

**CHEIRON: The International  
Society for the History of the  
Behavioral and Social Sciences**

**45<sup>th</sup> Annual Meeting  
June 20-23, 2013**



**University of Dallas, Irving, Texas**



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University of Dallas, Irving, Texas, June 20-23, 2013**

**PROGRAM AND ABSTRACTS**

**Program**

Local Host: Robert Kugelmann (University of Dallas)  
Program Chairs: Larry Stern (Collin College) & Barbara Lusk (Collin College)

All sessions will take place in Gorman A. Breaks will take place in the Gorman Faculty Lounge.

**Thursday June 20, 2013**

**1:00-2:45pm PAPER SESSION #1: Behaviorism**

Chair: Kathy Milar (Earlham College)

Russell A. Powell (Grant MacEwan University), Nancy L. Digdon, (Grant MacEwan University) and Christopher T. Smithson (Professional Genealogist), *Searching for Little Albert: Evidence of a Second Candidate for "Psychology's Lost Boy"*

Jose Maria Gondra (University of the Basque Country, Spain), *"Rich Men Over 50 Kill Selves for Lack of Woman Attention!": John B. Watson's Changing Views on Suicide*

Sergio Dias Cirino (Universidade Federal de Minas Gerais), Anna Christina P. M. Passarelli (Universidade Federal de Minas Gerais), and Rodrigo Lopes Miranda (Universidade Federal de Minas Gerais), *Circulating Behavior Analysis in the 1960s: An Analysis of Three Different Translations*

**2:45-3:00pm BREAK**

**3:00-5:00pm SYMPOSIUM #1: Exploring the Materiality of Psychiatric and Psychological Instruments**

Chair: Alexandra Rutherford (York University)

Brianne M. Collins (University of Calgary) and Henderikus J. Stam (University of Calgary), *It's a Leucotome not a Lobotome: Reconsidering the Traditional Lobotomy Narrative Using a Material Culture Approach*

Jennifer L. Bazar (York University), *The Utica Crib: Contextualizing the Role of Asylum Restraints in Contemporary Memory*

Jacy L. Young (York University), *Test or Toy? The Cattell Infant Intelligence Scale as Liminal Object*

Michael Sokal (Worcester Polytechnic Institute), *Discussant*

**6:00-7:45pm POSTER SESSION & RECEPTION (Upper Haggar)**

Alan S. Kornspan (University of Akron), *The Sport Psychology Laboratory at the University of Illinois: The Work of Alfred W. Hubbard, 1950-1970*

Alan S. Kornspan (University of Akron), *John Lawther: Contributions to the Psychology of Sport*

Miki Takasuna (Tokyo International University), *Important Literature Referenced in Textbooks on the History of Psychology: A Preliminary Citation Analysis Using 13 Textbooks Published after 2001*

Kohei Yoshida (Tokyo Metropolitan University), *American Sociological Research Response to the Post Great War Crisis: An Historical Account of the Rise and Fall of the Methodological Asceticism*

Chin-hei Wong (University of Hong Kong), *Two Studies on the Psychology of Sex in 1920s China and their Implications*

**8:00-9:30pm MOVIE NIGHT (Gorman A)**

Lizette Royer Barton (Center for the History of Psychology), Cathy Faye (Center for the History of Psychology), and Jodi Kearns (Center for the History of Psychology), *Advice for Parents: Films From the Center for the History of Psychology*

**9:30-11pm Pub Night in the Ratskeller**

**Friday, June 21, 2013**

**8:30-8:45am WELCOME (Gorman A)**

Introduction: Robert Kugelmann (University of Dallas)

Dr. Thomas Keefe, President of the University of Dallas

**8:45-9:45am PAPER SESSION #2: Conducting Psychological Research: Methods and Tools**

Chair: Cathy Faye (Center for the History of Psychology)

Thomas Heinzen (William Paterson University), *The Horse That Will Not Go Away: Spontaneous Social Self-Deception*

David C. Devonis (Graceland University) and Deborah Rowe (Clarke Community Schools), *An Excursion in the Archeology of Recent Apparatus: The Ball and Spiral Test*

**9:45-10am**                    **BREAK**

**10:00-12pm**                **PAPER SESSION #3: Applied Psychology**

Chair: Kenneth D. Feigenbaum (University of Maryland University College)

Michael Phillips (Collin College), *Doctors, Planters, and Doomsday Preachers: The Surprising Failure of the Eugenics Movement in Early 20th Century Texas*

Ann Johnson (University of St Thomas) and Elizabeth Johnston (Sarah Lawrence College), *“Up the Years with the Bettersons”: Gender and Parent Education in Pre-WWII America*

Cathy Faye (Center for the History of Psychology), *The Foreign Morale Analysis Division and the Study of Japanese Morale, 1943 – 1946*

**12:00-1pm**                    **LUNCH**

**1:00-3pm**                    **SYMPOSIUM #2: Issues In The Historiography of the Behavioural and Social Sciences**

Chair: Katalin Dzinás (Independent Scholar)

Adrian C. Brock (University College, Dublin), *Revisiting the Whig Taboo*  
Rebecca Lemov (Harvard University), *Teleology and Historiography from Behaviorism to Cybernetics*

Alexandra Rutherford (York University), *Gender Analysis and the History of Psychology: Developments and Future Prospects*

Henderikus J. Stam (University of Calgary), *Are There Still Historical Explanations? On the Question of a Historicized Psychology*

Wade Pickren (Ithaca College), *Discussant*

**3:00-3:15pm**                **BREAK**

**3:15-5:15pm**                **PAPER SESSION #4: Research Schools and Programs in the Social Sciences**

Chair: Christopher D. Green (York University)

Kenneth D. Feigenbaum (University of Maryland University College), *Maslow's Graduate School*

Jason Hughes (Brunel University) and John D. Goodwin (University of Leicester), *Norbert Elias, Ilya Neustadt and the "Leicester School" of Sociology*

Gerald Sullivan (Collin College), *Marcel Mauss, Embodiment & Exchange: Another Psychological Anthropology?*

**6:30 & 7:00pm**      **Dallas Museum of Art (Bus—Meet at the Tower—early bus and later bus)**

**9:30 & 10:00pm**      **Buses return to UD**

**Saturday, June 22, 2013**

**8:45-9:45am**      **PAPER SESSION #5: Building Disciplinary Histories**

Chair: Barbara Lusk (Collin College)

Michael R. Hill (Editor, *Sociological Origins*), *Writing and Editing Departmental Histories in Sociology*

Christopher D. Green (York University), Ingo Feinerer (Vienna University of Technology) and Jeremy T. Burman (York University), *Networking Psychological Review, 1894-1898*

**9:45-10am**      **BREAK**

**10:00-12pm**      **PAPER SESSION #6: The Contributions of Settlements and Women in the Developing Social Sciences**

Chair: Larry Stern (Collin College)

Joyce E. Williams (Middle Tennessee University) and Vicky M. MacLean (Middle Tennessee University), *Settlement Sociology in the Progressive Era: Early Contributions to a Public Sociology of Faith, Science, and Reform*

Shayna Fox Lee (York University), Michael Pettit (York University) and Alexandra Rutherford (York University), *Networking Science and Reform: The Prosopography of the First Generation of Chicago-Trained Female Social Scientists*

Allison L. Rowland (University of Colorado-Boulder) and Peter Simonson (University of Colorado-Boulder), *The Founding Mothers of Communication Research: Toward a History of a Gendered Assemblage*

**12:00-1pm**      **LUNCH**

**1:00-2:30pm KEYNOTE ADDRESS (Gorman A)**

Chair: Larry Stern (Collin College)

Mary Jo Deegan (University of Nebraska), *Jane Addams and the Hull-House School of Sociology: Specializations, Leaders, and Social Movements, 1889-1935*

**2:30-2:45pm BREAK**

**2:45-4:45pm PAPER SESSION #7: The Development of Psychology in International Contexts**

Chair: Wade Pickren (Ithaca College)

Zhipeng (Simon) Gao (York University), *Revisiting Chinese Psychology in the 1950s: Political Intervention, Dogmatism and Resistance*

Ana Maria Talak (Universidad Nacional de La Plata, Argentina), *Jose Ingenieros and the Former Psychology in Argentina: Knowledge, Practices, and Values in the Shape of the New Discipline*

Rodrigo Lopes Miranda (Universidade Federal de Minas Gerais), Sergio Dias Cirino (Universidade Federal de Minas Gerais), and Regina Helena de Freitas Campos (Universidade Federal de Minas Gerais), *The Psychological Laboratory of the Belo Horizonte Teachers' College: Circulating Knowledge for the Modernization of the Brazilian School System in the First Half of the 20th Century*

**5:00-6:15pm BUSINESS MEETING (Gorman A)**

**6:30-9:45pm BANQUET (Upper Haggar)**

**Sunday, June 23, 2013**

**8:00-10am PAPER SESSION #8: Institutions and Mental Health**

Chair: Gerald Sullivan (Collin College)

James M. M. Good (University of Durham), *William Stephensen and the US NIMH: Lost Opportunity or Springboard for a Revitalized Career?*

Ian Lubek (University of Guelph) and William Salmon (University of Guelph), *Historical Notes on Psychology's "Health": Tracing "Health Psychology's" Growth at one of its "Local origins" (SUNY at Stony*

*Brook) and its Recent Divergence into "Critical Community Health Psychology"*

Jenifer Dodd (Vanderbilt University), *Rapism: American Psychiatric Understandings of Rape in the 1970s and 1980s*

**10-10:15am**            **BREAK**

**10:15-12:15**            **PAPER SESSION #9: Philosophy/Phenomenology in Psychology**

Chair: Robert Kugelmann (University of Dallas)

Scott Churchill, (University of Dallas), *Koehler, von Uexkuell, and Heidegger: On the Question of Animal Worlds*

Rebecca Dance (University of Dallas), *A Historical Study of the Will*

Frank Scalabrino (University of Dallas), *A Brief History of the Problem of Agent Causation in the Human and Behavioral Sciences with a Recommendation for Future Research*

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**ABSTRACTS**

**Thursday June 20, 2013**

**1:00-2:45pm          PAPER SESSION #1: Behaviorism**

Chair: Kathy Milar (Earlham College)

**Russell A. Powell (Grant MacEwan University), Nancy L. Digdon, (Grant MacEwan University) and Christopher T. Smithson (Professional Genealogist), *Searching for Little Albert: Evidence of a Second Candidate for "Psychology's Lost Boy"***

Our presentation provides new evidence concerning the identity of John B. Watson's Little Albert. In 2009, Beck and colleagues published an article describing their discovery that Little Albert was likely Douglas Merritte, who tragically died a few years after participating in the Watson & Rayner (1920) experiment. More recently, Fridlund, Beck, and others (2012) have further claimed that Little Albert was neurologically impaired from birth, and that Watson, in a severe breach of ethics, deliberately hid this fact in his published descriptions of the case. In our presentation, we will outline evidence of another infant, William Albert Barger (later William Albert Martin, 1919-2007), who matches the characteristics of Little Albert as well as, and in some cases better than, Douglas Merritte. Like Douglas, William Albert was the son of a foster mother at Johns Hopkins Hospital. Also like Douglas, his age would have closely matched the reported age of Little Albert at the time the Watson and Rayner experiment most likely took place. Significantly, William Albert was typically called Albert throughout his life, and at the time of the Watson and Rayner experiment, he would most likely have gone by the name of Albert Barger—which matches the name, "Albert B.", given for him by Watson and Rayner. Unlike Douglas, but in accordance with Watson and Rayner's description of Little Albert, William Albert was not neurologically impaired and appears to have been relatively healthy throughout his life. We will also present evidence from the film clips of Little Albert (Watson, 1923) that seriously calls into question Fridlund et al.'s claim that Little Albert was neurologically impaired (e.g., that the film shows no evidence of Albert using a pincer grasp or displaying social referencing). Unfortunately, Beck et al.'s relative certainty at having found Little Albert, and Fridlund et al.'s claim that he was neurologically impaired, have been widely propagated, especially on the web where they are typically presented as facts rather than possibilities. To what extent will these claims persist as yet another "Watson myth," despite the evidence that now calls them into question? Finally, we will present information gathered from William Albert's niece that provides a possible answer (if

William Albert was Little Albert) to the enduring question of whether Little Albert grew up to have a fear of furry animals.

#### References

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#### **Jose Maria Gondra (University of the Basque Country, Spain), “Rich Men Over 50 Kill Selves for Lack of Woman Attention!”: John B. Watson's Changing Views on Suicide**

The above quoted phrase was the headline of a 1932 newspaper article by Croswell Bowen introducing John B. Watson's assessment of five notorious suicide cases. Heading a page with a large display of photographs, it was followed by this subtitle: “So says Dr. John Watson, noted psychologist, who has made an intimate study of the reasons for suicide that actuated George Eastman, Ivar Krueger, Van Lear Black, James J. Riordan and Alfred Lowenstein; all of whom, he claims, couldn't go on living in the horror of complete loneliness” (Bowen, 1932).

After examining the lives of these businessmen and many other wealthy people who had committed suicide, Watson found that all were either bachelors or separated from their wives or widowers, and this was, in his opinion, the cause of their depression and death. Correcting what he had written in *Cosmopolitan* magazine, where he predicted that in fifty years men would no longer marry (Watson, 1929b), he now believed that “a man ought to get married when he reaches the age of 45 or 50. At that age he will find a wife to be a psychological necessity.” (Watson, 1932, p.7)

Returning to suicide, his thinking underwent significant changes during this time, when he worked for the advertising industry and became a successful publicist. Thus, in the 1924 New York lectures on behaviorism (Watson, 1925) he spoke of implanting negative responses in children to prevent later suicides; the idea of the lack of female attention is missing in the research on suicide and never appeared in the press. As he wrote about it: “I decided to give the magazine reading public a “good” article. I put time and study on this and called it WHY I DON'T COMMIT SUICIDE. Not a single magazine would touch it.” (Watson, 1950, p.2)

Watson barely dealt with suicide before leaving the university in 1920. But after the Great Depression of 1929, he made it the subject of the last investigation in his life. There is also evidence to suggest that in 1932 and early 1933 he was depressed and

possibly even suicidal (Cohen, 1979; Hannush, 1987). Moreover, his son William took his life a few years later and his daughter Mary had many suicide attempts (Hartley, 1990). All these facts, together with the refusal of *Cosmopolitan* to publish his article, are worth further consideration. Why this interest on the subject of suicide? Why was the article rejected? What was Watson really saying in it? Why so many changes in his thinking?

In order to provide a tentative answer to these questions, Watson's theory of emotions will be analyzed, especially in regard to the implantation of negative responses in children as a way to prevent suicide. Second, his unpublished article and other radio talks will be considered giving special attention to his view of suicide as a disease, the values that make human beings go on living, and the ways to help people contemplating suicide. Finally, his relentless critique of American society, especially with regard to the lack of challenging values, will be discussed and evaluated in its historical context.

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**Sergio Dias Cirino (Universidade Federal de Minas Gerais), Anna Christina P. M. Passarelli (Universidade Federal de Minas Gerais), and Rodrigo Lopes Miranda (Universidade Federal de Minas Gerais), *Circulating Behavior Analysis in the 1960s: An Analysis of Three Different Translations***

This paper aims to describe and analyze the circulation of Behavior Analysis outside the United States of America. Specifically, we focus on the analysis of three publications of the *Journal of Experimental Analysis of Behavior* (JEAB), in the early 1960s. The objects are three articles composed of lists of terms of Behavior Analysis in English, translated into German, French and Brazilian Portuguese. We understand that each of these publications participated in the circulation of Behavior Analysis in each of these countries. The French and German publications are from 1960 and the Brazilian, from 1963. The translated words were not the same in all three papers, and one can observe terms appearing only in one of the publications and not in the others. The articles were written by important figures in the history of Behavior Analysis of each country, namely: Halmuth H. Schaffer, Marc Richelle, Rodolpho Azzi, Maria Ignez Rocha e Silva, Carolina Martucelli Bori, Dora, and Fred S. Keller. The terms that were more common in the three translations are related to the definition of the object of study for the area, such as: "contingency", "reinforcement", "generalization" and "discrimination". This aspect suggests that the use of terms that already had a previously established meaning may have been the context for the standardization of the translation, within the Behavior Analysis. Additionally, one can suggest an initial effort to a more solid scientific work in these countries, with the definition of the basic concepts of the discipline. It is also possible to observe terms often related to laboratory practice, as "box", "schedules of reinforcement" and "chaining". From this, one can interpret that these practices represented much of the Behavior Analysis and they would be one of the main ways to disseminate the discipline around the world. These terms appear in the three articles translated and are detailed and categorized into different types and according to the contexts in which they can be used (i.e.: various types of reinforcement schedules, and response chaining). While anchored in their own specific contingencies, the translations represent the need for bibliographies and texts adapted to local contexts of circulation of Behavior Analysis. Possibly, they were characterized as teaching material, standardizing and disseminating topics related to the field. Moreover, they seem to have contributed to a more standardized use of behavior-analytic terms to be used in the teaching of psychology in different countries.

**3:00-5:00pm**

**SYMPOSIUM #1: Exploring the Materiality of Psychiatric and Psychological Instruments**

Chair: Alexandra Rutherford (York University)

Despite calls for historians of psychology to draw on material culture in their research that date to at least the 1970s (e.g., Sokal, Davis, & Merzbach, 1976), there remains a preferential reliance on archival sources within the field. Harvey (2009) indicates that history is severely limited without the study of material culture. This field is eclectic, with varying interdisciplinary approaches used to study a wide range of objects from architecture (Piddock, 2007) to the packaging of birth control pills (Gossel, 1999). According to Prown (1995), materials should be studied in order to “to understand the culture, to discover the beliefs—the values, ideas, attitudes, and assumptions—of a particular community or society at a given time” (p. 1). Objects can be studied simply for functionality; what does an object do? Alternatively, or in addition, they can be investigated for meaning and symbolism (Maquet, 1995). If objects afford such possibility, it is surprising that limited work has been produced in the study of psychological and psychiatric material culture. Existing studies in this realm include Galison’s (2004) study of the Rorschach test, Schmidgen’s (2005) analysis of the Hipp chronoscope, and Sturm and Ash’s (2005) consideration of the relationship between experimentation and psychological instruments. Our symposium seeks to contribute to this literature by considering: the development of restraint beds and their role in contemporary memories of the asylum era; the role of the leucotome in refining the traditional lobotomy narrative; and Psyche Cattell’s Infant Intelligence Scale as illustrative of the fine line between test and toy in children’s intelligence tests. By bringing discussion of these three seemingly unrelated objects together our goal is to explore some of the many possible topics that address materiality in the history of psychiatry and psychology.

#### References

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**Brianne M. Collins (University of Calgary) and Henderikus J. Stam (University of Calgary), *It's a Leucotome not a Lobotome: Reconsidering the Traditional Lobotomy Narrative Using a Material Culture Approach***

The history of psychosurgery is typically constructed in the literature using similar kinds of evidence. Traditionally historians have been most interested in what textual sources—published papers, correspondence, and the like—have to offer. As a consequence, there is a conventional lobotomy narrative that has emerged (e.g., Valenstein, 1986). This narrative is typically US-centric and focused on the work of Walter Freeman and his colleague, James Watts (see Collins & Stam, 2012). To date, however, virtually no research has been done on the surgical instrument(s) used to actually complete the controversial operations. Here we suggest that studying the leucotome—the surgical instrument(s) used to perform several types of psychosurgical procedures from the 1930s through 1960s—from a material culture approach offers new insights into the history of psychosurgery. The basis for studying material culture “is that human-made objects reflect, consciously or unconsciously, directly or indirectly, the beliefs of the individuals who commissioned, fabricated, purchased, or even used them and, by extension, the beliefs of the larger society to which these individuals belonged” (Prown, 1995, p. 1). As a consequence, a study of the leucotome can offer a more refined understanding of those individuals involved in the design and development of the instrument, but also those who ordered the instrument for their own use. Moreover, it offers a way to explore why the broader culture embraced such a procedure and allowed for the use of the instrument on patients, family members, and sometimes themselves. This approach also suggests that attention be given to the discourse of a particular time, as that discourse is often “gracefully shaped into artifacts” (Glassie, 1999, p. 44). In the case of the leucotome, the medical discourse in the mid-1930s contributed to the creation and modification of instruments used for the procedure; in turn, the instruments also shaped the discourse. In this paper we describe how the instruments associated with psychosurgery differed, and how those differences offer a broader story about the kinds of surgeries and techniques used in the medical community. By exploring the materiality of the leucotome in the history of psychosurgery, the conventional lobotomy narrative is revised and expanded.

#### References

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**Jennifer L. Bazar (York University), *The Utica Crib: Contextualizing the Role of Asylum Restraints in Contemporary Memory***

The October 1846 issue of the *American Journal of Insanity* featured a translated article originally published in the *Annales Medico Psychologique* the year prior. Titled “Aubanel’s Restraint Bed” the short write-up described the development of physician M. H. Aubanel in Marseilles: a covered bedstead used to help keep patients in bed. Intended for the excited and the epileptic it was to be used as an alternative to the restraint camisole or bed straps (Anonymous, 1846). It was adopted and eventually modified in the United States by Amariah Brigham, superintendent of the Utica State Hospital in New York state, and eventually became known as the ‘Utica Crib’. Use of the crib, it was argued, was beneficial because it afforded the individual a degree of freedom of movement not permitted in other restraining devices, as well as a recumbent position (see Redjinski, 1971). It nonetheless soon found itself a part of the ongoing debate among members of the Association of Medical Superintendents of American Institutions for the Insane (AMSAAI) surrounding the use of restraint devices. Brigham, for his part, had been a voice of opposition in the AMSAAI to the use of restraint devices with cases of insanity since their first meeting in 1844 (Barton, 1987; see Tomes, 1988). Despite resolutions adopted by the AMSAAI throughout the nineteenth century, which counseled against such devices, the Utica Crib went on to be adopted at many institutions for the insane across the United States and Canada. Today there remain a handful of these wooden cribs in museum collections across North America, but the debate behind their usage has long been forgotten. They have become a part of the collection of restraint devices – leather cuffs, muffs, straitjackets, and restraining chairs – which have become iconic in the public mind as (erroneously) characteristic of the asylum era (Labrum, 2011). This paper aims to contextualize the history of the Utica Crib and explore its new role in the formation of public memory during the period of institutionalization in North America from a material culture perspective.

#### References

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**Jacy L. Young (York University), *Test or Toy? The Cattell Infant Intelligence Scale as Liminal Object***

There exists an extensive literature on the history of intelligence testing, yet little has been written on the materiality of such tests. More broadly, the limited discussion of material objects that has taken place within the history of psychology scholarship has largely occurred with respect to the brass and glass laboratory instruments that characterized early experimental psychology (e.g., Schmidgen, 2005). In this paper I extend such discussion of psychological instruments to those often used outside the laboratory in more applied settings: intelligence tests. Using the Cattell Infant Intelligence Scale as a case study, this paper examines the materiality of children's intelligence tests in the first half of the twentieth century and draws particular attention to the parallels between such tests and children's toys. Psychologist Psyche Cattell, daughter of early American psychologist James McKeen Cattell, developed the Cattell Infant Intelligence Scale while working at Harvard University in the 1930s. First released in 1940, this intelligence test was a downward extension of the Stanford-Binet and was meant to be used to assess the intelligence of children between two and thirty months of age. The testing materials themselves came housed in a case, which given its contents might easily be mistaken for a toy chest. Among the dozens of items that make up these materials are numerous objects indistinguishable from everyday children's toys: red wooden blocks, a small cat figurine, a doll, a rattle, etc. As these objects suggest, as a psychological instrument the Cattell Infant Intelligence Scale, and others like it, straddle the boundary between test and toy. In inhabiting this ambiguous space between test and toy, and in trading on the innate appeal of play for children, the testing process may have been both helped and hindered.

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**6:00-7:45pm                      POSTER SESSION & RECEPTION**

**Alan S. Kornspan (University of Akron), *The Sport Psychology Laboratory at the University of Illinois: The Work of Alfred W. Hubbard, 1950-1970***

Recently the history of sport psychology has continued to be understood more fully (e.g, Green & Benjamin, 2009). Specifically, recent literature has provided an in-depth examination of the work of Coleman Griffith who initiated the first sport psychology laboratory in North America in 1925 at the University of Illinois (Green, 2009). Although significant contributions to the advancement of the field of sport psychology were made, the Athletic Research Laboratory at the University of Illinois was closed in 1932 (Green, 2009). After this laboratory closed, little is known about other laboratories that investigated the psychology of sport between 1932-1965. However, recently, Vealey (2006) noted that during the early 1960s brochures advertising the sport psychology laboratory at the University of Illinois were sent to potential students. Thus, a question of interest is after the subsequent closing of the Athletics Research Laboratory in 1932, when and how was the sport psychology laboratory at the University of Illinois reinstated? Therefore, the main purpose of this poster presentation is to explain how the sport psychology laboratory at the University of Illinois was re-instituted, provide a description of the activities of the sport psychology laboratory between 1950 and 1970, and to specifically present a timeline of sport psychology related events at the University of Illinois. First, background information will be provided about Alfred W. Hubbard. This will then be followed by an explanation of why the idea to reinstitute the sport psychology laboratory may have been developed. Next, a description of how the sport psychology laboratory was re-instituted and the roles of the University of Illinois provost, Coleman Griffith, the department chair Seward Staley, and Alfred Hubbard in restarting the laboratory are described. An overview of the activities of the sport psychology laboratory between 1950 and 1970 is provided. Finally, Alfred W. Hubbard's ten-year plan for the advancement of sport psychology at the University of Illinois is presented.

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**Alan S. Kornspan (University of Akron), *John Lawther: Contributions to the Psychology of Sport***

During the 1920s, Coleman R. Griffith the Director of the Athletic Research Laboratory at the University of Illinois began to publish journal articles and textbooks related to the psychology of athletics (Green, 2009). Although Griffith published one of the first textbooks related to the psychology of coaching (Griffith, 1926), few additional books were published on this topic during the 1930s and 1940s. Although scarce resources related to coaching psychology were published during this time period, this subject matter began to be viewed as an important component of the study of physical education. This was evidenced in a 1939 *Life* magazine article that emphasized that students in the physical education program at Springfield College completed a course on the psychology of coaching (Men of Muscle, 1939). As the psychology of coaching began to be taught at colleges and universities in the 1930s and 1940s, there became a need for an updated text on the psychology of coaching. Thus, John Lawther, who received a master's degree in educational psychology at Columbia University and was also the head men's basketball coach at Pennsylvania State University, published the *Psychology of Coaching* (Lawther, 1951). Contemporary sport psychology professionals have viewed Lawther's *Psychology of Coaching* as an influential development in the history of sport psychology. In fact, Weinberg and Gould (2011) considered the publication of this textbook as one of the five main highlights in the history of sport psychology in North America between 1939 to 1965. Although the publication of the *Psychology of Coaching* authored by John Lawther has been documented as a significant event in the discipline of sport psychology, little detail about John Lawther's work leading up to and subsequent to the publication of the *Psychology of Coaching* has been documented. Thus, the present poster presentation provides details of Lawther's significant contributions to the field of sport psychology. First, Lawther's initial work related to the psychology of coaching at Westminster College in the 1930s will be presented. This is followed by a description of Lawther's early years at Pennsylvania State University between 1936-1949. Next, discussion of Lawther's development of the *Psychology of Coaching* text and an analysis of how the book was received during the 1950s is presented. Finally, Lawther's additional contributions to the field of sport psychology during the 1960s and 1970s are described.

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**Miki Takasuna (Tokyo International University), *Important Literature Referenced in Textbooks on the History of Psychology: A Preliminary Citation Analysis Using 13 Textbooks Published after 2001***

This year marks the centennial of John B. Watson's declaration of behaviorism. Considering the various events that have occurred in the history of psychology since then, it is worth exploring early source materials to see which ones are still referenced in modern textbooks on the subject.

Textbooks I selected for their historical references include those published after 2001. They met the following conditions in that they 1) were written by only one or two authors; 2) comprised general psychology, not the history of specific psychology (such as experimental psychology); and 3) included only a most recent edition (as far as available) should more than one edition exist. In all, about 7,000 references were collected from 13 books, including 8 written in English (all North American publishers), 4 in German, and 1 in Norwegian. Each citation was collected from the (below) reference list and analyzed.

The most frequently cited literature was Wolfgang Köhler's *Intelligenzprüfungen an Menschenaffen* (original 1921) and B. F. Skinner's *The behavior of organisms* (1938). Both books were cited in 12 textbooks. However, between North American and German books, some cited references differed. For example, W. Preyer's *Die Seele des Kindes*

(1882) was cited in all 4 German textbooks but in no North American publications. Note that the most frequent citations are listed in more detail on the table here.

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#### **Kohei Yoshida (Tokyo Metropolitan University), *American Sociological Research Response to the Post Great War Crisis: An Historical Account of the Rise and Fall of the Methodological Asceticism***

The paper presents a study on the impact of war on social scientific thought. The study is founded on cases drawn from American sociological research performed during and after WWI, since they are informative about the interactions between a thinker's impression of a situation and the corresponding response in their practice. Though the ways in which the research practices of this discipline increased rapidly after the war are well-studied in the U.S., what occurred – intellectually and internally – has been merely attributed to an assumed process that an idealist view was substituted for a realist view.

The present study first shows how the process differed between generations, suggesting that people from 30 to 50 years old were the most reconstructive. Second, it is suggested that they did not necessarily adopt a realist methodology, but rather that some of them neither believed in an idealist nor a realist approach in the 1920s. Third, it is demonstrated that the war itself provoked not only the difficulty with the idealist view of progress, but that it also restricted the idea of scientific methods, resulting in a subsequent decade (the 1930s) that saw the defeat of practical conceptions of research.

To that end, the author avoids some inappropriate framework when interpreting the case studies. First, although the intellectual orientation of interwar empiricism was generally pursued in the name of objectivity, this does not explain why some practitioners

turned to versions of objectivism (Bannister 1987) while others turned to objective accounts of subjective aspects (Matthews 1977). Nor does it explain why after the Great War they changed their conceptions of research, increasingly devoting their research development to non-realist methods. Second, since the war provoked materialistic-realistic violence, it is often assumed that sociologists now substitute ideals for reliable facts; rather, they in fact found idealistic aspects in the war itself. Therefore, it is doubtful that when tackling the idealistic movement of the patriotism or nationalism in wartime, merely objective outcome of research was as effective as assumed today. Third, when objective research methods were required, it was not assumed that sociologists cut themselves off from society. On the contrary, in the 1920s their research was regarded as effective at least in terms of its ability to recreate the *social purpose* then needed, thereby explaining what has been termed, “methodological asceticism” (Mannheim 1932).

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### **Chin-hei Wong (University of Hong Kong), *Two Studies on the Psychology of Sex in 1920s China and their Implications***

When studying the history of psychology in the Republican China, there has been a long scholarly tradition suggesting that the early development was merely a passive “copy” of the contemporary knowledge emanating from the west (e.g. Petzold, 1987; Yang, 2006). It was not until recently that this myth was debunked and various attempts and areas of indigenization were identified (e.g. Blowers et al, 2009). But what about the transference and the reception of the western understanding of the psychology of sex, a highly sensitive topic that posed challenges to the highly conservative moral and social systems that have been laid down and taken for granted in China since the time of Confucius thousands of years ago? Were there any attempts to indigenize this knowledge and what implications can we draw?

Two studies on the psychology of sex in China from the mid-1920s will be discussed. The major ground-breaking publication of sex, *Sex Histories* (1926) written by Zhang Jingsheng, a professor from Beijing University (who ironically lost his job because of this book) and was more commonly known as “Dr Sex”, will be examined first to illustrate a radical attempt to transplant western psychological knowledge (in this case, Havelock Ellis’ work) to China without any modification. In more or less the same period, one can also find the first (though commonly neglected) systematic survey on sex carried out by Zhou Tiaoyang published in *Xinli Magazine* (the first psychology journal in China) in 1923. This is a psychological study that investigated some highly private issues of sex (for example, masturbation and homosexuality) that had long been regarded as taboos in Chinese society. The historical background, as well as the reception of these works, particularly among contemporary scholars (most of whom also received their training from the west) will be examined in the presentation. A careful analysis of the reception of the two studies will reveal the difficulty of directly transplanting western psychology into China without any attempt to adapt it to the socio-cultural context of the 1920s.

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#### **8:00-9:30pm MOVIE NIGHT**

**Lizette Royer Barton (Center for the History of Psychology), Cathy Faye (Center for the History of Psychology), and Jodi Kearns (Center for the History of Psychology), *Advice for Parents: Films From the Center for the History of Psychology***

From the baby biographies that emerged at the close of the 1800s to the best-selling works of Dr. Benjamin Spock in the 1940s, psychologists, psychiatrists, and physicians have always played a pivotal role in defining childhood. They have also served as advisors on childhood, providing parents and educators with advice and guidance on everything from temper tantrums to potty training. As early as the 1920s, child guidance clinics, research laboratories, philanthropic foundations, and professional organizations were devoting significant time, energy, and financial resources to charting and influencing the physical and mental growth of the child (Anderson, 1956; Schlossman, 1981; Smuts, 2006).

The rise of the child development movement of the 1920s coincided with increases in the use of motion pictures as tools for capturing research data, documenting laboratory practices, and educating general and specialist audiences. The use of these motion pictures as a method of reaching parents and educators has continued from the 1920s through to the present, providing a unique look at the ways in which childrearing, education, psychology, and American culture more generally have changed over the last century.

Much of the history of parenting advice in motion pictures is captured in the Center for the History of Psychology's Moving Image Collection. The Collection, containing more than 12,000 moving images from the history of psychology and related sciences, contains a wealth of materials related to childrearing from the 1930s to the present. Titles include: "The Baby's Bath," a 1946 film describing the importance of the bath for social contact and physical exercise; "Frustrating Fours and Fives," a 1953 film produced by the Canada Department of National Health and Welfare; and "The Vanishing Mommy," a 1977 film on working mothers. In addition, the CHP Moving Images Collection contains a significant collection of "Gesell Institute Reports" that feature Dr. Louise Bates Ames answering parents questions on a variety of topics, such as fear of the dark, the "facts of life," being honest, and the effect of television on child development. The collection also contains numerous recordings of talk shows and news programs where psychologists like Lee Salk and Lewis Lipsitt served as experts on topics such as Sudden Infant Death Syndrome, talking to children about war and disaster, changing trends in family life, and the development of family values.

In this presentation, we will show excerpts from some of these films, exploring the history of parenting advice in the human sciences as it was presented in motion pictures from the 1930s to the 1980s. Manuscript collections, artifacts, still images, and special interest materials focused on parental guidance will also be highlighted.

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**Friday, June 21, 2013**

**8:45-9:45am            PAPER SESSION #2: Conducting Psychological Research:  
Methods and Tools**

Chair: Cathey Faye (Center for the History of Psychology)

**Thomas Heinzen (William Paterson University), *The Horse That Will Not Go Away: Spontaneous Social Self-Deception***

Oskar Pfungst's (1911) book brought the final verdict of science and an apparent end to the Clever Hans affair. The Russian trotting stallion could not perform any of the mathematical feats that had led to his international fame. He was, however, good at responding to visual cues promising food. But Pfungst's experiments also revealed a human fondness for self-deception and this paper (a) describes the factors that initiated and maintained that particular self-deception; and (b) connects them to a stubbornly popular, bogus intervention for people with autism: facilitated communication (FC).

The self-deception about Clever Hans grew out of his owner's belief in equine intelligence. Mr. Wilhelm von Osten first taught his horse by bending forward, lifting the horse's right forefoot, and then looking up with a food reward after it had tapped out the correct answer. Four years later, the horse had become so sensitive to visual cues that merely narrowing and raising eyebrows could start and stop the horse's hoof. His owner did not have to be present because, after asking a question, people would lean forward to observe Clever Hans' hoof, and then lean backward in amazement when he had tapped out the correct response: Spontaneous social self-deception.

The self-deception about FC also grew out of a similar belief that even people with profound autism could demonstrate intelligence comparable to that in the general population. A trained facilitator holds some part of the arm of someone with autism, senses their intentions, and then guides their finger to a on an alphabet board. Significant skepticism about FC did not emerge until judges began accepting FC-inspired messages charging widespread sexual abuse by parents. The Clever Hans effect has once again taken the public imagination by storm.

Pfungst's (1911) research was exhaustive, well written, widely publicized, and respectful toward the characters in that drama. But that did not stop a succession of horses (e.g. Rosa, Mohammed, Beautiful Jim Key, Lady Wonder) from achieving international fame for their supposed intelligence or clairvoyance. Why didn't Clever Hans put an end to human self-deception about horses, much less FC? First, experiments in social cognition demonstrate that we humans automatically turn to readily available explanations first and turn to science only when sufficiently frustrated. Second, believing an obvious lie became a social norm. Third, public demonstrations and expert opinion appeared to scientifically validate each of these phenomenon. Finally, we don't tell the story of Clever Hans often enough. The last significant publication about Clever Hans was the summary of a 1981 conference edited by Thomas Sebeok and Robert Rosenthal.

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**David C. Devonis (Graceland University) and Deborah Rowe (Clarke Community Schools), *An Excursion in the Archeology of Recent Apparatus: The Ball and Spiral Test***

In the storage closet of the Psychology department of a small college, we found, about a dozen years ago, among the myriad of obsolete experimental apparatus, a decidedly odd duck. It was made of plywood, about two feet square, with 1"x 1" handles jutting out at the corners at 45-degree angles. But its primary distinguishing feature was a concentric ascending spiral about 2 inches wide which rose gradually, Tower-of-Babel style, from the plywood base to a height of about four inches, terminating in a flat circular disk about four inches in diameter with a half-inch deep round divot in the center. Numbers were painted on the spiral track in correspondingly ascending order at every half-revolution, starting at 1 and ending near the top with 7. For several years it languished as a curiosity. We had never seen or heard of anything like it: although it could not have been more than a few decades old, it was without history or pedigree, and served as an occasional example of an unsolved problem for history students. One day, five years ago, the first author was looking for something else in *Assessment of Men* (OSS Staff, 1948) and came across a photograph of a similar device in use. Suddenly all speculations and bad guesses as to its purpose fell away. It was a physical device for testing cooperative problem solving, one of several in the extensive battery of tests developed to identify likely OSS candidates. Groups of six, each person holding one of the handles, tried to roll the ball from the base to its nest on the top disk. Mystery solved! Now we could tell our students what it actually is, and since then it has been enjoyed in introductory and advanced classes alike.

But as it turns out, the device did not originate with the *Assessment of Men* group with its emphasis on personality, but instead came from a contemporaneous approach to social psychology very different both in its theory and in its stance toward war and peace. The proximal site of origin of the ball-and-spiral apparatus (for so it is properly called) in the history of psychology is in the dissertation work of a young Lewinian at Harvard in the late 1930's, John Robert Putnam French Jr. (most frequently recalled today as the 'French' in French & Raven's Forms of Power Theory.) The device played an incidental role in French's 1940 dissertation study of the topography of differently cohesive groups in situations of fear and panic (French, 1940; French, 1941; French, 1944). In this presentation we trace the dissemination of the ball-and-spiral apparatus, its brief heyday in the 1950's and 1960's, and its subsequent occasional reappearances through the 1980's.

We suggest that analysis of the trajectory of dissemination of the ball-and-spiral in psychology partially supports Danziger's (2000) historical thesis that social psychology's Lewinian influences were sapped by subsequent researchers' focus on highly controllable laboratory procedures as well as by the development of an atomized and anomic conception of the individual vis-a-vis society. Against the developing texture of a social psychology focused on intra-individual competitiveness, the inherent theme of social cooperativeness embodied in the ball-and-spiral apparatus vanished along with the device, an example of how rapidly good ideas can go out of fashion. But, due to its untraceable distal origins and its equally untraceable end, and to other anomalies that this episode presents for the decay-of-Lewinian-influence thesis, the career of the ball-and-spiral also suggests something more like archaeology than history—the story of the persistence of an artifact outlasting impermanent, temporary social and scientific cultures.

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#### **10:00-12pm                    PAPER SESSION #3: Applied Psychology**

Chair: Kenneth D. Feigenbaum (University of Maryland University College)

#### **Michael Phillips (Collin College), *Doctors, Planters, and Doomsday Preachers: The Surprising Failure of the Eugenics Movement in Early 20th Century Texas***

During the first three decades of the 20<sup>th</sup> century, eugenics — a misapplication of biological science to public policy concocted before the mechanics of human heredity were well understood — deeply shaped government policy. This pseudo-science provided an excuse to impose harsh immigration restrictions, strip civil rights from African Americans, Latinos, and poor whites, and led to state laws across the country allowing involuntary sterilization of the supposedly genetically unfit. Eugenicists panicked the nation about the dangers of rapid, widespread immigration from Eastern and Southern Europe, Mexico and Asia to the United States, claiming a horde of racial inferiors threatened the nation's biological and political future.

During the time between the turn of the century and World War II, twenty-seven states passed mandatory sterilization laws aimed at preventing the poor, epileptics, and

the supposedly unintelligent and mentally ill from passing their dysgenic natures to another generation. Virginia in the 1930s, for instance, targeted poor whites with eugenics legislation, in one case rounding up children deemed “feebleminded” in the Brush Mountain region and shipping them to institutions like Western State Hospital, where doctors subjected them to involuntary vasectomies and tubal ligations. Yet, in spite of the strong influence of eugenicists in Klan-dominated Texas, the Lone Star State remained a peculiar exception.

Texas did not rank among the states that passed sterilization laws or other eugenics measures. Two major factors – the economics of rural Texas which depended on the labor of supposedly unintelligent Mexican Americans and Mexican immigrants, and the emerging dominance of apocalyptic religious beliefs among evangelical Protestants that discouraged political activism, most likely made Texas different during the eugenics era.

This paper will explore why, despite the efforts of pioneering eugenicists in the state such as 19<sup>th</sup> century physician Gideon Lincecum, the lobbying of such leading national eugenicists in the Lone Star State such as professor A. Caswell Ellis at the University of Texas at Austin and Hermann Joseph Muller at what was then called the Rice Institute, and the support of the politically powerful Ku Klux Klan in the 1920s, the Texas legislature never implemented eugenics legislation.

This presentation will draw from diverse sources, mostly archived at the Center for American History in Austin, such as the papers of Lincecum, Ellis and Muller, the Texas Mental Health Hygiene Survey of 1924, the publications of the state’s Ku Klux Klan such as the *Texas One Hundred Percent American* and the writings of one of the nation’s leading apocalyptic theologians, the Rev. Cyrus Scofield of Dallas. The paper will cover the period roughly from the 1880s to the 1930s and will provide a unique mirror into what happens when greed, bad science, and religious fundamentalism collided in one of the United States’ most important racial borderlands.

**Ann Johnson (University of St Thomas) and Elizabeth Johnston (Sarah Lawrence College), “Up the Years with the Bettersons”: Gender and Parent Education in Pre-WWII America**

In the 1920s and 1930s hundreds of psychologists participated in a national parent education movement influenced by progressive era thinking to improve the quality of child-rearing in America. Rockefeller funding was harnessed to a growing perception that scientific experts needed to step in and educate American mothers who were, in the words George Hecht, founder of *Parents Magazine*, making “sort of a mess out of bringing up their own children” (Schlossman, 1985, p. 71). The parent education movement opened doors for many second-generation women in psychology by providing training and jobs (see Cahan, 1991). Women psychologists of the parent education movement spread the emerging “gospel of child development” (Schlossman, 1981) to other women – mothers – in a variety of formats. Like their better-known contemporary, John Watson, they published books and magazine articles aimed at remediating poor mothering in the 1920s and 1930s. While Watson promoted a traditional role for mothers focusing on role adjustment (Harris, 1984), there is evidence that women psychologists

introduced ways of thinking about family life and the mothering role that challenged tradition, encouraging role expansion through enhanced social agency and self-realization.

An example is provided by Minnesota women psychologists who produced radio programs on childrearing. The University of Minnesota housed one of several Institutes of Child Welfare founded in the 1920s with Rockefeller funding. It was the academic home of Florence Goodenough, prominent child development researcher and mentor to a large number of second-generation women psychologists. As a Land Grant institution, the University of Minnesota was charged with disseminating knowledge broadly to all state residents, and did so partly through parent education radio programs. When they were inaugurated in 1927 the format was strictly didactic. Women with recent doctorates who worked as institute instructors and parent educators delivered the latest ideas about child development in lecture format and based on the latest research. In 1932 a new format was introduced: advice about child rearing embedded within stories featuring a fictional family; the series was called “Up the Years with the Bettersons.” Most episodes focused on Mrs. Betterson and how she resolved home life difficulties through application of psychological ideas. The Betterson format provides an opportunity to identify implicit (and sometimes explicit) values and norms in the programs regarding mothering and gender roles. Although the early twentieth century ‘separate spheres’ doctrine that organized a gendered division of home and work for middle class families was alive and well in these programs there are indications that gender roles were shifting in more egalitarian directions with an awareness of new identity options emphasizing individualism and self-realization for women. Some of the programs addressed the emerging tension for women between individualistic goals such as careers and the living-through-others prescriptions for women from the separate spheres era.

Though not motivated by feminist activism, women psychologists of the parent education era can be seen as significant shapers of the new therapeutic ethos in psychology that, as Herman (1995) points out, paved the way for more radical transformations in the options for self-definition offered to women during the second wave of feminist activism in the 1960s and 1970s.

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**Cathy Faye (Center for the History of Psychology), *The Foreign Morale Analysis Division and the Study of Japanese Morale, 1943 – 1946***

In the spring of 1944, the United States Office of War Information (OWI) created the Foreign Morale Analysis Division (FMAD), a governmental unit charged with researching and measuring morale among Japanese soldiers and civilians (FMAD, n.d.). The unit had a staff of thirty sociologists, anthropologists, psychiatrists, and others, headed by psychiatrist and anthropologist, Alexander Leighton. The team's work consisted of analyzing reports from interrogations with prisoners of war (POWs) and analyzing captured diaries, letters, military documents, newspapers, and broadcasts. In the course of their work, they translated 314 diaries and coded more than 2500 interrogations reports (Herman, 1996; Leighton, 1949).

The work of the FMAD team was by no means straightforward or simple. Leighton and his team struggled with the messy, incomplete, and sometimes dubious nature of the data they received from other government branches. Prisoner interrogations were focused on strategy and military resources; questions about morale were scant. Policy makers questioned the sincerity of prisoners' responses; would POWs provide true responses and could the responses of captured prisoners be seen as an adequate representation of all Japanese soldiers? Furthermore, morale data collection was not uniform or controlled: some prisoners were questioned immediately after capture, while others were questioned much later. Because of these difficulties, data was difficult to categorize and quantify; it came to the team in fragments. Adhering to the usual required rigors of experimental or controlled research was nearly impossible for the FMAD. The staff aptly described the Division as "a small group, working in a field where it had to explore its way and learn as it went and where data employed were exceedingly crude" (FMAD, n.d.).

This paper examines how the FMAD struggled with and navigated this difficult situation and sheds light on how the ideals of laboratory science shifted, changed, and were negotiated in the chaotic and uncontrollable environment of wartime science. For the FMAD, this meant quantifying and tabulating as much data as they could, but it also meant expanding their view of the scientific process and emulating sciences that were more concerned with prediction in the absence of controlled data collection (FMAD, 1945). In this vein, Leighton and his team adopted a "clinical" rather than a "statistical" approach. Leighton likened this approach as more akin to that of a medical doctor studying the spread of disease or a meteorologist plotting weather trends (FMAD, 1944). Though he constantly urged policymakers to help institute more standardized procedures for gathering morale data, he employed alternate methods and relied on these kinds of metaphors to describe and substantiate the work of the Foreign Morale Analysis Division.

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**1:00-3pm**

**SYMPOSIUM #2: Issues In The Historiography of the Behavioural and Social Sciences**

Chair: Katalin Dzinás (Independent Scholar)

Historians of the behavioral and social sciences have not displayed a great deal of interest in historiography. One only needs to look at the past programs of Cheiron or ESHHS or the articles in the *Journal of the History of the Behavioral Sciences* or *History of the Human Sciences* in order to see this. They all contain work on this topic but it is relatively rare. Some might argue that it is their job to write history, not to talk about it. The classic retort is to be found in that famous statement of Karl Jaspers: “There is no escape from philosophy. ... Anyone who rejects philosophy is himself unconsciously practising a philosophy” (1954; p. 12). It could similarly be argued that anyone who writes history is practising a philosophy of history, whether it is consciously recognized or not.

The topics in this symposium are diverse. The first paper is a re-examination of the prohibition on writing “Whig history” that was originally formulated by Herbert Butterfield in the 1930s. The second paper compares behaviorism and cybernetics and argues that there is more continuity between them than is usually acknowledged. The third paper critically examines the impact of gender analysis on the history of psychology. The fourth and final paper argues for the importance of narrative frameworks in the history of psychology.

These papers cover only a small sample of the historiographical issues that could have been discussed. It would be an unexpected bonus if this symposium were to lead to historiography having a more prominent place on the programs of Cheiron in years to come.

Reference

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**Adrian C. Brock University College, Dublin), *Revisiting the Whig Taboo***

Early in my training as a historian of science, I came across the terms, “Whig history” and “whiggism”. They were often accompanied by terms like “celebratory history”, “presentism”, “progressivism”, “justificationism” and “positivism”. I was left in no doubt that these were things to be avoided. What was the alternative? Some writers advocated a “critical history” while others wrote of a “historicism” which involved understanding the past “on its own terms” (e.g. Stocking, 1965). The latter was often accompanied by a relativism that involved suspending belief in the truth or falsity of historical actors’ views (e.g. Bloor, 1976). I initially accepted these views but as time went on I began to have doubts. There are surely aspects of the history of the behavioral and social sciences that we might want to criticize. Scientific racism and the traditional views of women that they endorsed are obvious examples. The view that homosexuality is a mental illness is another. Could there be anything that we would not want to criticize? Negatives imply positives. If we think that treating homosexuality as a mental illness was wrong, we must surely consider its removal from the DSM in 1974 to be a step in the right direction. I do and I am not ashamed to admit it. I am also happy to acknowledge that I hold this view because I was born in a certain place at a certain time. Had I been born in another part of the world or in a different historical era, I might hold different views. This merely shows that, like all human beings, I am a product of my time and place. I would be suspicious of anyone who claimed to be anything else.

These considerations led me to conduct a search of the literature on this subject. I was surprised to find that it is extensive and, for the most part, critical (e.g. Hull, 1979; Hall, 1983; Harrison, 1987; Wilson & Ashplant, 1988; Mayr, 1990; Biagioli, 1996; Jardine, 2003). My initial disappointment at realizing that my doubts were not original was tempered by the knowledge that they are shared by others. Biagioli (1996), for example, has defended what he calls, “neo-Whig history of science”, though he defends it on the grounds that it is “not so whiggish after all” (p. 201). It differs from traditional Whig history in that it does not necessarily endorse the status quo. At the same time, it acknowledges that the present is the only frame of reference that we have. One point that is often overlooked is that social critics are as much a product of the society that they criticize as those who defend the status quo.

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**Rebecca Lemov Harvard University), *Teleology and Historiography from Behaviorism to Cybernetics***

Historical treatments of behaviorism and cybernetics have not tended to stress links between the two. Aside from more substantive reasons for this mutual disregard, there is a divergence of historiographical fortune: pre-World War II behaviorism is about as out of fashion a research topic as can be imagined, whereas post-World War II cybernetics has been enjoying a surge of interest among historians of science for over a decade. A *New York Times* writer's recent dismissal of cybernetics as an obscure redoubt of twentieth century science -- "Odds are," the reviewer submitted, "you are only dimly aware of cybernetics, if at all" because it was a "science [that] simply failed in the court of ideas" -- is not one that historians of science appear to share. To the contrary: "There is something philosophically or theoretically pregnant about cybernetics," according to Pickering, which explains its popularity as a research site. Yet there is more to the intra-field historiographical silence than the pregnancy of one versus the barrenness of the other. Nor are they opposite.

Because behaviorism appears in most accounts largely as a between-the-wars phenomenon, historians tend to figure its postwar existence as a series of aftereffects. Such postscripts to classic behaviorism are said to include the burgeoning of powerful "behavioral sciences" and their foundation-supported interdisciplinary workings; the influential postwar figure of B. F. Skinner, who nearly made "operant conditioning" a household word; and beginning in the 1940s and 1950s the proliferating sheer usefulness of behaviorist technique as a mid-level technocratic "applied" sinecure in pastoral counseling systems, managerial technologies, and self-help behavior-modification programs. When historians do paint behaviorism as an active postwar force in basic research, it is generally in contrast with other developments such as phenomenology and the person-centered human potential movement, as well as the "cognitive revolution" in the brain sciences.<sup>1</sup> The picture is of a once powerful but suddenly diminished method for running rats through mazes and extracting a vast, hubristic "theory of everything"--in short, an intellectual paradigm and research program that ran aground with the war, save for the popular success of Skinner and the many ways it disseminated commercially and therapeutically.

Yet this paper argues -- by exploring behaviorism's end as well as cybernetics' somewhat murky beginnings -- that there is much continuity to be found between the two

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<sup>1</sup> Cf the best-selling *Beyond Freedom and Dignity* (New York: Knopf, 1971); Skinner appears as no. 1 in a recent website announcing the "100 Most Eminent Psychologists of the Twentieth Century." <http://edtech.tph.wku.edu/shaggblo/sl.htm>; from Steven J. Haggblom et al, "The 100 Most Eminent Psychologists of the Twentieth Century," *Journal of General Psychology*, Vol. 6, No. 2 (2002), 139--152.

movements, continuities largely neglected among historians. A common thread of interest in teleological engineering forms a link.

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#### **Alexandra Rutherford (York University), *Gender Analysis and the History of Psychology: Developments and Future Prospects***

In this presentation I revisit Joan Scott's classic 1986 article *Gender: A useful category of historical analysis* as a starting point from which to critically examine the impact of gender analysis on the history of psychology. I briefly review what is meant by

gender analysis, outline its impact on the history of science more generally, then review some recent work that points to an increasing engagement with gender analysis in our own field. I conclude by offering some thoughts on the future prospects and potentials of gender analysis for conceptualizing and writing P/psychology's history.

Since the 1960s, feminist historians have produced a rich historiography on women and gender in science. This historiography has both restored women in science's history and examined the structural factors that have affected their participation and prestige (e.g., Ainley, 1990; Jack, 2009; Rossiter, 1982, 1995, 2012; Scarborough & Furumoto, 1987; Schiebinger, 1989; Warren, 1999). Further, feminist scholars have critiqued the androcentric assumptions of much of Western science and epistemology (e.g., Code, 1991; Fox-Keller, 1985; Haraway, 1989; Harding, 1986; Merchant, 1980) and demonstrated that scientific practices and theories are imbued with gendered and racialized stereotypes that have served to maintain and enforce relations of power (e.g., Bederman, 1995; Haraway, 1989; Hrdy, 1981; Martin, 1991; Schiebinger, 2004).

Scott reminded us that while the exclusionary *outcomes* of the gendering and racializing of science had begun to be well-documented, we must ask more often *how this happened* in order to understand why it happened. She also noted that most studies of gender and science had focused on *women's* experiences of and in science. She reminded us that to produce a fully gendered understanding of history (and science) it was insufficient to focus on the experiences of women in isolation from the experiences of men.

To what extent have historians of psychology engaged with the project of gender analysis? To what extent have we taken up the call to examine how gender has functioned in Psychology to maintain or enforce relations of power; to unpack how psychological concepts, theories, and methods become imbued with and express gender ideologies; or to analyze how gender itself, as a psychological category, has been constructed and deployed by psychological scientists? Although we now have a substantial and sophisticated historiography on women in psychology (e.g., Scarborough & Furumoto, 1987; Johnson & Johnston, 2010; Johnston & Johnson, 2008), to what extent have we engaged with gendering as a process that occurs relationally, involving the construction of both masculinities and femininities? I examine these questions by reviewing some recent work that engages with gender analysis in productive ways (e.g., Hegarty, 2007, 2013; Nicholson, 2011; Pettit, 2012; Shields, 2007).

Finally, I suggest that gender analysis offers some particularly rich historiographic potential for psychology. Psychology is a science that is not only gendered on multiple levels but also produces scientific knowledge about gender itself. It is a powerful *contributor to* (as much as it draws upon) the 'beliefs about gender' that affect everyday experience. As Kohlstedt and Longino (1997) have argued, "To study gendering in science is to study the masculinization and feminization of ideas, practices, and institutions *as well as the ways the sciences have themselves construed gender as a topic of empirical investigation*" (p. 6, emphasis added). The deeply reflexive nature of psychology has been discussed extensively by historians (see Morawski, 1992, 2005; Richards, 1996; Smith, 2005, 2007). Examining how the gendering of Psychology has influenced Psychology's own knowledge-generation about gender can help us begin to disentangle this reflexivity and examine science's relationship to gender - and gender's relationship to science - in new ways.

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**Henderikus J. Stam University of Calgary), *Are There Still Historical Explanations? On the Question of a Historicized Psychology***

As a point of departure for this paper I will begin with John Dupré's startling claim that "ultimately, human evolution and human history are the same thing" (2001, p. 99). Dupré argues that cultural evolution is the evolution of cultural factors that have the capacity to elicit certain kinds of behavior from creatures with human brains, just the sort of creatures incidentally that are disposed to behave in accordance with rules. On Dupré's argument, not only are history and biology thoroughly entwined, but as a consequence, so are history and psychology. For surely if our biological inheritance and historical rootedness are inseparable then so must our psychological nature be likewise historicized. But now we are on difficult ground for just how or of what our psychological capacities are constituted is not only a deeply contested matter but one fraught with preconceptions. History and psychology come together however in our linguistic and lingual capacities, that is, in just those capacities to not only speak but to act in a way that presupposes language, for example, the capacity to play cards or to build airplanes. In this manner speech, language, discourse and history converge leaving aside for the moment the question of whether narrative is also a form of 'explanation,' an issue that has exercised philosophers of history for some time. Instead, I want to note that narrative structures are relevant not only for history but mimic our ordinary, everyday explanations (as David Carr has argued). Questions of plot, storyline, and actors are features of literature, plays, movies but also the daily flotsam and jetsam of giving accounts of ourselves and others, and making sense of the trajectories of a life. And of

course there is a tradition of narrative psychology in the work of such people as Ted Sarbin, Jerome Bruner, Michelle Crossley, Dan MacAdam, Michael Bamberg and others. It even qualifies as a kind of method and is included in various qualitative methods books. To acknowledge the lingual nature of our action and the narrative structure of our accounts is to connect our present concerns directly back to Collingwood and his concerns that human action is always already historical. Narratives have certain features inextricably bound up with “diachronicity, “particularity” and “intentional state entailment,” among other features, as Bruner had it (1991). That is, narratives structure around time, but may subvert that structure too, they reference particular happenings, and they tell us about people’s reasons for acting even as events may frustrate their reasons. Stories then tell us about departures from a canonical script, which themselves may be highly conventional as in tragedies, comedies and romantic narratives. But narratives cannot function as explanations, either in history or psychology. Whether history needs ‘explanations’ of a conventional kind is a debate that still resonates in contemporary historiography. Psychology on the other hand has rejected by and large such non-explanatory frameworks. I shall argue both history and psychology are implicitly indebted to narrative frameworks without which they could not get off the ground.

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#### **3:15-5:15pm            PAPER SESSION #4: Research Schools and Programs in the Social Sciences**

Chair: Christopher D. Green (York University)

#### **Kenneth D. Feigenbaum (University of Maryland University College), *Maslow's Graduate School***

Abraham Maslow was a gifted undergraduate teacher both at Brooklyn College and in his early years at Brandeis. With the creation of the Graduate School at Brandeis in 1953 his interest in teaching undergraduates began to wane. By 1962 it was nonexistent. The Graduate School became the vehicle for an unabashedly “Social Darwinist” model of education.

Although Maslow established personal bonds with several of his graduate students his negative views of them based upon his philosophy of graduate education become “legion.” According to Hoffman (1988, p.220 ) “his relationship (with his graduate students) was perhaps his only real failure...he attributed their poor performance to characterological shortcomings. They were deemed as unmotivated, over-dependent, and not sufficiently deferring to him...by the mid 1950’s he had decided that

empirical research would draw valuable time and energy away from his quest to transform the very nature of the psychological enterprise.” The comparative psychologist that Maslow was in the 1930’s had morphed into a “continental philosopher” in the last years of his life. As St.Paul in the Gospels did, his life was then devoted to spreading his “good news”.

This paper will try to give credence to the above by presenting:

- a) evidence from his diaries
- b) presenting excerpts from his unpublished Graduate Memo
- c) explorations of my personal memories as a faculty member of the Brandeis Department during the years 1962 to 1965
- d) and perhaps through personal narratives from some of his former undergraduate and graduate students

The key to Maslow’s philosophy of education can be most clearly found in his “Deuteronomist” memo submitted to the Graduate Council in 1957. Here, he outlines his views on what graduate education in psychology should be. His perception of inclusion into the program he created at Brandeis was that it was only for the “gifted” self motivated student whose principal of instruction would be mentoring by a corps of “intellectual generals” and in a “padrone” relationship with the faculty. This paper will explore Maslow’s memo in detail.

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**Jason Hughes (Brunel University) and John D. Goodwin (University of Leicester),  
*Norbert Elias, Ilya Neustadt and the “Leicester School” of Sociology***

Norbert Elias (1897–1990) and Ilya Neustadt (1915–1993) are widely credited with establishing one of the United Kingdom’s pre-eminent sociology departments of the 1960s and being responsible for the training of a generation of the UK’s most prominent sociologists – from Giddens and Golthorpe to Wilson and Zubaida. In a recent figurational analysis of epistolary forms in the correspondence between Elias and Neustadt we have argued that it was the emergent dynamic of the interdependent relationship between Neustadt and Elias that undergirded the development of sociology at the University of Leicester, and that it was this Elias/Neustadt relationship that gave Leicester sociology its distinctive character and prevailing intellectual climate during the 1960s. We have made the case that Leicester in this period became an important sociological training ground, and that Elias dominated the intellectual agenda (whether you were for his ideas or against them) simply by positing a continental model of sociology engaged with long-term processes of development (see Goodwin and Hughes

2011). However, our work to date has been based on analysis of Elias and Neustadt's own assessments of their significance to sociology at Leicester as expressed in their personal correspondence. It is likely, therefore, that such accounts will be contested by 'significant others' who lived through that period and whom were active members of the sociology department. These other perspectives must be considered if we are to understand fully the significance of the Leicester sociology department for British sociology. Using interview data from more than 30 past members of the sociology department, the central aim of this paper is to explore the competing voices on Leicester sociology during this period. What emerges from our analysis is a more complex picture than that which we elucidated from a consideration of the correspondence on its own. For example, we find some who claim that Neustadt was the key intellectual figure, not Elias (though this is very much a minority view). We find that sociological training was disputed, contested, and often a source of division in the department, and that some in the department were not drawn to the intellectual promise offered by Elias's work. Beyond the specifics of the Leicester school of sociology, the paper also provides insight into the difficulties attendant upon combining and reconciling archival data with interview data, narrative reconstructions with documents of life, and the more general methodological issues related to utilizing fragments of data gleaned from informal sources to forge a coherent and accurate historical record. (411 words)

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#### **Gerald Sullivan (Collin College), *Marcel Mauss, Embodiment & Exchange: Another Psychological Anthropology?***

Claude Lévi-Strauss ([1950] 1987:4-5) begins his discussion of the work of his eminent predecessor, Marcel Mauss, with a favorable comparison of Mauss' essay *Les Techniques du Corps* of 1934 (see Mauss [1950]1979a:95-123) to the contemporaneous work of Ruth Benedict (1934) and Margaret Mead (1935). Lévi-Strauss contends that Mauss anticipates the central argument of *Patterns of Culture*. Further, he claims Mauss was developing a theory of embodiment and embodied experience not unlike that which

Mead was coming towards during and after her New Guinea fieldwork of 1932 and 1933 which lead to her squares hypothesis (see Sullivan 2004, 2009).

Benedict, Mead and Mead's third husband, Gregory Bateson, are often grouped together under the rubric of culture and personality or more occasionally the configurationist school. Mauss, by contrast, was the heir to Emile Durkheim and one of the important forerunners of a structural anthropology associated with Lévi-Strauss. Any possible connection between these two important strands of anthropological endeavor goes largely unmentioned upon.

Mauss' 1934 essay was not the first time he made reference to psychological matters. As early as 1924, Mauss spoke before the *Société de Psychologie* on the relations between sociology and psychology (see Mauss [1950] 1979a:2-33). For Mauss sociology was fairly self-evidently a part of anthropology. Sociology, anthropology and psychology were all part of a larger discipline of biology with the important proviso that human society both shared an evolutionary past with other animal societies but differed from those animal societies because of the role sentience plays in all humans and their lives together. Further, in the 1924 essay, almost a decade before Edward Sapir ([1933] 1949:46-60) made claims for the psychological reality of phonemes for speakers of languages, Mauss claimed that collective representations were psychological realities.

Perhaps Mauss' most important, and certainly his best known, work, *The Gift* turns upon the psychological reality of three sorts of obligation inherent in patterns of exchange: (1) the obligation to give, (2) the obligation to receive, and (3) the obligation to give in return. As presented by Tamati Ranaipiri, a Maori elder, to Elston Best, these obligations are not mere human constructions but part of the order of nature itself. Echoes of this approach can also be found in Mauss' never finished doctoral dissertation: *On Prayer* (Mauss [1909] 2003).

This presentation explores this largely underappreciated aspect of Mauss' work.

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**Saturday, June 22, 2013**

**8:45-9:45am PAPER SESSION #5: Building Disciplinary Histories**

Chair: Barbara Lusk (Collin College)

**Michael R. Hill (Editor, *Sociological Origins*), *Writing and Editing Departmental Histories in Sociology***

This paper encourages the wider writing of departmental histories in sociology and cognate disciplines. Departmental histories, much like the intellectual biographies of esteemed disciplinary leaders, tend to be written about those organizations thought to be in some sense particularly influential or institutionally significant. The numerous monographs relating to the venerable sociology department in The University of Chicago comprise a powerful example of this phenomenon, one which collectively presents a highly-skewed record of professional sociology as a whole. As a remedy, this paper presents practical steps (including the use of lists, organizational charts, networks, maps, photographs, sociological life history documents, and timelines) for devising histories of departments in schools that are, at present, without formal written accounts. Would-be chroniclers of lesser-known departments are also alerted to potential political and ideological pitfalls to avoid, including the ever-present inclination to put everything “in a good light.”

**Christopher D. Green (York University), Ingo Feinerer (Vienna University of Technology) and Jeremy T. Burman (York University), *Networking Psychological Review, 1894-1898***

The great amount of primary source information that is available about early American psychology forces us to employ some sort of "reduction" strategy so that we can make out the broad outlines of the discipline at that time and try to understand what its overall structure was. Conventionally, we have done this by using categories that were invented by the historical actors themselves: the "schools" of psychology, e.g., Structuralism, Functionalism, and later Behaviorism. Although this approach has enabled us to come to some consensus about what the main intellectual groupings were and where the primary regions of contention lay, it fails to retain in the discussion a number of significant individual psychologists. Even some major areas of research (e.g., emotion, color vision) are not terribly well represented by the "schools" approach. R. S. Woodworth once opined that a majority of psychologists did not identify with any of the available "schools" in his time. In an attempt to find a more inclusive solution to the problem, we took every substantive article published in the influential journal, *Psychological Review*, during the first 30 years of its existence and subjected them to a social network analysis, in order to see (1) what "communities" of articles emerged, (2) how densely interconnected the articles were within each of the resulting communities, and (3) how the various communities were related to each other. We will present the results for the first five years (1894-98) here. We discovered a number of well-defined communities that are typically excluded by the "schools" approach, as well as a complex community that represented much of the early Functionalism.

**10:00-12pm                      PAPER SESSION #6: The Contributions of Settlements and Women in the Developing Social Sciences**

Chair: Larry Stern (Collin College)

**Joyce E. Williams (Middle Tennessee University) and Vicky M. MacLean (Middle Tennessee University), *Settlement Sociology in the Progressive Era: Early Contributions to a Public Sociology of Faith, Science, and Reform***

In this work we present findings from a collective case study analysis of major US social settlements operating at their peaks during the progressive era (1890s-1920). Contributions to sociological practice are assessed in the areas of scholarly research, community reform activism, and teaching. The progressive era represents an important historical period wherein public sociology was practiced outside of the university, even while it served to shape the early discipline both through collaboration and political contest. Settlement sociology has only recently been restored to the historical canon as an alternative and diverse interpretation of the founding of a public sociology that is experiencing a renaissance today. Using original archival resources from Boston, Chicago, and New York collections we identify some of the common and unique contributions to sociology across settlements and discuss some of the lessons we can learn from the past for practicing a contemporary public sociology for social change.

Edith and Grace Abbott Papers, Joseph Regenstein Library, University of Chicago  
 Florence Kelley Collection, 1894-1981, University of Illinois at Chicago Special  
 Collections, Richard Daley Library  
 Graham Taylor Papers, Newberry Library Manuscript Collections, Chicago, IL  
 Greenwich House Papers, Tamiment Library. Robert F. Wagner Labor Archives. New  
 York University  
 Hull House Collection, 1889- University of Illinois at Chicago Special Collections,  
 Richard Daley Library  
 Lea Demarest Taylor Papers, University of Illinois at Chicago, Special Collections.  
 Lillian D. Wald Papers, Rare Books and Manuscripts Division. The New York Public  
 Library, Astor, Lenox, and Tilden Foundations  
 South End House Records, Houghton Library, Harvard College Library  
 Social Settlements Collection. Sophie Smith Collection, Smith College Archives

**Shayna Fox Lee (York University), Michael Pettit (York University) and Alexandra Rutherford (York University), *Networking Science and Reform: The Prosopography of the First Generation of Chicago-Trained Female Social Scientists***

The history of the social sciences during the Progressive era is well-traversed terrain (see Furner, 1975; Haskell, 1977; Ross, 1991) and has included major studies of the relationships among many of the well-known male social scientists of this period (Bulmer, 1984; Deegan, 1988, on Jane Addams' connection to the men of the Chicago School (O'Donnell, 1985; Rodgers 1998; Ross, 1991). Systematic historical studies of the *networks of female* social scientists working both inside and outside the academy and across the newly distinct disciplines of psychology, sociology, anthropology, and political science in this period have been relatively fewer (but see Rosenberg, 1982, on a selection of these women; and for disciplinary-based studies see Deegan, 1978, 1991, on women in sociology; Scarborough & Furumoto, 1987, on women in psychology; Sklar, 1985 on women social reformers at Hull House). In part, this may be due to the relative historiographic complexity of tracing the career trajectories, activities, and relationships of women in the period during which academic social science was becoming marked as "masculine cultural terrain" (Silverberg, 1998, p. 9).

The masculinization of academic social science proceeded via 1) the literal exclusion of women from full-fledged faculty positions and funding at the major research universities - even those as welcoming to female graduate students as the University of Chicago, and 2) the ways in which gender itself was encoded in the "conceptual apparatus and professional ethos of the new disciplines" (Silverberg, 1998, p. 9). In the first case, as the social sciences embarked on their professionalization projects, female PhDs found it difficult to attain faculty positions except at women's colleges, where many were still funneled by even their most sympathetic male mentors (Furumoto, 1987; Rosenberg, 1982). Thus, the ability to track intellectual networks (i.e., through co-authored publications, PhD supervision, etc), when many women worked either outside the academy or in colleges where they could not supervise graduate students, becomes tricky. Second, in this period, the distinction between social *scientists* and social

*reformers* became more rigidly drawn, with – largely female - reform work being generally excluded from the academic purview. As has been well-documented (Fitzpatrick, 1990; Leach, 1989; Platt, 1996; Rosenberg, 1982; Ross, 1998; Sklar, 1998), many of the women working outside the increasingly conservative academic environment were engaged in producing new forms of social knowledge that were grounded in social problems, and were explicitly reformist and policy-oriented. Centers for this female-led ‘research-and-reform’ agenda sprang up in New York, Boston, and Chicago. In this presentation, we take a closer look at the career trajectories, and relationships among female social scientists who attained their PhDs at the University of Chicago from 1890-1920 in order to more fully explore these dynamics.

Specifically, we employ the methods of digital geographic mapping and network analysis to try and uncover some of these more elusive patterns of influence and activity, and establish a visual prosopography. The digital mapping method has the advantage of allowing us to uncover potentially under-acknowledged or unidentified participants in this network. By systematically searching the University of Chicago alumni directories for women who obtained their PhDs in philosophy, political science, political economy, psychology, education, sociology, and anthropology between 1890-1920, we have begun filling in the geographic details of their academic and professional careers before and after their time at Chicago, and we can begin to ask (and answer) the following questions: Were there inter-disciplinary and inter-generational connections made through the university? What insight into the substantive directions of their careers pre- and post-graduation is provided by tracking their locales? In what ways did the University of Chicago’s inclusion of women endow the Progressive era with a cohort of female intellectuals whose contributions history has not yet been able to fully appreciate? Who has been lost from this history and why?

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**Allison L. Rowland (University of Colorado-Boulder) and Peter Simonson (University of Colorado-Boulder), *The Founding Mothers of Communication Research: Toward a History of a Gendered Assemblage***

This paper blends historical retrieval, feminist political intervention, and assemblage theory in an attempt to remember the founding mothers of the fields of communication and media studies. Before WWII, opportunities for women in the early field of communication dilated and women made consequential contributions to the field in the thirties and forties. Yet communication's feminist historiography practices aren't nearly as robust as other behavioral and social scientists, such as sociology, the home discipline for much early communication/media research. This essay argues for not only for the inclusion of these women in histories of the field, but also a reconception of intellectual history in the first place. Entwined humanistic and post-humanistic impulses create space for voices, labors, bodies, and other material things that deserve our attention. At the narrative and moral core of the story is a group of remarkable yet unsung women who, from the late 1930s through the early 1950s, helped to invent practices, produce research, shape thinking, and establish social relationships that laid foundations for media and communication study in the U.S. and the world. These women are the figures we call the founding mothers of our field. Focusing on the Paul Lazarsfeld-led Office of Radio Research (ORR) and the Bureau of Applied Social Research (the Bureau) from 1937-1949, we attend to four representative women (Herta Herzog, the "associate director," Hazel Gaudet, the "data analyzer," Thelma Ehrlich Anderson, the "interview trainer," and Rose K. Goldsen, "the secretary") as means of illustrating the range of roles played by women in an assemblage of human and non-human agents. Herta Herzog is the best known in a group of more than fifty women that came out of the pioneering Princeton Radio Research Project, the University of Newark's Office of Radio Research, and Columbia University's Bureau of Applied Social Research—a trio of organizations Lazarsfeld founded or co-founded after he emigrated to the U.S. in 1933. We present them as both individuals and representative characters, embodied figures who invented and animated new research methods, operated their technologies, mediated the communicative experiences of ordinary people, and did much of the material work that issued in the publications for which Lazarsfeld, Robert K. Merton, Elihu Katz, and other

Columbia men would gain credit as “founding fathers.” The collective story of the founding mothers is much larger than we can detail in this essay, but we provide a start, and in so doing illuminate a forgotten episode in the gendered history of our field. We end with a call for further extensions of the assemblage frame and further excavation of forgotten figures and other agencies in the history of the field internationally.

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**1:00-2:30pm KEYNOTE ADDRESS**

Chair: Larry Stern (Collin College)

**Mary Jo Deegan (University of Nebraska), *Jane Addams and the Hull-House School of Sociology: Specializations, Leaders, and Social Movements, 1889-1935***

Addams worked with a theory and practice, feminist pragmatism, anchored in her home and workplace, the social settlement Hull-House. She and her allies worked in at least twenty specialized areas of theory and practice that comprised the Hull-House school of sociology (HHSS). Each area had specialized leaders and organizations and only a few of the leaders, especially grass-roots leaders, are recognized today. My thesis is that we, at best, only partially understand the actual steps Addams used to successfully do this work, the large circles of people she knew, and the process of changing the world. I discuss four of these areas, race, arts and crafts, and peace, because they are particularly important and controversial today.

**2:45-4:45pm PAPER SESSION #7: The Development of Psychology in International Contexts**

Chair: Wade Pickren (Ithaca College)

**Zhipeng (Simon) Gao (York University), *Revisiting Chinese Psychology in the 1950s: Political Intervention, Dogmatism and Resistance***

The existent literature involves certain confusions regarding what happened to Chinese psychology in the 1950s and requires revision. Concerning the pre-1957 period, it has been argued that Chinese psychology was developed to meet Chinese cultural and social needs under the guidance of Marxism (Ching, 1980; Higgins & Zheng, 2002; Jing & Fu, 2001; Li, 1994). Against this view, I argue that Chinese psychology in that period often dogmatically served as an ideological token for maintaining the legitimacy of the communist governance; it neither followed Marxism nor met the needs of Chinese society. Concerning the year 1958, it is generally accepted that psychology suffered from a leftist “attack” (Jing, 1994; Petzold, 1994; The Executive Committee of Chinese Psychological Society, 1983; Yue, 1994). Against this face-value interpretation, I argue that the 1958 criticism not only was partially justified, but also, along with the consequent 1959 recantation by Chinese officials, made possible an “intellectual thaw” against the exclusive domination of Soviet psychology in China. Taking advantage of this opportunity, Chinese psychologists initiated a nationwide movement to reflect on the most fundamental theoretical issues of psychology, to re-orient psychology toward practical issues, and to re-introduce western psychology into China.

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**Ana Maria Talak (Universidad Nacional de La Plata, Argentina), Jose Ingenieros and the Former Psychology in Argentina: Knowledge, Practices, and Values in the Shape of the New Discipline**

It is known that the shape of a discipline is a collective development and not the outcome of a single author's work. However, it is an interesting question to explore the precise role that certain figures could play in the development of a discipline. The beginnings are often periods that can better show the particular role of these authors in the reach of theoretical and institutional consensus. We claim that José Ingenieros took part in a crucial way in the process of definition of theoretical boundaries of the former psychology in Argentina, during the two first decades of 20<sup>th</sup> century. He published his *Principles of Biological Psychology* in 1911, a book which was addressed not only to the teaching of the main topics of the discipline at university, but also to approach the major questions of psychology from the base of the international research of the field. In this definition, there were involved not only the conflicting debates on the relationships between psychology and other disciplines (such as physics, evolutionary biology, physiology, social sciences and philosophy), but the different professional practices that increasingly used psychological knowledge in their areas (clinics, criminology, education) as well. We are going to show that those multiple dimensions involved in the definition of the discipline were related with the different roles Ingenieros played in the local intellectual field and the relationships he established with foreign authors and the international developments of psychology. Besides, we argue that the conceptual frame of psychology, defined by Ingenieros, had to deal with the problem of values (political, ethical, genre values), that he tried to assume as natural dimensions of the social life, from an evolutionary view. Finally, we show how theoretical and practical problems of

psychology were so closely related to Ingenieros' ideas, within a broader perspective of interpretations of social and political life. The analysis of the different uses of the concept of adaptation will let us illustrate the role of values in the presumed free values science of psychology of those years. We also discuss the thesis that claims that Ingenieros introduced in *El hombre mediocre* (1913) (*The mediocre man*) idealistic categories hard to combine with the positivistic deterministic system (Terán, 2008: 38). On the contrary, we claim that those values were present in the core of the previous psychological system, and in the positivistic intellectual culture of the first years of the century.

In pursuing these aims, we are going to analyze and assess: 1) the conceptual frame of the biological psychology defined by Ingenieros and the intellectual operations he made in such task; 2) the performance of Ingenieros in the local intellectual field, before and after his crisis and his retirement in Europe (university, hospital, clinical private practices, Institute of Criminology, Society of Psychology of Buenos Aires, edition and publication of journals and books); 3) the relationships he established with foreign authors and the international field of psychology, through the base of his publications, his letters and travels; and 4) the impact his work had on the local field of psychology.

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**Rodrigo Lopes Miranda (Universidade Federal de Minas Gerais), Sergio Dias Cirino (Universidade Federal de Minas Gerais), and Regina Helena de Freitas Campos (Universidade Federal de Minas Gerais), *The Psychological Laboratory of the Belo Horizonte Teachers' College: Circulating Knowledge for the Modernization of the Brazilian School System in the First Half of the 20th Century***

Our goal is to describe and to analyze the Psychological Laboratory of the Belo Horizonte Teachers' College, located in the city of Belo Horizonte, Minas Gerais state, Brazil, from 1929 till 1946. This Laboratory was one of the first in the country and one of the most important. Two main aspects of the works done there are investigated: (1) the circulation of foreign psychologists; and (2) its instruments, as evidence of the circulation of psychological knowledge in Brazil at the time. The main sources were: (a) papers and reports produced by the Psychological Laboratory staff; (b) pictures of the laboratory; (c) letters and postcards; and (d) legal documentation issued by the government.

The circulation of psychological knowledge in Belo Horizonte during our time frame can be understood linked to the modernization of the city that is an example of the modernization of the country in the first decades of the 20<sup>th</sup> century. This process was associated to the expansion of urbanization and of industrialization, and the growth of the city's population as well. Belo Horizonte was a planned city built in the late 19<sup>th</sup> century to be the capital of the state of Minas Gerais, one of the leader states of Brazilian federation, in economic and political terms. For the city's intellectual and political elites, schools should be considered as institutions that facilitated the management and guidance of the emerging urban masses towards occupational opportunities, a belief widespread in the country at that time. Public elementary education was considered a priority and a series of educational reforms were then initiated with the purpose of improving training programs for teachers, mainly through knowledge of the scientific literature on education. Psychology was considered one of the main scientific disciplines among the sciences of education, since it helped to expand knowledge about the student, so that his/her education could be better planned.

In the Psychological Laboratory relevant data for the standardization of teaching methods and to know the learning needs of students could be collected. Several foreign specialists visited and worked at that laboratory. Among them: Théodore Simon, who had invented intelligence tests with Alfred Binet in Paris some years earlier; Leon Walther, a Russian psychologist working in Geneva on industrial psychology and professional guidance; Edouard Claparède, founder of the Rousseau Institute in Geneva, in 1912, a leader of the Progressive Education movement in Europe; and Helena Antipoff, a Russian educator, specialist in educational psychology, who had been Claparède's student and teaching assistant in Geneva before being invited to direct the Laboratory from 1929 onwards. According to Antipoff, the purpose of the Psychological Laboratory of the Belo Horizonte Teachers' College was to help Brazilian education to attain higher standards, placing a priority on psychological knowledge concerning children, child development, and elementary education in Minas Gerais. The Laboratory was equipped with a set of apparatuses that were common in other countries, such as Argentina, Switzerland and the United States of America. For example: a D'Arsonval Chronoscope and a Kymograph. These instruments, imported from Europe, were used for the training of elementary school teachers, students at the Teachers' College, in basic psychological measures. Working as a pedagogical tool, the Laboratory accomplished one of its goals: to prepare those teachers to understand scientific laws in psychology, and to do research with the purpose of expanding knowledge on children's intellectual and social development using standardized methods of inquiry. From this perspective, it played an important role in the establishment of psychology as a science capable of producing findings for teacher training and for the modernization and planning of the public primary school system in Minas Gerais.

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**Sunday, June 23, 2013**

**8:00-10am**

**PAPER SESSION #8: Institutions and Mental Health**

Chair: Gerald Sullivan (Collin College)

**James M. M. Good (University of Durham), *William Stephenson and the US NIMH: Lost Opportunity or Springboard for a Revitalized Career?***

When William Stephenson took up an appointment in 1948 as Visiting Professor in the Department of Psychology, University of Chicago it might reasonably have been expected that a secure future in the discipline of psychology lay ahead of him. Already in his mid-forties when he resigned his post at Oxford, Stephenson had been Director of the Institute of Experimental Psychology. He had also served with distinction during the war both as a civilian and military consultant to the British Armed Forces. By 1954, however, it was becoming clear to him that he was unlikely to get a tenured position at Chicago and his efforts to find a secure post in the United States, or even in the UK, intensified. In the early 1950s, while at Chicago, Stephenson had not only completed the manuscript of his 1953 book, *The Study of Behavior* but also two other manuscripts which were never published, *Intimations of Self* and *Psychoanalysis and Q-Methodology*. An initial draft of the latter book was submitted to the University of Chicago Press. Although the book was refereed favourably for the Press, the referee's recommendation was that more empirical studies were required to support it. In January 1955 Stephenson had spent nearly two months as a consultant at the Laboratory of Psychology, NIMH, Bethesda where he had met its first Director, William Shakow. Through Shakow arrangements were made for Stephenson to carry out such studies as Acting Chief of a new section on 'Personality and its Deviations'. In a 1962 interview, Stephenson recalls that he was unable to find an affordable house for his family in the Bethesda area and that he had to forego the NIMH post, taking up a position at more than twice the Bethesda salary as Director of Research at Nowlands & Co in Greenwich, Connecticut. By moving to a research position in market research, Stephenson effectively cut himself off from mainstream American psychology. Although he was not to lose interest in his longstanding quest to develop a quantum science of subjectivity it had to take second place to his concerns to make a place in the Missouri Journalism School 'for something other than the psychology of advertising' (Stephenson, 1962). Nonetheless, Stephenson subsequently noted that 'the field of advertising was a happy hunting-ground... and, at a graduate level, with researches under the rubric of Communication Theory and Research, he was free to develop not only a subjective approach to advertising but also a theory of communication and consciousness' (Stephenson, 1979). In this paper I outline the background to Stephenson's planned move to Bethesda, the nature of the post that was tailor-made for him and explore the reasons that led him not to take up the appointment. I also describe the vicissitudes of his Connecticut appointment, the termination of which eventually led to his move to the University of Missouri in 1958. I assess the significance of this episode in Stephenson's life for his subsequent career. The paper will conclude with some reflections on the challenges posed for the biographer by the unanticipated consequences of life events such as those described in the paper. The paper which is offered as an interesting fragment in the life and career of William Stephenson can also be seen as a footnote to the definitive study of the National Institutes of Health by Farreras, Hannaway and Harden (2004). The paper will draw upon material from the William Stephenson Archive, University of Missouri, the David Shakow papers, AHAP, Akron, and Stephenson family documents.

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**Ian Lubek (University of Guelph) and William Salmon (University of Guelph), *Historical Notes on Psychology's "Health": Tracing "Health Psychology's" Growth at one of its "Local origins" (SUNY at Stony Brook) and its Recent Divergence into "Critical Community Health Psychology"***

As a follow-up to a series of studies mapping the development of "Health Psychology" (Hanif et al, 2011; Lubek et al, 2011; Salmon et al, 2012a,b) we further employ quantitative indicators of growth of this sub-discipline, including the appearance of articles in journals and textbooks, the funding of research, and creation of professional infrastructures, university courses and training programs. We originally examined health psychology's emergence from neighbouring areas such as "mental health", "public health", and, in particular, "social psychology", the home discipline of the senior author whose own research had followed a shift from social to health psychology (Lubek, 2005). We juxtapose an autobiographical account with the oral histories of other observers at SUNY Stony Brook in the late 1960s to mid-1970s. Casting a historicist eye, it is noted that many of the early health psychology efforts were not given that label at the time. However, the authors of early research programs (e.g., urban stress; dental pain), and pioneering health psychology textbooks and Handbooks had congregated there, importing a bio-social, behavioral-cognitive model (e.g., Schachter and Singer, 1962) into a behavioural-clinical context (Ullman & Krasner, 1969). This paper also examines more closely one consequence of "normal science's" paradigmatic growth-- the emergence of a critique of mainstream research which has evolved into a critical (community) health psychology. (Hepworth, 2005).

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**Jenifer Dodd (Vanderbilt University), *Rapism: American Psychiatric Understandings of Rape in the 1970s and 1980s***

My paper is concerned with psychiatric understandings of rape in the 1970s and 1980s. In this era, the American Psychiatric Association attempted to pathologize certain types of rape by including a mental illness called "rapism" and later "paraphilic coercive disorder" in the Diagnostic and Statistical Manual of Mental Illnesses. Using archival materials from the APA, I will discuss how psychiatrists came to consider rapists as mentally ill.

Throughout the 1970s, psychiatrists had become increasingly interested in rape and a number of treatment centers for rapists had been opened. In these centers, rapists were considered an intermediary between criminal and insane—too dangerous to be put into standard psychiatric populations, but certainly in need of counseling and rehabilitation that they would not be given in prison. Yet, though a number of psychiatrists did consider rapists mentally ill, little research on the subject indicated that it could be considered a paraphilia. Rather, psychiatrists working with rapists typically found them to be suffering from various personality disorders that merely manifested in acts of rape. Embedded in

this was an acknowledgement of patriarchal norms much in line with feminist analysis; psychiatrists working with rapists argued that their mental illness manifested itself in a hyper-masculine way and that they took social norms to criminal extremes. The confusion on whether or not to classify rape as a paraphilia, I argue, may be due in part to the structure of the DSM itself. Psychiatrists working with rapists saw a need to include rape in the DSM and found alternative proposals—that rape be subsumed under existing categories like sadism or the various personality disorders—to be insufficient in capturing the realities of rape. Accordingly, Rapism emerged as a logical, if perhaps imperfect, diagnosis.

I will also offer a brief discussion of the backlash against paraphilic coercive disorder. After its inclusion in the DSM was publicized, numerous women's groups, mental health professionals, and regular citizens wrote to the APA in opposition to the disorder's inclusion in the DSM. This backlash was rooted in broad attempts by the women's movement to reform what they saw as patriarchal institutions—psychiatry chief among them. To these women, the pathologization of rape necessarily represented its decriminalization and many feared that the disorder would even further decrease legal penalties for rapists. Moreover, its status as a paraphilia was doubly offensive; the inclusion of rape in a category that included such innocuous things as foot fetishes implied, to many women, that the APA failed to take rape seriously.

In the battle between the APA and its detractors, discussion of whether or not rapists should truly be considered mentally ill was lost. I argue that, while protesters were correct that the idea of rape as a paraphilia was unsubstantiated, the acrimony of the debate ultimately shut down productive work concerning rape. The women's movement succeeded in keeping paraphilic coercive disorder out of the DSM, but the end result was the APA's ultimate decision, as an institution, to no longer consider rape as a psychiatric problem.

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**10:15-12:15**                    **PAPER SESSION #9: Philosophy/Phenomenology in Psychology**

Chair: Robert Kugelmann (University of Dallas)

**Scott Churchill, (University of Dallas), *Koehler, von Uexkuell, and Heidegger: On the Question of Animal Worlds***

The premise of this paper is that there are overlooked methodological guidelines to be found in the early works of ‘Gestalt’ researchers such as von Uexkuell (in biology) and Koehler (in psychology), as well as phenomenologists such as Husserl, Heidegger, and Merleau-Ponty, that might help to re-direct contemporary research into animal psychology.

To begin with, we will examine Koehler’s (1921) writings on the psychological study of apes, and then move on to von Uexkuell’s (1909, 1921) writings on “*Umwelt-research*” which describe his method of *participatory observation* where observation [*Beobachtung*] amounts to ascertaining which environmental signs registered in the observer’s own experiential world are also registered by the living being under observation; and, *participation* [*Teilnahme*] is the reconstruction of the animal’s *Umwelt* by means of a vicarious “sharing” of the processes which occur during the organism’s behavioral activities.

It was in fact Heidegger who had insisted upon a sympathetic reading of Uexkuell, even if the latter approached the question of animality from the evidence of his observations, whereas Heidegger himself preferred to start by observing that “we find ourselves moving in a circle” around two unsatisfactory approaches – the first being the mechanistic conception of life, and the second “employing a psychology crudely adopted from the human domain” in order to understand animality (1929-30/1995, pp 186-187).

What Heidegger attempted, in the 1929-1930 lecture course, was to articulate how it is that we stand in relation to the animal that stands before us. His position was that our understanding of everything -- of stones, of animals, of people, of psychiatric patients – is co-constituted on the one hand by the being that stands before us and on the other hand by our illuminating presence towards that being or realm of beings. Heidegger’s goal, in the texts where he was reflecting carefully on von Uexkuell, was to circumscribe the fundamental character of living beings. Following the lead of both Husserl and Dilthey, Heidegger’s own method was one of “transposing oneself” [*sich versetzen*] into the animal world.

My paper will examine the nature of this “transposition” – a term that resonates with the language of Dilthey, Husserl, Uexkuell, Koehler, and Merleau-Ponty. And yet, it seems that all of these thinkers fall short of a demonstration of a *genuinely empathic seeing* of the animal world. We will argue, on the basis of our own experiences encountering chimpanzees and bonobos in captivity – as well as on the evidence drawn from the work of those who have devoted a significant portion of their lives to living with and interacting with chimpanzees and bonobos-- that self-transposition into the world of the animal requires a kind of improvisational comportment in which we participate in the

gestural lives of our animal companions, as they participate in ours: *All zoology assumes from our side a methodical Einfuehlung into animal behavior, with the participation of the animal in our perceptive life and the participation of our perceptive life in animality.* (Merleau-Ponty, *Themes from the Lectures*, 97-98)

In developing the ideas of *Einfuehlung* and self-transposition in reference to understanding animal worlds, we will consider the deeper meanings of what it means to be the “witness” of behavior (of which M-P speaks in his 1945/1964 essay “The Film and the New Psychology”) – and of how the role of the witness changes once we move from “third person” to “second person” perspectives (Thompson, 2001).

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### **Rebecca Dance (University of Dallas), *A Historical Study of the Will***

In the early twentieth century, the will was historically accepted as an important category relevant to the study of psychology. It has been defined, described, and dissected by various early thinkers such as: William James, James R. Angell, Wilhelm Wundt, Narcissus Ach, Johannes Lindworsky, and Mary W. Calkins. A historical analysis will demonstrate the importance of the will in psychological theory. This paper will inform the reader of doctrines of the will from these six historical figures. From

William James we understand the will to be desire plus attainability with emphasis on the effort of attention and the effort of consent. James R. Angell adds to our understanding of the will by pointing to the effort as an indication of dammed up impulses and thus that our mental abilities are working within an imperfect system. Wilhelm Wundt gave us an affective will linked directly to emotions. One may find emotion without will (demonstrated through a lack of action), but one can never find a will without emotion close by. Narcissus Ach imparted to us various phases of the will beginning with physical strain followed the result or aim held in full view. From Johannes Lindworsky we learn that the will is an advantageous and realizable act. Lastly, Mary W. Calkins taught the overall importance of the will as it is related to the self as well as broadening the horizon to the notion of a domineering will. The argument will be made that the will is still relevant to psychology today. As a parallel to this taken-for-grantedness of the will a century ago, consider the consensus science has about the term 'observation'. There are papers written expounding upon the notion and chapters in textbooks dedicated to its explication, and certainly there are differences of opinion regarding its significance. However, you will neither find many papers dedicated to the defense of observation in science, nor many papers proclaiming the desperate need and proper place for observation in science. It is generally understood and generally accepted. So too, was the will a taken-for-granted category, at one point in the history of psychology. For psychologists like Calkins, the will was sufficiently prevalent in psychological thought and theory so as to be used as a tool, much like observation is used in modern times, to verify and give credibility to other psychological phenomenon.

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**Frank Scalabrino (University of Dallas), *A Brief History of the Problem of Agent Causation in the Human and Behavioral Sciences with a Recommendation for Future Research***

Agent causation contrasts with event causation, and the contemporary problem of agent causation may be characterized by the question: Does agent causation reduce to event causation? Further, a history of the problem of agent causation inevitably must touch upon the notion of free will in the human and behavioral sciences. Recall, for example, B.F. Skinner's famous claim, "There is no such thing as free will" (Skinner, 1978). Hence, a history of the problem should speak of both how event causation has been variously understood to eliminate or rule out free will and how agent causation has been variously formulated such that its possible reduction to event causation is now a question. Because the presentation of the history of the problem of agent causation here must be brief, I will speak to both of these criteria by centering the discussion on what I take to be the central issue, i.e. "purpose."

Whereas proponents of reducing agent causation to event causation have tended to define purpose in terms of desire fulfillment, proponents of agent causation have tended to define purpose in terms of intentional action. In this way, on the one hand, questions such as, “Can intentional actions be accounted for in terms of physical, chemical, or behavioral events?” have become central in the history of the problem of agent causation. On the other hand, identifying a necessary and non-reducible component of purposive, or goal-directed, action has itself become a valuable goal toward a resolution in favor of agent causation. Yet, how is “non-reducible” to be understood here, and in what way does “non-reducible” suggest freedom in regard to will?

Taking these final questions as points of departure, I will discuss the relation between memory and the will as it pertains to purposive behavior. Specifically, I have been working out an account of the anticipatory functions of memory as aspects of purposive action non-reducible in terms of event causation (cf. Scalabrino, 2011a, 2011b, 2012). And, sports psychology provides numerous examples of purposive behavior to examine; catching and kicking being perennial agent causation examples. Hence, I will provide a recommendation for future research to conclude discussing a brief history of the problem of agent causation and its relation to purpose in the human and behavioral sciences.

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