointment to the McGill Medical School was part of Hincks' aim to extend the work of the National Committee and the New Psychology into Montreal (Bridges, 1966). The teaching of psychological principles to medical students and staff was conducted at the outpatient clinic at the Royal Victoria Hospital in partnership with faculty and staff at the Montreal Mental Hygiene Institute. In 1924 Bridges took over this teaching appointment which had been held by Tait.

In 1929, Bridges was promoted to full professor in the Medical School. By then, Bridges was teaching at least four courses as part of the Department of Abnormal Psychology to medical students: a basic course in both normal and abnormal psychology which was to follow regular courses in anatomy and physiology (Bridges, 1966), a course on mental measurement for students in psychiatry, a course on child psychology for students specializing in pediatrics, and a course on psychology in industry intended for students in industrial medicine (Ferguson, 1982).

Psychology in the Faculty of Medicine was doing quite well under Bridges and was considered an important subject by the Dean of Medicine; the 1920s and 1930s were the most productive in Bridges' career (Ferguson, 1982): In 1930, he wrote a textbook, *Psychology, Normal and Abnormal*, and in 1932 he published *Personality, Many and One*, a book concerned with personality variables, and their integration. In 1935 another book, T*he Meaning and*

Varieties of Love, was published. He also wrote a number of articles and monographs on juvenile delinquency and problems of mental health and abnormal psychology based on his work with the National Committee (Bridges, 1966). Bridges accepted short term teaching positions as a visiting professor at Clark University in 1926 and conducted summer courses at University of California Berkeley in 1930 and 1932 (Bridges, 1966).

Early intervention and parent education were viewed as essential elements in any effective preventive mental hygiene program. Encouraged by Hincks, the LSRM funds were used to establish a Child Nursery School and Child Study Centre at both McGill and Toronto, respectively. The objective in establishing such schools in university settings was to achieve, through child development and family relations research, mental health goals. Hincks and the Committee for Mental Hygiene were convinced that mental health had its beginnings in childhood. Bridges' wife, Katherine W. Banham-Bridges (1897-1995), was also an academic psychologist. She completed her degrees at Manchester and the University of Montreal, specializing in the emotional behavior of children (which she published in both English and French), and was the first woman to graduate with a Ph.D. from the University of Montreal in 1934 (Wright, 2002). She came with her husband from Toronto and joined the Nursery school at McGill when it was established under the direction of pediatrician A. B. Chandler (n. d.) in 1925. Banham-Bridges was responsible for research, and Bridges took on an advisory role. She published several studies from this time, worked with her husband on various problems of juvenile delinquency (e.g., Bridges & Banham-Bridges, 1926) and developed a genetic theory of emotions relating early childhood to mental health problems (Banham-Bridges, 1932; Northway, 1973).

Unlike the Child Study Centre established at the University of Toronto, the Nursery ultimately failed to receive adequate support from the Department of Psychology (Wright, 2002): Faculty members and students showed little interest in collaborating or conducting research at the nursery, which meant Banham-Bridges tended to work alone (Banham, 1983). The National Committee withdrew its financial support after five years since a wider research program had not been established during this time -- the nursery had not been used by psychologists or any other department at the university for teaching or research purposes. The Institute for Child Study at Toronto, on the other hand was a striking success (Pols, 2002). Bridges later established himself at Sir George Williams University (today Concordia University) where he remained from 1940-

1963 and Banham continued academic work in the field of child development at Duke University in North Carolina.

When conditions at the Rockefeller Foundation changed and funding for mental hygiene research at McGill was no longer forthcoming in the 1930s, Bridges and Banham-Bridges lost their appointments at the university. Bridges describes the conditions leading to his departure as bureaucratic: "We got a new Dean of Medicine and a new University Principal. The Medical group decided that students should have Psychology in the Arts Course before entering Medicine, and of course, they were backed by the Principal. This put me in a difficult position. So I resigned in 1938" (Bridges, 1966, p. 405). This account, however, neglects to mention the interdepartmental difficulties and the loss of Rockefeller funding, both would have tremendous bearing on the viability of the Department of Abnormal Psychology at McGill. In my research I've discovered evidence of ongoing attempts throughout the 1920s and 1930s by the university administration and National Committee to bring together the two strands of psychology – the department in the Faculty of Arts & Science and the department in the Faculty of Medicine.

These attempts were unsuccessful primarily because Tait refused to work with Bridges. This interpersonal conflict would affect the direction of psychology at McGill throughout the interwar period as Tait was unable (or unwilling) to establish the sorts of networks and alliances that the fledgling discipline of psychology needed for either strand of psychology to truly flourish during this period (as it had at the University of Toronto through partnership with psychiatrists). Indeed, in the following chapters I will demonstrate how much of psychology's early success in establishing itself as an independent discipline (from philosophy) was, counterintuitively, by aligning itself more closely to other disciplines (i.e., those of biomedicine).

The department of psychology at McGill represents an important case study: before and after World War II its relationship to medicine shifted dramatically (from an extension of the helping professions to a laboratory-based scientific field). I am arguing close collaboration between psychology and medicine has been a boon for its development in Montreal and at McGill, one which I will explore in greater depth in later chapters. For now, it is relevant to note some of the ways the interpersonal and professional challenges affected the growth of Canadian psychology. For example, psychology graduate students ceased to be co-supervised by faculty in other departments; faculty were hired internally and thus often failed to bring fresh new ideas or existing academic networks with them (strong ties); and some of the most prominent graduate

students during this period decided to leave McGill for more hospitable and cooperative pastures (e.g., MacLeod, Spence, and Hebb).

4. c. Burning Bridges: Interpersonal conflict between Tait and Bridges

The separation of psychology from Medicine at McGill shaped the development of this discipline in Canada for almost a quarter century. In this section, I examine some of the reasons academic psychology divided so starkly into these two different strands in the 1920s and 1930s. My main argument is interpersonal and best understood in the context of the role of the mental hygiene movement in the promotion of early psychology in Canada: At its core, this plays out in terms of the conflict that arose between Tait and members of the National Committee (such as Bridges, Hincks, and Martin). In correspondence among members of the university administration and the National Committee repeated concerns can be found regarding a disunified psychology at McGill. This section outlines the interpersonal and disciplinary differences that led, on the one hand, to two separate strands of psychology at McGill and on the other, to the gradual loss of support for psychology from the National Committee as the Rockefeller Foundation gradually lost faith in interdisciplinary public health initiatives in the early 1930s.

Although prominent psychologists, like William James (1842-1910), were among its founding members, the role of psychologists in the U.S. National Committee was less pronounced at its inception than its later Canadian counterpart (Pols, 1999). This early participation by psychologists had an important role in the direction mental hygiene would come to take in Canada compared to the United States: it transformed the focus of Canadian activities towards normal child development rather than a strict concern with mental illness or mental deficiency (Pols, 1999). This created space for psychological expertise in the child study program developed at Toronto but which lacked the same kind of support at McGill (designated the site for “the organic side of psychiatric research”). This created a fragmented mental hygiene movement in Montreal as medical men (like Silverman and Bridges) dominated the attention (i.e., allocation of funding for research and teaching) and support of the National Committee.

The relationship between Tait and Bridges begins in 1910, shortly after Tait was appointed to the Department of Philosophy. He taught Bridges' first class in psychology using James' *Briefer Course*. With Tait's help Bridges obtained a scholarship to Harvard the following year and completed a Ph.D. with Münsterberg, as Tait had done (Bridges, 1966; Ferguson,

1982). From 1915 to 1921, with the exception of the two years he spent working at the Surgeon General's Office during World War I, Bridges was Assistant Professor at Ohio State University. It was here that Bridges published *An Outline of Abnormal Psychology* (1919), an early attempt to systematically organize psychological abnormalities after his work during the war. This book was dedicated to Tait, which may signify a degree of admiration and/or gratitude Bridges felt towards Tait in these early years. This positivity was reciprocated as Tait spoke highly of Bridges having been an exceptional graduate student (Tait, Dec 14, 1920).

Bridges was well-established as a medical psychologist. He had worked with E. E. Southard (1876-1920) and with Yerkes as a "Psychological Intern" at the Psychopathic Hospital in Boston before the war. He was deeply interested in the applications of psychology to medicine, having been greatly influenced by the works of Morton Prince (1854-1929). Years before Bridges was hired by the McGill Faculty of Medicine, Tait suggested to Caldwell that he be recruited to the nascent Psychology Department in response to the shortage of teaching staff (Tait, Feb 3, 1921). However, Kellogg was later hired as a replacement for Hickson instead; regardless of whether or not an offer was ever formally extended, Bridges took a position to work with Bott at the University of Toronto in 1921 (Bridges, 1966).

The same year Tait finally managed to establish his own Department of Psychology independent from Philosophy, Bridges was offered the position of Associate Professor and took over the teaching of normal and abnormal psychology to medical students, a role Tait had occupied since having argued for its necessity several years earlier (Tait, 1920). While I cannot be sure about how Tait felt about Bridges' appointment at this particular time, given Tait's expressed interest in the expansion of psychology into applied and practical domains, and the importance of an association between the School of Medicine and the new Department of Psychology, I feel it is safe to assume he was not pleased. In fact, I think the hiring of a well-respected and highly-trained psychologist to the Faculty of Medicine, who was unaffiliated with the new Department, likely struck Tait as a threat to his dominance and authority over the development of the discipline at McGill and in Montreal. Indeed, the decision to allocate Rockefeller funds for psychological research to the Faculty of Medicine rather than the newly established Department of Psychology, likely struck Tait as significant indication of lack of confidence in his research capabilities. Given rumors that Tait was "antagonistic towards Mental

Hygiene" and characterized as "l'infant terrible" of the psychology department (Tait, Mar 2, 1928), it is not surprising that Hincks and Martin were hesitant to support his work.

I would also argue that, given lengthy correspondence regarding an overburdened psychology department, the additional allocation of scarce resources to the Department of Abnormal Psychology in the Faculty of Medicine rather than his own fledgling Department of Psychology likely struck him as an affront to his dedication to the National Committee. It is possible that the establishment of the new psychology department in Medicine was a move to contend with Tait's abrasive personality. Tait found it difficult to conceal his disdain for this decision. For example, in a department report to the Dean of Arts and Science, Tait decided to include his thoughts about the Nursery: "If a portion of the money paid out in salaries had been distributed in scholarships there would be a different story to tell to-day ... There would be four workers in place of one. However, its founders [the National Committee] had other ideas [...]" (Tait, Apr 22, 1930).

From my examination of Tait's correspondence, I believe Tait held a grudge against Bridges because he was hired and subsequently promoted over (and instead of) Tait's long-time colleague Kellogg in the Department of Psychology (with whom he shared a strong tie). Tait and Kellogg worked in close collaboration throughout their careers at McGill and while there is no direct evidence that this was the source of conflict, this hypothesis corresponds to certain facts and overall timeline. Regarding the allocation of resources to Medicine instead of Psychology, I found many letters between Tait and Currie regarding Kellogg's promotion. For example, in a letter to the Acting Principal, Tait suggests that Bridges' promotion was at the expense of the more deserving Kellogg (Tait, Sept 28, 1932). He goes on to accuse the Principal of demonstrating clear favouritism towards the Faculty of Medicine, as it is seen by the university as more important than Psychology (Tait, Sept 28, 1932). Indeed, Kellogg would not be promoted to full professor until 1947 (three years after Tait's death and one year before his own death — Kellogg had spent 23 years as an Associate Professor). It is not entirely clear why Kellogg's promotion was so often postponed. Perhaps, ultimately, and as usual, it was a matter of money. The funding situation in the department was dismal. Some instructors, such as Morton and Bois, worked at reduced wages. The fact the university was able to find space for Bridges and his wife but not for long-deserved promotions within the department no doubt irked Tait. It's also likely that Tait (and by extension, perhaps the whole department) felt a measure of jealousy

or rivalry towards Bridges (as the more academically successful of the two): Bridges was "clearly the most productive psychologist at McGill [in the 1930s]. His work was valued and respected. The Dean spoke highly of it in his annual report" (Ferguson, 1982, p. 48); the same cannot be said for Tait and his department at this time.

Tait actively blocked Bridges' attempts to work with the Department of Psychology. According to the Dean of Medicine, "[Tait] is apparently *not* interested in cooperating [with Bridges] as regards a unified plan" (MacKay, Mar 5, 1927, emphasis added). The lack of integration among the two departments of psychology was a longstanding concern to the leadership of the National Committee. In a letter to President Currie in 1930, Hincks expressed a deep concern regarding Bridges' treatment at McGill and the impact it was having on his career: "I have always felt that Professor Bridges could make his greatest contribution to the Department of Psychology of McGill, but he has never been able to integrate in a real way into that Department. Unquestionably he is one of the best psychologists in America" (Hincks, Feb 17, 1930). Indeed, tensions over the ownership and identity of psychologists (applied and clinical versus academic and experimental) related to scarce resources would persist well into the postwar period in different forms (see Chapter Four).

It seems to have been at least from 1927 onwards that Tait actively antagonized Bridges, both in terms of his research and activities. For example, there are a series of letters from October 1927 in which Tait expressed his disapproval of Bridges' decision to take part in local Practical Psychology Clubs (Tait, Oct 10, 1927; see Benjamin Jr., 2012). Tait sees Bridges' participation as tarnishing the public image of Psychology and legitimizing the activities of non-scientific groups (which Tait compared to chiropractors): "I assumed that as a McGill Professor and as a member of the American Psychological Association that you would not compromise the prestige or dignity of either by again appearing before this club. Evidently the assumption has been ill-founded […] This Department is new and is now making its tradition, it cannot afford to construct ones of this kind…" (Tait, Oct 10, 1927). These comments by Tait to Bridges became an issue as Bridges brought these matters up with the Dean of Medicine, who then wrote to the Principal, who then spoke to Tait about these matters.

This incident also added to the already strained relationship Tait had developed with members of the National Committee and the Faculty of Medicine at McGill. For example, in 1928, Hincks wrote to Principal Currie a letter expressing his frustration with Tait (Hincks, Feb

1, 1928): While Tait had been enthusiastic about the role of the New Psychology in the efforts of the National Committee, his attitude changed shortly after the Department of Psychology had been established. The leadership of the Committee seemed not to understand his change of position: In Hincks' letter to Currie he notes how, ever since Tait resigned from the Executive Committee, he had "assumed a more or less belligerent attitude" towards the mental hygiene movement as a whole. Hincks went on to explain that, "In a course of lectures in Ottawa and in his own lectures at McGill he has gone out of his way to ridicule types of mental hygiene activities [...] Recent articles he has written in scientific journals show more than traces of a caustic vein [...] Confusion is produced in the minds of the student body when they hear conflicting statements concerning our subject" (Hincks, Feb 1, 1928). In confidence to his friend and President of the Committee, Dean Martin, Hincks added that, "As a matter of fact, Tait has gotten under my skin. It is disconcerting in my travels throughout America to be asked about the Department of Psychology at McGill. While Tait is standing still, every other Department on the Continent is growing by leaps and bounds. I happen to know, as you know, that Tait is more or less of a discredited scientist to-day and, if he were in the Faculty of Medicine, he wouldn't last for a minute" (Hincks, Feb 1, 1928).

In addition to his personal dislike of Tait, Hincks was concerned about what the disunity of psychology at McGill would mean to the future of the mental hygiene movement in Montreal. In his letter to Principal Currie he emphasized that, given Tait's role as "Chair of probably the most important contributing subject [to mental hygiene]" (Hincks, Feb 1, 1928), there was a very real risk that Tait's attacks risked undermining their efforts in Montreal. If the two departments could not be convinced to work together they might lose Rockefeller funding and the National Committee might have to relocate their "chief citadel in Canada" (Hincks, Feb 1, 1928). Considering that Rockefeller funding was withdrawn only a few years later, there was certainly more to Hincks' warning than a hypothetical concern.

Despite his enthusiasm for applied psychology Tait recognized the importance of experimental work (though he seems not to have done any of his own) and expressed concern for the undue popularization of psychology in the interwar period. From Tait's perspective, mental hygiene had not fulfilled its promise of supporting the work of psychologists at McGill. Tait was wary of the National Committee's activities regarding parent education and public outreach and advocated for local research in schools and clinics. It is not clear whether Tait was opposed to

the mental hygiene movement itself or particular members, like Bridges. Tait himself maintained he was not against mental hygiene per se, but rather how it was being promoted and the ways in which funds were being allocated (Tait, Mar 2, 1928). Tait was certainly aware of the disparity between the development of psychology at Toronto and at McGill. It is likely he attributed this, in part, to misappropriated funding in Montreal. In correspondence with Dean MacKay, Tait explained his belief that mental hygiene was at its core the application within the school system of research done in child psychology. By Tait's estimation, it was research in child psychology within the universities that needed more attention (Tait, Mar 2, 1928). The Dean agreed that "too much money is being applied at the present time in an attempt to popularize subjects upon which as yet we know very little in a sound, practical, scientific way" (MacKay, Mar 5, 1928). Having noted this, Tait remained opposed to working with Bridges or Banham-Bridges at the Nursery School on these exact subjects.

When it became clear that two separate departments of psychology were developing, attempts were made both by McGill administrators, like the Dean of Arts and the Principal, and the National Committee (Hincks and Martin) to bring together psychology and medicine in the 1920s. Part of this was due to funding concerns as it was through partnership with the Canadian National Committee for Mental Hygiene that McGill received significant funding for its researchers (for example, the teaching of psychiatry at the outpatient clinic). The university was required to follow guidelines set by private philanthropies like the Rockefeller Foundation upon which university research funding was dependent during the interwar period.

The Rockefeller Foundation believed a unified and productive centre for mental hygiene research at McGill was dependent on contributions from academic psychology in Canada (Pols, 1999) and the Committee leadership expressed the belief that, "Professor Tait holds the Chair of probably the most important contributing subject to a sound mental hygiene" (Hincks, Feb 1 1928). The inability of psychologists and psychiatrists to work together in the same manner they had at the University of Toronto came to represent a serious threat for the renewal of support from the Rockefeller Foundation in the years after 1924. Both the university and the National Committee were dependent on this funding for their operations. Therefore, a number of steps were taken to attempt to bring together the two strands of psychology at McGill. For example, in a memo titled "The Situation as regards Psychiatry and Psychology," prominent McGill physician Jonathan Meakins (1882-1959) expressed the importance of (re-)organizing the

departments of psychology, psychiatry and mental hygiene following the recently acquired Rockefeller grants, "in order to produce satisfactory results from both the practical and scientific standpoint" (Meakins, Mar 5, 1927). The goal that Meakins articulates is the need to bring together these units to create something similar to what had been done in Toronto (e.g., the Child Study Institute). Meakins laments that, given Tait's refusal to work amicably with Bridges, "Psychology in Arts will be kept entirely separate from Psychology in Medicine [and] cannot, therefore, cooperate with the work done in Medicine nor with the efforts that we are now making in the Nursery school" (Meakins, Mar 5, 1927). The point of Meakins' memo was clear: the university was desperate "that something may be done to induce Professor Tait to gather about him these various units in order to develop a better spirit and a more comprehensive piece of work" (Meakins, Mar 5, 1927). Despite the support of the administration at McGill Tait failed to craft the kind of psychology that was capable of thriving: "While Tait is standing still, every other Department on the Continent is growing by leaps and bounds" (Hincks, Feb 1, 1928).

In March 1927, following the memo from Meakins, Tait and Bridges had "several informal conferences" with the Deans of Medicine and Arts regarding "the re-organization of the work being done in Psychology in the University." In these meetings some measure of agreement of cooperation between the two strands of psychology was established (MacKay, Mar 21, 1927). It is not clear to what degree Tait agreed with these decisions, however, as he appears to have continued to block cooperation between the two departments. Indeed, despite the condition that Bridges not be promoted before Kellogg, I can't help but think the initial promotion to Associate Professor in both Psychology Departments might have especially irked Tait, causing the rift to persist for almost another decade.

Dean MacKay continued to hold out hope that the two strands might come together. In a letter a year after the initial meetings, MacKay noted that "... all members of the University in the Department of Psychology and the Medical School and others professionally interested in this subject [child psychology] should get together […] I feel convinced that a frank, sound statement from McGill to the Rockefeller Foundation at the present time would greatly improve our relations with the Foundation" (MacKay, Mar 5, 1928). This unfounded optimism in common research purposes between the two departments continued. In response to Principal Currie's suggestion the Bridges be made full professor in Psychology in *both* departments, Dean MacKay dismissed the idea, stating: "I am sure that this Faculty [of Arts & Science] and its

Department of Psychology will be willing to offer Dr. Bridges and all his interested colleagues in the Medical Faculty every possible facility for carrying on their work in the psychological laboratory" (MacKay, Mar 6, 1930). He went on: "the psychological laboratory and all opportunities for psychological study in the University should be common ground for both the Faculty of Arts and the Faculty of Medicine" (MacKay, Mar 6, 1930). Unfortunately, relations did not improve and the inability to bring together psychology and medicine eventually led the Foundation to lose faith in the mental hygiene project in Montreal. Following some major changes at the Foundation in 1929, funding for mental hygiene activities in Canada and the United States gradually decreased (Pols, 1999; Pols, 2001).

5. Academic Psychology at McGill in the 1930s

The decision by the Rockefeller Foundation to reduce funding to the Canadian National Committee, which at that time was led by Hincks and Martin, contributed to the termination of the Department of Abnormal Psychology, the McGill Nursery experiment, and the appointments of Bridges and Banham-Bridges. With institutional elimination of the second department of psychology at McGill, Tait's academic psychology, would take a decidedly subordinate role in relation to existing professional (psychiatric/medical) and state (government) powers. Psychologists at McGill continued to be supported by the Canadian mental hygiene movement through the 1950s. These patrons of academic psychology supported the development of psychological technicians, especially in the realm of mental testing. This section examines the interdisciplinary ventures of McGill psychologists in the 1930s.

5. a. Psychology, mental hygiene, and the Rockefeller Foundation, 1929-1939

After the reorganization Rockefeller philanthropies in 1929, Foundation officers like Wickliffe Rose (1862-1931) began to redefine the mission of the Foundation in terms of the naturalistic sciences (Pols, 1999). The idea was that funding should be directed towards basic research because, only from fundamental knowledge, could new and useful technologies be derived. Alan Gregg (1890-1957), Director of the Medical Sciences Division from 1931 onwards, promoted a program in psychiatry which focused on stimulating research in major universities (Pols, 2001). Canadian mental hygienists, in collaboration with their National Committee ties to academia, took a public health approach which emphasized strategies for primary prevention (Richardson, 1989). The focus of its funders had changed and the activities of mental hygienists no longer fit

well with the Foundation's new approach. The Rockefeller Foundation became increasingly reluctant to make use of intermediary organizations like the National Committees.

In February 1930, Dean Martin reported to Principal Currie that the National Committee would be unable to continue its contribution to the stipend for the maintenance of the teaching of psychology in the Faculty of Medicine (Martin, Feb 10, 1930). This meant Bridges six-year appointment was coming to an end, but it happened gradually. Bridges continued to teach on and off for several years afterwards and Banham-Bridges pursued her PhD at the University of Montreal. Appeals were made to try to hold on to Bridges but Tait refused to take him into the academic department. In a memo from Principal Morgan regarding a meeting he had had with Tait, he reported that "[Tait] is not prepared to have Bridges in his Department, as he regards him as disloyal to himself personally" (Morgan, Nov 21, 1935). There is a fair amount of correspondence back and forth with Bridges and the administration regarding his untimely (and unfair) firing (e.g., Bridges, Jun 24, 1937; Brittain, Aug 9, 1937). Finally, in late 1935 Principal Morgan wrote to Martin saying, "I have explored every possibility of finding a niche for J. W. Bridges. It is quite clear that whatever may be his virtues it is impossible to think of his fitting into the Department of Psychology. It just won't work" (Morgan, Dec 7, 1935). Bridges was shut out of McGill's department.

Support for mental hygiene continued to decline in the 1930s as Hincks' plan for future mental hygiene research was too expansive and was therefore poorly received by Foundation officers (Pols, 2001). In particular, Gregg was not impressed with Hincks' optimism in the establishment of broad projects aimed at fostering the mental health of entire school populations (Pols, 1999). With the departure of Edwin E. Embree (1883-1950) in 1927, mental hygiene lost one of its most ardent supporters within the Foundation. When Gregg came to coordinate all work in mental hygiene in 1931, he intended to end support for both the American and Canadian National Committees (Pols, 1999, p.137). He said, "I do not believe that it is sound procedure to devote much money to more widespread and thinly spread endeavors in education and public health" (quoted in Pols, 1999, p.139). Indeed, it seems the National Committee's determination to focus on the promotion of positive mental health initiatives was likely the decisive argument for the Foundation's ultimate termination of its relationship with the movement. In 1939, Gregg announced that further grants would not be forthcoming.

5. b. Psychologists and the McGill Social Science Research Project

Throughout the 1930s, the psychology program in the McGill Faculty of Arts continued at a relatively slow pace as graduates of the department filled necessary gaps in teaching duties. Graduate theses in psychology were supervised by Tait and Kellogg and were earned between 1930 and 1943 at a rate of about two MAs a year. All four PhDs at McGill prior to World War II were earned between 1933-1939.

The National Committee continued to foster interdisciplinary relations between psychologists and other disciplines. The most notable example of this was the McGill Social Science Research Project (MSSRP). The Great Depression made high levels of unemployment an important national problem. In 1930 McGill received a five-year grant of $15,000 a year from the Rockefeller Foundation to support a program of interdisciplinary research on unemployment, the first of its kind in Canada (Irving, 1982). The MSSRP had close ties to the mental hygiene movement in Montreal and was led by both Martin and Hincks. The Department of Psychology was among the seven departments at McGill who collaborated on this project.

The goal of the MSSRP was to do something similar to what Yale had done with the Department of Human Relations but to have a more definite and practical objective. Each participating department chose one topic or more on the general theme of unemployment and proceeded simultaneously in a number of fields of specialized knowledge; the Psychology Department chose "juvenile placement and the industrial and qualitative character of the unemployed." From 1930 to 1940 this project determined in part the pattern of research in the department and addressed the employment crisis facing social scientists during this period (Finison, 1976; 1986). Under the guidance of Leonard Marsh (1906-1983), a recent graduate of the London School of Economics, a substantial number of social science monographs were produced: in psychology, Morton published *Industrial Diagnosis: A Manual for the Employment Exchange* in 1935 and E. G. Webster published *Guidance for the High School Pupil* in 1939. Several graduate students received financial support under this project and earned graduate degrees in the Department of Psychology on related topics, for example, Barbara Robertson (n. d.) completed a Master's thesis on "Occupational traits in clerical work: a study of employed and unemployed women in Montreal" in 1935 and Kenneth Norris (n. d.) completed a Ph.D thesis titled "The permanence of school learning as indicated by a study of unemployed men" in 1939.

5. c. Psychology in Medicine after Bridges (1934-1939)

There seems to have been little cooperation between the Department of Psychology under Tait and the Faculty of Medicine at McGill after Bridges' departure in 1935. In 1937, when Penfield needed qualified psychologists to work with him at the Montreal Neurological Institute, rather than have Tait or Kellogg (or their students) assist, he appointed psychologists working in the United States (Donald Hebb and Molly Harrower). Indeed, no substantial association of psychology with psychiatry occurred at McGill until 1945 when Robert B. Malmo (1912-2002) was appointed (Ferguson, 1982).

For the most part the Department of Abnormal Psychology at McGill which had developed in the Faculty of Medicine ceased to exist without Bridges. There also seems to have been a lack of effort from the Faculty of Medicine itself in terms of initiatives that would bring psychologists and psychiatrists together at McGill. Part of this might be due to the comparatively smaller number of psychologists compared to the University of Toronto, especially prior to 1924 (Myers, 1982). Psychiatry was surprisingly slow to develop as its own speciality at McGill; the first practicing psychiatrist in Montreal wasn't established until the 1920s, whereas Toronto had a Department of Psychiatry in 1908 and a retired Dean of Medicine as influential proponents of mental hygiene (i.e., C. K. Clarke) at the helm of the Psychological Laboratory (Myers, 1982). This kind of direct cooperation between medicine and psychology did not develop at McGill.

Chapter One Conclusions

From the turn of the nineteenth century through the first decades of the twentieth century, academic psychology at McGill developed from a few mental philosophers in the early twentieth century into its own small department by 1924. This shift was in large part due to the growth of interest in psychological knowledge and the negotiation of a separate disciplinary identity from philosophy and psychiatry. At McGill, these changes came about through specific developments, such as increased enrolment in undergraduate psychology courses in the Department of Philosophy, the establishment of the Psychological Laboratory—despite a lack of support from the university—and increased opportunities for psychologists outside the academy (e.g., the mental hygiene movement). Some historical disciplinary accounts of psychology are firmly positioned in the university system (e.g., Dobson, 1995; Wright & Myers, 1982); this chapter demonstrates the role of movements such as mental hygiene as a contributor to the growth of

psychology in Canada before the war. Indeed, with the encouragement of members of the Canadian National Committee, psychologists were invited to participate as scientific experts in new sites for the production of psychological knowledge: for example, the school and the clinic (Pols, 2002; Danziger, 1990).

William Tait, among a small handful of academic psychologists in Montreal in the 1920s, was enthusiastic about the new possibilities for psychologists that were being opened up by the mental hygiene movement at this time. This is demonstrated by his work in the development and administration of surveys, the teaching of psychological subjects to medical students and staff, the publication of articles supportive of mental hygiene and his affiliation as a member of the Executive Committee. The benefits of partnership between psychologists and psychiatrists under the auspices of mental hygiene were multiple and officers of the Rockefeller philanthropies who funded both groups at Toronto and McGill attributed the involvement of universities (such as McGill and Toronto) to the relative success of the Canadian Committee compared to its American counterpart (Pols, 1999). However, while psychology at Toronto developed in close relation with the medical school and the National Committee (Pols, 1999), the same cooperative relationship was not achieved by Martin and Hincks in Montreal.

I've outlined a number of reasons for the lack of cooperation between psychologists and medicine at McGill. These included: 1) Tait's generally abrasive personality and conflict with key members of the Committee, 2) undeveloped collaboration between psychologists and medicine due in part to the strategic division of research and training in mental hygiene between Toronto and Montreal, and finally, 3) lack of strong leadership in psychology. There was no one able to successfully bridge the divides that existed at the time between differing versions of disciplinary identity at McGill; there was no one quite like Bott, Blatz, or Clarke who could articulate these possibilities and mobilize the necessary structures until after World War II. Indeed, the lack of institutional cooperation between psychology and medicine outside of National Committee sponsored initiatives underscores the importance of the mental hygiene movement in spearheading early collaborations. At the University of Toronto the association with psychiatry in the medical school proved fortuitous for the psychologists, first, in achieving departmental independence and, second, in acquiring funding for psychological research (Pols, 1999). Without this kind of institutional cooperation, McGill psychologists, by contrast, struggled for space, staff, and research funding throughout the interwar period.

Chapter Two: Psychology, McGill University, and the Second World War

World War II had a decisive role in the development of academic psychology in the twentieth century (Capshew, 1999a; Isaac, 2007). Compared to the role of American psychologists, little has been written about the status of Canadian psychology and its role in World War II (Ferguson, 1992; Wright, 1974). Montreal psychologists, especially those from McGill University, played an important role in the organization of academic and professional psychology in Canada. This chapter synthesizes existing studies of Cold War behavioral psychology and expands on how World War II changed the Department of Psychology at McGill, and with it, the whole of academic psychology in Canada.

It is important to examine how World War II affected academic psychology in Canada in ways both similar and dissimilar to the United States. The global effects of the war constituted a specific context from which Canadian psychology emerged. Providing this context helps situate the conditions (such as the creation of national associations and Donald O. Hebb's appointment to head McGill's Psychology Department) contributed to the emergence of new arrangements between psychology and medicine at McGill (e.g., the nascent field of cognitive neuropsychology).

First, it is important to emphasize that academic psychology in Canada developed much slower than it had in the United States. Before World War II there were only a handful of psychologists associated with Canadian universities (Myers & Wright, 1982). Consequently, much that has been written about the history of psychology and medicine in North America has focused exclusively on the United States and on the relationship between psychology and psychiatry (e.g., Buchanan, 2003; Burnham, 1988). This chapter and the next take a fresh look at this history from the standpoint of Canadian psychology and in terms of the broader relationship with physiology and medicine. Rather than focusing on the development of clinical psychology in relation to psychiatry (e.g., Benjamin, 2005; Buchanan, 1997; 2003), these chapters will examine how behavioral sciences associated with medicine, such as neurology and physiology, became enmeshed in various institutional configurations related to identity/professionalization and to research funding of academic psychology in the postwar era.

1. Canadian Psychology and World War II

Like in the United States, the war had a considerable effect on the development and trajectory of psychology in Canada (e.g., Ferguson, 1992). Canadian psychologists, like other social and physical scientists, participated actively in the war effort. Indeed, one of the most important roles for Canadian psychologists during the war was to develop and administer intelligence tests, skills tests, and tests of emotional stability to help select officers and to coordinate the training and assessment of soldiers in various lines of military work (Ferguson, 1982; Wright, 1974).

The psychological cost of the war was tremendous, as was the resulting demand for trained experts in realms associated with mental health (Wright, 1969; Pickren, 2007). Indeed, it was the immediate postwar period that clinical training in psychology departments was first introduced in Canada, government funds for basic research were created, and the conditions for a new way of thinking about the relationship between mind and behavior (i.e., cognitive neuropsychology) developed in Canada at McGill University.

1. a. McGill psychologists during WWII

Similar to the United States, the interwar period was an important episode for the self-fashioning of academic psychology in Canada (Capshew, 1999a). Given their comparative size, the cities of Toronto and Montreal were the primary sites for the development of psychology in Canada (Wright & Myers, 1982). The departments at the University of Toronto under E. A. Bott and at McGill under W. D. Tait were forerunners of modern psychology in Canada and provided models upon which smaller universities later developed. Outside affiliation with either of these two universities there was little academic work for psychologists in Canada (Ferguson, 1982); it wasn't until after the war that most psychology departments formally separated from their philosophical roots (Wright & Myers, 1982).

The psychology department received little support from the university for equipment, travel funds, or administrative staff. Canadian psychologists came to rely heavily on financial and organizational support from the United States. Prior to 1939, the organization of Canadian psychology was reliant upon American leadership and disciplinary infrastructure, such as associations, conferences, and journals (Wright, 1974). What little research was conducted was predominantly related to schools and industry: mental measurement for education, personnel

selection, and industry management (Danziger, 1990; Rose, 1988). In addition to the McGill Social Science Research Project, described in the previous chapter, the most significant contribution of McGill psychologists to the war effort was a questionnaire developed for use in the Canadian Armed Services (Ferguson, 1982).

This questionnaire was developed as one of the first tasks of the newly established Canadian Psychological Association (CPA). While most psychologists in Canada were members of the American Psychological Association (APA), it had been widely recognized that Canadian psychologists (especially those at the University of Toronto) would be better served by a Canadian Association. In the spring of 1938 a survey of Canadian academic psychologists was circulated (likely by Bott, who had chaired the first meeting; Dzinas, 2000). On the question of forming a Canadian Association, it was reported that "the response was very favourable," but the matter warranted further discussion (National Archives, 1938). The sequence of events that led to the decision to form the CPA began in 1938 (Dzinas, 2000); as international tension increased leading to World War II, Canadian psychologists expressed concern that, in the event Great Britain and Canada went to war, the APA would be unwilling or unable to provide the kind of national leadership required to safeguard Canadian interests. The CPA was established to ensure Canadian psychologists would play an appropriate role in the coming war effort (Wright, 1974; see also Dzinas, forthcoming). The founders of the CPA were well-aware of the success earned by psychologists in the United States Armed Services after WWI and sought to emulate the disciplinary growth that followed, particularly in the realm of mental measurement and intelligence testing (Capshew, 1999; Kevles, 1968; Samelson, 1977; Sokal, 1987; Carson, 1993).

The efforts of Canadian psychologists were coordinated through the work of the CPA whose membership and leadership reflected existing academic departments. The University of Toronto and McGill University made up the greatest proportion of the association's initial membership, with E. A. Bott of Toronto, George Humphrey of Queen's, and Roy Liddy of Western taking on various leadership roles. When Canada joined the war in September 1939, the Department of Psychology at McGill comprised William Tait, Chester Kellogg, N. W. Morton, and Alfred B. Udow. Each of these men had experience with mental testing on behalf of various organizations. For example, Tait and Kellogg had developed educational assessments while Morton and Udow helped update intelligence tests for personnel selection.

On October 2, 1939, less than a month after war was declared, the National Research Council (NRC) convened a meeting in Ottawa on how psychologists might best serve the Canadian government at war. This meeting was attended by representatives of the three armed services and heads of three departments of psychology—Bott (Toronto), Humphrey (Queen's) and Tait (McGill)—as representatives of the CPA. At this meeting the three psychologists were able to convince members of the government that their expertise, in the areas of mental appraisal, personnel selection and work training, was too valuable to the war effort to ignore (Ault, 1948). This decision led to the proposal of the Test Construction Committee, charged with the development of tests with wide application for a variety of selection purposes within the Canadian Armed Services (Wright, 1974) and Royal Canadian Air Force (English, 1992). While chaired by Roy Liddy of Western, Montreal-based psychologists were influential on this committee. Members included Chester Kellogg, N. W. Morton, and Father Noël Mailloux as well as young psychologists George Ferguson and Donald Hebb, among others at different times (Ferguson, 1992). The main product of the Test Construction Committee was the development of the Revised Examination M, what would become one of the most utilized mental tests during World War II.

With the assistance of private funds from the London Life and the Sun Life Insurance Companies, Kellogg had been working to revise the US Army Beta Examination that Robert Yerkes had developed for soldier selection during World War I (Wright, 1974). Much of the work involved in the preparation of this test was done by his graduate student, N. W. Morton. Morton was the Department of Psychology's first doctoral student (Ferguson, 1982; Morton, 1933). The Psychological Corporation published Kellogg and Morton's first revision in 1935, which they had originally wanted to call the 'McGill University Revision' but Principal Arthur Currie refused to allow the use of the university's name, saying the psychologists were "unjustified in calling printed intelligence tests 'McGill University Tests'" (Matthews, Dec 11, 1931; Currie, Jul 10, 1933). Instead these tests would come to be known as the Revised Examination M - where, according to Morton (1970) - this "M" stood for Montreal, not McGill.

After the test's initial development, funding for the CPA's activities remained uncertain. In 1941 a grant from the NRC was finally obtained to continue to expand the application and research related to mental testing (Humphrey, 1942). During the next few years the Test Research Committee constructed three forms: a verbal test for officer candidates, a pattern

perception test for general use, and a screening test for navy recruits (Humphrey, 1944). The Revised Exam M became one of the most widely used psychological tests ever developed in Canada and was administered to over one million Canadians, both during and after the war (Ferguson, 1992).

McGill psychologists during this time were also involved in defense research related to attitudes, morale, and human factor research. In 1941, after the development of the Revised Exam, McGill psychologists N. W. Morton, J. S. A. Bois, and E. C. Webster joined the newly formed Directorate of Personnel Selection in Ottawa. Under the directorship of Brock Chisholm (1896-1971), this initiative spawned the Directorate of Special Services, which was charged with addressing psychological matters, such as the problem of morale among soldiers and assessing the attitudes of Canadians towards the war. The Directorate was led by Bois and Webster (both graduates of and instructors at McGill). In early 1944, Morton left Ottawa to assume command of the personnel selection group at the Canadian Military Headquarters in London, England. While McGill psychologists worked primarily in personnel selection and training, other psychologists, like those from Toronto (e.g., J. D. Ketchum and J. A. Irving), joined the Wartime Information Board, an organization concerned with public opinion during the war (e.g., Irving, 1943) and would come to influence the practice of systemic public-opinion polling in Canada (Ferguson, 1992). The need for women workers during the war led to a corresponding need for quality day care and Canadian psychologists, such as those involved with Institute of Child Study like William Blatz (1895-1964) and Dorothy Millichamp (1908-2001), were involved with the establishment of wartime day nurseries in Great Britain and Canada (Wright, 1974; Wright, 1992).

1. b. Psychology in the immediate postwar period

The Department of Psychology at McGill offered numerous courses throughout the 1930s, it is therefore little surprise that the activities of this small department consisted mainly of fulfilling teaching obligations, both in the department and outside (Ferguson, 1982). Similar to other small departments, there was little in the way of experimental research, due to a lack of resources (time and funding), if not also interest and expertise (neither Tait nor Kellogg were trained experimentalists). There was, as a result, delayed development for Canadian psychology, compared to the growth of academic psychology and research facilities in the United States

(Capshew, 1999) and Europe. For example, at McGill between 1924 and 1946, while almost forty Master's degrees were granted, only four PhDs were awarded (Ferguson, 1982).

The CPA membership at this time indicate there were fewer than forty academic and perhaps a dozen non-academic psychologists in the whole of Canada (Wright, 1974). Most departments of psychology in Canada were not established until the late fifties and sixties (Wright, 1969; Myers & Wright, 1982). Prior to the development of these departments—in the immediate years following the war—was a critical period for the formation of Canadian psychology as the CPA asserted a leadership role over the future of the discipline through the reorganization of research funding in Canada (see Chapter 4). Among pressing concerns related to the future of the discipline, the CPA was particularly concerned with 1) the establishment of funding for research and training, 2) the development of standards to assure quality psychological training, and, 3) the balance of needs and interests between professional/applied psychologists and experimental/basic psychologists (see Wright, 1969; 1974).

The delayed development of psychology in Canada was due in part to the lack of support from the Canadian government. This changed dramatically after the war. The early work of the CPA throughout the war years had demonstrated the value of psychologists and their capacity to contribute to problems facing Canadian society at large. The grant awarded to the CPA by the NRC in 1941 for the development of the Revised Exam M was the first time a Canadian federal agency had provided funds for the support of psychological research (Wright, 1974). This funding represented the beginning of a new relationship between psychologists and government based on mutual interests and recognition for the capacity of psychological management (Rose, 1988; Danziger, 1990); thus began a new era for federally-supported Canadian psychology. Psychology had effectively been “sold” to the Canadian public and its services were now demanded by government, industry, health and education (Wright, 1974; Danziger, 1990).

This newfound interest in the skills and expertise of psychologists created both unprecedented numbers of individuals trained in psychological techniques and an unprecedented demand for psychological expertise. Psychologists returning from war were inspired by recent achievements and eager to pass on, not only the skills they had developed, but also their enthusiasm for applied work. These conditions led to increased tension between Canadian psychologists who identified as psychological professionals (those who offered their services as applied scientist-practitioners) and those who identified as experimental psychologists (those

who sought recognition as natural scientists with interests in basic research). Thus, the immediate postwar period in Canada was characterized by disciplinary identity-crisis stemming from the inherent tensions arising from psychology as both an experimental science and an applied science (see Chapter 4).

World War II catalyzed the development of modern psychology in Canada. As a result of the war, Canadian psychologists established themselves independently of their American counterparts upon whom they had heavily relied prior to the war. With the founding of the CPA in 1938-1939 and the activities of this organization throughout the war years, academic and professional psychologists finally began to catch up with the disciplinary progress of their American and European colleagues. One of the most important events which laid the foundation for the resumption of academic activities after the war was the securing of funds for the support of basic research (Wright & Myers, 1982). For the CPA, it was important to establish a dependable financial base for postwar research and training of psychologists. Thus, as the war drew to a close, members of the CPA (e.g., Bott, Humphrey, and Line) initiated discussions with the NRC about how to obtain permanent status for psychology in the Council and how to arrange its eligibility for a share of the NRC's funds. This led to the creation of the Research Planning Committee in 1946. It was charged with making a case for the support of psychological research in terms of its national significance (Wright, 1974). As such, the Departments of Labour, Defence, Health, Veteran's Affairs, Secretary of State, Civil Service Commission and others were invited to make official requests for psychological research and to provide an estimate of the funds required. Morton served as liaison officer for the CPA in its negotiations with government departments and his assistance played a large part in bringing to a successful conclusion the 1947-48 negotiations which resulted in the establishment of federal grants for psychological research from the NRC as well as from the Defence Research Board (DRB) and the Ministry of Health and Welfare. The newly established Defence Research Board (1947-1977) was created within the Department of National Defence to address practical concerns related to postwar military research and took over coordinating defence research from the NRC (Turner, 2012).

These newly established sources of federal funding, however, were not universally well-received. When the enthusiastic responses to these invitations were reported at the annual meeting they were met with protest from the CPA membership (Bott, 1948). This outrage,

according to Bott, was due to the concern psychologists had that they would be forced to engage in a kind of contract-based research arrangement in order to obtain funds. Furthermore, the demand for psychological expertise from these government agencies, in the form of teaching, training, and research, outstripped existing available manpower in Canada given postwar arrangements (Wright, 1974; Ault, 1948).

After the war, the Department of Psychology at McGill was the first to resume basic psychological research (Wright, 1974). The department at the University of Toronto, for a full decade after the war, changed little in orientation; the department remained applied and focused primarily on the training of clinical students on the scientist-practitioner model (Myers & Wright, 1982). This was due in part to the fact that much of the faculty from before the war remained and little effort was expended to recruit or maintain new faculty (despite soaring enrolment after the war). The Department of Psychology at McGill, on the other hand, developed much differently.

The loss of key members, like Morton and Bois, devastated an already struggling psychology department at McGill. By the early 1940s both Tait and Kellogg were growing old and in increasingly poor health. Frances Alexander (n. d.), a clinical psychologist, was appointed sessional lecturer in 1942 and Assistant Professor in 1945 and, as Tait's health continued to deteriorate, M. C. DeJersey was also appointed as a sessional lecturer in 1944, and as a lecturer in 1945. Tait died in 1944 after 35 years at McGill and Kellogg became acting chairman of the department until former McGill graduate Robert B. MacLeod arrived in 1947.

2. The Reconstruction: Robert MacLeod and Postwar Psychology

The situation at McGill after the war was dire. With Canada's provision of educational credits for veterans, university enrollment soared in the late 1940s (Frost, 1984). The Department of Psychology at McGill was not prepared for this sudden influx of students. This situation was reflected in a letter D. L. Thomson, the Dean of the Faculty of Graduate Studies, wrote to Principal James. Following reports Thomson had received from students, he expressed to James concern about the quality of graduate instruction and the direction (or lack) of research within the department (Thomson, Dec 11, 1945). Only a few months later Dean Thomson followed this letter with another in which he described the lack of necessary space, equipment, and staff as “extremely serious” (Thomson, Feb 20, 1946). He advised Principal James that if the university

was unable to remedy this situation quickly they would have no choice but to "restrict severely" the number of students entering the graduate program (Thomson, Feb 20, 1946).

In the first meeting of the Selection Committee following the death of Tait in 1945, the troubling situation in the department was discussed (McGill Selection Committee, Jun 18, 1946). At this meeting Chairmen and Deans relevant to the Department of Psychology agreed that two new full-time faculty would greatly ameliorate the solution. The names of acceptable candidates, who would first and foremost have "extremely great capabilities as an experimental psychologist" as well as being "a good teacher on the undergraduate level" (McGill Selection Committee, Jun 18, 1946), were considered. There was explicit support among members of the committee for a scientific psychology at McGill, able to cooperate closely with existing departments in physiology (Hoff) and in psychiatry (Cameron): both psychiatrist D. E. Cameron (1901-1967) and physiologist H. E. Hoff (1907- 1987) emphasized that "[at least] one of the men should be solely a scientist" (McGill Selection Committee, Jun 18, 1946). Everyone except Cameron agreed that N. W. Morton, given his relationship with McGill and Canadian government, should be made chairman of the department.

Despite the poor condition of the department and the demand for additional faculty to be added, two and a half years passed with Kellogg as acting chairman before concrete steps were taken by the university to resolve the situation by hiring a new chairperson tasked with appointing new full-time staff. With Tait no longer in charge, Morton having remained in government military service, and Kellogg being quite ill by this time, the "old guard" of McGill psychology was coming to an end all at once and "new blood" was introduced for the first time since its inception. Minutes from selection committee meetings between 1945-1947 indicate that Hoff and Cameron were instrumental in decision making related to the rebuilding of the Department of Psychology (McGill Selection Committee, Jun 18, 1946).

The long wait for a replacement Chairman of psychology was likely due to the fact that faculty and staff were in short supply after the war. Besides MacLeod and Kellogg, the calendar listed two part-time assistant professors: E. O. Webster (1909-1989), who had completed his Ph.D. with Kellogg in 1936 and was appointed to the department after returning from the war, and Frances Alexander (n. d.), a clinical psychologist. There is unfortunately little known about Alexander or her clinical practice, though she is described by MacLeod as having not been

interested in scientific psychology (Myers, 1974). Listed, but technically on leave at the time, was N. W. Morton.

Morton had been a key member of the department prior to his transition into government and military service in 1941. Principal F. Cyril James (1903-1973) had anticipated Morton's return as Chairman of the Department of Psychology at McGill after the war. This did not go as planned. Correspondences between McGill and the Department of Defense indicate that the university petitioned the release of Morton from service numerous times but the government was reluctant (e.g., Ross, Jun 14, 1945). Indeed, Morton received an additional leave of absence in 1946-1949 to do "special work for the Indian Government" (McGill Selection Committee, Feb 7, 1946). This put McGill psychology in a difficult position. It was decided in early 1946 at a Selection Committee meeting that the university could no longer wait for Morton and would have to go ahead because, as Dean Cyrus Macmillan (1882-1953) explained, the department at McGill is "in urgent need of re-organization and of additional instructors" (Macmillan, Sept 19, 1945). A list of prospective individuals for the Chairmanship of the Department of Psychology were considered. The list included Carney Landis (1897-1962), William Line (n. d. - 1964), Kenneth W. Spence (1907-1967), and B. F. Skinner (1904-1990), among others.

It is likely that, had someone else like Landis or Skinner been appointed Chairman of the department at McGill, the development of psychology in Canada would have taken a very different course. Rather than McGill, Skinner became chair of psychology at Indiana University in 1946 where he remained for a year before taking a position at Harvard University. It is interesting to note that Donald O. Hebb (1904-1985), who would later shape the department at McGill, had initially been offered the Harvard position by E. G. Boring (1886-1968) in 1947 but had turned it down (Devonis, 2012). Canadian psychology remained fairly separate from the neobehaviorism of Skinner and Hull that swept the United States. This allowed for a considerably different kind of psychology to emerge in the postwar era (see Chapter Five). The names put forward to lead psychology at McGill suggested that the university (i.e., the members of the Selection Committee) was interested specifically in recruiting an established and proven experimental (and perhaps behavioral) psychologist – likely to complement the applied emphasis of Kellogg and Webster, and to establish McGill's department as oriented towards basic research (as opposed to the department at the University of Toronto which was applied in orientation).

While MacLeod was ultimately chosen, Morton kept up relations and assisted future chairpersons to secure government funding throughout the 1940s and 1950s (Myers, 1974). Even after MacLeod had assumed his position as Chairman, MacLeod tried to lure Morton back to McGill from his position at the Defense Research Board but was unable to offer a competitive salary (Myers, 1974). Morton remained with the Canadian government, conducted military operational research (1952; 1956), collaborated with the Indian government (1946-1949), and in 1952 became President of the CPA.

McGill psychologists (e.g., Morton, Webster, Tait, and Kellogg) had established their institution as a beacon of disciplinary advances in psychology with the formation of the CPA during the war. With the deterioration of the department, the university recognized that if Canada was to have a place in the coming “age of system” (Heyck, 2015), it would require significant re-investment in the social sciences (Shore, 1987; Solovey, 2004). This imparted particular importance to the role of the new chairperson of psychology; they would be responsible for crafting the next stage of psychology in Canada. In order to sustain the reputation McGill University hoped to project as an internationally-acclaimed institution, the Department of Psychology would have to undergo substantial improvements. To attract the best students, the department needed to have instructors capable of training these students in modern psychological methods. The university needed someone recognized by his peers as 'forward-looking.' Robert B. MacLeod (1907-1972) was precisely that kind of person; he was widely recognized within his discipline as an organizational man and drew broadly on multiple theoretical traditions, able to bridge internal intellectual divides between North American and European psychological traditions (Wertheimer, 1973).

In 1946, Principal James was able to persuade MacLeod, who had been conducting war service with the Office of Strategic Service in both the US and Europe, to come to McGill. MacLeod was among several candidates discussed. In a letter to Principal James during the war Tait suggests several McGill graduates (e.g., Hebb, Spence, and MacLeod) as prospective replacements for him, "when the time comes" (Tait, Aug 17, 1942). Since the department had been “badly crushed by the war and had to be built up again” (MacLeod quoted in Myers, 1974, p. 109), MacLeod's background was likely seen by the committee as uniquely suited to the task of taking Canadian psychology into the future. MacLeod had traveled and studied extensively in Europe, spoke many languages, was adept in a wide range of psychological subjects, and

understood well the divisions and tensions that existed at the time between various traditions and schools of psychology in Europe and North America (Wertheimer, 1973). When MacLeod accepted the chairmanship in 1946, Dean Thomson expressed relief that he would be "re-organizing graduate work in Psychology here" and further admitted that he could not disguise "the dismay with which I saw more and more graduate students registering in this subject in 1945, when I knew that the staff could not possibly give adequate supervision in research to more than a few" (Thomson, Sept 13, 1946).

MacLeod had done his undergraduate and M.A. degrees at McGill with Kellogg. Indeed it was Kellogg, MacLeod later reported, who had sparked his interest in the potential of applied psychology and especially mental testing (Myers, 1974). MacLeod worked as Kellogg's assistant and completed his M.A. in 1927. His Master's thesis was related to the controversy over instinct at the time and the position he held ended up being similar to that of E. C. Tolman (1886-1959) regarding cognitive maps (Myers, 1974).

Upon graduation MacLeod was awarded a Moyse Travelling Scholarship. This was awarded to him as a 'science scholarship,' which Tait and Kellogg supposedly took as "proof that psychology was a science" (Myers, 1974, p. 108). He spent two years in Germany, studying principally at the University of Berlin (1928-1929). His experiences at the Psychological Institute in Berlin brought him in touch with Max Wertheimer (1880-1943), David Krech (Krechevsky, 1909-1977), and Kurt Lewin (1890-1947), who greatly influenced his subsequent thinking and interests (e.g., see MacLeod, 1949; Myers, 1974). He was an important contributor to the critical reception and uptake of Gestalt psychology and phenomenology to North America in the 1930s and 1940s (Wertheimer, 1973). He believed training in phenomenology was beneficial to experimental psychology (MacLeod, 1964) and helped Wolfgang Kohler (1887-1967) and Hans Wallach (1908-1999) immigrate from Berlin and join the department at Swarthmore in Pennsylvania. He promoted what is sometimes called 'the Swarthmore-Berkeley axis,' which was the mutual exchange between those two institutions of ideas, of students, and of jobs (Cornell University Faculty Memorial Statement, 1972). In 1937, Krech described MacLeod's department at Swarthmore as "a unique and altogether improbable site on the American psychological scene. On the campus of a small undergraduate Quaker college, Robbie MacLeod had assembled an outstanding faculty, had established the world headquarters for Gestalt psychology and had created an altogether excellent intellectual, scientific, and teaching

enterprise in psychology" (Krech, 1973). MacLeod was historically-oriented and known for his 'long-view' contextual approach to understanding what he called "the persistent problems of psychology" (MacLeod, 1975); Bott reported that MacLeod had "an unusual gift of formulating fields of interest and showing how they stand in perspective" (Bott, 1948, p. 13).

MacLeod had held a position at Swarthmore since 1933 when Principal James approached him about the chairmanship at McGill in 1946. It was D. L. Thomson (trained biochemist and Dean of Graduate Studies and Research at McGill) who had initially suggested MacLeod at a Selection Committee meeting shortly after Tait's death in 1945 (McGill Selection Committee, Jun 18, 1946). By this point MacLeod was well-connected and well-respected in a broad number of fields in the psychological community both in North America and Europe (Myers, 1974; Wertheimer, 1973). After MacLeod had completed his Master's and left for Germany at the age of twenty, he chose to remain in contact with Tait and Kellogg throughout his career (Ferguson, 1982). Psychology at McGill, its development and potential, was something MacLeod valued. How James convinced MacLeod to switch universities is not entirely clear, though MacLeod later mentioned his loyalty to McGill and salary were both factors (Myers, 1974). What is known is that the Principal of Swarthmore was not pleased; he even sent a rather brusque letter to James regarding MacLeod's appointment with the opening line: "I don't like you at all" (Nason, May 20, 1946). The Principal of Swarthmore evidently recognized a major loss with MacLeod's decision to lead the department at McGill, such was the value of man capable of bridging the gap between North American and European psychologies at the time.

2. a. MacLeod's arrival at McGill

In 1947, MacLeod became McGill's first new chairperson of psychology since the department's creation in 1924. He found upon his arrival that he was lacking the staff and resources necessary for the development of a modern psychology department. MacLeod had spent nearly a decade building the department at Swarthmore and had made it into a world-class institution; he promised Principal James he would do the same at McGill. This time, as MacLeod later explained, it would have to be from the ground-up: "When I got there it was obviously a formidable job [to rebuild the department] because there was almost nothing left. Tait had died. Kellogg was still there but in very poor health […] We couldn't go down any further" (MacLeod quoted in Myers, 1974, p. 110).

While Principal James had persuaded MacLeod to come to McGill on the basis of an assurance he would be permitted to rehabilitate the department and to develop its graduate work (see MacLeod, Oct 5, 1946), these attempts were occasionally met with resistance. Despite there being a new chairman at the helm of the department, relations between the university and the psychology department do not seem to have improved greatly. MacLeod described how Cyrus MacMillan, the Dean of the Faculty of Arts & Science (who was also Chairman of the English Department), was "impossible to get along with" and had failed to provide the financial support Macleod required (Myers, 1974). This is corroborated by correspondence indicating frequent conflict between the two men over the course of 1947. Although MacMillan retired later that year, his replacement A. H. S. Gillson (1889-1954), was "equally inefficient" though more approachable (Myers, 1974, p. 110). Correspondence indicates these initial challenges revolved mainly around material conditions of the department, such as the urgent need for additional space and staff. With the full promotion of Kellogg—who had remained at a lower position at the university for the last twenty years—MacLeod set about addressing the problem of an understaffed department.

While evident to the chairman, the lack of eminent professors in the department of psychology at McGill was a concern he spelled out for Dean MacMillan: "I am still convinced that McGill could develop a Department worthy of the university and able to compare favorably with the best on this continent [however] before we can rise above mediocrity we must improve the calibre of our teaching staff" because effective teaching and research "does not exist at present" (MacLeod, Oct 5, 1946). Eventually the university agreed to appoint two additional full-time staff in the department. While seemingly meagre (the university was undergoing significant growth during this time in all other departments; Frost, 1984), this action represents the greatest support shown by the university since the establishment of the department in 1924. These positions, which were recognized as leadership positions in the new department and for psychology in Canada, were designed to fill gaps in existing academic representation; they were part of the postwar planning of academic psychology in Canada (Wright, 1969). How then did MacLeod and the Selection Committee come to decide on who should fill these influential positions?

2. b. Planning psychology at McGill and for Canada

MacLeod believed strongly in the organization of professional and academic psychology in Canada and demonstrated this through his dedication to the CPA and to the building of both Swarthmore and then McGill (and subsequently Cornell). In an article in the *Canadian Journal of Psychology* MacLeod (1947) shared his vision of the possible future of psychological research in Canada which was emerging from his involvement with the Canadian Psychological Association's Research Planning Committee (1946-1948). This article is not a detailed plan or a considered argument but rather an outline of identified problems and potential areas of future research (MacLeod, 1947).

The unique conditions of Canada and of its cities (specifically Montreal) are used by MacLeod to argue for the development of a flavour of psychology focused on dealing with "psychological problems which must be defined in Canadian terms and attacked with the human and material resources of Canada" (MacLeod, 1947, p. 178). These included broad considerations of the "academic and the practical approaches" (p. 178) of psychology in areas we would now recognize as military, industrial, political, and social and cultural psychology, but also on specific topics like camouflage, propaganda, and prisoner interrogations (areas in which MacLeod felt the potential contributions of psychologists had not yet been realized). The only caution MacLeod suggested was in the domain of health and therapeutics where he warned that "a premature application of our present meagre knowledge may result in the 'freezing' of clinical psychology on the level of a technical service ancillary to the medical profession [...] [Moreover,] clinical psychologists may not merely tie themselves to the coat-tails of the medical profession but also absorb into their thinking the biases of the clinical practitioner […] The deviant population of the psychological clinic should not be allowed to define the conceptual framework within which normal people are understood" (MacLeod, 1947, p. 190). This consideration of the "premature professionalism" (MacLeod, 1955) would be a central theme in debates regarding the training and funding of psychologists (see Chapter Four).

In addition to framing the problems, the conditions of Canada are often described by psychologists of this period as providing unique opportunities for psychological adventure. For example, MacLeod held that Canada's small population presented the possibility of a balanced, scalable research programme and its history of national unity in spite of cultural diversity as potential "social laboratory" for the investigation of group differences (MacLeod, 1947, p. 185).

Similar arguments had been made in the establishment of Montreal and Canada as key sites for the mental hygiene movement (see Chapter One): “McGill in the field of the social sciences could utilize Canada as its laboratory, and that this laboratory is unrivaled because the country is young, is in the process of active development and is less crystallized by traditions than older civilizations” (Hincks, Mar 4, 1930).

Part of this vision was the development of research planning on a national scale as a central function of the CPA (MacLeod, 1947). Planning of this kind would assure every branch of psychological research is represented somewhere in Canada (in the event of another national emergency like the war). According to MacLeod, when specialized knowledge and skills are required, “the Association should be able to point at once to the place where the equipment is available and to the man who can be requisitioned” (MacLeod, 1947, p. 179). What was therefore needed at McGill was a well-balanced and fully-representative psychology -- which, ironically, explicitly excluded professional and clinical psychology.

With both experimental and applied psychological approaches represented by MacLeod and Webster, respectively, two similar positions needed to be filled. For these, the Selection Committee, guided by MacLeod's recommendations, made offers to Donald O. Hebb (1904-1985) and George A. Ferguson (n. d.) in 1947. Ferguson was hired as assistant professor with a background in psychological measurement and Hebb was hired as full professor in physiological psychology. These men were likely chosen to fulfill specific plans for the department as it developed into an important site for psychology in Canada; Each arrived with strong research potential and their respective skills in statistical analysis and physiological methods expanded the representation of academic psychologists at Canadian research universities.

It is important to note that when Donald Hebb was offered a position at McGill in 1947 he was in his early forties, had completed a significant part of his psychological training in Montreal, and was well-known within the Canadian psychological community for the studies he had conducted on brain-lesioned patients before the war with Wilder Penfield (see Chapter Three). MacLeod needed to “get somebody with some stature” and, as he saw it, Hebb was the logical choice (MacLeod, quoted in Myers, 1974). While MacLeod and Hebb mutually participated in the foundation and development of the CPA in the late 1930s and early 1940s, they had become familiar with each others' work much earlier, through the mutual acquaintance of David Krech (Myers, 1974). Hebb and Krech had both been graduate students under Lashley

in the mid-1930s in Chicago and had formed a close friendship (Hebb, 1982). MacLeod was familiar with Krech's work and recruited him to Swarthmore (twice actually, once before and after the war). Therefore, Hebb's promise as a researcher and teacher was well-known and MacLeod knew he would have to appeal to his Canadian roots and connection to McGill in his offer (Myers, 1974).

Contrary to what is generally known, Hebb had been in conversation with Principal James about coming to McGill for some time prior to officially being hired in 1947. Minutes from a meeting of the Selection Committee in which names were put forth for candidates for a good fit for the job of rebuilding psychology at McGill included Hebb. Given the range of candidates being explored in 1946 (such as behavioral psychologists Spence and Skinner), Hebb's recruitment cannot be presumed as prior endorsement of his physiological training and neurological vision; the university seems to have been keen on an esteemed experimentalist above all else (McGill Selection Committee, Feb 7, 1946). When Hebb was initially considered as chairman he was described simply as, "well known here for his work with Dr. Penfield. Former school principal Verdun. Has done first class research in comparative and physiological psychology and mental tests" (McGill Selection Committee, Feb 7, 1946). This comment affirms Hebb's reputation and interest to the university at this time.

Ferguson, hired as an Assistant Professor in the same year as Hebb, was interested in statistical methods of mental measurement. His background was rural Nova Scotia (like both Tait and Hebb) and his training was in educational psychology. He had studied under Sir Godfrey Thomson at the University of Edinburgh in the 1930s. Before coming to McGill he had had previous connections to members of the department. He had worked with N. W. Morton and J. S. A. Bois in the early 1940s as part of the Directorate of Personnel Selection in Ottawa. After the war, Ferguson became a professional industrial psychologist in Montreal. Ferguson was considering a position as Chief Psychological Consultant in the Montreal Office of Stevenson and Kellogg ("Management Engineers") prior to his recruitment in June 1947. While details about Stevenson and Kellogg are unknown, it is possible this was a private firm created by Chester Kellogg for extra-academic work as a professional psychologist, which might explain his lower position in the department.

The difficulties MacLeod faced upon his arrival at McGill are suggestive of the kinds of challenges regularly met by psychologists by the mid-1940s. Even with the support of Principal

James, MacLeod had to negotiate and plea for necessary support from the university. He continued the struggle for additional offices and laboratory space that Tait had started in the 1920s. Indeed, when MacLeod arrived, he was using the same sparse laboratory space Tait had managed to acquire in the remodeled Molson Wing of the Arts Building nearly twenty years earlier. This consideration by the university was representative of the comparatively low status of psychology among the social and natural sciences. An example of this struggle is highlighted in a letter to Dean Macmillan written shortly after MacLeod's arrival in the fall of 1946 in which MacLeod's request for additional necessary space failed to garner much sympathy. Instead, the Dean pointed out that, contrary to MacLeod's beliefs, "the Department of Psychology at the present time has far more space than any other Department in the building" and concluded with assurances that he is "doing everything possible in a very difficult situation" (Macmillan, Oct 11, 1946). Despite gains made in the recognition and status of psychology during the war, Macmillan's reaction to MacLeod's request is indicative of the comparatively low status and persistent skepticism being leveled against psychology by humanists and scientists alike (Wright, 1969). Having said this, it is possible that the material requirements of a modern experimental psychology were simply difficult for university administration to understand at the time (Macmillan was an English professor and for many administrators, the developments taking place within scientific psychology were likely unknown to them).

Only later in 1947, as part of the postwar expansion of the Faculty of Medicine was MacLeod able to secure additional space in the Donner Building (see below) as it was nearing completion. Upon MacLeod's urging, Principal James chose to dedicate one of its floors to the revitalized department he was in the process of creating. Though the square footage was meagre, MacLeod meticulously designed the laboratory space available to him (Myers, 1974). Unfortunately, MacLeod left for Cornell before the laboratory was completed. Throughout the late 1940s and 1950s the Department of Psychology was housed in about five different locations on or near the McGill campus (Ferguson, 1982). While perhaps this circumstance detracted from a sense of departmental unity, it would also distribute the activities of McGill psychologists across disciplinary boundaries as the department extended into hospitals, clinics, and research laboratories (see Chapter Four). It wasn't until 1965 that all branches of the department were brought together and integrated within a unified department in the newly constructed Stewart Biological Sciences Building where it remains today, adjacent to the Faculty of Medicine in what

was then the new McIntyre Building at McIntyre Park. The physical proximity of the Department of Psychology to associated departments, always within short walking distance throughout its history, undoubtedly shaped relations. The Department of Psychiatry, the Montreal Neurological Institute, the Mental Hygiene Institute, and the Faculty of Medicine were all located within a few blocks of one another.

2. c. MacLeod's departure

In late 1947, MacLeod announced his decision to leave McGill and take a position at Cornell University, where he had taught for a few years after completing his Ph.D. (1930-1933). The reasons for his departure are unclear. In a later interview, MacLeod referred to his concern that if he stayed in Canada too long, as a naturalized US citizen, he would lose his citizenship (Myers, 1974). However, correspondence with the university suggests the conditions for the chairman were not ideal at McGill and might have contributed to his early departure. For example, in a letter to the Dean regarding his decision, notes how “it is difficult to maintain a happy staff with the salary scale in existence at McGill” (Gillman, Apr 7, 1948).

Despite challenges, by the end of MacLeod's first year he had managed to relocate and upgrade the laboratory for the first time since its establishment in 1924, and recruited the highly skilled experimentalists and instructors he had hoped would bring the “effective teaching and research” that would attract the best students to enroll in psychology at McGill (MacLeod, Oct 5, 1946). MacLeod spent the summer of 1948 as visiting professor at the University of California, Berkeley and returned to Cornell as Chairman of the Department of Psychology.

MacLeod had been at McGill almost two years before he left to join Cornell to build it up as he had done at McGill (Myers, 1974; Ryan, 1982). An article MacLeod had written on the phenomenological approach in psychology (MacLeod, 1947b) generated much interest in the psychological community. This article made its way to Dean Cornelius de Kiewiet of Cornell who had been looking for a replacement chairman of psychology since Henry P. Weld’s retirement in 1945 (Ryan, 1982). Therefore, following MacLeod's departure, and seemingly without much discussion or debate among the Selection Committee, it was decided that Hebb would take over as Chairman of the Department of Psychology in 1948.

Postwar psychology at McGill under MacLeod, in many ways, was an experiment in national scientific research planning (MacLeod, 1947; Bott, 1948). The specific form and role of Canadian academic psychology was unknown and throughout the coming decade there would be

numerous concerns about its future, which tended to revolve around issues of funding, professional development, and certification (discussed in Chapter Four). The direction of the department's research activities, it's orientation to the discipline, and possibilities available to psychologists in the late twentieth century were largely established under the chairmanship of Hebb in the 1950s. It is therefore towards Hebb's influence and direction of the department over the following decade that the next chapter is focused.

Chapter Two Conclusions

Until the end of WWII, Tait and Kellogg had largely represented psychology at McGill. With the end of the war and Tait's death, a new era began at McGill led by R. B. MacLeod. MacLeod replaced Tait as Chairman of the department in 1946. While he stayed only two years at McGill during this time MacLeod obtained substantial funding for the re-design and development of the departmental laboratories and had persuaded two proven experimentalists, Donald Hebb and George Ferguson, to join the staff. When MacLeod left to become Chairman of Cornell's department, Hebb took over chairmanship of the department at McGill, where he would remain for almost a decade.

MacLeod remarked that, in 1946, the department "had been badly crushed by the war and had to be built up again" (Myers, 1974, p.109). While the war had devastated the department, events going back to Tait's refusal to cooperate with Bridges also contributed to the dire state of the department in relation to the rest of the university, especially medicine. Following World War II, there was impetus for the organization of psychology in Canada. University-affiliated research psychologists shaped psychology in postwar Canada. As one of the few established departments of the time, McGill psychologists played an important role in the direction and form of psychology in Canada.

Chapter Three: Donald Hebb's "Neuro-Psychology" at McGill University

The postwar years comprise a particularly interesting time for the history of psychology in Canada. It was during this period that Canadian psychologists established themselves internationally, through a number of important contributions to scientific psychology such as research in restricted sensory environments, the experiential roots of pain psychology, and the discovery of the brain's reward centre, among others. Among the most notable was the publication of Donald O. Hebb's *The Organization of Behavior: A Neuropsychological Theory* in 1949, which has been described as "a keystone of modern neuroscience" (Milner, 1993) and which "helped clear way for the cognitive revolution" (Kline, 1999). In this chapter, I argue much of this acclaim can be attributed to a new kind of psychology that developed in the Psychology Department at McGill University following World War II.

Central to the development of this 'new kind' of psychology was the appointment of Hebb as chairman of the Department of Psychology in 1947. It was here that his vision, as outlined in *The Organization of Behavior* and subsequent publications throughout the 1950s, was put into practice. Hebb's vision for a neurological psychology has contributed to the dominant form of Canadian research psychology of the postwar era: Hebb became for Canada the champion of basic experimental research in psychology and shaped the new image of psychology at McGill which we have today (Wright & Meyers, 1982).

Hebb plays a central role in this Canadian historical analysis. His particular background and training, described in the first part of this chapter, was both important and unique for a Canadian psychologist at this time. In many ways Hebb was the product of his mentors, rather than his particular time or place. His mentors included the psychobiologist Karl S. Lashley (1890-1958) and the eminent neurosurgeon Wilder Penfield (1891-1976). This academic pedigree, as well as his own experiments on the psychological testing of emotion and intelligence in apes and neurological patients, resulted in him being widely known and well-regarded within academic psychology circles in Canada and the U. S. prior to coming to McGill. During a time of immense societal and cultural change, McGill afforded Hebb the freedom to shape academic psychology at one of Canada's most established universities. Hebb's goals were neither modest nor small in scale; his aim was to "take the leadership of psychology in Canada"

and to deliver “one of the ranking psychology departments on the continent” (Hebb, Feb 22, 1947).

These ambitions were based in part on his confidence that the ideas he had outlined in his book would “make a fundamental change in psychological theory” (Hebb, Mar 31, 1947). The change, Hebb explained, was a return to fundamentals, he planned to “bring [psychological theory] back to an essential concern with t*he physiological-psychological problem*” (Hebb, Mar 31, 1947; emphasis added). Hebb believed McGill was favourably positioned to take a leadership in this regard; Hebb understood the configurations of interdisciplinarity necessary to address fundamental questions. At the time, only two or three schools – Harvard (Lashley), Yale (Beach), and Wisconsin-Madison (Harlow) (Hebb, 1980) – had researchers actively dealing with this problem (and he deemed them as doing so “not too well”). With Hebb's help, “McGill can get a head start easily in this field” (Hebb, Mar 31, 1947). McGill did not become a world leader by emulating then dominant programs of behaviorism common in America (e.g., that of Hull or others), but by deciding to invest resources (e.g., staff, laboratory space, and funding) into an entirely new (neurologically-informed) approach to psychology throughout his chairmanship in the 1950s.

McGill was particularly well-situated to bring about changes in the direction of psychology in North America, Hebb believed, and he had a plan for how this could be done. This plan was based largely on his experiences as a student in Montreal and a vision he was constructing (from his observations of disciplinary shortcomings) about the future of scientific psychology. The integration of his comparative-physiological studies over the last two decades, including an extensive review of 1940s neurophysiology, was described in a manuscript he had brought with him to McGill in 1947. This manuscript, which would become his highly influential book, *The Organization of Behavior: A Neuropsychological Theory* (1949) included an outline of major research problems for the students of the postwar era. Broadly speaking, the kind of psychology Hebb envisioned was based on a thoroughly physiological and neurological account of psychology (Hebb, Mar 31, 1947). The aim of this new psychology would be to more fully account for and better integrate existing psychological phenomena with recent developments in neurophysiology. Neuropsychologist Mortimer Mishkin recalled that, “[psychological theory] paid lip service, in a sense, to neurons. Hebb was the first to have a theory of how the brain worked that involved connections among neurons” (Mishkin, 2001, p. 6).

It was throughout the 1950s that modern academic psychology at McGill, as well as much of Canada, was formed (Wright, 1969; Wright & Myers, 1982). During this time the Department of Psychology at McGill became one of the key sites for this development and soon became recognized as a leading research centre at an international level (Wright & Myers, 1982). The approach pioneered at McGill contributed to some of the most important developments in postwar psychology in North American, such as the influence of early environment on adult intelligence and beginnings of neural network theory. Hebb's work, and those of his students, have inspired and revolutionized our understanding of learning, memory, pain, and motivation. Principles of Hebbian learning are utilized in the development of artificial intelligence and the concepts it inspired provoked what is sometimes described as the "cognitive revolution" in psychology (Gardner, 1985; Baars, 1986).

How did the department transform itself from a marginal contributor into one of the most important centers for research and theory in psychology? To understand the conditions giving rise to these changes it is important to examine the articulation and implementation of the vision that guided this period. MacLeod had improved the department and put many of the important pieces into place (such as additional staff and increased laboratory space), but it was Hebb's vision of psychology, which he moved from theory to practice during his chairmanship of the department from 1948-1958, that most characterized psychology at McGill in the postwar period. The duties of the chairperson are legion and, mirroring the traditional German-style university model, this individual's particular priorities and concerns was expected to set the research agenda and direct the department's activities, explicitly shaping how the field is to be practiced at the institution. While Hebb's scholarly influence can be hard to trace directly. He was known to exclude his own name from projects to which he contributed (apparently, in support of the work of his own faculty and students). However, he was not shy about sharing his concerns for the future of psychological theory and practice and was actively involved with both the American and Canadian Psychological Associations during this period. It is largely from these archival documents—including personal correspondence, annual reports, and committee minutes—that this chapter was developed.

This chapter examines how Hebb understood "the physiological-psychological problem" as an essential concern towards which McGill should get a head start. An understanding of Hebb's approach to this problem provides insight into the conditions that shaped psychology at

McGill and established a framework from which developed the field of neuropsychology in North America (Bruce, 1985). This chapter begins with an examination of how Hebb's particular background and training shaped his understanding of the possibilities for a neurologically-based psychology and how these possibilities became articulated in the vision presented in his hugely popular 1949 book.

1. Hebb, Psychology, and the American Functionalist Tradition at McGill

Hebb originally had literary ambitions and enrolled to be a school teacher at Dalhousie University in the early 1920s. In 1925 he received his B.A. and got an appointment as a high school principal in Nova Scotia. He taught for a year before becoming discouraged and decided to take a year to travel West, working various manual jobs. He returned to Eastern Canada and settled in Montreal, still hoping to get a job teaching. It is not clear why he chose to travel back across the country to Montreal, but an encounter with Freud's works in the late 1920s had encouraged him to take a closer look at the burgeoning field of psychology at Tait's department at McGill University (Hebb, 1980). Given that Hebb's grades had been "worse-than-mediocre," his chances of being accepted to graduate studies were slim. However, a small-town family connection helped secure his future in psychology. W. D. Tait, the chairman of psychology at McGill, had known Hebb's mother in college and both were from the same small town in Nova Scotia (Hebb, 1980). Similar to Hebb's own family background, McGill had emerged from an English-speaking Scottish merchant class (quite separate from the French Catholic Church which dominated much of Québec society). In September 1927, Tait agreed to take Hebb on as a part-time graduate student. This would not be the last time that women in Hebb's family would broker a place for him at McGill (his sister would also come to play a major role in Hebb's return to Montreal and his work with Wilder Penfield a decade later).

Hebb was accepted with the condition that he spend a year familiarizing himself with both William James' *Principles of Psychology* (1890) and George T. Ladd and Robert S. Woodworth's *Elements of Physiological Psychology* (1887) (Hebb, 1980). Tait's assignment of James' *Principles* and Ladd & Woodworth's *Elements* to Hebb prior to formal entry into the program represents the emphasis Tait, and by extension the Department, placed on American functionalist psychology. When Tait assigned these works to Hebb as a neophyte in 1927 he was indicating the works he believed were foundational for McGill psychologists to master.

Functionalism in psychology emerged in the late nineteenth century in response to the stagnation of William Wundt's psychological research program (see Danziger, 1990; Robinson, 2001) and as an attempt to make psychology more appealing within the highly pragmatic American context. Functionalism was generally situated in opposition to the structuralist tradition of Edward B. Titchener (1867-1927), the representative of Wundt's German physiological psychology in America. While structuralists held that an understanding of mental structures was necessary for a scientific psychology, functionalists emphasized the role of consciousness as a product of natural selection and sought to understand its various functions within an evolutionary framework. This contrasted with the sterile laboratory-based analyses of the structure of consciousness (i.e., structuralism). The functionalist position is articulated in what is now widely regarded as the founding document of the school by John Dewey (1896): in sum, the objects of the world are responded to by psychological beings not simply on the basis of their physical qualities but also on the basis of their practical significance to the organism. This functionalist approach to psychology was important for the emphasis in academic psychology of the application of psychology to their discipline outside the scientific laboratory, including to child and developmental psychology, clinical psychology, psychological testing, and industrial/vocational psychology. It was towards these applications that psychology at McGill had developed in the late 1920s (see Chapter One).

Green (2009) notes that functionalist psychology, broadly conceived, effectively dominated the American psychological landscape from the 1890s to the end of World War I. Functionalism in psychology spread widely from the Chicago school (e.g., Dewey, Angell, Mead) to take on different features in different places, all while sharing a Darwinian emphasis on the inherent value of mental testing. There are several indications that the school of functionalism was dominant in the kind of psychology practiced at McGill. These include, 1) Tait's initial involvement with schools and broader commitment to the mental hygiene movement, 2) the lists of research topics being pursued in the department (e.g., Tait, Apr 22, 1930), 3) the kinds of theses being completed in the department, 4) and the textbooks recommended to Hebb. This orientation persisted throughout the 1930s. The mental hygiene movement developed comparatively later in Canada than the United States and involved more psychologists (Richardson, 1989; Pols, 1999). These factors may have contributed to the flourishing of functionalist psychology at the University of Toronto and McGill. When Hebb

approached the chairman of the department in 1927, Tait had largely severed ties with the movement, but continued to define the department's utility in terms of its applied work (see Chapter One): the activities of the Psychology Department were largely focused on the various applications of psychology to Canadian society (e.g., understanding social problems, such as unemployment and mental hygiene).

Characteristic of the psychological tradition that had developed at McGill in the 1920s, Hebb began his career in psychology with close ties to American Functionalism. This is most clearly illustrated in his initial interest in the applications of the new discipline to what he already knew, the field of education (Hebb, 1980). Hebb had had an ambivalent relationship to formal schooling growing up; his parents, both rural physicians from Nova Scotia, had been influenced by the ideas of Maria Montessori (1870-1952); he had taught himself to read before starting his formal schooling when he was eight, and although Hebb initially excelled, he quickly became frustrated and struggled in school (Hebb, 1980). When Hebb later chose to study psychology at McGill, his own experiences as a student and later observations teaching in the mid-1920s, put him at odds with learning theories of the Watsonian behaviorist tradition and its variants (i.e., Samelson, 1981; 1985). While at McGill, Hebb leveraged his training as a teacher in Nova Scotia to assist with the school-based psychological research in which Tait and Kellogg were engaged. The same year Hebb began his studies in psychology and was studying the works of James and Ladd & Woodworth, he was appointed as a high school teacher by the Verdun School Board in Montreal. The following year, when he was admitted to a qualifying year in the department, he was made principal of an elementary school in a working-class district of Montreal (Hebb, 1980). Here he challenged contemporary views of teaching methods (Hebb, 1930). True to the functionalist and applied focus of psychology at the time, Hebb integrated his work as an educator in Montreal with his part-time psychological studies at McGill.

The pursuit of applied psychological work alongside academic activities was commonplace at McGill throughout the interwar period. Canadian psychologists, similar to those in the United States, were keen to demonstrate the value and utility of psychological knowledge and its scientific methods in school-based settings (Wright & Myers, 1982; Pols, 1999). Tait had followed the teachings of his mentor Hugo Münsterberg (1863-1910) and had taken the department in a decidedly applied direction. He and Kellogg regularly served in roles outside academia, in areas of industry, education, and public service. Indeed, the lack of support from the

university—compared to, for example, the University of Toronto—also pushed the department in a necessarily applied direction; the conditions for basic research (especially in the form of equipment and expertise) were simply not available at McGill as they were at the time at the University of Toronto and other American departments. McGill psychology, nevertheless, felt the pressure characteristic of this period and aspired to the status of a scientific discipline. As such, Hebb's connection to the Verdun School Board was likely recognized as an important asset to the development of psychological research at McGill. Throughout the period Hebb studied at McGill (1928-1933) there were a number of degrees awarded for theses on topics related to education and schooling.

Functionalism and its Darwinian approach to the mind found applications besides mental testing. Whereas mental variability was the focus of the testing movement, other strains focused on the adaptation of the organism to its environment. The department under Tait and Kellogg developed slowly and maintained a focus on applied topics, such as vocational guidance, industry, child development, and school psychology. Tait's own publications reflect a broad array of social and political concerns (education, public health, and crime, for example) and Kellogg's research interests were also very broad and included psychological testing (particularly the testing of illiterates), test construction, statistical method, aesthetics, and, in his later years, parapsychology. Both men exerted a considerable influence on the work of the Department of Psychology for more than two decades (Ferguson, 1982).

Given the range of topics pursued by faculty and students in the department, it is fair to assume that neither physiological nor behavioristic psychology managed to get a foothold at McGill prior to WWII. Nor was there a “Freudian bent that held sway in the department at the time,” despite assertions (Feindel & Leblanc, 2016, p. 286). The functionationalist orientation may have been the result of the comparatively lower stature of psychology among the departments at McGill. This meant there was a lack of funds (and interest) for animal research, and a lack of 'fresh blood' (as Tait seems to have preferred, or been forced, to hire internally). The Department of Psychology did not have the resources or experimental-integrative vision to remain abreast of disciplinary developments abroad (for example, the Gestalt movement, discussed later). The department under Tait and Kellogg maintained an orientation characteristic of American functionalism which prioritized applied psychological research and mental testing. This was not unusual; Binet-type intelligence testing was the primary technology for marketing

the practical capabilities of early American psychologists (Brown, 1992; Sokal, 1987; Danziger, 1990). This orientation is perhaps best explained by Tait's own training in the laboratory of Münsterberg in the late 1800s when James was also at Harvard (Hebb, 1980).

Hebb's interest in education aligned with the department's. In a 1930 departmental report, Hebb's work with Kellogg was indicated as, "Psychological Survey of Rushbrooke School with Special Reference to Grading, Mental Hygiene and Curriculum" (Tait, Apr 22, 1930). Hebb never completed this study at the Rushbrooke School. In 1930 he developed a severe hip infection and was bedridden for the better part of the year (Hebb, 1980). His illness, however, proved fortuitous. Hebb described having used this time to become familiar with the works of C. S. Sherrington (1857-1952) and I. Pavlov (1849-1936), in particular, *Lectures on Conditioned Reflexes* (1928). While it is unknown how exactly Hebb became interested in physiological approaches to psychology (perhaps dissatisfaction with the narrowness of the program at McGill under Tait, he does not say), it is worth noting that this orientation towards psychology came about not through experimental practice (e.g., working in a neurological or physiological wet laboratory) but by reading the works of renowned physiologists.

Watson's behaviorist approach, popular at the time, was offered a highly naturalistic account of psychology. Despite institutional separation (see Chapter One), there is evidence that Tait was interested in collaboration with physiologists at McGill. Shortly after the appointment of the eminent physiologist Boris Babkin, Tait urged Dean MacKay to arrange a cross-disciplinary seminar: "As you know," he explained to the Dean, "his work on the conditioned reflex brings him into close contact with the problems of habit and learning in Psychology" (Tait, Mar 2, 1928). Tait was not characteristically uncooperative. He proposed to form a "group of Psychologists, Physiologists and Psychiatrists" to establish weekly inter-departmental colloquia (MacLeod, Mar 6, 1929). While Hebb credits his own curiosity for his interest in physiological approaches, it is entirely possible—since neither Tait nor Kellogg had any experience in this area—that his parents, both physicians, or elder sister (who had trained as a neurochemist) was responsible for encouraging Hebb to pursue studies in Pavlovian conditioning with Babkin in the Department of Physiology. Nevertheless, Tait seemed to be aware of the importance of bringing psychology at McGill into closer contact with the natural sciences.

It is also reasonable to suspect that Hebb had aspired to be at the cutting-edge of his field; which often meant challenging deeply held (behaviorist) assumptions. While Hebb was studying

at McGill, a fellow graduate student, Kenneth Spence (1907-1967), was awarded a fellowship to Yale to complete his Ph.D. with renown American psychobiologist Robert Yerkes (1876-1956). Spence, like MacLeod, was a senior graduate student when Hebb started at McGill in 1928. Spence had worked closely with Hebb when he was an assistant in the department, referring fondly to Hebb as "my first student" (Ferguson, 1980, p. 54). Spence was keenly interested in experimental methods in psychology and studied the limitations of the animal maze as a behavioral measure (Spence, 1930). Spence and Hebb would each go on to help shape postwar psychology through theoretical and experimental contributions to learning theory and motivation. Similar to Spence, Hebb looked to the work of comparative psychobiologists like Yerkes and his contemporaries, such as Karl S. Lashley (1890-1958) at the University of Chicago, who were making waves in biological approaches to psychology in the late 1920s and 1930s (Weidman, 1994; Dewsbury, 2006).

When Hebb recovered from his illness he "devoted the evenings and weekends of a year and a half to being trained in Pavlovian conditioning methods [under Babkin]" (Hebb, 1980, p. 279). Babkin had arranged for Hebb to work with another Russian émigré and former student of Pavlov, Leonid Andreyev (1891–1941) (Frost, 1984): In Hebb's words, "Andreyev was to provide me with a proper training, and I would be a proper psychological and North American representative of conditioning as it should be, Russian style" (Hebb, 1980, p. 283). Mirroring the Russian physiological tradition, the department used a variety of experimental animals, including dogs. This was Hebb's first encounter with experimental animals; the Psychology Department would not develop its own animal facilities until after World War II. Interestingly, Hebb's sister Catherine Hebb would later do her Ph.D. with Babkin in the Department of Physiology at McGill and go on to a successful research career in neurophysiology in England. No doubt Hebb's relationship with his sister played an important role in shaping his orientation towards psychology and it's position as a natural science. Hebb frequently fostered relationships with students from other disciplines. Hebb later reports: "I had clearer ideas about natural science than I might otherwise, partly as a result of long discussions with graduate students in chemistry at McGill [in the late 1920s and early 1930s]" (Hebb, 1980, p.274).

Hebb's Master's thesis, submitted in 1932, was a theoretical argument based on the possibility that spinal reflexes were learned *in utero* and included a discussion of the functioning of the synapse, based largely on Sherrington (1906) and Pavlov (1928). Although Kellogg was

Hebb's supervisor, this project was outside both his and Tait's expertise and therefore Hebb arranged for Babkin to be part of the examining committee (Hebb, 1980). While he later characterized the work in his thesis as "nonsense," it reflected at least two themes that would persist in Hebb's subsequent work: an interest in the effects of early experiences on the developing nervous system and a general willingness to take an unpopular position. It is also likely Hebb recognized, given his family connections to medicine, the relative disciplinary value of a physiologically-based thesis project.

Hebb's formative education at McGill in both the psychological laboratory of Tait and Kellogg and in the physiological laboratory of Babkin and Andreyev were important to his decision to pursue his Ph.D. with Lashley in 1934. From his experiences, Hebb was intimately familiar with the limitations of both functionalist psychology and Pavolvian physiology and was likely drawn to the exciting discoveries (for psychological theory) emerging from the psychobiological work of individuals like Yerkes and Lashley.

Hebb continued to work with Babkin after he completed his Master's (1933-1934). These years would be particularly difficult for Hebb: his educational experiments in collaboration with the Verdun School Board were being terminated, he found himself increasingly disenchanted by Pavlovianism, and his wife of 18 months was killed suddenly in a car accident in Nova Scotia on his 29th birthday (Hebb, 1980). Hebb resolved to leave Montreal and, with Spence's assistance, had initially planned to pursue graduate training with Yerkes at Yale. He was instead encouraged by Babkin to apply to work with Lashley at Chicago (Hebb, 1980).

Lashley, who was President of the APA in 1929, published *Brain Mechanisms and Intelligence* (1929) and then a year later, *Basic Neural Mechanisms in Behavior* (1930), both highly influential works. Lashley's book laid forth some of his most important ideas and had demonstrated that methods from laboratory physiology could be used to fundamentally challenge the presumptions of the then dominant behaviorist theory (Weidman, 1994), about which Hebb had garnered his own misgivings (Hebb, 1949). A young Hebb was likely encouraged by the possibilities of a biologically-based science of psychology and recognized early that a thoroughgoing intellectual grasp of physiology was fundamental to contemporary psychological understanding, an insight that would serve him well throughout his career.

Hebb's experiences at McGill in the late 1920s and early 1930s presents a slightly different picture than is usually presented about psychology during this time. Psychology at

McGill, and in Canada, lagged behind American university counterparts. Psychology in Canada and at McGill is often not accurately represented by historical narratives of behaviorist hegemony in North America (Samelson, 1981; O'Donnell, 1985). Given the broad areas of functionalist graduate research at McGill, it is perhaps the case that Montreal was somewhat insulated from the dominant influence of experimental behavioral psychology. Indeed, this situation might have created a comparatively 'neutral' environment largely devoid of the most divisive positioning of other, larger departments of psychology at the time (e.g., Harvard and Yale). While a broader discussion of the cultural and politics factors is important, this particular account focuses mainly on the institutional and interpersonal factors that shaped psychology in at McGill University.

Outside a theoretically broad commitment to traditional Functionalism, it is difficult to ascertain how Hebb's education might have been steeped in any particular school of thought. Indeed, Hebb later described psychology at McGill during this period as "far from exciting" -- the lack of disciplinary training at McGill, he discovered upon arriving at the University of Chicago, had left him "ignorant of much current work in psychology" (Hebb, 1980). Hebb's capacity to remain affiliated with the Verdun school board and the Department of Physiology while completing his Master's in psychology part-time speaks to the 'hodge-podge' approach of the department. By providing Hebb with greater theoretical flexibility, this approach was advantageous; he studied school psychology alongside physiology, which would have an important influence on his later thinking – despite his characterization of both as "false starts" (Hebb, 1980, p. 280). Had psychology at McGill been dominated by methodological behaviorism in the 1920s (as it had elsewhere) or opportunities for interdisciplinary work not been encouraged (partly out of necessity), Hebb might have failed to recognize the value of Lashley's physiological approach (Weidman, 1994) or to have taken the steps necessary to receive the training and endorsement from Babkin and Andreyev that helped facilitate his introduction to the integrative psychobiology present at the University of Chicago in the 1930s (Dewsbury, 2002). Therefore, psychology in Canada, at least insofar as the experiences of psychologists at McGill went, was much more varied than is usually described suggesting that behaviorist dominance did not extend quite as prominently into Canada and McGill as it did to other parts of North America.

2. Hebb's Training with Lashley at the University of Chicago and Harvard

In the 1920s and 1930s, Lashley's laboratory at Chicago was among the foremost American labs studying the neural bases of behavior (Milner, Preface to *Organization*, 2002). When asked many years later, Hebb reported that apart from Lashley, and later Beach (at Yale) and Harlow (at Wisconsin-Madison), there was almost no research being done on physiological psychology at the time: “Positivism and the black box were the style, Hull avoiding, and Tolman and Skinner denouncing, any involvement with the brain” (Hebb, 1980, p. 299). When asked many years, Hebb was adamant that “there was no neuroscience in psychology in Canada before 1963” and he had almost no contact with neurophysiological research going on in Canada “if there was any” before he returned to McGill in 1947 “apart from keeping in touch with the work at the MNI” (Hebb, Jun 13, 1984).

At the University of Minnesota, Lashley conducted a series of experiments on the brains of rats. He applied a surgical method he had learned from training with Shepherd Ivory Franz (1874-1933). Lashley destroyed portions of the rats' brain and, after allowing them to recover, tested them for their ability to learn and remember a series of tasks. He found that, by and large, despite missing parts of their brains, the rats had not lost these abilities; specific pathways could be disrupted without significant effect. This convinced Lashley that the brain must somehow function as a whole: memories were not stored in a single region but must be distributed throughout the brain. This is what he referred to as the *theory of equipotentiality*, a major challenge to existing theories of cerebral localization at the time. Lashley’s initial interpretations of results seemed to support Watson's behaviorist theory and, by the late 1920s, he was convinced these results demonstrated an important conflict between equipotentiality and what he interpreted as a *doctrine of localization*, which he increasingly came to oppose (Weidman, 1999). It was through these experiments -- and similar critiques arising from prominent Gestalt psychologists, like Wolfgang Köhler (1887-1967) and Kurt Koffka (1886-1941) -- that Hebb and others came to realize that the prevalent “switchboard” model of brain functioning had been demonstrated theoretically insufficient (Lashley, 1929; 1930a). Therefore, in Hebb's mind, a better and more theoretically rigorous and physiologically-grounded explanation was desperately needed (Glickman, 1996).

Lashley was an eminent and respected physiological psychologist in the early 1930s. He had pioneered the idea that the highest and most complex mental processes could be addressed

by the tools of biological science (Weidman, 1999; Bruce, 1986). In many ways, Lashley had made the "neural correlates of conscious experience" a worthy and reputable problem for science; he showed his followers that the strengths of physiological psychology could be directed toward the mysteries of thought, feeling, and memory (Weidman, 1999); indeed, Lashley stood at the crossroads of psychology and neurology "just as the traffic was beginning to roar" and had made that intersection a natural place to stand (Stellar, 1992). The notion of the field itself an inherently “cross-roads discipline” appears in the history of Canadian psychology (e.g. MacLeod, 1955).

Having recently published two major works (1929; 1930a) and been President of the American Psychological Association, the early 1930s represents some of the most active and productive periods of Lashley's career. It was during this time that Hebb applied to work with him; Lashley accepted Hebb's request in July 1934 in a letter describing the various animal experiments on vision that the department was conducting (Glickman, 1996). Lashley would have a profound and lasting influence on his graduate students and Hebb was among a group of early integrative psychobiologists who shared an outlook on a variety of fundamental issues (Dewsbury, 2002).

2. a. Hebb's Ph.D. research at Chicago and Harvard

During Hebb's time with Lashley in the 1930s he spent most of his time looking at the effect of brain ablation on learning abilities in rats. This technique emphasized the pathological model embodied by experimental physiological (i.e., Claude Bernard, Walter Cannon, Ivan Pavlov). He began working on problems of spatial orientation and place learning at Chicago. When Lashley travelled to Massachusetts after accepting a position at Harvard in 1935, Hebb agreed to accompany him. At Harvard Hebb was compelled to change research projects because of limited time and money (Glickman, 1996).

Delimiting the boundaries of theoretical understanding often involves the exploration of edge cases. Lashley was known for his emphasis on theory. What are the conditions in which the experiment produces observations that the theory fails to predict? Lashley relished the capability of experiment to challenge conventionally held theories. Much of Lashley’s research explored visual perception. Since Hebb was interested in exploring the effects of early environment on learning (e.g., Hebb, 1932), Lashley suggested he compare perceptual differences between rats reared in complete darkness to those reared normally. The goal of these studies was ostensibly to

reveal the role of early experience in shaping what would otherwise appear to be innate behaviors by radically altering the animal's early environments. Since behaviors were understood by behaviorists to be the outcome of complicated conditioning histories, being able to show certain behaviors as innately organized went against basic reflex theory. Lashley and Hebb wanted to show some of the ways these conditioning histories become organized and how early experiences patterned available conditioned outcomes (i.e., behaviours). This research, on the organization of visual perception in the rat, would come to form the research topics of Hebb's Ph.D. dissertation, which he published in a series of papers in the late 1930s (i.e., 1937a; 1937b, 1938a).

Contrary to the empiricist ideas of his Master's thesis at McGill, Hebb had attempted to demonstrate in his doctoral dissertation that rats reared in complete darkness were nevertheless able to learn relative size and brightness similar to any normal animal. This finding indicated that the organization of the visual system was innate and (at least somewhat) independent of environmental cues: Hebb concluded that, "in the rat the figure-ground organization and perception of identity in such geometrical patterns as the solid triangle, outline of triangle, and triangle circumscribed by a circle are *innately determined*" (Hebb, 1937a, emphasis added). These studies appeared to confirm that which Lashley and Hebb had predicted: there were no differences in the two groups of rats because perceptual organization was innate and did not require experience for organization (Hebb, 1937a; 1937b). This view coincided with Lashley's theory of equipotentiality, which suggested that the general pattern of connectivity (rather than any sort of strict localization) governed the organization of behavior.

Hebb received his Ph.D. during the late Depression when there were very few jobs in physiological psychology available, and none in Canada. During a postdoctoral year at Harvard, Hebb completed the work he had begun on spatial learning and orientation in rats. This work yielded evidence of innately organized figure-ground separation, consistent with the Gestalt position, and of similarity between normal and dark-reared rats in the perception of horizontal versus vertical patterns shown by transfer tests. Hebb published these experiments that year (Hebb, 1937a; 1937b), they were heavily based on his original thesis experiment on spatial orientation and place learning.

Almost a decade later, while writing *The Organization of Behaviour*, Hebb re-examined these data and realized there was more going on than he had first realized. The dark-reared rats

took six times longer than normal to learn to distinguish vertical from horizontal lines. The groups of dark-reared and normal rats were similar indeed, but as it turns out, only after perceptions had already been established (Hebb, 1980; Hebb, 1949). Only many years later, after he had again changed his ideas about the relative importance of innate and learned mechanisms, did he appreciate the significance of the result, which he took to mean the organization of basic functions, such as vision, which were indeed affected by early environments. Hebb later explained why he had not noticed these results or noted their importance by suggesting that, "I was young and completely immersed in the climate of opinion in the laboratory" (*New Scientist*, 1975).

2. b. Hebb's experiences at Chicago and Harvard

The late 1930s, the period in which Hebb completed his doctoral studies, was a time of profound theoretical debate in psychology. This period, until the early 1960s, is usually characterized as around the time when the intellectual traditions of neobehaviorism—with notable figures such as Hull, Spence, and Skinner—were the reigning schools of thought within the discipline. Psychobiologists such as Yerkes and Lashley evidently complicate this picture. The kind of biologically-oriented psychology that they would establish in the United States would come to figure importantly into considerations for the foundations of cognitive neuropsychology in Canada.

Lashley was sympathetic to Gestalt psychology and was critical of associationist theory of all kinds (Weidman, 1999). He tended to regard the emphasis on environmental influences that conditioned reflexes as misplaced and was considered by most psychologists as strongly hereditarian (Weidman, 1999). Lashley worked in the physiological realm of behavior while simultaneously publicly denouncing attempts at assembling neurological theories to explain behavior (Glickman, 1996; Lashley, 1930a). As Lashley's graduate student in the 1930s and research colleague in the 1940s, no single individual exerted more influence on Hebb's intellectual development than Lashley (Bruce, 1996). Hebb's time with Lashley convinced him of the limitations of both Pavlovian conditioning and the mechanistic elementalism of contemporary neobehaviorism (Hebb, 1980); the two men shared a strong conviction that psychology should be a laboratory science based firmly on neurological principles (Weidman, 1999; Bruce, 1986).

While Hebb's time at Chicago was brief, Hebb recalls it as being formative to his intellectual development (Hebb, 1980). He described in particular the vibrant community of physiological psychologists, both at Chicago and then at Harvard, from whom he learned a great deal during this period: this included lectures in factor analysis from L. L. Thurstone (1887-1955), in phenomenology from Köhler, in neurology from C. J. Herrick (1868-1960), and in physiology from Nathaniel Kleitman (1895-1999), among others (Hebb, 1980). Hebb emphasized in later interviews how much of the intellectual stimulation he received was from fellow graduate students, including David Krech, Walter Lurie, and Douglas Smith, in the form of "bull sessions." This group of students, including Hebb, referred to themselves with the amusing label of "The Cosmic Academy of Science." Their goal to critique their own and their colleagues' intellectual challenges and figure out what their professors were talking about (Hebb, 1976). After Hebb completed his Ph.D. he continued to attend regular informal seminars with students and faculty. These included Frank Beach from Kansas, George C. Drew from England, Edwin E. Ghiselli from California, and Andre Rey from Switzerland, among others. Hebb characterized these meetings as "both argumentative and friendly, critical and constructive, and mostly educational" (Hebb, 1980).

Perhaps the most salient point from this period was that, from his publications, Hebb was theoretically aligned with Lashley's thinking about the brain (e.g., see Weidman, 1999). The aspects of thinking about the brain and of physiological psychology is perhaps best described by Dewsbury (2002). Dewsbury describes some of the core beliefs that graduate students developed working with Lashley at the University of Chicago in the early 1930s. Dewsbury's "Chicago Five" included Donald Hebb, Frank Beach, David Krech, Norman Maier, and Theodore Schneirla. While members of this group differed in some important ways—Krech and Schneirla were both Marxists who adopted early notions of neuroplasticity, while Beach and Hebb emphasized the nativism characteristic of Lashley's thinking—the beliefs that guided this group, or family, was a focus on organized cognitive processes from an evolutionary-comparative perspective that was grounded in physiological science (Dewsbury, 2002). Many of the particular differences between these physiological approaches were synthesized in the early 1960s through the language and concept of "critical periods" of development (Almli & Finger, 1987).

Hebb seems to have been more reluctant than Lashley to discard speculative theory and associationism wholesale. The neuropsychological theory he would later develop (1949)

integrated what he recognized as the core value of connectionist theory (Medler, 1998): it did not propose direct links between afferent and efferent pathways, rather, networks served “to establish central autonomous activities, which are the basis of further learning” (Hebb, 1949, p. xix). Hebb remained a functionalist; he attempted to incorporate those functional (neurophysiological) properties thought to be required for cognition. His theory of information processing (connectionism) relied on parallel processing, using statistical properties instead of logical rules (Medler, 1998). The role of experience (and learning), therefore, not only in perception, but for the overall organization of the brain and its functioning, became a major theme in Hebb's career and one for which he is best known. These views were cemented by the influences Hebb encountered (mostly through his interactions with fellow graduate students) while working with Lashley in the 1930s. Hebb's later thinking, developed through subsequent studies of intelligence in neurological patients, would conflict with Lashley's anti-theoretical and anti-associationist views of the mind.

3. Hebb at the Montreal Neurological Institute and at Queen's University

While still at Harvard, Hebb was contacted by his sister about an opportunity she had noted while completing her dissertation at McGill: the famous neurosurgeon Wilder Penfield (1891-1976) was looking to recruit psychologists to the Montreal Neurological Institute (MNI) to study the intellectual capacity of patients who had undergone brain-surgery (Feindel & Leblanc, 2016). Hebb jumped at this opportunity and was one of only two psychologists to get the coveted position. Hebb's appointment to the MNI in 1937 is significant for a number of reasons.

First, psychologists in medical research settings were uncommon at the time and Hebb was among the first psychologists to have been appointed to such an institution. Hebb had maintained his many connections in both the Department of Psychology and the Faculty of Medicine at McGill and had expressed interest (after five years abroad) in returning to Canada (Hebb, 1980).

Second, Hebb's role at the MNI was that of a mental tester, something with which he had no direct prior experience. He had experience testing the mental aptitude of rats in Lashley’s laboratories, but had never conducted studies or administered tests to adult humans. He was, however, a biological psychologist and his aptitude and background knowledge made him a uniquely qualified candidate; Hebb was familiar with experiments using ablation techniques

from having trained with Lashley. Surgical methods had advanced sufficiently to be able to now conduct these techniques on humans; Montreal was a North American hub for neurological research even before the MNI was established in 1934 (Feindel & Leblanc, 2016). While Penfield conducted brain surgery for therapeutic purposes (i.e., to relieve symptoms of Jacksonian epilepsy), Hebb recognized the opportunity being presented for research: the intelligence of rats could only be determined indirectly following ablation, neurological patients could be assessed with existing intelligence tests. Hebb was hired to ascertain whether psychological damage was being done to patients through these new (and often experimental) surgical techniques; he was able to use this newly available source of data about brain mechanisms to better understand how brains responded to direct, specific, and systematic manipulation for the first time.

Third, it was through his experiences working alongside neurologists at the MNI that Hebb recognized a return to physiology in psychology was imminent and necessary. The kinds of questions Hebb was interested in were those being made possible by breakthroughs in neurological technologies--such as advanced neurosurgical techniques, the interpretation of brain images (i.e., EEG measurements), and pharmaceutical innovation--many of which were being developed at the MNI (Feidel & Leblanc, 2016). The results of Hebb's work during this time plunged him into a period of theoretical reconciliation as he struggled with the problem of adult intelligence and its relationship to brain functioning. As Hebb publicly remarked, “The situation here was unique; I do not know where else a psychologist could have found such training, nor such stimulation to research” (Hebb, 1959).

3. a. Role of mental testing in medicine prior to WWII

In the late 1930s Hebb transitioned from a comparative laboratory psychologist to a mental tester working in a clinical setting as part of a group of medical men pioneering the treatment of neurological disorders. To understand Hebb's role within this context it is important to examine the role of mental testing in relation to the clinical settings of this period.

Before World War II, demarcation disputes between psychology and medicine were primarily voiced by psychiatrists who were concerned about psychologists trespassing perceived professional boundaries (Buchanan, 2003). The use of mental tests by nonmedical and lay experts became increasingly common by the 1930s; where psychiatrists perceived psychologists approaching the therapeutic domain, signs of friction emerged. The professional relationship

between psychologists and psychiatry was one of both rivalry and mutual dependence based largely on common self-interest (Buchanan, 1997).

After World War I the roles for psychologists expanded to include various applied areas (Capshew, 1999; Burnham, 1988). The mental hygiene movement played an important role in North America (see Chapter One); it helped psychiatrists overcome their traditional isolation as custodial caregivers in state asylums and created opportunities for allied professional groups (Grob, 1983; Horn, 1989). Applied psychologists with an interest in education and psychopathology found work in state mental hospitals, training schools for the mentally defective, child guidance clinics, and psychoeducational clinics affiliated with schools and universities (Buchanan, 1997). Indeed, it was through their role as educators in the child guidance movement and informal private consultation that psychologists' interest in clinical applications developed (Napoli, 1981; Zenderland, 1988). There were few restrictions on what constituted treatment of children and psychologists increasingly and overtly contested the limits of psychiatric authority. However, as long as psychologists did not describe their attempts to deal with mild affective disturbances and learning problems as *therapy* they tended to escape legislative scrutiny (Buchanan, 2003); demarcation disputes prior to World War II arose in the context of boundary work; debates tended to crystallize around any practice described or construed as "diagnosis" or "treatment," such as psychotherapy (Buchanan, 2003).

Throughout the 1930s, the relationship between psychology and medicine remained poorly defined; psychologists interested in psychopathology—what would later become clinical psychology—had not yet adopted a professional identity independent of psychiatry. At this time training opportunities for psychologists in areas related to medicine were severely limited, and virtually non-existent in Canada until after World War II. Those practicing outside universities were comparatively small in number and most were women, restricted by a subservient "nursing" image (Buchanan, 1997). Few in number, unorganized, and a lack of support tended to put psychologists at considerable disadvantage when dealing with other professional groups and served to maintain medical control of treatment-related practices, such as psychotherapy until the 1950s (Buchanan, 1997, 2003). Unwilling and unable to challenge the existing medical monopoly on practices regarded as treatment, psychologists established their expertise in relation to medicine as *mental testers* in the 1930s.

Among the roles of the psychologist practitioner, mental testing (assessment) met with the least resistance from those in medicine who had already staked claim to the domain of psychological treatment (i.e., psychiatrists). In state mental hospitals and affiliated outpatient clinics, testing was the defining characteristic of psychologists' work. In the 1920s, intelligence scales and their derivatives dominated the applied psychologist's practical repertoire. The sheer popularity and the relatively routine application of these tests encouraged their use by the nonqualified which cheapened the expertise levels and professional status of those who depended on them. Applied psychologists labeling themselves “psychometricians” became the most poorly paid of these practitioners and, without appropriate psychiatric training, psychologists in medical settings were either not hired or failed to be promoted to more senior positions (Buchanan, 1997). Aware of the vulnerability that went with direct psychiatric supervision, and under increased pressure from demands for applied services, hospital-based psychologists were usually confined to very narrow psychometric testing (Darley et al., 1939; Watson, 1953).

3. b. Penfield, Hebb, and Intelligence Testing

In 1937 Penfield created a Fellowship at the Montreal Neurological Institute to recruit psychological experts to join his team pioneering neurosurgical procedures. Penfield's specialty was the treatment of focal epilepsy by surgically removing scarred areas of the cerebral cortex. He was acutely aware that he was operating on the organ of the mind and his work provided him with unique access and perspective on the relation between the conscious mind and the nervous system. This experience undoubtedly influenced his decision to appoint psychologists to his team and explained the close interest he took in their findings.

Neurologists had long been interested in the effects of damage to the brain to the overall functioning of the mind. These neurologists were opportunistic by necessity; they had to wait for situations, like accidents or war, in which brain-damaged patients were available for study. Lashley's work in the 1920s was notable in part because he had found a way around these opportunistic challenges. Using an evolutionary framework, comparative psychologists used animal models for deriving insight about human psychology. Techniques of cortical ablation could control the area and extent of damage; Lashley conducted meticulous studies of the effects of ablation to specific areas of the rats' brains to understand how behavior functioned and was organized in the brain. The ever-manipulated brain-damaged rat became the subject of Lashley's

experiments, a platform upon which his mind-brain theories were developed throughout the 1920s and 1930s.

Penfield wanted to know what kinds of effects the surgeries—which typically involved removing large parts of the cerebral cortex—had on patients' overall normal mental functioning. Penfield was well aware how little was known about the possible consequences for patients, especially in terms of psychological effects (such as intelligence) which was much harder to ascertain and largely unknown. Hebb's main responsibility was to study the nature and extent of any intellectual changes in patients as a consequence of cortical excisions. Such research, in principle, was not new: it began after WWI with the psychometric testing of soldiers who had suffered penetrating head wounds and continued later in patients with brain tumors. In many cases, the lesions produced significant intellectual loss, but their locus and extent were difficult to determine. In contrast, surgical removals were more precisely defined, and epileptic scars tend not to cause the kind of widespread damage that bullets or tumors do. The neurosurgical experiments at the MNI in the late 1930s therefore provided a unique opportunity for systematic study of the relationship between specific lesions to overall brain functioning, understood at the time to be captured by the notion of "intelligence," or total brain functioning. Hebb's first task was therefore an extension of Joseph P. Evans' (n. d.) work on the psychological effects of removing sections of the frontal lobe (Hebb, 1939). The lesion studies he conducted while at the MNI provided sufficient clinical data to challenge his fundamental understanding of intelligence.

Hebb worked with Penfield and colleagues at the MNI from 1937 to 1939. This resulted in a series of papers on "Intelligence in man after large removals of cerebral tissue" (Hebb, 1939a, 1939b, 1941c) and an article with Penfield on the subject (Hebb & Penfield, 1940). Hebb reported a number of sources of psychometric data in these studies (1939a). In his main studies (Hebb, 1939a, 1939b), he reported using the revised Stanford Binet (Form L) to determine mental age, various tests for language comprehension, non-language intelligence tests, and the McGill revision of the Army Beta.

Hebb encountered challenges when applying well-known intelligence tests to the assessment of brain-damaged patients at the MNI. In clinical and psychiatric contexts, there was limited applicability of existing scales, such as the Binet-type IQ tests for measuring intelligence and for psychomotor performance, common methods included maze navigation, block design, and drawing tests (Buchanan, 1997). The separation of the two strands in Montreal continued to

silo the concerns and interests of psychology and medicine from each other (see Chapter One). As St. Elizabeth's Hospital psychologist Winifred Richmond complained in 1924, "We are greatly in need of more and better tests, tests standardised on normal adults. The ones we have cannot be used according to rule but must be adapted to our purpose […] We are not concerned in rating our patients or classifying according to 'mental age' or IQ, but in discovering how much of a given ability is present and capable of function" (Richmond, 1924, p. 310, quoted in Buchanan, 1997, p. 173). The emphasis on psychological function, understood within the context of pathological diagnosis, reflects the broader concerns of systems thinking and categorization which were ascendant in the social sciences of the postwar era (Rose, 1988; Heyck, 2015). Hebb was interested in the functions of the brain in a structural sense, which could be represented in the form of a model which accurately captures the relationships between various discrete parts (Hebb, 1949).

Hebb's studies with Penfield were particularly important for highlighting limitations to the concept of intelligence itself for diagnostic testing (Hebb, 1940, 1942a). Hebb struggled to accurately reflect the complexity of outcomes he observed among brain-lesioned patients using mental tests not originally developed for the purpose. He bemoaned the deficiencies of the psychometric tests available at the time. "The relation of intelligence to the brain was a far greater problem than anyone had suspected," Hebb said in an address to the MNI a decade later (Hebb, 1959, p. 263). Hebb's understanding of what intelligence signified was keenly shaped by his time at the MNI. The relationship between intelligence and neurological function, Hebb had discovered, was far less synonymous (compared to organization) than had been previously recognized (e.g., Hebb, 1959).

After his fellowship ended in 1939, Hebb continued to modify and further develop different kinds of tests for intelligence. For example, while at Queen's he designed standardized intelligence tests for rats (Hebb & Williams, 1946). The concept underlying the notion of intelligence, as something both innate and malleable, was elusive. New tests were needed for new ways of thinking about the questions. Hebb worked with N. W. Morton, then teaching in the Department at McGill, to develop tests able to measure a broader understanding of normal mental functioning (i.e., the judgment of human behavior in common situations). The McGill Verbal Situation and the McGill Picture Anomaly series were created and included in the Adult Comprehension Examination, which aimed "to supplement the more abstract problems of

existing tests" (Hebb & Morton, 1943). Of these tests Hebb wrote that, "it is misleading to suppose that one can measure intelligence by the available methods," but these tests "measure a kind of cultural comprehension which […] may provide a better basis for estimating or rating intelligence than many current tests" (Hebb & Morton, 1943). The idea of intelligence, to Hebb, was itself changing.

Hebb's appraisal of his research during this period was that he had failed to clarify the problems and may even have compounded them (Hebb, 1959). Two decades after these studies, Hebb characterized the research at the MNI as having "disposed" of both the localization theory and of Lashley's principle of mass action (the main alternative approach), which he noted was "very disconcerting" (Hebb, 1959, p. 263). In the period following this work—at Queen's and then at Yerkes Lab—Hebb continued to search for a way to satisfactorily bring the results of his experiences on intelligence into greater coherence with what was known in terms of the biological basis of learning, memory and motivation.

3. c. Penfield, Harrower, and Projective Testing

As the roles of clinical psychologists continued to expand in the 1930s, mental tests also changed. These tests were developed increasingly with the needs of clinicians in mind. Psychologists adapted psychometric measures for psychiatric purposes and personality inventories were constructed explicitly to reflect psychiatric symptomatology. One class of measurement, strongly based in Freudian psychoanalytic framework, were *projective tests*. These kinds of tests share the idea that the process of ambiguous interpretation is itself a means for revealing hidden emotions, internal conflicts, and maybe even psychopathology. Enthusiasm for new projective techniques speak to the promissory power of mental testing for addressing the diagnostic needs of interwar psychiatrists (Buchanan, 1997).

Hebb was not the only psychologist the MNI hired in the late 1930s to investigate the psychological condition of patients following surgery. The other psychologist hired was Molly Harrower (1906-1999). As a clinical psychologist, she came from a different tradition from Hebb. Harrower had received training as an experimental Gestaltist at the University of London, England and completed her Ph.D. with Kurt Koffka at Smith College in Northampton, Massachusetts, in 1934. Before joining Penfield and colleagues at the MNI, she had spent six months with neurologist and recent immigrant, Kurt Goldstein at Montefiore Hospital in New York City (Harrower, 1984). She studied Gestalt therapy with Goldstein, who had a history of

collaboration with psychologists and was open to the use of experimental psychology in medical settings (Harrington, 1998; Pickren, 2003). Allan Gregg, vice-president of the Rockefeller Foundation, directed her to Penfield and the MNI, where she arrived in 1937, in time to contribute a chapter on the psychological assessment of epileptic patients in *Epilepsy and Cortical Localization* (Penfield & Erickson, 1941).

Hebb and Harrower represented two forms of psychology familiar to medicine at the time. While Hebb was brought to the MNI to assess the mental functioning of Penfield's patients with intelligence tests, Harrower conducted a different kind of mental test, the projective test, of which perhaps the Rorschach inkblot is the most widely known.

The work of Hermann Rorschach (1884-1922) was brought to America in 1924 by psychiatrist David Levy (n. d.) with the first studies having been conducted in 1930 (Beck, 1930; Buchanan, 1997). The "projective hypothesis" was that the person's response to the ambiguous stimuli was the "projections" of his or her feelings, emotions, and underlying personality. Individual differences in the style and content of responses were assumed to be a function of personality differences. By the late 1930s the United States had become a major site for research on the Rorschach (Klopfer, 1973) despite being largely ignored or resisted by mainstream experimental psychologists (Buchanan, 1997). By the late 1930s homegrown variants of projective techniques began to appear as well, the most notable of which was Henry Murray's *Thematic Apperception Test* (Morgan & Murray, 1935).

Harrower had been trained in the use of the Rorschach inkblot test in England and had planned to conduct these tests with Penfield's patients. While Hebb was conducting his studies applying various McGill protocols to brain-damaged patients, Harrower began work on a group Rorschach test. Having worked in the interdisciplinary atmosphere created by Goldstein, Harrower recognized the lost opportunity created by the lack of structured cooperation between psychology and medicine at McGill in the late 1930s. When she approached the Chairman of Psychology however, Harrower reports that Tait responded by suggesting that, "this is a man's war and there is no place for you or anything you have to suggest" (Harrower, n. d.). While this obviously smacks of overt sexism, this response also likely reflects the attitudes of academic psychologists to clinical psychologists during this period. Indeed, unable to convince "experimental purists," clinical psychologists found themselves stalemated by a lack of support from their academic colleagues (Buchanan, 2003).

There was considerable internal dissent around the interpretation of projective tests prior to World Wwar II, particularly in the case of the Rorschach. There was much concern from academic psychologists that its administration and scoring were far too haphazard and unreliable, and interpretation far too subjective (Exner, 1969; Reisman, 1991). These projective tests contrasted sharply with traditional psychometrics, for example those with which Tait, Kellogg, and Morton were involved. Indeed, as far as scientific psychology was concerned, these new projective mental tests were fundamentally flawed due to their reliance on the psychoanalytic framework for their interpretation. Intelligence tests, on the other hand, were standardized and therefore seen by many Canadian academic psychologists as being somewhat more objective. Why then did the MNI hire a projective clinical psychologist?

Despite apprehension about the scientific legitimacy of the test, the Rorschach was imported with the allure of sophisticated continental ideas, most notably psychoanalysis. Because the Rorschach had clinical utility – it was applicable to a wide range of individuals, less threatening and overtly evaluative than conventional tests and interviews, and the experience often mirrored that of other clinical encounters – the Rorschach spread into contexts dominated by psychodynamic psychiatry (Buchanan, 1997, p. 177). Given the absence of Freudian influence at the MNI (if anything, antipathy), it is telling of the complicated relationship between projective testing and mainstream medicine at the time that Harrower would choose to conduct Rockefeller Foundation funded research at the MNI and that Penfield and colleagues were seemingly accommodating of her type of work (however, it is important to note, we cannot really be sure they were). Correspondence indicates a close working relationship between Harrower and Penfield. Penfield was not a psychologist, but he was deeply invested in understanding the mind's relationship to the brain (e.g., Penfield, 1975). However, the degree to which Penfield's own views towards psychology were relevant to his specific choice of psychological appointment is not known (see Prkachin, 2018).

While Hebb's Master's supervisor, Kellogg, had worked with Yerkes during World War I and been a skilled test developer, Hebb's research was theoretical and never involved human subjects. Similarly, his training with Lashley involved almost exclusive use of rats. He had received no special training in the mental assessment of human subjects. Harrower's work with Goldstein prepared her for clinical work with human patients. However, while neither Hebb nor Harrower had received any formal medical training, both had considerable knowledge of

neurology and physiology, especially for psychologists in the late 1930s. Hebb brought to the MNI a unique set of skills, among them a comprehensive neuroanatomical understanding of the rat's brain. Hebb later observed, "I was a babe in the woods in that clinical setting […] interest in anatomy established my credentials. They [the Fellows at the MNI] proceeded to give me an intensive course in pathology. And various clinical problems. This was invaluable in reporting my results and trying to communicate with colleagues elsewhere" (Hebb, 1980, p. 291). Indeed, for the niche that Hebb was entering, neurological expertise, the capacity to speak the language of clinical medicine, was more important than psychometric experience – given the stature of mental testing in medicine, it is easy to see why (e.g., Buchanan, 1997).

The different approaches taken by Hebb and Harrower represent the two dominant views of psychology's utility to institutional medicine, as largely driven by the interests and priorities of funding organizations (e.g., the Rockefeller Foundation and Gregg, who had established the fellowship): the utility of empirical assessment (intelligence tests, and later personality inventories, which produced numerical scores on linear scales and compared to a population distribution) and subjective diagnosis (projective tests, like the Rorschach, which produced a clinically-useful psychodynamic interpretation). Hebb's appointment represents the privileging of a certain type of psychologist yet to fully emerge: an expert of normal human mental functioning that used physiological understandings of normal/pathological familiar to medicine, rather than the problematic and elusive notion of normality associated with popular forms of applied psychology and mental testing (as mobilized, for example, by the mental hygiene movement) (Canguilhem, 1978). The establishment of the fellowship at MNI and the appointment of the two psychologists might be viewed as a response by Penfield and the MNI to pressures from the university and philanthropists to bring together scientific research on the mind during this period (Gavrus, 2008; Pols, 1999). Penfield's work affected the possibilities for consciousness research; having recognized this, Penfield was likely interested in bridging his work with developments in psychology broadly, as the men trained at the MNI did not typically receive training in any kind of psychology at the time (Hebb, 1980).

Due to the sophistication of technique and volume of patients, the neurological patient was able to become a new kind of scientific object (Daston, 2000). Hebb and Harrower, with Penfield and colleagues were among the first to examine the psychological implications (changes in brain function) and associated scientific value of these scientific objects(-subjects). The

limitations of mental testing Hebb encountered were taken as experimental evidence in clinical practice, and to propose a new experimental framework in psychology (Hebb, 1949). For Hebb, it seems, the difference between disciplines was the level of explanation, rather than methods, theories, or subjects.

4. Hebb returns to work with Lashley: The Yerkes Laboratory

4. a. Post-MNI, Queen's University, and the Problem of Intelligence

At the end of his fellowship with Penfield in 1939, Hebb accepted a position at Queen's University. Here he resumed a line of research similar to that which he had begun with Lashley. He continued his work examining the effect of cortical lesions on learning in rats, extending the observations he had made with human subjects. His experiences at the MNI raised questions about the relationship between intelligence and common understandings of brain function at the time. Hebb continued to struggle with these questions throughout the following years. He described this theoretical insight "less dramatic … but perhaps more important" than his later research (Hebb, 1980, p. 300). Unable to find answers to the questions raised by these tests -- Hebb had demonstrated that intelligence was much more malleable than had been assumed -- he dedicated his research activities during his time at Queen's on the limitations of existing notions of intelligence at a fundamental level.

In September 1939, the same month Hebb began at Queen's, Canada followed the United Kingdom in declaring war on Germany. The Canadian Psychological Association was in the early stages of formation (Dzinas, 2000), and once again the applications of psychological knowledge to the war effort was in high demand (Capshew, 1999a). Hebb focused on basic research during this time, but his activities were inflected with the urgency of the time. The problem of intelligence was widely recognized; his research had suggested a broader consideration was necessary, and in doing so pivoted once again from rat to human subjects.

Hebb went to work demonstrating that insight derived from basic research (on rats) would shed light on brain functioning in adults; he developed more comprehensive intelligence tests based on his work with rats, and continued to regularly publish on intelligence and brain function in humans (e.g., Hebb, 1940; 1941a; 1941c; 1942d). He developed with Kenneth Williams (n. d.) a general method for the development of intelligence tests for animals and proposed the Hebb-Williams maze, though the paper was not published until much later (Hebb &

Williams, 1946). Hebb theorized intelligence as consisting of *different kinds* (A and B) and contributed to what would become Raymond Cattell's theory of fluid and crystallized intelligence (Brown, 2016). He also wrote an article in which he proposed that early experience could permanently influence adult intelligence (1942a), a relatively radical claim at the time and a theme that runs through much of Hebb's later writing.

The concept of intelligence is important to understanding both Lashley and Hebb's theories of mind-brain relations (Weidman, 1999; Orbach, 1998). For Lashley, the notion of intelligence did crucial work in bringing together the mental and the physical, psychology and neurology. Lashley considered intelligence a mental and a biological entity that had both physiological and psychological dimensions. It could be represented not only in the behavior of an animal or a person, or in their performance on tests, but also by the physical amount of functional brain mass that they possessed (i.e., Lashley's theory of mass action). From experiments that Penfield had conducted, Hebb helped show that patients who had had large areas of their cerebral cortex removed were not as adversely affected as had been assumed. In some cases, counterintuitively, intelligence seems to have increased with the removal of parts of the brain (e.g., Hebb, 1941c). Hebb maintained the idea that intelligence, fundamentally, referred to the intellectual capacity from which perceptual organization and behavior is developed through experience with the world. He provided evidence of the functional independence of some of the factors making up human intelligence in relation to the problem of distinguishing the direct expression of intellectual power from its effects during growth (Hebb, 1942a).

As the war continued, Hebb was not pleased with the situation in which he had found himself at Queen's University (Hebb, 1980). His teaching schedule and involvement with the newly formed Canadian Psychological Association—he was the first editor of the organization's flagship journal, *Canadian Journal of Psychology*, and was a member of several committees—left him with little time for the development of his research. Most of his publications during this time were in relation to the work he had done while at the MNI on the relationship of human intelligence to brain-injury (e.g., Hebb, 1940, 1941a, 1941c, 1942a, 1942d).

Convinced existing intelligence tests were insufficient for capturing cognitive change in neurological patients, Hebb developed his own test with N. W. Morton. Morton had been Hebb's teacher at McGill and was then part of the Defense Research Board of the Canadian government. The intelligence test Hebb and Morton devised to better understand changes in neurological

patients was based on whether an individual was able to correctly identify whether a particular object in a picture was "funny or out of place" (Hebb & Morton, 1943): They called it the McGill Picture Anomaly Test (MPAT). While useful for understanding the relationship between visual recognition and the right temporal lobe, its diagnostic use was limited (e.g., Shalman, 1961). Though Morton would not return to McGill after the war, they maintained a close relationship and both played an important role in shaping the development of psychology at McGill and in Canada throughout the postwar period.

4. b. Hebb joins Lashley at the Yerkes Laboratories in Orange Park, Florida

While Hebb was at the MNI and then Queen's, Lashley had been appointed Director of the Yerkes Laboratories of Primate Biology, a chimpanzee breeding and research station in Orange Park, Florida. Lashley contacted Hebb in April 1942 to tell him about his new position and need for quality physiological psychologists at the Laboratories (Glickman, 1996). Lashley had a colony of New World monkeys and a group of about fifty chimpanzees. His plan was to focus the institute's work on research into the biological basis, or constitutional component, of complex behaviors (Weidman, 1999); and to conduct neurological and endocrinological studies of sex behavior, emotional and temperamental differences, and abilities in abstraction and generalization (Dewsbury, 2006). Although Hebb reports having little interest in studying chimpanzees, the flexibility of a full-time research position and the salary he was offered (annual salary of four thousand dollars plus traveling expenses) was sufficient to convince Hebb to leave his job in Ontario. Hebb moved with his family to Florida in the fall of 1942 (*New Scientist*, 1975; Hebb, 1980).

At Orange Park, Lashley proposed that his researchers study brain structure and function by examining the symptoms and course of recovery from localized brain lesions in animals. His goal was to demonstrate how both intellectual behavior and sexual behavior could be isolated by holding environmental influences constant (Dewsbury, 2006). The activities of the Laboratories under Lashley were divided into seven main topics, one of which was the "the structure of intelligence" which focused on the "independent functions" that comprised intelligence (Weidman, 1999). They used several different methods in the study of intelligence: the comparative study of the abilities of different animals (Hebb adapted his maze test to assess intelligence in chimpanzees; Hebb & Williams, 1946); factor analysis of individual differences

using Thurstone's methods; and the disintegration of behavior by brain injuries (Weidman, 1999).

Hebb was tasked with examining the biological origins of emotion (particularly strong instinctual emotions, such as fear) and to develop ways of measuring differences in the emotional temperament of chimpanzees, while Lashley and Henry Nissen (n. d.) worked on intellectual function (Hebb, 1980). The idea was that once standardized measures were established, Lashley would operate on the animals' brains and Hebb would re-test them to ascertain what type of brain injuries affected what type of behavior. This method was the same, in principle, as that which Hebb had used with rats while working with Lashley at Harvard and with Penfield at the MNI with frontal-lobe operated patients. Consistent with his interests and research practices he had adopted while at the MNI (e.g., Hebb, 1942a), Hebb requested a comparison of the effects of lesions at an earlier stage of development with those of adult chimpanzees (Glickman, 1996). Hebb was already beginning to theorize that young (inexperienced) brains were somehow fundamentally different than adult brains.

Prior to any surgery, however, Lashley wanted to train the chimpanzees on about thirty different habits. This proved much more difficult than anyone had anticipated, neither Lashley nor Hebb had worked with chimpanzees before. It was largely taken for granted that teaching behaviors to chimpanzees, because of their superior intellect compared to rats, would be much easier. As it turned out, a discrimination task, for example, which could be learned reliably by a rat in approximately sixty trials would take close to three hundred for a chimpanzee (Weidman, 1999). The first surgeries, therefore, weren't performed until the end of Hebb's fifth and final year, just months before his departure for McGill in 1946.

4. c. Observational studies, primate emotions, and the roots of Hebb's neuropsychological theory

These setbacks made the study Hebb had proposed impossible. Instead, Hebb spent his time at the Yerkes' Labs devoted to observational studies of behaviors related to emotions (Hebb, 1945a), especially what he called “spontaneous fears” (Hebb & Riesen, 1943; Hebb, 1946a), individual differences in temperament in captive chimpanzees (Hebb, 1946a; 1949b), and a pioneering study on captive dolphin behavior at Marineland with “Mister Porpoise” Arthur F. McBride (McBride & Hebb, 1948). Two of these studies were published in the *Psychological Review* in 1946, and like the others, represent a synthesis of Hebb's work under Lashley's supervision during this time. For example, “Emotion in Man and Animal” (Hebb, 1946a),

described a novel procedure for categorizing emotions and identifying emotional states in individuals. Hebb used global judgments of human observers and then employed this information to analyze the process of assigning emotions in both people and chimpanzees (see discussion of Hebb's theory of emotions in Chapter Five).

Later that year, Hebb published a paper on fear responses in chimpanzees as a result of learned expectations (Hebb, 1946b). This paper is important because it is among the first to include a fair degree of neurological speculation regarding the physiological events underlying emotional reactions, uncommon in the literature at the time. Indeed, it was during this period, while searching for a theoretically compelling explanation for the emotional behavior of chimpanzees, that many of the insights articulated in *The Organization of Behavior* (1949) were initially developed (Hebb, 1980; Brown & Milner, 2002). It is evident from the discussion in these articles (e.g., Hebb, 1946b) that Hebb was in the process of working through the implications of the discovery of reverberatory circuits by Rafael Lorente de Nó (1902-1990) in 1939, which Hebb had come across in the work of E. R. Hilgard and D. G. Marquis (*Conditioning and Learning*, 1940) some years before (Hebb, 1980). Hebb was committed to pursuit of directly relating the behavior he had observed in primates to neurological theory. In fact, the term "phase sequence" first appeared here among a set of properties, in a slightly different form, but well in advance of the publication of *The Organization of Behavior* (e.g., Hebb, 1946b, p. 269). These were not simply empirical reports on observations of animal behavior, but the early underpinnings of neurobiological theories of psychological organization and function (Hebb, 1949).

Hebb also discussed in this article the conditions associated with the stability of these sequences and began to outline a neurologically-based theory of the conditions under which such sequences would become disorganized (i.e., emotion), as in various fearful states (Hebb, 1946c). He concluded this article with the statement that, "fears are evidently not determined by a sensory event alone, and the behavior is not intelligible except on the assumption that its control is a joint product of sensory and 'autonomous' central processes […] An adequate hypothesis of the nature of fear *cannot be framed in psychological terms alone*, but must utilize physiological concepts of cerebral action […] fear originates in the disruption of temporally and spatially organized cerebral activities" (Hebb, 1946c, p. 274, emphasis added). According to citation data provided by *Web of Science*, this article (*On the nature of fear*, 1946c), is Hebb's second most

cited article and is the first in which he attempts to broadly synthesize neobehaviorist and Gestalt psychology through a neuropsychological framework; references include Hull and Skinner alongside Köhler and J. J. Gibson (1904-1979).

At Orange Park, Lashley guided the work of the laboratories away from the emphasis his predecessor had placed on social engineering and towards an investigation of the biological basis of both intelligent and sexual behavior (Weidman, 1999; Dewsbury, 2006). As part of these efforts, the challenges Hebb faced in both establishing reliable measures and with attempting to standardize the behaviors of chimpanzees, compelled a greater degree of observational research than Hebb had expected (Hebb, 1980); he worked closely with these apes. And despite initial hesitation, it seems he came to appreciate this time among them. Indeed, psychobiologists believed chimpanzees, as laboratory animals, offered special insight into the minds of humans (Haraway, 1989). The value of these model animals was that they were both engineered (in the sense that they served the specific interests of the experimenters) and were natural (in that they were understood as mirrors for humanity devoid of cultural influence) (Haraway, 1989, p. 62). Hebb saw in the apes he observed the raw power of emotions which contemporary psychology had failed to adequately address. Hebb wrote that he had "learned more about human beings during that time than in any other five year period of my life, except the first" (Hebb, 1980, p. 293); he is reported to have said that watching chimpanzees was like seeing humans with 'the veneer of culture stripped away' (quoted in Glickman, 1996). Without culture, it would seem, Hebb was able to justify a more direct focus on the "true" determinants of emotion, which (in his case) were conceptions of neurophysiological action (Hebb, 1946c). Hebb's experiments with rats had provided the neurophysiological grounding for visual perception and learning, Penfield's patients for intelligence, and now chimpanzees for emotion. The study of the affect of experiment was part of a broader turn towards the emotions of laboratory animals during physiological encounters (Dror, 1999; Young, 1998). At a time when emotion signified the collapse of the laboratory's ideal of reliable control, replicability, and standardization, Hebb's neurophysiological interpretation of chimpanzee emotion represents an attempt to reconcile a broader late nineteenth century crisis of knowledge in physiology (Dror, 1999).

It was during this period in the mid-1940s that Hebb formulated many of the ideas that would consist of the neuropsychological framework proposed in *The Organization of Behavior* (Hebb, 1980; 1949, p.viii). Beyond Lorente de Nó's (1939) contribution, Hebb's specific

influences during this period are not clearly stated. They likely emerged from the hodgepodge interdisciplinary dialogue during which "weekly colloquium and persistent theoretical debate" provoked by the challenges presented by the behaviors of laboratory animals. It is interesting to note that it was through theory-laden neurological speculation that explanations for his observation of chimpanzees were initially sought (e.g., Hebb & Riesen, 1943; Hebb, 1946c). Perhaps the challenges presented by these experimental subjects, due to their human-like social and emotional complexity, demanded a more thorough explanation than previous subjects (i.e., rats). Or, given the many setbacks Hebb experienced, he simply had more time to dedicate to completing his manuscript in this highly collegial atmosphere.

Long before the publication of *The Organization of Behavior* (1949) Hebb, like other psychobiologists of his time, was dedicated to the project of bringing psychology into closer alignment with the biological sciences. Hebb's background and training uniquely positioned him to address problems which he understood to be, at their core, an issue of disciplinary integration. It is important to note how unusual Hebb's particular training in physiological psychology was at the time, especially for a Canadian psychologist (there were few graduate training programs in Canada, none dedicated to this area). Behavioral science was devoid of contemporary physiological research and the means to bring neurophysiological research into greater contact with contemporary psychology was not readily available. This section attempted to show how Hebb came to embrace physiological thinking during a time when psychologists tended to eschew such associations in favor of some form of neobehaviorism in academia and Freudianism in the clinic. In the next section, I discuss how psychology came to look upon physiology with suspicion (as a threat to the discipline) and how Hebb's concern about bringing psychology back to its roots was a reaction which embodied the struggles of Canadian psychologists at a time critical to the discipline's history.

5. Hebb's Neuropsychological Vision

Hebb's vision for psychology in the postwar period can be represented by a number of persistent concerns and recommendations for psychological theory and practice (e.g., Hebb, 1948; 1949; 1951). Developed over the previous decade, Hebb's vision was outlined in a series of materials that he published shortly after arriving at McGill in 1947. These include articles (e.g., Hebb, 1951), public addresses (Hebb, 1953, 1960), and an introductory textbook (Hebb,

1958). Hebb's most thoroughly articulated account of his vision was presented in his influential 1949 monograph *The Organization of Behavior: A Neuropsychological Theory*, which represents the culmination of Hebb's thinking about the relationship between psychology and neurophysiology during his time with Penfield and Lashley. In these works he returned to themes essential to understanding both Hebb's view of the major challenges facing a Canadian scientific psychology and his perspectives on potential solutions. As developments in the Department of Psychology at McGill are examined in the next chapters, this vision will provide the context for understanding some of the ways psychology at McGill took on its particular characteristics during this period.

Three themes formed the backbone for the neuropsychological theory Hebb promoted: 1) the importance of explicit theory, grounded in neurophysiology, 2) the use of neurologically-informed models, or "hypothetical constructs," in the development of these theories and 3) a more thoroughly interdisciplinary psychological science. These themes are highly interrelated and are based on Hebb's overarching program to "bring [the discipline of psychology] back to an essential concern with the physiological-psychological problem," as he understood it (Hebb, Mar 31, 1947). A close examination of Hebb's vision, as articulated in his published material of the late 1940s and 1950s, helps illuminate, 1) why and how Hebb believed psychology had moved away from "the physiological-psychological problem" in the first place, 2) why Hebb thought it was paramount to the future of the discipline to bring it back as psychology's primary concern, and 3) how Hebb conceived of his own role both in encouraging fellow psychologists to re-think the orientation of their field and in using his position of chairman of the department at McGill to bring about his vision in practice throughout the 1950s.

5. a. The role of explicit theory in Hebb's vision for psychology

In Hebb's theoretically-oriented works of the postwar period he often emphasized the internal schisms between competing schools of psychological thought (e.g., Hebb, 1949, 1951, 1953), particularly in accounts of learning and perception. The challenge, as Hebb saw it, was the reconciliation of the phenomenal world of psychology (as emphasized by Gestalt psychologists like Wertheimer, Koffka, and Köhler) with the American psychological tradition of behavior through reinforcement (as emphasized by neobehaviorists like Hull, Skinner, Spence, and to some degree Holt and Watson). Hebb typically referred to these two groups as "S-S theorists" and "S-R theorists" (Hebb, 1951; Spence, 1950), respectively. Hebb saw himself as an outsider;

pitted against these two extremes. Hebb's own ideas, like those of Lashley and Tolman, did not fit soundly into either camp (Carroll, 2017). Hebb's solution was a re-visioning of general theory at the core of psychology, a natural science co-extensive with physiology and neurology. Hebb's research program in the 1950s (see Chapter Five) speaks to the kinds of problems that are raised by the synthesis of biological and psychological experimental principles.

Hebb was among many psychologists who framed North American psychology in the 1940s as divided roughly into these two groups. In an address to the APA in 1948, Spence discussed what he saw as the main distinctions between cognitive (S-S) and stimulus-response (S-R) theories of learning (Spence, 1948; 1950). Cognitive theories of learning "emphasized the formation and modification of cognitive patterns representative of the relationships in the environment" (Spence, 1950, p. 161). For the most part, within these theories, such as those of Koffka (1935), Köhler (1940), Lewin (1936), and Tolman (1932), learning was construed as part of a larger problem of perceptual organization and reorganization with experience. By contrast, stimulus–response theories, such as those of Guthrie (1935), Hull (1943), Spence (1936), and Thorndike (1898), emphasized such constructs as habits and S–R bonds, which referred to hypothetical learning states or "intervening variables." S-R theories provided rules relating stimulus factors (such as reward magnitude, number, and timing) to the strengths of those intervening variables as well as rules relating them to empirical response measures (such as frequency and degree). On the whole, Spence saw few points of disagreement between these two theoretical positions and attributed most of the disagreement between the camps to misinterpretation of S–R theory by cognitive theorists (Holland, 2008). In his overview of theorists affiliated with these positions, researchers outside this dichotomy (such as Lashley and Hebb) are not mentioned in Spence's review (i.e., they do not fit neatly into either camp).

Hebb emphasized the importance of recognizing the tradition of the S-R theories, which for him, begins with realist-empirical philosophers Thomas Hobbes (1588-1679), John Locke (1632-1704), and John Stuart Mill (1806-1873), extends to Wundt and Titchener; and later, in a closely related line, to Thorndike, Watson, Holt, and Hull: "These men were narrow; they were wrong; and without them, without the simplification they achieved, modern psychology would not exist" (Hebb, 1953, p. 101). Developments in 1930s neurophysiology, however, were outstripping the abstract utility of the S-R theorists; for a thoroughgoing theory of (neuro-)psychology, a more precise model was needed (Hebb, 1949).

Hebb identified important limitations to both the S-S and the S-R approach to learning (Hebb, 1949). Experimental and clinical data presented persistent problems to each approach, the source of which Hebb believed was the failure of psychology to handle the complex problem of cognition and the failure of neurophysiology to adequately describe the nature of cortical transmission (Hebb, 1949). These limitations, he believed, contributed to the weakness of psychology to scientifically understand the organization of their object, human consciousness. Hebb's vision developed as a direct response to these problems: the need to reconcile mainstream behaviorism with the criticisms leveled against it by Gestalt psychologists, psychobiologists like Lashley, and early cognitivists like Tolman (Carroll, 2017). To do this, Hebb emphasized a revised role for psychological theory, based on recent findings in neurophysiology and constructed in terms of explicitly physiological, but easily generalizable, neurological models (Hebb, 1949).

The Importance of Theory for Psychology

Much of Hebb's vision for psychology was in response to the status of psychology as a science in the 1930s, which had transformed in the early twentieth century from a science of consciousness (characterized by approaches such as structuralism and functionalism) into a science of behavior (O'Donnell, 1985). This school of thought maintained that consciousness could not be directly observed and therefore could never adequately serve as the basis of an experimental science. Hebb traced this tradition back to Pavlov's conditioning studies at the turn of the century, through Watson's behaviorism and to the influential work Skinner and Hull in the 1940s. Hebb described his own frustration with proponents of the S-R theory in terms of their tendency to downplay or dismiss the relevance of a rigorous theoretical model, which necessarily included some role for cognition, or, as it was described at the time, 'autonomous central processing' (e.g., Hebb, 1946c). For Hebb, they had thrown the baby out with the bathwater; the cost for simplicity in light of underdetermined theory was the excision of coherent grounding in structural materialism (i.e., neurophysiology): “One cannot logically be a determinist in physics and chemistry and biology, and a mystic in psychology” (Hebb, 1949, p.xiii).

The state of the discipline in the postwar period concerned Hebb. The introduction to *The Organization* (1949) and the main topic of subsequent theoretical writings (e.g., Hebb, 1951) emphasized the perils of aversion to explicit theory-building. Hebb saw this aversion as characteristic of psychology in the 1930s, and commonplace in the field by the late 1940s. While

he admired Hull's systemic theory-building (Hebb, 1980), he firmly believed neobehaviorists were incapable of satisfactorily integrating either the findings from Gestalt psychology or new physiological evidence being produced in the areas of perception, motivation, and learning (Hebb, 1953, 1960): Hebb noted, “if we cannot deal with the comparatively simple behavior of animals without taking account of thought, how adequate can a thought-less human psychology be?” (Hebb, 1953, p. 99). This lack of adequate theory was rendering the discipline intellectually stagnant. Hebb reminded his readers, “theory in some form is the only source of order in dealing with facts” (Hebb, 1953, p. 101). Hebb saw the work of scientific psychology as explaining higher-order processes using theories that accounted for physiological data. After all, as Hebb pointed out, “Theorizing at this stage is like skating on thin ice--keep moving, or drown" (Hebb, 1949, p. xii).

Hebb was not only worried about explicit efforts to eliminate the practice of theory-building in psychology, he was also troubled by the associated unconscious tendencies he saw in the uncritical uptake of ‘new’ ideas. In particular, he was concerned that an over-reliance on outdated theories had stymied thinking in psychology. In the preface to his first book, he reminded his readers that, "It is too easy, no matter what formal theory of behavior one espouses, to entertain a concealed mysticism in one's thinking about the large segment of behavior which theory does not handle adequately” (Hebb, 1949, p. xiii). Furthermore, he worried that “[barriers to thought] are so effective that they efface themselves completely. As psychologists we must look at them, however, pleasing or not” (Hebb, 1953, p. 107). Hebb saw himself as an unaligned intermediary, sympathetic to the criticisms of the cognitivists while unabashedly aligned with the methodological behaviorists. He believed this position as an intermediary (more so than his background and training in neurophysiology) granted him a more balanced (and therefore more scientific; Daston, 1995) perspective on how to proceed in the new landscape being established for postwar behavioral science (Isaac, 2007; Crowther-Heyck, 2006).

The vision put forward in *The Organization of Behavior* (1949) was for psychology to become a theory-driven science of mind. What made his vision novel in the middle of the twentieth century was that it provided an alternative to the dominant theory-deprived science of behavior; Hebb maintained it was cognition, not behavior, which was “indubitably the central problem of psychology—at once the most difficult and the most important that scientists face” (Hebb, 1953, p. 99). Hebb's postwar writings characterize disciplinary psychology as being at a

crossroads. Hebb's public statements (e.g., 1953; 1960) identify the issue as that of an unconscious, over-reliance on outdated physiological theories as the basis of modern (i.e., predominantly S-R) psychology. Hebb's vision was informed by Gestalt/cognitive criticisms of behaviorism, many of which were not yet well-known by mainstream behaviorist psychologists in America. This fostered in Hebb the total embrace in the value of presenting theoretical ideas in preliminary form, on the presupposition that they would ultimately lead to better theories. This attitude of congenial cooperation among disciplines was likely formed as a result of the theoretically engaging environments Hebb had enjoyed and were fostered by his mentors (Hebb, 1980; Glickman, 1996).

Hebb's experiences with Lashley and Penfield had demonstrated that the scientific imagination was fundamentally guided by theoretical considerations and limited by assumptions. Whether available theories promoted the development of new and useful ideas, or whether they contributed to the limitation and stagnation of current thinking, depended on the accuracy of these explanatory models in light of available (neurophysiological) evidence. An historical understanding of the development of theory was paramount: "The idea that was useful at one stage in the development of a theory may become constricting later, and its deleterious effects may only be recognized if one known where it came from" (Hebb, 1958, p. 316).

Hebb was keenly aware of the limitations of available psychological theories in the interwar period and the over-reliance on assumptions underlying S-R theories. Now with new insight, Hebb re-interpreted the findings of his dissertation; he reconciled clinical data with existing understandings of the relations between structure and function in humans. Likewise, in his work with chimpanzees, he found their behavior amenable to rudimentary explanation by way of a neuropsychological theory (i.e., Hebb, 1946c). The reception of these ideas supported a comprehensive neuropsychological theory of mind (Hebb, 1949) able to account for the activities of *autonomous central processing*.

Reliance on pre-existing theories as implicit to scientific practice and progress was something Hebb understood: "it is only with the rubble of bad theories that we shall be able to build better ones [...] without theory of some kind, somewhere, psychological observation and description would at best be chaotic and meaningless" (Hebb, 1951, p. 39). While at the MNI the experiments that Hebb conducted cast doubt on long-held assumptions about adult intelligence; and in recognizing the limitations of existing theory, Hebb recognized the need to expand (rather

than avoid) further theoretical work. This insight, Hebb recognized, was based on his own experiences with these notions (i.e., his training with Lashley) and understanding of and familiarity with the history (genealogy) of ideas relevant to disciplinary theory: “the idea that was useful at one stage in the development of theory may become constricting later, and its deleterious effects may only be recognized if one knows where it came from” (Hebb, 1958, p. 316).

Hebb firmly held that psychological understanding required two things: an intelligible theory of the phenomena and the capacity to recognize its incompleteness. Scientific understanding was “the impact of theory on perceived reality” (Hebb, 1953, p. 99). Hebb was explicit about the need for productive psychological theory to play a central role in scientific understanding; this theory should be based on the bridging of disciplines through a unified theory (i.e., the mutual reliance on models and evidence across physiology and psychology). The disciplines Hebb had in mind were keenly associated with the medical sciences (e.g., neurophysiology and biochemistry). Hebb's vision for psychology was based on a shared reliance on evidence emerging from these hybrid disciplines (of which psychology belonged). It was only starting to be understood in the 1940s how this evidence should come to bear on questions of psychological importance.

Lashley and Hebb on Theory-building

The relationship between Lashley and Hebb was complex and multifaceted (Dewsbury, 2002; Bruce, 1996). Hebb had worked with Lashley across three different settings over the course of almost a decade. It is difficult to directly trace Lashley's influence on Hebb's thinking as Lashley's standing as a hard-nosed scientist drew to him a cadre of similarly minded physiological psychologists from whom Hebb would develop his perspectives on the scientific possibilities of a neurologically-informed psychology (Dewsbury, 2002; 2006). For example, Hebb seems to attribute more to weekly colloquia and persistent theoretical debates while working with Lashley, than to Lashley's ideas themselves (Hebb, 1949, p.viii). Nevertheless, Lashley's works are cited throughout Hebb's publications and it is evident that the psychological problems with which Hebb struggled (i.e., intelligence, cognition, visual perception) were framed in terms of his mentor's contributions to this field (e.g., Lashley, 1933, 1938; Orbach, 1998).

Lashley's disagreement with behaviorist thought, and especially that of Clark Hull in the 1930s and '40s, was complicated (Weidman, 1994; 1999). Based on the experiments Lashley had conducted in the 1920s, he was convinced the stimulus-response version of brain function (i.e., associationism) could not fully account for the phenomena he observed: ablation did not reliably correspond to cognitive deficiency in laboratory animals. He consequently rejected the idea that brain functions themselves (such as *the engram* in the case of memory) must be isolated to particular regions of the brain (localizationalism). These were among the core tenets of behaviorism (O'Donnell, 1985). Therefore, in place of behaviorism, Lashley suggested the brain functioned as a whole through the patterning of stimulation, there were no discrete brain compartments specific to certain functions (Lashley 1929). This was a view held by Lashley the rest of his life (Weidman, 1994). Lashley's disavowal of behaviorism and subsequent conflict with Hull were ways of keeping psychology close to biology and to genetics (Weidman, 1999). Yet, Lashley remained largely opposed to using physiology as evidence in psychology: In a letter to Edwin Boring (1886-1968) in 1963, Hebb described Lashley as having been "the main cause of Psychology's loss of interest in physiology for a quarter of a century" (quoted in Glickman, 1996, p. 233). Lashley (1930) believed there were "few principles [in neurophysiology] from which we may predict or define the normal organization of behavior, whereas the study of psychological processes furnishes a mass of factual material to which the laws of nervous action in behavior must conform" (p. 24). From the mid-1920s onward, Lashley rejected a collection of popular behaviorist theories, and by the 1950s was rejecting the notion that *any* theory could explain the complexities of human psychology.

Hebb did not share this view. Although Hebb was also deeply skeptical of the direction mainstream behaviorism had taken the discipline (e.g., reluctance to speak openly about cognitive processes), he believed modern psychology was merely in need of revision, or re-orientation. Hebb was drawn to the appeal of a systematic, and unified theory of mind, demonstrated in his frequent defense of S-R theory (e.g., Hebb, 1951, 1953, 1960), part of a broader organizational revolution taking place in the 1940s and 50s (Heyck, 2015).

An important part of Hebb's vision was the emphasis on theory development that he felt psychologists were being discouraged from exploring (Hebb, 1949, 1953). Indeed, the importance of his book (1949) is often attributed to Hebb's decision to put forth a theoretically-useful (albeit incomplete) framework able to effectively synthesize existing physiological and

psychological research (e.g., Glickman, 1996; Fentress, 1999; Brown & Milner, 2003). It is plausible that this concern about the practical utility of tentative speculation arose through close collaboration with Lashley (Bruce, 1999).

Lashley is often portrayed as having been anti-theoretical (Weidman, 1994; Hebb, 1980). Hebb was not only concerned with the possibility that existing theory was inadequate, but also "the equally sterile posture of the hypercritical," noting an equivalent threat to psychological theory coming from "psychologists who can accept no theory that is not perfect, the ones who either do not experiment at all because they are waiting for the perfect experiment or experiment only to show that existing theory is no good" (Hebb, 1953, p. 101) – the latter perhaps referring to his mentor Lashley who would set forth several theories "only to dismantle them"—without feeling compelled to offer one of his own (Weidman, 1994, p. 164). Hebb would later declare that "Lashley was so interested in criticizing others' theories and so little interested in developing one himself […] He made a career of finding things that were hard for others to explain, and he was fond of saying that he had destroyed all theories of behavior, including his own" (Hebb, 1980, p. 297). However, Bruce (1996) has argued Lashley's early lack of support for Hebb's neuropsychological vision (e.g., he declined to co-author *The Organization* in 1946) had more to do with disagreements with its substance than with a preoccupation towards criticizing existing theory. Nevertheless, the bias against theory that Hebb encountered throughout his training seems to have positioned him well to adopt an openness towards speculative theory-building. This became central to his vision for psychology and that which often distinguished his thinking from his contemporaries. Given the retrospective importance of this stance for the so-called "cognitive revolution" that would come to pass in the next decade, it would seem Hebb was indeed responding to a broader trend towards organizational and systems-thinking within the discipline (Heyck, 2015).

5. b. Integrating Neurophysiology: The Use of Hypothetical Constructs in Psychology

The role of neurological models played a key role in Hebb's vision for psychology moving forward after the war. The use of these kinds of models for explaining psychological phenomena was based on a particular form of operational analysis adopted by psychologists in the early twentieth century (Green, 2000). Psychological models require shared understanding and something approximating disciplinary theory. Hebb believed psychology lacked adequate disciplinary theory and that, "understanding will come only as our inadequate models of man's

thought become more adequate" and, while understanding is not restricted to models, "it still stays within hailing distance, and can only advance as they advance" (Hebb, 1953, p. 106). The attention to models and description of processes in terms of systems is characteristic of trends in 'high modern social science' within a bureaucratic worldview (Heyck, 2015) and relied heavily on the viability of the concept of hypothetical constructs and intervening variables (see MacCorquodale & Meehl, 1948; Hyland, 1981).

Theoretical concepts in psychology are generally referred to either as hypothetical constructs or as intervening variables, although this distinction is sometimes ambiguous. MacCorquodale and Meehl (1948) distinguished between these terms; they suggest that *intervening variables* should be used for analytic or abstract theoretical terms whereas *hypothetical constructs* be used for existential or hypothetical terms. The fundamental difference is that hypothetical constructs refer to things that are proposed to actually exist and intervening variables refer to things which merely refer to abstractions from data. While it is not entirely clear *how* some hypothetical constructs are supposed to exist, the implication of their 1948 paper was that they should have some form of physiological representation (i.e., exist in the same way that nerves exist). An intervening variable is an abstraction from a measure of behavior (e.g., intelligence is what intelligence tests measure) which is operationally defined whereas a hypothetical construct exists independently of the test used to measure it (e.g., intelligence tests measure intelligence); a hypothetical construct can be operationalized but not operationally defined (Hyland, 1981).

The hallmark of neobehaviorist explanations was that theoretical terms should be limited to their operationally defined terms, that is, limited to intervening variables. Hebb's vision, however, called for greater attention to the role of models and the proposal of hypothetical constructs (e.g., cell assemblies) for developing theory in psychology: "we must ask of each new model what effect it has on the way in which we see the whole broad range of human behaviour, what guide it can offer us in the approach to reality" (Hebb, 1953, p. 110). Hebb's vision offered both a compelling argument about how developments in associated fields could be used to extend existing psychological explanations (e.g., the notion of cell assemblies) and a method for constructing theories of brain function. These methods likely emerged from Lashley and Penfield's use of clinical case studies in theory-building (following the pathological model of other disciplines in medical science, such as experimental physiology). However, before Hebb

would be able to convince his peers and students of the value of hypothetical-neurological constructs, he would have to address the long-standing bias against "physiologizing" in psychology.

The Use and Abuse of Physiology in Psychology: "Physiologizing"

An analysis of Hebb's most influential works suggests the theoretical issue at stake tended towards the appropriate relationship between psychology and physiology (as representative of *the* modern laboratory science). If the subject matter of psychology is the mind and it is a science insofar as it is able to provide understanding, then to what degree and in what ways does our understanding of physiology meaningfully inform this understanding? For much of the history of scientific psychology this question was not clear. Eventually psychologists decided their subject matter was not the mind itself (i.e., consciousness), but rather the study of behaviors (Watson, 1913; O'Donnell, 1985). This view persisted throughout the first half of the twentieth century. While perhaps unintuitive today, the question of whether or not evidence derived from (or in terms of) physiology would help or hinder the development of psychology was openly debated. Using physiology as the basis of psychological explanation had earned a bad reputation in the early 1920s (e.g., Bevan, 1958), and little was done to improve this situation. The obligation to connect academic psychological theory (i.e., neobehaviorism) to physiological understandings was largely seen as passé by the 1930s, the derogatory term used to describe attempts to explain (or reduce) psychological constructs in terms of physiology was known as *physiologizing*.

Prior to the 1950s, physiologizing in psychology was to be avoided, there was an "anti-physiological point of view" taken up by those who had "renounced the shackles of neurology" (Hebb, 1951, p. 40). There were several reasons for the turn against neurology: For one, physiological evidence was sparse and few psychologists (outside a specific group; see Dewsbury, 2002) were trained to understand, interpret, and produce this kind of knowledge. Moreover, such disciplinary divisions in expertise between psychologists and the medical professions were constructed so as to protect the professional status of medicine. Secondly, it was difficult for psychologists to interpret the contribution of physiology and neurology to their theories as anything other than a hindrance. The views of this "vigorous movement" (as Hebb described it) against physiology in psychology were supported by prominent psychologists in the 1930s. For example, in *The Behavior of Organisms* (1938) Skinner said: "I venture to assert that

no fact of the nervous system has as yet ever told anyone anything new about behavior […] It is towards the reduction of seemingly diverse processes to simple laws that a science of behavior naturally directs itself. At the present time I know of no simplification of behavior that can be claimed for a neurological fact" (Skinner, 1938, p. 425). Similarly, Lashley expressed publicly a rather pessimistic view towards the inclusion of physiological evidence. In an address as President of APA in 1929 he said, "The chapter on the nervous system [in textbooks] seems to provide an excuse for pictures in an otherwise dry and monotonous text. That it has any other function is not clear" (Lashley, 1930a, p. 1). This seemed to some (e.g., Hebb, 1980) to provide justification to those who wished to promote a purely behaviorist psychology without ties to theories in physiology.

There was also the simple reality that little neurophysiological work was done with psychological theory in mind. There was little neurophysiological data to connect meaningfully with the problems of psychology, even the most basic functions such as learning, perception, and memory. For example, in comparing the neurophysiological models used by psychologists to atomic models of physicists, Spence (1950) wrote that unlike the physicist who uses these models merely for expository purposes and does the real theorizing in terms of mathematical constructs, "the psychological theorist is more often likely to have only the model […] one does not usually have any theory at all but only a simple analogy which more often than not explains nothing" (Spence, 1950, p. 160). Spence argued the physiological basis of reflexes did little to inform the construction of S-R theories. Indeed, Spence (1950) stated that "picturing neurophysiological processes without specifying the hypothetical relations [...] is almost a complete waste of time so far as furthering our understanding of learning" (p. 164).

Part of Hebb's success in spreading his ideas was in recognizing the interdependence of psychological and physiological concepts. A viable means for disciplinary communication was becoming possible, and therefore, necessary for the advancement of certain questions of mind: "It is not necessary that the student of personality talk in neurological terms, but his terms should be translatable when necessary into neurology. Physiologizing is not a substitute for psychology but an aid to it" (Hebb, 1951, p. 53). Hebb's thinking about the relationship of psychology to physiology was influenced by Horace English's article on "The Ghostly Tradition and the Descriptive Categories of Psychology" (1933) (Hebb, 1951). Essentially, English argues that psychology and physiology study two abstractly distinct aspects of organic process (English,

1933). Their distinction is an abstraction based on how they are considered as part of integrated wholes rather than the way they are in and of themselves. This view contributed to Hebb's notion of synthesis of explanation between and across disciplines (e.g., Hebb, 1951). For English, and presumably Hebb, "conceiving the interaction of mental processes with the physiological […] enables us to avoid the pitfalls of a metaphysical dualism which has for so long confused the terminology of psychology" (English, 1933, p. 513). Hebb notes that mental processes and neural function "are perfectly correlated" such that "one is completely caused by the other" (Hebb, 1949, p.xiii).

Going back to his graduate studies at McGill, Hebb seems to have had a keen interest in the effects of environment on the development of neural activity and its organization. Hebb's later experiments, like those of Lashley, had attempted to use techniques derived from experimental physiology (such as selective extirpation) to shed light on psychology's enduring problems (e.g., nature versus nurture); he had shown that which we would consider "hard-wired" to have been subject to a process of *developmental organization*. The key to understanding the organization Hebb and his colleagues observed, was in meaningfully bridging the gap between psychology and neurophysiology. In doing so, Hebb hoped to draw psychologists back to a science of mental life by demonstrating that even a speculative linkage with the nervous system (or the *conceptual nervous system*, as he later called it; Hebb, 1955) could indeed be scientifically fruitful for progress.

According to Hebb, arguments against the integration of physiological evidence and theory into psychology were rooted in problems of modern positivism that emphasized 'explanation' as a statement of relationships between observed phenomena (Hebb, 1949). Hebb stated that his book was "written in profound disagreement with such a program of psychology" and pointed to the critical work of Pratt (1939) and Köhler (1940). While logically defensible, the anti-physiological position seemed to Hebb to stem from a misconception about the early stages of the scientific method, it "preached a counsel of perfection, disregarding the limitations of human intellect" (Hebb, 1949, p. xv). Psychologists are in need of physiological theory to develop into a full-fledged science because "the problem of understanding behavior is the problem of understanding the total action of the nervous system, and *vice versa*" (Hebb, 1949, p. xiv).

Hebb believed the problem was that when Watson, Thorndike, and Holt were establishing the fundamentals of S-R theory, the nervous system was thought to be a set of paths running from receptor to effector, some longer and less direct (i.e., through the cortex, in the process of learning), but all one-way streets; no back connections, no feedback within the system, no looping circuits in which an excitation could maintain itself without sensory stimulation; all neural transmissions had to be straight through, from sense organ to muscle or gland. *The Organization of Behaviour* (Hebb, 1949) brought together lines of psychological and physiological evidence that completely revised this understanding.

From Hebb's perspective, psychologists who failed to connect their theories to existing physiological understanding ignored developments that affected the entire discipline. The irony Hebb notes, is that those averse to physiologizing do so regardless, they are simply unaware of their doing so; the "tacit preoccupations about neural function" of S-R theorists are now "outmoded" (Hebb, 1953, p. 103). The preeminence of S-R theory is no longer warranted. As examples of the ways that neurophysiology was directly affecting the discipline of psychology, Hebb pointed to the work of Hans Berger (1873-1941; inventor of EEG), British electrophysiologist Edgar Adrian (1889-1977), and Spanish neurophysiologist Rafael Lorente de Nó (1902-1990). He noted the pioneering work into specific and non-specific thalamic projections of Morison and Dempsey (1942) as fundamentally challenging older "sense-dominated" theories (Hebb, 1953, p. 103). In Hebb's presidential address to the CPA in 1953 he emphasized to the audience that "it is no longer true—even remotely—that the primary effect of physiological ideas in psychology is limiting and stultifying" (Hebb, 1953, p. 104). After all, "physiological ideas have been a stimulant in the past [and] does have an essential connection with it--which may be good or bad, depending on whether the relation is recognized" (Hebb, 1958, p. 321).

Hebb notes that despite the value of the S-R formula, it has had a limiting effect when its neurological origin was forgotten by psychologists who thought that neurological ideas were unimportant and who thus were unconcerned that subsequent developments in neurological knowledge had considerably modified the significance of the stimulus-response idea (Hebb, 1949; 1951). Hebb pointed out in his textbook: "Neurological theory has been a stimulant, providing us with basic conceptions for analyzing behavior […] devising new experiments to test the theory, but it has also been limiting: more limiting to those who *believed* S-R theory, less so

to others" (Hebb, 1958, p. 316; emphasis original). In particular, Hebb (1951) refers to an article by Roger Loucks (1941) in which he argues the contributions of a physiological approach to a science of behavior have been downplayed due to fundamental misunderstandings. Loucks concluded, and Hebb concurred: "The data of neurophysiology adds to the sum total of knowledge; they cannot be ignored" (Loucks, 1941, p. 120). Referring to a focus on the internal organization of thought, he goes on to argue "there is not only every justification but an imperative need for a more extensive development of the internalistic approach" (Loucks, 1941, p. 124) as opposed to the externalistic approach taken up by strict behaviorists. Furthermore, Hebb believed that physiologizing in psychology was not only necessary, but in many ways, was inevitable (Hebb, 1951). *The Organization* (1949) was laudable in part because it offered a tentative roadmap to a necessary next step (i.e., the integration of cognitive processes) for neurological psychology.

The main point of Hebb's article on the relationship between psychology and neurology (1951) was that if we, as psychologists, are necessarily influenced by developments in physiology, then it behooves us, as scientists, to make use of the best available evidence from physiology. As Hebb noted, "if we must neurologize"-- and he believed they did --"let us use the best brand of neurology we can find" (Hebb, 1955, p. 243). Hebb argued that positivists who objected to physiologizing on grounds that physiology had not significantly helped psychological theory were simply neglecting the fact that much work had been done in neurophysiology since the 1920s and "psychology has not yet assimilated these results in full" (Hebb, 1949, p.xvi). By earnestly adopting recent developments in related disciplines, Hebb believed he had found the path towards a richer psychological science: He figured out how to "make it possible to regain some of James' breadth without losing the benefits gained from [Ramon y] Cajal and Sherrington and built into psychological theory by the litigation of Hobhouse vs Thorndike and Lashley vs Pavlov" (1951, p. 41).

Hebb's studies on the effects of brain lesions had complicated notions of adult intelligence but had failed to illuminate the "autonomous central processes" involved in learning, memory, perceptual constancies, and form recognition within the confines of a connectionist nervous system. Hebb's article, *On the nature of fear* (1946c), represents the first significant attempt to incorporate various psychological concepts (such as set, attention, and expectancy) into a broader account of behavior explained in terms of neurological functioning; an explicit

(and risky) attempt by Hebb to demonstrate that physiologizing can, despite the attitudes at the time, be useful. Hebb was emboldened in this approach during his time at Orange Park by the work of Edward Tolman (1886-1959) and Lorente de Nó (1902-1990), both of whom would come to have an important contribution to the ideas presented in *The Organization of Behavior* (1949).

Tolman and Hebb on Neuro-hypothetical Constructs

The work of Edward Tolman (1886-1959) strongly influenced Hebb's thinking about cognitive processes in the brain (Hebb, 1949; 1951). Hebb believed what Tolman offered psychologists was "a modification and synthesis" of the physiological thinking of the Gestalt psychologists with that of Watson and Holt – which Hebb described as being "superficially incompatible approaches, both of which were affected in their main outlines by ideas of neural function" (Hebb, 1951 p. 43).

Behaviorists took great pains to distinguish their field from other sciences, particularly from biology (O'Donnell, 1985); they feared the perception that "the science of behavior must at bottom be a study of physiology" (Hull, 1943, p. 19). In order to defend psychology's autonomy, many behaviorists followed Tolman's lead in distinguishing between "molecular" and "molar" actions (Tolman, 1932). Biology, they argued, should study the molecules of nerve physiology, whereas psychology should focus on the larger molar units of behavior. Hebb shared Tolman's formulation but argued that both the molecular (physiological) and molar (behavior) are essential to understanding "the total action of the nervous system, and vice versa" (Hebb, 1949, p. xiv). Psychological constructs that were invented and subsequently refined in the context of studying behavior do not depend on first knowing how the brain functions fundamentally. Instead, we can learn about how the brain functions, Hebb argued, from the behavior itself, beginning with these psychological constructs: "One adds to or may correct the other" (Hebb, 1958, p. 318). Hebb goes on to note that "logically, the physiological and anatomical data have priority in the analysis of brain function, but conclusions drawn from these data alone may be wrong and may be corrected by the evidence from behavior" (p. 318). Hebb's proposed method involved learning as much as one can about how parts of the brain function (primarily the physiologist's field), and relating behavior as far as possible to this knowledge (primarily for the psychologist); then seeing what further information is to be had about how the total nervous system works, from the

discrepancy between (1) actual behavior and (2) the behavior that would be predicted from adding up what is known about the action of the various parts (Hebb, 1949, p.xv).

In developing his theory, Hebb drew upon the research of not only Lashley, Tolman, but also Köhler and Krech (Krechevsky). Like Lashley and the Gestaltists, Hebb saw himself as a rebel-outsider more than psychological-theorist per se (Hebb, 1953, p. 102). One of the main points of Hebb's book (1949) was that learning results in structural changes to the nervous system in the form of cell assemblies, or networks of physical nerve cells. Much of his book was necessarily speculative, given the state of knowledge of the central nervous system at the time, but it was provocative about what clinical data could provide experimental psychologists.

Tolman had presented a neurological model in 1918 shortly after taking a position at the University of California, Berkeley but within a few years he had concluded that physiological models were premature and set out to build what he described as "non-physiological behaviorism" (Tolman, 1925; Carroll, 2017). He did, however, continue to follow developments in physiological psychology. He was especially interested in Lashley's line of research with rats in the 1920s and incorporated his lesion studies in an early review of the literature on higher mental processes in animals (Tolman, 1927). He credited Lashley's studies of transfer (Lashley, 1924) and mass action (Lashley, 1929) as major influences on his research on latent learning and adopted a similar stance on the limitations of physiological models (Tolman, 1938; Lashley, 1930).

According to Krech, Tolman's shift in thinking about brain models (e.g., Tolman, 1949) was a consequence of his endorsement of hypothetical constructs: "The reason Tolman is willing to suppress his qualms is, obviously, not that he believes that neurology now really has come of age and has all the necessary facts which it lacked a number of years ago, but rather he has realized that *if he is to work with hypothetical constructs* he must define his constructs neurologically—whether neurology is ready for us or not" (Krech, 1950, p. 289; emphasis in original). By the mid-1940s, Tolman was beginning to entertain some notions about the physiological substrate of some of his concepts. For example, in his famous 1948 article he made explicit reference to the nervous system. A year later, the same year *The Organization of Behavior* was published, Tolman publicly acknowledged he had changed his mind on physiological psychology: "for many years I have objected to what I have conceived to be premature neurologizing. It seemed so obvious that psychology was handicapped and led astray

by the narrow neurological concepts which it took over uncritically from, say, the physiologists's account of decorticate frogs" (Tolman, 1949, p. 48).

Hebb read Tolman's change of mind as vindication of his long-held commitment to the integration of physiology and psychology. His article, *The Role of Neurological Ideas* (1951), is an attempt to bolster Tolman's (1949) views. Indeed, Hebb's vision for experimental psychology emphasized an endorsement of the notion of hypothetical constructs which entailed a commitment to exploring their neurological substrate, the 'neuro-hypothetical.' This was Hebb's basis for his argument in favour of neurologizing which he had articulated in *The Organization* (1949). Hebb was right about Tolman: In writing to Hebb, he made no apologies for his earlier stance, but readily admitted that his views had changed substantially: "I certainly *was* an anti-physiologizer at that time and am glad to be considered as one *then*. Today, however, I believe that this (physiologizing) is where the great new break-throughs are coming and to you belongs much of the credit" (Tolman, 1958, as quoted in Carroll, 2017, p. 187).

Tolman and Hebb worked towards and achieved similar goals. They contributed studies that helped transform accounts of learning, a fundamental issue in psychology in the 1930s and 1940s, from conditioned responses to cognitive processes. Equally important, these men demonstrated that it was possible to study cognition within a behaviorist framework, combining the methodological approach of behaviorism with a cognitive interpretation of behavior. That demonstration was a key factor in the resurgence of cognitive psychology in the 1950s and early 1960s (Carroll, 2017; Gardner, 1985; Baars, 1986).

Lorente de Nó's reverberatory circuits and the limits of the S-R theory

Where Lashley and Hull clashed (Weidman, 1994), Hebb and Hull seemed to agree: there needed to be some kind of symbolic representation of thought (intentionality) for a comprehensive explanation of cognition (see for example, Green, 2000). To guard against the tendency to introduce mentalistic terms into psychological analysis, Hull had suggested a "prophylaxis against anthropomorphism" (Hull, 1945). This prophylaxis was to regard "the behaving organisms as a completely self-maintaining robot, constructed of material as unlike ourselves as may be" (Hull, 1945, p. 27). Hull wanted to move the study of mind away from the brain using analogous mechanical models (Weidman, 1994) and Hebb wanted to establish these models specifically in the brain (Hebb, 1949). This is an important distinction that would come to characterize the kind of cognitive neuropsychology Hebb would come to espouse; whereas

later cognitivists like George Miller and Herbert Simon followed Hull's agnosticism about the medium of cognition (whether brain tissue or computer program), Hebb was specifically interested in the organization of consciousness as it was experienced, similar to the Gestalt approach (i.e., "the total action of the nervous system," Hebb, 1949, p. xiv).

By the 1940s, however, psychologists understood that even simple behavior in rats could not be fully described in strict terms of the relationship between sensory and motor processes (i.e., stimulus and response): "even in the rat there is evidence that behavior is not completely controlled by immediate sensory events: there are central processes operating also" (Hebb, 1949, p. xvi). There was evidence, from cognitive theorists like Tolman and Gestalt psychologists like Köhler, who struggled with problems of perception and learning, needed a coherent explanation of the processes intervening experience and development of the organization of cognition in animals and humans. These "central processes" underlie the neofunctionalist view of behavior as adaptive and purposeful (Carpintero, 1996). Hebb defended a rigorous physicalism, deplored vitalistic thinking, engaged with a rather shallow scientific philosophy, and was convinced that a specific structure must underlie behavioral determination. As a result, his familiarity with physiological developments (rare for psychologists at this time) drew him towards ongoing debates in the nature of cortical transmission, the structure of connections between parts, and the underpinnings of consciousness (Hebb, 1949; Valenstein, 2007).

Dominant explanations of neural connections at the time can be broadly categorized as those that endorsed a "switchboard" model (John, 1972; Crowther-Heyck, 1999) of complex sensory-motor connections (i.e., reflex arc theory) or the equipotential field theories of Lashley (e.g., Lashley, 1930). For Hebb, both theoretical approaches were overly focused on the mechanisms that prompt transmission of sensory excitation to the motor behaviors while failing to provide a coherent description of central neural mechanisms that would account for the delay in transmission between stimulation and response, which is characteristic of thought (e.g., memories). The particular formula chosen to fill this explanatory gap—increasingly represented by data gathered in clinics rather than laboratories—determined the nature of the psychological theory.

Hebb wanted to address this schism directly. The assumption of sense dominance in these theories left no possibility for higher-order activity which could serve to guide central processes responsible for behavior and complex combinations (Hebb, 1953); there was no space for top-

down hierarchy of control. Concepts related to this higher-order activity, because they did not conform to the dominant S-R theories (often as overly mentalistic) tended to be discarded or ignored; however, these ideas tended to find their way back into these theories (Morgan, 1943). Hebb argued that certain psychological constructs (such as image and expectancy) that were problematic within existing S-R theories were nevertheless valuable for their ability to offer insight about the mechanisms of central processing that underlie normal mental functioning. While recent experiments had demonstrated that dominant psychological theories were inadequate to explain certain observed phenomena (i.e., Lashley, 1930; the experiments of Tolman and the Gestalt psychologists), rather than discard the theoretical project altogether (as Hebb believed Lashley and the behaviorists had done), Hebb argued it was the task of modern psychologists to integrate recent developments in neurophysiology (despite the fact that psychologists had been systematically excluded from the sources of disciplinary knowledge that would have allowed them to do so, such as training and work in medical settings).

Hebb was among those early psychologists who wanted to re-introduce concepts of belief and desire into psychology (purpose and motivation), while still protecting it from the kinds of criticism that behaviorists had used to bring down full-blown mentalism at the beginning of the century (O'Donnell, 1985; Green, 1996). The problem Hebb saw with behaviorism, however, was that it had thrown the baby out with the bathwater. Although notions of “consciousness” and “free will”—the fundamental issues of Wundt (1897) and the Chicago functionalists (e.g., Angell, 1904) -- were recalcitrant from a scientific point of view (behaviorists had shown them to be deeply problematic) there were still other aspects of mind (such as learning, perception, and motivation) that would benefit from investigation using a neuro-psychological approach (Hebb, 1949).

Hebb's insight was to integrate recent work in neurophysiology with the challenges of behaviorists and to think differently about how to conceptualize and address some of the most enduring problems in psychology. In brief, Hebb's 1949 theory stated that any frequently repeated specific stimulation leads to the development of a 'cell-assembly,' a diffuse structure of neural cells capable of acting as a closed system for a short time. This cell structure facilitates other analogous systems. A series of such events consists of a 'phase sequence': this is the process of thinking. Any cell-assembly may be activated by a previous assembly or a sensory event. Central facilitation of one of these activities upon the next is the prototype of 'attention.'

According to Hebb (1949), the answer to the question about the directionality of thought, a subject of utmost importance since Humphrey's (1940) in-depth review of it, lies in this central facilitation. For Hebb, the important recognition was that “neurophysiology not only permits, it requires the assumption that the activity of the brain is not under complete sensory control” which “radically changes the theoretical problem” (Hebb, 1953, p. 103). The physiological evidence that bore on Hebb's theory (1949) was related to the existence and properties of a continuous cerebral activity (e.g., demonstrated by electroencephalography; Borck, 2008) and the nature of synaptic transmission in the central nervous system (Valenstein, 2007).

In thinking through the crucial issue of describing a concept in terms of neural mechanisms, Hebb recalled that he was “stalled, partly because, like everyone else, I was still thinking of the brain as a through-transmission device and partly because of difficulty in reconciling the facts of learning (which must be localized in specific synapses) and the facts of perception (which, it seemed, is not localized)” (Hebb, 1980, p. 292). Hebb's reconciliatory formulation was made possible by recent demonstrations that changed the fundamental understanding of cortical transmission: the discoveries that transmission is not linear but involved closed circuits (i.e., feedback loops) and that a single impulse cannot ordinarily cross a synapse, two or more must act simultaneously, which suggested a kind of computation at work (autonomous processing). Hebb recalled that it was in coming across the work of Spanish neurohistologist Rafael Lorente de Nó (1902-1990), in the work of Hilgard and Marquis (1940) in the early 1940s while at Orange Park, that changed his thinking about thinking (Hebb, 1980).

Lorente de Nó, who had been a student of Santiago Ramon y Cajal (1852-1934), is best known for his contribution to the development of the nature of synaptic transmission in the central nervous system, including a proposed outline of the evolution of neural action, in which the neocortex is formed by a large network of 'closed or reverberatory paths' (Martinez & Gil, 2003). Lorente de Nó's description of the relationship between the reverberatory action of synapses and their structural changes (Lorente de Nó, 1934) and Hebb's conception of cell-assemblies share strong resemblances (Martinez & Gil, 2003). In 1944 Hebb wrote to Lorente de Nó (while at Orange Park) to ask if he could spend a month at the Rockefeller Institute to “become familiar with your work” but Lorente de Nó declined, saying that “my work is concerned with the relationship between the production of the nerve impulse and the metabolism

of the nerve, a problem that is of little immediate interest to a psychologist" (quoted in the Forward to *The Organization*, 2002, p. F8).

In *The Organization* (1949) and other works (e.g., Hebb, 1958, 1980), Hebb attributed overcoming the impasse in the theoretical schism that emerged over the physiological basis of complex behaviors to Lorente de Nó's work. Hebb wrote to Lorente de Nó in 1944: "I am profoundly convinced that psychological progress will depend on research such as yours […] that psychological theory can go no farther without a more detailed basis in neurophysiological fact" (quoted in the Forward to *The Organization*, 2002, p. F8). Indeed, Hebb pointed out that the psychological criticism advanced by Koffka (1924), Lashley (1929, 1930), and Köhler (1929), against the S-R theory of behaviorism applies only to the older theory of linear, sensory-motor connections: "The attack on neural connections as an explanation of behavior was really an attack on this particular conception of the way connections operate; modern neuroanatomy and electrophysiology have changed the question completely, and the significance of the synaptic connections must be examined all over again" (Hebb, 1949, p. 11). The rigidity characteristic of older ideas of neural transmission no longer dominated and the language of function, models and systems entered psychological discourse (see Heyck, 2015).

According to Hebb, these discoveries, described by Lorente de Nó, "have revolutionary implications for psychological theory" (Hebb, 1949, p. 10). From reverberatory circuits Hebb developed the concept of cell-assemblies and phase sequences which were cornerstones of his neuropsychological theory and, by presenting a neuro-structural mechanism for understanding 'central processing,' constituted a major part of the cognitivist challenge to the behaviorist hegemony in psychology during the middle part of this century (Gardner, 1985; Baars, 1986).

5. c. Psychology as an Interdisciplinary Science of Mind-Brain Relationships

For Hebb, an understanding of human cognition is predicated on coherence between various levels of explanation:

> "The fact is that we cannot really draw a sharp line between psychology and physiology. What we can say is that the physiologist is mostly concerned with the functioning of the different parts of the body and the segments of behavior that these parts exhibit; the psychologist with the functioning of the whole organism, and the way in which the segments of behavior are coordinated to form complex actions and sequences of action. The focal problem of psychology is found in the patterns of behavior shown by the whole animal of a higher species in adjusting to his environment over appreciable periods of

> time; to say the same in a different way, it is found in the mental processes of the higher animal" (Hebb, 1958, p. 8).

The 'whole animal of a higher species' reflects the idea that humans and other 'higher species' are reliant on a number of systems which—in their relations to one another—affect the state of the organism and taken together this is the 'whole animal.' This inclusive definition of the problem presents scientific psychology as a science of adjustment, an idea with a long history and bolstered by the postwar bureaucratic worldview (Heyck, 2015).

Another persistent theme in Hebb's theoretical writing was a degree of frustration regarding disciplinary boundaries and his support for broad problem-based research (e.g., the relationship between mind and brain; Dewsbury, 2002), which made him the "last of that breed [of theorists]" (Baars, 1986, p. 220). The brain sciences of the 1940s and 1950s required disciplinary flexibility. Psychology, a field believed to be related more by subject matter than method, would need to become a fundamentally interdisciplinary science. The vision that Hebb outlined in his book, articles, and public addresses included direct emphasis on the building of bridges between disciplines (specifically, neurophysiology), and positioned psychologists as interdisciplinary by nature, as well as by necessity. For example, in his introduction to *The Organization of Behavior* (1949) Hebb emphasized the interdisciplinary aim of the work as central: "The clinician and the physiologist frequently have direct access to data of first importance for psychology, sometimes without recognizing the fact" (p.vii) and therefore this book "makes a sedulous attempt to find some community of neurological and psychological conceptions" (p. 1).

Interdisciplinarity, as a matter of practice, came to play an important place in Hebb's vision of psychology. He understood larger disciplinary problems and turned to other natural sciences as models (e.g., Hebb, 1951). While support for interdisciplinary work was not uncommon at the time, Cold War social science is replete with examples (e.g., Solovey, 2013), Hebb's repeated emphasis on its importance is notable: From where did Hebb's support for interdisciplinary work come? The answer can perhaps be traced to Hebb's formative experiences with Lashley at Chicago and later in Florida. Lashley himself had been a laboratory biologist before being trained in the surgical techniques he employed in his experiments on rats and learning (Bruce, 1986; Weidman, 1999). Lashley had little training in psychology and borrowed heavily in technique and theory development from neurology and neurophysiology (though he

chose to affiliate with neither). His disciplinary allegiance was always to psychology: "Lashley removed the site of his science from the medical clinic and located it firmly in the [psychological] laboratory" (Weidman, 1999, p.10). In many ways Hebb's vision was to return the site from the confines of the behaviorist's laboratory (normal) back to the physiologist's laboratory (abnormal) which was more closely aligned to the medical clinic because it attempted to understand and explain higher-order functions in humans. Indeed, Hebb is largely responsible for the transformation of the concept of 'neuropsychology' as Lashley used it (as the study of functions of the normal animal brain) into its modern therapeutic usage, as a branch of experimental psychology that locates the source of mental disorders in brain lesions. While the clinical application of psychology was utterly foreign to Lashley's perspective (Weidman, 1999), Hebb's tended not only to align but to embrace the pathological approach characteristic of medicine as a new source of experimental data (see Chapter Five).

The opportunities Hebb had pursued in his study of the biological basis of behavior with Lashley and Penfield put him in a unique position to address the practical concerns of interdisciplinarity. Working with Lashley and Penfield had taught Hebb an important lesson: “The thinker in each area is guided by those around him, provided he can use their language” (Hebb, 1951, p. 53). Hebb noted how these experiences were “invaluable in reporting my results and trying to communicate with colleagues elsewhere” (Hebb, 1980, p. 291). Hebb's aptitude in presenting his research in the case study method, familiar to clinicians, assured a disciplinary cohesion between Hebb's psychology and other fields (for example, the American Philosophical Society; Hebb, 1942).

The status of psychology as an independent scientific discipline was a notable concern, especially in the context of behaviorism (e.g., Crowther-Heyck, 1999; Morawski, 1986; O'Donnell, 1985; Ross, 1991; Weidman, 1994) and professional/clinical practice (i.e., psychotherapy and consulting: e.g., Benjamin, 2005; Buchanan, 1997; 2003). Hebb is notably less concerned with the possibility of psychology simply becoming a branch of neurophysiology, in fact, it's not clear this is not his intention (Hebb, 1949; 1951).

Academic psychology in Canada developed more slowly than in the United States and there was much uncertainty about the future of the discipline. This instability, exacerbated by lack of funding, training, and certification, might have contributed to a reluctance to collaborate with other disciplines; the possibility that the young discipline might be subsumed by established

fields (such as medicine) was likely seen as threatening to both professional and academic psychologists (for example, see Crowther-Heyck, 1999).

Hebb understood the relationship between psychologists and physiologists as reciprocal and based on mutual cooperation: “Psychologist and neurophysiologist chart the same bay—working perhaps from opposite shores, sometimes overlapping and duplicating one another, but using some of the same fixed points and continually with the opportunity of contributing to each other's results” (Hebb, 1949, p. xiv). This perspective is perhaps naive and ignores the actual ways in which these disciplines might have interacted, but reflects the long history of alleged affinity with biology (e.g., Tait, Dec 14, 1920).

It is difficult to assess whether, in attempting to establish themselves as experts of the same bay, psychologists were not at a disadvantage compared to experimental physiologists (in their affiliation to clinical medicine). Indeed, Weidman notes how early (behaviorist) psychology wanted to separate itself and its subject matter from other (more established) disciplines and therefore objected to Lashley's desire to bring together psychology and neurology/biology (Weidman, 1999, p.12-13). An emphasis on the continuity of psychology with physiology gave it a basis in “hard” science (with the benefits, such as authority, status, and funding that go along with that), without threatening to reduce psychology to that basic science (or so it seemed). Linking psychology with neurophysiology also tied it to the biomedical apparatus that was emerging in association with the modern research university, assuring future research and funding opportunities in Canada (Hannaway, 2008).

Aware of disciplinary tensions of the time, Hebb was careful about assigning predominance to either psychological or physiological theory (e.g., Hebb, 1951; 1958), he wanted to come across as neutral as possible, though he clearly recognized physiological evidence as taking priority. Hebb was explicit about his disavowal of physiological reduction of psychological constructs; he did not believe psychology should become a branch of physiology (Hebb, 1951). Hebb valued large scale units of analysis and the methods of behavioral study upon which these analyses were based (Hebb, 1958). He warned against the belief that neurological entities are more substantial, more “real,” than psychological entities: “The wood is as real as the trees; a shower of rain as much an entity as the drops that compose it. There must be different levels of analysis in natural sciences […] “reality” consists of the unanalyzed units

whose existence is taken for granted as the basis for analyzing the next higher level of complexity" (Hebb, 1958, p. 320).

Value for interdisciplinarity is represented throughout Hebb's work. Specifically, this is reflected in his characterization of change in psychology as being driven by its integration with existing science. I agree with those skeptical of popular depictions of developments in psychology at this time as revolutionary (e.g., Leahey, 1992; Benjamin, 2007). I believe a key aspect of Hebb's vision is in its call for greater continuity between existing disciplinary formations than revolutionary change within them: "Modern physiology has presented psychology with new opportunities for the synthesis of divergent theories and previously unrelated data, and it is my intent to take such advantage of these opportunities as I can" (Hebb, 1949, p. xix). Hebb concludes his textbook with the following note to students: "Psychology is not physiology, but does have an essential connection with it—which may be good or bad, depending on whether the relation is recognized. Failure to keep in mind the source of a physiological idea, and hence failure to see its limitations, may allow it to become restrictive instead of stimulating" (Hebb, 1958, p. 321). Here and elsewhere (e.g., Hebb, 1951) he is reiterating the central point of his vision for psychology: explicit formulation of psychological theory grounded in physiological science.

Chapter Three Conclusions

Hebb's vision for psychology, his unique emphasis on 'central processing' and interdisciplinary synthesis, emerged from the interdisciplinary training Hebb received while at the MNI with Penfield and with Lashley at the Yerkes Primate Lab during World War II. These experiences brought the major tensions of mid-century American psychology (i.e., the limits of available psychological theory) to Hebb's attention. Hebb brought together various schools of psychology present in the 1940s (behaviorism, cognitivism, Gestalt) to bear on advances in neurophysiological understanding (e.g., Lorente de Nó). Hebb developed a (neuro-)psychological theory of mental representation as a result of his attempts to account for unanticipated findings both regarding the relationship between intelligence and brain function as well as the emotional reactivity of chimpanzees at Orange Park. The neurological patient and captive primate represented new kinds of psychological subjects (Danziger, 1990; Haraway, 1989). While tentative in many areas, this theory underscored a unique and generative synthesis

which held out the promise to account for many of the difficulties behaviorism faced (e.g., intentionality) while leaving thornier questions about consciousness and free will out of the picture.

Hebb's mentors (Lashley and Penfield) had little, if any, contact with each other during their lives, and their disciplinary circles, one academic psychology and the other neurosurgery, rarely intersected during this early period. Tolman's cognitive behaviorism resonated strongly, and in many ways mirrored Hebb's own vision, which is demonstrated in the integration of hypothetical constructs and intervening variables into increasingly neurologized terms. Indeed, Hebb's background and expertise is very much a reflection of the broad network he relied upon (which included family and colleagues early in his career and institutional patronage and government support later, see Chapter Four) to broker new opportunities in an academic landscape largely devoid of physiological psychologists.

This analysis of Hebb's vision introduced three characterizing themes for psychology in the postwar period: theory-driven, based on neuro-hypothetical constructs, and necessarily interdisciplinary. Of his own book (1949), Hebb said that while "there is a considerable use of neurological assumption" he believed "this search for liaison, the attempt to stick as far as possible to the physiologically intelligible, produced a broadening of the psychological horizon" (p. 54). Indeed, it was this broadening of the psychological horizon that is Hebb's most enduring legacy to psychology. Hebb demonstrated that changes in the brain (e.g., in the process of learning) can be represented by the formation and organization of cell assemblies and phase sequences (Hebb, 1949). These structures came to represent certain mental concepts (e.g., memories). Traditional S-R accounts were insufficient; telephone switchboards do not employ symbolic representations to mediate their "behavior." The reverberatory circuits described by Lorente de No provided the mechanism of feedback that allows for behavior to change in accordance with stored knowledge.

How Hebb managed to do this has not been adequately explored. His hope was that his book would contribute to a new direction for theory and research in psychology; he believed the essential value of *The Organization of Behavior* was "the way in which it repeatedly drew attention to behavioral relationships not noted before, or re-arranged the evidence more meaningfully, gives some basis for feeling that the general line it follows may be the direction that future theory will take" (p. 53). Based on "an estimate of the effect of new physiological

conceptions on psychology," Hebb predicted that "the next ten to fifteen years will see a great development of theory, in which Canadian psychology can have its part" (Hebb, 1948a, p. 14). In the next chapter I will examine this general line and attempt to show how these aspects of Hebb's vision were put into practice during Hebb's chairmanship of the Department of Psychology at McGill throughout the 1950s.

Chapter Four: Psychology in Canada and at McGill After World War II

In early 1949, shortly after Robert B. MacLeod (1907-1972) had left McGill for Cornell University, he addressed the Ontario Psychological Association on the broadening importance of psychology to Canadian society (MacLeod, 1949). MacLeod summarized his perspective on the future and meaning of the budding discipline: "I believe that the essential thing about a science is not its particular techniques of investigation, nor the degree of precision with which it measures, but the significance of the problem it attempts to solve" (MacLeod, 1949, p. 211). This concern, over the kinds of problems that psychologists should solve, was among the key struggles shaping psychology in North America after the war (Capshew, 1999; Herman, 1995). Following wartime mobilization of psychological service, many psychologists hoped to take the same technologies and techniques which had been developed and tested (e.g., mental assessment, personality inventories, and diagnostic categories) and apply them to challenges faced by the postwar reconstruction effort. One of the main effects of the war for the discipline of psychology was to dissolved previously held boundaries assumed to exist between notions of an experimental (or "basic") science (characterized by the laboratory setting), and notions of an applied psychology, (in military, clinical, and industrial settings). This chapter explores these changes and how psychologists in Canada and at McGill negotiated these shifting boundaries while shaping the identity of postwar psychology.

Disciplinary tensions mark this period. As those identifying as psychologists grew dramatically in the postwar period, thinking about these problems increasingly positioned the interests of research psychologists against those of applied psychologists (e.g., Fernberger, 1947; Tyron, 1963). For example, MacLeod was concerned that "new [applied] psychologies […] threaten to present us with three different types of psychology and to create three different types of psychologist" – the experimentalist, the clinician, and the social psychologist (MacLeod, 1949, p. 212). Debates over the disciplinary status of psychology, its relation to the biological sciences and to the helping professions, would come to shape psychology at McGill and in Canada after the war and have lasting effects on the discipline in North America.

The previous chapters examined how Hebb's vision for psychology was formed throughout the 1930s and 1940s. These ideas, which he developed working alongside colleagues at the Montreal Neurological Institute and the Yerkes Primate Laboratory in Florida, would come

to affect the direction of early academic psychology in Canada. It was at this time, when Hebb was nearing completion of his manuscript in the late 1940s, that he assumed the position of Chairman of the Department of Psychology at McGill at the age of 44. Similar to the German university tradition upon which Canada's early university system was based, Hebb's duty as Chairman was to direct the activities of the department, to mold it to his own disciplinary image, which he did as Chair of Psychology at McGill for a decade (1948-1958). After his tenure as Chairman, Hebb remained at McGill until his retirement in 1972 at 68, after which he returned to Nova Scotia and continued to teach graduate seminars at Dalhousie University until his death in 1985. Hebb's influence on thinking about cognition, memory and perception—through concepts such as cell assemblies and neural networks—has left a considerable mark on Canadian psychology (Fentress, 1999; Posner & Rothbart, 2004). He is broadly considered one of the fathers of the discipline of neuropsychology, having pioneered techniques and theories explicitly linking mind and brain at a time before the field now recognized as 'cognitive neuroscience' existed.

Hebb synthesized the shortcomings of existing psychological theory contributing to dramatic insight being achieved in the brain sciences in the early 1950s (see Chapter Five). His particular background and stature (with ties to important institutions and figures) also shaped the university's psychology department and played a significant role in the formulation of Canadian postwar psychology. Compared to the United States and Britain, the community of Canadian academic psychologists during this time was extremely small, indeed, sufficiently so that personal contacts and the actions of one or two individuals could have profound effects. One of the main arguments of this chapter is that psychology in Canada, and specifically at McGill, developed differently than in the United States and elsewhere. Hebb's impact at McGill had profound effects on Canadian psychology, making it distinct in important (but sometimes subtle) ways from American and British psychology during this same era. While intra-disciplinary and proto-professional debates within psychology have long histories in the United States (e.g., Capshew, 1999; Samelson, 1985; Herman, 1995), the story of its development in Canada is less well known. The identity and understanding of what psychology, as a discipline, could and should be was an open question. Debates about the future of the discipline provided sufficient cause to categorize its activities as either an applied or basic science, and often both. This chapter offers insight into some of the conditions that shaped the new postwar reality for psychologists in

Canada and describes how a relatively small group at McGill contributed to the development of the discipline during this period, with lasting effects throughout the twentieth century.

1. Rise of Applied Psychology in Postwar Canada

The postwar period was a boom time for Montreal in terms of the city's population, economy, and size. After the hardships of depression and war the postwar period was spent making up for lost time; a strong desire to modernize swept across a broad range of areas from urban renewal and infrastructure to consumer production and investment (Linteau, 2007). Indeed, Montreal's economic boom came on the back of a boom for Canada as a whole. Montreal reached the milestone of one million inhabitants in 1951, putting it ahead of the next two largest cities (Toronto and Vancouver), combined. These demographics are explained by the baby boom and subsequent waves of immigration from war torn Europe. Consequently, Canadian universities experienced an explosion of enrolment after the war putting new pressure on these institutions to address rising demand (Frost, 1984). Between 1950 and 1970, the number of students in the Faculty of Arts and Science increased from 2,500 to over 6,000 (Frost, 1984).

Psychology at McGill and in Canada consequently developed much more rapidly after World War II. Montreal was the economic and cultural capital of Canada until at least the 1980s. Outside a handful of Canada's oldest universities (i.e., McGill, Toronto, Queen's, Dalhousie), most departments of psychology in Canada were established in the postwar period (Wright & Myers, 1982; Wright, 1969). Compared to the United States, departments of psychology took longer to be established: Myers (1968) reports the median year for the establishment of American departments of psychology was 1937 while Canadian departments was 1955. Thus, Canadian academic psychology was itself a postwar development, and at the time, the country turned to Hebb and McGill University as the "rising star" upon which other departments would model their development during this period (Wright & Myers, 1982; MacLeod, 1955).

Shifting demographic changes meant the late 1940s and 1950s were a time of rapid and expansive changes to the teaching and practice of Canadian psychology (Wright & Myers, 1982). These developments, many of which were direct responses to postwar challenges, included changes to the structure of research patronage (i.e., funding and support), tensions emerging from debates over professionalization, and the growth of academic research psychology (Wright, 1974). These developments shed light on broader questions about the

disciplinary identity of postwar psychology, the role of psychology in Canadian society, and the changing relationship of psychology to other professions (such as medicine and business) at mid-century.

Many of these changes were brought about by a substantial increase in both enrollment in undergraduate psychology courses and in the availability of funding for psychological research for the first time. This created a surge of new graduate students that the Department of Psychology at McGill was unable to accommodate prior to MacLeod's arrival in 1947. MacLeod was hired as part of Principal James' support for a department of psychology in desperate need of revitalization after the war. He was provided new full-time faculty (starting with George Ferguson and Donald Hebb as the initial hires in 1947) and was able to secure additional laboratory space (after almost three decades of sparse use). Expansion of the department indicated that the postwar climate had finally affirmed the value and role of psychology in Canadian society, in much the same way it had done in the United States (Capshew, 1999; Pickren, 2007). Similar expansions occurred at other universities across Canada over the next decades (see Myers & Wright, 1982).

After the war, the Canadian Psychological Association (CPA) took charge of the organization of Canadian academic and professional psychology. Postwar experimental research originated at McGill— as Toronto, the largest and oldest department in Canada, maintained its' wartime focus on applied and clinical training for another decade (Pols, 2009)—the ability of the university to secure funding in support of basic research, due to its close ties with government during the war, explains McGill's head start in this direction (Wright & Myers, 1982).

Psychologists at Toronto and Western tended to be oriented towards social and clinical aspects which received some support from the Canadian Social Science Research Council. In general, the emphasis on clinical and applied psychology has been even greater in the French than in the English Canadian universities, such as at the University of Ottawa and Montreal (Wright & Myers, 1982; Shevenell, 1948). The CPA was among the four bodies which sponsored the formation of the Canadian Social Science Research Council in 1940 and John D. Ketchum (1893-1962) from the University of Toronto represented psychology until 1946 when Roy B. Liddy (n. d.) of the University of Western Ontario took over (Ketchum, 1947).

Much has been written about the surge of interest in psychology after World War II (e.g., Capshew, 1999; Herman, 1995). Among the many factors, this growth is often attributed to the

utility of psychological knowledge demonstrated during wartime, the increase in demand for clinical personnel as a result of psychological trauma from the war, and with broad interests associated with high modern social science and an emerging bureaucratic worldview (Heyck, 2015). At the same time, demographic changes to postwar Canadian society (such as the influx of soldiers returning from war and the arrival of large numbers of immigrants) placed strenuous demands on psychological services in the areas of education, health, and industry. Indeed, most of the growth in psychology since 1945 has been in areas involved in the practice of psychology: clinical, counseling, school, and industrial/organizational (Pickren, 2007). These "applied" psychologists, while certainly not new, experienced unprecedented demand in Canada after the war.

This demand on Canadian government and industry in turn put pressure on universities, such as McGill, to train more and more psychological professionals and psychological experts. A McGill administrator noted that "there was a demand for professional psychologists from industry, the Dominion Government Department of Health and Welfare, and from other organizations" and that these needs "could not be met by medical men trained in psychiatry, who were too few and too interested in private practice" (Matthews, Mar 21, 1949). McGill's Department of Psychology, for the first time since its creation in 1924, was in a position to grow; there were still few departments in Canada and even fewer equipped for adequate graduate training at the end of the war (Wright & Myers, 1982). Psychologists would be needed to rebuild Canada and adjust its citizens to the new demands of the postwar era (Rose, 1988).

Compared to other departments at Canadian universities, such as those of the physical sciences, funding was insufficient for adequate research training. Before the 1950s, Canadian psychologists were unlikely to be in a position to be able to negotiate funding or support for experimental work outside very few institutions. This was likely due to a combination of a lack of both institutional support and disciplinary expertise in Canada. Experimental psychologists relied upon existing connections to experimental medicine to extricate their image from that of philosophers and the social sciences; this is similar to the situation in Britain (e.g., Collins, 2006), with some important differences. Securing a stable base of funding quickly became a central task of the Canadian Psychological Association. Indeed, for Canadian psychology to thrive, two fundamental challenges needed to be addressed: 1) the establishment of a dependable financial base for the support of research and training, and 2) a method of assuring the quality of

psychological service that was increasingly being offered to both government agencies and to the public (Wright, 1974).

McGill's Department of Psychology was seen as a model department in the 1950s (MacLeod, 1955; Frost, 1984) and was considered the center of experimental psychology (Wright & Myers, 1982). This placed Hebb, during his decade-long tenure as Chairman, among the organizational elite in Canadian academic psychology (certainly within experimental circles). During his decade-long tenure, Hebb held several positions of influence within the CPA. In 1953, Hebb became President of the CPA (following N. W. Norton, another McGill graduate and professor). While there are some notable exceptions (e.g., Capshew, 1999; Solovey, 2004), comparatively few studies explore the changing nature and role of patronage and intradisciplinary tensions for postwar behavioral science. Fewer still have looked at postwar behavioral science in the Canadian context. However, shifts in patronage were closely linked to both intellectual and institutional changes which enabled a series of behavioral revolutions and interdisciplinary syntheses (Crowther-Heyck, 2006); the neuro-psychological approach characteristic of Hebb's department at McGill is better understood through a closer examination of these factors.

McGill University under Tait had begun to establish psychology as an important contributor to matters of practical concern to society, such as mental hygiene, unemployment, and mental testing. Tait's support for applied psychology mirrored those of his mentor Hugo Münsterberg (1863-1916; see Moskowitz, 1977). When Tait died at the end of the war, Edward C. Webster (1909-1989) joined the faculty in 1946 and took over the applied portion of research and teaching at McGill. Webster had a few years of industrial experience and had conducted wartime morale research in the Canadian Army (Rowe, 1990).

In the United States the rise of applied psychology was even more pronounced (Capshew, 1999). The federal government established agencies and committees to overview the expansion of clinical psychology after World War II. While Webster maintained the tradition of school and industrial-related research at McGill, applied psychology flourished at the University of Toronto under guidance of Bott, Myers, Ketchum, and others (Wright, 1974; Myers, 1982). Throughout the 1950s the responsibilities for running the department (i.e., supervision, teaching, and administration) were divided between Webster, Hebb, and Ferguson. In 1953, upon Hebb's recommendation, Webster established the Applied Psychology Centre, from which McGill's

clinical program and the McGill Guidance Service later emerged. Webster followed Hebb as the Chairman of the department in 1958.

2. Psychology as a Basic Science

Throughout the early twentieth century North American psychology was dominated by activities in the United States. Psychologists in Canada watched uneasily as the numbers of applied and clinical psychologists in the United States grew significantly. Many ranking members of Canada's organizational elite, such as those of the newly formed Canadian Psychological Association, disagreed with this trend. They believed Canadian psychologists should forge their own path when it came to determining the future of disciplinary psychology (Bernhardt, 1947; Dobson et al., 1993).

When Hebb assumed the role of Chairman of the Department of Psychology at McGill in 1948, it was under the pretense he would bring with him a hard-nosed scientific psychology that would improve the disciplinary status of psychology during a particularly uncertain time (e.g., McGill Selection Committee, Feb 7, 1946); Hebb would mend the broken bridges and re-introduce both a spirit and pragmatics of cooperation between long-siloed departments. The behavioralist patronage system, described by Crowther-Heyck (2006) and embodied in Hebb's postwar work, was shaped by the belief that work on meaningful problems necessitated a synthesis not only of different disciplinary perspectives but also of theory and practice (Hebb, 1951); theory could advance only by being put to the test in experimental situations, and practical problems could be solved only through the advancement of tested theory. Hebb had outlined (1949) a synthesizing discipline: "the problem of understanding the total action of the nervous system" (p. xiv).

His reflexive approach to scientific psychology and emphasis on theory-building -- which marked him as among "the last of that breed [of 'big-theory' boys]" (Miller quoted in Baar, 1986, p. 220) -- played an important role in defining Canadian psychology during this period. Through his many positions of influence and his specific training in physiological psychology with Lashley and Penfield, Hebb represented an alternative to existing institutional relations between the psychological laboratory and the neurological clinic. These relations had been framed primarily in terms of technical assistance in the realms of clinical diagnosis and testing. In Britain, Collins (2006) has argued that the essential role of Cambridge psychologist Oliver

Zangwill (1913-1987) in the emergence of neuropsychology in the UK "must be understood in terms of the practical engagement of psychologists with the problems faced by patients with brain damage and by the neurologists dealing with such patients" (Collins, 2006, p. 90). Hebb, on the other hand, demonstrated a deep ambivalence about his support of psychology in clinical settings (Hebb, Apr 6, 1949). His role in the emergence of neuropsychology in North America tells a concurrent yet different story.

Collins argues there was little direct collaboration between physiologists and psychologists in the late 1930s; Zangwill and Hebb had each conducted psychological experiments in clinical settings (Collins, 2006). Zangwill and Hebb each succeeded in developing a British and Canadian neuropsychology, respectively. Importantly, the psychological expertise produced by early neuropsychology did not threaten the existing authority of medical doctors (as opposed to claims to psychotherapy; Buchanan, 2003), as psychologists had struggled to do prior to World War II (Pickren, 1995; Buchanan, 1997). Both men used psychological expertise they had gained through research with neurological patients (Hebb at the MNI and Zangwill at the Brain Injuries Unit in Edinburgh) to advance networks of cooperation between neurologists and psychologists in the postwar period (Collins, 2006). The approach Hebb promoted (e.g., Hebb, 1949) emphasized different relationships to medicine from those emphasized by Zangwill and his colleagues. Rather than promote the utility of psychological expertise to clinical practice as Zangwill had in Britain, Hebb bolstered psychological expertise through continuity of purpose with the physiological sciences and held strong to the understanding of psychology as coextensive with the biological sciences. This approach, which became Hebb's bid to safeguard the future of the discipline, shaped differences in the emergence of neuropsychology in these two countries. A comparative history of these developments (in Britain and Canada) is a worthy endeavour, though beyond the scope of this particular study.

The biological kind of psychology promoted by Hebb assumed that the mind, together with the products of human action, is what the brain does and that we are essentially (though not exclusively) our brains. This idea (i.e., that 'we are our brains') is not a corollary of neuroscientific advances, but a prerequisite of neuroscientific investigation (Vidal & Ortega, 2017). By the late nineteenth century, cerebral localization, differentiation of function, and the correlation of site and effect, structure and function, had become investigative principles. In the

work of Lashley and Penfield clinical and experimental methods came together, with cerebralization as an underlying presupposition that dictated how research was done and how results were interpreted. Hebb formulated this 'neurocentric' vision as a (re-)turn in psychological inquiry, a restoration of "an essential concern with the physiological-psychological problem" (Hebb, 1947; Hebb, 1949).

A few years into his tenure as chairman of the psychology department at McGill, Hebb explained to the Dean of Graduate Studies how he was in the process of "trying to get the biological kind of psychology established elsewhere than McGill" and identified colleagues -- including George Ferguson (McGill University), N. W. Morton (Defense Research Board), Julian Blackburn (Queen's University), John Zubek (University of Manitoba) and William Thompson (Queen's University) -- as proponents of the neurological approach to psychology that Hebb had pioneered (Hebb, Sept 23, 1954). These were Hebb's closest allies and at some point each had been a member of McGill's psychology department.

Psychologists had long lamented the "fragmentation" of the discipline and the absence of accepted theories, methods, and subject matter; they voiced distress about the fractionalizing apparent in the competing "schools" and "systems" of psychology (e.g., Bode, 1922; Heidbreder, 1933; see also Green, 2015). Hebb had come to believe that many of the core challenges psychologists were facing stemmed from disunity as a discipline (Hebb, 1949; Hebb, 1951). This view was not uncommon (e.g., MacLeod, 1949); there was a notion that psychology lacked the same firm theoretical foundations that grounded other scientific disciplines in common concepts, objects, and methods. In many ways, the only shared characteristic of the various branches of psychology is its having been the result of historically contingent and opportunistic processes (Walsh-Bowers, 2010; Teo, 2010).

The 1920s through the 1960s was a time when the great foundations and the federal government came to play major roles in scientific research in the United States (Geiger, 1993, Solovey, 2013) and the same was true in Canada (Frost, 1984). In the two decades after 1950 Canada enjoyed unprecedented growth and prosperity (Linteau, 2013). Economic activities thrived and the country embarked on a new phase of industrial development, spurred by large-scale electronic, aeronautic, nuclear, and chemical engineering (Frost, 1984). American capital fueled Canada's economic growth (e.g., through direct investment and American ownership of factories). Similarly, many of the funds available for experimental research in psychology came

from large American foundations (such as the Rockefeller and Ford Foundations) or the American government (such as the National Institute of Mental Health, the United States Public Health Service, and the Department of Health, Education, and Welfare; which had emerged from pre-war mental hygiene initiatives). However, as nationalist sentiment grew with the intensification of the Cold War, calls for national unity under Louis Saint-Laurent's new Liberal leadership cast new perspectives and concern about Canada's dependence on capital from the United States. The organizational elite within Canadian psychology were similarly nervous about undue American interests exerting influence on the direction and development of the discipline after the war (Wright, 1969; 1974).

Central to Hebb's view of psychology was that it should be properly situated alongside disciplines such as physiology and neurology as a branch of the biological sciences. The importance of having psychology recognized as a proper member of the biological sciences was a recurring theme in Hebb's lectures and in his writing (Hebb, 1949; 1951; 1974). For example, in a letter to his American patrons in 1951, Hebb described his particular brand of psychology: "I am a 'physiological and comparative' psychologist, convinced that psychology must stay in close relation with physiology, that it will ultimately be continuous with physiology although distinct" (Hebb, Mar 14, 1951). He saw his own psychology as being on the vanguard of reunification and later described his own role as "an ambassador maintaining communication between psychology and other biological science" (Hebb, Apr, 1958c, p. 10).

Unlike contemporary American cognitivists, such as George A. Miller (1920-2012) and Herbert A. Simon (1916-2001), Hebb was dedicated to the focus of intermediary position (between psychology and biology). This was the "cybernetics moment" (Kline, 2015). Whereas Miller and Simon abstracted the workings of the mind (as medium agnostic algorithms), Hebb wanted to use models (i.e., cell assemblies) that integrate explicitly with existing neurophysiological science. For Hebb, he wants to use our own understanding of neurophysiology to understand our cognitive experiences as biological organisms. This is a slightly different approach from that characterized by the pioneers of the cognitive revolution (Baars, 1986; Gardner, 1987).

Few Canadian psychologists shared Hebb's particular brand of biological psychology in the postwar period. Outside McGill, psychology in Canada was still very much associated with the departments of philosophy (Wright & Myers, 1982; Wright, 1974). For example, when Hebb

taught experimental methods at Queen's University between 1939 and 1942, they were still listed as philosophy courses. Modern Canadian psychology did not take shape until after World War II. Consequently, its organization and direction vis-a-vis the American model remained an open question into the postwar period (MacLeod, 1947a, 1955). It is difficult to imagine one better positioned to affect the orientation of postwar psychology in Canada than Hebb; his background, connections, expertise, and forward-thinking vision aligned well with the emerging interests of psychologists (as experimental scientists) and their postwar patrons (e.g., the Government of Canada). Hebb was a "man of affairs" (Isaac, 2009) among "men of action" (Gavrus, 2011). In some ways, the publication of *The Organization of Behavior* might be considered a powerful rhetorical and positioning device for Hebb's opportunistic foray into the emerging interdisciplinary spaces created by the last war (Crowther-Heyck, 2006). Hebb had hoped that his book would "reverse the trend of psychology away from the nervous system" and show that "a neurologically-oriented theory in psychology may be broader, psychologically more adequate, than others" (Hebb, Mar 14, 1951).

Hebb himself had a very pessimistic take on the state of Canadian psychology in the immediate aftermath of the war. He noted that basic experimental research was "the goose that laid the golden eggs of applied and clinical methods" and that, in Canada, if it was not dead it was "very skillful at feigning death" (Hebb, 1948a, p. 13). The worry that flowed from this sentiment was that applied and clinical psychology siphon off the best students from experimental research and that the fundamentals of the discipline would be ultimately left to "second-raters" (Fernberger, 1947).

Hebb's conviction was that all sound psychology necessarily emerges from sound experimental principles. Therefore, "a Department of Psychology cannot continue healthy without a strongly established section of experimental [science]," the respect of the discipline in the eyes of more established sciences was paramount (Hebb, Apr 1958c, p. 6). He went on, "Fundamentally, strategy calls for compelling the respect of the other scientists in the community: once it has achieved this, a department of psychology can proceed to plan its work pretty much as it likes; but otherwise it is headed for difficulty (another of Hebb's pronouncements *ex cathedra*)" (Hebb, Apr 1958c, p. 9). When Mary Wright became Head of psychology at Western she sought the advice of Hebb on how to go about developing her department (Neal & Wright, 1982). She recalled that he told her that if the department was to

thrive, it “must first earn the respect of the ‘hard’ scientists [...] by demonstrating that psychology did ‘hard’ science, basic research” (p. 127). Once this was accomplished, he assured her, support for all aspects of psychology would be forthcoming. Hebb emphasized the importance of scientific authority as the bedrock of academic psychology, and was perhaps more a disciplinary maneuver than a specific epistemological act (Stam, 2004).

For Hebb, it was important that each department begin with a strong experimental core around which the rest of the department develops and depends. Psychology, through its methods and practices, should become continuous with the fields of neurology and physiology. Hebb had identified a number of challenges. Psychology, according to Hebb, could not follow along the same path as other biological sciences because of its heritage: “psychology entered the realm of the biological sciences by the back door, so to speak, and the result is that we still have major barriers to communication and cooperation at a time when it is more important than ever [...] the effective liaison does not exist” (Hebb, Apr, 1958c, p. 1). Hebb wanted to establish this “effective liaison” but first he would have to address an issue he had noticed, psychologists experienced an aversion to working with biological scientists.

Hebb believed one of the main reasons psychologists chose not to collaborate with biological scientists was that psychologists were unjustifiably worried that collaboration with a biological scientist would result in a 'taking over' of psychology or subordination within a research group (Hebb, 1951; Hebb, Apr, 1958c). According to Hebb, experimental psychology had “encysted itself” and as a result, “was not accompanied by a corresponding marriage with an existing biological discipline” which has produced misunderstanding and difficulty in communication with biologists which would “not have existed if psychology had been an outgrowth of zoology or neurophysiology” (Hebb, Apr, 1958c, p. 2). Hebb’s experience suggested to him that this idea was rooted in disciplinary insecurity rather than actual experience, having worked within such interdisciplinary research groups himself. He believed much more could be gained through a closer relationship with the biological sciences than would be potentially lost; this was the gamble he was willing to make. Hebb identified in correspondence with Principal James three areas improved as a result of greater scientific authority: status as researchers, funding as researchers, and students as researchers (Hebb, Mar 31, 1947).

Hebb’s strategy, which he encouraged other heads of departments across Canada to adopt, was to create alliances with the most established and venerated fields of physiology and

medicine. This maneuver would assist fledgling departments with the more practical concerns of acquiring experimental space and gaining access to laboratory equipment. For example, acting on Hebb's advice, Wright made connections with the Physiology Department, which loaned the department space and made possible the appointment of neurophysiologist Gordon Mogenson (1931-1991) in 1965 (Neal & Wright, 1982). Similarly, a department was created under McMurray (who had studied with Malmo and Hebb) in close association with physiology at the University of Saskatchewan. The department was housed in the new Medical Building and cooperative use of their laboratory space made possible the teaching of experimental psychology (McMurray, 1982). Indeed, McGill had "a decisive effect" on the shaping of the department through the early faculty appointments of McGill graduates (McMurray, 1982, p. 189).

The status of psychologists within the academic-laboratory ecosystem was also a concern. Psychologists as researchers (rather than tool-based experts) appealed to the general fear by experimentalists that psychologists might become mere technicians after the war. This concern was particularly prevalent in debates around psychology's relationship to medicine in the first half of the century (Pickren, 1995; see also MacLeod, 1955).

Public relations represented another challenge. Many Canadians in the public service, upon whom psychologists were becoming increasingly dependent for research funding, were simply unsure what it was that psychologists *did*. The broad range of techniques, theories, and objects that psychologists employ to various ends has been a consistent source of difficulty (see Benjamin, 1986); scientific aspirations seem to fly in the face of this inherent disunity (Green, 2015). Confusion about the kind of work that psychologists were involved (i.e., experimental laboratory research), and their relationships to the religious and philosophical thinking at the time, put them at risk of either being lumped together with the humanities or being relegated to the social sciences, both which tended to receive significantly less funding. With less funding departments of psychology would be unable to conduct research or attract the best students. This, Hebb saw, as a feedback loop: without the best students to contribute positively to the growth of the discipline and improve its public image at large, psychology would remain in the eyes of its funders a group of expert technicians at best and pseudo-scientific charlatans at worst.

It is important to observe that benefits associated with closer affiliation to the biological sciences relate specifically to the development of psychology as a basic science rather than to the growth of psychology as a profession. Additional funding from government agencies and

institutions did not translate into greater investment into training for private practice or any kind of certification. For reasons I've outlined above, Hebb and others believed that such support must be considered secondary to investment in basic research. Hebb and his colleagues defended a pure psychology untainted by social and political concerns, experimental at its core. The development of psychological knowledge will inevitably improve professional practice; academic research must always be prioritized; greater fundamental/biological understanding of the processes of thought contributes to the total psychological understanding of the mind. It is only once understanding is achieved that professional practice can be developed.

Hebb was at the forefront of the strong bias towards experimental psychology that characterized the beginnings of modern psychology in Canada (Dobson, 1995; Webster, 1967; Wright & Myers, 1982). Hebb's dedication to experimental "hard-nosed" psychology contributed not only to the reputation of McGill psychologists but to the image of Canadian psychology at this time (Dobson, 1995). Despite McGill's growing reputation, the danger of not being able to attract their share of the most capable students remained a concern (Hebb, Apr, 1958c, p. 5).

If psychology at McGill was going to establish itself as a biological science it would have to demonstrate its utility, within circumscribed domains. In order to organize research in Canada the guiding organization for academic psychologists (i.e., the Canadian Psychological Association) would need to resolve the problem of adequate funding for experimental research. The ways funding was organized affected how power at McGill and much of Canadian psychology was oriented towards academic research institutions.

3. Funding Canadian Psychology After the War

Psychology departments in Canada had long struggled to find funding for their research activities (Wright & Myers, 1982). Much of this had to do with both its comparatively small population and the delayed development of academic departments at Canadian universities. While federal funding for universities rose dramatically after the war (Frost, 1984), Canadian psychologists negotiated their disciplinary identity and position in the new system of patronage that was taking shape. For much of the first half of the twentieth century few average Canadians understood what psychologists did, fewer still were able to say whether psychology was a legitimate science or merely a passing fad (e.g., Leacock, 1924). Many lingering ties, to both philosophy departments and to the mental hygiene movement, countered the narrative that Hebb and his colleagues wanted to establish for psychology in the postwar era.

The influx of postwar research funding was in stark contrast to the continual struggle for research support that characterized psychology at universities prior to the war. According to Hebb, Tait had done "nothing to improve the situation" of psychology at McGill (Hebb, Sept 23, 1954) and correspondence between MacLeod, and the university suggest that even after the war, administrators were reluctant to fully support the fledgling discipline (see Chapter Two). What little funding was available tended to be tied to applied or project-oriented research that had been established prior to Hebb's arrival; this research was provided in large part by philanthropic organizations with science advisors who had specific intentions about the direction of research (e.g., Clarence Hincks; see Chapter One). What was possible in terms of basic research was shaped largely by the research priorities of these organizations (e.g., the role of the mental hygiene movement; Gleason, 1999; Pols, 1999).

3. a. From American Philanthropy to Federal Funding for Psychology at McGill

The 1950s was a period of transition as the reliance for support for research and training of psychologists in Canada changed from major American philanthropies (such as the Rockefeller and Ford Foundations) to the federal governments of the United States and Canada. For the first time, direct funding from the federal government became available for the social and psychological sciences. While foundations remained an important source of funds for basic research (Shore, 1987; Capshew, 1999), the transition from direct support for research activities declined as federal funding increased substantially over the 1960s (Appley & Rickwood, 1967; Shore, 1987). The amount available to psychologists was $63,000 in 1958, by the end of the following decade this amount would increase again to $1.5 million (Wright, 1968, p. 232). Substantial increase in Canadian funds for psychology gradually reduced dependence on American funding over the early 1960s, from over one-half to about one-third by 1966 (Appley & Rickwood, 1967). For psychologists interested in basic research, the shift towards greater reliance upon public funding created new tensions for the relationship between applied and basic psychologists in the postwar period which affected the direction of academic psychology in Canada after the war.

Hebb saw himself as among the vanguard of a new biological psychology (Hebb, Sept 23, 1954). As head of psychology at McGill (and de facto representative of comparative and physiological psychology in the CPA; Hebb, 1948a), Hebb was personally invested in the new arrangements that were being developed at this time between the federal government and

research psychologists at universities. McGill, in particular, emerged well-positioned to gain from postwar expansion of government-funded research, especially in areas related to health and welfare.

Many top ranking Canadian psychologists during the war, both in the CPA and the Canadian government, had been graduates and professors of the Department of Psychology at McGill. These ties between government research and academia became critical to the establishment of experimental psychological research in Canada. Both the founding members of the CPA and several McGill psychologists in service to the Canadian government (often the same people: for example, MacLeod, Hebb, Morton, Webster, Bois, and Ferguson) recognized the opportunity the war had presented for highlighting the relevance of psychological expertise.

In the wartime years the Canadian Psychological Association's Test Research Committee, headed by George Humphrey (1889-1966) of Queen's University, was the driving force of the involvement of Canadian psychologists in World War II. The Committee was supported through annual grants provided by the National Research Council (NRC) and worked in close collaboration with the Personnel Selection Directorates of the Canadian Armed Services. This kind of close relationship, between government and academic psychologists, mirrored the professional and disciplinary achievements that American psychologists had gained during the First World War with mental testing (Capshew, 1999). Many Canadian psychologists had worked in these settings and brought with them the same sensibilities to the opportunities provided for disciplinary expansion in postwar Canada.

After the war, Canadian psychologists hoped to maintain the government support they had received from the NRC while they transitioned to peacetime activities. During the war, investment by the Canadian government in the physical sciences was recognized as a national priority; the same status was hoped for the achievements of Canadian psychologists. Once the Canadian Psychological Association (CPA) had begun to make progress towards convincing federal agencies of the value of psychological expertise, they intended to hold on to or expand the degree of support they received. Members of the CPA attempted to convince key figures of government that the growth and development of scientific psychology was in the country's best interest and should therefore be funded by the federal government (Wright, 1974). Canadian psychologists relied heavily on a combination of philanthropic and government grants from the United States to be able to conduct basic research. And one of the key strategies for funding was

to emphasize the fact Canadian psychologists could no longer rely as they had in the past for American support. Canadian sovereignty and the need for a reliable source of psychological labour/expertise during times of national crisis were among the reasons provided (e.g., MacLeod, 1947a). Canada's unique historical and geographical conditions were similarly presented as both urgent challenges and available opportunities for Canadian psychologists (MacLeod, 1947a). The CPA explained to the Canadian government that, for research to most benefit Canadian citizens, it must be developed in Canada by their own psychologists.

The first source of federal funding had come in 1940 from the NRC and it maintained some degree of support for defense-related work throughout the war (1940-1945). The Defense Research Board (DRB) was established in 1947 to carry on defense-related psychological research after the war (Turner, 2012). In 1948 the Department of Health and Welfare also made grants available to psychologists. Canadian psychologists, such as Hebb, continued to rely on American grants from agencies such as the National Institute of Mental Health, United States Public Health Service, and the Department of Health, Education and Welfare. Research at academic institutions like McGill went from being dependent on private American philanthropy to being maintained by tax-payer supported scientific granting agencies, following a similar trend in the United States in the 1950s (e.g., Crowther-Heyck, 2006; Brison, 2005).

This shift in the late 1940s and continuing dramatically into the 1960s was the result of the efforts of a handful of CPA-affiliated psychologists (Hebb among them) to have efforts of the academic discipline be recognized for their important contribution to the war effort (Wright & Myers, 1982) and to Canadian society at large. The application of psychological expertise to the understanding of social issues and to the problems facing the government (health, crime, industry) were explicitly highlighted after the war in the attempts made by members of the CPA to establish ongoing sources of research funding from the government (e.g., MacLeod, 1947a).

3. b. Postwar Research Planning: National Research Council and the Defense Research Board

Shortly after the war, the Test Research Committee which had been led by Humphrey became the Research Planning Committee. The main task for this Committee was in "establishing permanent federal contact" between the CPA and the NRC in Ottawa (Bott, 1948, p. 11), the primary source of Canadian funding at the time. E. A. Bott assumed leadership of this Committee with Humphrey as secretary-treasurer (until his departure for Oxford University, after which MacLeod assumed his position; Ault, 1948).

The hope for the Committee was twofold: 1) to develop a broader basis for psychological research and training fellowships under government sponsorship and, 2) to identify areas most important and in need of research. The Committee recognized two (potentially conflicting) sources of interest in psychological research: practical problems such as various departments of government might wish to prescribe for investigation, and fields which research psychologists (mainly at universities) might deem significant.

In 1946-47, the Committee systematically canvassed and consulted with the technical staff of several federal departments (i.e., Labour, Defence, Health, Veteran's Affairs, Secretary of State, and Civil Service Commission) to ascertain the relative importance of psychological research and expertise to these branches. These consultations resulted in an overwhelming interest in what Canadian psychologists might be able to offer the government (Bott, 1948). The report concluded that, indeed, there were federal government departments outside the Department of Defense that had problems that could be addressed through the application of psychological knowledge, training, and expertise. Subsequently, at the CPA's July 1947 meeting, it was decided that members of this Committee, representing the five major areas of psychology, would outline "some promising paths for research" (Bott, 1948). The aim of these reports was to highlight both the urgent need for investment in Canadian psychological research as well as the many practical problems to which psychological expertise could be applied (e.g., Ketchum, 1947; MacLeod, 1947a).

These reports served as a means to officially articulate and formally disseminate the needs to the discipline in terms that would highlight the need for postwar research funding. The Research Planning Committee, headed by MacLeod in 1947, represented the men at the heart of the organization of postwar psychology in Canada, they provided "a blue-print for psychological research in the immediate" (Bott, 1948, p. 13). The NRC funnelled annual grants to the Committee in order to adjudicate how best to work out the CPA's relationships to departments of the federal government (Ault, 1948). Hebb, as a member of this committee, was the main representative for experimental, physiological and comparative psychology. He was concerned that Canadian psychologists had overemphasized practical psychology at the expense of basic research (Hebb, 1948). The problems the Committee had mapped out for future research were issues with which the government was most interested, and represent primarily areas of applied social psychology (Bernhardt, 1947). Among the concerns of academic psychologists during this

period was likely the integration of the American Association for Applied Psychology into the American Psychological Association in 1945 (which allowed non-academically affiliated psychologists and those without Ph.Ds to join the organization). Indeed, it seems only after the war that the rise of professional and applied psychology became a major concern for this committee.

Together Hebb and MacLeod positioned McGill as a key site for the orientation of postwar psychology in Canada (Bott, 1948). As a prominent leader of the postwar Canadian psychology and member of this committee, the push towards basic research and away from practical and applied psychology was something to which Hebb contributed throughout the 1950s.

3. c. Donald Hebb, the Defense Research Board, and early experimental research in Canada

Similar to the development of academic psychology in the United States, much of the early support for Canadian research in psychology came from defense research contracts. The agency that contributed most to Hebb's early research program was the DRB. After the war, the NRC recommended substantial growth in government-funded science "on a specialized, departmental and polycentric basis" (Phillipson, 1991, p. 181). One of the agencies that spun off from the NRC's wartime work was the DRB, created in 1947 to continue defense-oriented research during peacetime. The DRB became instrumental in getting psychological research established in Canadian universities. A representative visited every campus around 1947 to encourage staff to apply for grants (Webster, 1984) with the result that, "much of the Canadian psychology research productivity of the 1950s and 1960s can be traced to the DRB" (Webster, 1984, p. 196).

It was believed this board, due to the appointment of Morton as its Director, would "make permanent provision for psychological research in the interests of defence" (Ault, 1948, p. 188). Morton was a graduate and professor of the Department of Psychology at McGill. He had served the Canadian Army in World War II, conducted research in personnel selection, and collaborated with Hebb on the development of the McGill Verbal Situation and Picture Anomaly series (Hebb & Morton, 1942; 1943) and co-authored with him a theoretical article on adult intelligence (Hebb & Morton, 1944). He was likely sympathetic to Hebb's cause and named by Hebb as those helping to bring "a biological kind of psychology" to Canadian universities (Hebb, Sept 23, 1954).

At the first meeting of the DRB in 1947, Morton was hired as a consultant on psychology and personnel. Two years later he became the Director of the newly formed Operational Research Group (Morton, 1956). Morton remained with the DRB for the rest of his career working as a Director, Division Head and Chief of Personnel (Turner, 2012). Hebb served on the DRB's Psychological Research Panel (Turner, 2012), worked closely with Morton throughout the 1950s, and relied upon DRB support for some of his more important research (see Chapter Five). It is important to note how funding from the defense department often came with certain conditions. Much of the work conducted could not be publicly disclosed (an issue Hebb took issue with) and the research had to be of direct relevance to military interests (Hebb was adept at composing broad grant applications that linked a number of concerns relevant at the time).

Hebb continued to receive support from the Rockefeller Foundation throughout the early 1950s as new funding became available from the NRC and the DRB (e.g., Hebb, Sept 22, 1952). For the first time since its creation at the onset of World War I, political backing and enthusiastic cooperation of university scientists provided the NRC with both a general plan for national development and the resources to carry it out (Phillipson, 1991). In order to oversee the rollout of the expansion of funding for psychological research in Canada the NRC decided in 1948 to appoint an Associate Committee on Applied Psychology. This Associate Committee existed for the purpose of fostering fundamental psychological research and its applications to problems of national importance in Canada. This Committee--the result of ongoing collaboration with the federal government--was given the mandate to examine the role of psychology in Canadian society, to implement the efforts of psychologists, and to assist in the training of graduate students for research. This committee consisted of O. E. Ault (Director of Personnel Selection in the Civil Service Commission, Ottawa), C. G. Stogdill (Dept. National Health and Welfare), E. A. Bott (University of Toronto), G. G. Brown (Anthropologist, Toronto), T. W. Cook (Saskatchewan), N. Mailloux (Montreal), and D. O. Hebb (McGill) -- these were “men of affairs,” “committee men,” grant-getters, proponents of interdisciplinary inquiry, and institution-builders (Isaac, 2009). N. W. Morton was made chairman of the committee and would be in charge of coordinating psychological work already in progress in defence and operations from wartime efforts with research to be developed under universities and various government departments moving forward (Ault, 1948, p. 188).

Meetings of the Associate Committee were held in Ottawa in late 1948 to decide how to allocate federal funding for psychological research. These decisions were made in close collaboration with government officials. Unsurprisingly, the list of "areas of special interest" emphasized subjects of direct interest to the Canadian government, such as those related to industry, health, and crime (Ault, 1948, p. 189-190). The Associate Committee on Applied Psychology had developed a strategy of aligning the CPA's interests for research funding to government priorities. This was, however, met with some consternation by the general CPA membership (Bott, 1948). At the CPA's 1948 Annual Business Meeting there was heated debate about the shortsightedness of potentially mortgaging the Association's limited resources by patterning them too closely to suit then current requirements or policies of particular departments of government. Both the scarcity of existing researchers and the need to safeguard academic freedom were voiced as reasons why the CPA should be cautious about endorsing steps that might siphon off resources and limit the choice and evaluation of research problems moving forward.

Bott (1948), however, dismissed these concerns on the basis that Morton's appointment mitigated many of these worries. Morton was an insider; attached to the Biological Division of the DRB, and a liaison between the Canadian government and the CPA. Indeed, Morton's close work with the CPA would earn him the status of Association President in 1952. In a notable maneuver by the ranking members of the group, the NRC's Committee on Applied Psychology was eventually renamed the Committee on Experimental Psychology, representing the explicit change in the orientation of funding from applied to basic research. This decision was among those later cited as evidence of academic psychology's move away from applied and professional priorities that would come to characterize this period in the 1950s (Webster, 1967).

3. d. The Massey Commission and government support for psychology

An important element to understand as a condition of postwar psychology was the precarity that academic psychologists faced in the postwar years, both in terms of funding and disciplinary direction (i.e., professional/applied versus academic/experimental). The attempt to present psychology as a handmaiden to government, seen as necessary to the continued support of psychological science, was met with resistance among the organizational elite (such as Hebb) who wished to establish psychology as a basic (i.e., biological) science. NRC support throughout the late 1940s was important to the development of the kind of psychology Hebb wanted to see

thrive (Hebb, Sept 23, 1954). The new models for university research funding that emerged put psychologists, such as those at McGill, in a difficult position. Confusion and disagreement about how best to strategically advance psychology in Canada, as simultaneously an applied and basic science, created substantial challenges. Resources were limited and the interests between professional and academic psychologists varied dramatically. The direction Hebb hoped Canadian psychology would take was different than the path American psychology had taken after the war.

While McGill received some provincial support after World War II (see Frost, 1984), as an institution it remained heavily reliant upon private donors to operate, as it had throughout its history. With the postwar boom in enrolment (what came to be known as the "veteran bulge," Myers, 1982), no amount of private fundraising could cope with the steadily increasing costs. In 1949, F. Cyril James (1903-1973), as chairman of the finance committee of the National Conference of Canadian Universities, secured an interview with the prime minister, Louis St. Laurent, and urged federal aid for all of Canada's universities. He received a sympathetic hearing, and was informed of the government's intention to establish a royal commission on the development of arts, letters, and sciences, a body which came to be known as the Massey Commission (Frost, 1984).

The commission published its report in June 1951. It found that "universities have become essential institutions of higher education, of general culture, of specialized and professional training, and of advanced scientific research." But it also found that "our universities are facing a financial crisis so grave as to threaten their future usefulness … the universities face the twin spectres of falling revenue and rising costs" (quoted in Frost, 1984, p. 250; see also Royal Commission on National Development in the Arts, Letters, and Sciences, 1951)

One of the Commission's tasks over the next several years was to create an arts council of the government of Canada which would foster and support humanities and social science research at universities. This placed Canadian psychologists in a strange position. Following its expansion after the war, the NRC reverted to its pre-war civilian role and a number of wartime activities were spun off to newly formed organization, such as the Atomic Energy of Canada Limited, Medical Research Council of Canada, and the Defense Research Board. Despite Morton's support at the DRB, academic psychologists found support from the government (i.e.,

NRC) to be lacking after the war. In January 1954, George Ferguson wrote to the then Director of Operational Research at the DRB (Morton) to emphasize the insecurity of funding for psychological research and the urgent need to expand existing Canadian sources.

The question whether academic research psychology should fall under the umbrella of the new arts council or the primary national research and technology organization of the Canadian government (i.e., NRC) was unclear at this time: Where did Canadian psychology place in relation to structures of government patronage then emerging in the postwar period? (Granatstein, 1984). The answer to this question would potentially settle psychology's identity (and future) in Canada as either a primarily applied science of technical-professionals or a basic science of academic researchers. The debate that emerged over the status of scientific psychology in this period has not been adequately explored and its consequences for the development of psychology in Canada has not been fully examined.

An important outcome of the Massey Commission, after investigating the state of arts and culture in Canada, was the federal funding of a wide range of cultural activities, which resulted in federal aid for universities and in the creation of the Canada Council for the Arts (Stewart & Kallmann, 2016; Granatstein, 1984). The Commission held 114 public meetings across Canada in 16 cities and heard from some 1,200 witnesses. About 450 briefs were heard, and experts in various fields were invited to present special studies. Ranking members of the CPA received a request from E. W. R. Steacie (1900-1962), the President of the NRC, that the association should prepare a statement regarding their proposed funding relationship with the new Canada Council for the Arts.

The need for research psychologists to position themselves in new federal funding schemes prompted the development of the Committee on Research Finance at the annual meeting of the CPA held in Montreal in June 1954. With the development of the Canada Council of the Arts, they were worried that research psychologists would lose their support from the NRC (Blackburn, Dec 16, 1954, p. 4).

The first meeting of this committee was held in Montreal a couple months later and its members concluded that funding for psychology ought to be included in *both* the NRC and the Canada Council (CPA, Aug 22, 1954). The Committee agreed that, given "[psychology's] experimental nature, [necessary funds] might be considerably in excess of that required by the

humanities" (Morton, Aug 30, 1954) and therefore should not rely solely on funding available through the Canada Council.

The case for the funding and development of "the biological kind of psychology" was fully expressed in an official memorandum to the NRC by the Committee on Research Finances in 1954 (Blackburn, Dec 16, 1954). In this memo, the Chairman of the Committee identified McGill as one of the exemplary institutional centers for this new kind of psychological research thanks to NRC support. He cites, for example, that between 1948 and 1954, more than half of the 97 papers and books published or accepted for publication were physiological or biological in content (Blackburn, Dec 16, 1954, p. 4).

The concern expressed in this memo was that "academic" or "tough-minded" psychology was dependent on scientific funding and that reliance upon the Canada Council risked undermining what was being developed by Canadian psychologists. The future of Canadian psychology was associated with its status as a natural science in the eyes of funders: "Physiological psychology is coming closer and closer to neuro-physiology, so that there is frequently a close collaboration between the two […] there is a real prospect that the work of some of the younger men will make Canada outstanding in this field in the next decade" (Blackburn, Dec 16, 1954, p. 5). Robert B. MacLeod, now at Cornell, would also join in support for McGill's biological approach to psychology in his own report to the Social Sciences and Humanities Research Council the following year (MacLeod, 1955).

Later that month Morton wrote a candid letter to Julian Blackburn, the head of the Committee, about the report sent to the Royal Commission and the NRC. He explained that the report had been interpreted as emphasizing the applications of psychology rather than its experimental and research potential as a basic science. He noted that, the way the government saw it, "… The social sciences are grouped with the humanities and if psychology has to be fitted in somewhere it is with the social sciences. There is apparently no serious thought of the psychologist being described as a scientist" (Morton, Aug 30, 1954). This was what Hebb had feared.

While it is not clear why the report to the Royal Commission failed to convey its intention, I get the sense that in hoping to get federal funding from both the NRC and the Canada Council, the CPA leadership might have overplayed their hand: they pitched the discipline as being primarily applied when describing their practices and objectives, and downplayed

psychology's role as a basic science when articulating their value to industry. It is in this letter from Morton to Blackburn that the issue of federal funding for psychology is well captured: As Morton notes, "It has been made quite clear to me by the NRC previously that it has, since the Massey Commission Report was issued [in 1951], regarded itself as possibly doing nothing more than producing for psychology on an interim basis until the Canada Council is established […] it is improbable that in the meantime the [National Research] Council will make more than the base minimum financial provision for psychology […] until the parentage of the child [i.e., Psychology] is determined, the temporary custodian will merely ensure that he does not actually starve to death" (Morton, Aug 30, 1954). This quote, which highlights the low perceived value of establishing scientific psychology in Canada, points to the kind of resistance Hebb faced in the early 1950s.

The withdrawal of NRC funding for psychology concerned both Hebb and Dean Thomson. Recognizing the implications for the disciplinary status of psychologists being supported by an agency for the funding of the humanities, Hebb expressed some candid remarks about the development of the discipline in Canada. He wrote to Dean Thomson in the fall of 1954 to explain that although he understood the "hostility" towards psychology by its "skeptics," and whereas he had "some sympathy" for these attitudes given the history of its development in Canada, he firmly expressed his belief that psychology should be, for the most part, funded by the NRC rather than the Canada Council. The reason Hebb gave was that psychology, as practiced and trained at McGill, was firmly situated as a natural science; the needs of the laboratory for psychological experimentation, he noted, would not be adequately appreciated, to the detriment of research. Moreover, he urged Thomson, as Dean of Graduate Studies and Research, to be more of an advocate of psychology at the NRC (Hebb, Sept 23, 1954). Hebb goes on in this letter to explain and sympathize with skeptics wary of psychologists in the past: He blames Bott for "expanding his department without any real academic substance as a basis," Humphrey for "not having the faintest notion of how one goes about systematic research," and Tait for having done "nothing to improve the situation" (Hebb, Sept 23, 1954). Hebb assures Thomson that these unscientific false-starts are in the past and that he and his colleagues (i.e., Ferguson, Morton, Blackburn, MacLeod) are establishing in Canada "the biological kind of psychology" (Hebb, Sept 23, 1954). This, Hebb explained he hoped to do, through the strategic

placement of physiologically-sympathetic academics at key institutions throughout the country with "excellent relations with medicine and biology" (Hebb, Sept 23, 1954).

Certain Canadian psychologists, especially Hebb, were keen to maintain the structures of funding they had helped establish (and controlled) during the war. Since federal funding for psychology was limited and American philanthropies could not be counted on to support Canadian interests, experimental psychologists were reliant on expanding financial relations with the government and military. This strategy was based on the understanding that psychology should move closer to the biological sciences and eschew development into applied and professional domains (contrary to the direction of changes in the US). The reasons provided for why Canadian psychologists should make this move -- eligibility for federal support, access to superior students, and respect associated with scientific authority -- were interconnected and mutually reinforcing.

4. Tensions between Academic and Applied Psychologist in Canada

According to MacLeod, the "burning question of mid-twentieth century psychology in every country" was whether psychology was "too strongly oriented towards professional practice" (MacLeod, 1955, p. 51). Academic psychologists lamented what they saw as a crisis of identity that threatened the discipline's journey towards authoritative science and affected their opportunities for expansion and financial support from the government. While the period following the war is sometimes referred to as the "golden age" of the NRC (Philipson, 1991), funding for research psychology remained concentrated among a relatively small group of academic psychologists, many of whom had direct links to McGill University as some of the first doctoral graduates from the psychology department (Ferguson, 1982). The total number of academic psychologists in Canada in the 1950s was small compared to the United States (Wright, 1974) and Montreal--Canada's fastest growing city and 'poster child' for postwar prosperity and modernity (Linteau, 2013)--quickly rose to prominence as a hub of experimental university research (Frost, 1984; Shore, 1987). For example, Montreal was host to the International Congress of Psychology which brought such luminaries as Cambridge University's Frederick Bartlett (1886-1969), Berkeley-based cognitive behaviorist Edward Tolman (1886-1959), and French developmentalist Jean Piaget (1896-1980) to Canada in 1954. Montreal played a pivotal role in the development of postwar psychology and key members of various

CPA committees, such as MacLeod, Morton, Webster, and de Bois, were highly sympathetic to Hebb's vision for Canadian psychology (e.g., MacLeod, 1955).

There was considerable animosity between those interested in advancing experimental approaches to psychology and those interested in applying these new understandings to practical concerns (such as industrial or clinical applications). As Ferguson (1984) many years later noted, "Hebb was completely dedicated to psychology as a science, and wished no association with its applications" (p. 198). Many Canadian psychologists spoke out about their concern regarding the broadening of psychology's sphere into matters of practical affairs; the idea that "the demonstrated usefulness of psychology may have played an unnecessarily prominent role in its subsequent development" (MacLeod, 1955, p. 13) was not uncommon (Dobson et al., 1993). As time went on "available funds were directed more and more to basic scientific research" (Ferguson, 1984). This confluence of power led to rising tensions between basic and applied psychologists throughout the 1950s.

At its core, the expressed concern was that psychology, as a scientific discipline, had not yet advanced sufficiently to be able to provide the kind of service that its professional advocates often sold to clients. This was a concern over the 'overreach' of psychology into matters they were not suited to address. It risked diminishing the gains (towards greater legitimacy) achieved during the war and affecting the public perception of the field during a crucial period in its development. The stakes were seen as nothing short of the future of psychology in Canada. This perspective, that psychology had not yet developed sufficiently as a science, contributed to the attitude that the interests of scientific psychology were in inherent opposition to those of professional psychologists (e.g., Fernberger, 1947; Tryon, 1963).

Many of the organizational elite in Canada maintained that experimental research constituted the search for the underlying unity of psychology which would ultimately be expressed as a set of principles upon which the science would be based (MacLeod, 1949; Hebb, 1949, 1951). The commitment to the search for these underlying principles was what united psychologists, rather than the existence of any specific shared theories or practices. Psychologists interested in applying psychological knowledge (in the absence of such principles) could be seen to contribute to the fragmentation of psychology, driven by "man's age-old interest in the problems of epistemology, of ethics, and of politics" (MacLeod, 1949, p. 212).

With massive postwar changes to university-based behavioral research came a greater sense of obligation towards the public good (as demonstrated by the primacy of government utility). The organizational elite recognized that ongoing public support would rely upon the capacity of psychologists to demonstrate the value of significant public expenditure. The hope among an influential segment of the organizational elite in Canada was that psychology must be guided along a path similar to that of other biological sciences towards greater truth and epistemological unity; it was the responsibility of academics that oversaw the expansion of Canadian psychology to reign in their public promises, learn from their mistakes (i.e., mental hygiene), and strive towards greater scientific authority (Hebb, Sept 23, 1954).

5. "More than a single discipline": Tensions between applied and basic psychology

The end of the war brought with it new concerns regarding the future of psychology in Canada. These concerns were centered mainly around tensions that arose between applied and basic psychology. The postwar period of Canadian psychology has been referred to as the "frustrating fifties" (Williams, 1992) and as a "divisive" period (Ferguson, 1984). The call for more applied psychologists after the war, combined with the availability of new sources of federal funding, provided significant incentive for the rapid development of professional psychology in Canada. As Wright notes, "Psychology had been 'sold' and its services were demanded by government, industry, health and education. Society was not prepared to allow psychologists to retreat into the ivory tower … " (Wright, 1974, p. 120). This led to increased uncertainty regarding the orientation of the discipline.

In the United States, the tensions that arose between basic and applied psychologists led to the support and growth of professional psychologists, especially in the realms of clinical psychology (Buchanan, 1997; Conway, 1984). In Canada, concern arose because of the way in which the government had decided to distribute funds (Myers, 1958; Wright, 1969). The nature of this distribution likely had something to do with the way in which the organization of psychology in Canada was dominated by experimental psychologists, such as Hebb, who had sought to prioritize the development of a scientific psychology, and who contributed importantly to the establishment of funding for academic psychology in the first place.

According to Ferguson, prior to the war, "there is no indication of conflict between the scientific and the applied facets of our discipline" (Ferguson, 1984, p. 197). This may have been

because there were too few psychologists for conflict to arise, or that there were simply too few resources over which to compete. After the war, a survey of introductory psychology courses in Canada was conducted (Liddy & Neal, 1947). It is interesting to note that applied topics were among those identified as deserving more attention, while scientific and experimental topics were "frequently mentioned as receiving too much emphasis" in Canadian universities (p. 64). This points to a schism between the organizational elite and the majority of university-affiliated psychologists.

The organizational elite of Canadian postwar psychology, such as MacLeod and Hebb, shared the concern articulated by psychophysicist Samuel Fernberger (1887-1956): "Are we killing the goose that laid the golden eggs?" (Fernberger, 1947). MacLeod mirrored these concerns when he cautioned that it was "easier and more profitable to apply a few apparently established techniques than to continue the quest for further truth" and reminded his readers that the application of psychology to practical affairs must "always be kept secondary" to the experimental ambitions of its practitioners (MacLeod, 1949, p. 212). Ferguson (1984) notes that the relations between applied psychology and scientific psychology at McGill were "divisive" and relations "were not harmonious" (p. 198). Ferguson (1984) explains that, "it was thought by some that their [applied] work might 'contaminate' the research of their more pristine scientific associates [...] Hebb was completely dedicated to psychology as a science, and wished no association with its application" (p. 198). For the most part, the funding that became available from the NRC and DRB during these early years was *not* available to those with interests in clinical and professional psychology. This meant research and training in applied areas was consistently underfunded and available funds were directed towards basic scientific research. The relative growth of applied, compared to basic, research during this period was described as creating disciplinary "bipolarity" (Tryon, 1963).

5. a. One department, divided

While Hebb guided the direction of psychology at McGill, this is not to say the department was immune from the same infusion of interest in applied psychology as the rest of psychology in Canada. Hebb expressed reserved appreciation for the applications of psychology for the betterment of society (after all, he was a functionalist with a background in education) and was optimistic about the social changes that would be brought about by psychological expertise (e.g., Hebb, 1953; 1958). In a letter to university official Hebb noted, "Applied

psychology in its proper sphere has demonstrable value, in the hands of one who is trained for it and who knows its limitations" (Hebb, 1949, #151649). However, Hebb expressed concern (e.g., Hebb, Feb 23, 1953) about the dangers of overselling the utilitarian potential of applied psychology.

There was a long history of applied work under Tait (see Chapter One) and both Webster and Bois continued the industrial-organizational division after Hebb and Ferguson arrived in 1947. Frances Alexander (n. d.), a clinical psychologist about whom little is known, ran what little existed of the clinical aspects of the department. Throughout the 1950s applied psychology continued to grow in Canada and at McGill. Training was largely at the master's level. During the period 1943-1957, there were 1,047 graduate degrees awarded in Canada, of which 84% were master's degrees (Dorken, Walter & Wake, 1960). Canadian research was generally applied in orientation. Wright (1969) reported that in 1948 about 90% of the 43 master's theses that year in Canada were applied.

In 1948, just before MacLeod departed, he and Webster launched what was originally known as the Staff Developments Institutes (McGill University, 1960; MacLeod, Nov 4, 1947), which comprised a number of smaller 'course delivery units' (such as the Administration and Management Problems Institute and Sales Management Concepts) (McGill University, 1960). Today, these university courses would have been part of a business school; they served a range of Montreal-based companies.

These kinds of institutes were commonly established at universities to provide advanced training for new applications in industry, public administration, and social policy (Heyck, 2015). McGill modeled its institutes on the Purdue Personnel Institute (MacLeod, Nov 4, 1947). For example, in 1957 the Human Engineering Institute was added to the Staff Development Institutes to train professional engineers and designers in recent advances in human factors research. The Staff Developments Institutes became an important part of the department, both as a source of department funding (enrollment in courses and workshops) for applied research and as the face of psychology in the 1950s. The Applied Psychology Centre was established in 1953 and Webster was made its Director, until 1958 when he became the Chairman of the Department following Hebb. By the mid-1960s, however, the CPA's support of industrial psychologists began to wane such that many applied and industrial psychologists moved away from

psychology departments and were integrated as part of MBA programs at Canadian universities (Dobson et al., 1993).

Hebb's strategy for dealing with applied psychology remained consistent throughout his tenure as chairman: to establish a degree of distance from, and subordination to, more basic forms of academic psychology (such as experimental and laboratory-based psychology). Hebb enacted a strategy of demarcation when, shortly after he had become chairman in 1948, he introduced to the McGill Senate the creation of an entirely new degree to be offered separate from the Ph.D. in Psychology: the Psych. M. and the Psych. D. degrees. The central purpose of these new degrees would be to simultaneously recognize the utility of psychology outside academia while also preserving the purpose of the Ph.D. degree designation as one having been trained to conduct scientific research – a qualification seen as unnecessary for applied or professional psychology in the 1950s. The decision to adopt these degrees likely followed the same recognition of the coming professionalization of psychology that prompted the Boulder model in the U.S. (see Baker & Benjamin, 2000).

Hebb's stance was that graduate training in psychology was to prepare individuals for academic research work. The tendency for his department to emphasize psychological research was based on the understanding of the primacy of experimental activities. For Hebb, experimental research on behavior was inherently physiological (Hebb, 1980) (see Chapter Three). He noted that, "though experimental is not more important than applied psychology or social psychology it does have a different status in training. Psychopathology, or group dynamics, is not basic to experimental psychology; but experimental is basic to clinical or social … " (Hebb, 1958c). His writing suggests a perceived asymmetry in the relationship of applied to basic psychological research and practice. Hebb's activities remained closely tied to scholarly work in psychology and historical evidence for Hebb's support for the kinds of spaces created by corporate or government research (e.g., the Staff Development Institutes) is not easily apparent.

Most accounts agree that the efforts of the CPA to promote the relevance of psychology to all aspects of Canadian life after the war was largely successful (e.g., Gleason, 1999; Wright, 1969). Demand from government and industry was such that universities, in large part, were unable to meet this new demand for applied psychologists (in service to state and industry). Departments were under tremendous pressure to produce trained psychologists able to contribute to the specific concerns of government and industry. One of the pratical concerns held by

experimental psychologists, such as Hebb, was simply that excellent researchers were being lured away from experimental work and towards applied lines of work. The introduction of the 'Psych' degrees in the late 1940s was thus part of a negotiated attempt to address this shortage. Courses for this degree would be taken not only in psychology but also subjects relevant to the selected applied field, such as mental hygiene, educational psychology, student guidance, and industrial applications.

The creation of these degrees was met with criticism and concern (e.g., Hebb, Apr 6, 1949). Hebb defended these degrees in a letter to T. H. Matthews (Registrar) by the request of the Senate (Hebb, Apr 6, 1949). The main objection was that persons with such degrees might turn to the treatment of mental illness ("or to some other form of quackery"). Hebb noted that the dangers of potential "quackery" already existed and that these degrees would do the opposite, it would actually minimize these dangers. MacLeod (1955) later described these psychologists (who would later become clinical psychologists) as "disappointed pre-medics" and "ill-balanced individuals with an interest in the abnormal that approaches the morbid" (p. 46). Psychology was a basic science.

The new degrees might better satisfy this particular demand and Hebb was explicit about the reasoning behind his support for these degrees. He said, "I want to protect the Ph.D., and, by sending the applied psychologist to the class-room, the library, and the work situation, to avoid lowering our sights in our demands on the research man and to get more time to give him individual attention" (Hebb, Apr 6, 1949). Hebb recognized that psychology was in the process of "becoming more than a single discipline" and the new degrees reflected that fact; Hebb wanted to make sure there was adequate space for "the research man" in the unfolding postwar organization of psychology.

5. b. Establishing boundaries: the Applied Psychology Centre, 1953-1965

Despite Hebb's arguments in favour of these degrees, he was unable to establish them at McGill. While he had managed to convince the university administration that both degrees were necessary, he was unable to convince most departmental staff at McGill or elsewhere to endorse it (Webster, 1988). The doctoral degree (Psych. D.) was quietly dropped after one year and the Master's degree (Psych. M.) was changed to M.Sc. (Applied). Similarly, the University of Ottawa Psy. D. programme was proposed in 1954 but never received Senate approval (Shevenell, 1984). Thus, the problems associated with applied psychology (as Hebb saw them) continued

throughout the 1950s. More recently, the Psy. D., or Doctor of Psychology, is designated a professional doctoral degree for practice in psychotherapy or psychological testing (Peterson, 1997), different from what Hebb had proposed in the early 1950s.

In 1953, Hebb wrote to Principal James concerning "the whole problem of applied psychology here [at McGill]" (Hebb, Feb 23, 1953). For Hebb the main concern was 1) the relation of academic psychology to the training of applied psychologists, 2) applied psychological research, and 3) the provision of psychological services at the University (Hebb, Feb 23, 1953). Hebb's suggestion was to shore up resources for the applied aspects of psychological practice. The problem, as he described it, was that "our ivory-tower research is too successful, drawing students away from the applied area, including those whose aptitude is not really academic" and goes on to say that he believes this to be one of the reasons that Webster was discussing *resignation* (Fieldhouse, Dec 11, 1952): "he has not been getting enough good men to work with him, whereas four years ago he was" (Hebb, Feb 23, 1953) -- though other factors were likely at play as well, such as the arrival of Hebb and subsequent focus upon laboratory-based research. On the topic of intra-departmental tension, Ferguson later reflected, "I have viewed, at times with astonishment, the rigid antagonism against applied work held by my friends and colleagues of the scientific persuasion" (1984, p. 198). And, to be sure, the professional-scientist schisms of this period -- which were deep and bitter -- were in no way isolated to McGill. Similar rivalries existed, for example, at the University of Toronto during this same period (Myers, 1982).

Shortly after MacLeod departed, and unable to have the Senate adopt his new degrees, Hebb proposed an effective division within the department between his experimental program and Webster's applied program. In 1953 this division was formalized with the creation of a "semi-autonomous administrative unit" under Webster's direction (Ferguson, 1982, p. 63), the Applied Psychology Centre. Hebb's expressed hope for this Centre was that it might bear the same relation of psychology as the School of Commerce to the Department of Economics and Political Science. The function of this centre would be 1) to divide academic functions and in doing so lighten the administrative burden for the chairman (i.e., Hebb), 2) to limit the number of students admitted to graduate work in the academic area of the department (i.e., experimental program), and 3) to offer freedom to provide extension courses and access to what Hebb described as, that "untapped supply of contract research and development work for business and

the armed services that we have not been able to do anything about" (Hebb, Feb 23, 1953). The Centre's staff included W. N. McBain, E. G. Poser, and part-time lecturers D. Dörken, G. Dufresne and B. Wigdor. It was the principal agency for training and research in clinical, industrial, and counselling psychology at McGill from 1953 to 1965 (Ferguson, 1984). In many ways, Hebb hoped this Centre would compartmentalize and therefore alleviate the tension between the kind of psychology that was in popular demand (applied) and that which he planned to build (scientific). Having relegated much of the practical activities and external demands of the department to the Applied Psychology Centre, Hebb was able to focus, as chairman, on what he saw to be the more important experimental and laboratory activities of the department (see Chapter Five). Historian Otniel Dror describes postwar psychology at McGill as an "emergent, bifurcated, and schizoid field" (2016, p. 231).

5. c. Clinical Training at McGill in the 1950s

In addition to helping the government figure out how to fight the next war, psychologists were also involved in rehabilitating casualties from the last one. The high incidence of neuropsychiatric problems among Canadian veterans created a demand for psychological diagnostic and therapeutic services that was filled by newly trained clinical psychologists, primarily in the United States (Pickren, 2007; Capshew, 1999). The education of these new clinical psychologists was largely underwritten in the U.S. by the Veterans Administration and the U. S. Public Health Service. In Canada, the situation was different. Compared to the United States, there was a lack of incentives for the development of professional training programs (Dobson, 1995). According to Webster (1984), while "Ottawa wanted to develop applied psychology, including clinical practitioners [...] It was the academic psychology community that rejected much of the offer" (p. 196).

When Hebb had introduced the new applied psychology degrees in 1949, certain medical members of the university had expressed fears they might lead to "some of the difficulties that had arisen in the United States over professional 'clinical psychologists'" (Matthews, Mar 29, 1949). The difficulties to which they referred were likely related to the role of psychologists in mental treatment, specifically, ownership of psychotherapeutic practice (Buchanan, 2003) and the rise of projective testing (Benjamin, 2005; Buchanan, 1997).

Hebb was familiar with early attempts to apply psychological understanding to problems in education and public health and the detrimental effect psychology as a scientific discipline had

endured as a consequence. In the last chapter of *The Organization of Behavior* (1949) he notes the general state of ignorance regarding the etiology of mental illness and the pitfalls of the mental hygiene movement (1949, p. 264). He stated brusquely, "Before one can have applied science, one must have a science to apply" (Hebb, 1958, p. 19). Hebb was cognizant of the fact he was in the process of shaping the discipline of psychology as it would be practiced in Canada; he walked a fine line that courted public interest and investment while upholding a strict adherence to the standards and scientific norms of the biological sciences. Hebb had done much to develop his standing as a 'hard-nosed' scientist and he did not want to jeopardize this reputation by losing control of the experimental focus of academic psychology in his department. Indeed, professional psychology in the United States was in the process of diverging from its academic base (Dobson, 1995) and the Canadian organizational elite looked upon these developments with trepidation.

Postwar psychologists sought to sidestep many of the challenges their American counterparts faced by orienting the discipline firmly towards the natural sciences. Part of this orientation involved narrowing psychology's therapeutic reach. This was done, in part, through the direction of federal spending on applied psychology. Whereas funding for experimental psychology was channelled directly to academic departments of psychology, the funds for applied research were less direct (Wright, 1969).

It was not until 1948 that additional funds from the Department of Health and Welfare were made available (Williams, 1992). These grants contributed, for the first time, to the creation of university positions in areas of clinical psychology in Canada, but were eventually discontinued (Ferguson, 1982). Abraham Luchins (1914-2005), who supervised a number of graduate students at McGill between 1950-1954 (including Norman Endler) were employed under these grants. It is unclear why these programs failed. Ferguson later suggested that it might have been because "the spokesmen for psychology, whoever they were, lacked the political will or power, or both to effectively influence government policy and practice" (Ferguson, 1984, p. 198). This seems to have been the case for the department at McGill: Hebb was "extremely critical" of clinical psychology and opposed its development (Hebb, Apr 6, 1949), despite having financed two academic staff positions with health grants up until the late 1960s (Webster, 1984), and ongoing attempts by Webster within Hebb's department to develop the clinical program as part of the Applied Psychology Centre (Webster, Mar 8, 1956).

The situation at McGill supports Buchanan's (2003) claim that clinical psychologists were "stalemated by a lack of support from their academic colleagues," and were, "unable to convince experimental purists" (p. 228); however, Hebb complicates this picture. Although Hebb opposed the establishment of clinical training, he encouraged his students to work alongside these settings in support of the activities of the Montreal Neurological Institute and Laboratory of Psychological Studies at the Allan Memorial Institute, mirroring his own experiences working alongside physicians in the late 1930s. Perhaps the most notable example is that of Brenda Milner (b. 1918) who was encouraged by Hebb to study the lateralization of temporal lobe function with Penfield at the MNI in the early 1950s (see Chapter Five). The history of psychology at McGill is replete with examples of collaboration between psychologists and neurologists (e.g., Feindel & Leblanc, 2016; Prkachin, 2018). Hebb, like Oliver Zangwill (1913-1987) in the UK (Collins, 2006), established elite networks of research neurologists (such as Francis McNaughton and Herbert Jasper) and multidisciplinary psychologists (such as Peter Milner, James Olds, and Ronald Melzack). Reflecting on her first encounter with Hebb, Brenda Milner (1998) noted that what made Hebb attractive as a doctoral adviser was not his theories *per se*, but rather that his biological focus that made her own experience in the more biologically-oriented psychology of England relevant and applicable within an environment dominated by behaviorism (North America). Both Zangwell and Hebb shared a vision for the role of psychologists in relation to medicine: not in subordination or in competition with the clinical professions, but rather as behavioral scientists who worked alongside medical practitioners in the service of an experimental biological science of mind.

Historical accounts of this relationship, between psychology and medicine, in the postwar period tend to be marred by conflict and disciplinary boundary-work. The picture suggested by the relationship at McGill is different. While early clinical psychologists struggled for recognition and authority through the legitimation of their expertise (see Buchanan, 2003; Ward, 2002), Hebb offered an alternative. Hebb proposed psychological expertise (and therefore its utility) was in continuity of purpose with the biological sciences. Hebb promoted and contributed to the emergence of a particular expertise and gained access to and helped create networks between the Faculty of Medicine, the MNI, and the Department of Psychology at McGill. He leveraged the access he had through this network for his students, many of whom in turn gained access to neurological patients (clinical case studies) through these connections.

Fundamentally, the continuity with biology meant that this approach did not challenge or undermine existing medical establishment as professionalized health in the early nineteenth century had (e.g., mental hygiene, nursing, social work, etc.). Hebb explained how psychologists play an important role in the synthesis across biological disciplines through the dual mirrors of nature (heredity) and nurture (environment) in the organization of behavior (i.e., learning and motivation) (Hebb, 1949; 1953).

In their respective countries, Hebb and Zangwill played crucial roles in the identification and creation of opportunities to test patients with brain damage; both leveraged this expertise to build their careers. Hebb, unlike Zangwill, however, was surreptitious about promoting the relevance of psychological expertise in clinical settings. Instead, he wanted to foster cooperation through shared subject matter (and increasingly shared methods) with neurologists (specifically, those available through partnership with the MNI; Prkachin, 2018).

In Canada, clinical psychology developed differently than it had in the United States. In contrast to the postwar development of clinical training in the US (e.g., Raimy, 1950; Roe, 1959), clinical training did not develop as a strong professional specialization, at least not at the doctoral level; there was little professional status according to the clinicians of this era (Myers, 1958). They were denied status by employers, by those established in other professions, and by their academic colleagues in psychology (Conway, 1984). Their roles in applied settings were restricted, their power was negligible, and their salaries were meagre (Gibson, 1974; Sutherland, 1964; Wright, 1969).

At McGill, while applied psychology continued to expand under Webster and Bois, clinical psychology failed to develop outside the Applied Psychology Centre. When Hebb had originally suggested a graduate degree dedicated to applied psychology, Dean Thomson reminded him that, “Our medical friends naturally take great alarm at any mention of anything called Clinical Psychology [...] They think that clinical psychologists without medical qualifications are very dangerous when they set up in private practice” (Thomson, Mar 21, 1949). Hebb reassured him that, “I have been, and am, extremely critical of clinical psychology” (Hebb, 1949).

Hebb took advantage of a strategic division of labour: while Father Noel Mailloux (who ran a clinic at the University of Montreal) and Robert B. Malmo (who ran a laboratory at the Allan Memorial Institute) waded into the dangerous waters of psychotherapy and

psychosomatics, respectively, Hebb focused on expanding the biological scope of psychology in the Department of Psychology: “McGill with its emphasis on biologically oriented research, and the University of Montreal, with its excellent facilities for clinical and developmental studies, have developed their departments in complementary fashion [...] There has been a constant and fruitful interchange between the two” (MacLeod, 1955, p. 50). Indeed, while clinical psychology expanded in the United States, no courses or clinical staff were added to the department at McGill. This was in large part due to Hebb's particular stance on clinical psychology, which reflected a very narrow relationship between psychology and medicine along therapeutic lines: as long as psychologists didn't step on any toes, there could be a role for psychology in medicine. In the clinic, Hebb believed the psychologist had only two possible functions, “1. research [i.e., Malmo’s Psychological Laboratory], and 2. assistance to medical staff under medical supervision [i.e., psychological testing at the MNI]” (Hebb, 1949).

While clinical programs were being established all over the United States, in Canada these programs were delayed. Although Hebb would later deny he played a significant role in curtailing the development of clinical psychology at McGill (Hebb, 1984: see response to Conway, 1984), he was clearly uninterested in its development. In a letter defending the utility of the new professional degrees, Hebb explicitly separated these from therapeutic activities. In addition to being “extremely critical of clinical psychology,” Hebb notes, “we have steadily refused to offer training in this field” (Hebb, Apr 6, 1949). It was only after Hebb stepped down as chairman in 1958 that the movement towards establishing a clinical program at McGill, through the appointment of Virginia Douglas (1927-2017) who pioneered clinical training at McGill in the scientist-practitioner model she had adopted while at the University of Michigan (Bazar & Giri, 2014). The clinical work initiated by the Applied Psychology Centre therefore continued in the form of a clinical Ph.D. programme designed by the scientist-professional training model following the Couchiching Conference in 1965, the Canadian equivalent of the Boulder conference (see Conway, 2000 for more about the history of CPA certification 1947-51).

The surge of interest in applied psychology taking place at the time failed to take hold at McGill as it had in the United States. In his later years, Ferguson, who had been hired with Hebb by MacLeod in 1947, attributed this to the fact that psychologists such as MacLeod and Hebb who had spent the war years outside Canada lacked knowledge of the successes of that period

and failed to see the broader potential of scientist-professionals (Ferguson, 1984). As a consequence, starting in the late 1940s, and continuing until the mid-1960s, the activities of the McGill psychology department were divided between those of the Applied Psychology Centre, run by Webster, and those of Hebb's experimental research program, which established close ties to Montreal's major English-speaking psychiatric and neurological institutes (see Chapter Five).

6. In Support of a Science of Unsolved Problems: The MacLeod Report

One of the most influential reports to affect the direction of psychology in Canada and at McGill was the publication of the *MacLeod Report on Psychology in Canadian Universities and Colleges* (1955). This report has been used during and since to understand the status of psychology in Canada in the 1950s (e.g., Myers, 1958; Wright, 1969; Belanger, 1992; Conway, 2000). Written explicitly for "everyone engaged in the teaching of psychology in Canada" (Liddy, 1955, p. 145), it was recommended that the report be "read and re-read, digested, and absorbed into the bloodstream of all who call themselves psychologists in Canada" (Ketchum, 1955, p. 99).

Robert B. MacLeod, former McGill graduate and department chairman (see Chapter Two), Head of the Psychology Department at Cornell (Ryan, 1982), and key contributor to disciplinary education in the field (e.g., Buxton et al., 1952; MacLeod, 1964), was jointly commissioned by the Canadian Psychological Association and the Canadian Social Science Research Council (Liddy, 1955) to conduct a survey concerning the state of psychology in Canada. This survey examined the place of psychology in the academic community and in a liberal arts curriculum and evaluated the facilities and programs of training in Canada. The result of this report was twofold: 1) it recommended that applied topics be reduced or removed from the undergraduate curricula, and 2) it explained how the situation of academic psychology in Canada was such that a meeting should be called to formally address its greatest challenges.

In general, it was presented as a cautionary report on the direction psychology was taking and an urging to "restore the position of psychology as a science" (MacLeod, 1955, p. 16). Taken in historical context, this report might be read as both part of an ongoing strategy by a small group of academic psychologists and an explicit move away from professionalization (Conway, 1984). This group of psychologists, which included Ferguson, Morton, Zubek, and Thompson, were each affiliated with the department at McGill and all sought to advance Hebb's "biological kind of psychology" (Hebb, Sept 23, 1954). Members of this group filled the ranks of the CPA

in the early years (Conway, 2010), and many, after spending time with Hebb at McGill, went on to shape departments of psychology in other universities. For example, Zubek was appointed Head of psychology at the University of Manitoba in 1953, Thompson returned to Queen's in 1954, and McMurray (who had studied with Malmo at McGill) went on to create "a strong academic, experimental and research-oriented department" at the University of Saskatchewan in 1949 (McMurray, 1982, p. 184).

The rhetoric characteristic of this period (e.g., fear over applied psychology) was also associated with broad concern regarding the relative power of the experimental "core" and its control over the federal structures of patronage that were taking shape (i.e., the ongoing Massey Commission). The threat that was looming was expressed in the report as the danger of "becoming a profession before we had become a science" and of "being unduly influenced by the success of psychologists in applied fields" (MacLeod, 1955, p. 5).

MacLeod explained that among the challenges Canadian psychology faced was the reality that "scientifically trained university men, who had acquired practical experience in the armed services, were now being offered far more lucrative posts in industry and in government than their universities could provide, and the universities found it difficult to lure them back into teaching" (MacLeod, 1955, p. 15). MacLeod stated it plainly: "There were simply not enough psychologists to go around" (p. 15). Canada's organizational elite felt this concern; scarcity of personnel and resources was certainly the situation MacLeod had faced when charged with the rebuilding of the department at McGill (see Chapter Two). The maintenance of balance was important and only about a third of psychologists at the time were affiliated with any university of college (MacLeod, 1955).

The concern that psychologists might lose disciplinary privilege and prestige by becoming overly professionalized was expressed. The rapid creation of new applied positions outside universities, according to MacLeod, threatened to relegate psychologists to members of "the 'technique' fields" (p. 38), as tensions between clinical psychologists and psychiatrists intensified at this time (e.g., Buchanan, 2003): "most 'professional' psychologists in Canada are now rated and paid as technicians" (MacLeod, 1955, emphasis added). MacLeod believed basic research was being threatened by what Liddy (1955) described as an "emphasis on hasty techniques for manipulating our fellow men" (p. 145), noting the rise of popular psychology at this time (e.g., Benjamin, 2012).

Among the primary problems MacLeod identified regarding applied psychology was the perceived difficulty associated with attracting high quality students and instructors. MacLeod expressed concern that applied subject matter decreased the quality of teaching and that students with experimental aptitude would gravitate away from a discipline associated with the "technique" fields (MacLeod, 1955). Furthermore, without scientific prestige, Canada would continue to contribute many more psychologists to the United States than it would receive in return (MacLeod, 1955, p. 42). The threat to Canadian sovereignty was real: "Canadian psychologists are becoming steadily Americanized" (p. 61), a concern to many Canadians at this time (Wright, 1969). Therefore, Myers, along with Hebb and many other leading academic psychologists in the mid-1950s, were convinced the only way to improve the discipline was to attract and retain the very best scientific research faculty possible.

In addition to a general lack of support for the development of applied programs among Canadian academic psychologists, there was the stated belief, held by MacLeod, Hebb and others that the application of psychology was simply premature, popularizing the term "premature professionalism" to capture the state of the discipline (MacLeod, 1955) in relation to developments in the United States and as a scientific field as a whole.

Concern over premature professionalization in Canada had been growing since the end of the war. The "enthusiastic movement" toward the professionalization of psychology in Canada has endangered both "psychology's position as an essential contributor to liberal education," and "the future of psychology as a legitimate profession" (MacLeod, 1955, p. 60). By the late 1950s, it was estimated that the ratio of professionals to scientists was 6 or 7 to 1, that only a quarter of the psychologists in applied settings possessed a doctorate, and that an alarming 21% had no graduate degree at all (Myers, 1958). Certification of Canadian professional psychologists was a subject of much interest and debate in the early 1950s (see for example Conway, 2000, p. 10-17; Bott, 1947; Bois, 1948; Myers, 1958).

This report also opened doors to broader concerns about how psychology as a discipline (academic and profession) was viewed by the Canadian public (Steer & Cox, 1957; Gaddes, 1960). MacLeod (1955) had identified at least four ways that psychology was being presented to the public: "In Canadian educational institutions it is not clear whether psychologists are philosophers, natural scientists, social scientists, child specialists, mental testers, diagnosticians, therapists, or efficiency experts, or a strange combination of all of these" (p. 60). MacLeod

noted, “one wonders whether any other discipline is as confused as to the nature of its own subject-matter” (p.38). Recognizing the danger presented by threats to a clear scientific identity, MacLeod described psychology as a “cross-roads discipline” (p.43), an interdisciplinary characterization with which Hebb likely agreed (Hebb, 1949).

MacLeod's recommendations, in addition to the survey he conducted in 1954, was based on conclusions reached by his participation in a small group of American psychologists who had spent a summer together in 1951 at Cornell University to discuss the undergraduate curriculum in psychology (Buxton et al., 1952). MacLeod's report echoed the same sentiment: for the good of the discipline, psychology must align with the practices and identity of the natural sciences: “[Psychologists] have been more fully accepted where the psychological emphasis is on the experimental approach, less where the emphasis has been clinical; [...] the psychologist, even when his problems are not understood, is generally regarded as a respectable citizen, if not as a fully respectable scientist” (p. 39).

In many ways MacLeod was critical of how Canadian psychology developed during this period. He saw the “practical” orientation which had historically characterized departments of psychology as a weakness, and strongly advocated the development of a rigorous basic science in psychology and the strengthening of academic psychology. In his report, MacLeod singled out the department at McGill, by virtue of its standing and degree of government support, as a model site for the amelioration of tensions that had arisen between applied and experimental psychology. In many ways, the McGill department was represented by Canada's academic elite as a *proof-of-concept* department for postwar psychology.

This report came at a crucial period for Canadian psychology. There were many interpretations of what psychology was at this time (e.g., Steer & Cox, 1957) and the report contributed to a particular vision for psychology, one which was supported Hebb’s psychology: MacLeod believed that “psychology must be presented as *a science of unsolved problems* that are not merely interesting but urgent” (p. 46, emphasis added).

7. Opinicon and the Future of Psychology in Canada

The concluding recommendation of MacLeod’s report was for the CPA to seek funds to support a meeting of a small group of leaders in Canadian psychology to discuss the problems he had highlighted (MacLeod, 1955). While this recommendation was echoed by many in the CPA

(e.g., Liddy, 1955), it was not set into motion until 1958 when a planning committee could be formed and an appropriate time and place could be arranged. Unsurprisingly, the membership of the Steering Committee ended up being "strongly oriented towards academic or strictly laboratory type research" (Bromiley, 1959, p. 105); there was only one member who represented the interests of professional psychologists, and that was Webster from McGill. Hebb was a key organizer of this conference and headed efforts in the CPA to secure funding for its planning and execution after he stepped down as chairman of the department; Webster assumed his position as chairman.

The *Opinicon Conference on the Future of Canadian Psychology* was organized for the summer of 1960 at Chaffrey Locks near Kingston, Ontario. It was jointly funded by the NRC and the Canada Council, the two main federal funding bodies. Applied and professional psychologists involved with the conference hoped tensions characterizing much of the postwar period would be addressed and a specific plan for training programmes similar to the United States would finally be developed (Myers, 1958; Baker & Benjamin, 2000). These psychologists were disappointed.

The funding bodies had apparently imposed conditions on the grants for this conference (which Hebb had secured) and in so doing stipulated that it would be limited to the teaching of psychology as a science and the development of scientific research training (Bernhardt, 1961; Belanger, 1992). The NRC and SSRC were restricted from providing funding for professional training of any kind. Participants of the conference were therefore limited to those "engaged in research training in Canadian universities" (Bromiley, 1959, p. 105). Despite professional credentials and the clarification of the relationship between academic and professional psychology being the original purposes of the conference (Conway, 1984), it was instead focused on strategy related to the promotion and expansion of experimental research in psychological training (Wright, 1984). An "academically peremptory" definition of psychology was promulgated by Opinicon (Mooney, 1961): psychology was defined as primarily a scientific discipline and secondarily a field of professional practice; this was now the official position of the organizational elite in Canada..

The deliberations which took place at Opinicon had a "substantial" (Wright & Myers, 1982, p. 19) impact on the direction taken by psychology departments during the period of rapid expansion that occurred in the 1960s (Wright, 1984). The Opinicon conference served as impetus

for the further development of research and laboratory training (Wright & Myers, 1982). The 1960s became the time when adequate resources for the development of high-quality graduate training programmes in psychology finally became available in Canada, and departments used them to redress their weaknesses. They geared up to be in tune with the times; to create laboratories, acquire technological equipment, and hire top-level researchers. One of the most influential participants at Opinicon was Hebb (Wright & Myers, 1982). And the goal that Hebb had enshrined as core to Canadian psychology was materialized in the aim to create a lively research climate in each university department in Canada. The surge of interest in academic research psychology had the opposite effect on the development of professional training (Conway, 1984). Applied programmes in the 1960s were "eschewed" (Catano, 1984, p. 208), and "often neglected, or their development deferred" (Wright, 1984, p. 194), though not completely abandoned (Gibson, 1965).

The Opinicon Conference was broadly recognized as having been organized for, and dominated by, research academics, as a consequence of the conditions attached to its funding (Bromiley, 1959; Conway, 1984; Wright, 1984). Frustrated that issues related to professional and clinical psychology, such as the training of psychologists for community service, were not addressed, another conference was organized (this time, dominated by applied psychologists). This conference was held at the Geneva Park Convention Centre on Lake Couchiching, north of Toronto. At the Couchiching conference, the Canadian equivalent of the "Boulder Model" (established 1949), which outlined training for clinical psychology (the scientist-practitioner model), was established (Raimy, 1950). Despite vigorous efforts, no grant support for this conference or the publication of its report were forthcoming from either federal or provincial governments, and the cost of both almost bankrupted the CPA (Webster, 1967; Myers, 1982).

Opinicon angered professionals and lingering divisions between academic and professional psychologists persisted into the 1980s (Myers, 1982; Conway, 1984). Reflecting on the period decades later, Hebb argued that academic opposition to applied and clinical psychology "had little effect" (Hebb, 1984, p. 192; response to Conway, 1984). Hebb argued that psychologists at McGill were unjustifiably blamed for stalling the development of clinical psychology in Canada. He justified their refusal to train clinicians "for whom there would be no jobs on graduation" and noted dryly that "we held to the peculiar idea that an applied science must have something to apply" (Hebb, 1984, p. 192). Hebb argues few academics were aligned

with MacLeod's report at the time, though historians disagree (e.g., Wright & Myers, 1982). Hebb claimed that he was not speaking for, or on behalf of, McGill in any way (Hebb, 1984), though historian John Conway disagrees (1984). Hebb resisted characterizations that clear divisions between applied and basic psychologists clearly existed, noting that the projects with which he was involved at the time failed to fit neatly into either category: "I claim to be located as much in the world of reality as in that of theory" (Hebb, 1984, p. 192).

It is likely the MacLeod Report had an impact on the direction of psychology in Canada. For example, a year after this report was published, Bott retired from the University of Toronto and consequently the department took a decidedly "scientific turn" (see Myers, 1982, p. 90). Myers cites the Opinicon Conference to have strengthened his resolve to have the department at the University of Toronto 'go scientific' (Myers, 1982, p. 91). This direction was taken by numerous other Canadian departments of psychology at the time: With the establishment of McMaster's department there was "a deliberate decision taken early in the department's life that we would develop an exclusively experimental department and thus concentrate our resources rather than spread them over a wide range of subject matter" (Newbigging, 1982, p. 135). John Zubek (1925-1974), who was "influenced most by Donald Hebb with whom he worked at McGill [1950-1953]," became Head of psychology at Manitoba (Wright, 1982, p. 174).

It is almost taken for granted that the tensions in the 1950s arose due to the stronger emphasis on experimental psychology within Canadian departments of psychology (Dobson, 1995; Wright & Myers, 1982). However, historians have tended to neglect an investigation of the sources and consequences (both good and bad) of this uniquely Canadian experimental-bias. I think important similarities can be drawn from the efforts of Hebb to separate scientific psychology from professional psychology in the 1950s to the efforts of Tait thirty-years earlier to separate psychology (which was largely functional or applied) from its roots in philosophy (see Chapter One).

The factors that influenced the character or flavour of psychology at McGill in the postwar period were multifaceted. They included institutional limitations (both at the level of universities and government) as well as the particular interests of organizations (e.g., CPA) and individuals (e.g., MacLeod and Hebb). The history of this period cannot be understood solely in terms of the achievements of individuals or a gradual progression of disciplinary cohesion, the history from this period suggests the development of psychology in Canada to have been much

more contingent and reliant upon the interests of networks of individuals in response to what they saw as specific challenges to their status and future.

Chapter Four Conclusions

Canadian psychology in the immediate postwar period was at a critical juncture. In the early postwar years arose early academic and clinical neuropsychology, which came from a set of specific conditions unique to Montreal and McGill at the time. Disciplinary tensions, availability of support, and the broad societal recognition of psychologists in Canadian society created an atmosphere of both enthusiasm and concern; at stake was the organization of future psychology, both academic and professional. Canadian psychologists looked to their neighbors to the south, as they had always, but now expressed caution about uncritically following without adequately taking into account the conditions unique to the Canadian context (e.g., MacLeod, 1948). One of the major issues at stake was how best to balance the demand from outside the discipline (i.e., the need for a practical applied psychology by government and industry), with the recognition inside the discipline of the dangers associated with overextending or overselling its utility (i.e., at the expense of basic research). Canadian psychologists in the 1950s, with limited resources, funding, and staff, struggled over how best to organize itself to develop into a discipline that mirrored the natural sciences; these struggles for identity, in context, help us understand how psychology in Canada and at McGill developed into the kind of psychology it became during the postwar period. It also provides an alternative account of the rise of neuropsychology that more fully takes into account the contributions of psychologists at McGill University.

After World War II, experimental psychologists affiliated with research universities at McGill, Toronto, Dalhousie, Queen's and Western dominated leadership positions in Canadian psychology (i.e., the CPA). These psychologists hoped to maintain the funding and support that they had established during the war. While strategies consisted mainly of selling psychology on its usefulness to government, the “difficulties that had arisen in the United States” (Matthews, Mar 21, 1949) suggested a different path for Canadian psychology was crucial. Hebb, and the department at McGill, were at the vanguard of these developments.

Psychology at McGill occupied a particular interdisciplinary niche at McGill, which expanded to include not only advances in 1940s neurophysiology (see Chapter Three) but also

research at the forefront of neurology coming from collaboration between members of the department of psychology and the MNI (see Prkachin, 2018). Similar to the “Harvard complex” described by Isaac (2012), these connections intimately shaped the institutional milieu in which McGill psychologists constructed their models of scientific practice. These psychological practices linked more to those of neurology than contemporary behaviorism (i.e., Morawski, 1986) and eschewed therapeutic expertise (e.g., Collins, 2006), placing McGill psychology more closely aligned with the biological sciences.

Canadian psychologists in the 1950s, such as Hebb, felt considerable pressure to compete with applied psychologists in order to define the discipline, both within academia and in society. This pressure contributed to the development of a particularly biological-oriented psychology at McGill, and created a psychological tradition specific to the period. This was possible in no small part to the wartime connections that Hebb and others fostered after the war and contributed to a disproportionate allocation of defense-related spending at universities such as McGill University. After Hebb stepped down, the 1960s came to reject the narrative of meaningful separation between applied and basic psychology, especially in the field of clinical research and practice. Research in the 1950s was characterized by the desire to establish psychology on the firm footing of a biological science, in the 1960s (post-Opinicon) Hebb's vision for psychology as an integrative field (Hebb, 1949) was made to reconcile with the realities of the challenges faced (Hebb, 1960).

After the Couchiching conference the argument about “premature professionalism” was gradually replaced with a different set of concerns. The post-Hebb 1960s saw a flourishing of clinical research and training at McGill. To what degree then should we take serious worries about 'over-selling' psychological expertise to the public? Were they simply worried about losing control of an emerging discipline? Given the integration of experimentally-trained psychologists into academia after World War II, it is not surprising that conflicting interests emerged between them and the new classes of professional psychologists that were being created to address the new realities of a increasingly managed postwar psychological society.

Chapter Five: Between Brain and Body - The Legacy of Hebbian Neuropsychology

This chapter explores the unique styles of psychological research that emerged at McGill after World War II under the chairmanship of experimental psychologist Donald O. Hebb (1904-1985). By unique style of psychology, I refer to something less sweeping than A. C. Crombie's (1994) but aligned closely to Hacking's notion of 'styles of reasoning' (Hacking, 1992): a laboratory style that is mostly public, not entirely impersonal, but restricted in scope, in time and in space. This period, which spanned 1948 to 1958, marked the emergence of a psychology comprised mainly of blending the laboratory style of neurophysiology (in the traditions familiar to the Yerkes Laboratories and the Montreal Neurological Institute; see Chapter Three) with unique Canadian postwar professional and academic concerns (such as the patterns of funding and the promissory nature of biological science; see Chapter Four). It was in the reconstruction of the "frustrating" fifties that Canadian psychology had "come of age" (Wright, 1969; Wright & Myers, 1982). Yet, little historical research has yet few have examined the conditions giving rise to this remarkable period of discovery and innovation in Canadian history. Rather, the shifts and struggles characteristic of psychology at this point in its disciplinary history are incorporated into broader histories of the neurosciences, downplaying continuities with behaviorist thought and obscuring the specific, local conditions that shaped the kind of neuro-psychology that was made possible in Canada and at McGill at this time.

Hebb has been described as having "helped clear the way for the cognitive revolution" and to have "rejuvenated interest in physiological psychology" after World War II (e.g., Klein, 1999, p. 1). Indeed, he did so in a way that reflected a set of historical concerns and priorities that should be closely examined. The research conducted by Hebb, his colleagues, and their students during the 1950s put Canadian psychology on the map (MacLeod, 1955; Brown & Milner, 2003) and contributed importantly to the resurgence of the localizationist paradigm that undergirds much of our modern understanding of the human brain (Prkachin, 2018). While Lashley and the and early psychobiologists displaced the localization theory dominant in the 19th century (e.g., phrenology; Harrington, 1987), Hebb and his colleagues contributed importantly to the revival of localizationist thinking in psychology. Importantly, while this thinking is often tied to the rise of computational reasoning (the systems approach of the

cybernetic moment; Kline, 2015; Heyck, 2015) and the cognitive revolution in psychology (Gardner, 1985), Hebb's neuro-psychology is surprisingly "lively" (biological) in its approach to understanding thinking, or cognition (Hebb, 1949). The discoveries highlighted in this chapter (i.e., the psychology of isolation, rewards, and pain) are notably located in the bodies as much as the brains of rats, dogs, and humans; this chapter therefore draws on the history of emotions (Dror, 1999; 2001; Leys, 2010), cerebralization (Vidal, 2009; Vidal & Ortega, 2017) and subjectivity (Rose, 1998; 2003) to make an argument for the particular kind of psychology that emerge at McGill in the 1950s: a neuro-psychology of mind.

Following the momentum established by Wilder Penfield (1891-1976) and the pioneering efforts of the Montreal Neurological Institute (Guenther, 2016; Gavrus, 2011), Montreal and McGill were designated sites of national research priority in neurology and neurosurgery. The MNI and the Department of Psychology were supported by increases in postwar spending by the Canadian government in health sciences and higher education (Frost, 1984). McGill in the 1950s was becoming a hub for innovations in neurology, neurosurgery, psychiatry, and neuropsychology (Prkachin, 2018; Parent, 2017). This was the "golden age" of the MNI (Feindel & Leblanc, 2016). Among neuroscientific medical luminaries active in Montreal in the 1950s were William Cone (1897-1959), Herbert Jasper (1906-1999), David Hubel (1926-2013), and countless others (Preul et al., 1993). Even today, Montreal markets itself as a "city of neuroscience" (Tourisme Montreal, 2017).

The postwar surge of immigration to Montreal was accompanied by considerable investment in health services and medical research. For example, the Montreal General Hospital transformed from a community hospital serving anglophone Montreal to an internationally recognized academic centre during the 1950s and '60s (Hanaway & Burgess, 2016). New professionals such as economists, labour relations specialists, social workers, and psychologists constituted a growing share of Montreal's elite (Linteau, 2013). Academics were quick to leverage this newfound visibility and Montreal enjoyed scientific prestige as the site for international conferences (e.g., the International Symposium for Psychology in 1954), major scientific conferences, and significant infrastructural investments throughout the postwar period (Frost, 1984; Linteau, 2013).

To understand the conditions of Canadian research psychology in the 1950s, it is important to consider broadly the implications of Hebb's ideas on the thinking and work of

fellow psychologists at McGill. The department of psychology received, trained, and inspired numerous student and post-doctoral researchers both in Canada and abroad. The sensory deprivation studies conducted by Woodburn Heron, B. K. Doane and T. H. Scott involved "opening up new ground" while his other graduate students, including Peter Milner, Seth Sharpless, Mortimer Mishkin, were more involved with "cleaning up and systematizing the areas opened up earlier" (Hebb, 1952). Many simultaneous lines of research were being pursued in the department throughout the 1950s. In a report to the CPA in 1955 the department reported no less than fifteen concurrent areas of research activity associated with McGill University (CPA, 1955), significantly more than other Canadian universities at the time. McGill was among the most productive and influential departments of psychology in postwar Canada. In a 1957 survey, McGill contributed more articles by institution than any other. Hebb was (by far) the most referenced psychologist, followed by Americans L. L. Thurstone (a psychometrician) and Kenneth Spence (Hullian behaviorist) (McCormack, 1958). To be sure, McGill was not the only major hub for academic psychology after the war; Toronto and Ottawa each had well-established departments by the early 1950s (Wright & Myers, 1982), though the orientation remained dominated by applied concerns until the 1960s. At McGill, during the same period, the direction of academic psychology took a decidedly different direction.

Compared to the history of psychology in the United States and Europe, comparatively little has been written about the history of psychology in Canada. That which has been written consists primarily of "insider" histories. Administrative and departmental records prior to the 1970s are few and far between. The challenge of piecing together this period is made more challenging by both the scarcity of historical records from this period and the fact Hebb often chose *not* to include his name on projects to which he contributed, as a means to heighten the visibility of his students (Fentress, 1999). Furthermore, the kind of face-to-face influence that comes about through professional interaction (e.g., having a conversation or eating lunch together because the Department of Psychology and the Department of Experimental Medicine at McGill are physically located in the same building) tends to be more difficult to reconstruct through archival traces than collaborations based around written correspondence.

This chapter tells the story of how a handful of postwar psychologists at McGill—including Donald O. Hebb, Dalbir Bindra, Peter Milner, Seth Sharpless, and Ronald Melzack, and others—(re-)built their department and in doing so pioneered a different kind of psychology

in Canada. A series of case studies will illustrate how research and innovation affected, and were affected by, multifaceted concerns and priorities from this period. These cases include experiments into sensory isolation, explorations of reward mechanisms in the brain, and pioneering studies of pain psychology. I will examine some of the scientific and institutional contexts to which psychology at McGill during this period belongs, going beyond a disciplinary account of the achievements of Hebb and his ideas. This examination involves a close consideration of the factors that shaped the conditions of discovery and in doing so hopes to provide a more accurate understanding of the position of Hebb and McGill within the historiography of Canadian psychology.

1. Taking Shape: Hebb's Postwar Research Agenda, 1948-1958

Similar to many Canadian departments of psychology after the war, McGill underwent a period of unprecedented growth (Wright & Myers, 1982). When Hebb and Ferguson joined the department in 1947 the only other full-time faculty members had been MacLeod and Kellogg, with Webster and Alexander as part-time staff (Ferguson, 1982). Morton was still serving as part of the Defense Research Board in Ottawa (an important liaison to the department) and Murray de Jersey was on leave (MacLeod, Sept, 1946). Together with MacLeod, Hebb was given a mandate to modernize psychology at McGill, to bring it to the forefront of scientific excellence. The disciplinary changes induced by the war had created an identity crisis in Canadian academic psychology, one that had existed but had been largely dormant until this time (see Chapter Four). Hebb's appointment to McGill and subsequent leadership of its department of psychology symbolized an important commitment from the university and research community to establish Montreal as a hub for experimental and biological psychology in Canada. Hebb's first move, therefore, was to firmly establish the department's position. This involved the establishment of animal laboratories, the call for particular kinds of psychological (and non-psychological) expertise, and the mobilization of scientific research efforts across disciplines.

1. a. New students, new expertise

After Hebb assumed chairmanship of the department in 1948, his first hire was another Harvard-trained comparative psychologist interested in the biological underpinnings of behavior, Dalbir Bindra (1922-1980). In 1948 Bindra completed his Ph.D. at Harvard under the supervision of Joseph C. R. Licklider (1915-1990) on motivation and hoarding behavior of rats.

Licklider, who would become one of the most important figures in the history of computer science and general computing history (e.g., Licklider, 1960), was at that time a research fellow and lecturer in the Psycho-Acoustic Laboratory at Harvard. Bindra's study of behavior and motivation was deeply influenced by the work of E. G. Boring, Gordon Allport, S. S. Stevens, and others (Hebb & Ferguson, 1981); these Harvard psychologists contributed importantly to the shaping of postwar American psychology (Isaac, 2012) and a psychological form of Bridgman's operationalism (Green, 1992). Bindra, sharing Hebb's wide range of research interests in the neurophysiology of emotion and the neural correlates of intelligence, was among his closest collaborators in the 1950s. Though interestingly they only published once together, on scientific communication (Hebb & Bindra, 1952). Bindra was elected President of the CPA in 1958 (the third from McGill in five years) and Chair of the Associate Committee on Experimental Psychology of the NRC. In 1975, he was appointed Chair of the Psychology Department of McGill. Bindra published *A Theory of Intelligent Behavior* in 1976; this book extended Hebb's concept of the "cell assembly" and proposed an integrative system to explain adaptive behavior.

A number of Canadian psychologists who spent time at McGill in the early 1950s would go on to establish experimental practices elsewhere. John Zubek (1925-1974) and William Thompson (n. d.), two recent graduates, rural Canadians trained in the United States (like Hebb) with hopes to pioneer the new scientific discipline at Canada's major research universities. Zubek was a Czech-Canadian immigrant who completed a Master's degree in social psychology at the University of Toronto and a Ph.D. from Johns Hopkins University in 1950. Zubek spent three years as an assistant professor at McGill (1950-1953). Zubek was enthralled by the sensory deprivation research carried out by Hebb, although he was not directly involved with this work (Raz, 2013). Zubek and Hebb were close, he was among those Hebb depended to run the department during the early years (Hebb, 1980). Zubek accepted a position as Chair of the Psychology Department at the University of Manitoba in 1953. He continued and extended the studies Hebb and his group at McGill were in the process of conducting and maintained liaison between the two (Rosner, 2016). Zubek was "interested primarily in establishing psychology at the University of Manitoba as a distinct academic discipline […] the driving spirit of the department […] John made psychology at Manitoba known internationally" (Wright, 1982, p. 173). He established the Manitoba Sensory Isolation Laboratory and became an international authority on the subject (see Raz, 2013b and Harper & Bross, 1978).

William R. Thompson completed a Master's at Queen's University and a Ph.D. from the University of Chicago in 1951. Thompson spent his postgraduate years (1951-1954) working with Hebb in the Department of Psychology at McGill. Thompson was trained as a physiological psychologist in Lashley's long-shadow; his dissertation was titled, "Discrimination behavior of the cat after selective ablation of visual cortical area" and much of his work explored the heritability of intelligence and emotionality in rats and dogs (e.g., Bindra & Thompson, 1953; Thompson & Heron, 1954a; Thompson & Melzack, 1956; Thompson, 1957). Thompson returned to Queen's in 1954 for a couple years before accepting a position at Wesleyan University in Connecticut. In 1966, Thompson again returned to Queen's, this time as Head of the Department of Psychology (Inglis, 1982).

Haldor Enger Rosvold (1916-1997) was hired soon after Hebb's arrival to teach a course on personality and to supplement the supervision of students. Rosvold had just finished his Ph.D. at Stanford, where he had worked with Calvin Stone (Mishkin, 2001), a pioneer of early psychobiology and one of Lashley's early physiological psychology graduates (Rosvold, 1955; Pickren, 2006). Among Rosvold's first students were Sam Rabinovitch and Mortimer Mishkin (Rabinovitch & Rosvold, 1951; Mishkin, 2001). Rabinovitch, trained as a psychologist, was involved in the in the early development of child psychiatry in Canada; through his clinical expertise and work on learning disabilities, he established the Learning Clinic at the Montreal Children's Hospital (MacDermot, 1961; Ferguson, 1982), which has since been re-organized as the Child Development Program. Rosvold supervised Mishkin's Ph.D. research, which was being conducted in partnership with the Yerkes Primate Laboratory and neurosurgeon Karl Pribram (1919-2015), who had been one of Hebb's colleagues at Orange Park with Lashley in the 1930s (M. Mishkin, transcribed interview, 2001, p. 15).

Mishkin went on to conduct groundbreaking studies in the areas of memory and cognition; his work contributed to the discovery of two separate brain processes for encoding memories (Mishkin et al., 1983). He later became the National Institutes of Mental Health's chief scientist and was awarded the National Medal of Science in 2009. Mishkin would later note that, "Hebb was perhaps my most influential mentor ever, not because I had a lot of direct interaction with him, but because I was a student in his lab" (Mishkin, 2001, p. 5). In addition to teaching and supervising at McGill, both Mishkin and Rosvold were early neuro-psychologists and Hebb's allies, instrumental to the establishment of the animal laboratories in Canada and

abroad. Miskin and Rosvold both departed Montreal only a few years later to work with Carlyle Jacobsen as part of the Lobotomy Project at Yale (M. Miskin, transcribed interview, 2001, p. 9; Morawski, 1986).

Hebb ran the dominant experimental division of the Department of Psychology with the close help of Bindra, Thompson and Zubek (Hebb, 1980). Both Woodburn Heron and Peter Milner were "*de facto* instructors before they graduated" (Hebb, 1980, p. 300). This small group worked tirelessly to create an environment ideal for scientific training; in many ways Hebb aspired to model the pedagogical practices characteristic of the intellectual dynamics he had experienced in the interdisciplinary laboratories of Lashley (Dewsbury, 2002) and Penfield (Prkachin, 2018; Feindel & Leblanc, 2016). Accounts by graduate students from this time frequently reflect on the freedom they were provided to pursue their own interests and to experiment without restriction on subject matter or method (e.g., Mishkin, 2001; Fentress, 1999). Among the first graduate students to be formally supervised by Hebb were Bernard Hymovitch (PhD, 1949), Woodburn Heron (MA, 1949), Thelma Gordon (MA, 1949), and Donald Forgays (PhD, 1950).

The relations Hebb had established with the Montreal Neurological Institute (as well as other hospitals) meant that psychological research had access to clinical and experimental data that, for psychologists, was rarely available. With the range of experience and expertise Hebb had gathered at McGill, new experimental possibilities emerged (Prkachin, 2018). For example, Miskin recounts how, as a researcher in Hebb's department, changes in intelligence after surgery could be explored by comparing the intelligence tests of war veterans who had become schizophrenic and were 'treated with lobotomy' at St. Anne's Hospital. Ties to neighbouring hospitals were supported by the university and the MNI. This facilitated collaboration and access. Mishkin reflected, "This had never been possible before" (M. Mishkin, transcribed interview, 2001, p. 8).

Hebb's department established numerous ties with other McGill-affiliated institutes such as the Allan Memorial Institute and the Mental Hygiene Institute (both a few blocks away from each other). Psychiatrists and clinical psychologists from these institutions regularly supervised graduate students in Hebb's department. From outside the department Robert Malmo and Abraham Luchins supervised the most students. Malmo was a Yale graduate (1940) who had also worked at Yerkes Laboratory in the late 1930s and was appointed Director of the Laboratory

of Psychological Studies at the Allan Memorial Institute in 1948. He was known at the time for his electrophysiological research, contributions to experimental psychopathology (Bernhardt, 1958), and advocacy of psychosomatic approaches to medicine (Malmo, 1952). Luchins was a Gestalt psychologist who had been a student and close collaborator of Max Wertheimer's after the latter fled Germany to the United States in the late 1930s. MacLeod, who had studied psychology in the 1920s in Berlin, invited Luchins to serve as Lecturer in the late 1940s (Luchins, 1993). He conducted research on the role of mental set (*Einstellung effect*) and group psychotherapy. While Luchins was not formally appointed to the psychology department, he nevertheless provided an important perspective and later remarked how he was often called upon to 'defend the Gestalt position' in the department (Luchins, 1993).

Key to attracting high quality faculty and students was the opportunity to conduct innovative research. The connections that Hebb fostered with associated departments and institutions provided some opportunities for research but most importantly the department's own laboratories were in desperate need of improvement. The highest among Hebb's priorities was the creation of an experimental animal laboratory.

1. b. Establishing the animal laboratory

The psychological laboratory is often central to histories of psychology as a scientific discipline (Caphew, 1992), the importance of the laboratory underscores its capacity to provide insight about the cultures of modern psychology (Danziger, 1990). In Hebb's first year at McGill, he worked closely with MacLeod to establish a modern psychological laboratory, unlike any that had existed in Canada at the time (Wright & Myers, 1982). Hebb oversaw the creation of facilities for experimental animals (rats at first, then later dogs), similar to those he had attempted to establish at Queen's in the late 1930s (Murray, 2012; Inglis, 1982). The acquisition of laboratory animals, paired with the comparative psychological expertise of Hebb and Rosvold (the Yerkes-Lashley connections), kicked off a new era of experimental work at McGill and the MNI (Prkachin. 2018).

Both MacLeod and Hebb were convinced that the basis of academic psychology in so far as it was recognized as a well-established scientific discipline (see Chapter Four). If Canadian psychology was going to break its dependence on American funding and become capable of attracting the highest-calibre students, the department at McGill would need to become world-class, able to offer excellent opportunities to practice and develop practical research skills. The

attitude embraced by Penfield and the MNI throughout the 1940s supported the work of psychologists in clinical settings (see Chapter Three), though Penfield remained aloof to the world of academic psychology generally (Prkachin, 2018) Both Hebb's connections to the neurological community in Montreal and the development of the animal laboratories created more opportunities for his colleagues and students to conduct research with unique clinical and experimental data. Psychologists at McGill used these new data to conduct experiments in order to, in part, show the value of investment to the Canadian government, bolster the international standing of the university, and to train new graduate students in the principles of the biological psychology Hebb had outlined (1949) and MacLeod advocated (1947a).

The animal laboratories, originally housed in the Donner Building, opened in the fall of 1948 (Ferguson, 1982). Little is known about the specifics of the laboratories during this period outside the descriptions of research provided in graduate theses. Throughout the 1950s parts of the department were housed in about five different locations on or near the McGill campus.

While Ferguson (1982) suggests that distributed nature of the department detracted from a sense of departmental unity, it is possible that this extended base helped foster connections with adjacent departments (weak ties). Little is known about the specifics of the physical department during much of the 1950s. In 1964 all branches of the Department were brought together, and integrated as a unified department in the Stewart Biological Sciences Building.

According to Hebb's publication records and the topics of students he supervised in these first few years, Hebb engaged deliberately in building bridges between psychology and the Faculty of Medicine at McGill -- bridges in need of serious repair -- and refining his theoretical vision for the role of psychology within it. Hebb published position pieces in the *Annual Review of Psychology* (1950), the *Journal of Personality* (1951), and the *British Journal of Animal Behavior* (1953) that emphasized and extended the vision for a biological psychology informed by recent advances in neurophysiology. This biological orientation in McGill psychology was unique in Canada at the time. Early Canadian psychology departments simply did not have the kind of resources, expertise, or university support to properly leverage an experimental laboratory able to conduct the kind of research McGill became famous for during this period.

1. c. Lines of (neuro-psychological) attack: building up and building out

Hebb's background and biological approach to psychology led him to conclude that cognitive determinants of behavior such as adult intelligence and emotional responses were the

function of a complex interplay of both evolutionary heritage and early experiences. Hebb's theory (1949) proposed that thought processes (and therefore the operations of intelligence) consisted of an activity of cell assemblies, and that these assemblies are organized by way of the sensory stimulation from the early experiences of an animal. With the laboratory established, Hebb's department became "devoted to getting evidence that would show that the theoretical approach would pay off in new experimental results" (Hebb, Mar 14, 1951).

Hebb's main research objective in returning to McGill in the late 1940s was to better understand how this interplay contributed to the organization of behavior in animals. Building upon his work on perception and instincts, Hebb's research throughout the 1950s dealt primarily with topics related to motivation and learning. He wanted to figure out how motivation and learning affected the behavior of animals and in turn how early experiences affected the capacities of animals to learn and adapt. The research Hebb and his colleagues conducted at McGill in the 1950s contributed to a fundamental rework of our understanding of the relationship between organisms and their environments, especially the importance of early environments for adult development. Neobehaviorists of the postwar decade turned increasingly to theories of information processing to explain human behavior related to learning, memory, and perception. In doing so they abstracted the cognitive apparatus from its embodied (neuropsychological) experience. Hebb (1949) had demonstrated, through his physiological approach to psychology, that connections between higher-order cognitive functions (such as pleasure, pain, and emotion) could themselves be described in terms of neurophysiological processes without reducing one to the other. This focus made Hebb unique among many of his contemporaries (Gardner, 1985; Baar, 1986). In the early 1960s, Ulric Neisser (1928-2012), the "father of cognitive psychology" (Hyman, 2012; Neisser, 1967), lamented a "misunderstanding of the nature of human thought" in current debates (Neisser, 1963). This, he argued, was reflected by the neglect of a number of uniquely human processes, namely the growth and development of thinking, the intimate association between thoughts and emotions, and the multiplicity of motives that exist simultaneously in any given situation. He claimed that, "the focus of difficulty will no longer be pattern recognition, learning, and memory, but in an area which has no better name than 'motivation'" (Neiser, 1963, p. 195). While behaviorists and early cognitivists had tended to avoid much consideration of higher-order processes, Hebb and his department at McGill sought an explicit integration of human experience through existing neurophysiological theory. It was

an interest in ‘motivation’ broadly that drove his research agenda throughout the postwar period (e.g., Hebb, 1955). In the remainder of this chapter I will describe how this integrationist perspective contributed to multiple lines of original, groundbreaking research which has shaped psychology at McGill and in Canada ever since.

Hebb's theories, as outlined in the *Organization of Behavior* (1949), led him to believe that intellectual and problem-solving ability (as measured primarily by IQ tests in humans) was much more a product of early experience than had been recognized. Hebb's research agenda throughout the 1950s can be described as an investigation of this basic tenet. Upon arrival at McGill, Hebb established two broad lines of research, “one opening up new ground, the other cleaning up and systematizing the areas opened up earlier” (Hebb, Septt 22, 1952). Hebb's initial research funding came from the Canadian Defense Research Board (DRB), and later the Rockefeller Foundation (Hebb, Septt 22, 1952) and Department of Health and Welfare. Hebb was a member of the DRB's Psychological Research Panel, which was overseen by N. W. Morton (Turner, 2012), a graduate of McGill and member of the department before the war. Compared to the United States, investment by the Canadian military in psychological research was proportionally smaller (Turner, 2012; Capshew, 1999); Hebb capitalized on these sources of research funding.

The research of the department of psychology at McGill during the 1950s can be roughly divided into several phases: the first are the early years (1947-1951) when Hebb was establishing the department, testing some initial theories (with rats), then the early 1950s (1951-1955) when DRB-funded research into deprivation into both man and dogs is developed alongside studies on addiction and motivation (Zubek, Beach, P. Milner) and temporal lobe function (B. Milner), and late 1950s (1956-1958) in which Hebb developed his introductory textbook that integrated much of psychology using the framework provided by his 1949 theory, applying his network theory to heritability, learning and memory, motivation, perception, thought, and development (1958). He later extended his textbook to emotions in their social contexts and individual differences in intelligence, and abnormal psychology.

The “cleaning and systematizing” work, done by Hebb and his students, involved primarily the refinement of animal intelligence testing techniques (e.g., Rabinovitch, 1949, PhD; Heron, 1949, MA) and standardizing emotional behavior among their experimental animals (e.g., for rats, see Thompson, 1953; for dogs, see Mahut, 1955). By the early 1950s, the activities at

McGill covered a wide range of topics and reflected a mature department, “healthy and full of life” (Hebb, Septt 22, 1952), and on the cutting edge of biological and behavioral research (e.g., CPA, 1955). Hebb described it to his funders at the Rockefeller Foundation: “Our research ranges from microelectrode work with single cells, through studies of specific nuclear structures in the brain, to human perception and the thought processes underlying language. […] We are still fundamentally concerned with the nature of human thought and emotion and with the nature of complex learning processes” (Hebb, 1958; #6194-6195). The fundamental concern with the nature of emotion is interesting for this particular period and will be explored more later.

In the early 1950s much of the work focused on an “extensive study of human motivation done under contract with the Defence Research Board” as well as the research Zubek and Beach were conducting on “the mechanism of addiction” (Hebb, Septt 22, 1952). The department tackled a number of interrelated issues and Hebb understood his research program in the early 1950s to have been effective in large part due to a “combined attack at different levels of abstraction” (Hebb, Jan 20, 1958).

Hebb's students explicitly sought to extend his earlier findings (i.e., Hebb, 1947) by using a larger number of experimental animals under more carefully controlled conditions in order to further analyze and investigate the factors underlying the effects observed regarding the relationship of early learning to maturational processes (e.g., Hymovitch, 1949, PhD). A number of Hebb's students, such as Forgays and Lansdell, completed their doctoral research on the effects of brain damage on higher cognitive functions in rats. For example, Ruth Hoyt-Cameron —who was among Hebb's initial students —first examined the effects of electroconvulsive shock in rats (Hoyt & Rosvold, 1951) before completing her Ph.D. on the effects of lobotomy on adult intelligence (Hoyt, 1952).

2. Experiments on Sensory Restriction, 1949-1954

Hebb's research interests, from his time as a McGill graduate to his experiences with Lashley and Penfield, revolved around an investigation of the effect of developmental experience (e.g., through the ablation of specific parts of the brain or through sensory restriction) to later functioning of the adult brain, particularly in relation to factors presumed to have been inherited such as intelligence (e.g., maze solving ability) and instincts (e.g., emotional responses such as fear). Hebb had established himself an expert in the comparative measurement of both human

and animal intelligence (i.e., cognitive abilities before and after some intervention or procedure). Hebb developed both theoretical approaches in which experimental animals raised in radically different environments could be compared with normal animals and the ways in which the results of these comparisons could be explained using the latest understandings in neurophysiology.

The role of early environment on the later development of cognitive abilities had been the main focus of the mental hygiene movement of the 1920s and 1930s. According to Hebb (1949), their failure was due primarily to a fundamental lack of scientific rigor in understanding the causes of mental disturbance (i.e., premature application of tentative findings). After the war there was renewed interest in the links between environments and mental states, though notably absent the imperative towards community health. This renewed interest was due partly to opportunities for systematic psychological investigation created as a byproduct of processes related to the mobilization for war (e.g., mental testing; Capshew, 1999). Similar to the previous World War, the psychological data generated by the military between 1939-1945 was unprecedented. After the war, the interest in treating its psychological casualties and to leverage the psychological insight that had been derived from these new data drove government and military investment in new scientific (i.e., neurobiological) understandings of the human brain (Capshew, 1999). Hebb was in a position at McGill, having identified the need to address the shortcomings of existing psychological models, to take advantage of this shift in attitudes towards increasingly biological approaches to mind (Pressman, 1998).

Interested in demonstrating the influence of the environment on cognitive ability Hebb recruited the help of his two daughters in rearing rats at their home in Florida in 1947 (Hebb, 1980). These "enriched" rats (i.e., pet rats) were compared on a series of learning tasks to rats that had been raised exclusively in cages. As part of the same study Hebb compared rats blinded at birth with those blinded at maturity. Both the laboratory group and the pets were tested with a rat intelligence test that had been developed with Kenneth Williams at Queen's (Hebb & Williams, 1946) and later revised by Rabinovitch and Rosvold (1952). Later that year, at the meeting of the American Psychological Association at Cornell University, Hebb presented these findings, which he said showed "a lasting effect of infant experience on the problem-solving of the adult rat" (Hebb, 1947). At this same meeting psychologist Austin H. Riesen (1913-1996), with whom Hebb was then working at Yerkes Laboratory, presented findings of chimpanzees that had been raised in darkness. Riesen would later go on to become a leading researcher in

deprivation experiments in animals (Rumbaugh, 1998). These experiments, the outcome of a critical period of Hebb's intellectual development during which he was writing the bulk of his famous manuscript (Hebb, 1949), would shape his research agenda for the next decade.

Hebb encouraged one of his first graduate students to pursue more formal investigations and in similar experiments Hymovitch showed that the environmental effect occurs only during the period of growth (i.e., childhood) and that a favourable environment thereafter cannot reverse the damage that has been produced by an unfavourable environment earlier (Hymovitch, 1952). Similar to the results Tolman (1948) had demonstrated with "cognitive maps," Hymovitch had also demonstrated the surprising extent to which visual-perceptual learning was possible with the rat (Hymovitch, 1952; Forgays & Forgays, 1952).

Indeed, there was rising interest within comparative psychology circles (e.g., Karl Lashley's group at Orange Park and Harry Harlow's Primate Laboratory at University of Wisconsin–Madison) that restricted early environments would produce behavior that was difficult to explain with existing behaviorist models. Members of these circles, such as Hebb and Riesen, became fascinated by reports of the strange and awkward experiences of individuals following the removal of congenital cataracts (e.g., von Senden, 1932). These individuals commonly reported great difficulty recognizing and interacting with forms such as circles, squares, and triangles when removal of the cataracts enabled them to see for the first time. These reports motivated Riesen's sensory deprivation experiments in animals (Warren, 1996) and Hebb's theory applied to perception (1949), which relies heavily on Riesen's chimpanzee study (1947) and von Senden's (1932) account of restored vision, despite methodological limitations (Wertheimer, 1951).

Studies in which the early environment was manipulated in a particular way to examine the effects on later cognitive development were not new. But then, why did sensory deprivation studies not emerge prior to World War II? It seems that while Hebb and his colleagues in Lashley's laboratories were interested in various ablative techniques for localizing various brain functions, it was not until the late 1940s that a systematic investigation of the psychological effects of these kinds of experiments were seriously explored. This highlights the novelty and influence of what Hebb was to soon propose, he had established a framework for understanding behavioral changes strictly within the context of biological models that cohered to existing neurobiological understandings. Prior to the publication of *The Organization of Behavior* there

was no satisfying or scientifically coherent way (and therefore no reason) to explain the effects in animals and no rationale to study human subjects. That was soon to change.

2. a. Contract X-38: Hebb and the Sensory Deprivation Experiments, 1951-1955

Within the context of the Cold War, the study of psychological methods that could be used to modify human behavior resonated both with intelligence officials and the Canadian public. Communist trials, prisoners' false confessions, and the fear of secretly turning citizens against their own country had established concerns regarding "mind control" and the capacity to "brainwash" American soldiers in the late 1940s (see for example, Weiner, 2008; Raz, 2013b; Jacobson & Gonzalez, 2006; Otterman, 2007; Rejali, 2007). Physicians and researchers were called to weigh in on this debate. In June 1951, military representatives from the U. K. and the U. S. met with members of the Defense Research Board (Omond M. Solandt and N. W. Morton) at the Montreal Ritz-Carlton Hotel to discuss the brainwashing techniques of Communists during the Korean War (1950-1953). Closely tied to academia, the DRB regularly funded research programs associated with Canadian universities and relied on the consultancy of university professors (Turner, 2012). Hebb and two Montreal psychiatrists (James Tyhurst and T. E. Dancey from the Allan Memorial Institute) were invited to participate in the meeting (Cooper, 1986).

At this meeting Hebb suggested that they might learn about how captured soldiers came to expound Communist propaganda by studying the effects of perceptual isolation (Hebb, 1961). This proposition was inextricably linked to contemporary debates on the psychological methods of behavioral modification and control (McCoy, 2007; Raz, 2013b). Indeed, Hebb later recalled that the work at McGill itself began "with the problem of brainwashing" and that the "chief impetus [...] was the dismay at the kind of 'confessions' being produced at the Russian Communist trials" (Hebb, 1961, p. 6-7).

In September 1951 Hebb was awarded "contract X-38" by the Defense Research Board to study the effects of restricted environmental experience. The human subject studies were conducted at McGill by Woodburn Heron, W. H. Bexton, T. H. Scott, and Benjamin K. Doane between 1951 and 1955 (Brown, 2007). These studies examined 1) tolerance of subjects for perceptual isolation, 2) willingness to listen to distasteful dull materials, 3) change of attitude, 4) impairment of intellectual function, 5) hallucinations and other effects of sensory deprivation (Cooper, 1986).

Hebb's team offered male graduate students $20 a day to remain isolated in small chambers with little more than a bed. In addition to goggles completely obscuring their vision, they wore gloves and cardboard tubes over their arms to limit their sense of touch. A U-shaped pillow and the hum of an air conditioner masked outside noises. Subjects wore long cardboard cuffs from the middle of the forearm to beyond the fingers. Thus, they received sensory stimulation, but all of it "unpatterned." Hebb had hoped to observe his subjects for up to six weeks but most elected to discontinue the experiment after a few days, and none more than a week.

The results of these experiments astounded Hebb and his research team. After the first day the subject's ability to solve problems "in his head" became increasingly more difficult. By the second day they found it nearly impossible to organize thoughts, to plan ahead or review mentally, and there were complaints that subjects could no longer daydream. Beginning about the third day there were increasingly complex visual and somasthetic hallucinations, with disturbances in visual perception coming after the experiment had ended.

Subjects suffered from hallucinations, delusions, disorientation, 'out of body' experiences, and demonstrated diminished cognitive abilities. As part of the research protocol, subjects were asked about their attitudes towards controversial topics both before and after periods of extended sensory deprivation. During isolation recordings of contrary arguments were played to subjects (i.e., that scientists who don't believe in ghosts or extrasensory perception are biased). The surprising results of these experiments motivated Hebb to write to his funders exclaiming that, after only a few years since taking over the department, he was confident that "we have reached the point [in 1951] at which we can show that the large expenditure will be justified by results" (Hebb, Mar 14, 1951). Hebb and his colleagues had a theoretical explanation for these unexpected results. At the Canadian DRB symposium in early 1952, Hebb and his team presented preliminary findings that repeat testing had shown sensory deprivation was capable of rendering individuals susceptible to attitude change (Hebb, Heron, & Bexton, 1952). The DRB classified the research as 'SECRET" and denied Hebb's requests to present the results at future psychological meetings (Brown, 2007; McCoy, 2007). Interest in exploring the possibility that attitudes might be changed through a technique that involved sensory deprivation (such as prolonged solitary confinement; see Rassmussen, 1973), as well as the involvement of the defense department in these studies, remained undisclosed to the public throughout the 1950s.

The importance of Hebb's research was not lost on his funders. Despite being classified, and unknown to Hebb, the DRB sent copies of his reports to the British and American military and the CIA (Cooper, 1986). D. Ewen Cameron (1901-1967), who was funded by the Society for the Investigation of Human Ecology (a CIA front) from 1952 to 1959, used Hebb's perceptual isolation procedure as part of his "psychic driving" method (Cameron, 1956; Cameron & Malmo, 1958) and his colleague Hassan Azima (1922-1962) used it in "anaclitic therapy" (Raz, 2010), both of whom worked at the Allan Memorial Institute only a few blocks from where the original studies had been conducted. While Cameron and others used Hebb's ideas in his work, Hebb was "contemptuous of Dr. Cameron's work in the field of sensory deprivation (as well as his work in psychic driving)" (Cooper, 1986, p. 37) and Cameron had already isolated the department of psychiatry from neurology and psychology at McGill (Prkachin, 2018).

Hebb recognized that these classified experiments held more significance to psychological research than military concerns provided. Hebb's department was not exclusively funded by the DRB (see Chapter Four). In 1953, Hebb described to his funders (in the vaguest terms) that in the past six months his department had made "two discoveries of some importance" that had put them in the position "to make a permanent change in the direction taken by psychological theory—with any luck, a major change" (Hebb, Feb 23, 1953). The discoveries to which he referred were likely the discovery of the "pleasure centre" in the brain by James Olds and Peter Milner (discussed later) and his own military-funded sensory deprivation studies, both of which had the potential of opening realms of psychological investigation previously unavailable. Hebb argued with the DRB that all of his research should be made public, but they refused this request (Cooper, 1986). Only after much negotiation was Hebb allowed to present his research as it applied to human motivation and boredom (e.g., Heron et al., 1953); he was forbidden to discuss any aspect of his studies on attitude change (McCoy, 2007; Brown, 2007).

Only in 1954, after descriptions of these studies had leaked to the popular press (Scott, 1954) and his funding with the DRB terminated, was Hebb given permission to report his results to the scientific community in any detail. Hebb and his team conducted this research between 1951 and 1955, when Hebb submitted his final report to the DRB (Hebb & Heron, 1955). Hebb's students published four papers on their research (Bexton, et al., 1954; Heron et al., 1956; Scott et al., 1959; and Doane et al., 1959), as well as a *Scientific American* paper (Heron, 1957). The measures of attitude change were excluded from these articles. The 1954 article became the first

of a series of publications in the nascent field of sensory deprivation which developed in laboratories across North America such that within seven years of the initial publication over 230 articles had appeared in leading scientific journals, most of them citing Hebb's work (Raz, 2013). Students and colleagues who had participated in these early studies, such as Maitlin Baldwin and John Zubek, went on to assume leading positions in sensory deprivation research throughout the 1960s and '70s. In 1958, Harvard University held a symposium on sensory deprivation, funded indirectly by the intelligence community (McCoy, 2006).

2. b. Physiological functioning and the 'total action of the nervous system'

Physiological psychologists in the 1940s had understood that sensory input during critical periods of development had an important role for normal cognitive-perceptual development in adulthood. Herbert Jasper (1906-1999), through his pioneering work in electrophysiology at the MNI, had shown that even during unconsciousness the brain is never truly at rest (Jasper, 1937; Jasper, 1941). These insights provoked a number of young psychologists (such as Hebb) to question the dominant understanding of behavior as sense-dominated. The thinking among experimental psychologists was that variables such as sensory input could be manipulated (removed or reduced) from the equation to more directly observe the activities of other components of the system (i.e., "central processing"). These activities or influences would be observed in behavior, the outcome of disruption to normal cognitive abilities. This pathological method was a central tenet of experimental physiology (Bernard, 1865) and espoused by Hebb's more clinically-trained mentors (Weidman, 1999; Gavrus, 2011).

At first glance it may appear Hebb's work with the Canadian military was peripheral to his broader institutional and academic concerns regarding the establishment of a biological psychology at McGill. I argue this was not the case and that by examining the origins of this research we see how sensory restriction represented a specific set of priorities that privileged a particular approach to address enduring scientific concerns regarding intelligence, perception, and motivation. The so-called "New Look" approach to perception and learning, which emphasized an organism's own selectivity and interpretation in response to a given stimulus (pioneered by Leo Postman and Jerome Bruner and influenced by Tolman and Krech; see Nairne & Coverdale, 2017, p. 260-62; Krech, 1949; Luchins, 1951), had shaped Hebb's considerations of the problems of organized cognition (e.g., Hebb, 1958a). Fundamental and specific disruption to normal functions (such as through ablation or restricted environments) helped reveal the

significance of top-down influences on perception and critical shortcomings of the behaviorist approach. Hebb's achievement was in bringing together critical insight from the Gestalt and "New Look" approaches to perception and learning (e.g., Krech, 1949; Luchins, 1951) with empirical behavioral methods, infused with the sensibilities of the physiological laboratory, all while paying homage to, and effectively side-stepping 1940s behaviorist dogma.

Hebb and his students wanted to show how the brain was related to, and dependent on, continual external sensory stimulation for its organization and normal functioning. Indeed, Hebb himself described contract X-38 (on 'human motivation') as that "which contributes directly to the theoretical aims of our program [at McGill]" (Hebb, Sept 22, 1952). It was widely believed that "the brain would deteriorate if it didn't have a continuous stream of sensory input" (P. Milner, as cited in Mechanic, 2008). Therefore, if psychologists could observe a disruption to the organization of the mind, they might isolate the "mediational processes" (see Ghassemzadeh et al., 2013) contingent on Hebb's proposed notion of cell assemblies (Hebb, 1949). It was an attempt to account for inexplicable emotional responses upon which theories of physiological disruption were based (Dror, 1999; 2001). Hebb drew upon the work of his colleagues in neurophysiology. For example, in his oft-cited "On the nature of fear" (Hebb, 1946c), Hebb had concluded: "By distinguishing between processes which break down and those which restore physiological organization in the cerebrum, the variability of fear behavior is accounted for [by the disruption of cell assemblies (Hebb, 1949)]" (p. 275). This explanation extended not only to conflict arising between expectancy (including desire) and what one perceives, but also to the absence of accustomed sensory input, which accounted for everything from fear of the dark by children to the depression of loneliness or homesickness in adults (Hebb, 1980).

Since funding for Canadian experimental psychology had emerged from its ties to defense research, Hebb had been primarily focused on demonstrating the value of these expenditures to the military. However, the research activities of his department in these early years was intimately tied to Hebb's broader project of establishing an experimental psychology in Canada (Hebb, 1948a; Hebb, 1951). Most academic psychologists (following neobehaviorists such as Skinner and Hull) had largely abandoned the neurological approach in psychology (see Chapter Three) and had moved towards greater abstraction. In addition to growing concerns regarding academic competition (Riesen, Harlow, and Lashley were among those asking similar questions about early environment on later cognitive capacities), Hebb needed to demonstrate the

neuropsychological approach he had outlined (Hebb, 1949) could provide meaningful insight comparable to that which was taking place at the time (1950s) in clinical neurology and the pharmaceutical sciences.

In addition to the much-needed funding that this research provided to the department, the sensory isolation studies also reinforced newly established relationships between psychology and medicine beyond the university, which Hebb continued to nurture throughout his career. These relationships were particularly important for the opportunities they provided young graduate students in the department. Psychiatrists such as Tyhurst, Dancey, and Malmo conducted research with graduate psychology students through research partnerships with Montreal's veteran hospitals (e.g., Queen Mary and St. Anne's), the Allan Memorial Institute, and the Montreal Neurological Institute, among others (despite ongoing tensions between these institutions; Prkachin, 2018).

In this context, Hebb's choice to pursue studies of healthy adults in restricted environments (urged greatly by military interests) likely presented a means to show the efficacy of his line of research (i.e., the intervening processes at the heart of his critique of S-R psychology). When these studies began to yield unexpected results, the emergence of a new field of study was almost too appealing for psychologists. Hebb believed this research, given its relevance, would soon be declassified (Hebb, Sept 22, 1952). In less than a decade Hebb's experiments on sensory deprivation, originally influenced by Cold War concerns, came to provide insight into a range of phenomena ostensibly associated with "stimulus deprivation" from unexplained medical conditions to the very tenets of Western culture (Solomon et al., 1961). For example, renowned psychoanalyst Karl Menninger (1958) suggested the analytic session itself involved sensory deprivation and as an etiological model to explain why children from low-income homes often struggled in academic settings (Raz, 2013a). In doing so, sensory deprivation gained a privileged role as a cutting edge scientific theory and explanatory model throughout the late 1950s and 1960s (Raz, 2013b). Indeed, by 1969 there were 17 experimental 'centres' in North America dedicated to sensory deprivation research, each employing their own elaborate modifications in design and technique (Williams et al., 2018). While Hebb seems to have initially pursued this line of research in order to provide strong evidence for the important role of central processes in organized behavior, what he ended up doing was supporting a program of research into psychological manipulation and torture (McCoy, 2007).

With the termination of military funding in 1955 and the public backlash that ensued (Hebb's studies were often associated, incorrectly, with those of Cameron's), he moved on to other work: teaching, supervising, and working on his textbook (1958). The sensory isolation studies represent an important historical case because they demonstrate one of the ways in which Hebb's theory (1949) was initially put to the test in his own department. These experiments had demonstrated to Hebb that his theory (of cell assemblies) was "of the right kind but has far to go" (Hebb, 1980, p. 97). Experiments on restricted environments had dramatically revealed how the mind's embeddedness in its environment went much further than had previously been understood.

While Hebb was unable to report on the results of these studies, he was not prohibited from sharing the insights they provided about the inner workings of the mind. Hebb worked out a notion he referred to as the "conceptual nervous system" in discussion with his students at McGill, especially Dalbir Bindra and Peter Milner (as well as with Leo Postman during his visit to Berkeley in early 1950s) (Hebb, 1955). At the annual meeting of the APA in New York in September 1954 he shared some of the main insights he had derived from these experiments in the context of what they implied about drive and motivation theory. This paper (1955) is cited second only to his first book (1949) (according to Web of Science v.5.32) and represents a synthesis of some of the main research themes of the early 1950s. This article also counter-indicates the suggestion that Hebb's applied military-funded research represented the primary activity of the department during this time, as has been suggested (e.g., McCoy, 2007). While the sensory deprivation studies are perhaps the most notorious and memorable from this period, they do not represent the culmination of department research activity; this view neglects broader themes, concerns, and connections among a range of scientific endeavours during this time. My thinking is more aligned with scholars who have incorporated the role of physiological theory in psychological thought (e.g., Bruce, 1996; Weidman, 1994) and who trace Hebb's roots to experimental medicine and American Functionalism. A broader focus on some of the theoretical and disciplinary factors involved suggests deeper themes at work during this important period of Canadian psychological history.

The initial early-environment program (e.g., Hymovitch, 1952; Thompson and Heron, 1954; Melzack and Scott, 1957) was an important part of what shaped postwar psychology in Canada. Important to this program was the role of experience, specifically, the subject's own

understanding of context. In this regard Hebb was influenced by holistic, top-down approaches such as Gestalt and "New Look." Radical isolation, functioning like sensory ablation, was thought to reveal the workings of the mind through its systematic and prolonged disruption of "central processes" (see Chapter Three). The concept of disruption as a means to better understand the underlying structure of a complex system, Hebb borrowed from experimental medicine, among other places (e.g., Bernard, 1895). This laboratory approach to psychology (i.e., physiological psychology), which models itself as an extension of biological science, shaped the research agenda at McGill and its fundamental theory and method contributed to a top-down neurophysiological approach.

3. The Discovery of the "Reward Centre" in the Brain, 1953-1954

In the early 1950s Hebb described to his funders at the Rockefeller Foundation two areas positioned to make permanent change in the direction of psychological theory (Hebb, Feb 23, 1953). The first, the discoveries related to sensory restriction, the second, an unusual observation one of his graduate students had made while working together with his postdoctoral student on the neural basis of attention in rats These young researchers were Peter Milner (1919-2018) and James Olds (1922-1976), respectively. What they had observed was what appeared to be a specific part of the brain that, when stimulated properly, would directly affect the behavior of their laboratory rats. This chapter examines how Olds, a Harvard-trained organizational psychologist with no laboratory experience, and Milner, a British electrical engineer, both come to Montreal in the early 1950s to study the brain with Hebb.

Considered among the most important psychologists of the twentieth century, Olds and Milner are recognized today for having discovered and done much of the initial mapping of the "reward" centre in the brain (Thompson, 1999). The concept of motivation was among the core interests to behavioral psychologists throughout the twentieth century (Cofer, 1981): As Milner would later note,"What goes on in the chicken's brain before it crosses the road is a fundamental neuropsychological question" (Milner, 2011, p. 318). The discovery in 1954 that there might exist a specific system in the brain dedicated to the processing of motivational reward was monumental.

Olds and Milner (from Harvard and Cambridge, respectively) had both read and were deeply influenced by the ideas presented in *The Organization of Behavior* (1949); both cite

Hebb's book as the main reason they decided to continue their studies at McGill (Milner, 2011). To Milner, physiological psychology was a piecemeal attempt to discover the function of the brain by studying the effects of different brain lesions in animals and human patients; but Hebb's approach, which used the properties of neural elements in an attempt to explain function, struck him as fundamentally different (2011, p. 315).

Peter Milner had originally come to Canada from England with his wife, Brenda Milner (née Langford), in 1944 as a physicist at the Chalk River Nuclear Laboratories outside Ottawa. Brenda Milner had completed her undergraduate education in psychology at Newnham College in Cambridge under Oliver Zangwill (1913-1987) and spent much of the war at the Cambridge Psychological Laboratory working on aircrew selection techniques with Frederic Bartlett (1886-1969). Significantly, it was through Zangwill that she developed interest in organic brain damage (Collins, 2006).

When she came to Montreal with her husband, Brenda Milner decided to pursue an M.A. in experimental psychology at the University of Montreal (McGill's department was "in hibernation"). She was hired to give a series of lectures on Bartlett's theories of memory, which eventually secured her a full-time position. Brenda Milner was invited to attend a series of evening seminars led by Hebb, who was seeking feedback on his manuscript for *The Organization of Behavior.* There were about a dozen students in the evening seminars, including Mortimer Mishkin, Herbert Lansdell, Sam Rabinovitch, and Lila Ghent (Mishkin, 2001). Brenda Milner remembered how "discussion after the seminars often continued late into the night [...] It was an exciting time and hastened my decision to do a Ph.D. at McGill" (Milner, 1998, p. 282).

With the end of the war, Peter Milner's work was less demanding and Brenda shared with him what she was learning in psychology. He became especially interested in speculations about the physical neural circuitry of the human brain (Milner, 2011) and met with Hebb in the spring of 1947 to discuss enrolment into the graduate program. He was required to take a qualifying year; during which he learned from Herbert Lansdell (1922-2000), one of Hebb's graduate students (Mirsky, 2001), how to operate on a rat's brain (Milner, 2011). Brenda completed her M.A. in experimental psychology at the University of Montreal in 1949 and subsequently became a Ph.D. candidate under Hebb at McGill. In 1950, Hebb brokered an opportunity for Brenda to work with Penfield at the MNI; she completed her Ph.D. on lateralization of temporal lobe function in 1952 (for more on Brenda Milner and her career as a pioneer of modern

neuropsychology, her studies of memory, and the most famous patient in cognitive neuroscience, Henry Molaison, formerly known as patient H. M.; see Corkin, 2013; Squire, 2009; Dittrich, 2017).

Peter Milner also attended Hebb's evening seminars and joined the department in the fall of 1948 as one of Hebb's early graduate students, along with Bernard Hymovitch, Woodburn Heron, Thelma Gordon, and Donald Forgays. Given his interest in neurophysiology, Hebb suggested that Milner begin by establishing ties with the MNI. Milner's background in electrical engineering suggested to Hebb that he follow up on Penfield's work on direct electrical stimulation of the brain. Following up with opportunities for collaboration with the MNI, Hebb had recently decided to include dogs as well as rats in a new set of experiments and facilitated Milner's involvement with neurosurgical teams to learn how to implant cortical electrodes in dogs. This demonstration of the importance of the links between the Department of Psychology at McGill and the MNI, which drew upon the legitimacy of psychologists Hebb and Harrower had established in the late 1930s, put Hebb's students in the same teams, classrooms, and surgical rooms as the fellows of the MNI. Indeed, Hebb "knew many of the staff and sent several of his graduate students to participate in their seminars" (Milner, 2011, p. 310) and encouraged them to "take their courses in neuroanatomy and neurophysiology" (Melzack, 1993, p. 9). Among those who worked closely with graduate students in the Psychology Department were Francis McNaughton (1934-1983) and Herbert Jasper (Milner, 2011; Melzack, 1993).

When Peter Milner was attending lectures from Jasper at the MNI, Jasper had only recently discovered that electrical stimulation in part of the brainstem of anesthetized cats produces cortical activity resembling that of an alert cat. The following year Moruzzi and Magound (1949) had localized this effect to the brainstem reticular system and throughout the late 1940s the relation of this system to sleep, waking, and arousal was a subject of much investigation (e.g., "we heard a great deal about it during the seminars," P. Milner, 2011, p. 310).

In human patients, Penfield was able to affect normal speech processes through electrical stimulation of the cortical speech areas in the brain. With assistance from John Zubek, Milner set out to look for similar stimulation-produced interference (or *disruption*) of learned behavior in dogs. He gave up due to technical difficulties shortly thereafter. Dogs, as it turned out, are less tolerant of being wired-up than rats and the electrodes frequently fell out or got infected (Hebb, Sept 22, 1952). With little time left to complete his Master's degree, Milner explored his options:

he needed an experiment that involved little or no surgery and could be investigated with animals other than dogs (Milner, 2011). This is how Milner came to the problems of motivation in rats.

Milner decided to test the hypothesis, common at the time, that animals must learn what food is good for them. Using rats deprived of potassium, Milner was able to show that it was improbable food preference depended solely on learning and established long-lasting preference for the potassium salt solutions in rats, independent of deprivation (Milner, 1950; Milner, 2011). Intrigued by the implications for motivation and drive (see Marks, 2011) and the recent discovery of the reticular activation system in states of consciousness (Moruzzi & Magoun, 1949), Milner teamed up with Seth Sharpless to explore whether stimulation of the reticular system with implanted electrodes would influence behavior in a maze. They observed in the rat avoidance behavior that mirrored the reaction an unpleasant electrical shock would elicit, but nothing more. Considering it a failure, Milner returned to his dissertation work which he had begun on the role of attention in subjective experience of time, which resonated with the then influential "New Look" approach to perception (Nairne & Coverdale, 2017).

3. a. James Olds and the Mystery of Reward

While working on his dissertation research in the summer of 1953, Milner was introduced to James Olds, one of Hebb's new postdoctoral researchers. Olds had spent his early twenties serving as part of the Persian Gulf Command in Tehran and Cairo during World War II. He decided to attend Harvard to study psychology the same year *The Organization of Behavior* was published and studied under Richard Soloman (1918-1995). Soloman had only recently joined the Faculty at Harvard, having worked on defense control systems for the Office of Scientific Research and Development (LoLordo, 1996) and best known for his later studies on avoidance learning. Olds received his Master's degree in 1951 and his Ph.D. in 1952, both in experimental psychology at Harvard. He remained a year as a lecturer and research associate in the Department of Social Relations where he worked with sociologist Talcott Parsons (1902-1979; Thompson, 1999; see Isaac, 2012).

The attempt to specify the mechanism by which rewards *reward*—by satisfying needs, reducing drives, or reducing the stimuli associated with drives—expanded in scope in the 1950s with the research on curiosity, exploration, and manipulation, much of it conducted by behaviorists who could not, or would not, look inside organisms (Marks, 2011). Olds was deeply interested in questions of motivation and learning. Like Hebb, Olds was convinced fundamental

psychological theory had to be derived from the physiological functioning of the central nervous system. He applied for and received a U. S. Public Health Service fellowship to do postdoctoral research at McGill with Hebb from 1953-1955.

Shortly after the publication of *The Organization of Behavior* (1949) Olds had taken up Hebb's discussion of the cell assembly to address problems related to latent learning, proposing a "functional anatomy of motives" which he outlined using a neural model of Tolman's learning theory (Olds, 1954). Hebb's book dealt primarily with his theory as it related to perceptual studies, and both Milner and Olds (as well as much of the department) shared a wish to extend and revise Hebb's neuropsychological theory towards a better understanding of motivation and behavior and "break the barrier of levels between psychology and electrical neuro-physiology" (Olds, 1956).

Because Olds had no experience with experimental animals or the brain, Hebb tasked Milner with bringing him up to speed, which included teaching him the electrode procedure Milner had been adapting from Spanish neurophysiologist José Delgado's experiments with monkeys at Yale (Milner, 2011). Milner had taken over instruction in surgical and histological methods by 1953 (Hebb, 1980): "[Peter Milner's] contribution to Jim's [Olds] training was invaluable in terms of showing him the techniques of implantation, stimulation, and recording and, in general, contributing his knowledge of physiological techniques to the training of a postdoc more schooled in the theoretical than the experimental aspects of that field" (Thompson, 1999, p. 250). Delgado had begun to demonstrate that direct stimulation of the brain could produce emotional conditioning and motivate learning of instrumental responses (e.g., Delgado, Roberts, and Miller, 1954) and Olds had begun his studies at McGill hoping to develop this theory with Hebb's help.

Encouraged by Milner and Sharpless in the department, as well as ongoing research using electroencephalography at the MNI, Olds decided to implant the electrode within the area of the reticular activating system. His aim was to produce conditional firing of the arousal system, which Olds and Milner theorized would facilitate learning in a T-maze paradigm (Milner, 1989). After recovery, they tested the rat in the maze and observed how it sniffed around the region where the electrode stimulation had occurred and would return to the location if given the opportunity. Olds claimed the rat was being "rewarded" but Sharpless and Milner responded with skepticism. Only after Olds had demonstrated the reinforcing behavior with a Skinner box

(which Milner had to build because their department "may have been the only one in North America at the time that did not possess a Skinner box"), they were finally convinced (Milner, 2011, p. 312). However, complications arose when they were unable to replicate the effect with other rats.

This might have been the end of the story. Luckily, the Donner Building, where the psychology department was located, also housed the Department of Experimental Surgery, which had an X-ray machine. Access to this machine allowed them to examine where the electrode had been placed -- or misplaced, rather, as it was located in the vicinity of the septal area instead (Milner, 1989). Olds and Milner followed up this discovery and designed a set of experiments to explore the possibility that intracranial electrical stimulation would act as a reward mechanism (Olds & Milner, 1954); they went about gathering data that would allow them to identify the area of the brain being stimulated by the electrodes (in or near the nucleus accumbens or the medial forebrain bundle) and to improve upon the design of the electrodes to make them easier to implant using techniques developed by Swiss physiologist Walter Rudolf Hess (e.g., Hess et al., 1953).

Olds and Milner postulated that this stimulation excited a system in the brain, the function of which was to reward behavior. They believed this technique could be used to explore the physiological basis of the reward process itself, directly. Hebb reported this discovery to his funders in 1953: "Olds and Milner have achieved a most dramatic and theoretically significant result in the past three weeks. Somewhere close to the midline, in the dorsal and anterior hypothalamus, stimulation produces what is apparently a strongly pleasant affect. At any rate, the rat will do anything that will get him more of the stimulation, and this seems more effective than food or avoidance of pain as a means of getting the rat to learn something. The whole business is incredible to me, and there may be some joker somewhere, but Olds and Milner have repeated it in four animals and there seems no doubt about it" (Hebb, Dec 16, 1953). Not only will animals work for food when they're hungry or for water when they're thirsty, but, even when sated, rats will work for electrical stimulation of their brains. This discovery directly challenged the dominant "drive-reduction" theory central to understanding motivation at the time (see Marks, 2011).

The discovery of the brain reward system led to an explosion of research in the field and for a period of years it was the most widely studied topic in physiological psychology

(Thompson, 1999). Subsequent papers by Olds and colleagues (e.g., Margules and Olds 1962; Olds 1958a, 1958b), as well as others (e.g., Delgado, Roberts, and Miller 1954; Routtenberg and Lindy 1956), showed the rewarding effects of stimulating a variety of subcortical sites in several species, including humans (for review, see Olds 1969). After confirmation of the reward effect, one of the first questions to be studied was the relation between brain stimulation reward and more conventional rewards, such as food. Olds and Hebb's student Rolphe Morrison (n. d.) investigated the effect of food deprivation on self-stimulation and the effect of self-stimulation on feeling (Morrison, 1955).

Olds (1958b) argued that the findings on brain reward provide *ipso facto* evidence against drive-reduction and drive-stimulus reduction theories. These theories, in the psychophysical tradition of Gustav Fechner (1801-1887) and early Freudian thought, maintain that reward results from a reduction in (unpleasant) internal stimulation. But in the experimental paradigm of Olds and Milner, reward comes from adding stimulation to the brain: it is possible to bypass the hypothesized stage of stimulus reduction and activate reward mechanisms directly. By implication, the reduction in a drive stimulus may be sufficient for reward but is not necessary. Spurred by the findings of Olds and Milner, the locus of research on long-standing topics of reward, reinforcement, utility, and pleasure moved into the brain (though the notion of "pleasure centre" has been complicated; see Wise, 1980). Indeed, Milner (2011) believed his work with Olds was significant, in part, because it "boosted Hebb's efforts to persuade psychologists to take the brain seriously" (p. 314).

Olds was perhaps uniquely situated to conduct these studies because of his interest in psychological theory and motivation; Milner later noted that "it would be difficult to imagine anyone better qualified for tackling the awesome task of thoroughly investigating the phenomenon" (2011, p. 314). Olds and his wife Marianne "Nickie" Olds (who did much of the early work on the anatomy and pharmacology of the reward system) significantly developed these experiments, mapping the brain for reward and punishment (Olds 1956; Olds and Olds 1963). They left McGill in 1955 to continue their investigation of the reward system at the Brain Research Institute of the University of California, Los Angeles. In 1957, Olds was appointed assistant professor at the University of Michigan. Milner finished his dissertation in 1954 and remained at McGill as a postdoctoral researcher until 1956 when he was appointed assistant professor to replace Rosvold took over the teaching of McGill's experimental and physiological

psychology courses and consulted for IBM in the 1950s. Attempts to clarify the functioning of the reward and reinforcement systems continued for many years, with more than 75 papers published by Milner's students from McGill or by their students (Milner, 1991).

In 1953, Hebb conveyed his excitement regarding recent research developments in the department to Principal James. He states emphatically that "the psychological research in the Donner Building has at least a possibility of doing a half-Rutherford," and goes on to say that while "'discovery' is not a word that can often be applied to psychological research […] in the past six months we have made two discoveries of some importance" and that the department is now "in a position to make a permanent change in the direction taken by psychological theory—with any luck, a major change" (Hebb, Feb 23, 1953). By "half-Rutherford" Hebb is referring to the early work of physicist Ernest Rutherford (1871-1937) at McGill which earned him the Nobel Prize in 1908 and title of father of nuclear physics. Rutherford's demonstration of radioactive decay and later discovery of subatomic particles rocked the world of physical science. By invoking the Rutherford legacy at McGill, Hebb implied that recent experiments had the potential of opening up entirely new realms of psychological investigation not previously available, analogous to the discovery of the building blocks of physical matter.

While the story of the discovery of the reward centre is often one characterized by serendipity, it is important to examine the conditions that made it possible at McGill. The stimulation of deep structures in the brain reflected a major and significant shift from a previous focus on the neocortex to a new focus on subcortical structures in explaining complex behaviors (Dror, 2016). This trend was observed by Hebb, Rosvold, Milner, and others because of their close proximity to neurosurgeons and neurophysiologists who were working on similar problems in human patients (i.e., Penfield, Jasper, and others).

Hebb's department was among the few that had the capacity to integrate behaviorist methodology with recent developments in neurophysiology. This was the environment that Hebb offered his students. Milner's thesis (1950) is an illustrative example. It involved electrical stimulation of the rat's brain while they performed learning tasks. The surgical techniques used in these kinds of experiments would not have been available had opportunities to learn at a world-class neurological institution not been possible. The psychology department was housed in the same building as the department of experimental surgery (Milner, 2011). Access to the resources of the department of experimental medicine meant that mistakes could be observed (with X-rays)

and techniques could be refined. The case of the discovery of the reward centre is representative of the kinds of new methods that were already being explored there: the electrical stimulation of deep subcortical regions in the brain, Hebb coupled these techniques with behaviorist methodologies and conditioning experiments more familiar to psychologists at the time (i.e., Hess's technique for probing the brain and Skinner's for measuring motivation).

Olds and Milner's discovery of the reward centre was canonized by Hebb (1958), who used it to promote the neuropsychological theory he had developed (1949). Baumeister (2006) points out how a pleasurable brain stimulation was previously observed in schizophrenia patients by Robert Heath (1915-1999) in the 1940s. His point is that Heath failed to recognize the significance of this observation, whereas Olds and Milner understood how to articulate their discovery within the relevant psychological discourse; Hebb's vision (1949) helped frame and create meaning for the discovery. Baumeister argues that, at least in part, this is because of preconceived notions Heath held about the etiology of schizophrenia. This episode in the history of neuroscience illustrates the importance of sagacity in serendipitous scientific discoveries. It also shows that "mental preparedness" can be either an opportunity or an obstacle to progress (Baumeister, 2006).

In the early 1950s Hebb contributed to the repositioning of the Department of Psychology at McGill by leveraging a network of psychobiologists and neurophysiologists both in North America and Europe. The experimental culture Hebb fostered (likely mirroring those he had experienced as a postdoc in Lashley's laboratories at Harvard and Orange Park), became the site for the emergence of neuropsychology's earliest pioneers. A key component of this positioning was the capacity to engender theoretical coherence. Hebb accomplished this, in part, through continuity with neurophysiology (see Chapter Three) and the clinical laboratory (MNI). Olds and Milner (as well as Seth Sharpless, Mortimer Mishkin, Brenda Milner, and countless others), benefited from an approach towards interdisciplinary experimentation that Hebb cultivated through the implementation of new tools, techniques, resources, and expertise uniquely available to him as he shaped psychology at McGill.

Principal James spoke favourably of the activities in Hebb's department. For example, in a letter to Robert Morison, neurophysiologist and Rockefeller Foundation official, he wrote: "the chief points of interest are the experiments on the isolation of human subjects for prolonged periods, and work on a "pleasure area" in the rat brain. Both projects seem to have a wide

general significance [...] In view of the quality of young workers attracted to this research and their capacity for independent thought, it may be expected that the significance of the programme will continue to grow" (James, Mar 19, 1954). Hebb shared this optimism: "Physiological psychology is coming closer and closer to neuro-physiology, so that there is frequently a close collaboration between the two […] there is a real prospect that the work of some of the younger men will make Canada outstanding in this field in the next decade" (Hebb, Dec 16, 1953).

The story of the discovery of the reward centre contributes to a more nuanced understanding of the kind of influence Hebb's ideas in the 1950s were exerting on the field of psychology generally. Hebb promoted the interchange of expertise and training available to him through his close relations to key interlocutors such as Penfield and Malmo, and their colleagues and students (Ewen Cameron, for reasons described by Prkachin, 2018, had alienated himself from much of the medical community by this time). The discovery that was made in the Donner Building at McGill in 1954 has been described as among the most important single discoveries made in the history of the brain sciences (Thompson, 1999) and has contributed immensely to our understanding of the brain mechanisms underlying substance abuse and addiction. The more we know about reward, and about the mental-behavioral processes and neural mechanisms underlying it, the more elaborate and nuanced its definition and meaning (Marks, 2011). More than half a century after Milner and Olds learned from neurosurgeons at the MNI how to implant electrodes into the brains of animals, the original methodology has been refined to produce intra-cranial self-stimulation, a paradigm that allows researchers to directly investigate the efficacy of rewards.

4. Pioneering Studies in the Psychology of Pain at McGill, 1951-1956

Similar to experiments at McGill with restricted environments and direct brain stimulation, the study of pain at McGill had its origins in the investigation of disorganized behavior in laboratory animals using neurophysiological techniques. In addition to discoveries across human and rats, Hebb's department sought answers to questions about early behavior in the emotional responses of dogs (Melzack, 1952; Mahut, 1955). From these early experiments emerged new ways of thinking about the relations between organisms and environments and pioneered studies of the psychological aspects of pain experiences (Thompson & Melzack, 1955;

Melzack & Wall, 1982). These studies, conducted in the early 1950s, and their connections to Hebb's postwar research agenda, have typically been discussed separate from the context of broader contemporary psychological investigation in Canada. Yet, the psychological study of pain is deeply woven into the fabric of the kind of psychology Hebb privileged, and a history of pain is incomplete with consideration of broader trends developing around the phenomena of pain in mid-twentieth century North America.

Today, pain — whether knife wound or back pain, headache or heartbreak — is recognized as something that does not emerge naturally from physiological processes, but in negotiation with social and cultural worlds (Bending, 2000; Bourke, 2014; Del Vecchio et al., 1992; Morris, 1991; Moscoso, 2012; Scarry, 1985). Pain is both a puzzle (Melzack, 1973) and a challenge (Melzack & Wall, 1983) at 'the interface of biology and culture' (Coakley & Kaufman, 2008). In psychology, pain has been approached in a number of ways: as a symptom to be treated, as a stimulus to be applied, as a means to modify behavior, and as an object to be investigated. Psychological categories and theories used to understand and direct the clinical management of acute and chronic forms of pain are recent innovations; contemporary scientific accounts of pain rest on a theoretical formulation developed in the mid-1960s.

After World War II, pain was understood in much the same way as it had been for centuries: as a specific response to physical injury (the so-called Cartesian "specificity theory"). Psychologists in the late 1940s and 1950s, heavily reliant on behaviorist S-R theory to inform experimental inquiry, considered pain narrowly. Outside the laboratory, clinical pain presented a different set of concerns (e.g., the experiences from psychiatric and medical treatments of veterans; Henry Beecher's observations, 1946). At the time, the most preferred medical explanation held to a Cartesian model of pain as something that traveled via nerve impulses to the brain. Psychosomatic explanations were inadequate. Experts on pain consisted primarily of neurologists and psychiatrists, who relied upon psychodynamic theories to explain intractable and clinically irregular cases of pain (Harrington, 2008; Merskey, 2004). This included differences in subjective suffering from similar injuries; pathological changes aligned with drastic pain symptoms in one person, but not in another person; forms of phantom pain; headaches without somatic symptomatologies; and the ever-lasting issue of how placebos and suggestive therapies led to successful pain relief (Melzack & Wall, 1988).

Postwar psychological theory could not account for the puzzle of pain experiences. Hebb and others recognized this as a shortcoming of the dominant S-R paradigm (Merskey, 1996). Discussions of pain among neurophysiologists were foregrounded by how best to categorize pain phenomena, framing them as separate objects with neurological underpinnings in the peripheral nervous system. Put differently, it was the question of whether pain was best thought about as a separate sense modality, mediated through nerve structures separate from those which mediate other sensations (Dallenbach, 1939). This position was represented historically by Maximilian von Frey (1897) and opposed by Alfred Goldscheider (1898) who understood pain as a non-specific sense, which could be mediated through tactile nerves from intensive summation of their excitations. Evidence by the 1940s, especially in experiments with analgesic drugs, suggested pain sensations represented a separate modality (Wolff & Hardy, 1947). However, many questions remained. Specific pain receptors were elusive and the nature of the adequate stimulus for pain puzzled researchers (Melzack & Wall, 1982). This challenge of pain was compounded by the inability of scientists to establish the peripheral pathways and higher centres of pain in the brain. Despite what the underlying theory suggested, surgical section of spinal tracts did not clearly abolish pain permanently, and no specialized cortical areas or pain nuclei had been discovered.

4. a. The roots of a psychology of pain at McGill

Hebb was well aware of the shortcomings of 1940s neurophysiology to adequately explain pain (Merskey, 1996). His neuropsychological theory took aim at the process involved in the mediation of pain and attempted to integrate recent neurological data. Several sections of *The Organization of Behavior* (1949) are dedicated to the problem of pain, discussed narrowly. For Hebb, the problem of pain was conceptual and neurological. He emphasized the important role of the smallest type of afferent fibre (i.e., free nerve endings) in the mediation of pain. The impulses of these small fibres are the slowest and weakest of known types, which seemed to oppose the dominant idea that the basis of pain was high intensity peripheral stimulation. Hebb was among those who argued strongly that it need not be either/or, a partial synthesis was possible: “Let us forget peripheral intensity, as the essential determinant of pain, and think instead of a central intensity” (1949, p. 184); he believed pain should be conceived as a central process of a high-intensity, wide-spread, disruptive nature. While this way of thinking about pain helped explain some of the phenomena that were then observed (such as avoidance learning and emotional

response), Hebb was the first to point out that this hypothesis was still far from a complete account (e.g., Hebb's student McMurray, 1949, lists a number of other challenges that existed at the time). This understanding fit the theory (Hebb, 1949) and pointed to the central notion of disruption. Changing thought about the mediation of pain in central processes (higher cognition) was important; it was towards the understanding of these processes that both experiments (e.g., lesion and drug studies) turned to for an explanation of their results.

George H. Bishop (1889-1973), an American physiologist, closely associated with the discovery of the differing action potentials of nerve fibres, engaged in a lengthy correspondence with Hebb in the early 1950s (Merskey, 1996). They struggled with the dilemma of understanding the relationship between physical and emotional factors contributing to experience of pains. While no concrete collaboration or theoretical solution emerged from this correspondence, it likely had a substantial influence on Hebb's thinking about the relevance of this problem to broader issues discussed in his theory (1949).

Hebb's thinking during this period was built upon those of early psychosomatic pioneers such as Harold Wolff (1898-1962) and Henry K. Beecher (1904-1976) who were beginning to distinguish between the "perception of pain" and the "reaction to pain" (e.g., Wolff & Hardy, 1947; Beecher, 1946). These insights emerged, in part, as physicians began operating on the brain of people suffering from intractable pain in the 1940s (for broader consideration, see Stahnisch, 2015; Bourke, 2014). Psychosurgical interventions (i.e., lobotomies) had an unexpected effect: patients would often report that, though they could become aware that they were experiencing something they identified as pain, they were undisturbed by it (Ostenasek, 1948). Both the perception of pain and the person-in-pain's own reaction to pain (which include early experiences and the shaping influence of social worlds) are important to consider. Hebb's neuropsychological theory provided not only the tools to think about mediation processes, it also contained within it the means to address questions about heritability and the role of early experiences in the organization of pain behavior (i.e., reaction to pain) as well as the experience of the sensation of pain (i.e., perception of pain).

Clinical cases were important for experimental psychologists. Hebb's monograph (1949) is replete with clinical as well as experimental data. Clinical evidence was essential to the scientific study of pain (Baszanger, 1998; Rey, 1995). Gordon A. McMurray (n. d.), one of Hebb's early students, working at the Psychological Laboratory at the Allan Memorial Institute

with Robert Malmo, completed his Ph.D. on *An experimental study of a case of insensitivity to pain* in 1949. McMurray's study explicitly built on Hebb's recently published theory (Hebb, 1949) and centred around the study of a single individual, in a format modeled on the medical case study (McMurray, 1949). As McMurray described, "At no time has she ever reported any form of ache or pain, such as headache, earache, toothache, stomach ache or menstrual pain. The whole of her ordinary activities of living from birth have repeated, again and again, this same theme of tissue damage which has gone unnoticed or looked upon indifferently [i.e., pain insensitivity]" (McMurray, 1949, p. 9). McMurray used Hebb's theory as means to synthesize and to critique (e.g., p. 10) existing explanations and used Hebb's theory of mediation in central processes to explain the neurophysiological and behavioral data that he gathered. The following year he was appointed head of the department at the University of Saskatchewan (McMurray, 1982). While teaching was the main focus of the department, McMurray introduced research on perceptual studies and pain sensitivity he had begun at McGill (e.g., McMurray, 1955) and established close ties with the Department of Physiology (McMurray, 1982). The small department included William Bexton (n. d.), who had left to pursue graduate studies with Hebb in 1951 and had been involved in his sensory isolation studies (e.g., Bexton, Heron, & Scott, 1954).

With the integration of clinical and laboratory medicine in the early decades of the twentieth century came challenges to long-held understandings of the complexity of pain experiences (Baszanger, 1998; Melzack & Wall, 1983). Neurophysiologists and neurosurgeons alike found theoretical explanations of pain mechanisms (e.g., S-R theories) to be inadequate for making sense of the complexity of experiences associated with pain (e.g., phantom limb pain, intractable pain, insensitivity to pain, etc.). The importance of the subjective (clinical) and behavioral dimensions of pain experiences emerged as an exemplary problem for Hebb's neuropsychological approach (Hebb, 1949). It was becoming more apparent that pain must refer to both a distinctive sensation and a resulting emotional state (which tends to produce strong avoidance behaviors).

Existing neurophysiological explanations could not account for the lived experience of pain or its many perplexing clinical and psychological manifestations, this was becoming increasingly evident after World War II. The idea that being-in-pain involved a myriad of factors that extend beyond the body proper opened the door for new (top-down) ways of thinking about

pain. It was shortly thereafter that a more holistic scientific investigation of pain, for the first time, became a necessarily interdisciplinary field, gaining traction around the world in the 1940s and early 1950s (see Rey, 1995; Baszanger, 1998).

It was into this field of confusion that Hebb interested a young protégé, Ronald Melzack (1929-2019), in the challenge of pain. Melzack initially met Hebb in the late 1940s following an interesting course in psychology taught by Lansdell. Melzack had the opportunity to pursue an honors thesis (on curiosity in the rat) with "the great Donald Hebb" (Melzack, 1993, p. 5). After Melzack completed his degree Hebb agreed to take him on as his graduate student. It was into this milieu in 1949 that Melzack soon became involved in the restricted dog studies and went on to revolutionize the field of pain psychology. He credited his interest in the study of pain to observations made during Hebb's early sensory deprivation experiments with dogs in the early 1950s (Shainblum, 2009).

Following Hebb's guidance, Melzack completed his MSc on irrational fears ("spontaneous fears of the uncanny") in the dog (1951), which explicitly extended the work Hebb and Riesen (Hebb & Riesen, 1943; Hebb, 1946) had done on chimpanzees while at the Yerkes Laboratories in the mid-1940s. Melzack published this study the following year (Melzack, 1952) and was among the first of Hebb's students to study emotion behavior in dogs, which was further extended by Helen Mahut (1955).

4. b. Emotional dogs and brains

When Hebb was awarded funding by the Canadian government to study the effects of restricted environmental experience in humans, he also proposed to study the behavior of dogs reared in restricted environments. Hebb had hoped to extend work done on the neuropsychological basis of motivation, emotion, and learned behaviors. Hebb's experiences with chimpanzees were challenging, he needed an alternative animal program in place relatively soon. Hebb chose purebred dogs. In 1950, with the financial support of the DRB, four Scottish Terriers of an inbred strain were obtained from the Jackson Memorial Laboratories (Clarke, 1951). The aim of the department program was to use the offspring of these purebreds in experiments on restricted environments, similar to those conducted by colleague A. H. Riesen (e.g., 1947).

Between 1951 and 1956 the department's dog studies were conducted by Ronald S. Clarke, William R. Thompson, and recent graduate, a young Ronald Melzack (Brown, 2007).

While Heron, Bexton, and Doanne were conducting the sensory isolation experiments on humans, Clarke, Thompson, and Melzack conducted a series of related experiments with Scottish Terriers; both extended Hebb's investigation of "central processes" (i.e., top-down influence), and both were intended to shed light on disruption associated with the restriction of sensory experience at various stages of development and to different degrees.

These experiments began, as new research programs often do, with the establishment of baselines. The group started with a systematic assessment of individual behavioral and emotional differences in dogs (Clarke et al., 1951). This line of study was broadly pursued and discussed in the department at this time. One of Hebb's early students, Helen Mahut (n. d.), was a Polish holocaust survivor who had aided the Polish resistance and fled to Canada (Benjamin, 2011). Mahut conducted research under Hebb "to extend the investigation of constitutional factors in emotional behaviour, and to relate these to the factors of experience" (Mahut, 1958). She completed a Ph.D. in 1955 on differences in emotional behaviors in various dog breeds (Mahut, 1955) which laid the experimental foundation for the Scottie group (i.e., Clarke, Thompson, and Melzack). Mahut spent most of her later career at Northeastern University in Boston studying the neuropsychology of perception, memory, and spatial learning in monkeys (Mahut, 1971).

The early restriction experiments in dogs began in 1951. After four weeks, once each litter had been weaned, the Scotties were divided into two groups. One group was adopted by family and friends of the researchers (or simply reared as "free pups" in the laboratory), while the others served as the experimental subjects, confined to separate cages, closed in with opaque sides. They were reared in complete sensory isolation. A mechanism was developed so, even during feeding and cleaning, the dogs never saw their keepers or each other (Thompson & Melzack, 1955). This was referred to as *radical isolation*. After seven to ten months living in this restricted environment they were released and given the same treatment as the control dogs (i.e., those who had been reared/socialized normally). This isolation experiment followed those conducted with rats (e.g., Hebb, 1947) and at the time more recently, with higher-order animals (Warren, 1996); at McGill, they wanted to understand how "marked disturbances of social behaviour and motivation may occur in the restricted animals" (Clarke et al., 1951, p. 156). Cold War concerns regarding "brainwashing" aside, the disruption of organized behavior caused by sensory restriction was unknown.

The comparison between these two groups of Scotties was remarkable. Melzack and Thompson (1955) report that it was immediately obvious even to a casual observer that restriction had profound and surprising effects. Melzack describes this as "the turning point" in his career:

> "when these animals came out of their cages, at first they would freeze, and then they'd become excited and then they became overwhelmingly excited and would run around the room, any room that they were in [...] they would run around our feet and we would try to get out of the way and we'd step on a paw, a tail -- *no evidence of pain*. I used to be a smoker in those days -- I lit up a match, they'd stick their nose in the match … " (Melzack, 1993, p. 7-8, emphasis added).

Whereas Thompson and Heron focused on specific problem-solving impairment in the restricted dogs (Thompson, Heron, 1954a; Thompson & Heron, 1954b), Melzack instead was interested in the peculiar response these dogs showed towards a pain stimulus (i.e., emotional behavior). Melzack noted that the control dogs raised as household pets exhibited a very different reaction to pain: "Whereas normal dogs usually dashed away from the pain-causing object, the restricted dogs spent considerably more time around it after they had been hurt than before" (Thompson & Melzack, 1955, p. 40). They determined that the dogs were not insensitive to pain, they simply reacted to it differently; they had developed a different relationship to the experience. For Thompson and Melzack, this raised important questions about the profound effect of early experience on behavior; which Hebb's psychology could explore with the neurophysiological tools available (and being developed) at McGill. Regarding the Scottie experiments, Hebb indicated the following year that they "given preliminary results that raise a number of new questions *for which methods of study are available*" (Hebb, Sept 22, 1952; emphasis added). Hebb's neuropsychological theory included both conceptual methods as well as empirical methods (such as behavioral measurement and surgical techniques). For Melzack, and gradually the rest of the discipline, "pain then suddenly became an interesting problem" (Melzack, 1993, p. 9).

4. c. The emergence of a psychology of pain

The studies of these dogs and subsequent observations became the basis of Melzack's dissertation which he completed under Hebb's supervision in 1954 on "The effects of early experience on the emotional responses to pain" (Melzack, 1954). These experiments led to the

conclusion that "early perceptual experience determines, in part at least, (a) the emergence of overt responses such as avoidance of noxious stimulation, and (b) the actual capacity to perceive pain normally" (Melzack & Scott, 1957). Thus, overt responses, or pain behaviors, are learned, as is the perception of pain itself. Hebb's skepticism of S-R theories and the conceptual limitations inherited with these models applied and were easily demonstrated in the assumptions of specific, straight-through pain pathways from skin to a pain center in the brain. Melzack created space for exploring/testing the psychological aspects.

This marks an important development to the scientific study of pain: Psychological expertise became an important and critical aspect of any thoroughgoing theory in the 1950s. This provided an alternative approach to existing psychiatric approaches, and had the benefit of being unaffiliated with Freudian or clinical psychology in the 1950s. Pain became the quintessential neuropsychological subject, stubbornly irreducible and outside of language (Scarry, 1985; Bourke, 2014); both completely physiological and psychological, demonstrably limited and incomplete without the other.

This recognition was gaining traction as Melzack was joining the department in 1950. Pain is discussed in *The Organization of Behavior* (1949) as a "disruptive somesthetic event" in neurological terms in the context of motivation and learning (p. 181-190). Hebb's interest in pain as a neuropsychological problem followed the publication of this book (Merskey, 1998). Melzack hoped to connect these emerging insights to reflect recent developments in neuropsychology (i.e., Hebb, 1949). While studies on pain had been conducted at the MNI (e.g., McNaughton, 1941) and at the Allan Memorial Institute (e.g., McMurray, 1949), Melzack's studies were different. They were able to speak to the organization of the (pain) behavior itself, which was rooted in the dog's early experiences. His breakthrough was in connecting studies of emotional responses in dogs to broader considerations of neuropsychological theories and the role of "central processing" arising from adjacent research (e.g., Bexton et al., 1954).

In 1953, William K. Livingston (1892-1966) visited Jasper at the MNI (Melzack, 2005). Jasper was aware of Melzack's dog studies and interest in pain. Jasper introduced Livingston to Melzack and after his Ph.D. Melzack went to Portland, Oregon to join Livingston's pain laboratory and clinic (among the first of its kind). Livingston was among the earliest critics of the Cartesian specificity pain theory. He wrote *Pain Mechanisms* (1943) which brought to attention the many pain phenomena that defied explanation in terms of existing specificity

theory. Livingston's clinic, which operated out of the University Oregon Medical School, was based on the model established by John J. Bonica in the 1940s (Baszanger, 1998). Melzack studied at this clinic between 1954 and 1957. He traces this period to the emergence of his fascination with the many ways patients described their pain (Melzack, 2005), which became the basis of the McGill Pain Questionnaire (Melzack & Torgerson, 1971).

The McGill Pain Questionnaire (MPQ) was developed in partnership with Warren Torgenson (1924-1999) who Melzack had met at the Massachusetts Institute of Technology in the early 1960s. The MPQ is an important tool in pain medicine. (It is interesting to note how William Tait was chastised in the 1930s for attaching McGill University to his intelligence tests, while Melzack was encouraged to use the name - the value of psychology to the university seems to have changed.) Today, together with the Visual Analogue Scale, the McGill Pain Questionnaire is probably the most frequently used self-rating instruments for the measurement of pain in clinical and research settings (Melzack & Katz, 2001). Melzack returned to McGill in 1963 as Associate Professor and in 1974 helped establish a similar pain clinic at the Montreal General Hospital with Joseph Stratford (1923-2007) and others (Merskey, 1998).

By the 1940s, with greater focus on complex and under-explained pain experiences (e.g., Beecher, 1946; Wolff & Hardy, 1947), Melzack had already entered a highly contested field. Specificity theory was too rigid and simplistic in the minds of psychologists and physicians who borrowed freely from both clinical case study and neurophysiology. The experience of pain is multifaceted; it involves separate, but closely interrelated, emotional, cognitive, physiological, and social dimensions. These aspects of pain, normally experienced together and indistinguishable, became separated in certain clinically-relevant circumstances. The effect of morphine, for example, is mostly to reduce the unpleasantness of a pain rather than making the pain unrecognizable as such. Lobotomy for intractable pain functions similarly; the patient reports that the pain is the same as before -- but it no longer bothers them. This more nuanced consideration led, for the first time, to calls for closer investigation of the mediating influences, or "central control" of pain by the (encultured and embodied) brain. One of the most influential theories of pain to do this was co-developed by Melzack in the mid-1960s.

4. d. Gate-controlled Pain

Melzack met British neurophysiologist Patrick D. Wall (1925-2001) while at MIT in the early 1960s. Like Livingston, Wall was interested in the neurological mechanisms that might

explain the bewildering experiences of complex pain sufferers and was particularly influenced by the work of the Dutch physiologist William Noordenbos (e.g., 1959). Both men operated at the periphery of the medical establishment and were ardent critics of pain medicine's dependence on surgery and drugs. Wall and Melzack grew intellectually close during this period at MIT (1960-1963). Wall was a young left-leaning Oxford graduate (1959) recently appointed professor of physiology (and affiliated with MIT's Research Laboratory of Electronics) and Melzack was jointly appointed to the Departments of Psychology and Biology at the time. As Wall recalls, "At this time, with a completely different background ... there was Ron Melzack ... He had worked with [William K.] Livingston on some sort of classical neurophysiology, so that he knew about physiology. But what he really knew about, which was highly unusual, was *animal behavior*, which was clearly very odd indeed [...] So he and I got talking together" (emphasis added, Wall, 1993, p. 16). Melzack's edge as a psychologist was due in part to the training he had received with experimental animals while at McGill in the early 1950s. These experiences, and their explanations in terms of Hebbian theory, challenged mainstream pain science for the same reasons that neuropsychological theory challenged dominant S-R theory broadly. They failed to adequately address the clinical and psychological experiences of their research subjects (whether Scottish terriers or human patients).

Melzack and Wall shared an abiding skepticism regarding 1950s pain science and what they saw as anatomical bias, which led to an article in *Brain* (1962) on theoretical issues underlying problems of somesthesis. It was scarcely noticed (Melzack, 2005). A few years later, after Melzack had returned to McGill, they tried again, with an almost identical paper in *Science* (1965). In their article, *Pain mechanisms: a new theory*. they offered the *gate-control theory of pain* which, basically, proposed that a gate-like mechanism exists in the nervous system such that pain signals can be modulated *before* they evoke perception and response. This paper challenged dominant pain theory, carefully discussed both specificity and pattern theories, and argued strongly for the modulation of noxious input within the spinal cord (the gating mechanism). It also took into account the influence of descending pathways ("top down" modulation) and provided the first unitary theory able to reasonably accommodate existing knowledge of the complexity underlying pain experiences.

Today, the gate control theory is considered the most promulgated of pain theories, having led to some of the most fruitful research in the field over the past fifty years (Moayedi &

Davis, 2013; Katz & Rosenbloom, 2015). Although modified and in part refuted since, the paper gave rise to enormous interest and stimulated much work; the theory remains a unifying concept of the pain studies field (Dickenson, 2002; Mendell, 2013). As of 2020 the gate-theory had received more than 5500 citations (Web of Science v.5.35). Neurobiologist Lorne Mendell noted that "general ideas put forth in the paper [...] have transformed our understanding of pain mechanism" (2014).

Melzack later observed that the theory was "riding in on the Zeitgeist [of the 1960s]" and that both he and Wall were astonished at its wide acceptance. The reasons for its uptake are complicated. Historian Keith Wailoo has suggested its appeal owed less to a "cultural spirit" whisking ideas along and much more to the fact that the theory resonated on multiple levels with the era's legal battles, cultural critiques, pain relief practices, and liberalizing political commitments. While Wailoo suggests it was "scientifically vague yet culturally appealing" (Wailoo, 2014, p. 78), others have noted instead the clarity and the general testability of ideas put forth in the paper (Mendell, 2013) as responsible for the broad scientific and social interest. The gate can be opened or closed by variable amounts and by various psychological processes, it became possible to attempt to close the gate by various means (Baszanger, 1998), leaving the door (or gate) open to a wider array of experts than was previously possible. As Wall himself wrote evaluating the gate control theory: "The least, and perhaps the best, that can be said for the 1965 paper is that it provoked discussion and experiment" (as quoted in Mendell, 2013).

Through realizations that started with the careful observation of the social and emotional behaviors of Scottish terriers, Melzack began to formulate a radical theory. Similar to the position Hebb expressed regarding the complex interplay of hereditary and environmental factors (see, for example, Hebb's two types of intelligence), Melzack held that the phenomenon of 'pain' does not emerge naturally from physiological processes, but in negotiation with early environments and social worlds (Morris, 1991; Bourke, 2014). Melzack explained in 1961 that, "pain is not a fixed response to hurtful stimulus … The amount and quality of pain we feel are also determined by our previous experiences and how well we remember them, by our ability to understand the cause of the pain and to grasp its consequences. Even the significance pain has in the culture in which we have been brought up plays an essential role in how we feel and respond to it" (Melzack, 1961, p. 41).

In the late 1950s, following Melzack studies, researchers at McGill (critical of traditional measurements of pain sensitivity) wanted to explore the degree to which the subjectivity of pain might be manipulated by the environment by introducing 'an ethnocentric prestige motive' (i.e., inter-group rivalry). In this research, they found that they could change the level at which people tolerated pain by infusing the behavior associated with pain tolerance with socio-cultural meanings (Lambert, Libman, & Poser, 1960). Such studies had an important impact on how pain research was later conducted (Bourke, 2014). Research began into the ways that the acuteness, salience, duration, and affective qualities of pain shifted according to the meanings attached to the noxious stimuli. Pain was more than tissue damage. It was intrinsically affected by interactions with other people and the environment. The scientific psychology of pain that Melzack, Wall, Livingston, and others pioneered provided clinicians and their patients new ways to understand and express their pain. A comprehensive psychology of pain required that attention be paid to ideological frameworks, social relations, and environmental interactions. But this was only possible once the active role of the brain in mediating its experience was clearly recognized (Melzack & Wall, 1965).

In the late 1950s and early 1960s, it is now apparent, the field was in ferment (e.g., Baszanger, 1998; Melzack, 1973). Evidence against specificity theory had accumulated, but no convincing alternative theory was available to take its place (pattern theory remained too vague). The evidence came from three fields: clinical observation (e.g., McMurray, 1955) psychological observations (e.g., Lambert, Libman, & Poser, 1960), and neurophysiological and anatomical studies (e.g., Hebb, 1949). Psychological aspects of pain, constructed as they are in biomedical notions of the body (Kugelmann, 1997; Radley, 2004), allowed for a broader conception of the subjective dimensions of pain than were previously possible. Melzack's work has extended to studies of phantom limb and labor pain, and into the mechanisms of opioid analgesia.

Conclusions to Chapter Five

Melzack's early experiments as a graduate student with Scottish terriers are important because they demonstrate how Hebb's neurological theory served as a toolkit for making sense of an array of previously unexplainable behaviors. Restricted environments produced a multitude of disorganized behavior and, once the pieces were laid out, Hebb's peers used his theory to put the picture back together again. Borrowing from the Berkeley-based "New Look" school of

perception, the Gestalt tradition, and the neurophysiology community, Hebb emphasized the role of *experience* in determining higher mental activities, and shaping emotional behavior (such as pain and reward). The research Hebb and his department conducted in the 1950s suggested strongly to him that *every* aspect of behavior (associated with both emotion and intelligence) is jointly determined by hereditary and environment (e.g., Hebb, 1953). The examples provided in this chapter each contributed to the broadening of the kinds of (neuro-)expertise considered relevant to unraveling the underlying structures of behavior and cognition, leading to the liberalization of methods (Harrington, 2008; Wailoo, 2014). Melzack was a product of postwar McGill psychology. Like the gate control theory, Hebb's neuropsychological theory endorsed diversity in the form of interdisciplinary collaboration, emphasized the role of top-down processes such as experience and emotions, and expanded the field's consideration of subjective experience (as part of legitimate medical and legal discussions).

Hebb's studies of the rearing of rats in enriched environments in the late 1940s initiated ideas about environmental input at key stages of neural development, leading to the recognition that sensory-neural connections are intimately shaped by experience (e.g., Hebbian learning). The cell assemblies and phase sequences that Hebb proposed offered guidance, but they were never the whole story; they got researchers to the next step. The cases of Olds and Milner's discovery of the reward centre and Melzack's insights about pain are illustrative examples of the direct influence of Hebb's ideas and 1949 monograph. For example, more than thirty years after Hebb attempted to reconcile learning, motivation and drive in the notion of a "conceptual nervous system" (Hebb, 1955), Melzack extended the idea as the basis of his own "neuromatrix theory" (Melzack, 1989), representative of an understanding of the nervous system as fundamentally integrative and intrasubjective.

Comparing the discovery of the reward centre with the pioneering studies of pain by Melzack emphasizes the bidirectionality of the research process Hebb advocated for postwar psychologists. Whereas Olds and Milner's intuition was to apply behavioral techniques to understand physiological changes they had induced in rats, Melzack's insights are derived from applying available physiological evidence about pain to the unexplained behaviors of his Scottish terriers; Olds and Milner took a predominantly bottom-up approach, while Melzack's was able to successful present an approach that was both bottom-up and top-down. By the end of the 1950s, Hebb's department had successfully fostered the means (through both technology and expertise)

to start to address the persistent problems of the dominant S-R psychology he had identified over the first half of his career. As a result, Hebb's place in the history of postwar psychology (not to mention the marginalized role of Canadian psychology in these histories in general) is challenging; he simply does not fit existing narratives.

Most histories that examine the rise of postwar psychology tend to consider psychologists to have belonged to one of two camps: mainstream neobehaviorists (represented by Skinner's brand of radical behaviorism and Hull's hypothetico-deductive model) and the Gestaltists. But between and against these two camps was a neuropsychological style that failed to fit neatly into historical narratives, as either clearly behaviorist or cognitivist. Hebb's problems, in contrast to the major cognitive breakthroughs of the 1950s (see Gardner, 1985; Heyck, 2015), were grounded in the lived experiences and emotional behaviors of animals rather than the information and computational interests of the cognitive psychologists that would follow.

Research on perception and memory, amenable to laboratory investigation, were common topics among 1950s psychologists, as they had been for physiologists (see Dror, 1999) and psychobiologists in the early twentieth century. Early cognitive psychology inherited these problem areas and extended their research into greater abstraction with the emergence of computation, information theory, and cybernetics (see Kline, 2015). Early cognitive psychology inherited these problem areas and extended their research into greater abstraction with the emergence of computers, information theory, and cybernetics (see Kline, 2015). Psychologists such as Herbert Simon (1916-2001) and George Miller (1920-2012) focused on the brain's capacity for informational processing and were drawn to systems science and the organizational revolution (Heyck, 2015). Psychologists had long been searching for their own set of immutable laws similar to those found in adjacent disciplines; Miller was "holding out hope of a marriage between the quantities of data collected by psychologists over the years and the rigorous new approaches of the engineering-oriented scientists" (Gardner, 1985, p. 90). Olds and Milner, in their investigations of reward circuitry in the rat brain, integrated both features of the behaviorist tradition (i.e., the use of Skinner boxes) and the new neurosurgical and engineering techniques (e.g., Delgado's electrode procedure) pioneered in adjacent departments (i.e., the MNI). Hebb's credit was to encourage a research practice that sought out opportunities to combine a top-down approach (i.e., purposive cognitivism) and a bottom-up approach (i.e., S-R connectionist theory).

The Organization of Behavior (1949) drew liberally from examples from perception and memory to make his points about cell assemblies and phase sequences, but the research that he and his colleagues conducted at McGill in the 1950s took the validation of these theories in a different direction than his cognitivist colleagues. Though Hebb was an early pioneer of neural networks, used in later computational models, he seems to have been on the outside of both the cybernetic moment (Kline, 2015) and 'cognitive revolution' (Gardner, 1985). Hebb's theory-building and integrationist agenda focused his department's attention on broader experimental concerns. Miller would later describe Hebb as "the last of the big-theory boys" (Miller, as cited in Baars, 1986, p. 220). He was "big-theory" because Hebb explored the experiencing subject (see Dror, 2001) in a way unique at the time, as demonstrated in the research on restricted environments, reward mechanisms in the brain, and the complex psychology of pain, that was pioneered in the 1950s at McGill University.

It is worth highlighting that early cognitive psychologists tended to avoid dealing directly with emotional states, which is in notable contrast to Hebb and his department at McGill. The research with which many psychologists of this era engaged, such as Neisser, Bruner, Miller, and Simon, was decidedly "cold" (areas such as cognitive processing and memory). In the 1950s Freudian thought infused psychological (and popular) thinking about our complex emotional inner lives. The relationship between psychiatry and neurology after war was tenable at best (on these connective and localizing traditions, see Guenther, 2015). The role of the emotions and lived experience was sidelined, with most 'hard-nosed' psychologists (e.g., Hull, Skinner) tended to downplay or avoid serious affective considerations in the development and validation of behaviorist psychological theory; it was too messy.

Hebb drew explicitly from physiological traditions, which has a much longer and more complicated relation to emotion in experiment (see Dror, 2001). During the late nineteenth and early twentieth centuries American physiologists struggled with the reality that the emotional experiences of their laboratory animals influenced their experiments (Dror, 1999). Psychologists, detached from the physiological roots of their discipline, had barely begun to grapple with these implications (few Canadian psychologists were familiar with these debates) and sought instead to ignore or downplay their role in experiments (Morawski, 1988). It was this shortcoming, in part, that Gestalt psychologists and New Look approaches addressed their critiques of contemporary behaviorism (S-R theory). As a physiological psychologist, Hebb was sympathetic to these

arguments (see Chapter Three), and shared with neurosurgeons, physiologists, and holists (such as Kurt Goldstein, Adolf Meyer, and Wilder Penfield) a need to consider the embodied, lived experiences of their subjects (Lawrence & Weisz, 1998). This emphasis on embodied or "hot" cognition (influenced by emotional experiences, such as sensory disruption, pain, and reward) was in contrast to the decidedly more "cold" cognition associated with the processing of information independent of emotional involvement.

The turn to emotions in physiology in the early twentieth century is embodied by the 'discovery' of the fundamental relationship between an organisms' internal and external environment (Dror, 1999; Oosenbrug, 2011). Hebb was starting to establish connections between cognitive and affective systems before the broader turn to affect in psychology in the late 1980s and 1990s (e.g., Ekman, 1993; LeDoux, 1995; Damasio, 1998). Indeed, affective processes cut through each of the cases presented in this chapter. At McGill, emotion was recognized as the outcome of a disturbance in an otherwise orderly system (Heyck, 2015); the discovery of the reward centre depended on seeing the peculiar behavior of rats not as inherently disordered, but as a physiological response to an internal stimulation. The emotional state of the rat, its *feelings* (cognitive appraisal), were part of the neuropsychological framework that Hebb's department regularly employed. This framework bridged emerging systems thinking with neurophysiological critique, without sacrificing the subject's own lived experiences in the process.

In the case of the spontaneous fear response, Hebb (1946) explained emotions (such as fear and anger) as "the disruption of temporally and spatially organized cerebral activities" giving rise to the behavior that seeks to restore integrated cerebral action (p. 273). Emotions and motivation were theoretically linked (something that "was lacking in psychology at present," Hebb, 1946, p. 275), both grounded in neurophysiology and the "total action of the nervous system" (Hebb, 1949, p. xiv). Indeed, the role of "higher processes" in neurological frameworks figured uniquely in the department's approach, for example, to reward and pain. In an essay Melzack wrote for *Scientific American*, he concluded with the radical proposition that pain was best understood as "a function of the whole individual, his present thoughts and fears as well as his hopes for the future" (Melzack, 1961, p. 49).

The dominant theory of affect at the time, the James-Lange theory, which proposed that physiological changes precede emotions, represented an incomplete picture to Hebb (Hebb, 1949). Considerations of emotion, Hebb argued, have been about as confused as that of any topic

in psychology, "partly because the terminology is often equivocal and partly because tradition carries great weight" (p. 236; see Dror, 2014). Hebb argued for an expansion of the Cannon-Bard theory of emotions: "we must go on next to ask how the hypothalamic expression is modified by the presence of a large thalamus and cortex, how learning affects emotional behavior, and the relationship of emotion to perception and thought" (Hebb, 1949, p. 236). Hebb was explicit: emotion to him "refers neither to an immaterial state of consciousness nor to the observable pattern of emotional behavior" (Hebb, 1949, p. 238). Hebb held a dimensional model of emotion (Hebb, 1955; Schlosberg, 1954). Emotion was arousal (either too much or too little) and it has a primarily disorganizing influence on cognitive functions; but, the nature and manifestation of emotional arousal (i.e., the expression of emotions) depended on capacities developed early in life (see Hebb, 1953; Hebb, 1958).

In the early 1960s, two different scientific approaches to the emotions were simultaneously proposed. One approach, associated with a famous (if problematic) experiment by Stanley Schachter and Jerome Singer, published in 1962, claimed to demonstrate that affect and cognition are indissociable. A rival approach, also first published in 1962, was associated with the work of Silvan Tomkins, who argued that the affects and cognition constituted two entirely separate systems and that, accordingly, the emotions should be theorized in anti-intentionalist terms (Leys, 2011). Hebb's neuropsychological theory aligned with Schachter-Singer's "cognitive appraisal" model, which would rise in popularity but has since been displaced by the Tomkins-Ekman approach and remains firmly entrenched in contemporary neuroscientific work on the emotions (Leys, 2011).

It is likely Hebb would have endorsed both the shared biological heritage of emotions and the understanding that affective processes might occur somewhat independently of intention or meaning. However, he would likely have disagreed that basic emotions (such as fear and anger) are absent cognitions or beliefs about the objects in our world, as Tomkins-inspired theorists suggest (Leys, 2011). In fact, Hebb's neuropsychology relates meaning closely to the emotions: "The clearest distinction between the different emotions is found in the ideas that go with them, and the actions that these ideas (mediating processes) give rise to" (Hebb, 1958, p. 239). Emotions are inherently embodied, intentional states rather than discrete patterns of physiological arousal; According to Hebb, "[emotion] refers neither to an immaterial state of

consciousness nor to the observable pattern of emotional behavior" but to the *hypothetical neural processes* that produce emotional behavior (Hebb, 1949, p. 238).

It is doubtful Hebb would have distinguished, as affect theorists now do, between affect and emotion. Hebb's 'theory of emotion' in the 1950s, limited as it was by what was known at the time, contained elements of both Schachter and Singer's cognitive labeling theory as well as the Tomkins-Ekman approach. Olds and Milner had demonstrated how reinforcing affect was capable of discharging in a self-rewarding (or self-punishing) manner, but not without regard to the situation that elicited them. Hebb and Melzack were interested in "the affective, 'driving' [motivation] component" of pain perception (Melzack, 1961, p. 48). This approach necessitated a deeper consideration of potential mediators of emotional experiences than was typical of psychological experiments in the 1950s. In Melzack and Wall's gate control theory inputs descending from the brain (such as attention, emotion, and experience) to the gating mechanism (in the spinal cord) are modulated *before* they evoke perception or response. Affect is inseparable from signification and meaning, in the case of being-in-pain, because appraisal (cognition) and intentionality is constitutive of the emotion itself. While this interpretation appears seemingly at odds with the "Basic Emotions" paradigm of Tomkin, Ekman, and others (Leys, 2011), it also suggests that a certain degree of affective processes (and even a kind of intelligence) go on in the body independently of cognition or consciousness (and that the mind operates too late to intervene; Leys, 2011).

Hebb's neuropsychology, if anything, emphasizes the complex interdependence (co-construction) of affect and cognition. He wrote, "We are accustomed to think of any particular response as either learned or innate, which is apt to be a source of confusion in thinking about such things [...] is the response inherited, or acquired? The answer is, Neither: either Yes or No would be very misleading. The irrational emotional disturbances of man and chimpanzee are fully dependent on learning, but are not learned in the usual sense of the term" (Hebb, 1949, p. 245). To Hebb, it is a form of emotional learning (from undifferentiated emotional disturbance to adaptive emotional behavior) that mainly differentiates the various emotions (Hebb, 1949, p. 250).

Historial Otniel Dror argues that, in introducing the truncated brain as the generator of emotions in the 1930s, physiologists had created a new type of emotion, one that marginalized psychological forms of knowledge about emotion. I believe that Hebb's vision (1949) is a

response to the historical separation of psychology from physiology that Dror (2001) describes and is grounded in the emerging systems thinking of the postwar period (Heyck, 2015). This chapter described some of the ways that research at McGill grew out of and extended Hebb's early studies of emotion and "central processes" in the 1940s and borrowing from an amalgam of theoretical influences coming from Functionalist thought, Lashley's psychobiology, Tolman's purposive behaviorism, Postman's "New Look," and many others. This chapter described the ways in which historical concerns and opportunities shaped the style of psychology that emerged at McGill in the postwar period, helped clear the way for the cognitive revolution, and rejuvenated interest in a thoroughgoing neurophysiological account of the mind with space for emotion and intentionality. Hebb's alignment of the discipline's interests with those of physiology served to expand the realm of possibility for psychology in the second half of the twentieth century.

Conclusion

In 1960, at the annual meeting of the American Psychological Association in Chicago, Donald Hebb gave the sixty-eighth presidential address. Hebb was the first non-American to be elected President of the American Association. In his address, titled "The American Revolution," he praised the theoretical accomplishments and clarification brought about by American experimental psychologists (such as Thorndike, Watson, Holt, Lashley, Cattell, Terman, and Yerkes) in the early decades of the twentieth century (Hebb, 1960). "Much of the progress that has been made in this century [in experimental psychology]," he explained, "is evident in the codification of ideas and terminology [i.e., a common behavioral language]" (Hebb, 1960, p. 744). This progress, however, came at a cost. Hebb observed that "the establishment of a thoroughgoing behavioristic mode of thinking [was] achieved, too frequently, by excluding the chief problems of human behavior" (p. 736). Disciplinary disunity, Hebb remarked, was due to incompatible approaches: one, the search for *consistent* explanation, characteristic of learning theory (e.g., Hull, Skinner, Spence), and the other, the search for a more *adequate* statement of the problems (i.e., neuropsychological theory). Hebb expressed an ambivalence towards the narrow, but useful, "S-R formula" and goes to great length to affirm his allegiance to the behavioral forefathers. His main argument, however, is that the discipline was entering a new phase. Hebb proclaimed that it was now time for "an equally thoroughgoing behavioristics of the thought process" (p. 736), an integrative approach focused on the role of cognition (as well as emotion and experience; see Chapter Five) on learning, motivation, and the problems of human behavior (Hebb, 1960). Hebb noted that, like the revolution of 1776, the American revolution in psychology was "only a beginning" (Hebb, 1960, p. 736).

The following decades saw an evolving return to attitudes and trends present prior to American behaviorism which had continued to thrive in other parts of Europe and the UK (Mandler, 2002), as well as Canada (e.g., functionalism). The Department of Psychology at McGill, like Canadian psychology in general, was not similarly dominated by behaviorist approaches (Gardner, 1985; Baar, 1986), and instead, due perhaps to Montreal's cosmopolitan connection to Europe and Canada's research structure (see Chapter Four), allowed for a different kind of (neuro-)psychology to emerge. In his 1960 address Hebb expressed the broader sense,

coming from a decade of postwar research at McGill, that the S-R theory was unsuited to central issues in human psychology (e.g., pain, reward, and motivation). The neglect of a number of uniquely human processes, namely the growth and development of thinking, and the intimate association between thought and emotions, was recognized by later cognitivists (e.g., Neisser, 1963) as well. And while many would take a "cold" approach, Hebb's neuropsychological theory emphasized ways to think about "hot" cognitions (see Chapter Five).

The "revolution" to which Hebb referred, was in actuality often slow and piecemeal throughout the 1960s, more characterized by the integration of different approaches and concepts (Greenwood, 1999; Mandler, 2002), than by revolutionary change (see Leahey, 1992). Given the influence of Hebb's psychology at McGill in the 1950s, it is doubtful Canadian psychology warranted a similar "cognitive revolution." Mid-twentieth century Canadian psychology was grounded in the experimental approaches Hebb had imported from Lashley's Primate Laboratory and Penfield's clinic, representing a separate, but overlapping, story of the rise of cognitive neuropsychology. This story does not emerge from a traditional behaviorist setting but from a department steeped in the bifurcated history of psychology as academic discipline and as applied profession. The neuropsychological theory that Hebb advanced represents a more embodied and physiologically-based science of behavior than the cognitive psychology that emerged at places like Yale, MIT, and Harvard. Indeed, Hebb and his department's work in the 1950s represent a style of thought and research that allowed for a more richly complex, environmentally responsive, and neurologically-consistent image of the human mind to emerge in the 1960s.

A uniquely Canadian postwar research style was centered at McGill which involved a plethora of students, visiting scholars, and postdoctoral researchers, drawn to Montreal from around the world. Hebb fostered connections to departments across Canada and in doing so helped shape the research agenda of postwar psychology at Queen's (Thompson), Saskatchawan (McMurray), Manitoba (Zubek), and others (Wright & Myers, 1982). In 1982 George Ferguson noted how, "students who have lived a few years of their lives in the McGill milieu, and have absorbed its perspectives, perpetuate the McGill tradition in psychology" (Ferguson, 1982, p. 65). The McGill psychological tradition, though stretching back to among the first university departments of its kind in Canada (for reasons outlined in Chapter One) started with Hebb. Through collaboration and mentorship of countless students (most of which he supervised in the

1950s), Hebb helped shape what would become the fields of deep-brain stimulation, pain psychology, and clinical neuropsychology during his chairmanship of McGill in the 1950s.

At his core, Hebb was an integrationist, able to see the bigger picture and eschew the siloed divisions that were starting to take hold through structures of funding and institutional organization. Hebb's own experiences, having spent his formative years in the 1930s with Lashley, found him at odds with the psychological mainstream early in his career (a position he likely enjoyed). The suitability of psychologists for interdisciplinary work as equal contributors to the basic sciences was clearly important to Hebb (e.g., Hebb, 1951; Hebb, 1958). For him, interdisciplinarity was baked into the cake of scientific psychology. He shared frequently his dedication to 'building bridges' as the 'crossroads discipline' and his conviction that "it is unlikely that we shall solve the problems of behavior without the aid of the biochemist, the geneticist, the anatomist, and the physiologist" (Hebb, 1958, p. 466). This integrationist approach sought space for psychological knowledge in medicine; it did not challenge medical authority (as had professional psychologists interested in clinical issues), but introduced a commonality of purpose in the search for hidden influences or underlying causes (and psychological effects) of medical conditions (i.e., psychosomatic medicine). Moreover, the integrationist approach in psychology, which adopts the laboratory as setting for the creation of knowledge, shares with physiology the recognition of intellectual value in instances of specific disruption of a system.

Much of the research Hebb conducted in the 1950s was to better understand the outcome of various disturbances in cognition and behavior. The pathological approach common to experimental medicine was adopted by this neuropsychological understanding. The breakdown of (central) processes reveals the underlying mechanisms and structures at work; conceptualizations of differences between normal and the pathological processes become the basis of insight (Canguilhem, 1991). An understanding of the pathological condition demands a rigorous understanding of its normative processes. This created an intellectual arrangement for clinicians and experimental psychologists to create mutually beneficial 'trading zones' (Galison, 1997; Collins, Evans, & Gorman, 2007). Hebb aligned closely with these pursuits. He saw psychology's unique role (as well as his own) as a broker between the social sciences and the basic sciences, but never at home in either.

When it came to the nature of the emotions, Hebb was often vague. They were "certain neurophysiological states" (Hebb, 1946, p. 89), sometimes following a behavioral interpretation

as “the neural process that is inferred from and causes emotional behavior” (Hebb, 1949, p. 148), other times following a physiological description, as a “special state of arousal” always accompanied by and inseparable from various “mediating processes” (Hebb, 1958, p. 328). Throughout the 1950s he rarely distinguished between emotion and cognition and instead focused on the role of emotion as *disruption* of a neural system (1949). What can be said is that Hebb considered behavior and cognition, including the emotions, to be fundamentally embodied and rooted in neurophysiological processes tied indelibly to our experiences in the world. Slippery concepts of suffering, desire, and purpose, each specifically and physiologically influenced by “central processes” hinted at the underlying and general structure and organization of cognition and behavior. The isolation experiments Hebb conducted were critical to the development of theories and experiments that followed from them, such as those that would establish pain psychology in the postwar decades. These experiments are too often relegated to fairly isolated discussions of torture (e.g., McCoy, 2007), sensory deprivation (e.g., Raz, 2013b), or wrongly associated with psychiatrist Ewan Cameron’s activities at the Allan Memorial during this same period. Given a fuller theoretical and contextual account, connections to the rise of neuropsychological thinking (i.e., somaticization; Trief et al., 1987) and notions of ‘brainhood’ (Vidal, 2009) provide greater insight into the kind of behavioral science that has become characteristic of our times (e.g., Rose, 1988).

Reflecting broadly on the insights derived from more than a decade of reorganizing psychology at McGill, Hebb concluded his 1960 presidential address by taking account of one of the most subtle but important understandings that a neurophysiological approaches affords: “The self is neither mythical nor mystical, but a complex mental process. It can be manipulated and analyzed by the isolation procedure, and certain clinical phenomena will anatomize it for our inspection. It has a developmental course that is influenced by learning [...] It is not really remote and inaccessible in the laboratory, any more than in the clinic” (Hebb, 1960, p. 743). This statement underscores the shift in discourse that Hebb (and the activities of his department) achieved in the postwar period. While this address may not mark the beginning of a new field or discipline, it certainly represents the ascendancy of a formulation of thought over forty years in the making: the neuropsychological self.

The Neuropsychological Self

Joelle Abi-Rached and Nikolas Rose have argued that the ascendency of the Neuroscience Research Program at MIT in the 1960s marked the "birth of the neuromolecular gaze," an epistemic orientation or 'thought style' that has come to characterize the modern neurosciences more generally. According to Abi-Rached and Rose, "Although the neurosciences were characterized by their founders [Schmitt et al., in this case] as an integrated multilevel approach to the brain and the nervous system, it is the molecular underpinning that prevailed … marking both the success and challenges of these new sciences of the brain" (Abi-Rached & Rose, 2010, p. 13). Yet Hebb's approach remained largely aloof from the growing dominance of cognitive psychology developing in the United States around institutions like MIT and Yale. The department of psychology at McGill represents an alternative to the traditional American-centered narrative of the cognitive revolution and the later emergence of the neurosciences. Hebb's experiments and neuropsychological theories remained a far cry from the computational and cybernetic theories of American researchers such as George Miller and Herbert Simon (Kline, 1999). Yet his integrationist approach was a prerequisite for the cognitive reaction to behaviorism in the 1960s. Hebb splintered notions of intelligence and learned behaviors into their different component parts, the grist for the cognitive scientist's mill. In many ways Hebb's theories (e.g., neural networks) presaged the connectionist movement that emerged decades later (Medler, 1998).

Nikolas Rose (2003) uses the term "somatic individuality" to describe the tendency to define key aspects of one's individuality in bodily terms, to think of oneself as "embodied" and to understand that body in the language of contemporary biomedicine (p. 54). Hebb was a monist to be sure; and his neuropsychology (1949) espoused an early materialist version of somatic individuality (e.g., Hebb, 1951). But he also understood, as a proper comparative psychologist, that higher-order animals, such as humans (but also rats, cats, and dogs) are deeply affective beings shaped by biography and experience. Somaticization to Rose meant that what was once mapped onto a psychological space is now mapped upon the body itself, in the brain. For Hebb, the embrace of neurophysiology did not represent a wholesale turn towards the "neurochemical self," but contained within it an emphasis on the affective, bodily dimensions inherent to cognitive phenomena.

This thesis demonstrates how the seed for greater consideration of these aspects had already been sown (to various degrees) in institutions outside those of the dominant narrative (such as McGill). Postwar interest in "central processes" involved in perception and motivation, associated with Gestalt psychologists, "New Look" theorists, and purposive behaviorists, was also characteristic to the style of psychology Hebb embraced at McGill in the 1950s (though formulated in physiological terms). The historical ascendance of Hebb's experimental neuropsychology, the Canadian revolution in experimental psychology, (re-)created (indeed, demanded) space for the role of psychological expertise in understanding the affective dimensions of human and animal cognition (e.g., pain and reward). Hebb's research on restricted environments and emphasis of his department on emotional behavior suggests some reasons why Hebb remained detached from the events and conferences associated with the cognitive revolution and cybernetic movements. Given similar broad concerns (i.e., cognition), however, this thesis serves to shed light on seldom recognized connections between 1950s Canadian psychology and the American cognitive revolution (as well as the role of Canadian psychologists and the Montreal-milieu in the history of North American postwar psychology generally). Much work remains to be done in this area; I hope this project contributes to early efforts to chart these waters.

The twentieth century saw a proliferation of hybrid neuro-disciplines -- neurochemistry, cognitive neuropsychology, electroencephalography, pain medicine, and many others -- and these fields have assumed increasing importance in our technoscientific world. These hybrid disciplines were not inevitable, but rather the historical product of specific assemblies of men and women -- sometimes working together smoothly, sometimes messily, but always under unique historical conditions. The Canadian story of Hebb and McGill is important because it emphasizes the contextual and historical development of these disciplines; the path towards the "cognitive revolution" was never a straight line. Understanding the backgrounds, beliefs, and experiences of the men and women serve as a way of investigating the links between individuals, and how these links combine into historical phenomena that are truly greater than the sum of their parts. If we take seriously, as a generation of science scholarship has argued, that science is a profoundly human activity, then our histories of science must attempt to capture those elements that are quintessentially human, and that are revealed in the intersecting biographies of scientists and their institutions. Tracing these connections is a painstaking process, but I can think of no

more important task for the historian of psychology, and of no subject more worthy of investigation than the sciences that touch so closely on human self-knowledge.

The role of the environment in shaping who we are is often taken for granted. The importance of understanding the historical conditions around the psychological notions that frame these discussions is important for creating new possibilities. Moreover, Hebb's legacy as an innovative Canadian theorist cannot be thoroughly considered without an understanding of the historical conditions of the structures and institutions that made them possible. Hebb's influence on contemporary notions of the neuropsychological self has only begun to be explored. It is my hope that this thesis encourages others to include a broader range of actors and institutions in their histories of postwar behavioral science and to include Canadian styles of psychology in these considerations.

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