

# Abstracts (University of Calgary Conference)

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The center of the U of C Main Campus (photograph courtesy of the U of C University Archives)

**Thursday Morning, June 16, 2011**

**General History Opening Lecture:**

**Wild West – Mild West; Myth versus Reality on the Alberta Frontier**

Warren Elofson, University of Calgary, Canada (elofson@ucalgary.ca)

Much of my research in the last several years has focused on the cattle ranching industry in southern Alberta from the early 1870s to World War I. Its main purpose has been to offer serious resistance to views about frontier society that have gained currency among academics and been disseminated to the general reading public. Essentially there are two of these views. One is that the frontier environment itself made a comparatively minor impact on the society that participated in this Alberta's first agricultural industry. Largely concentrating on the so-called "great ranchers" who leased their enormous spreads at a favourable rate from their friends in the Conservative government of Sir John A. Macdonald (1815-1891), historians have told us that what developed in western Canada was a high culture transplanted essentially intact from the East and overseas. They have insisted that because of their Old World links and their wealth the early cattlemen formed an elite that did not have to adapt to the crude, unsophisticated conditions of most frontier environments. In recent times journalists have endorsed this picture along with the other one I have contested – and the one I want to address in this paper. It is encapsulated in a 1988 Calgary magazine article whose central argument is succinctly enunciated in its title – the "Tame West." This society the article declares, "was hardly the 'rootin tootin' rodeo that some would have us believe in." It was, the author insists, genteel and refined at the

top and law-abiding at every level. Historians have identified the basic British character that so many of the early ranchers from both eastern Canada and overseas carried with them as a major pillar in support of this interpretation. The tame image is also a reflection of a widespread belief that certain forces of law and order were at work in western Canada that were missing in most new societies, in particular those south of the forty-ninth parallel. The most important of these forces was of course the North-West Mounted Police. The myth of the Mounties who always got their man is ubiquitous. Consequently, Canadian cattlemen did not have to fight for possession of their land. Moreover, they had to worry very little about the depredations of would-be rustlers. The “single most important function” of the Mounties “after the maintenance of peace and order, was to prevent the killing and stealing of livestock [...] on no other frontier was the cattleman afforded such protection.” In combating this interpretation, I want to present a picture that gives the majority their proper place. Most people did not occupy the stately homes of the great ranchers nor hobnob with the rich and powerful. Many lived with other hired hands in bunkhouses or by themselves in crude shacks with earth floors. When life is seen through their eyes, as well as those of their social superiors, what emerges is something much more clearly moulded by the frontier environment and much less controlled, orderly, law-abiding and insulated from violence and the coarser side of life than has traditionally been suggested. Ultimately, it becomes evident that in Alberta as elsewhere the frontier environment not only determined to a considerable extent the day-to-day practices utilized by cattlemen to run, protect, and nurture their livestock, it also did much to fashion their entire way of life, or culture, in the broadest sense. The ranching community from Calgary south thus took on a flavour very much like that in Montana and Wyoming. If some Canadians at times showed due regard for authority, others behaved in astonishingly undisciplined and intemperate ways. And in the roughest and most raucous stage in our history our much-vaunted police force was often largely powerless to stop them.

#### **Neurology & Literature:**

### **1. A Stroke is the Best Death: Apoplectic Syndromes in Dramas and Novels (1600-2000)**

Axel Karenberg, University of Cologne, Germany (ajg02@uni-koeln.de)

In recent years, the portrayal of neurological disorders in fictional texts has been studied with increasing interest. Surprisingly, stroke has rarely been the key subject of these publications. Therefore, this presentation takes comprehensive inventory of and analyzes available literary texts which include apoplectic syndromes. The author has identified relevant novels and dramas by means of handsearching and by databases. Each text was carefully evaluated along the following lines: clinical phenomena & etiology, diagnosis & therapeutics, patient's perspective & social reactions, physician's role & medical institutions, and symbolism. Up to the 1940s, dramas (Shakespeare, Lope de Vega) and novels (Goethe, Dickens, Balzac, Dumas, Flaubert, Zola, Dostojewski, Tolstoi, Steinbeck) present more or less superficial depictions of apoplexy using “abbreviated” versions of the clinical syndrome. Yet they engage on a larger scale in speculations about the origin of the disease by mirroring pre-modern neurological concepts (Buechner, Hoffmann, Fontane). The patient's experience and institutions play only a minor role. After World War II the personal view of the afflicted, authentic descriptions of signs/symptoms as well as diagnostic/therapeutic options including hospitals/rehabilitation centers appear as major topics (Simenon, Lenz, Schmidt). In various epochs “narrative strokes” occasionally exert a prominent function within a novel or drama (initiation, turning point, or end of story). Moreover, a careful analysis can detect metaphoric functions of apoplectic syndromes in several novels (Gontscharow, Griesemer). In conclusion, fictional texts are never simple reproductions of clinical phenomena or summaries of textbook knowledge. It is the para- and meta-neurological elements that generate the interest of physicians and broader audiences. For that reason, historical and literary multi-center studies on the representation of stroke across centuries and languages should be started.

## **2. The History of Neurology as a Foundation in the Development of the Novel**

Heather Wilson, University of Calgary, Canada (wilsonhl@ucalgary.ca)

The scientific treatises of Thomas Willis reveals the interior mechanical workings of the human body and elaborate, technically, methodically, on how the bodily senses and mental faculties including memory, passions, imagination, and desire, have both a physiological basis and mechanical process. The anatomical and physiological interiority that Willis reveals allows for a comprehensive understanding of human passion and imagination which, in turn, allows for a new type of interiority in fictional character development, one scientifically grounded and thus more authoritative. The novel rises as a genre due to its comprehensive examination into the interiority of characters and grants perfect access to experiment in writing with entertaining various hypotheses and various expositions of psychology. Willis uses the term “psycheology” in reference to such inward examinations of the soul and its interdependence with its corporeal body. Passions influence the rate of blood flow from the brain back to the heart, a current which transports the animal spirits that activate the body. It is crucial that we are conscious of interiority in its embodied, anatomical, physiological sense to fully grasp this development of character interiority. Only through this physiological foundation do psychoanalytic discussions of character become inevitable. I propose that it is necessary to medicalize fictional authors’ explorations into interiority in the early eighteenth century to comprehensively appreciate what they achieve with this still budding genre of the novel. Willis anatomizes the emotions and where knowledge is generated, but scientists, like novelists, can also merely theorize on the psychological correlations. I will examine the language of some popular novels in the 1720’s with an eye to determining whether a social discourse dealing with the still relatively new understanding of the anatomical, physiological and mechanical interiority of the human body, stimulated by theories such as from Willis, influences the genre of the novel’s still relatively new development of character interiority. My supposition is that there is, in fact, a definable phenomenon with the interiority of characters that becomes the basis of the genre of the novel having an intellectual context that depends on the relatively recent knowledge granted to society by the theories and the language of these pivotal neurological, medical texts, and that the discourse both directly used in and stimulated from these medical texts become embodied in the language of the novel.

## **3. The Rivers/Head Experiment in Nerve Regeneration: A Cautionary Tale**

Roberta Jackson, University of Calgary, Canada (rjackson@ucalgary.ca)

My paper begins with a brief biographical overview emphasizing W.H.R. Rivers’ contributions to social anthropology, psychotherapy, neurology and experimental psychology. Following this general introduction, I move on to a detailed discussion of an autoexperiment on nerve regeneration performed with Sir Henry Head of Trinity College, Cambridge, who had studied experimental neuropsychology in Prague. A surgeon severed then rejoined the superficial cutaneous branch of the radial nerve in Head’s left arm, and for the following four and a half years, from April 1903 to December 1907, Head travelling regularly from London to Cambridge where Rivers would test and record his recovery of sensation in an atmosphere that was free from outside influences. Based on his observations, he and Rivers divided his recovery into two distinct phases: an early crude response they called “protopathic,” and a later more discriminative response they called “epicritic.” Although their findings could not be repeated by their contemporaries, (W. Trotter and E. G. Boring), the idea of a two-stage hierarchical nervous system informed neurological thinking for at least the next forty years. In their report of the experiment, published in 1908, Rivers and Head maintained “introspection could be made fruitful by the personal experience of a trained observer only” (323); that is, an experimenter trained to filter out his subjectivity from observations. However, they failed to account for the unquestioned influence of concepts in Head’s “well stocked mind [which was] not necessarily free from pre-conceived notions” (Breathnach 414). Jonathan Miller identified one of the notions populating Head’s mind as originating in Hughlings

Jackson's papers on the evolution and dissolution of the nervous system. Jackson proposed that injury to the nervous system of animal results in a gradual descent down the evolutionary tree. When an animal is damaged so that its higher functions are impaired, an evolutionarily older animal emerges. Therefore, Miller notes, the protopathic is active but held in check by the epicritic, "like a dog beneath the skin" (75). Although Jackson's work appeared forty years before Rivers and Head conducted their experiment, Miller states that his ideas were still very influential. In Jackson's work Miller sees evidence of the social unrest present at the time that he was writing; England had just passed through a period of turmoil during which riots and public disorder had unsettled intellectuals. Furthermore, it had not yet been 100 years since the French Revolution had demonstrated the destructive power of the mob. In the paper's closing section, I extend this line of analysis to show that still older influences are present in the works of both Jackson and Rivers and Head. Finally, I will examine the significance and consequences of the dualist assumption that Rivers and Head built into their experiment: the unquestioned belief that Head could be a purely detached observer of his own body.

#### **4. The Lady and the "Eel": How Aphra Behn's 1688 Novel about a Royal Slave Introduced the Masses to the "Numb Eel" of Surinam**

Stanley Finger, Washington University in St. Louis, USA (sfinger@wustl.ed)

Although the history of nerve and muscle electricity is often presented as starting with Galvani's Commentarius of 1791, a good (but narrower) case for animal electricity had been made a few decades earlier with three types of fishes: "torpedo" rays, electric catfishes, and the electric "eel". The ancient Greeks and Romans provided good descriptions of the rays that could numb or torpify at a distance. More than 2,000 years earlier, Egyptian artisans had drawn electric catfishes on the walls of some tombs, and written descriptions of their numbing effects can be found in Golden Age Arabic writings before Europeans described them early in the 17th century. Explorers and missionaries discovered the more powerful eel when they first began to settle South America, but it was not until the 17th century that reasonably good descriptions of it also began to circulate. Aphra Behn, a Royalist who wrote during the Restoration Period, is often hailed as the first great British female playwright and novelist. Behn seems to have visited then-British Surinam in 1663-64, although the exact dates and reason she was there have been debated. We know that she served as a British spy in the Netherlands soon after returning, during the Second Anglo-Dutch War (1665-67) that ended with the Dutch acquiring Surinam and the British taking New Amsterdam. Behn did not write her novel about what she saw in Surinam upon her return or while in Holland. *Oroonoko; or, The Royal Slave* was hastily written and then published in 1688, over two decades later. It went through numerous editions and became a runaway hit when adapted for the stage in 1695, shortly after Behn's death. *Oroonoko* is about the plight and tragedy of an educated Black African prince who is taken to Surinam. The novel ends tragically, with *Oroonoko* being dismembered during a slave uprising. Although overlooked by historians of science, *Oroonoko* provided a colorful and accurate description of the creature Linnaeus would later label *Gymnotus electricus*. Indeed, Behn's "stunning" verbal imagery about how the royal slave was overcome by a "Numb eel" while fishing the Surinam River did more than any earlier publication, including George Warren's 1667 book on Surinam (which she clearly consulted), to introduce this frightening fish with the capacity to deliver repeated shocks of about 700 volts to a wide audience. At this time, no one was postulating that a moist creature, much less one living in water, could be electrical. The eel so wonderfully described by Behn, and presented in its historical context in this presentation, would emerge as the star performer in the physiological drama that would take place in the second half of the 1700s. The fact that natural philosophers would discover that its discharge could be conveyed across conductors of electricity but not non-conductors, and that it could generate sparks, would go far towards making animal (or at least fish) electricity a reality prior to Galvani, whose own research and theorizing was very much influenced by what Behn and others had regarded as one of God's most unusual creatures.

## Emotions:

### 5. Empathy's Many Faces: Metaphors, Methodology, and the Natural History of an Intervening Variable

Laura Edwards, East Carolina University, USA (edwardsla@ecu.edu)

Arguably, it was with some hesitation that Titchener translated Theodore Lipps' use of the term *Einfuehlung*, using the English word 'empathy' (Titchener, 1909, p. 21). Almost immediately psychologists began commenting on the existence of different versions of empathy (e.g., Southard, 1918; Dymond, 1950; Buchheimer, 1963; Hunsdahl, 1967; Gladstein, 1984; Arnett and Nakagawa, 1983; Basch, 1983; Eisenberg, Fabes, and Murphy, 1997; Preston and DeWaal, 2002; Jahoda, 2005; Pederson, 2008; Vivona, 2009; Gerdes, Segal, and Leitz, 2010). Since the mid-1930's, empirical psychologists have investigated empathy primarily in the context of 'access to the minds of other people' but views of this construct still differ greatly in substantive ways. Although this grossly oversimplifies the differences, three dimensions along which empathy constructs continue to differ in substantive ways include the mechanism of access (e.g., direct or indirect perception), the nature of the content accessed (e.g., emotions, thoughts, or both) and the intentions of the empathizer (neutral or prosocial) (e.g., Gallese, Keysers, and Rizzolatti, 2004; Ickes, Gesn, and Graham, 2005; Singer, 2006; Batson, 2010). Recently, Davis (2002) asserted that the proliferation of empathy definitions "should tell you that trouble is brewing" and attributed the problem to "bad scholarship (Davis, 2002, p. 32)." Philosopher of aesthetics, Pinotti (2010), offered a more scathing commentary, "The history of empathy in Western culture covers a large spectrum whose extremes are marked by ancient Greek thought and contemporary science fiction (Pinotti, 2010, p. 93)." There is at best an inconsistency between psychologists' long-sustained empirical focus on empathy and the long-standing lack of convergence as to exactly what empathy is. I argue, however, that history rather than poor scholarship provides a better explanation for the continued co-existence of multiple empathy constructs. First, by the time empirical work on empathy as an interpersonal process began, empathy definitions had already become "concretized" (Barclay, 1997) based on at least three different metaphors: (1) Hume's mirror metaphor, 2000/1740; e.g., Montag, Gallinat, and Heinz (2008) (2) J.G. Herder's 2002/1784 tactile *Einfuehlung*; e.g., Forster (2002); Dymond-Cartwright (personal communication, 2010); Pinotti (personal communication, 2010), and (3) Vischer's 1994/1873 *Einfuehlung*; e.g., Johoda, (2005). The lack of convergence, however, is further clarified if viewed in the context of methodological disputes that accompanied operationism and the rise of Behaviorism in the 1930's and 1940's (e.g., Boring, et al., 1945). Operationism and the accompanying disparagement of hypothetical constructs allowed the persistence of multiple, operationally defined empathy constructs (Israel and Goldstein, 1944; McCorquodale and Meehl, 1948; Cronbach, 1955). A close examination of the empirical literature on empathy beginning in approximately 1940 indicates that researchers used the analytic method. As Israel and Goldstein (1944) predicted would happen for psychology in general, workers investigating empathy came to view operationally defined constructs as individually valid variables. Empathy researchers asserted what they believed was the correct definition of empathy and used it as an intervening variable in subsequent research. Because the analytic method is subtractive, as each version of empathy emerged from a different scholarly tradition, others simply classified it as 'not empathy' or 'empathy'. While various concretized definitions were sometimes combined, no hypothetical construct of empathy was ever proposed. Therefore, there was no theoretical definition and purpose against which to compare or refute various versions of empathy empirically. All definitions could be asserted and defended on historical precedence and operational definitions. This process effectively insulated all definitions from empirical refutation, allowing many alternatives to co-exist in the research literature. [The author wishes to thank Professor Cheryl Logan for her indispensable guidance in researching and writing this paper.]

## 6.

### **Hans Selye and the Conceptual Roots of Modern Stress**

Eric Oosenbrug, York University, Canada (eosen@yorku.ca)

An editorial preface in the *Annals of the New York Academy of Sciences* noted that, “stress fully pervades our life and influences us as individuals, communities, and humanity” (Chrousos et al., 1995). Indeed, the concept of stress has become so integral to our modern sense of self that its role in the relations between body, mind, and environment often goes unexamined (Kugelman, 1992; Pollock, 1998). Our contemporary notion of “stress” is commonly attributed to a number of critical laboratory observations made by a young Canadian endocrinologist, Hans Selye, throughout the mid-nineteenth century (Selye, 1946; 1950; 1952). Yet, despite its thoroughly embedded use in contemporary language and health discourse, the relatively recent history of this physiological and psychological construct has only begun to be explored (Hinkle, 1987; Viner, 1999; Harrington, 2008). This paper will explore how Selye’s “stress syndrome” brought together particular notions of the animal body, the human mind, and an industrial society that reflected new public concerns and anxieties about health and illness post-WWII. I aim to challenge the conventional narrative of Selye’s initial discovery consistent in nearly all of his popular works and historical accounts of stress research. Moreover, it will attempt to recast the nature of his original contributions to experimental medicine in the context of the challenges that mainstream medicine was facing in the early twentieth century, particularly in terms of disease specificity and mind-body interaction. Selye’s concept of stress was both controversial and strongly contested within expert physiological circles (Hinkle, 1976), yet the idea of the “stress syndrome” spread quickly and became accepted in public discourse regarding health and disease. If, as medical historian Russell Viner points out, our contemporary uses of the word “stress” bear little resemblance to the physiological and theoretical postulates on which it was originally based (Viner, 1999), what did Selye mean when he described the “modern man” as being “under stress” (Selye, 1956)? And why, despite a lifetime of academic and popular appeal, did Selye’s particular concept of stress fail to garner support from the scientific community? This paper addresses these questions by critically examining Selye’s canon on the concept of stress and tactics he employed while seeking to create an enduring subject newly perceived as medically perilous, but scientifically identifiable and controllable. I am particularly interested in Selye’s published works directed towards a lay audience. Through these works Selye attempts to clarify scientific concepts in reference to particular analogies, metaphors, and anecdotes that will be helpful in uncovering the basic assumptions underlying his theory of Stress. It is essential to my argument to show that the indubitably “hard” science of experimental physiology shares a vocabulary, and hence a world of evaluative meaning, with the surrounding culture. My research investigates the translation of the concept of stress across disciplinary boundaries, from animal physiology to human psychology, and examines Selye’s efforts to translate the concept of stress from laboratory experiments to the life narratives of modern bodies. This research constitutes a discourse analysis relying mainly upon primary material, in the form of documents and media, from the Hans Selye archive at the Université de Montréal. Through an examination of the events and actions that defined, negotiated, and challenged the early meaning of stress. I will show that Hans Selye’s lifework with the “stress syndrome” and his particular idea of a universal non-specific reaction brought together and constituted one of the most important sites of modern subjectivity: biologic stress.

## 7.

### **Ancient Evidence for Pain as an Emotional State**

Nicole Wilson, University of Calgary, Canada (nicole.j.wilson@ucalgary.ca)

While pain is undeniably a physiological sensation, expressions of physical pain resemble associated emotions such as grief, fear, anxiety and even disgust. Was this complexity understood by the ancient Greeks and Romans? The Greek and Latin languages provide insight. Greek has five main words for

pain: algos, lupe, odune, pathos, and ponos. Each, however, can be used to refer to both physical and mental pain. Latin has one main word for pain (dolor) which also can be translated as both ‘pain’ and ‘grief’. There is also the Latin word angor, which means ‘anxiety’, but also refers to the physical constriction of the throat, a symptom of anxiety. It seems that each language attached a corporeal association to pain which in turn produces an emotional resonance. This emotional complexity of pain reveals itself in the tragic theatre of Greece and Rome. When Sophocles’ Philoctetes (409 BCE) and Seneca’s Hercules (1st century CE) cry out in physical pain, the former because of a foot wound that has festered for ten years, the latter because his flesh burns from a poisoned cloak, they cry out not only because of their physical pain, but also because of other emotional factors. Philoctetes has been isolated in his suffering on the small, uninhabited Greek island of Lemnos for a decade, while Hercules mourns the loss of the physical body that defined him as the world’s greatest hero. Their cries are formulaic sounds in antiquity. Like the very words for pain, these meaningless exclamations can be used to express both physical pain and mental anguish. Images of the Trojan priest, Laocoön, and his sons being strangled by snakes along with representations of the flayed satyr Marsyas reveal a visual vocabulary for pain. Tense body muscles, a furrowed brow, and an open mouth all reveal physical pain, for both today and antiquity. Do these expressions have emotional correlates? Indeed, some of these elements are also part of the expression of emotions. For example, according to the Facial Action Coding System (FACS), a method developed by Drs. P. Ekman and W. Friesen to categorize the facial expressions of emotions, pain and fear expressions are closely related. Did the ancients, then, think of pain as an emotion? The evidence seems to suggest that this is close to the answer. The complexity of pain caused its expression inevitably to become confused with a variety of other common emotions. Perhaps it may be more accurate to say that pain for the ancients represented an emotional state, something that took in a variety of other affects.

## **8. Empathy and the Ancient Greek Physician**

Amber J. Porter, University of Calgary, Canada (aj.porter@ucalgary.ca)

Empathy is an important issue in the study of neurology, and in the past few years has extended to such applied areas as modern health care, where there has been a great deal of research conducted on the empathy of physicians, nurses, dentists and other health care providers (Hojat 2002). Empathy may be considered an important quality today, but did ancient psychology and medicine display and value this emotion? The Greeks and Romans are not usually considered particularly empathetic or sympathetic, but did Greek scientists display no interest in empathy and sympathy as well? And how do we determine whether a scientific writer is displaying these emotions in his text? This paper will examine the nature of empathy and whether it played a role in the surviving medical literature of the second century physician, Aretaeus of Cappadocia.

### **Neurophilosophy & Neuroethics:**

## **9. Moral Problems with Moral Treatment: Historical Reflections on the Role of Ethics in Psychotherapy [this paper will be read]**

Louis C. Charland, University of Western Ontario, Canada (charland@uwo.ca)

One of the pivotal factors in the establishment of psychiatry as a medical science was the introduction of moral treatment. This novel form of therapeutic intervention was aimed primarily at ‘moral’ rather than ‘physical’ aspects of the patient’s condition. ‘Moral’ factors were often understood to be psycho-social in nature. But they were also often associated with ethics and morality. In the terminological and conceptual quagmire that ensued, the term ‘moral’ became highly ambiguous. Medical writers tended to endorse the more neutral, psychological, sense of ‘moral’. Yet they also often relied heavily on philosophical influences that stressed the importance of ‘moral’ matters in an ethical sense. This led to

ambiguities and paradoxes as psychiatry sought to establish its status scientifically.

**10. John Eccles, Karl Popper and the Dunedin Experience; an Important Relationship in the History of Neuroscience**

Catherine E Storey, University of Sydney, Australia (cestorey@bigpond.com)

John Carew Eccles (1903-1997), a graduate of Melbourne University was awarded the Nobel Prize for Medicine or Physiology in 1963 for his work on synaptic transmission. Eccles would later reminisce that he had been “a wanderer over the world for more than 50 years of active scientific life” and of how fortunate he had been to have had expert technical assistance and engineering support in all of his “five ports of call” following his period with Sherrington in Oxford (Eccles 1977). This was indeed the case in his second “port of call”, when, between the years 1944 and 1951, he held the chair of Physiology at the University of Otago, Dunedin, New Zealand. It was here in Dunedin, in 1945, that Eccles first met K.R. Popper (later Sir Karl) who had himself immigrated to New Zealand in 1937. Popper’s philosophy would have a very important influence on Eccles scientific method; while their friendship remained life long, and culminated in their collaborative work, *The Self and Its Brain* published in 1977 (Popper and Eccles 1977). Eccles had originally held firm to his belief that synaptic transmission was electrical, at variance with his colleague Henry Dale who hypothesized a chemical transmission and with whom he frequently debated this disparity. Under the influence of Popper, Eccles reformulated his electrical hypothesis in order to “to invite falsification”. In a definitive experiment in 1951, the results demonstrated that his electrical transmission theory was false and discarded (Brock, Coombs et al. 1952). Eccles was an immediate convert to a chemical theory. This period of time spent in Dunedin was undoubtedly very important in the scientific life of Eccles. The experiments which led to his reformulation of his ideas on synaptic transmission and his acceptance of the overturning of his long-held theory were the direct result of his fortuitous meeting with Popper, who had an enormous influence on Eccles’ scientific method for the remainder of his career.

**11. Pragmatism as a Psychological Standpoint: In Search of Good Ideas**

Dane Burns and Henderikus Stam, University of Calgary, Canada (dtburns@ucalgary.ca)

John Dewey is traditionally considered to have ended his career as a psychologist in 1904 when he left the University of Chicago (e.g., Hergenhahn, 2009). In traditional histories his career at Columbia University, if mentioned at all, is said to have focused primarily on philosophical and/or educational issues. This estranged relationship to psychology is corroborated by Dewey’s own reflections about the discipline of ‘psychology’ as he feared it was establishing itself as an overly ‘scientific’ enterprise (Dewey, 1950). However, this view obscures Dewey’s continued reliance on ‘psychology’ in his developing theorizing and the fact that Dewey wrote about and taught psychological issues well after this period (e.g.; *How We Think*; *Human Nature and Conduct*; *Individualism Old and New*). John Dewey’s early work argued for a holistic conception of human experience that he called the “psychological standpoint” (Dewey, 1886). He argued that a proper conception of human experience in relation to the world is needed to have a productive and ethical science of human life and should be the foundation for philosophical investigations. In this early period Dewey used a hybrid of the ‘new psychology,’ based on physiological experimentation, mixed with a type of Hegelian philosophy through the work of Thomas Hill Green (see, *Psychology*, 1887). As this approach began to wane (1892), with the development of a ‘functional’ approach in his often celebrated *Reflex Arc* paper (1896), elements of his pragmatic philosophy started to take shape (1900). Dewey’s pragmatic philosophy should be seen as an outgrowth of this early period and tied to a fundamental conception of human experience or a ‘psychology standpoint.’ Finding both the language of experimental physiology (sensationalism) and Hegel (intellectualism) lacking for their reliance on absolute formations of human experience which result in a

dichotomy between individual experience and the social reality that makes up and supports this experience, Dewey's psychology began to emphasize the individual as necessarily tied to social or political relations in an ongoing process of moral readjustment (1899). In this position 'functional,' that is, teleological or purposeful conceptions of human experience he argued are needed in a developing democratic world where the goal of individual activity is to enlarge human experience. This necessitated for Dewey the development of a system of reflective intelligence (i.e. pragmatism, 1908) that would provide people with the means to morally and actively adjust to a complex, changing, democratic world according to the needs of the moment. The pragmatic method seen in this light was developed as a tool for people to live intelligently and justly with one another in an evolving interrelated world where purely static conceptions of experience are impotent for addressing ongoing human problems.

#### **Featured Lecture I:**

##### **Reading between the Lines: An Exegesis of an 18th Century Text**

Harry Whitaker, Northern Michigan University, Canada (hwhitake@nmu.edu)  
and Ola Selnes, Johns Hopkins University, USA (oselnes@jhmi.edu)

Harris (1999) pointed out that, although one might take Prochaska, Swedenborg or even Willis, to be localizationists, they were not faculty localizationists as was Gall; progenitors of faculty psychology, such as Reid and Stewart, had little to say about the brain. Luzzatti; Whitaker (1996) noted that lateralization of language to the left hemisphere was easily deducible from Wepfer's data (17th century) but it was unclear that anyone in day-to-day medical practice had drawn that conclusion. In 1792 an R. Leny published a clinical report, "A Remarkable Case of a Boy, who lost a considerable portion of brain, and who recovered, without detriment to any faculty, mental or corporeal". This paper could be read as a surprising pre-antiseptic medical accomplishment along the lines of the Phineas Gage story, or as evidence that there was some 18th century medical knowledge that functions of the brain are localized, possibly lateralized. Our text analysis suggests that doctors at that time may have known some of the facts that later evolved into principles of localization and lateralization of function. This is supported by a 1793 essayist who wrote that Leny's report was "in no respect singular or new" and then discussed one of several cases of extensive brain damage followed by excellent recovery compared to other cases of brain damage accompanied by severe and persistent impairments.

**Thursday Afternoon, June 16, 2011**

#### **Conference Symposium I:**

##### **Crossing Boundaries: Faces in Neuroscience and Art**

Guel A. Russell, College Station, Texas, USA (russell@medicine.tamhsc.edu)  
and Nick Wade, University of Dundee, Scotland, UK (n.j.wade@dundee.ac.uk)

Art is usually studied by art critics and art historians. To view artistic creations from the perspective of what happens in the brain, or whether there is an 'aesthetic of the brain' is a fairly recent phenomenon. However, vision has been the sense of neuroscience for centuries, initially in observing the gross structures of the brain and nervous system and representing them graphically. Later, instruments were invented that extended its range so that microscopic details could be seen, cells could be stained and nerve impulses could be amplified. In the last decades vision has been replaced by visualization. Modern brain imaging techniques require visual mediation to render their results accessible thereby rolling back our ignorance of brain function.

Thus, historically neuroscience and art have always crossed boundaries in visual imaging, and continue to do so in numerous ways. On the one hand, artists have intuitively applied and demonstrated perceptual principles in their works that have only subsequently been provided with explanations by scientific research. On the other hand, scientists have also used and continue to investigate artistic creations (i.e. illusions) to explore the principles and assumptions underlying visual processing by the brain. As Purkinje said, visual illusions reveal visual truths. Neuroscientists have found that the study of art can inform a neurobiological understanding of the visual system. At times there are direct influences of science on art, where artists have deliberately applied scientific principles (e.g., visual optics, physics of light, theories of colour, cognitive neuroscience) as they understood them, incorporating them into theoretical manifestoes for new techniques in painting (e.g., Renaissance, Impressionists and Post impressionists). In this seminar we hope to explore the nature of some of these ‘crossings’ within an historical perspective, and the principal focus is the face. We consider ‘crossing boundaries’ as more representative of the mutual interaction than influence. There will be four presentations and six participants. 1. “Brain Research and Art? Some Considerations on the Relationship of the Practices of Natural Science and Experimentation in the Modern Neurosciences” by Theresa Bruncke (University of Heidelberg, Germany) & Frank W. Stahnisch (University of Calgary, Canada); 2. “Caricatures and Neurology” by Lorenzo Lorusso (Neurology Dept, “Mellino Mellino” Hospital, Chiari (Brescia), Italy and Marjorie Lorch (Birkbeck, University of London, UK); 3. “Neuroscience in Art: Theoretical or Empirical?” by Guel A. Russell (Department of Humanities in Medicine, College Station, Texas, USA); 4. “Portraits of Neuroscientists” by Nick Wade (University of Dundee, Dundee, Scotland, UK).

## **12. Brain Research and Art? – Some Considerations on the Relationship of the Practices of Natural Science and Experimentation in the Modern Neurosciences Boundaries**

Theresa Bruncke, University of Heidelberg, Germany (Bruncke@stud.uni-heidelberg.de)  
and Frank W. Stahnisch, University of Calgary, Canada (oselnes@jhmi.edu)

The sub-discipline of “neuroaesthetics” has quite recently emerged in the spectrum of other *neuro-* related sub-disciplines in the neurosciences, such as neuro-ethics, neuro-history, neuro-economics etc. (Skov and Oshin 2009). Yet apart from being an eye-catching term and from “neuroaesthetics” being used for carving out funding niches within the interdisciplinary field of modern neuroscience, there are some interesting aspects at stake, when the aesthetical, practical and methodological foundations of neuroscientific creativity and imagery are taken into account. Regarding the epistemological question of “scientific creativity”, the relationship between the notion of the “life sciences” and the multiple elements of the research practices and experimental methodologies are still much under-represented and under-researched. Some historiographical case studies on the second half of the 19th and the 20th century, nevertheless, have investigated the non-discursive practices of neurophysiology and neuroanatomy and shown that these progressed from a primarily collecting and comparative activity to a predominantly manufacture-oriented and finally industrial endeavour, which was importantly based on labour division in neuroscientific research (Dierig and Schmidgen 2001). The artistic presentations, aesthetical criteria of their selection and the interrelation of textual structures and the visual products have only recently become the subject of considerable historiographical research. While this tendency began in areas of art history and the media sciences, the historiography of neuroscience has now participated in these new analytical perspectives; the “iconic turn” (Nikolow and Bluma 2002) and the investigation of so-called “visual cultures” (Heintz and Huber 2001) moved in the direction towards of more in-depth analyses of the media products of the life sciences. This methodologically oriented paper will review some of the recent advances in neuroaesthetics research literature, analyze particularly their impact on case studies in

laboratory history as well as on the understanding of the emergence of historical imaging practices in the neurosciences. It thus draws both on recent scholarly work as well as on a research project at the University of Calgary (Stahnisch 2010), which examines the interdisciplinary logics, epistemic approaches, and forms of experimentation that are used in modern functional MRI imaging techniques.

### **13. Caricatures and Neurology**

Lorenzo Lorusso, Mellino Hospital Brescia, Italy (walton2002@libero.it) and Marjorie Lorch, Birkbeck, University of London, UK (m.lorch@bbk.ac.uk)

Caricatures and satirical cartoons have a long history of use in art and neuroscience. A caricature is a pictorial representation which by means of distortion holds up a person or a thing to derision. The word caricature comes from the Italian *caricare*, which means to overload or to exaggerate (the concept of ridicule only became linked to caricature in the Renaissance). By considering such graphic representations of individuals, institutions and innovations one can illustrate the main steps in the historical development of the neurosciences. Examples can be drawn from throughout Europe in the sculpture, painting and graphic prints of ancient Greece and Rome, the Middle Ages, the Renaissance, and succeeding centuries to the present. Realistic representations of neurological conditions such as peripheral facial palsy are found on ancient Greek and Roman statuettes and vessel in depictions of scenes from life and literature. During the Middle Ages, art was more abstract and symbolic rather than representational. Unnatural representations are found in the Grotesque reliefs and sculptures of medieval cathedrals, and “drolleries” decorated the margins of Gothic prayer books. Neurological disorders interpreted from a religious or mystical point of view were used to enforce moral lessons, to portray sinners and the Dance of Death. From the 1400s onwards, representations in European painting and sculpture were underpinned by a deeper scientific understanding of both anatomy and perspective. Distortion became the distinguishing mark of caricature. The development of caricature for the purpose of ridicule was set in motion by the breakdown of the authoritarian social structure of medieval society and subsequent emergence of the individual as a free agent. The Renaissance was, for all its progress, a period of inconsistency in medicine. Caricatures often depict the doctor as a fool, but a scientific and aesthetic approach to portraying neurological, and mental states and expressions was also carried out by such Italian artists as Leonardo and Michelangelo. During the 16th century, allegories became a favourite means of expression in arts. The Northern European illustrators such as Pieter Breughel painted allegories filled with representations of life in the folklore tradition. This approach rejected the Renaissance aesthetic ideal of the classical body. These canvases were filled with depictions of people with medical disabilities. Medical treatments such as cauterization as a cure for epilepsy and cranial operations were also represented. In the 17th and 18th centuries in France and in England caricaturists included representations of medical practices in their satires of the degradations of society. Physicians were portrayed as an oligarchy with control over the “lower orders” of practitioners. The medical feuds, jealousies and disputes that raged in the press and the law courts were documented by caricatures. The charged emotional atmosphere was represented by facial distortion for comic effect (the basis for the art of “Grimaciers”). This focus on the representation of facial expression led to more systematic consideration of physiognomy, the developing understanding of the anatomy of expression which were linked to phrenological concepts of the mental faculties. In the 19th century, the new clinical-pathological exploration of the nervous system saw the employment of visual art incorporated into medical practice. One example is Jean-Martin Charcot who recorded essential features of neurological diseases in sketches and caricatures and commissioned others.

### **14. Neuroscience in Art: Theoretical or Empirical?**

Guel A. Russell, College Station, Texas, USA (russell@medicine.tamhsc.edu)

Some of the leading neuroscientists have recently been turning to paintings to argue for a two-way relationship between art and visual science. There are periods in history of art associated with the influence of science on art. To consider paintings outside such periods may give us insight as to the nature of any possible causal relationships or creative impetus of science on artists, for example, in portraying faces. In the Renaissance, the use of visual optics and point-to-point correspondence in image formation by artists paved the way for the creation of ‘likenesses’ on a flat surface, culminating in ‘trompe l’oeil’ effects to trick the ‘brain’. In the nineteenth century, the artists increasingly abandoned the established veridical representation from a fixed point, and turned to capturing ‘impressions’ of light, and the subjective impact of the object on the viewer in a series of innovative movements (impressionism, post impressionism, cubism, surrealism, expressionism). The post impressionists, specifically the ‘pointillists’, constructed ‘form’ /shapes of objects from juxtaposed discrete points or dots of color that were visually integrated by the brain of the viewer unlike a normal canvas where there is a continual range and gradual transition of color. This remarkable paradigm shift in paintings has been largely attributed to the interaction of art with science, in particular to the developments in the physics of light, chemistry (and photography), for which there are reasonable grounds. Such interactions, however, are highly complex. There are much earlier works of art that have achieved visual effects, or used techniques prior yet similar to those of the subsequent Renaissance or the nineteenth-century innovations. Their existence indicates that visual perception in art is by no means the result of an inevitable dependence on developments in visual science, or reflects a parallel chronological evolution. Three representative examples dealing with portrayal of faces will be analysed that are widely separated in time: (a) the individualized mummy portraits of the dead (2nd century, Fayum, Egypt); (b) the dramatic Roman mosaic portraits which construct the image of a naturalistic face by the proximity of separate and differently colored small bits of stone that characterize the ‘pointillist’ paintings; (c) the sixteenth-century portraits by Guiseppe Archimboldo, that create faces from individual elements that bear no relationship to the features of the face, yet the configuration is clearly recognizable as a face, using techniques that centuries later emerged with the surrealists. What is achieved in these examples also raise questions for ‘face recognition: how much or how little information is required to recognize a face or needed for its visual likeness, and what elements are most important. In fact, these became an on-going major concern in neuroscience since the 1970s with the experimental research of Julesz and Harmon on how we recognize and respond to faces. To conclude: Outside the sphere of / in the absence of direct influence of science on art, the question remains to be resolved: Have the artists been aware of the perceptual mechanisms involved (that neuroscientists subsequently identify, and explain), or have they been simply applying empirically determined techniques?

## **15. Portraits of Neuroscientists**

Nick Wade, University of Dundee, Scotland, UK (n.j.wade@dundee.ac.uk)

Neuroscience has always been associated with the arts because the results of investigations are presented visually. Illustrations of the nervous system abound from the origins of modern neuroscience and many of them are beautiful. The same cannot always be said of pictorial portrayals of the neuroscientists themselves. Portraits are used increasingly in histories of neuroscience, and in textbooks generally. Why are we so interested in the portraits of people long dead who are known for their thoughts rather than their appearance? When we do recognize a portrait it is only by association with others we have seen of that individual. There is rarely evidence concerning the accuracy of resemblance of a particular portrait, and in some cases there may be very few portraits from which to select. The neuroscientists are usually portrayed when they were old and established, whereas their important work was mostly carried out when they were young. Moreover, conventional portraits rarely speak to the advances made by those portrayed and attempts to remedy this are made with ‘perceptual portraits’, which represent neuroscientists in an unconventional style. The portraits themselves are not always easy to discern – the viewer needs to apply the power of perception in order to extract the facial

features from the design which carries them. The aim of perceptual portraits is both artistic and historical. They generally consist of two elements – the portrait and some appropriate motif. The nature of the latter depends upon the endeavours for which the portrayed person was known. In some cases the motif is drawn specifically to display a phenomenon associated with the individual, in others it is derived from a figure or text in one of their books. The illustrations often require some effort on the part of the viewer to discern the faces embedded in them. It is suggested that such perceptual portraits both attract attention and engage the spectator's interest to a greater degree than do conventional paintings, prints or photographs. This visual intrigue will enhance the viewer's desire to discover why particular motifs have been adopted, and in turn to learn more about the persons portrayed: it is intended to be an instance of crossing the boundaries between neuroscience and art.

#### **Disease and Therapy:**

### **16. Non-drug Treatments for Migraine in the 19th and 20th Century**

Peter J Koehler, Atrium Medical Center, Netherlands (pkoehler@neurohistory.nl)

Several new invasive procedures for the treatment of drug-resistant migraine and trigeminal autonomic cephalalgias have evolved during the past decades, including occipital nerve stimulation, deep brain stimulation and transcranial magnetic stimulation. The application of invasive procedures for this indication is not new. In this review the history of non-drug treatments for migraine is discussed. Well-known books by physicians known to have written on headache and migraine (hemisrania), in the 19th century and mainstream 20th century neurology handbooks were analyzed. After its introduction in the mid-18th century, medical electricity became even more popular for the treatment of migraine following the discovery of vasomotor nerves in the mid-19th century, but at the end of that century, more critical sounds were heard. The discovery of the lumbar puncture (1892), roentgenogram (1895) and increased knowledge of intracranial pressure lead to a new series of invasive procedures for therapy-resistant migraine in the early 20th century. Vasospastic theories of migraine resulted in surgical procedures applied to sympathetic nerves. Following research by Graham and Wolff (1930s), emphasizing the vasodilatation concept of migraine, sympatholytic procedures again became popular, but now vessel ligation of the carotid and middle meningeal arteries were advocated. The influence of suggestion and psychological phenomena recognized by Paul Moebius at the end of the 19th century probably played an important role in many of the treatments that have been applied. Placebo effects, generally more powerful in invasive procedures, are discussed against the background of modern invasive treatments for migraine and other primary headaches, where they still are a matter of concern.

### **17. Historical Aspects of Geography, Latitude and Multiple Sclerosis**

Bijal K. Mehta (bijal\_mehta@hotmail.com); Bianca Weinstock-Guttman (bw8@buffalo.edu) and Edward J. Fine, University at Buffalo, USA (efine "at" buffalo.edu)

The hypothesis and observations associated with geographic variability in multiple sclerosis prevalence is only about 50 years old. This proposal has become re-popularized recently due to the emergence of vitamin D being associated with its prevalence. Geographic prevalence of multiple sclerosis was first proposed in the 1950's previously in relation to increased prevalence in northern Europeans. Acheson was the first to propose this concept in the contemporary medical literature in the 1960's as possibly a world-wide phenomenon. He defined high risk regions as having a MS prevalence of 40/100,000 and such areas did not exist between 40 degrees north and 40 degrees south latitudes. Over the next few decades a country by country review of prevalence and incidence of multiple sclerosis began to confirm this initial notion. A similar phenomenon was noted in the southern hemisphere. As more individual country data regarding MS prevalence presented itself, the pattern of geographic prevalence began to emerge. Explanations for the geographic variation of MS prevalence included variations in co-morbid

diseases, bacterial and viral infections, variations in hygiene, differences in nutrition, and variations in UV/sunlight exposure. The individuals involved in these theories are discussed. Early on, these explanations emphasized geography instead of latitude, as a reason for the prevalence variation. This was a natural extension of the epidemiologic phenomenon of disease pockets and outbreaks. Resistance to this theory, and those who put forth it, is reviewed. However, the sustained prevalence rather than momentary increases in the incidence began to question this mind set. Although, genetic segregation may be the reason for continued generational increases in prevalence, in the “modern world” with easy migration within 24 hours from latitude to latitude, put this concept in doubt. Kurtzke demonstrated that individuals emigrating towards the equator prior to puberty had decreased prevalence of MS in those who migrated versus cohorts in their higher latitude country of origin. A few exceptions to the rule and explanations associated with these exceptions appeared, but confirmation of the latitudinal gradient began to emerge. More recently, in the last decade, the idea of latitude playing a larger role was investigated further. Solar and UV radiation differences were sought as the cause. Although, sunlight and UV radiation variations are still being worked out as mechanistically involved in MS, vitamin D has caught the interest of scientists, physicians, and the public at large. The associations that UV radiation being required for adequate vitamin D levels in individuals plays well with the idea of latitudinal variation of MS. Higher UV indices are noted closer to the equator and therefore, higher levels of vitamin D are predicted in these populations. Vitamin D was initially only noted to have effects on calcium metabolism, but research in the last decade, has shown immunosuppressive effects of vitamin D in animal models of MS and MS patients. Ascherio and Munger have demonstrated in military personnel and nurses studies that vitamin D levels correlated with MS risk. However, vitamin D is not the cure for MS. This may be secondary to MS being multifactorial or that other solar mediated effects play a role. In conclusion, a review of the various directions of MS, geographical variations in prevalence, and theories associated, are reviewed in the context of future directions in this area of medical geography research.

#### **18. Malaria-Fever Therapy for a 19-Year-Old Boy with Dementia Paralytica**

I.M. Lens-Daey Ouwens and W.M.A. Verhoeven, Vincent van Gogh Institute for Psychiatry, The Netherlands); A. Ott, Groningen University, The Netherlands and P.J. Koehler, Erasmus University Medical Centre, The Netherlands; (ILens@vvgi.nl)

In 1938, a 19-year-old boy with a history of behavioral problems and incontinence presented at the Van Gogh Institute for Psychiatry with cognitive deterioration. A diagnosis of juvenile dementia paralytica was made. Dementia paralytica is a sign of neurosyphilis resulting from infection with *Treponema pallidum*. The patient was treated with malaria-fever therapy. The idea that fever may have a curative effect on mental diseases goes back to Antiquity. Hippocrates mentioned the beneficial effect of malaria infection on epilepsy. And Galen cited a case of melancholia cured after an attack of quartan fever. In the pre-antibiotic era, most dementia paralytica patients were destined to die a wretched, lingering death. In 1917 the Viennese Professor Wagner-Jauregg started to treat these patients with malaria induced fever. Within a few days after infection his patients developed high fevers lasting five or six hours, returning to normal about 48 hours later. Wagner-Jauregg allowed his patients to go through this two-day cycle three or four times, and then used quinine to treat the malaria. He reported clinical success in six of the first nine patients he treated. Malaria-fever was used to treat neurosyphilis throughout Europe and the United States until penicillin became available in the early 1950s. Generally, the success rate was about 30 per cent full remission and 20 per cent partial remission. Wagner-Jauregg always insisted on controlled trials, which he termed “*Simultanmethode*”, even though the concept of controlled clinical trials had not yet become a standard of experimental therapeutics. But still it is difficult to determine, whether malaria therapy was effective, because it was reserved for less seriously ill patients and because neurosyphilis typically varies in severity over time. Braslow has described how malaria fever-therapy, although not necessarily effective, gave physicians and patients a sense of hope, and also encouraged more positive and optimistic general clinical care of patients with dementia paralytica. Efforts to synthesize quinine,

necessary to treat malaria, led to the development of several important drugs, including the first effective antipsychotic-antimanic agents and to chlorpromazine in particular. Following malaria-therapy, the patient mentioned in the introduction showed some improvement of his clinical condition, but he remained hospitalized for the rest of his life.

### **19. When the Mind Leaves the Body: Contribution of Bryan Jennett and Fred Plum to the Therapy of the Comatose Brain Injuries and Diseases of Consciousness**

Yuri Zagvazdin and Kevin Tu, Nova Southeastern University, USA (yuri@nova.edu)

“Diagnosis of Stupor and Coma”, a seminal book written by the American neurologist Fred Plum (1924-2010) with Jerome Posner, and a simple scale for grading the level of consciousness, introduced by neurosurgeon Bryan Jennett (1926-2008) with Graham Teasdale from Glasgow (Teasdale and Jennett, 1974), became important milestones that shaped modern clinical examination and management of patients with severe brain injuries (Koehler and Wijdicks, 2008). The development of intensive care units triggered by the remarkably successful introduction of positive pressure ventilators to treat the poliomyelitis outbreak in Copenhagen in 1952, greatly increased survival of individuals with trauma, stroke and other diseases caused by brain damage (Jennett, 1990; Rushman, Davies, and Atkinson, 1996; Ibsen, 1999). Determining the course of treatment and prognosis in many cases was not an easy task. In the early 70’s, Jennett and Plum with a number of collaborators from Great Britain, United States and The Netherlands embarked on a project aimed to establish standardized quantitative means of assessment of patients in traumatic and non-traumatic coma (Levy, Knill-Jones and Plum, 1978). The study confirmed the usefulness of the Glasgow Coma Scale which became the most widely applied instrument for monitoring the clinical outcome in these patients. In 1972, the transatlantic partners coauthored a paper titled “Persistent Vegetative State after Brain Damage: a Syndrome in Search of a Name” (Jennett and Plum, 1972). This article clearly demonstrated the need for a distinct category of coma survivors who never regain recognizable mental functions. “Clinical and pathological reports about such cases are beginning to accumulate”, wrote Jennett and Plum, “whilst the ethical, moral, and social issues are provoking comment both in the health professions and in the community at large”. Indeed, following this publication, new and difficult ethical questions emerged as the value of life support after the loss of brain functions and consciousness were undergoing reassessment. Jennett and Plum passionately advanced the argument amongst the general public that ability of health professionals to prolong treatment in the intensive care units for some patients may not always be a blessing. Should life of the body be preserved at all cost regardless of its quality? In 1976, consciousness was recognized as the critical element of human life in the United States. That year the irreversible loss of cognition and awareness was accepted as the ethical and legal justification for the shutdown of the machine that ventilated Karen Ann Quinlan, a young woman in a permanent vegetable state. Several legal battles for the removal of life support from individuals incapable of recovering their consciousness followed and provoked continuing discussions in the United States, Great Britain and other countries (Jennett, 2002). The proactive stance of Jennett and Plum and their testimonies as expert witnesses in a number of such cases affirmed the right of patients to live and die with dignity.

#### **Physiology and Brain Function:**

### **20. The Newton - von Gudden Law and Neural Mechanisms of Interhemispheric Integration**

Ian Steele-Russell, College, Station, Texas, USA (Irussell@medicine.tamhsc.edu)

A conspicuous feature of the mammalian brain is its duplex anatomy. It consists of two separate hemispheres that are solely united by the various intercerebral commissures or decussations. The principal function of these commissures, on a priori grounds, is to functionally unify the separate hemispheres. Experimental evidence of this conjecture is commonly believed to derive from the work of

Myers & Sperry (1953). They demonstrated that sensory information was shared by both hemispheres using by information transfer via the optic chiasma to both hemispheres, as well as by transfer via the corpus callosum. In an earlier communication (Steele-Russell & Kocurek, 2010) to this society it was shown that the first decisive experimental demonstrations of interhemispheric information transfer in dogs by the corpus callosum was provided by the Exner laboratory in 1903. The purpose of the present paper is to argue that Newton provided a theoretical basis and von Gudden the experimental evidence for the role of the optic chiasma in interhemispheric integration of visual information even earlier in cats after section of the optic chiasma and corpus callosum.

## **21. Cardiocentric Neurophysiology: The Persistence of a Delusion**

C.U.M. (Chris) Smith, Aston University, UK (c.u.m.smith@aston.ac.uk)

The earliest ideas of human nature in Greek antiquity included a disjunction between an ‘emotional’ soul located in the torso, in the region of the diaphragm, denoted by the term thymos (Latin, fumus, Sanskrit, dhumas) and a more intellectual soul, denoted by psyche, associated with the breath. A development of this disjunction is to be found in Plato’s Pythagorean dialogue, the Timaeus, where the immortal soul is located in the brain and a warrior soul in the torso. Plato’s star pupil, Aristotle, is well-known to have disagreed with his master and to have relegated the brain to act as a mere coolant apparatus for overheated blood and to have located the hegemonikon, the centre of the body’s psychophysiology, in the heart. This relegation was hotly disputed by his immediate successors, Herophilus and Erasistratus, in the Alexandrian Museum, who showed that the brain played the central role in psychophysiology, and this was accepted and developed by the last great biomedical figure of classical antiquity – Claudius Galen. However, Aristotle’s cardiocentric theory did not entirely disappear and this paper traces its influence through the Arabic physicians of the Islamic ‘golden age’, especially Avicenna, into the European Middle Ages where Albertus Magnus’ attempt to reconcile cardiocentric and cerebrocentric physiology was particularly influential. It shows how cardiocentricity proved to be highly tenacious and was sufficiently accepted to attract the attention of, and require refutation by, many of the great names of the Renaissance, including Jean Fernel and René Descartes and was still taken seriously by luminaries such as William Harvey in the mid-seventeenth century. This paper, in rehearsing this history, shows the difficulty of separating the first-person perspective of introspective psychology and the third-person perspective of natural science. It also outlines an interesting case of conflict between philosophy and physiology.

## **22. Mihály Lenhossék (1863-1937), the Neuronal Growth Cone, and International Competition in Modern Neuroscience**

Andrew G.M. Bulloch, U of C, Canada (bulloch@ucalgary.ca)  
and FW Stahnisch, U of C, Canada (fwstahni@ucalgary.ca)

The Hungarian neurohistologist Mihály Lenhossék figures as a rather marginal pioneer in late 19th and early 20th-century neurology. In fact, he made major contributions to the development and morphology of the nerve cell, the growth patterns and regeneration processes of nerve fibres, the structure of the neurocranium as well as forensic issues of psychiatry, neurology and general medicine. Lenhossék was born in Budapest as a member of a Hungarian professorial dynasty, his father Josef von Lenhossék (1818-1888), his uncles and his grandfather (with the same name of Michael von Lenhossék, 1773-1840) all being university professors. Lenhossék Jr. studied medicine in Vienna and Budapest. While working in the anatomical institute of his father, he published his first piece of early medical research in his third year at medical school “On the Spinal Ganglia in the Frog” (in German) in 1886. Having completed his medical dissertation “On the Ascending Degeneration of the Spinal Marrow” (in German) at the University of Budapest in 1889, Lenhossék became the temporary head of the anatomical institute. He then decided to leave Budapest for the University of Basle in Switzerland. There, he continued his ground-breaking research on the nervous system for three-and-a-half years and graduated with his second,

Habilitation dissertation: “The Fine Structure of the Nervous System in the Light of Recent Investigations” (in German) in 1893. During this time, the doyen of German-speaking morphological brain research, Rudolf Albert von Koelliker (1817-1905) became aware of Lenhossék’s major progress with the recent neuroanatomical methods and offered him the position of an anatomical Prosector, which Lenhossék accepted for two consecutive years. In 1896, he moved to the University of Tuebingen for a faculty position, and finally assumed the Headship of the Institute for Descriptive and Topographical Anatomy in Budapest in 1900. Probably his most lasting contribution to the history of neurology was Lenhossék’s work on the neurohistology and the histogenesis of nerve cells. Together with the Swiss-German brain researcher Albert von Koelliker, the Swedish anatomist Gustav Magnus Retzius (1842-1919) and the Spanish neurohistologist Santiago Ramón y Cajal (1852-1934), Lenhossék is remembered as a major protagonist of the neuron doctrine. Likewise, he was instrumental in promoting the idea of the “nervous growth cone” (~ “*Wachstumssprosse*”), which he publicly presented in chicken embryo preparations during the 10th International Medical Congress in Berlin on Aug-7, 1890. Soon after the congress Ramón y Cajal published a brief research note (on Aug-10) on the discovery of the growth cone in the “*Gazeta sanitària de Barcelona*”. Ramón y Cajal further expanded upon this note in a split article sent to the “*Anatomischer Anzeiger*” in Germany for publication in Oct-20 and Nov-21, 1890. Based on this sequence of publications, Ramón y Cajal later claimed priority over Lenhossék’s discovery of the growth cone. Lenhossék’s pupil Károly Schaffer later stated that it was Lenhossék’s revision of the major textbook on “The Fine Structure of the Nervous System in the Light of New Investigations” (in German), which from 1895 onwards led to wide acknowledgement of the growth cone. It is plausible, similar to the situation of his adversary Ramón y Cajal in the growth cone debate, that Lenhossék’s geographical situation and many publications in Hungarian hindered the reception of many of his ideas. However, the acceptance of the “neuron doctrine” and the discovery of the growth cone as well as the research impact of his Hungarian students clearly demonstrate the important and lasting pioneering impact of Mihály Lenhossék’s work in the history of modern neurology.

### **23. The Wrong Animal for the Job? The 1856-1858 Brown Séquard – Philipeaux Debate on Albino Rats and Adrenal Function**

Sharman Levinson, The American University of Paris, France (slevinson@aup.edu)

Those interested in the history of experimental uses of albino rats will have noticed recurrent references to the neurophysiologist Jean-Marie Philipeaux’s research published first in 1856 in the *Comptes Rendus Hebdomadaires de l’Académie des Sciences*. Numerous authors consider these experiments to be among the first known cases in which albino rats were used as experimental animals. However, historical literature mentioning Philipeaux’s rat research, written principally by American psychologists and historians of psychology, tends to focus on 20th century rat experiments, and thus does not discuss the debate between Philipeaux and his colleague Charles Edouard Brown-Séquard. The former observed that albino rats tended to survive adrenalectomy for an extended period of time and thus that “the adrenal glands are not necessary to the life of animals”. The latter claimed that albino rats should not be the animal of choice to demonstrate or refute the vital necessity of the adrenal glands. Brown-Séquard further hypothesized that something about the rats’ albinism might be responsible for their unusually long survival. This paper presents and contextualizes the debate between Philipeaux and Brown-Séquard documented in a series of publications in the *Comptes Rendus Hebdomadaires de l’Académie des Sciences* and other French journals between 1856-1858. We first relate this debate to discussion in mid 19th century France concerning 1) the choice of experimental animal and 2) the fight to promote experimental physiology over anatomical deduction. With regard to both of these issues, Claude Bernard’s programmatic portrayal of the adrenalectomy experiments may have led future historians to lose sight of the debate itself. Revisiting this debate is also a chance to understand its place in the context of Pierre Flourens’ laboratory at the *Muséum National d’Histoire Naturelle* where both participants conducted their research and which was the epicenter in France for the quest to determine adrenal

function. Finally, the paper discusses recent historical research which tends to focus exclusively on one of the protagonists in the debate. Brown-Séguard's work on adrenal function is acknowledged in the history of neuroendocrinology. Philipeaux's experiments, though often mentioned in passing, appear consistently in the history of rats as experimental animals. Given evidence of much earlier experiments on rats, the paper will also consider what factors could have led to the recurrent reference to Philipeaux in particular. We postulate that the impact of the debate, though forgotten, was more important than could be previously assessed. Revisiting the debate reveals surprising facets of the somewhat unusual choice (for this period) of albino rats and may be of particular significance for contemporary interest in the history of experimental animals and animal models. It may also be an occasion to reflect on controversy in the prehistory of endocrinology, prior to Brown-Séguard's more (in) famous organotherapies.

**Public Outreach Event:**

**The Narcissism of the Powerful: Charisma and Fascination in Psychoanalytic Thought**

Elizabeth Lunbeck, Vanderbilt University, USA (elizabeth.lunbeck@vanderbilt.edu)

An enduring puzzle to observers of human nature is the fascination exerted by the figure of power. Psychoanalysts have long worried this issue, turning to charisma and narcissism to account for our apparently willing submission to powerful but manifestly flawed leaders. Why, they have asked, do individuals accept the illusory satisfactions offered by the narcissist over reality's more substantial rewards? Why do some barter away their independence, allowing themselves to be dominated by charismatic charlatans proffering magic? In this presentation, I focus on the analytic literature on submission to power as well as on analytically inflected discussions of leadership in the management literature, exploring the relatively recent emergence of the figure—construed as at once necessary and dangerous—of the powerful-man-as-narcissist, from the celebrity CEO to the charismatic politician. Poised between omnipotence and destructiveness, between magic and danger, this figure embodies many of the contradictions long thought characteristic of narcissists while at the same time marking a significant shift of focus in the popular discussion: from the fascinating but frivolous female narcissist who offends aspirational norms of asceticism in her vanity and greedy shopaholism to this figure's capacity for destruction or, as Freud put it, for damaging "the established state of affairs."

**Cheiron Film Session:**

**Highlights from the Center for the History of Psychology: John Paul Scott and the Division of Behavior Studies at the Jackson Lab**

Cathy Faye, The University of Akron, USA (cfaye@uakron.edu) and  
Lizette Royer, The University of Akron, USA (lizette@uakron.edu)

In 1929, C. C. Little, President of the University of Michigan, established the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine to study cancer and the effects of radiation. Less than twenty years later, in 1945, a new study division focused on behavior and genetics was also established at the Jackson Laboratory (Dewsbury, 2009; Mitman, 1992). Funded by the Rockefeller Foundation, the Division of Behavior Studies would focus on research with "some bearing on basic general theories of comparative sociology and social psychology, with the ultimate goal that these studies will be useful in solving human social problems" (Scott, 1947, p. 176). Through conferences, fellowships, publications, and research, the Division of Behavior Studies became a central hub in the establishment of the field of behavior genetics in America. John Paul Scott (1909-2000), a young faculty member in the Department of Zoology at Wabash College in Crawford, Indiana was hired to lead the Division of Behavior Studies.

Scott, a student of Sewall Wright at the University of Chicago, had previously conducted work on development, genetics, and social behavior and was well-known to the leaders and staff of the Jackson laboratory (Scott, 1985). Upon arriving at the lab, Scott and his colleagues began an extensive study of the temperament and social behavior of different breeds of dogs, a study that would eventually involve the testing of more than 300 puppies and the production of approximately 8,000 pieces of data (Scott, 1985). The results, published in a highly successful monograph (Scott & Fuller, 1965), were widely cited and contributed to work in a number of areas, including the training of guide dogs, research on the heritability of intelligence and temperament, and work on the nature of human and animal aggression (Dewsbury, 2001). The John Paul Scott papers at the Center for the History of Psychology document Scott's life, his work at the Jackson Laboratory, and his role in the establishment of the field of behavior genetics. The still image collection includes photographs of Scott's "school for dogs", views of the buildings comprising the Division of Behavior Studies at Hamilton Station, and photographs taken at conferences that were integral to the founding of the field of behavior genetics. The collection also includes a set of films documenting Scott's work from the 1940s into the 1970s. The films provide a look at more than 30 years of research on social behavior, genetics, development and aggression. They include: footage of Scott and his colleagues conducting tasks and gathering data with the dogs at Hamilton Station; research footage of social behavior among sheep, goats, mice and grouse; images of audiogenic seizures in mice; and scenes from a study on social facilitation or allelomimetic behavior in dogs. In this presentation, we will highlight the John Paul Scott papers by showcasing film, still images, and unpublished documents from the collection. Taken together, these records provide a detailed portrait of the life and work of Scott, the history of behavior genetics, the history of research on violence and aggression, and the history of behavior study at the Roscoe B. Jackson Memorial Laboratory.

#### **Keynote Lecture I:**

### **Fifty Years of Behavioural Neuroscience: Correcting Historical Missteps and the Emergence of General Principles of Cerebral Organization**

Bryan Kolb, University of Lethbridge, Canada (Kolb@uleth.ca)

A review of the key knowledge of brain-behaviour relationships in 1960 reveals that many of the beliefs regarding the relationship between cerebral cortex and behaviour were fundamentally flawed. There are many reasons for the early misunderstanding and one important one is related to erroneous conclusions reached by early pioneers of brain-behaviour studies such as Karl Lashley (1890-1958) and his students. It was after Lashley's death that a new field of behavioural neuroscience emerged giving birth to our current understanding of brain-behaviour relationships. For example, Lashley's influential views on the neural basis of learning and memory misdirected the field and it was only later that those general principles of cerebral organization related to multiple memory systems became widely understood. It is now possible to identify several fundamental principles of cerebral organization and function that paint a very different picture of brain-behaviour relationships than was appreciated in 1960. Consider a few examples. First, it is clear that the mammalian brain is organized in a highly conserved manner to solve both class-common and species-typical behavioural problems. Second, the cerebral cortex is organized in two parallel systems designed for object recognition and object-related movements. These systems provide a basis for both conscious and unconscious processing of sensory input. Third, cerebral organization is dynamic and remains plastic throughout life. One consequence of this understanding is the development of a new science of cerebral rehabilitation after injury. Finally, whereas it was once believed that the primate motor system was special in its control of fine skilled movements, this is clearly not the case. Indeed, an argument can be made that there are very few, if any, evolutionary discontinuities in brain-behaviour relationships across mammalian phylogeny. One of the main reasons for the fundamental changes in our knowledge is the development of a new science of behaviour and brain that has been best developed in the study of rodents and primates (human and nonhuman). In parallel, there has been an emergence of new noninvasive imaging technologies and computational

neuroscience, both of which have opened up new perspectives on how to study brain and behavioural relationships. As the general field of neuroscience continues to expand the importance of behaviour, which is the reason for a brain, can only become more and more central to the field.

**Friday Morning, June 17, 2011**

**History of Neurophysiology:**

**24. The Neuroscientific Origins of Cinema and Stereo**

Nicholas J. Wade, University of Dundee, Scotland, UK (n.j.wade@dundee.ac.uk)

Advances in neuroscience have often been associated with instruments that extended the normal range of vision, like microscopes and neuroimaging devices. Less attention has been given to the instruments that enabled examination of vision itself. The uses of such devices have extended neuroscience generally and visual neuroscience in particular. The instruments themselves were usually very simple like prisms, mirrors or rotating discs, but they served the function of removing the study of vision from its object base. Newton's prismatic experiments enabled colour vision to be examined experimentally using the methods of physics. In the early nineteenth century, similar developments occurred in the perception of space and motion. Experimental studies of stroboscopic (apparent) motion and stereoscopic vision have their origins in London in the decade from 1825-1835. The principal investigators included Thomas Young, John Ayrton Paris, Peter Mark Roget, Michael Faraday, and Charles Wheatstone. The instruments were called philosophical toys because they fulfilled the dual roles of furthering scientific experiment on the senses and of providing popular amusement. The developments were initially driven by the need for stimulus control so that the methods of physics could be applied to the study of perceptual phenomena. The mirror stereoscope was based on reflecting two slightly different perspective drawings of three-dimensional objects, but its scientific impact was dramatic, as was its popular appeal. Its invention made possible the experimental study of binocular space perception, and paved the way for theories of depth perception and single vision with two eyes; it rendered the normal conditions for seeing depth from disparity experimentally tractable. Stereoscopic vision was the near universal experience of using two eyes in the natural environment. The perception and representation of motion had a different history. Motion had been frozen in pictures and few novel techniques of alluding to it were developed. Prior to the invention of instruments for generating apparent motion, moving objects had been the source of scientific study. Thereafter, the synthesis of motion from sequences of still images was to have profound effects on popular culture as well as on artistic representation. Until that time, the experience of motion was almost always a consequence of object or observer movement: apparent motion was a novelty. Many varieties of stroboscopic discs and stereoscopes were devised thereafter and their popularity increased enormously after 1840, when combined with photography. Stereoscopes sold in millions, as they could be combined with paired photographs to provide a more compelling impression of scenes otherwise unseen. The stroboscopic disc proved to be the engine for the perception of apparent motion, to be experienced later in the century as movies. The instrumental revolution transformed not only our vision of pictures but also our picture of vision. Pictures could be paired, to appear in depth, or presented in rapid succession, to appear in motion, thereby replacing the two dimensions missing in static and single pictures. Theories of spatial vision developed thereafter emphasized its constructive nature, and placed greater emphasis on the manipulations that could be made of two-dimensional stimuli. It could be said that development of visual science was as dependent on these devices as early neuroscience had been upon the microscope.

**25. Faces and Brains in the Historical Evolution of Constructs of Human Cognition**

Lawrence Kruger, University of California at Los Angeles, USA (lkruger@ucla.edu)

From antiquity to the present, recognition of “Individuality” in our species has been progressively dominated by the depiction of faces and eventually a concept of “mind” (or “cognition”) related to the brain by comparing behavioral traits and human “races” in the late 17th century (Willis, Collins) and the emergence of the discipline of “anthropology” (Tyson). By the early 19th century competing disciplines, or cults, of “physiognomy” (“Lavaterianism”) and “cranioscopy” (“phrenology”) emerged, both providing an expansion of systematic depiction of the morphology of faces and brains in relation to behavior and to early concepts of “race”. By the late 20th century the importance of individuality in brain “representation” of faces and the uncovering of epigenetic factors in relation to unexpected degrees of “plasticity” of brain connectivity were related to facial recognition variants (autism, Williams syndrome) currently being studied by brain imaging and detailed genomic correlates of brain structures implicated in cognitive analysis of faces. This paper has not been written and thus a bibliography has not been assembled. I can allude to some of my previous publications relevant to this subject and am writing a book that includes this subject.

## **26. Mind and Brain Physiology in the Nineteenth Century**

J. Wayne Lazar, Garden City South, USA (jwayne314@aol.com)

Most generally, this paper examines the roles of consciousness, mind, and intellect in neurophysiological theory during the last 50 years of the nineteenth century. The paper shows how their meanings were interpreted in the light of new information. More specifically, tensions between traditional ideas and new research findings are assessed for several men who were prominent in neurophysiology and neurophysiological theory. Three generalizations help explain the reasons for much of the tension and many of the choices made by these men. They will be referred to as “the mind illusion,” “the definitional-trap,” and “the mind as agent”. By “the mind illusion,” I mean that the mind is “seen” when certain behaviors are observed (Dennett, 1991; Bering, 2010). James’ description of the behavior of lesioned and intact frogs will be used to illustrate this point (James, 1890). Probably no person of the period would deny that behavior was observed, but mind and consciousness were understood to be “behind” the behavior as surely as minds are inferred in others by ourselves. What may have begun as an inference became so pervasive in neurophysiology and psychology as well as in common parlance that to call it an illusion would not be an over statement. Behavior is understood with high level concepts like teleology and purpose and mind accounted for what was observed regardless of whether the nervous system was intact or not. The provocative term “definitional-trap” was chosen to emphasize that researchers of the 19th century were influenced by their definition of mind. If they were to be logically consistent, they were “forced” into finding mind and consciousness in places they would, perhaps, rather not have considered. Their choices within this context were relatively limited. The presence or absence of mind and consciousness depended on behaviors that could be characterized variously as adaptive, complex, coordinated, flexible, purposive, spontaneous, unpredictable, and even cries of pain. If these types of activities were observed, then mind and consciousness were implicated and voluntary as opposed to involuntary behaviors were identified (Bain, 1855, Bain, 1894; Hammond, 1876; Foster, 1890). Application of this definition was unfortunate for neurophysiologists because the distinction between voluntary and involuntary movements was not a physiological one. Its deciding criterion was an implied “feeling” or “idea” behind the response. It was the mind, or more specifically, the aspect of mind called volition, that was the originator and, therefore the efficient cause, of intelligent behavior. The mind in this causal role is what is meant by “mind as agent.” Not only is the mind present in all intelligent behaviors by definition, but it accounts for intelligent behavior. This idea of “mind as agent” is integral to the discussion of centers for brain functions (Fritsch and Hitzig, 1960; Ferrier, 1876, 1886, 1889; Jewell, 1876, Jewell and Bannister, 1877; Dalton, 1882).

### **Intelligence and Will:**

## 27. Hebb and Cattell: The Genesis of the Theory of Fluid and Crystallized Intelligence

Richard E. Brown, Dalhousie University, Canada (rebrown@dal.ca)

Raymond B. Cattell is credited with the development of the theory of fluid and crystallized intelligence. The genesis of this theory is, however, vague. Cattell, in different papers, stated that it was developed in 1940, 1941 or 1942. Carroll (1984, *Multivariate Behavioral Research*, 19, 300-306) noted the similarity of Cattell's theory to; "Hebb's notion of two types of intelligence"; which was presented at the 1941 APA meeting, but the matter has been left at that. Correspondence between Cattell, Donald Hebb and George Humphrey of Queen's University, Kingston, Ontario, however, indicates that Cattell adopted Hebb's ideas of intelligence A and B and renamed them. This paper describes Hebb's two types of intelligence, and shows how Cattell used them to develop his ideas of crystallized and fluid intelligence. Hebb and Cattell exchanged a number of letters, before Cattell's paper was rewritten in such a way that everyone was satisfied. This paper examines the work of Hebb and Cattell on intelligence, their correspondence, the development of the ideas of fluid and crystallized intelligence.

## 28. George Trumbull Ladd's Physiological Psychology

Miki Takasuna, Tokyo International University, Japan (takasuna@tiu.ac.jp)

In the history of psychology, George Trumbull Ladd (1842-1921) must be singled out for authoring the *Elements of Physiological Psychology* (1887), one of the earliest English-language books to use the term "physiological psychology" in the title. Ladd graduated from Western Reserve College in 1864, and then, in 1869, pursued theological training at Andover Theological Seminary. After ten years working at the Midwestern ministry, in 1881, Ladd was asked to join the Yale faculty as a professor of mental and moral philosophy. He accepted. During his professorship and even after he retired in 1905, Ladd visited Japan three times from the late 19th to early 20th centuries (1892, 1899, and 1907) (Armstrong, 1921). Though Ladd did not conduct experiments, young Japanese psychologists attended his lectures. Consequently, he exerted a strong influence on Japanese psychologists, such as Matataro Matsumoto (1865-1943), who traveled to Yale to study for his PhD under the direction of Ladd and Edward W. Scripture (1864-1945). In 1899, Matsumoto obtained doctorate in psychology with acoustic research. He would later become the first president of the Japanese Psychological Association, which was established in 1927 (Okamoto, 1976). Considering Ladd's early career in the ministry, it is unclear why he changed direction to pursue physiological psychology. Though the answer may be buried in the unpublished autobiography he left, I have not read it through as of yet. I can say that his book was "met with a warm welcome", since Wilhelm Wundt's German-language textbooks were "difficult for most readers" (Boring, 1950, p.525). Ladd used Wundt's *Grundzuege der physiologischen Psychologie* (2nd ed., 1880) as a model for his book that, like Wundt's, was later translated into Japanese (Ladd, 1901). Apart from his influence in Japan, Ladd's "Elements" long-impacted American psychology, as evidenced when a couple of decades later he collaborated on a revision with Robert S. Woodworth (Ladd & Woodworth, 1911). How much did Ladd owe to Wundt when writing the *Elements*? The overall structure of Ladd's book was similar to that of *Grundzuege*: In the first section, the brain and nervous system are described in detail, the second part explains sensation and motion, and in the third and final section, theory of the mind is discussed. To compare content similarities, a citation analysis was conducted in which every footnoted reference found in Ladd's *Elements* and Wundt's *Grundzuege* was collected. Tabulations showed that Ladd most frequently cited Wundt's *Grundzuege* (63 times, 8.5%) and Hermann Helmholtz's *Handbuch der physiologischen Optik* (23 times, 3.1%). However, from 319 references in the *Elements*, only 99 overlapped with *Grundzuege*. In other words, more than two-thirds of Ladd's references were based from his own self-study. This is especially evident in the anatomy portion where he includes 91 illustrations. Additional differences in Ladd's citations include

his omissions of Charles Darwin and Herbert Spencer, apparently choosing to distance himself from evolutionary theory at that point of time. Further, Ladd cites Hermann Lotze's *Medicinische Psychologie* (1852) far more often than Wundt (18 times vs. 9 times). This reflects Ladd's experience in translating and editing six volumes of the series Lotze's *Outlines of Philosophy* (1884-1887). The test of time makes it clear that Ladd's position in the history of American functional psychology is secure. His impacts on the field are the topic of today's discussion.

## 29. Will-Power and Will-Training

Robert Kugelmann, University of Dallas, USA (kugelmann@udallas.edu)

In the course of describing the structure of psychotherapy, Edward Boyd Barrett (1925) observed that abulia or "the incapacity to will" (p. 227) was a feature of psychoneurosis, and he claimed that "will-culture is essential for permanent cure" (p. 233). To remedy this defect, he prescribed among other things exercises in will-training, based on *Strength of Will* (Barrett, 1915). The earlier book contains extended serious discussions of the will from psychological, philosophical, and ethical points of view. What stands out, however, are the resolutions and exercises, together with introspective reports, on such prescribed tasks as: "Each day, for the next seven days, I will stand on a chair, here in my room, for ten consecutive minutes, and I will try to do so contentedly" (p. 148). Boyd Barrett grounded his self-improvement techniques in psychological investigations of the will carried out by Michotte and his colleagues, including himself, and in the work of the Wuerzburg group. Johann Lindworsky (1932) made similar recommendations with similar justifications. Several decades later, these techniques were still discussed. Two Italian psychologists, for example, had contrasting views. Roberto Zavalloni (1962) thought will-training too abstract in *Self-Determination*, stating it is better to focus on the person as a whole, rather than on motives in isolation. However Roberto Assagioli included Boyd Barrett's techniques in *The Act of Will* (1973), a book aiming to bring the will back into prominence in psychotherapy. All these psychologists pointed back to William James, who discussed the will and its education in a number of places (see, e.g., James, 1890, vol. 2, pp. 579-592). These are but examples of a significant and persistent form of self-cultivation (or technology of the self) that has persisted in the modern age (Maasen, 2007). Of course, regimes of self-governance have a long history. Its fuller study would include ascetic disciplines associated with religious, military, athletic, and artistic practices. The ascetic practices of present interest are those in the western industrial nation-states of roughly the past two hundred years. The terms "will-power" and "will-training" serve as guides through the labyrinth of this time to this type of self-knowledge and its associated techniques. This presentation begins with discourses of will-power in everyday language from the mid-nineteenth to the late twentieth century, drawing in part on the use of the term in psychology. Then attention shifts to texts that describe will-training. There are a large number of such guides, so discussion will concentrate on those texts that claim medical or psychological authority. Many of these texts present specific exercises to develop the will, and the texts themselves indicate their rootedness in ascetic traditions. Oppenheim (1991) writes that the will was a staple of nineteenth-century medical discourse. Ebbard (1907), Fothergill (1891), Worcester, McComb and Coriat (1908), and Walsh (1919) provide examples of will-training books that gave a medical justification. Works by Boyd Barrett, Lindworsky, both Jesuit psychologists, justified the exercises by comparing them with the *Spiritual Exercises* of St. Ignatius Loyola, the founder of their order. In these books, psychological warrants are given to traditional ascetic disciplines, and the purpose of the training had been largely detached from religious ends—and from traditional religious social organizations. Not all will-training texts hail from traditions of medicine or spiritual formation, but other warrants, especially economic, were provided. Meyer (1980) discusses some of these early twentieth-century manuals, noting that some abstracted the cultivation of the will from the Protestant religious context in which they had first developed. Maasen (2007) notes that some went further, and promoted a quasi-fascist notion of the self and the—to borrow a phrase—triumph of the will.

## **History and Historiography of Methods:**

### **30. Alan Fisher's Introduction of the Double Cannula to Intracranial Chemical Stimulation**

Mark A. Affeltranger, Bethany College, USA (maffeltranger@bethanywv.edu)

The early 1960's signaled a change in neuroscience methodology. Lesioning and electrical stimulation became obsolete, leading to incorrect conclusions about a brain where functionally-distinct areas overlap (Miller, 1965). Perfecting psychiatric drugs also required knowledge of neurotransmitters of the central nervous system. Intracranial chemical stimulation appeared to be more strategic and able to advance pharmaceutical research. Paul MacLean and Jose DelGado (1953) chemically stimulated the brain of monkeys and cats with stereotaxically implanted syringes. They stuffed the needle with crystal acetylcholine and sealed the end with bone wax. To stimulate the animal, they pushed a wire through the syringe and bone wax releasing the acetylcholine. DelGado (1955) then made improvements by employing a remote control stimulation device that pushed the wire through the syringe, restricting the animals less. With this early method, a researcher could only stimulate once as drug contaminated the inside of the needle and debris and CSF clogged the needle. Beginning a post-doctoral fellowship with Donald Hebb in 1955, Alan Edmund Fisher (1927-1984) proposed stimulating a single rodent with a variety of chemical solutions. Hebb demanded electrical stimulation and neural recording as well. Since Fisher needed to control everything above an open field box, he designed a system with a long tube and wires to allow free movement of rodents. Fisher found a single unit with all of these attachments too cumbersome to implant and more open to blockage and cross-contamination between solutions. He proposed a system containing two units, one implanted stereotaxically and a clip unit fit to the implant unit. The implant unit contained a 23-gauge needle cannula connected through a plastic holder. Two Tylon-insulated copper or silver wires, one for electrical stimulation and one for recording, adhered to opposite sides of the needle shaft terminating at the tip of the needle. He mounted the plastic part of the implant with jeweler's screws fastened into the skull. The needle extended 2 to 9 mm below the plastic holder depending upon the targeted brain area, with the needle tip being just above the point of stimulation. After a few recovery days, Fisher connected a clip unit to the implant. The clip unit contained alligator type jaws covered by flexible rubber tubing and enclosing a central block of hard rubber containing a 30-gauge needle cannula. The 30-gauge needle fit through the 23-gauge needle of the implant unit extending just below its tip. The alligator jaws made contact with the electrode terminals, and hearing aid wires extended from this clip system to either a stimulator box (0 to 12 volt, 60 cycles/second) or an EEG. Seven feet of polyethylene tubing (diameter 0.024 inches) connected the cannula of the clip unit to a microsyringe which released 0.00001 cm<sup>3</sup> of solution. After stimulating with one solution, Fisher replaced the clip unit (Fisher, 1961). Fisher (1956) used the new double cannula to administer testosterone sulfate. The stimulation worked in 10% of subjects inducing sexual behaviors if planted in the medial pre-optic area and maternal behaviors if implanted in the lateral pre-optic area. Those 10% demonstrated consistent effects to repeated testosterone but showed no effects to saline or electrical stimulation. Later, Sebastian Grossman (1960) at Yale adopted the double cannula with crystal chemicals. Fisher would also use crystal chemicals in his studies (Fisher and Courey, 1962; Levitt and Fisher, 1967). This procedure continued for years, appearing in procedure manuals such as Singh and Avery (1975).

### **31. John Dewey and the History of Social Psychology**

Sam Parkovnick, Dawson College, Canada (sparkovnick@dawsoncollege.qc.ca)

This paper will begin to look at the role of the philosopher John Dewey in the history of social psychology. It will first present Dewey's approach to social psychology and then take up the question of Dewey's influence on psychological social psychology (henceforth PSP), leaving sociological social psychology for another paper. Dewey never presented a complete approach to social psychology; he

worked out aspects of his social psychology over time and where necessary. This paper will try to do what Dewey never did, provide a complete presentation of Dewey's approach to social psychology. It will address, among other things, Dewey's take on psychoanalysis, instincts, and behaviorism, though the conference paper will include only Dewey on the individual and the social. John Dewey had a great deal to say regarding the individual and the social. What he had to say remained largely unchanged over the years. It appeared in his social-psychological writings like "Review of the Psychic Factors" (1894), "Psychology and Social Practice" (1900), "Interpretation of the Savage Mind" (1902), "The Need for Social Psychology" (1917), and *Human Nature and Conduct* (1922); his ethical writings, among them *The Study of Ethics: A Syllabus* (1894), *Ethics* (1908), and *Ethics* (1932), the latter two co-authored by James H. Tufts, as well as numerous articles; and, finally, his political and social writings of the 1920s and 1930s, including *The Public and Its Problems* (1927), *Individualism Old and New* (1929), *Liberalism and Social Action* (1935), and *Freedom and Culture* (1939). Dewey opposed a methodological individualism which would reduce the social to individuals or combinations of individuals. He also opposed a sociologism which would reduce individuals to the social. He held that individuals are to a large extent socially determined and that society is composed of individuals. Dewey gave priority to neither the individual nor the social, believing that one is not incompatible with the other and that what contributes to one also contributes to the other. This paper will argue that Dewey's position regarding the individual and the social, though an interesting point of departure was never thoroughly worked out and was deficient as an account of both the individual and the social. Regarding the question of influence, Peter Manicas has argued that Dewey had very little role in the development of American psychology and that *Human Nature and Conduct* (1922) in particular had very little influence on American psychologists (2002). This paper will take a position at odds with Manicas regarding Dewey's influence, restricting itself to PSP; in doing so, it will also question how Manicas seems to conceive of influence. Specifically, it will look at the influence of Dewey on Floyd and Gordon Allport, beginning with Chapter 5 of Floyd Allport's *Institutional Behavior*, titled "The Problem of the Public", which was a response to Dewey's *The Public and Its Problems* (1927).

## **32. The Varieties of Digital Experience; Or, Computational History Goes Mental**

Christopher D. Green, York University, Canada (christo@YORKU.CA)

Computational research methods are becoming increasingly common in the humanities, history included. Virtually all scholars now use electronic databases to do their literature searches. A great deal, perhaps the majority of journal articles are now downloaded from the internet, and electronic books are rapidly gaining ground. These basic modes of search-and-retrieval, however, are only the start of what promises to soon become a vast array of commonly used digital techniques. By developing web crawlers, for instance, one can have the computer search and find relevant materials that the human researcher might never have begun to look for. Dumb as the computer might be compared to the expert researcher, the computer has the advantage of relentless doggedness – it will chug away continuously for days, weeks, or even months at a time, retrieving far more information than human could read a process even over the course of an entire career. The sheer volume of source material that can be retrieved by computer makes it necessary for us to develop new methods of analysis – themselves also computational – that will enable us to begin to make sense of this vast quantity of words, images, data, audio recording, videos, or what have you. Ultimately, we will develop natural language parsers effective enough that they will be able to automatically provide précis of complete works. In the meantime, however, a general idea of a corpus' contents can be captured by methods as primitive as word frequency counts and various visual displays of their patterns. In this talk, I will report my initial foray into computational techniques for the investigating the history of American psychology. I will select the turn of the 20th century because this time period is well known to most historians of psychology and, so, it makes an excellent test case for evaluating what the new digital methods are able pick up that has already been well established by traditional means, as well as what they might enable us to discover that has not thus far been made readily

visible by orthodox historiographic approaches. Specifically, I will track individual authors of multiple books, such as John Dewey, James Mark Baldwin, and E. B. Titchener, through their careers to see how their use of language was transformed over time. I will also compare to each other the vocabularies of prominent psychologists such as William James, G. Stanley Hall, and E. L. Thorndike to see how their various approaches the psychology were reflected in their choice of words. The results will be couched not solely in lists of word frequencies, but also in graphs, word clouds, “heat maps,” and other novel visual displays.

#### **Keynote Lecture II:**

### **The Decline and Fall of Syphilization?: Mortality regimes in the Victorian Mental Hospital**

David Wright, McMaster University, Canada (dwright@mcmaster.ca)

Although the nineteenth-century lunatic asylum has been one of the most popular topics in the history of medicine, we still lack important information about the medical status of patients who were admitted to these extraordinary public institutions. This lecture examines one long-neglected dimension of the asylum experience – dying. It begins by surveying what little we know from four decades of institutional case studies about the relative frequency of death and discharge from what were amongst the largest medical institutions in the Western world. Second, as a case study, the lecture presents the cause of death data of over 5,000 patients who were admitted to (and ultimately died in) the four principal lunatic asylums in the province of Ontario (Canada), from 1841 to 1901. The results illuminate the alleged cause of death (in broad categories), revealing, amongst other factors, the impact of tuberculosis and syphilis. The data also suggest that there was a steady increase in life expectancy of those entering the mental hospital over the six decades under study, one that became pronounced amongst female patients. Third, the paper places the rise in average age of death within the context of the decline of mortality in the late nineteenth-century Anglo-American world. Did the asylum mirror the slow augmentation of life expectancy in the general adult population after 1850, or was the lunatic asylum somehow anomalous? The lecture then concludes with possible implications these mortality regimes may have for our understanding of the social (and medical) uses of the lunatic asylum during the Victorian era.

**Friday Afternoon, June 17, 2011**

#### **Conference Symposium II:**

### **Teaching the History of Neuroscience, Psychology, and the Social Sciences**

Samuel Greenblatt, Brown University, USA (samuel\_greenblatt@brown.edu) and  
Katherine Milar, Earlham College, USA (kathym@earlham.edu)

This panel focuses on teaching the history of neuroscience, psychology, and the social sciences at all educational levels, including public outreach. While realizing that there are ca. 60,000 professionals who work as researchers and educators in these areas, both the International Society for the History of the Neurosciences (ISHN) as well as Cheiron, The International Society for the History of the Behavioral and Social Sciences, have realized that the number of active teaching, graduate and training programs in the history of neuroscience, psychology, and the social sciences is rather small in comparison to the breadth and scope of the field(s). This panel will address such questions as (a) why is there such a small number of training programs; (b) how interest in historical education and research could be fostered; (c) how historical teaching and research could be better integrated with the multitude of available psychology and neuroscience programs; and (d) how in-depth research knowledge of the professional historians of science, medicine and psychology could be used as resources to nurture new interest in historiographical work in these burgeoning field(s)? This perception is based on the observation that public awareness of

the neurological and psychological sciences and ethical and policy issues are critical to maintaining public support for funding and research. A broad teaching experience, including social science and humanistic elements in neuroscience, behavioral science and psychology programs, must thus be a central part of the education of professionals, researchers and instructors. It should not be understood only as a marginal endeavor of a few interested individuals – it is the future of these fields.

### **33. The Future of History—Again**

Katherine Milar, Earlham College, Richmond IN, USA (kathym@earlham.edu)

Periodically, articles appear which wonder aloud about the future of the history course (Bhatt & Tonks, 2002, Rutherford, 2002), Tom Leahey, author of history of psychology texts and a past president of Society for the History of Psychology, in 2000 posted a message to the history listserv asking members to consider whether history of psychology is really essential, and suggesting that there are no “big pictures” to be gotten from the study of history that cut across the diverse subfields that make up today’s psychology. In doing so he echoed another SHP past-president David Krantz who wrote 35 years before Leahey (1965), “that there is little value in historical analysis since its roles, whatever they may be, are inadequate for contemporary psychology”. If we believed that, then I think most of us would not be doing what we are doing. Alternatively, perhaps we believe this to be true, but soldier on in our irrelevancy. I will briefly describe my undergraduate course in history of psychology which combines more traditional history content with readings that grapple with questions about the social contexts in which psychological knowledge has been produced and the influence of social values on the development of theories and methods and the interpretation of results.

### **34. An Integrated Historical Perspective in an Undergraduate Program in Neuroscience**

Keith A. Sharkey, University of Calgary, Canada (ksharkey@ucalgary.ca) and Andrew G.M. Bulloch, University of Calgary, Canada (bulloch@ucalgary.ca)

The Bachelor of Neuroscience program at the University of Calgary is a four year undergraduate program that seeks to engage students in a research-intensive curriculum that emphasizes experiential learning and an appreciation of other important academic disciplines. It expects to graduate students with a broad and comprehensive understanding of the nervous system; its physiology, pathology and treatment of neurological diseases. To fulfill this mandate, a core third year course was developed to provide students a historical, societal and ethical background in the neurosciences (Course teachers: Frank W. Stahnisch, Keith Brownell, Walter Glannon, Keith Sharkey and Andrew G.M. Bulloch, University of Calgary). This course integrated these elements in the context of key areas of neuroscience to provide students a perspective on the development of this field. Lectures and discussion of topics that included the discovery of the brain, neuron doctrine, synaptic transmission and learning and memory provided a background for the students as they approached more complex topics of consciousness, free will and aspects of neuroethics. The students used the history and ethics to explore the relationships between the brain, personality and society. We focused on showing how new technologies, such as functional neuroimaging and brain implants impact current thinking in our discipline and how they are viewed in society. Student feedback to date has been very positive and neuroscience faculty members appreciate and support this course.

### **35. How to teach a graduate seminar in history of psychology without having to mark essays over the holidays**

Christopher D. Green, York University, Toronto, Canada

For the past few years, in my history of psychology graduate seminars, I have dropped the traditional term paper from the marking scheme. Although it is true that the term paper constitutes reasonable (though far from perfect) training for one of the skills that advanced students need upon graduation – *viz.*, the ability to write journal articles – many courses will give them this training, and other skills that they require just as urgently are mostly ignored. In particular, the presentation of a short, focused conference talk based on original research is an activity in which they will engage probably even more frequently than publishing articles, but it is given little formal attention in the training of most graduate students. Partly for this reason, I have replaced the traditional term paper with an end-of-term "conference" at which each student presents on a topic of his or her choice, related to the material of the course, usually for between 10 and 15 minutes, and then fields questions from the audience. The audience is made up mostly of members of the class, but I often supplement with a couple of other, more senior graduate students (often who have taken the same course in previous years), or even with faculty members who have an interest in history. I do not forsake writing entirely, however. In my Foundations of Historical and Theoretical Psychology course, which is taken by students from a wide variety of research concentrations (not just students specializing in history of psychology), I have created a wiki on which we collectively create an electronic biographical dictionary of psychology. At the start of term, students are each assigned a different significant figure from the discipline's past and are required to research and compose a short entry on that individual's life and career (typically 500 words). Periodically after the initial entries have been posted, the historical figures are "shuffled" among the students, each is asked to expand (by perhaps 250 words), correct, and edit what the prior students have written. After a couple of rounds of this, around the middle of term, we begin with a new list of figures and go through the process again. As a result, over the course of the semester, each student conducts individual research on several different figures, and writes as much as 2000 words on the history of psychology.

### **36. Teaching the History of the Neurosciences**

Stanley Finger, Washington University in St. Louis, USA (sfinger@wustl.edu)

Teaching the history of the neurosciences allows for many different approaches, ranging from raw chronology to studies of great figures, from looking at field specialization (e.g., practice of medicine) to the drivers behind change, and from examining periods of relative calm as well as paradigm shifts. At Washington University, I have long tried to cover a little bit of everything in my course, which has been aimed at advanced undergraduates, graduate students, and individuals already in medical school. Our tour would begin with ancient cranial trepanation and continue into Egyptian and Greco-Roman cultures, then working its way through Middle Ages and the Renaissance, singling out some great figures (e.g., Galen, Vesalius) and controversies (e.g., heart vs. head), as well as important issues (e.g., dissections, religious influences) for discussion. Students would then be taken into the Early Modern period, where we would discuss such things as Willis' anatomy and the advent of electrical medicine, and follow a winding path into modern times, where we would examine various aspects of Sherrington's and Adrian's physiology, including the instruments they used, and even how Sperry and Levi-Montalcini changed the neurosciences. Students were given a midterm and a final based on these lectures plus the related chapters in one of my books ("Minds Behind the Brain") and an assortment of primary source material. They also had to submit a Neurognostics column question that would be evaluated for JHN and a term paper. Overall, about 20% of my students published one or the other with my assistance, but the polishing process usually required another semester or even year, during which I continued to work with them, albeit in my office and not as a part of another formal course. Students also had to give a 30 min. talk about their paper at the end of the semester. This course has always received very high grades from the students, who were fascinated by seeing how science, medicine, and the humanities could come together.

**37. Every Psychology Course is a History Course**

Elizabeth B. Johnston, Sarah Lawrence College, New York USA (Johnston@mail.sl.c.edu)

My goal is to describe some of my hybrid undergraduate courses that are not billed as ‘history of psychology’ classes, rather they fuse historical and current psychological work in a way that attempts to avoid the specter of presentism. Often the historical content forms the theoretical backbone of the course that is fleshed out through readings from the contemporary literature. For example, Frederic Bartlett’s 1932 text *Remembering* undergirds my Memory Research Seminar, Rudolf Arnheim’s 1954 text *Art and Visual Perception* is at the heart of my course of the same name, and William James’s 1884 paper ‘What is an emotion?’ is the first paper we read and frequently reference in my joint biology/psychology seminar, *The Feeling Brain*. In keeping with the joint ISHN/Cheiron meeting I will focus on neuroscience courses.

**38. Continuing Education in History of Neuroscience**

Samuel Greenblatt, Brown University, USA (samuel\_greenblatt@brown.edu)

This presentation is based on an event that was held at the annual meeting of the American Association of Neurological Surgeons (AANS) on April 12, 2011. There are always several historical events at this meeting, including Breakfast Seminars of 2 hours’ length. The arrangement is generally a panel of three or four speakers, with the audience in the usual rows of chairs facing the speakers and the projection screen. My Seminar had been different. It was conducted around a large table in the style of a seminar course in a history department, with all of the audience being the panel. Hence, pre-meeting “homework” had been sent to the participants. It consisted of 30-40 pages of (primary source) journal reports and some (secondary) book chapters about the earliest modern neurosurgical operations. The idea of the panel was to analyze the many questions that arise from actually looking at the available evidence for priority and other claims about those surgeries, which took place in 1879-1884. In the announcement of the seminar, this participatory arrangement was explained, and people were explicitly asked not to register for the seminar if they could not do the homework. Since the course is to be conducted in true seminar style, I limited the registration to 10 people. It was then oversubscribed by three or four on the waiting list, so I think the homework that I was requiring was actually intriguing people, and I like to report to the ISHN/Cheiron conference panel about a lively experience. It already appears that the seminar will be repeated at the AANS meeting next year (2012). This presentation will discuss why participants responded as they did and how the experience might be translated to seminars at the meetings (or other educational activities) of other professional groups.

**Conference Symposium III:**

**The Future Relationship of Psychiatry, Psychology and the Neurosciences in the Light of the Past –  
Reductionism or Complementarity?**

Frederic Weizmann, York University, Toronto, Canada (Weizmann@yorku.ca) and  
Frank W. Stahnisch, University of Calgary, Canada (fwstahni@ucalgary.ca)

Not very long ago, McGill Professor of Philosophy of Mind, Ian Gold, raised the question of “Reduction in Psychiatry” in the *Canadian Journal of Psychiatry* 54 (2009), pp. 503-503, in what was a brief but thought-provoking article. One of the main lines of the philosophical questions raised in it, concerned the examination of what Gold called “the doctrine of reductionism” in psychiatry. In short,

reductionist approaches nowadays hold that all psychiatrically relevant phenomena (in mental health and disease) can principally be reduced to neuronal mechanisms and biological explanations of brain function. On the treatment side, even if not fully realized today, reductionists assume that in the not-so-far future, treatment options in psychiatry will become purely biological, i.e. based on psychoactive drugs, gene-manipulation, and surgical interventions, such as deep brain stimulation, etc. While Gold performed a selective review of the available literature in examining the doctrine of reductionism in psychiatry, he came up with a fairly controversial conclusion, which (a) found the two major arguments in support of reduction in psychiatry to be unsatisfactory and (b) put forward the view that scepticism about reductionism must prevail (due to methodological and widely epistemological reasons). In concluding, he stated that “currently, there is little reason to think that any significant portion of psychiatric theory will be reduced to neuroscience or genetics”. Although a highly controversial thesis in itself, these views have not been contested by North American psychiatrists and neuroscientists. It is the aim of this 1,5 hrs. Discussion-Panel, to (a) review the status of current-day biological approaches in psychiatry and (b) to ask in the recent context in which, for example, Psychiatric Departments have increasingly become renamed or adjusted as “Molecular Psychiatry” units and Psychology Departments reinvented as “Behavioural Neuroscience Departments”, what the future of the relationship between psychiatry and neuroscience will bring? Given the opportunity, to have this panel at the joint ISHN-Cheiron conference 2011, historians may join the discussion and ask, (3) whether we are currently witnessing a *volte-face* back to “brain psychiatry” in the traditional Griesingerian sense?

**39. Psychiatry: Reductionism or Pluralism?**

Jorge Perez-Parada, University of Alberta, Canada (jperez@ualberta.ca)

Recent advances in technology have led to a wealth of innovative research in biological psychiatry. Modalities to elucidate and explore the genetic and molecular basis for many disease entities within medicine have sought replication within the field of mental illness. Despite the initial hope and promise of rapid advances in understanding psychiatric syndromes through molecular genetic, functional and drug treatment mechanisms, little has changed. The rising prevalence of mental illness and the complex heterogenous expression of psychiatric syndromes will necessitate a complementary approach, both to research and treatment, if any significant breakthroughs are to be made.

**40. How should we train students for the future of Psychology?**

Richard E. Brown, Dalhousie University, Canada (rebrown@dal.ca)

A student entering first year psychology today (2011) will receive a BSc in 2015, an MSc in 2017 and a PhD in 2021. Their career as a Psychologist will extend until 2058. In the 2002 NSERC reallocation report, I suggested that the future of psychology would focus on three areas: cognitive neuroscience, developmental cognitive neuroscience and behavioural neuroscience: the genomic revolution. In the last 9 years, the importance of neuroscience within psychology has grown to the extent that it is difficult to have a teaching or research program in psychology without considering neuroscience. Examples of recent research in cognitive, developmental, and social psychology, learning and memory, perception and clinical psychology will be cited to demonstrate the integration of neuroscience, behavioural, and most recently, genetic approaches to the study of psychological problems. I will discuss Posner and Rothbart's (2004, *Canadian Psychology*, 45, 267-278) argument that the work of Donald Hebb provides a roadmap for the integration of psychology and neuroscience. This will lead to a discussion of how the DSM-V will change psychology and how the human (as well as rat, mouse, zebrafish, etc) genome project has made genetics and epigenetics important for psychology. Finally, I shall discuss that which is unique to Psychology: the analysis of behaviour. All of this will lead to the ultimate question: How should we train students for the future of Psychology?

**41. What is the Relationship between Brain and Behaviour?**

Bryan Kolb, University of Lethbridge, Canada (Kolb@uleth.ca)

Behavioural neuroscience is an emergent field that changing our understanding of the relationship between brain organization and function and behaviour. Although this understanding has certainly had a reductionistic thrust to it, it would be more accurate to see it as the development of a better understanding of how cerebral circuits change in response to experiences, including “mental” experiences, and how this ultimately relates to both normal and abnormal behaviour. Understanding such molecular bases of brain and behavioural relations provides a new basis for developing and evaluating interventions, both biological and behavioural, to treat abnormalities in behaviour.

**42. Reflections on Psychiatry and Technology**

Frederic Weizmann, York University, Toronto, Canada (Weizmann@yorku.ca)

Modern models of disease rest on the idea that illnesses are caused by abnormalities of anatomical structure and function, and that these abnormalities produce the characteristic symptoms of specific diseases. Although these models date back to the 17<sup>th</sup> century, Stanley Reiser has pointed out that they led to little immediate change in medical practice. Physicians continued to rely largely on patient narratives, supplemented by non-invasive superficial physical examinations, as they had for hundreds of years, to determine underlying disease. Reiser notes that it was not until the development of medical technology, marked initially by the 19<sup>th</sup> century invention of the stethoscope, that physicians began to be able to probe beneath the surface of the body with ever-more powerful and precise technologies. The hallmark of modern medicine is, in large measure, the development of such technologies. However, as Reiser emphasized, these technological advances and, one might add the ways of thinking associated with them, changed profoundly the way physicians interacted with their patients. The position of psychiatry in this history is somewhat anomalous. One element (although not the only one) that has helped shape modern psychiatry is the failure of this technology to deal with many psychological and psychiatric disturbances. In a very real sense, it can be said that it was the failure of medical specialists, chiefly neurologists, to locate the “seat” of psychiatric disorders in the 19<sup>th</sup> century that led to psychoanalysis and psychotherapy. The psychiatrists and other mental health professionals who attempted to work with these recalcitrant disorders were forced to return to the earlier emphasis on patient narrative which modern medicine increasingly eschewed. At the same time, there was a push in other quarters in psychiatry, notably biological psychiatry, to demonstrate that psychiatric disorders were diseases like other diseases. In my comments, I will examine this ambiguous and contradictory heritage, and also look more closely at two of the main technologically based developments which have influenced the current search for the seat of psychiatric disorders, genetics and neuroscience.

**43. Psychiatric Taxonomies: The Case of Personality Disorders**

Robert Wilson, University of Alberta, Canada (rob.wilson@ualberta.ca)

In this impulse presentation, I would like to do discuss psychiatric taxonomies, one focused especially on the “personality disorders” and their pending revision in the Diagnostics and Statistical Manual of Mental Disorders (DSM-V) to “personality types”, on borderline personality disorders, and on Post-traumatic Stress Disorders (PTSD). These are all categories that Dr. Judith Herman has made significantly contributions to in connection with her work on sexual trauma. The discussion of these

categories fit neatly with issues about reductionism – in essence, that any attempts to reduce these kinds of important categories to neuroscientific ones, or to have them shaped in important ways by findings from neuroscience, are almost certain to be pre-empted by what an understanding of the history of those categories reveals about their nature.

#### **44. Neuropsychoanalysis – The cases “for” and “against” an interdisciplinary field**

Katherine Harper, York University and Toronto Institute for Contemporary Psychoanalysis  
(harper@yorku.ca)

On January 13, 2011, at the Waldorf Astoria in New York City, Nobel Laureate Eric Kandel stepped up to the podium at the winter meeting of the American Psychoanalytic Association and stated, “If psychoanalysis is going to survive, it must incorporate neuroscience” (Arehart-Treichel, 2011). Kandel’s (1999) message, however, is not novel to those working in the field of neuropsychoanalysis. In 1999, neuropsychoanalysis became an institutionalized field of study when the periodical *Neuropsychoanalysis: An Interdisciplinary Journal for Psychoanalysis and the Neurosciences* was launched. But, what exactly is neuropsychoanalysis, and why has it materialized at this point in time with Freud’s neurological neural network model, *Project for a Scientific Psychology* (1895), acting as its mascot? In the editor’s introduction to the inaugural issue of the journal *Neuropsychoanalysis*, Solms and Nersessian (1999) state that the goal of this new journal is to create a dialogue between psychoanalysis and neuroscience and provide a forum to integrate these perspectives. Moreover, they believe that Freud’s dream of creating a scientific psychology is still alive and that the integration of psychoanalysis and neuroscience can benefit both of these fields. While there are strong supporters of this movement, those who believe that psychoanalysis can have a neurological scientific foundation if it empiricises its theories (Kandel, 1999; Panksepp, 1999a, 1999b, 2000; Pribram & Gill, 1976; Reiser, 1984; Schore, 2003; Solms & Nersessian, 1999), there are others who meet this integration with skepticism, fearing reductionism and suggesting that the “biologizing” of Freud or “analyzing” of neuroscience can serve no benefit to either of these respective fields or the patients seeking care within their domains (Blass & Carmeli, 2007; Kitcher, 1995; Pulver, 2001). My commentary for this panel will begin with a very brief outline of the field of neuropsychoanalysis, explaining what it really is, and then move on to explore some of the mind-brain issues and reductionism controversies surrounding the idea of making psychoanalysis a more interdisciplinary field.

#### **International Book Viewing Address:**

#### **The History of Neuroscience Book Collection at the University of Calgary**

Robert M. Gordon, Baylor College of Medicine, USA (robook@aol.com)

In December, 2009 the University of Calgary and the Hotchkiss Brain Institute acquired a 2400 item rare book collection with over 1800 items related to the history of neurology and the neurosciences. The books and journal article were obtained from an “armchair collector”, a neurologist, who had painstakingly collected from book dealers and private sources many of the most important works in the evolution of the neurosciences. This collection, now named the Mackie Family History of Neuroscience Book Collection was received with great enthusiasm by the faculty of the University and the researchers at the Brain Institute, who planned to integrate the material in the collection with the research and teaching at the Institute, the medical school and the neuroscience undergraduate program. Digitization of the collection is underway in order to make it universally accessible on the internet, and the bulk of the collection itself is accessible in an especially dedicated room at the medical school library. Some of the rarer and more fragile items are housed separately in the special collections area of the new university library. The purpose of this talk is to acquaint the members of the ISHN and CHEIRON with the nature and some of the highlights of the collection so that its availability as a resource can become better known. There are more than 1800 works representing landmark discoveries, first descriptions and first editions as

well as less important but interesting volumes by or about important neuroscientists from the 1600s through the 20th century. In addition there are over 250 works of histories, essays and biographies mostly pertaining to neuroscience but also to other medical topics. There are smaller collections of important and interesting non-neurological medical works including significant collections of Osler material and Benjamin Rush material, books on book collecting and medical bibliographies, and finally a complete run of the Classics of Neurology and Neurosurgery Library. The collection is rich in neurological first descriptions and first editions of anatomical and physiological discoveries, terminology, diseases, syndromes, physical findings, treatments, diagnostic tests and landmark textbooks from the 17th through the 20th century. There are numerous important rare and scarce items in the collection including first editions of Descartes, Willis, Vieussens, Ridley, Pacchioni, Cotugno, Whytt, Monro, Galvani, Vic d'Azyr, Cheyne, Hooper, Carswell, Purkyne, Baillarger, Helmholtz, Broca, Fritsch and Hitzig, Huntington, Caton, Thudicum, Tourette, Golgi, Wernicke, Tay, Sachs, Nansen, Babinski, Quincke, Brodmann, Alzheimer, Gajdusek, Hounsfield, Prusiner, and many others. There are large collections of important authors such as Gowers, Cajal, Egas Moniz, Cushing, Dandy, Hammond, S. Weir Mitchell, Brown-Sequard, Sherrington, Freud (neurological works), Charcot, and others. Several selected works from the collection will be briefly shown and discussed.

#### **A Special Music Presentation:**

##### **A Life in Major and Minor Keys: Frédéric Chopin from a Psychiatric Perspective**

Axel Karenberg, University of Cologne, Germany (ajg02@uni-koeln.de)

The 200th anniversary of Fryderyk Chopin's birth offers the opportunity to present a sketchy outline of his "case history" as well as an overview of the abundant medical literature on the subject. What is striking is that nearly all medical papers deal exclusively with the identification of the deadly disease(s) he felt victim to (tuberculosis, cystic fibrosis, alpha 1-antitrypsin deficiency?). Moreover, several neuropsychiatric publications make an effort to assign his emotional condition to a modern diagnostic category (e.g. personality disorder, bipolar psychosis, temporal lobe epilepsy, etc.). Because of the impossibility of proving such hypotheses all these studies produce nothing more than erudite speculations. Instead of that my presentation will aim at incorporating the cultural and medical context of the early 19th century in order to explore new possibilities of medical biography. The talk will be accompanied by selected pieces of Chopin's music.

#### **Featured Lecture II:**

##### **Anthropology and the History of Experimental Psychology**

Emily Martin, New York University, USA (em81@nyu.edu)

Historians of psychology have described how the "introspection" of early Wundtian psychology largely came to be ruled out of experimental settings by the mid 20th century. In this paper I take a fresh look at the years before this process was complete -- from the vantage point of early anthropological and psychological field expeditions. The psychological research conducted during and after the Cambridge expedition to the Torres Straits Islands in 1898 had a certain impact on Ludwig Wittgenstein, who, among other things, became an important critic of experimental psychology. The paper begins by exploring the particular meanings "introspection" held for Wundt and his students. I look closely at the research of J. M. Cattell, a student of Wundt's, who arguably began the process of removing the need for "introspection" from the experimental setting. A very different path was followed by C.S. Myers, a member of the Cambridge expedition to the Torres Straits Islands, an event that has been considered foundational for fieldwork in social anthropology. Since the expedition's psychologists assumed that the social and natural environment determined the way the mind perceived the world, they also assumed that after immersion in the daily life of villagers on the islands, they could serve as appropriate experimental

subjects comparable to the native inhabitants. Their introspective reports of the time they took to react to a stimulus were measured and compared to the reports of native Torres Straits Islanders. Myers' studies in the Torres Straits Islands and later in the Cambridge Laboratory of Experimental Psychology (1912) focused on aural perception in music and rhythm. Throughout his career, well into the 1930s, Myers stressed the aesthetic aspects of music and rhythm and the centrality of their "intra-subjective appeal". One significant impact of Myers' work was that he worked for a time with Ludwig Wittgenstein in Cambridge. Wittgenstein demonstrated an experiment for the meetings of the British Psychological Society, introduced by Myers, which was recorded in the proceedings of the British Psychological Society in 1912. Wittgenstein's writings on psychology have played a significant part in critiques of the assumptions in contemporary experimental psychology. These critiques can be made even sharper if we consider Wittgenstein's thought in light of his knowledge of anthropological research. The references to "forms of life" and "thought experiments" about tribes, peoples, and creatures that pervade his later writings could well have been informed by anthropological sensibilities, owing something concrete to the early results of the Cambridge expedition. His frequent mention of rhythm and musical themes in our language and gestures suggest legacies of his involvement with Myers. His insistence that training and imitating are keys to understanding the normative aspects of social life seem closely related to the assumptions about the malleability of mind that the Cambridge researchers took to the Torres Straits Islands. Seeing Wittgenstein's work in light of his acquaintance with Myers' research also gives further support to scholars who have argued that his position in his later writings is deeply materialist rather than politically conservative, resting as it does "on a particular view of the relations between discourse and its material conditions". In his later writings, Wittgenstein frequently referred to "anthropological facts" and "anthropological phenomena." He articulated some of the central tenets of cultural anthropological analysis. His efforts to move the ground of analysis from philosophy to anthropology take on greater force in the light of his acquaintance with the early history of anthropology.

**Saturday Morning, June 18, 2011**

### **History of Psychiatry:**

#### **45. "No Satisfactory Costume": The Role of Clothing in Asylum Practices**

Jennifer L. Bazar, York University, Canada (jbazar@yorku.ca)

For a short period of time towards the end of the nineteenth century, male attendants at Danvers Hospital wore a silver shield pinned to their suits to help distinguish them from the male patients under their watch. The attempt was one of several made to provide a way to visually differentiate between male attendants and patients that would parallel the way female attendants and patients were distinguished thanks to the nursing uniform. Patients, for their part, did not typically have a uniform per-se but did face a divide within their ranks based on who had personal clothing provided to them by outside support and who wore the garments provided by the institution. This partition can be stretched farther when those who had private storage rights over their personal belongings are considered against those who had to select from the communal pile. Looking at these examples more generally, clothing – from the provenance of garments to access rights to the types of dress permitted - contributed to the development and determination of the social structure of an institution's inhabitants that was more complex than ward allocations, gender segregation, or the staff-patient split. By examining the influence of seemingly mundane factors such as clothing in asylums, I argue that we gain a more nuanced and enriched understanding of the daily, lived experiences of both staff and patients. This paper is based on my doctoral dissertation in which I look at the ways in which the environment of asylums determined many of the practices within North American institutions.

#### **46. What Did Madness Mean to an Ancient Roman?**

Peter Toohey, University of Calgary, Canada (ptoohey@ucalgary.ca)

In a world where there was little assistance to be had for the mad (and “madness” is the most accurate term to use in this context, for the Latin word for the condition was *furor* which is to be linked to the English “fury”), it appears that law offered the one achievable means by which problems, as varied as violence, the making of wills, marriage, or who should look after those subject to *furor*, could be partially resolved. Religion, society at large, and even medicine had little that they could provide: superstition, an absence of police or prisons, and the non-existence of hospitals saw to this. So it is that the vast legal code preserved as the Digest of Justinian (released in 529-534 CE, but providing material from the previous three and a half centuries) offers the best source of data for the depiction of madness in ancient Rome. Because of the centrality of law for the Roman response to madness the Digest provides a rich quarry of information on the position of the mentally ill in this pre-modern society. What then does the Digest tell us of the meaning of madness for an ancient Roman? It was, as its label would suggest, frequently a violent state and this, rather than say depression, more concerned the jurists. But violence is not meaning. Meaning seems to relate not to violence but to three other aspects of the condition, voice, perception, and a sense of self. For the jurists who feature in the Digest those who are subject to *furor* cannot speak for themselves – the Digest therefore will liken them to Roman captives in foreign lands, or young children, or animals, or to even inanimate objects. The *furiosi*, in addition, just like Roman captives in foreign lands, or young children, or animals, or inanimate objects also exhibit an absence of volition – they cannot choose to do anything because they are without *intellectus*, the faculty of perception: they are unable to perceive properly what is taking place in their Roman world. Furthermore they appear to lack, according to these legal writers of the Digest, a clear sense of self, a capacity that may be captured by the Latin term *sensus*. All of this may give the impression that Roman legislators were unsympathetic to mad. Far from it, the implication of the lack, for the *furiosi*, of voice, perception, and sense of self meant that they were treated in legal matters as innocents and as people requiring protection. According to the Digest they could not be held responsible for their actions, no matter how outlandish. They ought to be treated as individuals who were absent (*absentes*) or asleep (*quiescentes*) who required the protection of their families.

47.

#### Flute and Madness

Lorenzo Lorusso; Bruno Falconi and Alessandro Porro, University of Brescia, Italy;  
Antonia Francesca Franchini, University of Milan, Italy (lorusso.lorenzo@gmail.com)

The flute and its music are present in the mythology, with an influence on mind, and are related to the perversion or creative power of establishing an order. These mind alterations are also often represented as madness, i.e. a change of the rule of order, peace, discipline and human language. This could also be due to the observation of changes in the face (especially the swelling of the cheeks) during the performance of music. This observation may be conducted today and perhaps, not coincidentally, during one of the oldest wind instruments: launeddas of Sardinia, in Italy. The special needs of a particular sound had to be guaranteed by continuously breathing through the oral cavity to ensure the availability of air content also during nasal breathing (a function similar to that in bottle of the bagpipe). The other side of medicine (in terms of pathogenesis) can only take us to the dimension of mental illness and the effect of change in the mental status (and in the Dionysian rites, for example). Hence, the connection to the feminine dimension: the pitch is very short, both as regards the structure of the Greek *polis*, or in a broader sense. Not surprisingly, then, even in the nineteenth-century melodrama, some aspects of pathology (or tied at the ends of life – especially in women) are related to the presence of the flute (or other instruments, such as the glass harmonica). This instrument was played only by women and was set aside because it was thought could lead to mental state of madness. Different representations of madness in opera music were popular in the Romantic period, when predominantly the flute was used. For example, *Lucia di Lammermoor* by Gaetano Donizetti is a piece of music of the mad scene of Lucia, which was originally produced for the glass harmonica. Nevertheless, it eventually became written for the flute: a change of instrument had occurred, while the influences on the nervous system characteristics

stayed the same. On the contrary, in the Magic Flute by Wolfgang Amadeus Mozart the instrument creates a magical atmosphere through the use of various opera characters that are associated with crazy love. The flute continued to have an important role in the 17th century and in the 19th century by different composers (A. Thomas, C. Gounod, J. Messnet, J. Offembach, F. Borne, G. Verdi, J Strauss, A. Vivaldi) and it became further associated with surreal situations and extreme emotions of the mind.

#### **48. Psychiatry and Social Engineering in the Canadian and American Armies during the Second World War**

Russ Benneweis, University of Calgary, Canada (drbennew@ucalgary.ca)

The theme of this presentation will be the utilization of psychology, psychiatry and neuropsychiatry in the Canadian and American Armies during the Second World War. Composed of two sub-themes, the intention is to illustrate the means by which civic social systems shaped military organizations during World War Two. In both nations psychological testing with psychiatric follow-ups were utilized in the selection of manpower. However, once combat was reached, the treatment of 'Battle Exhaustion' casualties was exclusively the domain of the neuropsychiatrist. This presentation is part of a larger doctoral dissertation comparing the 3rd Canadian and 29th US Divisions, a socio-military work intended to illustrate the degree that Canadian social and military history are intertwined and to provide a better answer to the question of what it is to be Canadian more definitively, than simply, 'not American.' Although Canada and the United States entered the Second World War under different circumstances, both nations were determined to husband manpower resources in a more-efficient manner than had been achieved in the Great War. The result was a remarkably similar process in which both nations sought to utilize social and scientific engineering to profile inductees and volunteers in order to maximize efficiency in their armed forces, and equally importantly, in their industrial complexes. In Canada especially, this often pitted psychologist against psychiatrist as the latter remained unconvinced for the duration of the war that it was possible to single out upon induction those predisposed to breakdown under the stresses of combat. However, with the ardent support of the Canadian Army's top commander for most of the war, A.G.L. McNaughton, personnel screening upon induction would continue unabated. In 1941, the Canadians took social engineering to a whole new level with the introduction of the Directorate of Personnel Selection. Personnel Selection Officers would eventually comb the entire Canadian Army and reject thousands of soldiers, often those with more than two years of service. In the American system, recruits were categorized based on the psychologically based Army General Classification Test after being screened by a medical board. Until October, 1944, it can be differentiated from the Canadian system in that the role of the social engineer stopped at this point. Those recruits deemed as potentially psychiatric cases were forwarded to a psychiatrist, often a civilian general practitioner due to shortages of trained military psychiatrists, who was given roughly four minutes to decide the recruit's fate. No standardized test system was implemented for the duration of the war; each instance was left to the judgement of the local psychiatrist. If any indication of psychiatric distress was found, standard procedure called for the rejection of the recruit. In April 1944, standards were lowered dramatically due to high combat casualties suffered by the US Army. The second portion of this presentation will illustrate the methods utilized by the two nations in the treatment of battle exhaustion casualties. The American system was much more systemized than Canadian methodology in that a pre-combat plan was established that would see psychiatric casualties treated at battalion, divisional and army level treatment centres, all with the goal of returning as many traumatized soldiers to their unit as quickly as possible. On the other hand, the Canadians chose not to establish divisional psychiatric casualty centres, maintaining instead that battle exhaustion would not occur in great numbers in the Canadian Army. Mostly ad-hoc measures were implemented when the Canadian Army found itself faced with as many as twenty percent of its overall casualties being of the psychiatric nature after the invasion of Normandy in June, 1944.

**Perspectives on Gender:**

**49. The Suppression of Feeling: “Women’s work” on Emotion in Early to Mid-twentieth Century Psychology**

Elizabeth B. Johnston, Sarah Lawrence College, USA (Johnston@mail.sl.c.edu)  
and Ann Johnson, University of St. Thomas, USA (ebj@sl.c.edu)

Researchers reviewing the history of work in the burgeoning subfields of social and affective neuroscience have noted the dearth of work on emotion and emotion regulation in early to mid-twentieth century psychology (Dalglish, 2004; Gross, 2007). Emotion researchers Maria Gendron and Lisa Barrett divided the history of emotions research into three periods: the Golden Years of luminaries such as William James and Charles Darwin in the second half of the nineteenth century, the Dark Ages between 1900 and 1959 during behaviorist domination of the field, and the modern period that culminates in the current upsurge of work on emotion. Gendron and Barrett’s account of the history of emotions research is confirmed by analysis of a PsycArticles search on the term “emotion”. There was some work on emotion throughout the dark ages but it was dwarfed by comparison with other subject matter (Fernberger, 1938). Yet, Gendron and Barrett argued that the dark ages were not as dimly illuminated as most histories suggest by identifying 13 researchers actively working on the topic. Only one of the researchers identified by Gendron and Barrett was female: Elizabeth Duffy. A PsycArticles search reveals that Duffy was an active participant in the debates of her time, arguing vigorously that the lay term “emotion” should be abandoned by psychology (Duffy, 1934, 1941, 1948). Her work has not received much attention, now or then. Gendron and Barrett recovered Duffy’s contribution through questioning the assertion that a “dark ages” period existed in the history of emotions research; they were not motivated by questions of gender. Yet their discovery of suppressed contributions can be significantly augmented by casting a gendered lens on the historical period under review. Here we argue that there is an unwritten history of important and influential work in emotion research that lies outside the line of sight of theorists reviewing the field. We mean the “women’s work” with children and clinical populations that has been dismissed as less prestigious and influential. E.G. Boring’s 1951 commentary on “the woman problem” in *American Psychologist* crystallized a perception that still holds regarding what counts as prestigious in the psychological literature, and its connection to gendered patterns of historical analysis. From the 1920s through the 1950s women psychologists were regularly tracked into applied professions dealing with children and families and made significant contributions there, but those contributions were discounted as non-prestigious because they represented interest in “the particular” rather than in “broad generalities,” as Boring prescribed. We want to reassess that “women’s work” as a source of information about the changing understanding of the psychology of emotions, and particularly how psychological research was circulated to a wider audience than that of professional psychologists. To do so we examine the work of women psychologists like Lois Barclay Murphy, Florence Goodenough, Mary Cover Jones, and Rose Franzblau. Some of these contributors influenced the field of emotions research through examining the acquisition of emotional behavior, documenting emotional traits previously thought unavailable to children, and investigating emotional regulation strategies in children and their parents. In addition, we argue that the “women’s work” of disseminating psychological research on emotions to the public (through vehicles such as the parent education movement and newspaper advice columns) led to significant cultural shifts in how we view children and the parent-child relationship. The downplaying of applied work (and hence, women’s contributions) is a staple of disciplinary histories in the sciences. We argue here that accounts of emotion research that omit this angle neglect a rich story of the interplay between psychological knowledge and cultural belief and practice – a story that deserves to be included in the historical record.

## **50. Psychology's Foremothers and "Pragmatic" Feminism**

Ana Petrovic-Poljak, University of Calgary, Canada (apetrovi@ucalgary.ca)

The history of psychology is a gendered one. Despite the nascence of the field of psychology, the accounts of its history have been prolific. However, the contributions of women psychologists have often been overlooked. As noted by Russo and Denmark (1987) "the contributions of women psychologists have been largely unrecognized, undervalued and invisible in historical accounts"(p. 279). However, women psychologists' contributions to the development and growth of psychology have been resurrected in the recent decades and have led to recognition of their widespread participation and immense contribution to the field of psychology (Scarborough & Furumoto, 1987). Although the roles and contributions of women to the development and growth of psychology as a field have been noted, their contributions to the plight of women have underwhelmed many of the historians who compile the foremothers' accounts (Johnson & Johnston, 2008; O'Connell & Russo, 1983; Scarborough & Furumoto, 1987). Historians are often perplexed by the lack of explicit feminist objectives and noted that as a group the foremothers "suffered from low feminist consciousness" (Johnston & Johnson, 2008; Scarborough & Furumoto, 1987). Others have noted that the foremothers were largely ambivalent towards feminist objectives (O'Connell & Russo, 1980). What is important to note is that historical reflections are constructed by those who tell them. Therefore, when contemporary women in psychology reconstruct the histories of psychology's foremothers it is undoubtedly from a contemporary feminist stance. Although radical feminism was not prevalent among psychology's foremothers, the foremothers may be considered feminists in liberal terms. The choices they made in becoming scientists and the actions they took in order to gain acceptance and to establish the continuity for women in psychology lends support to this notion. However, the lack of organized opposition to the discrimination that these women experienced is what particularly appears to perplex historians. The foremothers took a largely individualized approach to countering discrimination and leading lives that were exemplary of "free women". External pressures, both social and professional, may have resulted in these women to adopt "strategies of silence and indirect action" (Morawski & Agronick, 1991, p. 571). Although not radical in nature, the foremothers of psychology sought logical and effective solutions to the barriers they faced, which has significantly contributed to establishing women as professionals in psychology. In this manner, the brand of feminism that they employed is of a pragmatic sort that underscores the importance of flexibility and adaptability in order to attain one's goals. The current paper seeks to argue that the brand of feminism that was encouraged by first and second generation women psychologists in the United States from late 19th to mid 20th century was more pragmatic in nature and effective in the establishment of women in psychology, through their maintenance of marriage, children and family, by actively maintaining employment and utilizing the "women's sphere" within psychology, and their opposition to oppression of women through research.

## **51. Despite the Odds: Early Jewish Women in Canadian Psychology**

Charmaine Thomas, University of Calgary, Canada (charmaine.thomas@ucalgary.ca)

The role of Canadian women in psychology's development has only recently become of greater interest to historians of psychology. Much remains unknown about their contributions, and even less is known about minority women's role. This paper presents the story of a little known demographic in the development of Canadian psychology: Canada's early Jewish female psychologists. This paper situates itself in the changing social, political, and economic contexts of Canadian history from 1920 – 1950 and traces the experience of being Jewish, female, and engaged in psychology. The life of Dr. Blossom

Wigdor is examined to provide insight into the factors that both impeded and assisted her academic and professional engagement in psychology as a Jewish woman during this time period. To be both Jewish and female represented dual oppression: early Jewish women faced twice the barriers in pursuing higher education and professional careers during this time period. Given such significant obstacles, how were these early women able to establish careers, despite the multitude of barriers they faced? It is believed that several factors contributed to Wigdor's ability to access academic and professional opportunities: the influence of Jewish values; connection to mentors; marriage to a feminist; the effects of War; and the ability to fill a niche. Wigdor represents a group of Jewish women with remarkable resilience and fortitude within the story of the psychology's development in Canada, and much can be learned from their determination to create a place for themselves in psychology, despite the odds. It is hoped that this paper will be a starting point in giving psychology's Jewish foremothers voice, and in situating their place in the history of Canadian psychology.

## **52. Controlling Female “Sexual Delinquents”: Eugenic and Progressive Reformers’ Use of Psychological Testing**

Ingrid Farreras, Hood College, USA (farreras@hood.edu)

The first law providing for the commitment of “feeble-minded” individuals in the United States was passed in 1915, in the state of Illinois. House Bill 655 not only allowed for the permanent, involuntary institutionalization of feeble-minded individuals, but it shifted the commitment and discharge authority from the institution superintendents to the courts. Clara Harrison Town, a graduate of Lightner Witmer, and the state psychologist at the second largest institution for feeble-minded individuals in the country, was instrumental in this law passing and in ensuring that psychologists, for the first time, be viewed as court “experts” when testifying as to the feeble-mindedness of individuals. Within the context of eugenics, the Progressive movement, and intelligence testing, I will argue in this paper that the 1915 commitment law was an example of a state-sponsored initiative to address various social problems. While Progressive reformers tended to subscribe to environmental causes and solutions to social ills, and eugenic reformers tended to hold hereditary views and solutions to such problems, both shared the belief that science should inform social policy and that the government or state should intervene to curb or regulate these problems. The industrial and urban growth of the late 19th century brought about a significant threat to established gender roles. The female labor force in Chicago increased five-fold between 1880-1930. The poor or working class woman who now worked in factories, department stores, and restaurants now had access to a broader social environment that included dance halls, movie theatres, and city streets. This new, unsupervised socialization led to behavior that deviated from and undermined 19th century Victorian ideals of female sexual purity, leading to a fear that mental and moral degeneracy would spread to future generations. As such, eugenicists, backed by Progressive reformers, used scientific (as opposed to the then moral) discourse to link sexual deviance to mental defect – as measured by psychologists’ new mental tests – and campaigned for the institutionalization of the mentally defective as a way to control reproduction of “the unfit” and curb racial decline. While the language used in House Bill 655 was gender-neutral and both male and female feeble-minded individuals were institutionalized, and while reformers attributed feeble-mindedness to all social ills such as alcoholism, poverty, (sexual) vice, and crime, it was female “sexual delinquency” in particular that attracted the most interest, anxiety, and concern, and filled the most pages of the literature at the time. Under the explicit agenda of protecting not only these fecund feeble-minded young women, but also of protecting the community they lived in from moral degeneracy, both eugenic and Progressive reformers used the science behind psychological testing to effectively control the sexual behavior of young women who deviated from normative, white, middle-class Victorian notions of femininity.

**Perspectives on Gender:**

### **53. What is Agricultural Economics?: Construction and Deconstruction of Disciplinary Boundaries (Cases of Russia and France)**

Olessia Kirtchik, State University, Russia (olessia@kirtchik.com)

Agricultural economics is a fascinating object for a comparative history of scientific disciplines, as it historically holds a particular place between more “traditional” natural and social sciences. This specific origin is, in my hypothesis, responsible for an ambiguous position of this discipline *vis-à-vis* fundamental tensions of economic sciences: universal vs. local knowledge, mathematical rigor vs. practical needs, etc. Agricultural economics has known an active development in the 1920s: during this period the analysis of agricultural markets was, for instance, the most formalized field of the economic science in the United States and in Soviet Union; agricultural economists were also the first among their profession to be regularly employed in the State administration. At the same time, this “practical” orientation is responsible for a relatively low position of agricultural economics in the academic hierarchy and for a lack of legitimacy within economic sciences after the Second World War. A methodological and theoretical alignment to mainstream economics was used by agricultural economists, firstly in the United States, as a main strategy to regain legitimacy. Consequently, agricultural economics has essentially become a branch of general economics applied to the farm business. Nowadays, a question whether agricultural economics represents any specificity would be of no relevance for an American economist specialized in agricultural sector, as well as for mainstream agricultural economists in other countries. My comparative historical research aims at understanding a recent transformation of the discipline in Russia and in France where a question of specificity of the agricultural economics still represents a matter of a more or less vigorous scientific, professional and political debate. The concepts of disciplinary (Fuller) or symbolic boundaries (Lamont & Molnar, Mallard) are useful to understand this collision in which a question of professional identity (what it means to be an agricultural economist?) is inseparably linked to a political one (to what extent agriculture may benefit from a particular attention of the State? what politics is best suited to assure a sustainable development of agriculture?). This theoretical framework permits to avoid reductionist interpretations of the conflict in exclusively political or ideological terms. Rather than a conflict of an ideology and an objective science, it should be seen as a contention between different groups of professionals for a definition of their discipline (a boundary-work, in terms of Gieryn). A political and institutional context of these jurisdictional and symbolic battles, the relations between academia, administration and business are constitutive of what one may call a national style of (agricultural) economics (Fourcade) which is historically changing itself. This paper is based on a study of agricultural economists conducted in Russia as part of my Ph.D. thesis and extended to France in a post-doctoral project (financed in 2009-2010 by a grant “Diderot” from *La Maison des Sciences de l’homme, Paris*). I have made numerous interviews with agricultural economists in both countries, as well as studied an extensive body of written sources (writings of economists, biographies and memoirs, various texts dealing with histories of institutions). A comparative research has, on its earlier stages, benefited from discussions during workshops and research seminars, but its main findings were not yet published or presented.

### **54. Do Good (Sub-Disciplinary) Fences Make Good Neighbours? Tracing the Paradigmatic Rise and Fall of Health Psychology (1960-2010)**

Ian Lubek; Asma Hanif; William Salmon and Michelle Green, University of Guelph, Canada  
(ilubek@uoguelph.ca)

The current view of “Health Psychology” puts it on an interdisciplinary terrain, comprising behavioural and community perspectives, overlapping with public health, behavioural medicine, and critical health psychology (Marks, et al, 2005). During the 1960s, clinical psychology had, in dialogues with psychiatry,

already defined research and practice boundaries for research questions and careers dealing with “mental health”. Social psychologists had in fact begun exploring health issues during the war effort-- nutrition, stress, “civilian morale”-- and continuing on through the 1940s and 1950s, with classic work by Kurt Lewin, Carl Hovland and other social psychologists who looked at behavior and attitude change. However, by 1960, top-down research agendas included the U.S. Surgeon General's naming of specific health risks with social origins (television and violence, smoking, drug use, alcohol overuse), and by the mid-1960s, the National Center for Prevention and Control of Alcoholism was established as part of NIMH; a research program on drug abuse was inaugurated within NIMH ( the Center for Studies of Narcotic and Drug Abuse), and funding agencies arose in the 1970s as the National Institute on Alcohol Abuse and Alcoholism (NIAAA) and the National Institute of Drug Abuse (NIDA). Discussions of health-related research topics and complete chapters on health psychology began appearing within social psychology textbooks (1960-2000). A preliminary study of 40 social psychology textbooks shows that, on average, 3.5% of their pages in the 1960s, 1970s and 1980s discussed health issues, compared to 5.5% in the 1990s. We examined the flagship *Journal of Personality and Social Psychology* (JPSP) for articles (1960-2010) dealing with health psychology, alcohol, drugs, marijuana, smoking, drinking, cancer, pain, stress, etc. (about 8% of all articles). The number of articles increased until 1985-89, then declined as new specialized outlets arose. A recent online search found specialty journals for the following topics: cancer research led with 84 specialty journals, followed by research on “drugs” with 27, while “pain” had 18; there were a further 9 “Alcohol”, 6 “stress”, and 2 “tobacco/smoking” research outlets. The American Psychological Association formally established “Health Psychology” as its Division 38 in 1978, adding professional legitimization with a “sub-disciplinary” identity. In 1982, *Health Psychology* began as the division's official journal (Matarazzo, 1982). U.S. government funding from 5 sources --NIAAA, NIDA, NIMH, NIH and NSF-- supported 54% of the JPSP health-related studies.

### **55. In Spite of the More Zealous Psychologists: The Discourse of Common Sense in American Child-Rearing Literature**

Alicia Puglionesi, Johns Hopkins University, USA (apuglio1@jhmi.edu)

In 1941, psychiatrist Leo Kanner posed a question to American women in his characteristic high-flung rhetorical style: “Whence rise the tides of uncertainty, apprehensiveness, and fear of wrongdoing among the mothers of children?” Responsibility for this maternal anxiety epidemic lay with “the more zealous psychologists” - the experts who for the past half-century had produced a dizzying succession of fads in scientific child-rearing. Mothers, according to a trope that developed in tandem with the field of child study, faced a paralyzing array of parenting recommendations that only exacerbated their fear of failure. Kanner offered no child-rearing panacea; rather, he pleaded that the parents and doctors “go forward with common sense.” What he meant by “common sense,” and its relation to the questions facing psychiatry in mid-century America, constitute the core of this investigation. I will argue that the notion of “common sense” child-rearing became politically charged in the context of a professionalizing child-development field and the popular embrace of Freudian psychoanalysis. It could be mobilized in opposition to scientific reductionism, ego psychology, and even the proliferation of advice literature, evoking a discourse of hardy American self-sufficiency stretching back to Emerson and Thoreau. For Kanner, common sense was entwined with the concept of “naturalness” defined against the artificiality and bureaucratization of modern American life. Superficially, the rhetoric of common sense appeared to realign psychiatrists with the overwhelmed mothers whom they stood accused of alienating – however, as the case of Kanner illustrates, it could also voice a subtle condemnation of womanhood in a rapidly changing society. This paper examines Leo Kanner’s self-fashioning as a child-rearing expert, placing him within an evolving psychiatric discourse that embedded normative themes of gender, nature, and individuality in its production of the “healthy” subject. Kanner’s career closely tracks the rising star of early-twentieth century psychiatry: he began his work in the state asylum system, joined the Phipps Clinic at Johns Hopkins (signing on to Adolph Meyer’s program of psychobiology), and by the 1930s dealt

largely with what Freud termed the “psychopathology of everyday life.” Although he joined many American psychiatrists in criticizing psychoanalytic theory, Kanner was engaged in a sympathetic project of relocating psychiatry from the asylum to the living room. Indeed, Kanner’s popular books on child-rearing could be found in living rooms across the United States. His counsel aimed to soothe the frazzled nerves of mothers, anticipating Benjamin Spock’s famous 1943 injunction, “You know more than you think you do...don’t be overawed by what the experts say.” The empowering tone of this rhetoric was deceptive, however, as his advice was deeply rooted in a quasi-ethnological, quasi-Romantic conception of what constituted “natural” knowledge in women. It was no coincidence that Kanner would be the first clinician to diagnose and study the condition he labeled “infantile autism,” endemic to the children of educated, professional mothers who had exchanged warm maternal instinct for the cold rationality of career advancement. Ultimately, Kanner’s rhetoric of common sense sought to equate the “sensible” in everyday life with the “normative” in his professional vision of psychiatry, the family, and human nature.

**Poster Presentation Session:**

**56. Experimental Antecedents to Psychoanalysis in the Career of Robert M. Lindner**

David Devonis, University Lamoni, USA (devonis@graceland.edu)

Robert Lindner is probably best remembered for creating a memorable American cultural concept by titling his 1944 book ‘Rebel without a Cause’ (Lindner, 1944). While the specific influence of that book on the famous 1955 film of the same name extended little further than its title, Lindner’s conceptualization of psychopathy fed into a growing general interest in that subject in psychology, psychiatry, and society at large that came fully of age in the 1980’s. Lindner is an interesting case not only for the conceptual history of psychiatry but also the history of psychology. For, though Lindner eventually became a noted psychoanalyst, associated with Theodor Reik and the NPAP (National Psychological Association for Psychoanalysis), he was fledged in academic experimental psychology. Lindner’s psychological background illustrates the degree of cross-fertilization and hybridization that existed in psychology in the period 1920-1940. Before Lindner became fully involved with problems of psychopathy through his work at the penitentiary at Lewisburg, Pennsylvania, he was a master’s and Ph. D. student at Cornell. There, his mentor and collaborator was Madison Bentley. With Bentley, Lindner, Assistant in the Cornell Department between 1936 and 1938, produced a master’s thesis on anticipative apprehension (Lindner, 1936) and a 1938 doctoral dissertation on a functional conception of emotion—‘emoving’—published under both Bentley and Lindner’s names (Lindner, 1938; Lindner and Bentley, 1939). But before this, Lindner’s undergraduate experience was even more formative and persistent: he began his career at Bucknell in 1932 where psychology was the province of the generalist Philip L. Harriman, chair of the department there from the early ‘30s to 1958 (Candland, 2004). Harriman was interested in everything psychological from the relation of behaviorism and logic (Harriman, 1936) to Rorschach testing (Harriman, 1935) to multiple personality (Harriman, 1943) to encyclopedic compilation (Harriman, 1946). Without this historical context, Lindner might be seen as radically shifting his viewpoint from experimental psychology to psychoanalysis. However, the persistence of ideas in Lindner’s later work reflecting the early influences of two psychological generalists shows his development to be evolutionary, and also shows a greater affinity between psychiatry, psychoanalysis, and American academic psychology than is usually expected.

**57. A study of Chen Daqi’s An Outline of Psychology (1919)  
: the First Chinese Psychology Textbook**

Chin-Hei Wong, University of Hong Kong, China (chinhei@hku.hk)

Chen Daqi’s An Outline of Psychology (or Xinlixue Dagang, thereafter Outline), written in the year 1918 for his philosophy undergraduate students studying in Beijing University, is now commonly regarded as

the first university-level psychology textbook available in Chinese (for example, see Hu, 2009; Yang, 2000). Sadly, this historical text remained largely unexamined by both historians and psychologists in the West and in China. Two years after its publication in 1920, an advertisement found in the Beijing University Monthly promoting the sale of the 250-page book, has a statement which reads “(the Outline was an attempt at) conciliating and mediating the fundamental differences between the psychological schools of structuralism and functionalism” by “incorporating the strengths of the psychological theories from both of the aforementioned schools” (Beijing University, 1920). This study is an investigation into how Chen Daqi, in his Outline, tried to achieve his stated aim. Methodologically speaking, the text of the Outline is cross-referenced with various major psychological texts available in the West. By comparing Chen’s text (and even figures) work with their original sources (the original texts were mostly in English or German, for example, Wilhelm Wundt’s Outlines of Psychology (1897), William James’s Principles of Psychology (1890) and Edward Titchener’s An Outline of Psychology (1896)), this project situates the work within the transforming China being confronted by a massive knowledge transfer in the 1910s. What is revealed in Chen’s text is a strong sense of pragmatism, the ideology which prevailed over the Chinese academia in the late 1910s. It was also found that problems with the translation of psychological terms, for example the term “mental”, from foreign languages into Chinese posed serious problems for understanding the “new” psychology from the West when transplanted onto the Chinese soil in the early Republican Era.

**58.**

### **Transforming Charles S. Myers**

Sharlene Walbaum, Quinnipiac University, USA (sharlene.walbaum@quinnipiac.edu)

Born in 1873, Charles Samuel Myers was surrounded by change: the industrial revolution was transforming work, social justice movements were gaining momentum, and Wundt would soon establish his lab in Leipzig. Echoes of these changes can be seen in Myers’ work and life story. He would have two transformative experiences, shifting first from medicine to experimental psychology and second from the lab to the applied world. Myers was nearly finished with a medical degree in 1898 when an unusual opportunity arose. He joined an anthropological expedition to Torres Straits and Sarawak. His goal was to identify ethnic differences in sensation and perception. Myers assumed that “primitive” or indigenous people would perform differently because they differed from him geographically, socially, and physically. He did find differences, but realized that language or cultural practices were the best way to explain them. Apparently “simple” psychological experience was complex, yet might be studied scientifically. After the expedition, Myers completed his medical degree, but started studying and teaching Experimental Psychology. He continued his cultural research and branched off into others like psychology of music. Soon, Myers was a full-time lecturer, director of a lab, and had published a textbook, completing his transition from medicine to psychology. At the outbreak of World War I, Myers couldn’t focus on the folk music data in his lab. He wanted to help, but, being over 40, couldn’t enlist. He ended up volunteering for the Royal Army Medical Corp in a field hospital in France. Witnessing the damage of war motivated Myers to apply his scientific training to relieve the suffering of soldiers. In Myers’ war diary, he described “shell shock” symptoms that were assumed to be the result of temporary neurological disruption caused by the concussive effect of an explosion. However, Myers noticed the symptoms in soldiers who had not been near exploding shells and reasoned that it must be psychological. Given his war work, the promise of the lab-based research paled in contrast to the applied needs he could see around him. Myers’ second transformation culminated in the non-profit National Institute of Industrial Psychology. The NIIP became a globally recognized research, resource, and training center for the application of scientific knowledge to occupational life. Myers’ intellectual curiosity and rigor served his profession and society well. He also used these qualities to think critically about his goals and behavior, giving him a life enriched by change.

## **59. Social Class, Pragmatism, & Good Intentions: The Fate of Work in Moral Treatment**

Sharlene Walbaum, Quinnipiac University, USA (sharlene.walbaum@quinnipiac.edu)

From the outset of the moral treatment revolution, practitioners disagreed about work's definition and function. While there were vigorous public debates about, for example, the ethics and practice of restraint, the element of work did not attract the same fiery rhetoric and righteous indignation. There were varying ideas about what constituted "work", who should do it, and why it was valuable, making it difficult to take one simple, dogmatic position. Moral Treatment originated in 1797 with a small community of the Society of Friends in York, England. It soon spread throughout England, Ireland, and Scotland and crossed the Atlantic via the Philadelphia Friends who founded their asylum in 1817. It was adopted at different times and adapted in particular ways, depending on the age of the institution and the wealth of the patients. It took almost sixty years to have an impact on the oldest, most entrenched, institutions like Bethlem in London. Quaker ideas about the meaning and function of work were grounded in their philosophy. They were obligated to help people who were "deprived of reason" to resume a purposeful life. Productive work formed a key part of their moral treatment. Work would "prevent the indulgence of gloomy sensations" for the melancholy or focus the energy of the restless. Regular work responsibilities helped "patients" regain self-control. Products of work – a hoed row or a mended shirt - offered concrete evidence of his or her importance to the social group, thereby enhancing self-respect and a sense of worth. Work paved the way for restored sanity or, if the condition was intractable, a way to live with lunacy. As moral treatment spread to existing asylums, the element of work presented a problem and an opportunity. For urban asylums serving the poor such as Swift's in Dublin, there was little space for outdoor work or even for indoor work-shops. For asylums serving the wealthy such as McLean's in Boston, "work" became a selling point when it was translated into "spiritual and intellectual stimulation". For new pauper asylums, such as Hanwell in London, productive work was a part of a treatment program. For others, such as Wakefield, pauper labor was intentionally used to defray building and maintenance costs. As work was refashioned to suit various purposes, it is reasonable to suggest that its restorative value varied accordingly.

## **60. Nerves and Surgeons at the Hospital School of Surgery in Milan in the 18th Century**

Antonia Francesca Franchini, University of Milan, Italy; Bruno Falconi and Alessandro Porro, University of Brescia, Italy; Paolo Galimberti, Granda Ospedale Maggiore Policlinico, Italy; and Lorenzo Lorusso, Mellino Mellini, Chiari, Italy (lorusso.lorenzo@gmail.com)

In 1456, the Maggiore Hospital of Milan was founded. Later in the 18th century a hospital School of Anatomy and Surgery was open. Paolo Gerolamo Biumi (dead in 1731) was one of the main teachers and considered as an outstanding figure. Biumi wrote a book addressed to surgeons education and entitled *Scrutinio teorico pratico di Notomia, e Cirogia Antica, e Moderna* (Theoretical practical Scrutiny of Anatomy and of ancient and modern Surgery), printed in Milan, by M.P. Malatesta, in 1712). This is a very interesting text, in which he adopted a dialogistical method based on questions and answers. That method was used in the rhetorical and Christian doctrine Schools because it made the teaching and the learning easier. The dialogistical method was an innovation, because before it barbers/surgeons or phlebotomists were required to have a five years-practice and to learn by heart the answers to one hundred of printed questions. Biumi's book is an up-to-date and suitable text for specific pupils (the surgeons). It also includes the wish that Surgery must not separate from Medicine. During the 18th century the dialogistical method was not considered a bar to a complicated and scientifically up-to-date knowledge (comparing with the different condition occurring in not-medical professions). The knowledge and theories of the time considered the nervous structures as vessels, i.e. within the limits of angiology. In Biumi's text we can recognize specific knowledge and competences necessary for the

surgeons. In addition to the knowledge nerves (in particular about the structure of brachial plexus and the upper and lower limbs innervations) Biumi proposes some semeiotic notes, especially about the recognition of nerves traumatic lesions. Biumi considers semeiotics as fundamental, together with the anatomical knowledge. The physiological outlines proposed by Biumi are distinguished according to the kind of lesion (point lesion, nerve partial excision, nerve total excision) and are considered according to the physiological vision of the time, together with the respective uncertainties and inaccuracies. Nevertheless the cited authors (from Sennert to Scultetus/Schultze, from Munnichs to Galen, from Pecquet to Nuck, from Falloppio to Paré), shows us Biumi's will: to suggest substantial and up-to-date knowledge about the subject, following the dictates of the most qualified ultramontane Schools. For this reason Biumi's work testifies the efforts made in the Maggiore Hospital of Milan in order to promote the cultural and scientific education of the surgeons.

## **61. Early Psychiatry in Alberta**

Robert Lampard, Red Deer, Canada (j.robert.lampard@gmail.com)

The first psychiatric beds on the prairies were opened in the Manitoba penitentiary, the year Manitoba became a province – 1871. A purpose built 35 bed psychiatric facility opened in Selkirk in 1884, and a larger one in Brandon in 1892. The Federal Asylum Act of 1905 completed the transfer of psychiatric care to the Provinces, the year Alberta and Saskatchewan were created. As the problems of distance, demand and a tripling of the Alberta population from 1901-1911 mounted, the town of Ponoka applied for the proposed provincial psychiatric hospital. Successful, it opened in 1911 and was already over-capacity by year end. The number of psychiatric patients was compounded by the high number of returning veterans with shell shocked symptoms and syphilis. J.S. Woodsworth, later the founder of the NDP, was appointed by the three Prairie Provinces (1916) to study the anticipated challenges. For Alberta he recommended building the Oliver Mental Hospital for long term psychiatric patients and opening the Provincial Training School for the mentally retarded. Both were in operation by 1923. By 1919 the UFWA's Irene Parlby viewed health care as a right. Elected in 1921, the UFA government ushered in 15 years of health care progress. Progress was enhanced by over 33 years of stability in the Health Minister, Deputy Minister and later Director of Mental Health positions. It helped that the MLA from Ponoka was Premier Brownlee. Legislative Acts were passed that created the first VD Act (1919), allowed two doctors to admit patients (1922), and permitted voluntary admissions (1924). A Travelling Clinic was established (1924) that later included a psychiatrist in the North. A Board of Visitors was established (1925). 1928 was a turning point. In February, Health Minister Hoadley appointed the White/Pattinson Inquiry to investigate State Medicine. The subsequent Hoadley Commission (1932-1935) recommended a subsidized health insurance plan for the province and leaving psychiatric care 100% government funded. The Federal government supported the plan in 1942 and proposed it for Canada in 1945. It became Canadian Medicare in 1968. That same month the government passed the Sexual Sterilization Act. In September, a depressed PMH patient, Dr. Arthur Hobbs, was severely assaulted by an attendant for not eating. He died. The cabinet brought in Dr Clarence Hincks to study psychiatric care in the province. His recommendations fostered the creation of a UAH psychiatric unit, two mental health outpatient clinics, the appointment of a provincial Mental Health Commissioner (Dr. Baragar), the starting of a psychiatric nursing program, and the construction of more beds at Ponoka and Oliver. Dr. Randall MacLean, a 1927 graduate from the new U of A medical school, and the director of the male wards at PMH was approved to take a two year LOA in 1930 to study neurology and psychiatry at Boston, Zurich, and London. Dr. MacLean became the PMH Superintendent in 1936 and the Director of Mental Health for the province in 1948. During his 17 years as Director he increased psychiatric beds by 2400 to peak at 5400 in 1966. He introduced Chlorpromazine in 1954, and with Dr. John McEachern began the Alberta branch of the Canadian Mental Health Association. Dr. MacLean retired in 1965 and passed away in 1976.

**62. Looks Kill the Debate, Have Those Families Gone Now for Lunch?  
Neurosciences vs. Genomics on Autism**

Yun-Csang Ghimn, University of Alberta, Canada (yghimn@ualberta.ca)

To reality check the much hyped decade-long neurobiological turn (Smail, 2008), I shall historically compare it *vis-à-vis* likewise rushed genetic explorations (warned against by Evans, Meslin, Marteau, & Caulfield, 2011) particularly in autism spectrum disorder including Asperger's syndrome (Draaisma, 2009), with which roughly 1 through 6 per 1000 children are diagnosed (World Health Organization, 2011) while such rate has increased vividly over twenty years (Feinstein, 2010 recognizing milder cases). Behaviourally they display non-reciprocity à la word repeating, little eye contact and stereotyped interests; one could hence ask where lies the problem—brain or gen(om)e? My post-1970s historical literature/media review will briefly indicate, to show that assumed place, visual construction (whether innovative positron emission tomography or deoxyribonucleic acid simulation) is a competitive strategy from both disciplines' expert/popular rhetoric. Noninterventionist imaging—yet whose semiotic codes favour its “perspectival” meaning since the late nineteenth or early twentieth century as intended—has shaped knowledge and communicated nature to broad public users (Daston & Galison, 2007). Timeline wise specifically: once they stopped blaming aloof “refrigerator mothers” psychoanalytically just four decades ago (Rimland, 1964), thus experimental physiology was claimed; an identical vs. fraternal/separate-egg twin comparison (Folstein & Rutter, 1977) would help determining the inherited root cause of autism, but first lacked sophisticated cortical pictures (whereas neural approaches have consistently provided these; e.g. Figure 2 in Luna, Minshew, Garver, Lazar, Thulborn, Eddy, & Sweeney, 2002, p. 838)—which held true only until burgeoning genomics starts explicitly diagrammatizing micro/real chromosomes (recently Noor, Whibley, Marshall, Gianakopoulos, Piton, Carson, Orlic-Milacic, Lionel, Sato, Pinto, Drmic, Noakes, Senman, Zhang, Mo, Gauthier, Crosbie, Pagnamenta, Munson, Estes, Fiebig, Franke, Schreiber, Stewart, Roberts, McPherson, Guter, Cook, Dawson, Schellenberg, Battaglia, Maestrini, Autism Genome Project Consortium, Jeng, Hutchison, Rajcan-Separovic, Chudley, Lewis, Liu, Holden, Fernandez, Zwaigenbaum, Bryson, Roberts, Szatmari, Gallagher, Stratton, Gecz, Brady, Schwartz, Schachar, Monaco, Rouleau, Hui, Raymond, Scherer, & Vincent, 2010). We expect then it must work among various lay audiences; however, resourceful ASD parents have also developed non-adherence to more fully ruminate over practical information themselves collectively if not politically (cf. Shaw, Huebner, Armin, Orzech, & Vivian, 2009, pp. 464-465) and some even lead families beyond visible evidence-based services towards complementary medicine for instance gluten-free regimen, a decision upon Nutritional neuroscience drawing attention, commentary & pure excitement.

**63. Hans Hugo Bruno Selye: Path from boyhood to global citizen, physician, scientist, discoverer and professor: I. Early years in Europe and America (from Johns Hopkins to McGill and the Université de Montréal) and II. Research/academic life at the Institut de médecine et de chirurgie expérimentales (IMCE) at the Université de Montréal**

M. Salas-Prato and Y. Taché (Hans Selye Foundation (HSF), Montreal, Canada, University of California at Los Angeles & VA Medical Centre, Los Angeles, USA)

Hans Hugo Bruno Selye (HS) was born in Vienna, Austro-Hungarian Empire on January 26th, 1907 from a Viennese mother and a Hungarian surgeon father. Following studies at the Benedictine school in Komarno, HS entered medical school at the German University in Prague. Following graduation, HS undertook Ph.D. research studies in endocrinology under Dr. A. Biedl, AB. Then he moved to Johns Hopkins University to continue his post doctoral research. The cultural shock to adapt to this new environment along with breakthroughs made by the Nobel prize nominee Dr. J. B. Collip's (JBC) for his insulin discovery, made him continue his training under the leadership of Dr. Collip at the

Department of Biochemistry at the Faculty of Medicine of McGill. It was in this department, where he discovered and characterized the stress syndrome (Nature, July, 1936). Due to HS's insistence in continuing in the novel stress field and elucidating the humoral/pathological effects/mediators/pathways of its response, he transferred to the Anatomy Department. Subsequently, he was offered the directorship of a new Research Institute at the university. However, seeking more independence, he turned this down and in 1945, founded and directed the *Institut de médecine et de chirurgie expérimentales* at the Faculty of Medicine of the U de M. HS's era at the IMCE was characterized by great discoveries, resulting in the expansion of the stress concept and of the endocrine/neuroendocrine fields in animals/humans. Consequently, his reputation and influence became worldwide. Another interest was specializing/perfecting surgical interventions to ablate endocrine glands and other organs to determine the effect of these deletions on the stress response/specific pathology investigated. During HS' studies at the German University in Prague, he developed and published a partial hepatectomy technique before that of Higgins and Huggins. Thus, HS's experimental approach reflected the early surgical imprinting of his family and, even though, the IMCE was not a hospital research facility carrying out clinical research, it focused on translational research, similarly to that of a clinical setting (daily rounds visiting the laboratories/animal "patients", daily autopsies/presentation of type-written consecutively numbered protocols/treatment plans for experiments, famous researchers, many Nobel prize winners or nominees, arriving to deliver the "Claude Bernard lectures"/Visiting professorships, etc.). The significance of his discoveries made him the recipient of forty three honorary degrees and the honorary fellow of sixty eight scientific societies and, although nominated 17 times for the Nobel Prize, he did not receive this ultimate distinction. One contribution of his remarkable academic legacy was the training of fellows and students. Dr. Selye achieved an impressive track record of successful mentoring, forging leaders in their particular fields of endeavour (i.e., Dr. R. Guillemin, Nobel prize winner for his discovery of hypothalamic releasing factors), their medical schools (i.e., dean and vice-deans, Drs. P. Bois, C. Fortier, P. Jean, C-L.Richer, S. Szabo), their departments (i.e. chairs, Drs. G. Jasmin, P. Jean, E. Kourstak, M. Somma), their institutes (i.e., directors, Drs. W. Feindel, C.P. Leblond, P. S. Timiras, Y. Tache) or their laboratories (Drs. M. Cantin, B. Messier, F. Babai, B. Tuchweber, Veilleux, R. Gagnon, I. Berczi). In addition, many of these, have become implicated as presidents/vice-presidents/directors of the Board of Directors of the International Stress Institute (Dr. J. Taché), the Hans Selye Foundation (Drs. Cantin, Tuchweber, Szabo, Y. Taché, Salas-Prato) of other Stress Foundations (Dr. P. Rosch, Ms. M. J.T. Smith, The American Institute of Stress) which Dr. Selye founded or co-founded. His contributions had enormous influence and implications in the understanding and treatment of illness, and his findings opened up new exciting and important avenues/fields of research in biology, medicine and in other disciplines.

**Saturday Afternoon, June 18, 2011**

**Psychology and Neurology in different National Contexts:**

**64. A 'Biologically Based Inner Nature': Abraham Maslow, Self-Actualization & Jewish Identity**

Ian Nicholson, St. Thomas University, Canada (nicholson@stu.ca)

Self-actualization is one of most culturally successful concepts in the history of American psychology. Although the term was first developed by the German psychologist Kurt Goldstein, it is most closely associated with the work of the well known humanistic psychologist Abraham Maslow. Maslow's initial interest in Goldstein's work and subsequent career as a humanistic psychologist has been well documented (Hoffman, 1988). Less consideration has been given to the cultural, and more specifically Jewish context of Maslow's interest in the 'self-actualization' concept. Maslow was an avowed secularist with little knowledge or regard for Jewish tradition. Nevertheless, he came of age intellectually in a stridently anti-semitic environment where questions of selfhood, authenticity, and assimilation were

constantly at play. Within this context, many secular Jews felt obliged to confront the basis of their identity. Could one's Jewishness be simply abandoned as a kind of irrelevant cultural accessory? Or was Jewishness as much a matter of biology as it was one of culture; a 'racial' identity that could be temporarily ignored but never completely denied. Despite his own religious indifference, Maslow was strongly influenced by a pre-war Jewish American literature which framed questions of identity in biological terms (Goldstein, 2006). This paper will examine the impact of this Jewish American literature on Maslow's subsequent work as a humanistic psychologist. Particular attention will be devoted to the work of Ludwig Lewisohn, a prominent Jewish American author and critic (Nilsen, 1987). Born in Berlin, Lewisohn's family emigrated to the United States and subsequently converted to Christianity. Despite Lewisohn's brilliance and professed Methodist faith, anti-semitic prejudice still conspired to thwart his career plans in academia. In response, Lewisohn affirmed his Jewish identity with renewed vigor and he became a well known critic of Jewish assimilation. Maslow read Lewisohn's best known work *The Island Within* (1928) while a graduate student and it had a profound impact on him as a Jew and as a psychologist. The book was a multi-generational chronicle of a fictitious Jewish family beginning with life on the Pale in Russia and concluding in New York City. The dominant theme throughout the narrative was the futility of assimilation and the 'biological' basis of Jewish identity. Happiness and fulfillment were reserved for those characters who remained true to their innermost 'biological' selves. In this paper, I will argue that much of the character of Maslow's subsequent psychological theorizing was forged by his reading of Lewisohn and his experience of assimilationist anxieties. Maslow would become a self-consciously Jewish psychologist, but more importantly the theoretical content of his work remained linked to the hereditarian, anti-assimilationist message of *The Island Within*. Where Lewisohn spoke of the importance of affirming an essential Jewish self, Maslow (1962) emphasized the importance of actualizing "an essential biologically based inner nature" (p.3). The duality of the true inner self and the false, outer, socially oriented self was to be one of the characteristics of his work as a psychologist.

#### **65. On Emergence of Neurosurgery in Middle Asia**

Liliya Nazarova and Danioer Mirsadyko, Pediatric Medical Institute, Tashkent, Uzbekistan;  
Boleslav Lichterman, The Burdenko Neurosurgery Institute, Russia (lichterman@hotmail.com)

The emergence of neurosurgery in Middle Asia in the 1930s followed international patterns. It was promoted both by neurologists and general surgeons. The aim of our presentation is to demonstrate the role of Turkestan State University in this process. The Medical Faculty of Turkestan State University (renamed Middle Asian Medical Institute in 1931) was established in Tashkent in 1919 with the help of the Soviet government in Moscow. More than 100 scientists (including 20 professors of medicine) and 65 railway cars loaded with equipment and literature were sent from Moscow to Tashkent. Mikhail Zakharchenko (1879-1953), a Moscow neurologist, became a chair of nervous diseases of Turkestan University. In 1925 he organized Tashkent society of neurologists and psychiatrists which he presided until 1939. His pupils, Yu. V. Neiman and D.K. Bogorodinsky diagnosed several cases of spinal cord tumors which were operated on at the clinic of faculty surgery headed by Prof. P.P. Sitkovsky (1882-1933). General surgeons of Tashkent were also interested in neurosurgical interventions. Apart from Sitkovsky these included professors Mikhail Astrov (1882-1957) and Ivan Orlov (1888-1952). For example, Orlov published 14 neurosurgery papers including several publications on causalgia. In 1925 Astrov published a book on gunshot cranio-cerebral injuries which summarized his WWI experience.

#### **66. Boleslav Lichterman (Elder) a Founder of Physical Therapy Applications in Russian Neurology**

Dmitriy Labunskiy, University of Northern California, USA.; Leszek Jańczuk, Warsaw Poland and  
Andrey Romanenko, Moscow, Russia (DLabunskiy@hotmail.com)

Boleslav Vladimirovich Likhтерman (1902–1967) was a Soviet medical researcher. He is best remembered as a pioneer in the use of high frequency electrical currents in the treatment of physical ailments and as an editor of the academic journal *Voprosi Kurortologii, Physioterapii i Lechebnoi Physicheskoi Kultury* (Problems of Balneology, Physiotherapy, and Therapeutic Physical Exercises). Likhтерman attended the gymnasium in Simferopol until his graduation, at which time he was awarded a gold medal for academic excellence. In December 1926, Likhтерman married Sara Evseevna Brusilovskaya, who later became a teacher of English. As 1926 came to a close, Likhтерman received his first medical appointment, serving as head of a medical office in Belorezk, Bashkiria, part of the Bashkirian People's Commissariat for Health. In June 1928, Likhтерman moved to the Sevastopol city outpatient clinic, where he assumed a position as a neurologist and physical therapist. Likhтерman remained in this position until World War II. From 1929, Boleslav worked at the I.M. Sechenov State Research Institute of Physical Methods of Treatment, founded by Professor Alexander E. Scherbak in Sevastopol. There he was a member of the committee studying the use of short waves as a part of therapeutic practice. He was made head of the Clinical Department and Neurological Clinic at the Sechenov Institute in 1932, achieving the title of “Docent” (Adjunct Professor). In 1939, Likhтерman was awarded the “Excellence in Healthcare” prize by the USSR People’s Commissariat of Health. He published a book about therapy application of the short waves, which was highly evaluated by the Soviet and foreign colleagues. As a young doctor he was very popular, he was invited for consultation and treatment of top Soviet officials, who took a rest in Crimea. But Likhтерman didn’t select his patients upon different categories. He took care for everybody, who needs his help on day or night in any weather. In September 1941, following an invasion of the Soviet Union by Nazi Germany, Likhтерman and the rest of the Sechenov Institute was evacuated to Kazakhstan in Soviet Central Asia. There Likhтерman worked as a chief of the Clinical Department in the hospital for the wounded and as a consultant at the Red Army Central Tuberculosis Sanatorium. His work with the ill took its toll and in 1942 Likhтерman himself contracted pulmonary tuberculosis. In July 1943, as the tide of the war began to turn, the Sechenov Institute was transferred west to Kislovodsk in Stavropol Krai, Russia. In the fall of 1944, the institute was moved back to the Crimea again, this time to the southern resort city of Yalta. For the next two decades, Likhтерman would work as the head of the Neurological Clinic of the renamed I.M. Sechenov Institute of Physical Methods of Treatment and Climatotherapy in Yalta. In 1948 upon the recommendation of the USSR Academy of Medical Sciences, Boleslav Likhтерman was conferred the degree of the Doctor of Medical Sciences for his academic publications to date without a formal defense of a dissertation. He was granted the academic rank of the Professor in Nervous Diseases. In 1952 for his outstanding service in the field of medical science, Likhтерman was awarded the Order of Red Banner of Labor by the Presidium of the Supreme Soviet of the USSR. In 1965, Likhтерman was elected an honorary member of the All-Union Society of Physiotherapists and Balneologists. Boleslav Likhтерman died in 1967 from leukemia.

#### **Memory:**

### **67. The Scotophobin Saga: Georges Ungar and the Search for a Molecular Code for Memory**

Larry Stern, Collin College, USA (lstern@collin.edu)

On June 28, 1972, nearly eighteen months after it was submitted, Georges Ungar and his co-workers’ paper reporting the isolation, purification, and structural identification of scotophobin, a peptide reputedly coding for the fear of the dark, appeared in the journal *Nature*. The editor, well aware of the controversial nature of the claim, set precedent by publishing the critical comments of one of the referees, Walter Stewart, along with the scotophobin paper. Running almost three times as long as the paper it critiqued, Stewart criticized virtually every aspect that had gone into the elaboration of scotophobin’s structure. To justify the inclusion of Stewart’s critical assessment, the editor stated in his weekly column that the reported transfer of learned behavior from one animal to another “is a subject so fraught with pitfalls that it seemed necessary, while publishing the results of Dr. Ungar and his colleagues, to set out clearly the

reservations that many people will doubtless wish to voice among themselves.” Due to the circumstances surrounding the publication of this paper, many believed that Ungar’s scotophobin claim was simply “dead on arrival.” Moreover, it was widely believed by most researchers then investigating neural processes of memory, as well as by later analysts of the case, that the memory-transfer research, *in toto*, had been dealt its final deathblow. Not so – reports of the death of the transfer effect were greatly exaggerated and proved premature. All of the evidence I shall present indicates that research on scotophobin continued apace. As shall be seen, Ungar continued his work largely unaffected, and he was joined by more than thirty labs that requested samples of scotophobin to either test or to include in their particular research programs. Although it is certainly the case that this research never won the assent of the majority of researchers mining the field – and that, in fact, reliable evidence supporting the theory behind it was never produced – the memory-transfer episode continued for many years after Ungar’s scotophobin paper appeared in *Nature*. Thus, this case may be profitably used to examine key processes involved in the production and reception of extraordinary claims in science and how such cases reach closure. Scientific claims that depart in significant ways from prevailing cognitive frameworks pose constant and formidable problems in science. Such claims, as history attests, might lead to truly consequential developments in science, while others might lead researchers astray. The problem, of course, is that once such claims are introduced there is no sure way of knowing whether they will be a “boon” or a “bust.” How, then, should such claims be handled by funding agencies, publication outlets and other means of scientific communication? If accommodated – if granted access – to these gate-keeping institutions, how long should the gates remain open? Equally important, who will choose to invest their time, energy, resources, and perhaps their reputation in the pursuit of such claims? How is closure reached in such cases? Here, I shall draw mostly upon Ungar’s personal papers and correspondence – although I shall also refer to his numerous conference presentations, experimental reports, and review articles – to address these questions. First, I shall describe Ungar’s efforts to nail down the structure of scotophobin including, of course, the trials and tribulations that occurred behind the scenes. Here, I shall have occasion to mention a remarkable yet shaky collaboration between Ungar and a team at Berkeley, under the patronage of Nobel laureate Mac Calvin. Next, I shall focus upon what happened after Ungar’s announcement of the structure of scotophobin. As shall be seen, Ungar was in no way marginalized. He continued to receive government funds for his work, received invitations to present his work from such prestigious places as the Salk Institute, Russell Sage Foundation, American Chemical Society, University of Chicago, California Institute of Technology, Rockefeller University, Max-Planck Institute, and the Smithsonian Institute. Invitations to present his work came from conference organizers throughout Europe and the United States as well. Last, I shall focus on researchers outside of Ungar’s lab that requested samples of scotophobin for testing. Responding to nearly every request, Ungar provided samples over the next five years to nearly thirty labs; many received samples on multiple occasions. This is not to say, of course, that each of these researchers believed in scotophobin. But they certainly were not of a mind to completely dismiss it without further careful scrutiny. And despite the fact that many of the experiments proved negative, the work nevertheless continued.

## **68. The Reception of Memory-Transfer Research in Germany: The Case of Goetz Domagk**

Barbara Lusk, Collin College, USA (blusk@collin.edu)

Fifty years ago, James McConnell published a report that startled much of the scientific community: the “memory” of a learned response was apparently transferred via ingestion when cannibalistic worms were fed trained donors. Thus began one of the more interesting episodes in the history of psychology, and one that eventually spilled into the history of the neurosciences. Historical accounts of the memory transfer episode have focused exclusively on work conducted in the United States. This research, however, was far from an “American-only” enterprise. In fact, memory-transfer experiments were conducted on six continents, in twenty-eight different countries ranging from Austria to India to the former Soviet Union to Yugoslavia, and by more than sixty independent researchers or research teams – roughly one-third of the

total number of researchers that chose to invest their time and resources in the pursuit of so-called memory molecules. Here, I shall rectify this analytical imbalance by examining the evolution and reception of a large-scale memory-transfer research program conducted in Germany between the years 1967 and the early 1980s by Goetz F. Domagk, Professor and Head of the Section of Enzyme Chemistry, housed in the Department of Physiological Chemistry at Gottingen University. The memory transfer phenomenon was never established to the satisfaction of most researchers investigating the neuro – and biochemistry of memory. That Domagk’s work, in collaboration with some of the top-tier scientists in Europe, continued in Germany for fourteen years is a matter of some interest. What was the intellectual climate in Europe, and specifically in Germany, that allowed the breathing room necessary for Domagk’s long-term research in memory transfer? How was the work received in Germany by the “established” scientific community? What was the dominant funding source for scientific research, and more importantly, what cultural factors specific to the German reward system contributed to the on-going funding of Domagk’s memory transfer research? Were there reputable publication outlets in Germany willing to publish such reports? Did Domagk’s career and/or reputation suffer because of his support of memory-transfer? After reviewing Domagk’s work I shall examine these questions in some detail. To do so I shall draw upon three types of materials: Domagk’s published experimental reports, an interview conducted with Domagk in 2001, and a ten year correspondence between Domagk and Georges Ungar, a physiologist-pharmacologist at the Baylor College of Medicine who, by all accounts, was the most important of the transfer researchers. The correspondence describes the problems involved in developing a reliable learning paradigm, the training and testing of exotic experimental animals, the chemical preparation and analysis of minute amounts of materials, the literal mechanics of the transfer of brain homogenate, the frustrating failures to elicit the phenomena and, of course, the tantalizing successes that held such promise. As shall be seen, Domagk’s foremost concern was to develop a unique and replicable behavioral assay or learning paradigm that could serve as the basis for deriving the structure – and thus validation of the existence – of so-called “memory molecules.” Using goldfish, mice, insects, baby chicks, and octopi as test animals in this quest, Domagk was creative, meticulous, and persistent. At the same time, Domagk expended considerable effort – more, in fact, than any other transfer researcher – to test the behavioral effects of both natural and synthetic scotophobin, the first so-called memory molecule that, produced by Ungar, allegedly coded for dark avoidance. It is well known that the memory transfer phenomenon understood from the perspective of the reception process in science offers significant insights. This paper will attempt to broaden this understanding of the place of controversial claims in science by emphasizing cultural factors that inform the process in significant ways.

## **69. Juan Huarte and the Brain Basis of Memory, Imagination and Understanding**

Rachael Goldsworthy, Northern Michigan University, USA (rgoldsw@nmu.edu) and  
Harry A. Whitaker, Northern Michigan University, USA (hwhitake@nmu.edu)

Aside from notes in history texts, most analyses of Juan Huarte de San Juan’s “Examen de ingenio para las ciencias” (1575/1594) are in Spanish; an exception is the excellent 1977 review by G.Mora who noted that Huarte’s psychology is “solidly based on physiological concepts, and in particular, on the structure and function of the brain” (p. 72). Huarte is considered an early differential psychologist (Santoyo, J. M., & Vega, L. G., 1990), a contributor to modularity of mind theory (Garcia, E.G., 2003) and an early discussant of hereditary influences on and biological bases of intelligence (Arquiola, E., 1986). The Aristotelian-Galenic origin of Huarte’s ideas have been discussed, although the full range of individual differences they encompass have not been appreciated. Rarely, if ever, discussed is Huarte’s use of humoral theory to explain age-related changes in personality and intelligence. Of particular interest to the history of neuroscience is Huarte’s major alteration to the Avicennan version of medieval cell doctrine (4 or 5 cerebral ventricles, not three) (vide Whitaker, H.A., 2007). Huarte rejected the standard functional localization of cell doctrine, arguing that memory, imagination, and understanding must be distributed in all four ventricles. His evidence in support of this claim is neuropsychological: a single injury impacts all

three functions. It is evident that Huarte was aware of the earlier literature on the effects of brain damage on memory and had analyzed those reports to conclude that intelligence was a distributed, not localized brain function.

#### **Memory:**

**70.**

#### **W.J. Crozier's Relation to Jacques Loeb**

Edward K. Morris, University of Kansas, USA (ekm@ku.edu)

This presentation reports, corrects, and discusses a recurrent error in the historiographic record concerning W. J. Crozier's professional relation to Jacques Loeb. Loeb advanced what he called an "engineering standpoint" in early twentieth-century biology (see Loeb, 1912). He was not alone. Among his successors was the general physiologist, Crozier. As for their relation, between the mid-1950s and mid-1980s, Crozier was generally reported to have "studied under" Loeb, "worked with" him, and been his "student." Skinner (1956), for instance, wrote that Crozier "had studied under Loeb" (p. 223). Alan Kazdin (1978, p. 92) and Laurence Smith (1986, p. 276) made similar claims. Phillip Pauly (1987) clarified their relation in his biography of Loeb in the context of Crozier's undergraduate studies, graduate studies, and career. In the first, Crozier may have met Loeb while he attended the City College of New York, but if not, he was a student of Loeb's work in a figurative sense. At the time, Loeb was at the University of California and then the Rockefeller Institute for Medical Research in New York City. In the second, Crozier attended Harvard University while Loeb was still at the Rockefeller Institute, but they did meet at the Marine Biological Laboratory in Woods Hole, MA, while Crozier was completing his dissertation (Crozier, 1915). In the third, Loeb was a supporter of Crozier's career and research. In none of these contexts was Crozier every literally a student of Loeb's. Pauly noted, however, that Crozier "worked with" Loeb at Woods Hole in a near-literal sense and that Crozier may have viewed himself as Loeb's "student" in a figurative sense. Pauly's scholarship notwithstanding, the received view has remained, in large part, that Crozier was Loeb's student (e.g., Hothersall, 2004, p. 516; Michael, 2004, p. 100). In a personal communication with me in 2007, Pauly affirmed what he had earlier written about Crozier's relation to Loeb, but suggested that their colleagues at Wood Hole, given its culture, might have referred to Crozier as Loeb's "student" in a figurative sense. This brings closure to the historiographic record, but not to the error. I address its nature in the context of a continuum of errors (see Thomas, 2007), for instance, misportrayals (see Goodwin, 1991), misinterpretations (see Costall, 1993), and fabrications (see Benjamin, Whitaker, Ramsey, & Zeve, 2007). I also discuss the error's likely sources, among them, Skinner's autobiographical writings, but not in all of them (e.g., Skinner, 1989); confusions between the literal and figurative meanings of terms (e.g., student); and an over-reliance on secondary sources (see Harris, 1979).

**71.**

#### **How the Concept of Invariance Shaped the Career of S.S. Stevens**

Mark A. Affeltranger, Bethany College, USA (maffeltranger@bethanywv.edu)

Although Stanley Smith Stevens (1906-1973) discovered a wide range of ideas from the measurement scales to the power law, all were discovered with the goal of invariance. Invariance, in physics, means the ability of a principle to remain constant despite changes in coordinates or context (Lindsay and Morgenau, 1957). Stevens sought "universal laws" instead of variability and even considered inferential statistics as a weakness of psychology (Miller, 1974). Stevens wrote that mathematician George Birkhoff (1884-1944) introduced him to this in 1940 (Stevens, 1973), but it appeared in Stevens's career before and after. His unstable, early life may have shaped his need for invariance. Marital infidelity, insecure finances, and losing both parents by age 17 made him seek out stability (Stevens, 1973). His Mormon roots demanded stripping Christianity of "-isms" to reveal one true faith (Smith, 1842). Nicholson (2005) believes this crossed into Stevens's early operationism papers to strip psychology of unnecessary and

variant perspectives (Stevens, 1939). Stevens based his graduate work on finding perceptual invariances to variations of light and sound frequencies (Stevens, 1973). Stevens sought to replace Fechner's logarithmic law of perception because Fechner, according to Stevens, overemphasized variance (Miller, 1974). He argued that Fechner's methods measured the noise or variability of the senses, increased variability by requiring the researcher to measure only on an interval scale. In contrast, Stevens' power law invariably originated from participants directly quantifying their own sensory experiences on a ratio scale (Stevens, 1961). Stevens chose phenomena that remained invariant across people and contexts. In his early work on physique, he showed that these characteristics stay stable across ages (Stevens, 1973). His power law extended across a dozen sensory modalities and received cross-modal verification so he could reliably predict how intense a single participant adjusted one stimulus until it perceptually matched a stimulus in another modality (Stevens, 1975). He extended the law to sensory physiology and eventually to social and political perceptions (Stevens, 1975). For measurement, Stevens needed Birkhoff's help. In 1940, the British Association of Science remained evenly split on the possibility of sensory measurement due to a problem in defining measurement. N. R. Campbell, who headed the committee, defined measurement simply as assigning numbers to observations, but left open "how" to assign (Stevens, 1946). Stevens tried to explain by discriminating his Sone Scale (Stevens and Davis, 1938) which had a defined zero from Fechner's confusion scale which did not. The International Congress for Unity of Science did not accept his psychophysical analysis. Birkhoff told Stevens to apply invariance mathematically, leading Stevens to define each scale by mathematical transformations that leave a scale invariant (Stevens, 1973). Stevens discovered a hierarchy of scales. Nominal occupies the bottom because it only provides equality information. To Stevens, this scale allows almost any transformation as long as one number represents only one class. Ratio occupies the top because it provides equality, ranking, difference, and ratio information. This scale allows only multiplicative transformations (e.g. centimeters = 2.54 x inches) allowing it to explain size and brightness constancy and be prevalent in physics. Essentially, the scale that allows the least transformations contains the most information (Stevens, 1946). Marks (1986) stated in the prologue to Stevens's last text that understanding Stevens requires invariance. For Stevens, the word may have been introduced by Birkhoff, but the goal permeated his entire career.

### **Keynote Lecture III:**

#### **Psychiatry and the Social Sciences in Post-World War II America**

Andrew Scull, University of California in San Diego, USA (ascull@ucsd.edu)

I propose to examine the inter-relations between psychiatry and some major social sciences – psychology, sociology, and (to a lesser degree) economics – in the period between the outbreak of the Second World War and the present. Besides the direct impact of total war on the mental health sector, we shall look at major developments in the post-war world: the impact of the creation of NIMH; the rise and decline of psychoanalysis; the development of clinical psychology; the changing relationships between psychiatry and sociology, from collaboration to conflict to mutual disdain; the impact of the psychopharmacological revolution and the associated rise of biological psychiatry; and the late-on emergence of an economics of mental health.

### **Featured Lecture III:**

#### **Vampire Fiction and the Emergence of the Rest Cure**

Anne Stiles, Washington State University, USA (astiles@wsu.edu)

From the 1880s until the 1920s, the standard treatment for female nervous illness in Western nations involved prolonged bed rest, massage, isolation, and force-feeding. The horrors of the "rest cure,"

pioneered by American neurologist and novelist Silas Weir Mitchell, are famously depicted in Charlotte Perkins Gilman's short story, "The Yellow Wallpaper" (1892), whose protagonist breaks down during a period of forced confinement. This paper traces the origins of Mitchell's rest cure to an unlikely source: British and Irish vampire fiction of the mid-nineteenth century, particularly James Malcolm Rymer's *Varney the Vampire: or, the Feast of Blood* (1845-7) and Sheridan Le Fanu's *Carmilla* (1871-2). Mitchell, an avid reader and writer of fiction, compared hysterical women to vampires throughout his published works, including early medical treatises such as *Fat and Blood and How to Make Them* (1877) and later novels such as *Roland Blake* (1886). By likening a nervous woman to "a vampire who sucks the blood of the healthy people around her," and feeding such patients a blood-like mixture of puréed raw meats, Mitchell literally demonized the sick women in his care. He did so in order to distance himself from these patients, who suffered from the same nervous symptoms that Mitchell himself experienced following the Civil War. By turning sick women into "vampires," Mitchell wove aspects of horror fiction into his clinical practice. One might even say that Le Fanu or Rymer's occult vampire tales helped revolutionize therapeutics for nervous disease during the second half of the nineteenth century. This paper is part of a larger project, *Resisting the Rest Cure: Women Writers and Alternative Therapies, 1870-1920*, which examines the work of the doctors who developed the rest cure, particularly Mitchell, alongside the fiction of three women writers: Elizabeth Stuart Phelps, Frances Hodgson Burnett, and Marie Corelli. These bestselling female authors all experienced nervous illness, but rejected the rest cure in favor of homeopathy, spiritualism, or Christian Science. They used their novels to promote these unconventional therapies. The popularity of such fiction on both sides of the Atlantic shows the extent of women's dissatisfaction with Mitchell's cure. Such writings also suggest that the late-Victorian conflict between mainstream and alternative medicine involved a battle of the sexes, pitting medical men against women steeped in occult traditions. This project builds upon the work of historians such as Joy Dixon, Alex Owen, and Rennie Schoepflin, who have shown that Victorian spiritualism, Theosophy, and Christian Science helped women achieve greater autonomy, and played a role in feminist causes such as women's suffrage.

**Sunday Morning, June 19, 2011**

**Race:**

## **72. Uniquely Useful Population: Racial Assumptions, Adoption Studies and International Research into Schizophrenia, 1960-1990**

Andrew M. Fearnley, University of Leeds, United Kingdom (A.Fearnley@Leeds.ac.uk)

If clinical psychiatrists practicing in the postwar US increasingly thought of schizophrenia as 'a black disease'—'the protest psychosis' according to Jonathan Metzl's recent study (Metzl, 2010)—psychiatric researchers did not. Yet many of those involved in investigating schizophrenia in the postwar period, still thought according to a particular set of racial assumptions. Those who investigated the etiology of this disorder believed that it was imperative to work with what they identified as 'genetically homogeneous' human subjects. They did so in the belief that only if they limited the variations between their research subjects would they be able to identify the causes of this disorder. But which human subjects counted as 'genetically homogenous'? And how could findings produced by such studies be used in the clinic to treat those from different backgrounds? In the US, studies undertaken in the states of Iowa and Oregon were believed to be exemplars, in part because of those state's purported racial homogeneity. When in the early 1960s, American investigators wished to repeat these studies, it was to Scandinavian countries that they turned, not least because of the "unusually fine sampling characteristics" of such groups, or the "cultural and racial homogeneity" such groups were presumed to possess (Hutchings and Mednick, 1975). At a time when medical surveys of syphilis carried out in Oslo, Norway were being used to draw racial comparisons with studies done on black men in Tuskegee, Alabama, American psychiatric geneticists' decision to use Scandinavian samples revealed a certain racial logic at work. It was a logic that only

emerged in the postwar period, evident in the National Institute of Mental Health's decision to start projects in Israel, Lebanon, and Japan, as much as in population geneticists' wish to work with those they identified as 'genetically homogeneous' (Lipphardt, 2009). Focusing on the first and the largest of the psychiatric studies that began in the postwar period, the Danish-American Adoption Study, this paper considers the racial assumptions that organized research across the field of international genetic research into schizophrenia. Beginning in April 1963, the Danish-American Adoption Study brought together some of the most important psychiatric geneticists in the US and Denmark. Jointly funded by the NIMH and the Danish government, the study drew on records and administrative systems found in the city of Copenhagen--a burgeoning center of medical research in these decades (Bauer, 2008). By recounting the processes by which this study began, this paper will examine how collaborative research projects were coordinated in this period, and the types of exchanges that they encouraged. It builds on recent scholarly interest in the global contours of medical research (Roelcke, et. al., 2010; Petryna, 2009), to examine the everyday translations that such research necessitated. Such acts of translation were often both practical (concerning issues of language and travel) and intellectual (determining what was schizophrenia, and how should it be recognized). In fact, the most significant such issue was as much as linguistic as conceptual: agreeing where the boundaries of the disorder lay. To answer this question in a way that would satisfy researchers from both countries, investigators adopted the concept of "subschizophrenic disorders," in turn helping to introduce that concept to the pages of the DSM. Translating what psychiatrists established through these programs of research on 'genetically homogenous' samples to US clinical wards, understood to be racially diverse, proved much less straightforward however.

### **73. Nature and Nurture in French Social Sciences, 1859-1914**

Martin Staum, University of Calgary, Canada (mstaum@ucalgary.ca)

The reputation of two learned societies founded in Paris in 1859, the Ethnographic Society and the Paris Anthropological Society, has been the representation of two polarities of interest in the human condition – the cultural and social environment for the ethnographers, and physical and racial classification for the anthropologists. But rather than always opposing an indelible nature to a malleable culture, members of the two societies displayed the recognition that nature and nurture were inextricably intertwined. The anthropologists were a particularly complex case, since it has long been known that many of them were Lamarckians who believed that environmental influences elicited changing organic needs and adaptation. However, many anthropological Lamarckians applied this viewpoint selectively, so that only Europeans (including European women) were easily adaptable. But rather than unanimously endorsing an indelible human nature, anthropologists included a culturalist countercurrent that refused to give cranial measurement the last word. Such debates in anthropology also affected the other emerging social sciences. From a dynamic standpoint, developments in psychology and sociology showed an increasing confidence in the importance of social influences, while at the same time preserving the fascination for racial classification and hereditary transmission of qualities. Two of the pioneers of French empirical psychology, Théodule Ribot and Alfred Binet, show how this tendency operated. Ribot wrote a highly successful treatise on psychological heredity that he never disavowed. However as a periodical editor, he published increasing numbers of articles with sociological viewpoints and near the end of his career saw sociological insights as complementary to his psychology. Binet turned away from the anthropological perspective but not before obsessively trying to correlate head measurements and intelligence. The foundation of societies and periodicals of sociology could naturally have been expected to stress the primacy of the "social fact" over hereditary or racial influences. However, among the non-Durkheimian sociologists comprising the circle of René Worms, there was a hardline faction that held out for significance of the older anthropological racial classifications, while a larger group insisted on the importance of the milieu. The Worms coterie, unlike the anthropologists, was more flexible on racial issues than on gender, not because they advocated an essentialist nature of women, but because they insisted on socially variable gender roles to maintain household stability and population growth. There

was also no strict correlation among individual members between their views on race and their views on gender. In the generation before 1914, a wholesale reorganization of ethnographic and anthropological institutions heralded a new linguistic and cultural turn that weakened the older physical anthropology. The old indicators for classification, whether facial angle, cephalic index, or cranial size, all seemed inadequate. Yet the enrichment of racial stereotyping with cultural and ethnic components enabled racial ideology to survive and to be turned into a sinister tool during the Vichy era by extremist anthropologists who were outside the mainstream of their profession.

#### **74. William Shockley, Margaret Mead, and Scientific Racism**

Kenneth D. Feigenbaum, University of Maryland University College, USA (kenfeigenbaum@gmail.com)

William Shockley was one of the co-winners of the Nobel Prize in Physics for his discovery of the transistor. From 1965 to his death in 1989 he was a leading proponent of what has become to be known as “scientific racism”. He promoted the notion that I.Q. is eighty percent genetic, and that there were significant differences among the so called races. He also promoted a form of Eugenics which was mainly directed at limiting the birth rate of African-Americans. This paper briefly traces the public expression of his views from 1965 to the mid-seventies. In particular it focuses upon his denial to speak after being invited to do so at Brooklyn Polytechnic at the 25th Annual Convention of Sigma Xi on May 29th, 1968. The convention was claimed to have been cancelled due to “threats of violence” if Shockley spoke. As part of the controversy regarding his speaking engagement Margaret Mead was called upon to intervene to try to limit the “after shock”. The paper will present the correspondence of Mead to Shockley; his responses to the cancellations; and the views of newspapers, scientists, and educators about the cancellation of the speech. Finally, this paper will raise the issue of academic freedom as applied to those whose views are deemed as “radically political” or non-normative in terms of the prevailing scientific consensus. There is a general consensus regarding the facts of what happened though details are somewhat different. In essence Shockley was invited to speak at a symposium entitled “What Man Can Be”. The planning committee of Sigma Xi at B.P.I. knew in advance what he was going to speak about. Some Social Science faculty at B.P.I. protested and asked Shockley to talk about something closer to his academic expertise. The cancellation of the program occurred three days before the date of the event. Shockley refused to change his address and the Convocation was cancelled. As a result of the cancellation proponents of civil liberties objected to the cancellation. There were also editorials in the N.Y. Times and the Wall Street Journal protesting the action. Harold Taylor, the President of Sarah Lawrence College stated: “If we are never to discuss any controversial issue for fear there might be demonstrations then the whole purpose of the university is destroyed (Deidre, 1976). The local Chapter of Sigma Xi at Brown University and the National Chapter objected to the cancellation of the talk. Others supported the decision, such as the Nobel Prize winner I. Rabi. Margaret Mead was asked to write to Shockley to “take the heat off” the B.P.I. Faculty. Mead wrote Shockley a five page typed single spaced letter. In the letter Mead detailed her objections to Shockley speaking on the topic of “Human problems and Research Taboos”. Her first objection was that Shockley was picked to speak at this scientific conference because of his expertise and his stature as a Nobel Prize winner in Physics. His speaking on the topic that he was going to was not related to his expertise and was a classic case of “bait and switch”. She also criticized Shockley for selective use of data on race and intelligence and provided many counterfactuals to the conclusions of Shockley. In a letter to Professor Robert T. Beyer, the President of the Brown Chapter of Sigma Xi, Shockley rebutted the notion that the cause of his cancellation was the fear of violence. Rather, he pointed out it was the fear of his ideas which mainly the Social Science faculty at B.P.I. objected to “vote to cancel on three days notice a program agreed upon more than two months in advance by responsible individuals because of objection to the presentation of one speaker's ideas is an action that I deplore as typical of the pseudo-intellectual distrust of open discussion of unpalatable ideas that characterize what I label inverted liberalism” (Mead, 1976). He castigated what he termed the inquiry-dogmatism-agony syndrome in academia that emulates the “type of self-deception that

led to the Spanish Inquisition, the Nazi horrors, the Salem witch hunt and the Lysenko “syndrome” (Mead, 1976).

#### **Psychology and Neurology in different National Contexts:**

### **75. Psychologists as Naturalists: The Collecting Instinct in Early American Psychology**

Jacy L. Young, York University, Canada (jlyoung@yorku.ca)

In 1901 American psychologist G. Stanley Hall asserted that, “the modern psychologist is also to some extent a naturalist” (p. 138), one who collects information on every aspect of mental life available to them. At the time this statement was made, Hall and his extended network of associates had been collecting, through the distribution of questionnaires, information on various aspects of mental life since the early 1880s and had distributed more than 80 questionnaires. This number would rise to 194 by the time Hall’s questionnaire project ended in 1915 (Hall, 1924). As early on in the project as 1896, Hall could assert that he had received “at least a hundred thousand returns” to his questionnaires (p. 184). As this assertion indicates, Hall’s questionnaire-based psychological research was a vast undertaking, yet it receives little, if any, mention in histories of psychology. Rather, the oft-repeated narrative of disciplinary psychology’s emergence focuses on the experimental, laboratory-based origins of early American psychology. In actuality, the new discipline of psychology adopted a plurality of methods, one of which was the questionnaire. In this presentation, I will discuss Hall’s questionnaire-based research as part of the larger social scientific effort to under the aggregate, as well as cast questionnaire-based research as an important precursor to psychology’s enthusiastic adoption of statistical techniques. By examining the specific questionnaires distributed by Hall and his colleagues, including one on the collecting instinct itself, early questionnaire research will be discussed in terms of its capacity to allow psychologists to take on the role of the naturalist, collecting, analyzing, and categorizing the natural world as they found it. Such early psychological questionnaire-based research was largely non-statistical, providing descriptive accounts of responses to the questionnaires and reporting nothing more than percentage values, if even that. Later psychological research would extend such practices by applying statistical techniques to collected data. While psychological questionnaire-based research was subject to criticism from the very beginning, and derided as unscientific, it will be argued that such research was simply another manifestation of the rise of measurement and the pervasiveness of positivism during this period. The amassing of vast quantities of information via questionnaire, albeit largely non-numerical information, was an important step toward the rise of quantification in psychology.

### **76. Inspiration in Science: The Case of Gestalt Psychology**

Kelsey M. Bradshaw, University of Nevada Las Vegas, USA and W. A. Hillix & Christyna Prounh, San Diego State University, USA (bradshaw.km@gmail.com)

Many influential researchers in various areas of study have been linked to interesting origin myths. Some stories have been passed down for hundreds of years while others have come directly from the researcher. Whether such accounts are accurate or inaccurate is not of importance and typically several types of sequential, overlapping, cognitive processes generally precede invention or discovery. Psychology has its own origin myth; it is Newman’s 1944 account of the origin of Gestalt psychology in a train ride taken by Max Wertheimer through the Rhineland. Wertheimer was reportedly traveling by train from Vienna on his way to Frankfurt, thinking about ideas related to apparent motion, when he had a significant insight into the phenomenon. Wertheimer stopped at Frankfurt am Main, purchased a zoetrope, and tested his ideas in a hotel room (King & Wertheimer, 2005). Wertheimer’s epiphany as he saw apparent movement from the train has been the core story of the founding of Gestalt psychology. However, Wertheimer’s inspiration may have accorded with Edison’s evaluation. The present paper reviews some additional influential factors in the history of Gestalt psychology. It connects these factors in order to show that, even though Gestalt psychology was born out of experiments in Frankfurt, which may have been inspired

by the circumstances described in the origin myth, some of its roots can be traced to Berlin, where the founders had started to conceptualize their own Gestalt theory. We focus on an obituary Herbert Langfeld wrote for Carl Stumpf in which he describes discussion of Gestalt ideas between the founders before the Gestalt movement in Frankfurt (Langfeld, 1937). Further, it describes the earlier links between the co-founders (Koffka and Koehler) and Wertheimer, thus relating the origin more closely to events that preceded it.

**77. “A Career Largely Concerned with Investigation”: EA Carmichael, the British State, and Clinical Neurology Research, 1925-1955**

Stephen T. Casper, Clarkson University, USA (scasper@clarkson.edu)

Government patronage of fulltime biomedical researchers was largely without precedent in Interwar Britain. The fulltime clinical investigator was rare. Typically the career path of young doctors included research towards the higher medical degree (MD), which in part established their reputations as practitioners in a special area of medicine. From there, however, it was rare that these doctors continued clinical investigations. Their time was almost wholly given over to hospital medicine with the ultimate goal being private practice. Thus it was often and widely remarked that many of the most important discoveries in British medicine had occurred as long, thankless and unrecognized evenings of work, often taking place in dark basements or other poor facilities that served as ad hoc laboratory space. While such rhetoric obviously served to fashion a heroic narrative about the advance of British medicine, it also spoke to the reality that research facilities in medicine, as well as funds to support them, were rare indeed. These observations were certainly true for British neurology. Consequently this paper describes the career of Britain's first fulltime clinical neurological researcher, Edward Arnold Carmichael, Director from 1932 of the Medical Research Council's Clinical Neurology Research Unit at the National Hospital, Queen Square. Carmichael's career perhaps best exemplifies the challenges to creating government positions for biomedical research. In the absence of precedents, no one was entirely certain how achievement and excellence in research performance could be measured. That problem haunted Carmichael and his unit throughout his career. It is a problem that seemingly remains a commonplace even today.

**Psychology and Neurology in different National Contexts:**

**78. A Legacy of the Edinburgh Phrenology Debates: Mind, Brain and British Psychology in the 1870s**

Michael A. Finn, University of Leeds, UK (ph07maf@leeds.ac.uk)

The journal 'Mind' was founded in 1876 by Alexander Bain. Though it is now better known as a journal of analytic philosophy, its original aim was to question "the scientific standing of psychology". Bain, a Scottish philosopher in the tradition of association psychology, was concerned with advancing the methods and discipline of psychology in Britain to that of a natural science, and personally covered the initial costs of 'Mind' to reach these ends. Two years later, in 1878, the journal 'Brain' was co-founded by one of Bain's former pupils, the physiologist David Ferrier, along with neurologist John Hughlings Jackson and psychiatrists John Charles Bucknill and James Crichton-Browne. Ferrier was famed for his recent experiments in cerebral localisation and 'Brain', a journal of neurology which continues today, became an organ for further localisation studies in the late-nineteenth century. The two journals, 'Mind' and 'Brain', represented two opposing approaches to the study of mental functions, and in themselves constituted important developments in British psychology and neurosciences. Drawing on a range of archive material and original articles, this paper will look at the background to these two journals, which it is argued is a shared one, stretching back to the notorious phrenology debates in Edinburgh during the 1820s. Those debates, which saw the established moral philosophers of the university clash with the

younger, outsider supporters of phrenology, have been greatly studied by historians interested by the roles of evidence and ideology in clashes of science. The two sides of the phrenology debates are here considered to be reflected in the later journals 'Mind' and 'Brain': on the one side, a more philosophically-minded group concerned with the use of introspection in studying the mind; on the other, a group convinced by the use of physical observation and experimentation as evidence of the brain's operations. Moreover, there was a strong lineage between the actors of the 1820s and the 1870s. Bain was intellectual heir to the Scottish moral philosophers, whilst the founders of 'Brain' – especially Crichton-Browne – were clear ancestors in the phrenological tradition: their work even being referred to as the “new phrenology”. The formation of the journals 'Mind' and 'Brain' represents an important moment in the disciplinary foundations of psychology and the neurosciences in Victorian Britain, and moreover their appearance reflected two alternative methods of study which can be traced back to the famous phrenology debates.

**79. Frenzy, Delirium, Melancholy and Madness: Cephalick Diseases and the Soul in Thomas Willis's *De anima brutorum* (1674)**

Susan McMahon, Independent Scholar (susan.mcmahon@shaw.ca)

The physician Thomas Willis may have been the most successful medical practitioner in London during the early Restoration (1665-1675). As a natural philosopher, he was already well known for his anatomy of the brain and nerves, described in the *Cerebri anatome* of 1664, and especially for the eponymously named Circle of Willis, the arrangement of the brain's arteries in cerebral circulation. In the *De anima brutorum* (1674), Willis's comparative anatomy of animal and human brains, he also attempted to define and describe disorders of the brain. He believed that brain disorders, or cephalic diseases, were afflictions of the soul. Diseases which he distinguished as belonging to the Corporeal Soul had a physical cause arising from the “marrowy part of the Brain”. Diseases of the Sensitive Soul were characterized as having either a disturbed or perverted imagination, or a deformed, distracted or confused intellect. However, Willis makes no clear demarcation between diseases of the Corporeal Soul and diseases of the Sensitive Soul; rather, he presents a continuum of diseased states that implicate both Souls. This paper will examine Willis's nosology of brain disorders and its impact for understanding mental health in the seventeenth century.

**80. Stuck Inside, Immobile, With the Motor Blues Again**

Paul Bernard Foley, Neuroscience Research Australia, Australia (p.foley@neura.edu.au)

*Encephalitis lethargica* (EL) was an epidemic neuropsychiatric disorder that appeared at a time when the separation of psychiatry (functional brain disorders) from neurology (organic brain disorders) appeared possible. EL disturbed the demarcation of the territories of the two halves of clinical neuroscience: its complex symptomatology included neurologic symptoms – including cranial nerve palsies, sleep disorders and parkinsonism, all of which could be referred to focal injuries to the brainstem – but also a broad range of psychiatric phenomena, including personality changes, as well as syndromes that resembled neuroses (including hysteria) or psychoses (schizophrenia). Psychiatrists and neurologists were enjoined by this realization to explore the internal experience of their EL patients and, in a few instances, their own experience of the disease. These explorations indicated that will was not a single, monolithic function of the 'self', but rather an intricate co-ordination of the highest and lowest levels of brain function, of the cortex and brainstem. This window into the mind was opened by the fact that, although deficits in cognitive and affective function were quite evident in EL, the majority of patients retained insight into their condition and suffered little intellectual loss, despite the external facade of motor and mental immobility. This provided an unprecedented opportunity to explore the inner workings of the human mind without recourse to speculative constructs. The individual experience of the EL

patient thereby evinced great interest from both neurology and psychiatry, and played a major role in contemporary discussions of the nature of neurotic and psychotic illness.

**Sunday Afternoon, June 19, 2011**

**Developments in Clinical Neurology:**

**81. A History of Trigeminal Neuralgia from John Locke to Harvey Cushing**

Edward J. Fine, University at Buffalo, USA (efine@buffalo.edu) and  
Bijal Mehta, University at Buffalo, USA (Bijal@yahoo.com)

The original description of trigeminal neuralgia (TGN) remains controversial as the current diagnostic criteria cannot be applied readily to prior case reports. John Locke described in a letter of 4 December, 1677, the facial pain of Countess of Northumberland. She suffered from “a flash of fire [...] all over the face and mouth, which made her shriek out, her mouth drawn towards the right eare [...]”. Nicholas André described in 1756 several patients’ facial pains as “cruel and violent... that puts the face in violent agitation, causing a hideous grimace”. He attributed the etiology to a form of convulsion. John Fothergill wrote in 1773 a description that closely resembles modern concepts of TGN: the disease afflicts the elderly, is “sudden and excruciating, lasting a quarter of a minute, then goes off.; the gentlest touch of a hand or handkerchief will sometimes bring on pain”. He stated that patients’ grimaces were responses to facial pain. John Fothergill advocated an extract of hemlock in 1769 as treatment of TGN, but reported in 1773 improved results with an extract of Peruvian bark, that is currently known to contain quinine. His nephew, Samuel, decried the term *tic douloureux* and expanded his uncle’s semiology of TGN. In 1820 Benjamin Hutchinson after extensively reviewing treatments that gave transient relief: purging, mercury, opium and arsenic, reported success with iron carbonate pills. Despite dubious claims for successful medical treatments, surgical excision of the ganglion of the trigeminal nerve was attempted after the trigeminal nerve was identified as the afferent supply of the face, mandible and maxilla. J.M. Carnochan in 1858 successfully excised maxillary branches of the fifth cranial nerve relieving severe TGN pain of 3 patients. In 1891 Victor Horsley, J Taylor and William Coleman described trephining through the temporal bone, elevating the temporal lobe and evulsing roots of the trigeminal nerve to control TGN pain. In the USA, William W Keen, Jr. (WWK) reported 11 trigeminal ganglion resections: 2 deaths from hemorrhage from rupture of middle meningeal artery and 1 death from infection, 7 successful surgeries and no improvement for 1 patient. Frank Hartley was first to develop an extra-dural operation to remove diseased Gasserian Ganglia. In 1900 Harvey Cushing (HC) modified Hartley’s method to avoid inter-operative rupture of the middle meningeal artery. Samuel Fothergill’s description of trigeminal pain approximates current semiology of TGN: sharp facial pain, usually localized to the lower 2 sensory divisions of the trigeminal nerve, lasting less than 2 minutes and elicited by light touch, cold exposure, speaking or mastication. Hartley’s and HC’s techniques of extra-dural extirpation of trigeminal ganglia reduced mortality and gave lasting relief from TGN.

**82. Emergence, Evolution, and Resolution: The History of Multiple Sclerosis Investigations in Canada between 1850 and 1950**

Aravind Ganesh and Frank W. Stahnisch, University of Calgary, Canada (aravindganeshy@yahoo.ca)

The modern medical profession’s quandaries with Multiple Sclerosis (MS) are seen to have begun in 1849, with the early description of the clinical and pathological features of what was termed *Hirnsklerose* (“brain sclerosis”) – by Friedrich von Frerichs (1819-1885), one of the first neuropathologist to diagnose the disease in living patients. This paper provides an overview of the century of research (1850-1950) that followed the emergence of this clinical entity, described by Hans Lassmann (born 1949) as a ‘golden centenium’ for the evolution of medical understanding of MS, with a focus on the Canadian perspective.

Using journal articles, reviews, and case studies, this paper outlines the diagnostic challenges that confronted early Canadian neurologists in their encounters with MS, as well as their attempts to understand its aetiology. These activities were influenced by developments in the field in Europe and the United States. Since MS initially emerged from the nosological category of *Paralysis Agitans* (“Parkinson’s Complex”), one of the major challenges encountered was the discrimination between these two conditions. Consequently, starting with Jean-Martin Charcot (1825-1893), who described the eponymous triad for MS, there was excessive emphasis on the intention tremor of MS, contrasted with the rest tremor of *Paralysis Agitans*. Other key clinical entities that confounded the diagnosis of MS included spinal cord tumours, hysteria, and neurosyphilis (particularly *Tabes Dorsalis*). Early physicians relied on clinical features and fine details of the neurological exam to differentiate these conditions with lab tests like the reaction of August von Wassermann (1866-1925) for syphilis and later cerebro-spinal fluid (CSF) cell counts eventually aiding diagnosis. The germ theory of disease spurred attempts to characterize an infectious aetiology for MS, while others explored various toxic aetiologies. Ultimately, the advancements made in the characterization of MS and the resolution of its differential diagnoses, set the stage for the modern era of immunologic and therapeutic research.

### **83. Lenin’s Neurologist: Vassily Kramer and His Impact on Soviet Neurosurgery**

Boleslav L.Lichterman, The Burdenko Neurosurgery Institute, Russia (lichterman@hotmail.com)

Vassily Vasiljevich Kramer (1876-1935) was one of the leading Soviet neurologists. He was a master of topical diagnosis and one of the doctors who examined Vladimir Lenin after repeated strokes from May 1922 until January 1924. Kramer’s unpublished memoirs about Lenin as a patient provide an unusual psychological portrait of a Revolutionary leader. In 1920s Kramer organized a neurological department at a State Roentgen Institute in Moscow. A first Moscow neurosurgery clinic emerged there in 1929. In 1932 it was transformed into Neurosurgical Research Institute (now the Burdenko Neurosurgery Institute) where Kramer became a deputy director. Kramer introduced Brodmann’s teaching on cerebral localization into clinical practice. He wrote on right- and left handedness, inverted vision, and described several clinical syndromes of cerebral tumors. Several of his pupils became professors of surgical neurology.

### **84. The Historical Milestones in the Development of the Science and Technique of Deep Brain Stimulation (DBS) Surgery Result from Centuries of Innovation**

Shabbar F. Danish; Casey H. Halpern and Gordon H. Baltuch, Robert Wood Johnson Medical School and Hospital of the University of Pennsylvania, USA (casey.halpern@gmail.com)

Although the brain and muscles of the body have been thought to communicate through electronic connections for over 200 years, the development of therapeutic electrical stimulation slowly followed that of cortical and extrapyramidal ablation surgery. Surgery for movement disorders was first attempted in 1890 when a cortical ablation procedure was performed for dyskinesia with devastating complications. Later, when an “extrapyramidal” motor system was described, new surgical targets developed, and electrocoagulation was attempted with promising results. Meanwhile, although the recording microelectrode was invented in 1921, it would not be fashioned for use in surgery for humans until 1961. The stereotactic head frame was invented in the early 1900’s, but a frame would not be suitable for targeting deep brain structures until Spiegel and Wycis incorporated ventriculography in 1947. Together, microelectrode recordings and the stereotactic head frame would allow for safer and more accurate surgical targeting. Nevertheless, with the introduction of L-dopa in the late 1960’s, deep brain stimulation (DBS) progressed as a technique only through its use in psychiatric diseases and pain control. In 1970 an implanted prosthesis was developed to provide chronic therapeutic stimulation, and when side effects of L-dopa therapy became major limitations in treating Parkinson’s disease effectively, movement disorder surgery in the form of DBS came to the forefront. We review the literature to piece together

pioneering work from multiple disciplines that has brought about this modern-day surgical technique proven effective in treating many intractable disorders.

#### **Developments in Clinical Neurology:**

### **85. The Society of Neurological Surgeons and the Making of a Professional Self**

Delia Gavrus, University of Toronto, Canada (delia.gavrus@utoronto.ca)

In the spring of 1914, a scandalous event took place at the New York Academy of Medicine. The members of the Academy walked into the meeting room to find a display of fifteen glass jars, each containing the brain of a child who had recently died on the operating table. As one newspaper recounted, a dozen children then “paraded down the centre aisle,” while doctors stood on chairs in order to get a better view of the unfolding medical spectacle. The surgeon giving the talk – Dr. William Sharpe from the New York Polyclinic medical school and hospital – was demonstrating the transformative effect of brain surgery for select cases of spastic paralysis. The next day newspapers reported the event under dramatic headlines (“Brain Operation as Paralysis Cure”), and Sharpe was immediately called in to face a special committee at the Academy of Medicine, which accused him of deliberately staging a dramatic performance in order to gain publicity. Sharpe denied the accusation, and the Academy did not formally punish him, but his flair for the dramatic did carry a steep professional price. Sharpe was excluded from an emerging community of brain surgeons, a community which slowly throughout the 1920s and 1930s began to set the official agenda for neurosurgery in the United States and Canada and dictated the official history of this medical specialty – a history Sharpe was written out of, both in terms of his therapeutic procedure for spastic paralysis and in terms of his own professional persona as a brain surgeon. Drawing on extensive archival material as well as on published newspaper and magazine accounts, I chart the formation of the first specialist neurosurgical society (the Society of Neurological Surgeons, est. 1920) by paying particular attention to the self-described neurosurgeons, like William Sharpe, who were excluded from this society. I do so in order to reconstruct the culture of neurosurgery in the first half of the 20th century and to explain how a particular professional self was fashioned and refashioned in this time period in conjunction with specific moral norms and concerns. The relevance of this paper is twofold: first, it illustrates how medical practice developed in conjunction with this specific professional self, and secondly, it recovers the universe of meaning that these neurosurgeons, drawing upon specific techniques of the self and echoing wider cultural repertoires manufactured in order to make sense of both their surgical practice and their medical identity.

### **86. The Global Expansion of Psychosurgery: Beyond Portugal and the United States**

Brianne Collins, University of Calgary, Canada (bmcollin@ucalgary.ca) and Henderikus J. Stam,  
University of Calgary, Canada (stam@ucalgary.ca)

In the 1930s, four somatic treatments—insulin coma therapy, metrazol shock therapy, electroconvulsive shock therapy, and psychosurgery—were developed in order to address a growing societal and medical need to deinstitutionalize the severely mentally ill (Bellak, 1948). Of these treatments, psychosurgery has been the most enduringly controversial for more than half a century. In the United States, the sensationalized work of Walter Freeman served to popularize the procedure; however, there are other individuals and countries that contributed to the worldwide development and use of psychosurgery. Historical accounts have often overemphasized the work of Freeman and his role in the psychosurgery movement (e.g., Valenstein, 1986). In 1937, Italy’s Amaro Fiamberti developed the transorbital lobotomy variation inspired by the work of Egas Moniz rather than the work of Walter Freeman and James Watts who published their prefrontal lobotomy standard technique in 1942 (Kotowicz, 2008). Italy is joined by other often overlooked countries including Sweden, Norway, Denmark, Israel, Turkey, Australia, the Union of Soviet Socialist Republics (USSR), England and Wales, Japan, Brazil, and

Canada. These are countries where historical sources are available in English. Despite this, there has not yet been a synthesis or evaluation of the international contribution to this radical movement in the treatment of psychiatric illness. Upon a combined analysis of these countries, several clear similarities and differences become evident. Most countries began conducting psychosurgery in the mid-1940s with a subsequent decline occurring in the mid-1950s when the first anti-psychotic drug was released. Commonly cited reasons for performing and justifying the procedure were that of decreasing the overcrowding in institutions, awarding the 1949 Nobel Prize to Moniz, and limiting dissent in the medical community and positive media portrayals. Differences include the political climate in various countries (e.g., Israel and the Israeli-Palestinian conflict), varying degrees of proselytizers promoting the treatment, the variation of the surgery performed, and the decision process used to determine who would receive the operation. Many of these countries share similarities with the United States; however, both the similarities and differences provide important insights into how psychosurgery and other treatments are able to expand so rapidly across the globe amidst clear lack of consensus in the medical community and questionable ethical practices. One important aspect of this project is an examination of the Canadian context. Although it may be surmised that Canada would closely follow its southern neighbor, research on the Canadian contribution to the psychosurgery movement has been largely unexplored. There is evidence that surgeries were being performed in Ontario by the mid-1940s (e.g., Miller, 1967). In addition, there are indications that Saskatchewan's Weyburn Hospital and Manitoba's Selkirk Mental Hospital also conducted psychosurgery (e.g., Lindsay, 1951). The extent of the Canadian use of psychosurgery is not yet known; however, this paper provides some support to indicate that Canada also practiced these procedures along with other countries.

## **87. The History of Remote Handling Technology in Neurosurgery**

Kristian Wasen, University of Gothenburg, Sweden and Meaghan Brierley, University of Calgary, Canada (forskning2003@yahoo.com) and (m.brierley@ucalgary.ca)

The evolution of medicine in general, and the art of surgery in particular, trace its history back to the ancient Greeks through the ritual pledging of the Hippocratic Oath. 'Modern' surgical practice has commonly been understood as either that in the period following the introduction of antiseptics/aseptic (Bynum & Porter, 1993; Harding-Rains, 1977) or that in the post-anesthetics period (Cartwright, 1967; Sullivan, 1996). The subsequent post-modern era of surgical practice began along with the adoption of complex remote manipulation technology (cf. Bicker et al., 2004). In surgery such remote handling technology began to replace the traditional handicraft work and human touch (Wasen, 2008). In this paper, we trace the early history of ideas of remote handling technology and medical tools beginning with transformations in communication technologies in the nineteenth century (Wilson, 1976; Peters, 1999), and cybernetics and robotics in the mid twentieth century (Couffignal, 1958). We then describe the circumstances surrounding the transfer of ideas from these diverse areas of expertise such as tele-robotics in the nuclear industry (Bicker et al., 2004) to a new generation of surgical equipment (i.e. remote technologies) and into one of the most complex medical fields in the neuroscience domain - neurosurgery. Indeed, the making of surgical equipment and instruments represents a long historical tradition of skilled craftsmanship (e.g. Goeranzon et al., 1980). These semi-specialized tools enlarge the tactile and manipulative capacity and power of the neurosurgeon's hands and represent extensions to the human body (cf. Mumford, 1934, 1952). Since the very beginning of traditional invasive neurosurgery, and even before the evolution of specially designed operating rooms (ORs), neurosurgeons have been comfortable in performing their art by putting their hands literally inside patients. Human touch has been a key feature in treating patients since the early days of surgery. The innovative robot technology in neurosurgery solves old constraints, such as the limited ability of human vision and precise hand movements. At the same time new robotic procedures create new constraints, for example it excludes abilities such as the sense of touch received from the surgeon's fingers (often called force or haptic feedback) as well as temperature, viscosity and other characteristics that provide the surgeon with much

information. What has led to dissolving boundaries around traditional cultural and social structures and to what effect? The predominant characteristic of work activities and interaction patterns in robotic neurosurgical work is best described as a “remote proximity”. Remote proximity is defined as “A technology-mediated interaction form in which work activities are carried out remotely, transcending space and geographical limitations by manipulating physical objects.” Neurosurgeons now interact without being physically co-present next to the patient or the OR team, their “interactions occur in abstract place, not in a locally situated place” (Tsoukas, 2005, p. 41). Following Jonsson and Holmstroem (2005), remote proximity creates new opportunities as information compatibility and mobility is gained. At the same time a sense of locality may be lost during the transformation between the close and the remote. Drawing on the historical case of the implementation of robotic technology in neuroscience, we argue that a new generation of groundbreaking technology may bring about changes in the social and organizational context of medical practice.

## **88. The Evolution of The Evolution of Robotic Neurosurgery: A New Paradigm in Surgical Education**

Jason Motkoski, University of Calgary, Canada (jwmotkos@ucalgary.ca) and  
Kristian Wasen, University of Gothenburg, Sweden (forskning2003@yahoo.com)

The goal of optimized patient outcome has united neurosurgeons throughout history. Operating within the cranial space is a challenge, but technological innovation has allowed the surgeon to become improve accuracy and therefore patient outcomes. These technologies have included the introduction of electrocautery to coagulate blood vessels and control blood loss, the operating microscope to better visualize the surgical site, operating lights to improve illumination of the pathology, and imaging technology for enhanced lesion localization. These technologies, when coupled with medical education and surgical skill, have led to the development of the ‘modern day’ neurosurgical training paradigm. The English word surgery originates from the two Greek words *cheir* (the hand) and *ergon* (a work), referring to manual labour in the medical art of healing. However, the notion of surgery as an art or a handicraft practice has been challenged in the last decades of the 19th century. More regulated and uniform teaching curriculums emerged during this period at medical schools and teaching hospitals. As Star (1982) maintains, this re-configuration can be understood as the professionalization of modern surgery, through ensuring that the licensed surgical professionals had a defined education and by associating that training with research and rigorous scientific principles (see also Wangenstein & Wangenstein, 1978). Moreover, groundbreaking discoveries in medical knowledge and new innovative technologies have led to an increase in subspecialization (Toyota, 2005, Weisz, 2003). Recently, robotic technology has entered the neurosurgical operating theatre and represents a new subspecialization within neuroscience and neurosurgery. Robotics has evolved, independently of medical applications, in many sectors including manufacturing, resource extraction and for other industrial applications where repeatability, precision, accuracy, strength, and endurance are required. In the operating suite, robotics can provide many of these same strengths, but must operate with utmost concern for safety of the patient and surgical staff. Unlike previous technology robotics have the potential to integrate many existing surgical technologies and simplify the number of independent technologies required for any single operation. For this reason, it is paramount that new surgical trainees are taught how to use robotic systems and the proper role of these systems in the contemporary neurosurgical operating room. The objective of this paper is to review the evolution of neurosurgical robotic technology and the changing conditions for neurosurgical training in education hospitals. This historical perspective enhances understanding of the roots of neurosurgical culture and apprenticeship in the more distant past, and informs current understanding of how neurosurgical education is continuing in its evolution.

### **History of Psychoanalysis:**

**89. Freud's Project for a Scientific Psychology (1895): Historical and Contemporary Explorations of an Early Neural Network Model**

Katherine A. Harper, York University, Canada (harper@yorku.ca)

In 1895, Sigmund Freud developed one of the earliest neural network models in his Project for a Scientific Psychology. In this work, Freud developed a neural network model emphasizing the importance of neuronal connections and the activity at cell junctions (contact barriers). Freud established a neurological explanatory framework for psychic functioning in which cellular proximity and neuronal groupings were crucial to the development of cognition, thought, and memory. Today, Freud's Project is the foundation of the recently developed field of Neuropsychanalysis, an interdisciplinary discipline based on the idea that psychoanalysis and neuroscience have similar goals and study similar "objects," and as such, should combine their research efforts. Moreover, neuropsychanalysts believe that Freud's model may be able to bridge the gap between psychoanalysis and neuroscience. That said, the question remains, why during the past decade has there been a renewed interest in Freud's historical manuscript? I speculate that this revival is in part due to a paradigm shift that has occurred in the area of neuroscience research where the focus is now on unconscious processes. More specifically, the so-called psychoanalytic "objects" of pleasure/pain pathways, unconscious motivation, emotion, and cognition are now being investigated (LeDoux, 1999, 2000; Panksepp, 2003; Schore, 1997, 2002; Solms, 1999). Thus, it is not surprising that Freud's Project, which theorized about these "objects," is making a comeback. The idea of integrating psychology and neurophysiology has a long history dating back to the mid 19th century with the advent of the neuron doctrine. Since this time, neural network theories have been contemplated, partially formulated, and then later dropped or ignored so there have been long interruptions in this particular line of investigation for a variety of reasons. Moreover, Freud's noteworthy effort to create a neural network theory of cognitive and emotional functioning has been omitted from the histories of neuroscience and the history of neural networks and this paper, in part, attempts to remedy this by explaining the neural network theories Freud put forth in the Project. The overall goal of this paper is to explore Freud's Project from both historical and contemporary frameworks. This paper will be divided into three parts. Part one will introduce the reader to Neuropsychanalysis, exploring the history of this field and explaining the role that Freud's Project for a Scientific Psychology played in its development, answering the questions, what is neuropsychanalysis and where did it come from? Part two of this paper will be historical in nature and will explain Freud's neural network model emphasizing his theories of pleasure and pain, consciousness, the unconscious, affect, cognition, and memory (the objects that are also the focus of contemporary research). Finally, part three of this paper will look at how Freud's Project is being applied in Neuropsychanalysis today. More specifically, I will be looking at contemporary Neuropsychanalytic theories through a historical lens as I raise questions about how today's theories differ from the historical models, how Freud's model is being integrated today, and how recent evidence about brain physiology, and today's technology, is allowing researchers to overcome the difficulties Freud faced in creating his neuropsychanalytic model more than 100 years ago. This paper will end with a brief discussion of the future of psychoanalysis as an interdisciplinary field, noting some of the advantages and disadvantages to integrating psychoanalysis and neuroscience, particularly, if Freud's Project is used as the foundation.

**90. Linked by Language: Integrating Neurology, Psychoanalysis, and Linguistic Anthropology at Yale's International Seminar of 1932**

Leila Zenderland, California State University, USA (lzenderland@fullerton.edu)

This paper explores a group of interdisciplinary and international social scientists who all worked together in the 1930s. It focuses on scholars associated with the "Seminar on the Impact of Culture on Personality," an unusual educational experiment sponsored by the Rockefeller Foundation and held at

Yale University's Institute of Human Relations during the 1932-33 school year. Participating in this seminar were a large group of American social scientists as well as 13 social scientists from Europe and Asia. This paper will focus in particular on work done by four of these researchers, two American and two European: Edward Sapir, Harry Stack Sullivan, Andras Angyal, and Niilo Maki. While these social scientists came from very different backgrounds, all four shared an interest in language that allowed them to blend together their research interests in neurology, psychoanalysis, and anthropology in unusual ways. The first and most influential of these was this Seminar's leader, linguistic anthropologist Edward Sapir. A student of Franz Boas as well as an expert on Native American languages, Sapir made questions of language, meaning and symbolism central to the work of this Seminar. The second of these scholars, psychiatrist Harry Stack Sullivan, was a close friend of Sapir's who became a frequent lecturer at this Seminar and who later worked closely with several of its international participants. Building upon Freud's work on aphasia, Sullivan developed his own theories about the significance of language in treating schizophrenia. The third Seminar participant, Hungarian psychiatrist Andras Angyal, had studied Gestalt theories in Germany and later developed his own holistic approach to psychotherapy; after this Seminar ended, he became Director of Research at Worcester State Hospital, where he too explored connections between language disturbances and mental illness. The fourth participant, Finnish psychologist Niilo Maki, had studied neurology with Kurt Goldstein and Adhemar Gelb in their laboratory in Frankfurt, Germany, in the 1920s. As a member of this Yale Seminar, Maki blended his past experiences in researching the effects of brain injuries on language and drawing with a new interest in cultural anthropology. My paper will focus on the ways that these four scholars, who worked in different disciplines and who studied different cultures, continued to interact with one another in addressing issues related to language after this seminar ended. For instance, at a 1939 meeting of the American Psychiatric Association, Harry Stack Sullivan and Andras Angyal (as well as Kurt Goldstein) worked together on a session called "Language and Thoughts in Schizophrenia"—material that later evolved into a book. In a similar way, Edward Sapir arranged for Niilo Maki to conduct anthropological studies at the School of American Research (SAR) in Santa Fe, New Mexico during the summer of 1933. Maki's study focused on left- and right-handedness among Native American artists. Returning home to Finland, Maki later became the head of a national hospital treating brain-injured soldiers. In recent decades, studies exploring language, discourses, and meaning have received enormous scholarly attention across many disciplines. This paper will consider an earlier attempt to use language to combine disciplinary insights. It will show how all four of these participants in the Yale Seminar of 1932-33 used the study of language to link together neurology, psychology, psychoanalysis and anthropology in ways that proved influential during the middle decades of the twentieth century in different parts of the world.

**91. A Natural History of the Freudian Movement Organization in the Mental Health Field:  
A Comparative Survey**

Samuel Lézé, Ecole Normale Supérieure de Lyon, France (samuel.leze@ens-lyon.fr)

My paper aims to test a theoretical model to explain an historical fact: the change in social authority of the Freudian movement organization within the mental health field. In particular, I address a smaller conundrum again less frequently addressed: the nature and function of the anti-Freudians in this history. Based on a comparative survey of the historiographic literature and an historical anthropology of the current situation in France, two kinds of general regularities are revealed in all national contexts under study : firstly, four main phases in the dissemination of the Freudian *mouvement* organization ; secondly, three core organizational tensions of the Freudian *mouvement*. This theoretical model, certainly incomplete and debatable, provides a stimulating platform for promoting the comparative analysis in the history of the behavioral and social sciences.