51st Annual Meeting

MacEwan University (Edmonton, AB, CA)

June 20-23, 2019

Conference Program and Abstracts

Acknowledgments

We would like to thank the following people for their much appreciated support and assistance in planning our annual meeting this year:

Cheiron Executive Officers:
> David R. Robinson, Executive Officer
> David C. Devonis, Treasurer
> Michael Dawson, Communications

Cheiron Review Committee Members: Rémy Amouroux; Nancy Digdon; Kim M. Hajek; Alan Tjeltveit; Phyllis Wentworth; Leila Zenderland.

Cheiron Book Prize Committee Members: Daniela Barberis, Kate Harper, Fred Weizmann, and Leila Zenderland (Chair).

Two most recent past program chairs: Robert Kugelmann & Jacy Young.

Proposal Reviewers: Each conference proposal was vetted by at least two anonymous reviewers, who provided valuable comments and recommendations.

Session Chairs: Katalin Dzinas; Christopher D. Green; Kim M. Hajek; Robert Kugelmann; Katharine Milar; David R. Robinson; Henderikus Stam; Alan Tjeltveit; Nadine Weidman; Leila Zenderland.

Local Helpers: We are grateful to many people at MacEwan who supported this conference: Melike Schalomon & Rob Wiznura from FAS Dean's Office; Susan Cooper, Kristine Monteiro & Jen Brunsch from Conference and Event Planning; Terri Suntjens, Krista Hanscomb & Ashley Albert-Hunter from kihêw waciston Indigenous Centre; Cynthia Zutter, Lyndsey Drozak & Bryce Wicks from Research Office; Aimee Skye & Psychology Department; Diana Porter from Residence; Marija Vukusic from M-Store; Jackie Paley from Communications; Provost Craig Monk; President Deb Saucier and Ray Baril on behalf of General Faculty Council; and student volunteers: Nour Al Adhban, Stephanie Belland, Leanne Buttery, Kennedy Garrett, Nick Hemmings, Mustafa Janoowalla, Hadla Omar, Geoff Rachor, Abby Riehl & Jessie Swanek.

We very much appreciate the financial support we received from a SSHRC Exchange Grant and from Conference and Event Planning, who gave us an affiliate discount. We could not have hosted Cheiron without this generosity.

Finally, we acknowledge that the land on which we meet in Treaty Six Territory is the traditional gathering place for many Indigenous people. We honour and respect the history, languages, ceremonies and culture of the First Nations, Métis and Inuit who call this territory home.

Cheiron: International Society for the History of the Behavioral and Social Sciences
51st Annual Meeting
MacEwan University (Edmonton, AB, CA)
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Local Host: Nancy Digdon (MacEwan University)

Program Chair: Phyllis Wentworth (Wentworth Institute of Technology)

THURSDAY JUNE 20
(Presenter name links to abstract; alt-right arrow returns)

12:00-2:00   **REGISTRATION (Allard Hall, outside of room 11–460)**

2:00-3:30   **Paper Session: Defining Psychology and Its Sub-disciplines**
            (Allard Hall, 11–454)
- Chair: Robert Kugelmann (University of Dallas)
- Michael R.W. Dawson, Cor Baerveldt, Vickie Richard, Evan Shillabeer, and Evan Li (University of Alberta), Exemplars of Philosophical Psychology: The Faces of Division 24 'Philosophical Psychology' at the 1963 APA Convention
- Alan C. Tjeltveit (Muhlenberg College), Interpreting the Boulder Conference III: Changing Normative Visions of the Science-Practice Relationship in Clinical Psychology
- Arthur Arruda Leal Ferreira and Hugo Leonardo Rocha Silva da Rosa (Federal University of Rio de Janeiro), On Early Laboratories for Experimental Psychology in Brazil: A Rainbow of Different Historiographies

3:30-5:00   **Reception and Poster Session (Allard Hall South Atrium, 11-200C)**
- David C. Devonis (Graceland University), Punishment as a Boundary Marker Between Sociology and Psychology.
- Nancy Digdon, Korbla Puplampu, Cynthia Zutter, Nour Al Adhban, & Melanie Ischewski (MacEwan University), Canada's Multicultural Policy, Social Inequality, and Introductory Textbooks' Portrayals of Ethnicity.
- Verena Lehmbrock (Erfurt University), Psychology of the Kollektiv. GDR social psychologists between ideology, scientific internationalism, and society (1960s/70s)
- Kristine Peace and Eric Legge (MacEwan University), Fantastic "Memories" and Where to Find Them: Understanding False Memories from the DRM Paradigm to Implanted Experiences.

- Jessie Swanek, Courtney Krentz & Kristine Peace (MacEwan University), The "Unscrupulous:" Historical and Modern Conceptualizations of Psychopathy from Deviant to Functional.

FRIDAY JUNE 21
(Presenter name links to abstract; alt-right arrow returns)

8:00-8:45   CONTINENTAL BREAKFAST (Allard Hall, 11–460)

8:45-9:00   Welcome from local host, Nancy Digdon (Allard Hall, 11–454)

9:00-10:30   Paper Session: U.S. Social Science in the Twentieth Century (Allard Hall, 11-454)
- Chair: David R. Robinson, Executive Officer of Cheiron
- Erik Baker (Harvard University), The Other Neoliberals: Joseph Schumpeter and U.S. Social Science in the Early Twentieth Century
- Leila Zenderland (California State University Fullerton), From Propaganda to Psychological Warfare: The Changing Terminology of Interwar Interdisciplinary Social Science
- Danielle Stubbe (Vanderbilt University), George W. Stocking, Jr.'s Culture Concept: The History of Anthropology and Disciplinary Myth-Making, 1942-1975.

10:30-10:45   BREAK

10:45–12:00   Indigenous Stories and Storytelling as a Form of Research (Allard Hall, 11–454)
- Chair: Nancy Digdon (MacEwan University)
- Krista Hanscomb & Ashley Albert-Hunter (kihêw waciston Indigenous Centre at MacEwan University)
- Short abstract: Exploring how Indigenous scholars, organizations and movements have addressed colonialism by deconstructing systemic processes through the reclamation of Indigenous stories and storytelling. The presenters will show how our roles and responsibilities as researchers need to be centered around a decolonizing approach which can be applied through various practices.

12:00-1:00   LUNCH (Allard Hall, 11–460)

1:00-2:00   Paper Session: Tools for Measuring and Treating Neurosis (Allard Hall, 11- 454)
- Chair: Christopher D. Green (York University)
- Ian Davidson (York University), Neurotic Introverts: Disciplining the Psychoanalytic Person

- Rémy Amouroux (University of Lausanne), Behaviorism with Pen and Paper

2:00-2:15   **BREAK**

2:15-3:15   **Paper session on Recent History: Psychology's Replication Problem and Data-Collection of Marginalized Students (Allard Hall, 11–454)**
- Chair: Henderikus Stam (University of Calgary)
- Christopher D. Green (York University), How Perverse Career Incentives Undermine Efforts to Fix Psychology's Replication Problem: A Recent History
- Jennifer Long (MacEwan University), Intercultural Competency and the Collection of Race-based Data in Small and Medium Sized Canadian Post-Secondary Institutions

3:15-3:30   **BREAK**

3:30-5:00   **Symposium: Writing the Human Sciences: Textual Organization, Collaboration, Interdisciplinarity (Allard Hall, 11–454)**
- Organizer: Kim M. Hajek (London School of Economics)
- Chair: Nadine Weidman (Harvard University)
- Daniela S. Barberis (Shimer Great Books School), A Journal as a Moral Community: The case of the Annee sociologique
- Kim M. Hajek (London School of Economics), Analogous Cases in Nineteenth-Century French Psychology: Narrating and Evaluating Double Personality
- Gerald Sullivan (Collin College), From Fear to a Steady State: Gregory Bateson's Narratives of Bali as an Anticipation of the Double-bind

5:00   **Trip to the brand new Royal Alberta Museum & dinner at local restaurants**

SATURDAY JUNE 22
(*Presenter name links to abstract; alt-right arrow returns*)

8:00-8:45   **CONTINENTAL BREAKFAST (Allard Hall, 11–460)**

8:45-10:15   **Paper Session: Mind, Body, and Soul (Allard Hall, 11–454)**
- Chair: Katalin Dzinas (Independent Scholar)
- Robert Kugelmann (University of Dallas), "The Irrepressible Desire for Unity:" Edward A. Pace on Psychology and the Soul
- Sean Hannan (MacEwan University), Knife-Edge & Saddleback: Augustine of Hippo and William James on the Psychology of the Specious Present
- Kate Harper (Wilfrid Laurier University), Alexander Bain's Mind and Body (1872): An Underappreciated Contribution to Early Neuropsychology

10:15-10:30   BREAK

10:30-12:15   Symposium: The Centre for Advanced Study in Theoretical Psychology (1965-1990) and the Search for an Integrative Psychology (Allard Hall, 11–454)
- Organizer: Cor Baerveldt (University of Alberta)
- Chair: Katharine Milar (Earlham College)
- Michael R. W. Dawson (University of Alberta), The Strange Intersection between Joseph R. Royce and Ludwig von Bertalanffy at the Center for Advanced Study in Theoretical Psychology
- Cor Baerveldt & Evan Shillabeer (University of Alberta), Recovery and Renewal: The Center for Advanced Study in Theoretical Psychology and the Quest for Conceptual Integration in Psychology
- William Smythe (University of Regina), Some Personal Recollections of the Center for Advanced Study in Theoretical Psychology in its Later Years
- Respondent: Leendert (Leo) Mos (University of Alberta)

12:15-1:30   LUNCH (Allard Hall, 11–460)

1:30-2:45   The Elizabeth Scarborough Lecture (Allard Hall, 11–454)
Sarah Igo (Vanderbilt University), Knowing Citizens: Reflections on Privacy, History, and the Human Sciences
Chair: Phyllis Wentworth (Wentworth Institute of Technology)

2:45-3:00   BREAK

3:00-3:45   Cheiron Book Prize (Allard Hall, 11–454)
Theodore M. Porter, University of California, Los Angeles
Genetics in the Madhouse: The Unknown History of Human Heredity
Chair: Leila Zenderland (California State University, Fullerton)

3:45-5:00   Business Meeting (Allard Hall, 11–454)

6:00   BANQUET (Allard Hall, 11-204 - Feigel Conference Centre)

SUNDAY JUNE 23
(Presenter name links to abstract; alt-right arrow returns)

8:30-9:30   CONTINENTAL BREAKFAST (Allard Hall, 11–460)

9:30-10:30   Paper Session: Treating & Educating Children (Allard Hall, 11–454
- Chair: Alan Tjeltveit (Muhlenberg College)

- Joshua Arduengo (Collin College), "The Scribble Head Riddle Likes the Light:" Gary Fisher's Treatment of Schizophrenic Children Using LSD-25
- Zhipeng Gao (York University), From Empirical Observation to Social Intervention: The Marxization of Psychology in China, 1949–1958

10:30-10:45   BREAK

10:45-11:45   Paper Session: Histories of Psychology Courses (Allard Hall, 11–454)
- Chair: Kim M. Hajek (London School of Economics)
- José María Gondra (University of the Basque Country), A Neobehaviorist Teaches History of Psychology: Edward C. Tolman's Lecture Notes on Psychology 124A.
- John Connors (Burman University) and Buddy Wynn (University of Regina), A History of the Only University Course in Parapsychology in Canada.

Abstracts

(Organized alphabetically by last name.)

Rémy Amouroux (University of Lausanne), Behaviorism with Pen and Paper

Historians of medicine have demonstrated that consultations through the medium of letters have begun in the Middle-Ages and were a fairly common practice in the 18th century (Barras & Pilloud, 2013; Weston, 2013; Wild, 2006). Letters at that time occurred mostly between an experienced and a more junior physician in order to get advice, or between a wealthy patient and a well-known therapist to get a diagnosis and a treatment. Then, in the beginning of the 20th century, "medicine by post" has been widely studied in the history of psychoanalysis (Borch-Jacobsen & Shamdasani, 2012; Ellenberger, 1970; Marinelli & Mayer, 2003). Here, the figure of Sigmund Freud played a central role, since he has used letters to analyze himself, his first students and even some of his patients. It seems obvious that psychoanalysts could be interested in private correspondence between individuals since this they have a particular interest in the role of relationships – real or not – and have proposed a rich conceptual apparatus to analyze it, notably the concept of transference.

But what do we know about the history of psychotherapy "by post" outside of the very specific field of psychoanalysis? One might indeed wonder if this setting could even be applied to every form of psychotherapy, and especially to behavior therapy which explicitly criticizes the psychoanalytical concept of transference? Moreover, what was then the interest of what we today call "remotely delivered psychotherapy" (Bee et al., 2008)? And why use letters in particular? We could also ask ourselves what are the historiographical value of those letters? What do they have to offer to psychotherapy historian?

This paper aims to begin answering those questions with the example of Jacques Rognant, a French psychiatrist and behavior therapist, who often used letters in his psychotherapeutic setting. Using letters in behavior therapy seems paradoxical. Indeed, why prefer a medium which promotes introspection when behaviorism is rooted in the critic of the very concept of consciousness? How is behavior therapy even feasible in such a context? Is it still behavior therapy? Interestingly, Rognant was not a minor character in the French history of behavior therapy. On the contrary, he played a significant role in its development since he was, in the early 60's, one of the first to practice. He then became one of the first presidents of the French movement in the early 70's, and translated Joseph Wolpe's pioneer work (Wolpe, 1975) into French. Rognant went as far as explicitly writing in his clinical articles that the therapeutic relationship often became a friendly relationship and that he exchanged letters with his patients.

This paper is based on the analysis of several interviews with Jacques Rognant, his academic writing and his personal archive. We will try to demonstrate that the use of letters in the practice of this psychotherapist are at the same time a necessity and a choice. A necessity, since Rognant practiced psychiatry in Brest, a provincial city of France far away from Paris, which means that many of his potential patients were not easily reachable. A choice, because Rognant claimed that it was part of a specific way to be and to interact with his patients which

contrasted with the psychoanalytic neutrality and the indifference – if not rudeness – expressed by some early behaviorists.

In this presentation, we will firstly briefly present the figure of Jacque Rognant and its role in the history of Behavior therapy in France. Then, using two case studies, we will endorse the claim that using letters was both a necessity and a choice. Thus, while studying an original practice of "psychotherapy by post", we will also be able to explore the beginning and indigenization of behavior therapy in the French context.

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Joshua Arduengo (Collin College), "The Scribble Head Riddle Likes the Light:" Gary Fisher's Treatment of Schizophrenic Children Using LSD-25

"When first introduced to me, [Nancy, 12-years-old], was in complete restraints twenty-four hours a day . . . This was necessary due to her extreme self-destructive behavior. If her hands were free, she would gouge out her eyes, hit herself in the head as hard as possible, bite her fingers, tear out her tongue. She was totally emaciated, covered with swellings and bruises, black eyes in sunken sockets . . . She looked like a beaten up, starved, wild, eighty-year-old woman . . . The attending physician felt that she would probably die. All known drugs had been tried. It was frightening to treat her with LSD, as my concern was her extremely frail physical condition and that she might die during a session . . . I was fearful that this would be our first and last LSD session. She was given 200 micrograms of LSD . . ."

At a California State Hospital in the early 1960s, Gary Fisher began a pioneering research program into the effects of LSD on seven autistic and schizophrenic children. To many people, especially members of the public, such a treatment would seem dangerous with significant potential for harm. However, as the above excerpt demonstrates, all previous therapies had failed to improve Nancy's symptoms. Fisher (1997) and his team - composed of a psychiatrist,

four psychology graduate students, and three psychiatric nursing technicians - felt like they had nothing to lose by trying this new treatment method.

Gary Fisher had his own encounter with LSD in 1959 after Al Hubbard, a renowned scientist from Saskatchewan, Canada, administered him the drug. Hubbard is a towering figure in psychedelic history and would go on to train Fisher in the practice of psychedelic therapy. At the time, it was common practice for therapists, staff members, and assistants to have had first-hand experience with the drug before being allowed to treat patients. Through his work in California, Fisher proposed that patients like Nancy had developed a massive defense system of repression-avoidance-denial against early childhood trauma. This defense system served to protect Nancy from re-experiencing the trauma, but it also kept her from further psychological development (Fisher, 1997). Fisher proposed that LSD could potentially break through this defense system, allowing the individual to re-experience the trauma resulting in the release of the trauma-associated pain. In several of his papers, reports indicate that Nancy, and several other children, showed significant improvements in their symptoms. Some children, Nancy included, demonstrated a significant transformation after their LSD sessions with a few of the children being able to return to school or visit their parent's home on the weekends (Fisher, 1970; 1997).

There exists a significant body of literature on LSD research from both primary and secondary accounts. Most of this research – although not all – was seriously focused on obtaining a better understanding of human consciousness and mental disorders. Sandoz laboratories provided ample supplies of LSD to doctors and researchers as early as 1949 and continued to do so until the early 1960s. Several prominent research groups conducted experimental work on patients with various ailments and disorders and the current work does not aim to re-tell that broad history. Rather, the aim of the current work is primarily focused on a small subset of LSD research conducted on autistic and schizophrenic children. Since the 1960s, the definitions of these two disorders have changed, but the symptoms include social withdrawal and isolation, extreme aggression, physical harm and injury to one's self and other patients, and what seemed to be visual or auditory hallucinations. Prior to the LSD sessions, these children showed little to no improvement in their quality of life despite their caretakers trying every known and available therapy.

According to a review by Abramson (1967), several groups administered LSD and psilocybin to children. The earliest reports of using LSD safely on children came from R.C. Murphy (1960a; 1960b) and T.T. Peck (1960). Additionally, at least four other research groups empirically investigated the effects of LSD among autistic and schizophrenic children: 1) Bender, Goldshmidt, and Sankar (1962), 2) Freedman, Ebin, and Wilson (1962), 3) Rolo, Krinsky, Abramson, and Goldfarb (1965), and 4) Simmons, Leiken, Lovaas, and Perloff (1966).

While I will touch briefly on these research groups, my main focus will be on Fisher's work, drawing upon three major sources: his papers located at the Purdue University Archives and Special Collections, an online repository known as *The Vaults of Erowid*, and papers from the Hoffman Foundation. After placing Fisher's work within the broader historical setting of LSD research, I will trace the evolution of Fisher's proposed hypothesis that LSD could cause a "breakthrough" experience releasing the pain of early childhood trauma. My analysis will begin with Fisher's own LSD experience in 1959 and end with his seemingly premature exit from academic life.

Next, I shall examine how the scientific community received his work by examining any other work that cites Fisher, as well as any personal communication he had with other researchers, including counter-culture figures like Timothy Leary. Fisher spent a brief amount of time with Leary, but eventually moved on from Leary's group in New York due to disagreements between the two men. Furthermore, and rather curiously, Fisher's work goes unmentioned in Abramson's (1967) review of the literature. And in at least one publication (Fisher, 1970), while Dr. Fisher outlines the work of Bender, Simmons, and others, he also makes sure to set his work apart from theirs as being the only work "which attempted to create an atmosphere which would optimize the therapeutic process" (Fisher, 1970, p. 129). Perhaps Fisher was left out of Abramson's review because his work was fundamentally different than the other work. Perhaps Fisher's work was unknown to Abramson (1967). Yet another possibility is that Fisher suffered professionally for his brief association with the LSD counter-culture. One of my aims is to clarify why Fisher is omitted from within literature of which one would expect to find him.

Last, I shall examine whether any of the work done with children in the 1960s has informed any of the current psychedelic research. We have observed a significant resurgence of research into the potential usefulness of psychedelics for treating mental disorders (*see M.A.P.S.*, the M*ulti-disciplinary Association for Psychedelic Studies* and the *Heffter Research Institute* for sources). If the older work with children has indeed influenced current research into the potential for psychedelics to successfully treat childhood mental disorders, can any of that influence be accounted for by Fisher's work? If not, what might be the reason for his exclusion?

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Cor Baerveldt & Evan Shillabeer (University of Alberta), Recovery and Renewal: The Center for Advanced Study in Theoretical Psychology and the Quest for Conceptual Integration in Psychology

Although the Center was officially approved by the Board of Governors of the University of Alberta in December 1965, recently recovered documents show that its founder Joseph Royce (1921-1989) already had the idea for the Center years before his appointment as the first head of the Psychology Department in1960 and that Center related activities already started just two years after his appointment as head. As Royce was on a sabbatical at the time of its inception as an independent administrative unit, the Center was temporarily headed by Ludwig von Bertalanffy, professor of theoretical biology and affiliate to the Center. The only other official member in the early period was William Rozeboom, a logical empiricist steeped in the tradition of Carnap and Feigl. Shortly thereafter the Center was joined by affiliates Herman Tennessen, a professor of Philosophy with an interest in continental philosophy, Thaddus Weckowicz, who held a joint appointment in Psychology and Psychiatry and Kellogg Wilson who held a joint appointment in Psychology and Computer Science. This diverse group held a broad variety of perspectives, based on phenomenological, existential and humanistic approaches as well as approaches informed by biological systems and computational theory. The Center would soon be joined be a growing number of faculty, post-doctoral fellows and graduate students (Mos & Kuiken, 1998). In the twenty five years of its administrative autonomy the Center's main activities involved organizing a total of five conferences (on topics including unification in psychology, the psychology of knowing, multivariate analysis, humanistic psychology, and behaviour genetics, see Royce, 1970, 1973; Royce & Rozeboom, 1972; Royce & Mos, 1979, 1981) and a regular series of so-called "seminars-in-the round". These seminars were inspired by the Ford Foundation-sponsored Intercollegiate Program of Graduate Studies and involved distinguished speakers from a broad variety of backgrounds in the human, social and biological sciences. In the period 1967-68 alone, these included philosophers Joseph Margolis and Michael Scriven; psychologists David Krech, Sigmund Koch, Duncan Luce, Kenneth Hammond, Bennet Murdock, James Gibson, Donald Campbell, James Bugental, Thomas

Natsoulas, and Robert Knapp; computer scientist John Holland; neuroscientist Karl Pribram; and psychiatrist P. Owen White. What makes the encounters between visitors and Center members so unique, is not merely the sheer variety of perspectives and disciplinary backgrounds, but the fact that those renowned scholars came together in a time in which the future of psychology was more than ever contested and in a way that allows us to see new and opposing ideas in their pre-sanitized, emergent form.

Our presentation will attend to some of the conversations in this early period, which historically marks a time when psychology was still finding a new language after the end of the behaviourist reign. Using recently recovered audio-recordings, two themes will be addressed in particular: first, the prominent role of systems perspectives in early theorizing and discussion in the Center (e.g. von Bertalanffy (1949, 1967, 1968a, 1968b), J.G. Miller (see Miller, 1978), E. László (see László, 1996) and second, the conversation between representatives of what we now consider to be the cognitive or 'mediational' perspective on perception and the ecological or 'direct' approach (as represented by K. Pribram (see Galanter, Miller, & Pribram, 1960) and J.J. Gibson (see Gibson 1950, 1966), respectively, who have indeed been recorded as having been in the same meeting room during their stay at the Center). By using a combination of conceptual and discursive analysis, we will be able focus not so much on ideas as finished and fully articulated, but rather on the way those ideas were actively conceived, 'tweaked' and 'tuned' in living debate between contemporary scholars. Moreover, discursive analysis will help us to identify interpretative repertoires (ways of constructing alternative, often contradictory versions of events through terminology, style, grammatical features and preferred metaphors and figures of speech) and discursive strategies (involving rhetorical organization and ways of attending to the normative accountability of one's words and actions). We will demonstrate that this juxtaposition of conceptual and discursive analysis allows us to disentangle intellectual ideas from historically specific stakes and interests, while preventing both sociological reductionism and 'great man' accounts of history. Implications of this method for the project of recovery and renewal of Center scholarship will be discussed.

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Erik Baker (Harvard University), The Other Neoliberals: Joseph Schumpeter and U.S. Social Science in the Early Twentieth Century

In the twenty-first century, the word most commonly attached to the name of Joseph Schumpeter (1883-1950) is "prophet." Contemporary readers of the Austrian-American economist's paeans to "creative destruction" and entrepreneurship have marveled at his anticipation of many of the central themes of today's conventional wisdom in economic and business circles. "America's hottest economist died fifty years ago," *BusinessWeek* announced in the title of a 2000 profile. *The Economist* has christened their business, finance, and management blog "Schumpeter."

Yet Schumpeter is absent from the vast historiography that has sprung up in recent years on the rise of today's dominant neoliberalism. Recent historians of neoliberalism, especially in the United States, have focused above all on F.A. Hayek and the Mont Pèlerin Society (MPS), the organization for free-market intellectuals and their supporters in business that Hayek founded in 1947. The basic story is one in which American conservative businessmen disgruntled with the New Deal drew on the organizational resources of the MPS to import the ideas of Hayek and others into a hostile postwar intellectual culture in the United States. Schumpeter indeed fits uneasily into this framework. Schumpeter was, like Hayek, a student of the Austrian School of economics and of its preeminent representative in early-twentieth-century Vienna, Ludwig von Mises. But his friendship with Hayek occasionally lapsed into coldness and friction. Schumpeter died only three years after the founding of the MPS, in which he did not participate. His fatalism about the long-term prospects of capitalism in his last years gave him little reason to participate in such a project.

But the most important reason that Schumpeter poses difficulties for the reigning historiography is that he was an active and influential participant in U.S. intellectual life from the time he arrived at Harvard in 1932. Kim Phillips-Fein has claimed that "in the early 1930s, a coherent body of conservative thought hardly existed" in the United States. On the contrary, in

this paper I show that when Schumpeter arrived in the U.S., he found a pre-existing community of conservative social scientists, preoccupied with similar questions to those Schumpeter had been exploring in Austria, and to which he would make his own distinctive contribution in the decades to come. These social scientists, particularly in the circles centered around L.J. Henderson and Elton Mayo at Harvard and around Robert Park at the University of Chicago, had read deeply in the work of the same turn-of-the-century European thinkers to which Schumpeter was chiefly indebted, such as Vilfredo Pareto, Max Weber, and Gabriel Tarde. In turn, the characteristic themes of the conservative U.S. social-scientific *milieu* underpinned Walter Lippmann's *The Good Society*, the book that prompted the 1938 Paris colloquium at which the term "neoliberalism" was first coined.

One reason why the connection between neoliberalism and the U.S. social science in the 1920s and 1930s, as embodied in the figure of Joseph Schumpeter, has been overlooked is that none of the key thinkers were particularly interested in the question of government "intervention" in "the economy," opposition to which is often taken as the key criterion of neoliberalism. They were indeed concerned about the prospect of socialism, but they saw its path being paved less by the decisions of certain policymakers and more by deep features of capitalist modernity: the development of large-scale, centralized organization; growing expectations of the capacity of human reason; and the expansion of democracy and an independent reform-oriented intelligentsia. The vision they formulated in response valorized entrepreneurship and "human-scale" organization; insisted on innate limits to human reason; argued for constraints on the scope of democratic action; and discouraged academic work oriented towards social transformation.

It was this vision, more than a distrust of "government intervention" *per se*, that was at the heart of the neoliberal transformation of U.S. society in the late twentieth century, and the later advocacy of Hayek and others. Returning to Schumpeter and his American interlocutors therefore underscores the extent to which neoliberal ideas were not just a tool that business conservatives used to resist government encroachment, but a response to a transcontinental sense of crisis in capitalism that became pervasive well before the New Deal. Such a reframing helps explain why the later rise of neoliberalism to hegemony entailed changes throughout society -- not only in policy thinking narrowly, but in political culture, academic practice, and the structure of capital accumulation itself.

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Daniela S. Barberis (Shimer Great Books School), A Journal as a Moral Community: The case of the *Annee sociologique*

The foundation of the *Année sociologique* journal played a pivotal role in the institutionalization of Durkheimian sociology in France. This paper will examine significant features of this publication, comparing it to other journals of the period and examining the writing practices developed by its collaborators.

Before becoming an independent journal, the *Année* was a rubric in a philosophy journal, the *Revue de métaphysique et de morale* (*RMM*). Célestin Bouglé opened the *RMM* to sociology and several years later contacted Durkheim to discuss turning the rubric into an independent journal. The *Années* were a journal format that had been popular for some time. There were the *Année artistique* (1878), the *Année médicale* (1878), and the *Année biologique* (1897) among others, but more significant for the Durkheimian enterprise were the *Année philosophique* (1891) and the *Année psychologique* (1895), both published by Alcan.

The *Année psychologique* was probably a model for the format of the *Année sociologique*. This is especially likely because, like the future *Année*, Alfred Binet's journal was the product of a group—rather than of a single man, as was Pillon's *Année philosophique*. It was the publication of the Laboratoire de psychologie physiologique at the Sorbonne, of which Binet was the director. The group of collaborators was remarkably stable between 1895 and 1901 and each volume was composed of three parts: the *mémoires originaux*; the reviews of the most important works of the previous year; and the bibliographic lists, which simply indicated all works that seemed relevant to psychology. The *mémoires* and the reviews took up about the same number of pages in the journal, and the *mémoires* were clearly separated into those written by members of the laboratory and those of external collaborators. While the *Année sociologique* never made this last distinction explicit in its pages, Durkheim did make it in its correspondence. Also, both journals reviewed both foreign and French literature (unlike Pillon's *AP* which only analyzed French publications).

This paper will explore the ways in which the *Année sociologique* modeled itself on other contemporary publications and the ways in which it purposely differed. The Année's explicit aim of being the collective product of a team, for example, set it apart from the leading philosophical journals, which, although they had programmatic statements, ultimately remained eclectic in practice (the editorial hand did pick the articles, but various factors to be discussed led to inclusion of material that did not follow the journal's aims/trends). The *Année* also had some this eclecticism at the start, but grew increasingly cohesive and programmatic over time, as will be explored.

I will argue that another significant difference was that Durkheim used his journal as a means of training his younger collaborators in a shared approach to sociology, expressed through the way they wrote reviews and in their original work. Through the process of creating this collective work, the Durkheimian group also produced a moral community, with specific moral-epistemic virtues.

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John Connors (Burman University) and Buddy Wynn (University of Regina), A History of the Only University Course in Parapsychology in Canada.

A university course in Parapsychology (Psych 300 Introduction to Parapsych/Psychical Research) was taught at the University of Regina from 1978-1997 by Dr. Buddy Wynn; it only ended when he retired. In the course syllabus it is admitted that "parapsychology is 'massively resisted' by most mainstream academics… as the phenomena seem so implausible and disconnected from, or even contrary to, other scientific findings … and there is yet no generally accepted explanatory theory." The textbook used was *Psychical Research: A Guide to its history, principles, and practices* (1987) – now out of print. The course was popular with students and always had good enrollment. This presentation will examine the history of this course and the influence of parapsychology research in Canada and around the world. For example, a unit on Parapsychology is now part of the Sensation & Perception course at the University of Calgary.

There is a long history of interest in Parapsychology in the history of Psychology. Some famous investigators include Anton Mesmer and mesmerism, Gustav Fechner and his idea of universal consciousness, William James' interest in the medium Mrs. Piper in 1886, William

McDougall who helped establish parapsychology as a university discipline in the US in the early 1930s, J B Rhine who founded the parapsychology lab at Duke University, Duncan Blewett at the University of Regina and his contacts with the psychic Eileen Garret, Arthur Koestler who endorsed a number of paranormal phenomena, such as extrasensory perception, psychokinesis and telepathy. There is now a Koestler Parapsychology Unit (KPU) at the University Edinburgh in Scotland. In the United States, both the Universities of Virginia and Arizona have academic parapsychology laboratories. In the early 1950s, the CIA started investigating the potential of psychic phenomena in military and domestic intelligence applications in a program known as Stargate.

Parapsychology is defined as 'the scientific study of the capacity attributed to some individuals to interact with their environment by means other than the recognised sensorimotor channels'. Surveys show that about 50% of the population believe in paranormal phenomena, and about half of these report paranormal experiences. Researchers have investigated the psychology behind these beliefs and experiences, including precognitive dreaming, out-of-body experiences, and hauntings. Research studies have been conducted using Zener cards, the Ganzfeld procedure, hypnotism, telepathy, and dream precognition.

Since many people claiming psychic and mediumistic ability have been shown to have been fraudulent, parapsychology research also explores the psychology involved in conjuring, deception, and eyewitness testimony.   Stage magician and skeptic James Randi has even demonstrated that magic tricks can simulate or duplicate some psychic phenomena. Despite these critiques, interest in parapsychology has continued for over 100 years. There are now 5 journals that focus on parapsychological topics.

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Library.

Ian Davidson (York University), Neurotic Introverts: Disciplining the Psychoanalytic Person

Conference attendees: do you consider yourself introverted? Maybe even a bit neurotic? In this presentation I will show some of my dissertation work on the history of those personality categories with which some of us might identify. Since the late-twentieth century, a large portion of personality psychology has become a sub-discipline of psychometricians exploring universal taxonomies of traits. Although versions and adaptations of a Five-Factor Model of personality traits (e.g., Openness, Conscientiousness, Agreeableness, Neuroticism, and Extraversion) have dominated this work since the 1980s, two of these traits have recurred in the tools and taxonomies of personality psychology throughout the entire disciplinary history: neuroticism and extraversion. Once referred to as the Big Two (Wiggins, 1968), this personality duo is pervasive and seemingly indefatigable—not only in disciplinary personality psychology, but allied human sciences and even popular understandings of the self.

This research focuses on the boundaries of psy-disciplines and their categories. Neuroticism and extraversion, as quantified personality traits, stem from psychoanalytic, psychiatric, and neurological categories. I explore the mostly European contexts from which these categories emerged, with a particular emphasis on psychoanalytic thought. Near the close of WWI, among the confluence of adaptations and critiques of psychoanalytically-flavoured techniques and neuroses (a battle-borne male hysteria among them), psychologists began incorporating these concepts into their nascent tools of measurement. During WWI, Robert Sessions Woodworth was tasked with categorizing the symptoms of shell-shocked American soldiers in order to make arguably the first formal personality questionnaire (Winston, 2012; Woodworth, 1920). Tools for measuring neuroses and emotional stability also gained traction in industry, where entrepreneurial psychologists were making profitable relationships with business owners during the early days of employee management (Gibby and Zickar, 2008). After the English translation of Carl Jung's *Psychological Types* was published in 1923, psychologists looking to quantify their subjects also began incorporating the notion of an introverted/extroverted typology.

From the 1920s into the 1930s, researchers produced a glut of psychometric tools for measuring everything from attitudes to masculinity. There were so many disparate scales that purported to measure introversion—or some variant of the concept—that some researchers began taking stock of their bountiful wares (e.g., Guilford & Braly, 1930). Although many argue that personality psychology did not become recognized as a substantial sub-discipline until the late 1930s, owing to the trajectory of famous venues like the *Journal of Abnormal and Social Psychology* and the promotional work of Gordon Allport, this earlier enthusiasm for psychometric research is essential (Davidson, 2018, Parker, 1986). Into the WWII era, despite ample critiques (e.g., Ellis, 1946), many psychologists researched and promoted quantitative techniques of measuring the self as superior to those more projective tools more obviously rooted into psychoanalytic and psychiatric traditions (Buchanan, 1994, 1997).

American psychologist Robert Gibbon Bernreuter stood at an important intersection of psychometric tools and the numerous concepts they purportedly measured. He was the first

psychologist to explicitly write about what he saw as the entangled nature of neuroticism and extraversion (Bernreuter, 1932). His conviction grew from his doctoral work that became his book and psychometric tool, simply called *The Personality Inventory*. Having spent his graduate school years under the advisement of intelligence popularizer Lewis Terman and then noted Australian eugenicist Stanley Porteus, Bernreuter viewed all facets of personhood as awaiting a quantification as successful as the measurement of intelligence. Much like little-known Oregon psychologist Edmund Conklin's attempt at disciplining Jungian typologies with theories and tools of the ambivert (Davidson, 2017), Bernreuter is a great case study of a fledgling field trying to assimilate concepts stemming from competing human sciences into their understanding of a correct science of psychology.

Tracing the history of the Big Two personality traits/types enriches the historical narrative of the oft-rancorous relationship between disciplinary psychology and psychoanalysis (e.g., Hornstein, 1992). Neurosis and introversion, or neuroticism and extraversion, and their associated psychometric techniques were vital for turf wars among physicians, therapists, and researchers during the professionalization of psychology. But along the lineage of the Big Two we do not only find the development of the widely used Minnesota Multiphasic Personality Inventory (MMPI); we also find the evolution of personality psychology into a largely psychometric sub-speciality of organizing trait taxonomies. Among Hans Eysenck, Raymond B. Cattell, and other (sometimes neo-eugenic) psychologists committed to factor analytic methods and pushing psychology toward the status of a natural science, the Big Two were bedrock. Despite in-fights over techniques yielding differing factor-solutions of the universal taxonomy of human personality traits, the prevalence of neuroticism and extraversion endured—even when other categories rooted in abnormal psychology, such as psychopathy, did not. Understanding the history of the Big Two might offer some insights into how our discipline has come to a rare near-consensus that there are five broad personality traits, applicable to all humans (and maybe canine and AI, too).

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Michael R.W. Dawson, Cor Baerveldt, Vickie Richard, Evan Shillabeer, and Evan Li (University of Alberta), Exemplars of Philosophical Psychology: The Faces of Division 24 'Philosophical Psychology' at the 1963 APA Convention

In 1962, the American Psychological Association (APA) approved the creation of Division 24 (Division of Philosophical Psychology). Division 24's history (Williams, 1999) begins with a brief letter arguing the need for the Division (Scott, 1960). This letter resulted in five individuals – Joseph Lyons, James Royce, Joseph Royce, Edward Scott, and Edward Shoben, Jr. – working together to form Division 24. They organized a successful petition for creating the Division. They then turned to recruiting members for the Division, to electing its officers, to designating

Division fellows, and to organizing a program for an APA convention. Division 24's first event was at Rice University's semi-centennial in March, 1963 (Wann, 1964). Division 24's first conference program was delivered at the 1963 APA convention in September.

This history glosses over a variety of practical problems faced by Division 24 organizers, problems that are revealed in their correspondence. We are exploring new archival materials that include Joseph R. Royce's Division 24 correspondence (Royce, 1962-1963). The practical problems (and their solutions) revealed by these materials are of interest for two reasons.

First, they show that historical accounts of the Division are incomplete. For example, there were important changes in who was involved in organizing the Division from its start in 1960 to its 1963 convention. Similarly, it has been noted that the Divisions' first conference had two invited addresses (Williams, 1999). While this was indeed planned, it did not actually materialize. There was only one invited address, presented by philosopher Ludwig Edelstein, who was recruited for this task a few weeks before the conference began.

Second, these materials reveal that Division organizers were deeply concerned with defining the nature of philosophical psychology (Royce, 1986, 1988; Royce, 1982). For instance, correspondence aimed at recruiting scholars for the invited address reveals that the founders of Division 24 had a much more pointed view of philosophical psychology than the one that they presented in their petition to the APA. In particular, Division 24 organizers strongly viewed philosophical psychology as a necessary reaction against psychological positivism.

In this presentation, we examine the Division 24 founders' approach to defining philosophical psychology by using this correspondence to investigate two of the problems faced in the earliest years of the Division.

The first problem that we explore is recruiting distinguished scholars for invited addresses at the 1963 conference. Letters show that Joseph Royce's original plan was to have two talks presented by two eminent scholars, with one of these scholars being European. A concerted effort to enlist Jean Paul Sartre as a speaker had failed by early February 1963. The organizers then pursued several other candidates, all of whom turned the Division down. The correspondence concerning this problem reveals the organizers' anti-positivistic perspective on philosophical psychology, both in terms of who they contacted and in terms of the contents of their invitations. We argue that this view was consistent with some of the organizers' other academic activities. For example, Joseph Royce established the Center for Advanced Study in Theoretical Psychology at the same time that he was helping found Division 24. The Center explicitly reacted against positivism (Dawson, Baerveldt, Shillabeer, & Richard, 2018); comparing the notions of philosophical psychology promoted by both the Division and by the Center provides an enlightening perspective on Joseph Royce's definition of the discipline.

The second Division 24 problem that we investigate concerns the makeup of Division 24 membership. We have discovered that the Division organizers made some difficult decisions to constrain who was involved in Division 24. One striking case in point concerns Henry L. Drake, an APA member and a registered clinical psychologist. Letters reveal that Drake was heavily involved in the early phase of the Division's creation. However, Drake was also associated with mystic Manly Palmer Hall's fringe organization (Sahagun, 2016). This resulted in Drake's involvement with the Division being dramatically curtailed. While Drake was a key organizer of the Division in 1961, by mid-1962 letters reveal urgent concerns about Drake, his goals, and the Division members he recruited. These concerns arose because of Drake's association with Hall's

organization, and intensified after Division organizers met Drake in person at the Rice Symposium. By the time the APA convention occurred, Drake had been shunned by the other Division 24 organizers.

Drake's case is interesting because letters reveal that the manner with which he was dealt with only arose after other organizers carefully considered the credibility of Division 24 in light of its membership and its public presentations. The 'Drake problem' letters provide additional evidence about how the organizers of Division 24 were focused on using the Division to define philosophical psychology and its relationship to psychology in general (Royce, 1986, 1988; Royce, 1982).

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Michael R. W. Dawson (University of Alberta), The Strange Intersection between Joseph R. Royce and Ludwig von Bertalanffy at the Center for Advanced Study in Theoretical Psychology

In 1960, Joseph R. Royce was planning to create the Center for Advanced Study in Theoretical Psychology at the University of Alberta (Royce, 1978). This led him to recruit biologist Ludwig von Bertalanffy in 1961 (Royce, 1981). Royce and Bertalanffy collaborated in developing the Center from 1961 through 1969 (Pouvreau, 2009; Royce, 1981; Royce, Bertalanffy, Tennessen, & Weckowicz, 1963).

The association between Bertalanffy and Royce seems highly unlikely. Bertalanffy received his PhD in 1926 under Vienna Circle founder Moritz Schlick, and then became a pioneering figure in theoretical biology (Bertalanffy, 1933, 1952). He was later celebrated for developing cancer detection techniques and for developing general systems theory (Davidson, 1983; Pouvreau, 2009; Ramage & Shipp, 2009). Royce was an experimental psychologist who earned his PhD in 1951 in Chicago under the supervision of L.L. Thurstone. Royce's research combined multivariate statistics and behavioral genetics (Royce, 1973; Royce & Mos, 1979); he became a leading figure in theoretical psychology (Dawson, Baerveldt, Shillabeer, & Richard, 2018; Mos, 1990; Williams, 1999).

What circumstances resulted in an American experimental psychologist and a European theoretical biologist uniting to establish a theoretical psychology center in Canada? This presentation explores the commonalities between the ideas of Royce and Bertalanffy.

Both Royce and Bertalanffy strongly believed in interdisciplinary education (Bertalanffy, 1953; Royce, 1961, 1964). Both were critical of educating specialists, and instead promoted the interdisciplinary training of generalist scientists. Such training was central in the Center.

Both had strikingly similar views of the role of theory. Royce believed that theoretical psychology's relationship to psychology was identical to theoretical physics' relationship to physics. For Royce, theoretical psychology was psychology (not philosophy) that had the role of developing better theories. Importantly, Bertalanffy made parallel points early in his career about the relationship between theoretical biology and biology (Bertalanffy, 1933).

The two also strongly opposed positivism (Dawson et al., 2018). In spite of his Vienna Circle training, this opposition characterized Bertalanffy's work throughout his career, beginning with his critiques of mechanistic theories of biological development (Bertalanffy, 1933). It was also a recurring theme in Royce's writings about theoretical psychology, because he believed that psychology's positivism caused it to value data more than theory.

Both believed in theory as an integrating force. This theme was important to Royce when the Center was created; its first international conference and focused on the possibility that theory could unify psychology (Royce, 1970). This theme was even more central for Bertalanffy. His early studies of biological development led him to propose that the organism is an open system (Bertalanffy, 1933), which in turn led to his general systems theory (Bertalanffy, 1952, 1968a). This theory sought parallel rules of organization in different systems; in the Center he applied it to psychology and to social issues (Bertalanffy, 1967, 1968b). In many respects the intersection between Royce and Bertalanffy was due the latter's use of an integrating theory to equate biology to psychology.

Surprisingly, Royce's eulogy for Bertalanffy indicates that Royce did not recognize the integrative power of Bertalanffy's theory during their time together (Royce, 1981). I argue that this failure had a profound impact on the Center's development and demise. Might the Center have persisted if it had used general system theory to unify its scholarly activities?

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David C. Devonis (Graceland University), Punishment as a Boundary Marker Between Sociology and Psychology

It could be argued that punishment is more protean a concept than can be captured by mere psychology or sociology, requiring a kind of über-social science interdisciplinary approach to do it justice, for instance, a sociology such as Michel Foucault's (Power, 2011). But outside of syncretic treatments of the genesis and rationale of punishment and imprisonment such as Foucault's, at least for the last hundred years or so within in the disciplines of psychology and sociology as typically constituted in North America, punishment has gravitated more toward sociology than psychology as a focus of concentrated attention. Some attention has already been paid to the interface between sociology and psychology at the point at which these disciplines (along with criminology) began to separate and go their independent ways early in the 20th century (Devonis, 2017). Now, starting from David Garland's *Punishment and Modern Society* (Garland, 1990) and working backward and forward in time, this poster will trace, in maplike fashion, sociological sources of thinking and writing about punishment as these connect to psychology, aiming to delineate the boundary between the two fields as it emerged between 1930 and 1990. One interesting collateral finding is that the sociology of punishment directly referenced emotion and emotional management in connection with prison experience, for instance in the work of Stanley Cohen. The presence of psychological terminology within sociology in this domain suggests that sociology, not only in relation to punishment but across wider, non-punishment related areas of interest in sociology (e.g., as described in Lively & Weed, 2014) developed a self-sufficient psychology of emotion that contributed to the development of insularity between and within the two fields.

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Nancy Digdon, Korbla Puplampu, Cynthia Zutter, Nour Al Adhban, & Melanie Ischewski (MacEwan University), Canada's Multicultural Policy, Social Inequality, and Introductory Textbooks' Portrayals of Ethnicity

Canada, like other nations with multiethnic groups, is confronted with a diverse ethnic profile. The history of Canada's multicultural policy (i.e., Official Multiculturalism in 1971; Charter of Rights and Freedoms in 1982; and Canadian Multiculturalism Act in 1988) was intended to create, even if theoretically, a platform to engage with diversity on the basis of equality (Dewing, 2013). But one major obstacle is that ethnicity has persistent and significant consequences for social inequality in Canada (Porter, 1965; Ramos, 2012). Porter's (1965) path breaking study, *The Vertical Mosaic,* laid bare the significance of ethnicity for employment possibilities, income, and the incidence of racism. Weber defined ethnicity as "those human groups that entertain a subjective belief in their common descent because of physical type or of

customs, or both, or because of memories of colonization and migration; this belief must be important for the propagation of group formation" (1996, p.35). In Weber's definition, the social construction of race is inferred; however, there are also instances where ethnicity and race are used interchangeably (James, 2003: Kendall et al., 2004). Ethnicity has been operationalized in different settings and for different reasons (Fleras, 2017, Puplampu & Tettey, 2005). Moodley (1983) cautioned that Canadian multiculturalism ideology resulted in "the usage of 'ethnic' to refer to all cultural subgroups including 'dominant ethnics' thereby obfuscating the cultural hierarchy and redefining ethnicity until it is meaningless." (p.320).

How ethnicity is conceptualized and its significance in human interaction is relevant to the role of universities in knowledge dissemination. Curriculum is important in view of ethnicity's centrality to social inequality in Canada. Anthropology, psychology, and sociology are particularly relevant to learning about ethnicity and introductory textbooks in them reach thousands of students. We conducted content analyses of portrayals of ethnicity in introductory textbooks published between 2018 and 1990 (i.e., providing a 2-year window for uptake of Multiculturalism Act). We compared successive editions, as well as US editions to their adaptations for the Canadian market. We compared treatments of ethnicity both within and between disciplines. We questioned how conceptualizations of ethnicity informed understandings of multiculturalism and social inequalities. The juxtaposition of examples shows a range of messages about ethnicity in the age of multiculturalism and we ponder the implications of these messages for student readers living in a multiethnic society.

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Arthur Arruda Leal Ferreira and Hugo Leonardo Rocha Silva da Rosa (Federal University of Rio de Janeiro), On Early Laboratories for Experimental Psychology in Brazil: A Rainbow of Different Historiographies

In writings on the history of psychology in Brazil, we observe that certain historical operations (Certeau, 1988) and historiographies occur with regularity, with certain characteristics according to the specific domain. Brazilian historians such as Venâncio & Cassilla (2010) and Wadi (2014) describe the historiography of psychiatry and psychopathology in Brazil with an intensively critical perspective, generally inspired by Foucault's (1978) *History of Madness*. In these works on the history of psychiatry in Brazil there is an absence of any progressive or evolutionary narratives. The mainstream historiography of experimental psychology differs from this exclusively critical approach. Since Boring's (1950) classic work, for example, there has been a strong tendency (present also in Brazilian articles and text-books) to consider the establishment of laboratories as historical landmarks, which distinguish the scientific from the pre-scientific past (see Massimi, 1990; Mitsuko 2012; Centofanti, 1982). Generally, it produces a kind of research devoted to finding the first and seminal laboratory in different contexts. Considering the split between the critical and the evolutionary versions of history devoted to different areas of psychology, we can say that the history of psychology in Brazil reveals very different and irreconcilable historiographies with different rhythms, meanings, and colors (Nietzsche, 2008). The aim of this presentation is to review the historiography through primary and secondary sources (Centofanti, 1982, 2006, 2014 and Monarcha, 2007, 2009) related to the description of the first psychological laboratories in Brazil, focusing on some standard questions: How did these early laboratories work? Who frequented them and was observed working there? What aspects of their lives have been experimented on? What was the importance of these places? When were they active? What were the purposes of psychological experimentation in schools and in psychiatric institutions? The exploration of these questions allows us to draw some conclusions about the frontiers between the diverse historiographical styles of the history of psychology in Brazil, revealing connections and barriers between the aforementioned approaches.

Some of these early psychological laboratories were created in association with psychiatric institutions: Waclaw Radecki organized such a laboratory in 1924, connected with the Colony of Psychopaths (an asylum) in Rio de Janeiro. This laboratory was mainly for diagnostics and prognostics, measuring the variations of pathological symptoms in the patients. Other early laboratories were connected to schools geared towards to students with special needs: the *Pedagogium*, in Rio de Janeiro (Bonfim, 1928), and the *Caetano de Campos Model School*, in São Paulo (Quaglio, 1921). In educational settings, the laboratory had an equivalent function to its psychiatric counterpart; it's objective was to examine the degree of abnormality of the student. Quaglio (1921) and his collaborators did research using psychological devices that explored organic and psychic shortcomings, leading to necessary interventions that aided the educational process. His proposal was to inspect all students, with the aim of creating a biographical portfolio for the schools and assisting the teachers in their pedagogical practice. In this sense, experimental psychology was added to a set of emerging disciplines since the 19th century—mental hygiene, anthropometry, pedagogy and pedology—that formed a cohesive

network of knowledge whose function was the examination of the students and of mental illness.

As initially proposed, the blue genealogy (Nietzsche, 2003) of these laboratories generally contrasts with the grey historiography of other psychological practices, such as those linked to psychiatry. Nevertheless, in Brazil the practices of these areas are connected in a greater degree that one may initially suppose. In our perspective, it is necessary to look for the connections between these stories as they are presented in the Brazilian scenario, evaluating the clues left by the first laboratories as to their occupants and their practices. It is this mixture of colors that we would like to propose as a more symmetrical approach (Latour, 1987) to the history of psychology in Brazil. If, as Nietzsche (2003) proposed in his *Genealogy of Morals*, genealogies may have distinct colors (his gray genealogy compared to the blue one of the utilitarian philosophers), we should ask ourselves about the color of the history that we propose for early Brazilian psychological laboratories. Certainly it would not be homogeneously blue (like that of the utilitarian philosophers), or golden (promising a golden age for psychology), or bronze (the color James attributed to laboratory instruments). It would no longer have the triumphant colors of the epic narratives; rather it would appear as a multicolored ball of yarn, representing diverse practices and actors, strangely combined in their heterogeneity and described in a more symmetrical perspective as a network combining scientists, experts, public interests, laboratory instruments, children, patients, asylums and schools (Latour, 2005). Perhaps this symmetrical historiography would clearly be like a rainbow, combining the bronze instruments with the blue certainty of the men of science and the added iridescence of more infamous men.

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Zhipeng Gao (York University), From Empirical Observation to Social Intervention: The Marxization of Psychology in China, 1949–1958

This presentation examines the history of Marxist psychology in the first decade of People's Republic of China, between 1949 and 1958. It argues that the Marxization of psychology led to the replacement of the conventional empirical observation model to a social intervention model in addressing the human mind. This transition encompassed three epistemological-ontological nexuses. First, Chines revolutionary scholars criticized ahistorical empirical observation for being blind to how the Chinese mental reality had been distorted by class domination. With a belief in human malleability, they argued that human being could realize its true potential only in a conducive environment emerging through the revolution. Second, Chinese revolutionary scholars criticized that the context of observation – be it experimentally designed or naturalistically selected – revealed the mechanistic aspects of the human mind only. They required psychologists to replace their conceptualization of objective reality with a sociopolitical worldview to allow the expression of human agency. Third, Chinese revolutionary scholars denounced the principle of value neutrality precisely because it eschewed political engagement, and because psychologists, in reality, had made implicit normative judgement guided by instrumental rationality. As a solution, they called for psychologists to shift their attention from mental process to mental content. Together, the three epistemological-ontological challenges required the empirical observation model in psychology to be substituted by a social intervention model exemplified in the field of education. This paradigmatic, transdisciplinary shift instantiates the program of psychological humanities and fosters reflection on how Chinese and Western psychologists' research assumptions are shaped by the sociopolitical milieu they find themselves in.

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José María Gondra (University of the Basque Country), A Neobehaviorist Teaches History of Psychology: Edward C. Tolman's Lecture Notes on Psychology 124A

This paper examines the lecture notes that Tolman wrote for the course of history of psychology that he taught during the first half of the academic year 1922-1923 (*Psychology 1924A*), which are held in the Edward C. Tolman Papers at the Archives of the History of the American Psychology (Tolman, 1922-1923). Being the longest and most detailed of all the lecture notes he wrote throughout his career, its fifty-one handwritten and non-paged sheets offer an interesting picture of Tolman's teaching style, based on the reading of primary sources and classroom discussions.

The first course of history of psychology ever held at Berkeley University was introduced in 1903 by George M. Stratton (1865-1957), then director of the Psychological Laboratory. After getting in 1896 his PhD at the University of Leipzig under Wilhelm Wundt (1832-1920), Stratton returned to the Department of Philosophy and scheduled a half-year history course that the next academic year would be offered as "a critical account of psychological theory and method from Aristotle to the present time" (University of California, 1904, p. 135).

However, in September 1904 Stratton moved to Johns Hopkins University and this course had to be given by Knight Dunlap (1875-1949), then assistant in the Psychological Laboratory. Back in Berkeley as professor of psychology in 1909, Stratton continued teaching it until 1922, when he was appointed as director of the newly founded Psychology Department. Because of this appointment, Tolman had to deal with an entire course of six units on a topic that he had not taught until then.

Edward C. Tolman had arrived to Berkeley by the end of World War I. Shortly after his arriving, he introduced a new course on Animal Psychology and another on Advanced General

Psychology that discussed the psychological systems of the time (Carroll, 2017). This course had an obvious relationship with the schools of psychology, the topic of the second history course (*Psychology 124B)* of which very few traces remain in the archives.

Psychology 124A was organized following Max Dessoir's *Outlines of the history of psychology* (1912), which was the main textbook used by Tolman together with *The Classical Psychologists* (Rand, 1912) as a book of readings. Classes were held on Mondays, Wednesdays and Fridays, beginning on August 25, 1922 and ending in December with the final examination. Friday classes were reserved for recitation and criticism of the assigned readings (University of California, 1923).

The choice of Dessoir's textbook seems somewhat striking in a behaviorist like Tolman, since Max Dessoir (1867-1947) was a Neo-Kantian philosopher with a special interest in aesthetics who played an important role in the founding of the German Society for Psychological Research with the purpose of investigating the paranormal phenomena (Sommer, 2013). His interest in spiritualism, however, was not quite alien to American psychology, given the work of William James in that field.

Tolman first lecture paid special attention to Dessoir's introduction in which he stated that in the earliest times the soul was studied from three different perspectives: religion, emerging natural science, and literature. The primeval religious soul-cults gave rise to the metaphysical doctrine of a divine immortal soul-substance which could be designated as *Psychosophy.* The identification of vital forces with the soul was to be the chief source of modern scientific psychology. This vision, however, would be incomplete without the literary and artistic point of view which was concerned with the riddle of character and could be called *Psychognosis.* According to Dessoir, the history of psychology couldn't be written by only considering the earlier stages of present scientific psychology because the metaphysical and scientific doctrines of the soul had always existed alongside or opposed each other.

After this introduction, Tolman started with the Orphic- Pythagoreans mystic doctrines, and then he spoke about the biology of the soul (psychology) by reviewing the Eleatics, Atomists, Plato and Aristotle, with special attention to topics such as the soul, nature of consciousness, association, and instinct. In addition, the students had to read and discuss long texts of these authors.

Following Dessoir up to the German philosopher Johannes Herbart (1776-1841), then Tolman took as guide Warren's *History of Association Theory* for British Associationism and Brett's *Modern Psychology* (1921) for the nineteen century physiologists, to conclude with William James's *Briefer Course* for pre-behaviorist modern psychology. Fechner's *Psychophysics* and Wilhelm Wundt's *Experimental Psychology* only received a single line in the whole set of notes.

Few psychologists are aware that Tolman taught this course since he did not mention it in his autobiography, nor his biographers made any reference to it (Carroll, 2017, Innis,1992). But it is encouraging to see that in the first decades of the twentieth century the course of history of psychology played an important role at Berkeley University and that such prominent figure as Tolman put so much effort into the job of teaching it.

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**Christopher D. Green (York University), How Perverse Career Incentives Undermine
Efforts to Fix Psychology's Replication Problem: A Recent History**

Of late, much has been made of the apparent pervasiveness of questionable research
practices (QRPs) among psychological researchers (e.g., *p*-hacking, optional stopping, selective
reporting; John, Loewenstein, & Prelec, 2012; Masicampo & Lalande, 2012; Simmons, Nelson, &
Simonsohn, 2011; Simonsohn, Nelson, & Simmons, 2014). In combination with the preference
of journals to publish only novel and statistically positive results ("file drawer problem" or
publication bias; Rosenthal, 1979), the credibility of a great deal of psychological research
appears to have been undermined. Many psychological phenomena have recently proven to be
non-replicable. Further, due to widespread editorial bias against publishing replications, we
were unable to discover this until it had seemingly consumed a distressingly large portion of the
field. It now appears that only about half of published psychological research reports reliable
phenomena (Klein et al., 2014; Klein et al., 2018; Open Science Collaboration, 2015). Research
psychologists did not fall into this quagmire by themselves, however. Incentive structures
created to satisfy administrative and political demands have played a strong role in shaping the
behavior of researchers and, worse still, in selecting for career "survival" those inclined to
prioritize personal career success over those who put scientific integrity first.

Since the rise of the modern research university, institutions of this sort have preferred professors who are active researchers to professors who were content to be teachers. In the US, this began, essentially, with the founding of Johns Hopkins University in 1876, but it did not catch on widely until the start of the 20th century. The influential scientometrican Eugene Garfield (1996) claimed that the origin of the now-ubiquitous phrase, "publish or perish," was a 1942 sociological study of academic life (Wilson, 1942). In fact, it was used (also in sociology) as early as the 1920s (Case, 1928). Regardless of the exact origin, the pressure upon academics to publish – at first regularly, later frequently, and finally continuously – steadily increased over the course of the 20th century. Traditionally, the assessment of this research activity and success was left to experts in the field – "peers" – who could most correctly evaluate the quality of the research. Increasingly, over the last quarter of the 20th century and into the 21st, metrics were used that putatively enabled people with no expertise in the field – administrators, politicians – to assess how active and successful at research candidates for hiring or promotion has been.

Originally, the sheer number of articles was used to measure research success. But the increasing pressure to publish more frequently meant that many articles were published that were rarely read and had little influence. In response to this self-created problem, new metrics were adopted to capture not the number of articles, per se, but the number of times these articles were cited in other articles. The Journal Impact Factor was first developed in the 1950s by the Eugene Garfield (1955). This obscure measure did not start being widely used outside of scientometrics, however, until the 1980s, when it began to be widely *mis*used to measure the influence of individual articles and individual researchers, rather than of whole journals (Moustafa, 2015). Even Garfield (1999) complained about this misapplication late in his life. Journal Impact Factor has recently started to be superseded by supposedly improved variants such as the h-index, the g-index, and the i-10 and i-20 indices. Because this pressure to produce not just articles but citations was produce a glut of both, the emphasis has shifted to only recent citations – those produced only in the past few years.

The problem with all of these metrics is that they draw attention away from the quality of the content and toward the numbers that serve as proxies for it. In combination with the rapid decline in the availability of career positions, the focus of many researchers has been forced away from producing the highest quality science and toward maximizing the numbers that would decide their academic futures. Thousands of fake journals rapidly appeared on the scene in the 2010s, willing to publish nearly anything for a price (Carey, 2016; Elliott, 2012). Some particularly ingenious, if unscrupulous, researchers have gone so far as to publish, under pseudonyms in fake journals, nonsense articles that profusely cite their legitimate work (under their real names) in order to create the illusion of greater impact.

In such a ruthless academic environment, it is little wonder that psychological researchers have been increasing cutting statistical corners in a desperate scramble to get their articles into journals more quickly and in greater numbers than ever before. But that is precisely the sort research that is least likely to later replicate. These sorts of career pressures, along with more common explanations may be the primary cause of the replication "crisis," and it is these perverse career incentives that must be addressed if the "crisis" is to be resolved.

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Kim M. Hajek (London School of Economics), Analogous Cases in Nineteenth-Century French Psychology: Narrating and Evaluating Double Personality

This paper takes up the question of how knowledge is generated out of sets of case histories (or clinical observations) in the human sciences, by exploring, as a first step, the role of writing practices in grouping individual cases as "analogous," or alternatively, as separating them into distinct category spaces. It asks, moreover, whether cases are narrated or evaluated differently by scholars trained in differing disciplinary traditions.

The scientific context of late nineteenth-century French psychology lends itself well to considering such matters, with the case of Félida X. providing a particularly apt focus for my

analysis. Variously described as suffering from double personality, periodical amnesia, and extraordinary neurosis, Félida was first studied by Bordelais surgeon Eugène Azam in 1858–59. Azam eventually published his observations in 1876. The case then quickly became an essential reference point and evidentiary source for *savants* attempting to establish psychology as a positive scientific discipline, distinct from philosophy (see Carroy 2001). Referenced, reworked, and retold, Félida's story threaded through enquiry in medicine, nascent scientific psychology, and official philosophy. At the same time, a number of "analogous" cases—or what were claimed as such—collected around Azam's accounts of Félida (and his frequent updates on her state): physicians communicated unpublished observations that seemed to fit the topical theme, or examined current patients in light of Félida's condition, while other scholars recuperated neglected cases to fill out categories of double personality or amnesic troubles. Then in 1879, philosopher Victor Egger and physician Léon Lereboullet jointly authored an article reinterpreting some of the cases, so as to draw out their full interdisciplinary potential.

In this paper, I explore these two groupings of cases in turn, contrasting the narrative elements and broader writing practices which function to link up (or to hold apart) individual cases. What larger narrative arcs serve to organize cases as ultimately "analogous" variations on a given psychological or pathological theme, even when they were published by different physicians (Azam, Dufay, Bouchut) at distinct points in time? How did these authors mobilize citations, concrete details, or specific narrative choices to configure their particular observations in relation to one another? And to what extent was the interdisciplinary and collaborative nature of Egger and Lereboullet's work essential to recasting narrative links between "analogous" cases?

After attending to textual features of cases published separately, I analyze Egger and Lereboullet's collaboration in greater detail, examining not only narrative elements in their published paper, but also the writing and collaborative practices that gave rise to it. Egger's extensive notes allow me to retrace the epistemic and textual bases by which he evaluated Félida-like cases; his philosophical training, notably a sense of "la critique historique" (1887, 309) prompted him to revise groupings that were somewhat taken for granted in the medical literature. But although the critical synthesis present in the published article also owes its textual form to Egger and Lereboullet's collaborative practices, archival traces of their "concours éclairé" (1879, 357) raise questions as to whether theirs was truly a cooperative endeavor, or whether, on the contrary, it should be considered as the product of distinct, even competing, disciplinary perspectives. If the latter, might narrative features then serve to conciliate disciplinary strands, as well as organizing individual cases, in psychological writing?

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Sean Hannan (MacEwan University), Knife-Edge & Saddleback: Augustine of Hippo and William James on the Psychology of the Specious Present

The question of time-consciousness has occupied centre-stage at key moments in the history of the behavioural and social sciences generally, as well as in the history of psychology specifically. It has, moreover, played a similarly central role in the development of Greek philosophy and (usually in conversation with the Greeks) that of Christian theology. Methodologically speaking, then, past debates over how the human mind perceives time provide us with an opportunity to build bridges between humanistic work on philosophies of temporality and psychological studies of temporal self-awareness.

Of the many questions posed about time by both humanists and social scientists, one of the most perplexing has been the question of whether the human mind directly experiences a present phase of time. While, on a commonsensical level, the present time would seem to be the most 'real' component of temporality, it turns out to be difficult to pin down exactly what the present time would be, since it is always slipping into the past in order to make way for the future. To ancient observers like Aristotle or the Christian bishop Augustine of Hippo, the present seemed to be an illusion, which meant that human time-consciousness must be a more complex affair, taking account of both an ever-arriving future and an ever-disappearing past, with only a 'specious present' posited as lying in between.

During the dawn of scientific psychology in the nineteenth century, this problem of the 'specious present' re-emerged as an item of peculiar interest to those aiming to map out the functions of the human mind. On the European continent, empiricists like Franz Brentano (in his

1874 *Psychology from an Empirical Standpoint*) singled out present experience as important for further study, which influenced later generations of both psychologists and phenomenologists. Within the English-language world, similar concerns became evident in the works of William James, such as his 1890 *Principles of Psychology*.

James' approach to time-consciousness is characterized by a general disdain for the paradoxes which so plagued previous philosophers. It is true, he admits, that the philosophical notion of a mathematically exact instant of time presents many problems; but, he counters, psychological accounts do not need to deal with such an instant, since human mental experience proceeds by way of a specious present that has duration. It is not point-like; rather, it lasts, albeit for a potentially arbitrary span of time. In one memorable formulation, James argues that the psychological present is not a "knife-edge" but rather a "saddleback."

In order to argue that the specious present offers a firm foundation for the analysis of mental experience, James must also explain how this specious present works. Some earlier psychologists, like the German William Stern, had suggested that the present itself possessed an extension that counted as real (at least for the purposes of scientific psychology). James was less convinced of that; instead, he argued that what we call the 'present' is in fact the retained residue of a rather recent past. Accordingly, he can be classed as having a 'retention theory' of the present, not unlike Brentano had offered earlier in the nineteenth century.

By turning to retention as the key to the specious present, James was in a sense arguing that memory, far from being relegated exclusively to the past, actually had a role to play in creating the present. In making this move, both James and Brentano were, whether they wanted to or not, putting themselves back into conversation with the ancient philosophers of time. In particular, they were reigniting a debate entered into most vociferously by the aforementioned Augustine of Hippo. He too, in works as varied as his *Confessions*, his *City of God*, and his *De Trinitate*, had ceaselessly argued for the centrality of memory to human time-consciousness and even to the constitution of a livable, human 'present.'

Since it would be impossible to fully appreciate the parallels between nineteenth-century psychology and fifth-century theology in one paper alone, the goal here will simply be to argue for the ongoing relevance of Augustine's account of memory in his *Confessions* to our understanding of the history of the psychology of time-consciousness. To throw that relevance into boldest relief, Augustine's account of time-consciousness will be contrasted against James' embrace of the specious present in the *Principles of Psychology*. As a result, we will see that, for the ancient African bishop, if not for the American social scientist, the elusiveness of the real present does still cause some problems for the human mind's attempt to organize itself in time.

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Kate Harper (Wilfrid Laurier University), Alexander Bain's *Mind and Body* (1872): An Underappreciated Contribution to Early Neuropsychology

Alexander Bain (1818-1904) is well known for founding the first psychological journal *Mind* in 1876, for his scholarly writings in education, rhetoric, and logic, and for his two-volume book, *The Senses and the Intellect* (1855) and *The Emotions and the Will* (1859). These texts went through numerous editions and became the standard psychology texts used in Britain and the English-speaking world for nearly fifty years, at least until William James (1842-1910) published *The Principles of Psychology* in 1890 (Robinson, 1977).

Additionally, Bain was among the first to provide a chapter on the functioning of the brain and nerves in these texts, and he has been called one of the first philosopher-psychologists who attempted to integrate neurophysiology and psychology (Young, 1968). In light of his influence in psychology, Bain has been studied from a biographical point of view (Flesher, 2000; Hearnshaw, 1964; Johnson, 1986) and from the perspective of his impact on British psychology, philosophy, and culture (Brett, 1921; Johnson, 1986; Mischel, 1966; Murphy,

1932; Rylance, 2000; Staley, 2004). Accounts and the impact of Bain's association theories have also been published (Boring, 1950; Cardno, 1956; Greenaway, 1976; Robinson, 1976; Shearer, 1974; Warren, 1921). Almost all of these references to Bain focus on his two influential textbooks (1855;1859). To date, however, there has been no thorough examination of Alexander Bain's 1872 *Mind and Body: The Theories of their Relation* from either a historical or theoretical standpoint. This paper attempts to remedy this.

In *Mind and Body*, Bain integrated associationism and neurophysiology as he theorized about emotion, thought, learning, memory, and conscious and unconscious processes. Bain's integration of two fields to systematically solve specific problems or answer particular questions about the mind-body relationship was a unique endeavor, particularly in light of the fact that it had been almost one hundred years since David Hartley had theorized that brain particles vibrated as sensory information entered the brain (1749). Bain's use of more recent neurological findings gave him a strong advantage over Hartley, which I argue allowed him to theorize more accurately about the connection between neurological structure and function.

Although *Mind and Body* was quite popular in its day, going through nine editions, numerous reprints, and several translations, it has received surprisingly little explicit historical attention, most often being referenced only in passing (Boring, 1950). Many histories of neural networks often begin with the associative writings of Aristotle and jump to the 1949 work of Donald O. Hebb (1904-1985) with little or no consideration given to Bain (Quinlan, 1991; Valentine, 1989).

 While these scholars in psychology have made note of *Mind and Body*, some historians of neuroscience have also recognized it. For example, Macmillan (2000a; 2000b; 2004) compared Bain's neurological theories to that of Freud and Ferrier in two seminal papers, and Finger (1994; 2000) briefly mentioned Bain's theory of memory stating, "The idea that synaptic growth could account for memory was anticipated in 1872 by Alexander Bain, David Ferrier's teacher in Scotland and a leading mental associationist. When Bain is mentioned in reference to the history of neuroscience, it is often in relationship to David Ferrier (1843-1928). Ferrier having been one of his students at the University of Aberdeen.

Wilkes and Wade (1997) appear to be the first to recognize Bain's *Mind and Body* as an early neural network model and detail particular aspects of this work. Their goal was to give Bain historical recognition by comparing his work to that of Hebb (1949). More specifically, they noted continuities between Bain's theory and Hebb's idea that neurons that fire together go on to be wired together. They stated, "Bain anticipated certain aspects of connectionist ideas that are normally attributed to 20th-century authors—most notably Hebb" (Wilkes and Wade, 1997, p. 295).

This paper expands on the previous shorter work of Wilkes and Wade (1997) by detailing Bain's neural network model and demonstrating just how Bain created an interdisciplinary model of the mind by integrating ideas from some of the most highly recognized philosophers and physiologists of his day. Secondly, this paper allows me to situate this work in the contexts of mental philosophy, psychology, and physiology and consider why *Mind and Body* did not stimulate further elaboration by him or receive the historical notice his earlier works attained (1856; 1859). Finally, this study, drawing primarily on Bain's text (1872) and his autobiography (1904), identifies Alexander Bain as the originator of the "stream of consciousness" theory, propagated by William James. It is safe to say that Bain was the

originator of the theory, while James, who cited Bain, popularized it. This point is expanded on in this paper as I reflect on why Bain's work (1872) has been overlooked.

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Robert Kugelmann (University of Dallas), "The Irrepressible Desire for Unity:" Edward A. Pace on Psychology and the Soul

Speaking in 1912 at the meeting of the American Psychological Association (APA), James R. Angell (1913) began with these remarks:

> Two years ago at the meeting of this Association we attended the obsequies of the term 'soul' as a member of the vocabulary of psychology. . . . It marked the passing of the problem with which the term was concerned. Religious and metaphysical thought will no doubt continue their interest in the soul. But for psychology in its technical aspects, the chapter is apparently closed. (p. 255)

Edward Aloysius Pace (1861-1938), a psychologist who did think that the soul had a place in psychology, might have nodded his head in agreement upon hearing Angell's words, if he attended that APA meeting. This presentation will focus on Pace's complex analysis of the place of the soul in psychology.

Like many of his contemporaries in psychology, Pace had a background in philosophy and theology, studying them in Rome in the early 1880s. Later in that decade, he pursued philosophy and biology at Louvain, and philosophy, physiology, and chemistry at the Sorbonne

(Braun, 1968). Becoming aware of Wilhelm Wundt's work while in Paris, Pace went to Leipzig to study psychology with Wundt and physiology with Karl Ludwig. "He was the first Catholic and the third American to study under Wundt" (Braun, 1968, p. 4), and he received a Ph.D. in 1891 under Wundt. That same year, he joined the faculty at the newly founded Catholic University of America, first as professor of psychology (1891-1894) and then as professor of philosophy (1894-1935). In 1892, the charter members of the APA elected him to membership. As his career progressed, he contributed less to psychology and more to philosophy and education.

Pace was one of the leading American Neoscholastic (or Thomistic) philosophers of the first half of the twentieth century. The philosophy of Thomas Aquinas had received an official renewal with Pope Leo XIII's (1879) encyclical, *Aeterni Patris*. Central to Leo's call to renew scholastic philosophy was that it should engage the modern sciences: "strengthen and complete the old [that is, scholasticism] by aid of the new [the natural sciences in particular]" (Section 24). Pace embraced this call.

Many Catholic thinkers saw the expulsion of the soul in psychology as symptomatic of a grave development in the modern world: The rise of naturalism. For Eugene Magevney, S.J. (1894), naturalism signified "the elimination of the supernatural, with all that it implies, and the substitution in its stead of a religion whose horizon is the tomb and whose purposes are not concerned with any but material and earthly interests" (1894, p. 234). The consequences of naturalism were dire, according to Magevney, who saw psychology implicated in this decay masquerading as progress: "Psychology is being fast resolved into physiology" and mental phenomena explained entirely in terms of "the nervous or muscular systems" (p. 236). Worse was yet to come, when this soul-less psychology will undermine "popular credulity," leading the average person to believe that "the forces at work within us are purely material" (p. 236). Magevney spoke for many in the Catholic Church at the time, and Pace would have had to address these anxieties. Indeed, many of his writing on psychology were in Catholic journals, so his efforts were directed to co-religionists.

Although a psychology without a soul was seen by many Catholics as an assault on Christian religion, Pace was not one of them. For him, empirical investigation was valuable in itself, needing, however, completion in philosophical terms. Introducing Catholic readers to experimental psychology, Pace (1894) defended its lack of interest in the soul: "'Psychology without a soul' is a phrase often misused to brand, and presumably crush, the audacious offspring before it is old enough to speak its defence" (p. 523). For Pace, "the science is still in its infancy" (p. 535). This new psychology neglects the soul as a result of specialization in research, which for Pace (1894) was "the main-spring of scientific progress" (p. 543).

Specialization was necessary to the sciences but that did not imply their isolation in the overarching structure of knowledge. With the Neoscholastics, it was often the same person who addressed empirical and philosophical psychology (e.g., Pace, 1898; Gruender, 1912; Moore, 1924). Empirical psychology had its autonomous domain, but this domain belonged to a larger kingdom.

This presentation will delineate Pace's conception of science, of scientific psychology, and the relationship of science to philosophy. In these conceptions, the soul has the central position, for although empirical research need not address the soul, the soul was essential for understanding the significance of empirical research, for understanding the foundations of empirical psychology, and for situating psychology in relation to philosophical and theological

knowledge. Above all, preserving the soul was seen as upholding a conception of the human as an *imago Dei*. Finally, there is an "irrepressible desire for unity" (Pace, 1986, p. 194) in our knowledge of reality, because there is a unity that underlies the natural world. Pace's vision of psychology represented an alternative to the dominant empiricist foundation in North American psychology.

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Verena Lehmbrock (Erfurt University), Psychology of the Kollektiv. GDR social psychologists between ideology, scientific internationalism, and society (1960s/70s)

My postdoctoral book project examines the relationships among social psychology, marxist-leninist ideology, and socialist reality in the German Democratic Republic (GDR). The story will be exemplified and expanded by a description of the scientific and private friendship between the two main protagonists.

The leading social psychologists of the GDR were Hans Hiebsch (1922-1990) and his 10 years younger co-worker Manfred Vorwerg (1933-1989). After Hiebsch had been appointed to the chair of psychology in 1962, Jena university became the national research and training centre for social psychologists. Hiebsch & Vorwerg created a Marxist position of social psychology which resulted in a textbook reprinted 10 times and translated into several languages. While the two already set the tone in the Eastern bloc they meanwhile had to face fierce ideological trouble at home as their conception was accused of not being truly marxist.

According to a socialist ideal of the K ollektiv (a particularly valuable form of the small group) for almost two decades small group research remained the main research topic in Jena. Triggered by an official mandate the scientists claimed that their research would help to enhance the output of (industrial) working collectives in order to contribute to socialist economy. Indeed, their invention of a training programme for working group leaders became successfully applied in GDR industries. Another strong involvement of state and academic interests was the cooperation of one of the protagonists with the GDR intelligence service. The history of Jena social psychology presents a fruitful case to analyse how psychological research has been shaped by political and ideological factors and how, in turn, protagonists used their psychological expertise to impact society.

Findings of my research suggest a complex relation between repression and innovation within GDR academic knowledge production. As Hiebsch & Vorwerg actively forged networks within both the western scientific community and among the former Comecon-countries the project also investigates the practices of scientific internationalism from the angle of the GDR during the height of geopolitical and ideological conflict between the two blocks.

Throughout the future book biographical traces will be followed. Hiebsch & Vorwerg were not only mentor and phd-student, prolific coworkers and ambitious global teamplayers but also close friends who spent their leisure time together and shared a summer house. My research suggests that their personal relationship was shaped and eventually destroyed by political and ideological aspects either. New private sources promise to explain why Hiebsch's & Vorwerg's friendship came to an tragic end in the 1970's. Sources: Universities of Jena and Leipzig/Germany, Section of Psychology archival records; Personal records of Hans Hiebsch, private (to be transferred to University of Jena Archives); Archive of the European Association of Experimental Social Psychology, Leuven/Belgium; Archives of the GDR Intelligence Service STASI; publications, interviews with historical witnesses.

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Jennifer Long (MacEwan University), Intercultural Competency and the Collection of Race-based Data in Small and Medium Sized Canadian Post-Secondary Institutions

In recent years, Canadian post-secondary institutions have committed to making progress toward equity, diversity and inclusion (EDI) initiatives meant to solve longstanding

issues related to low enrollment and lagging success rates of historically marginalized students. In 2017, for example, Canadian universities, including MacEwan University, committed to uphold seven "inclusive excellence" principles that sought to demonstrate real progress on EDI commitments over time (Universities Canada 2017). The Academic Women's Association at the University of Alberta have recently released 'disaggregated and intersectional' data according to federally-designated equity groups as identified by the *Employment Equity Act* that included data on gender, Indigenous peoples, and visible minorities. Their research sought to identify how members of these groups were represented among Canadian University's research, administrative, and faculty positions (AWA 2018). In line with findings from the recent *Equity Myth: Racialization and Indigeneity at Canadian Universities* and the *Underrepresented and Underpaid* report from the Canadian Association of University Teachers, the AWA study (2018) found that the academic workforce does not represent the growing diversity found within the student population and, that racialized and Indigenous faculty are significantly are underrepresented among academic faculty. Despite decades of attention to EDI initiatives, Canadian Universities continue to be largely lead and taught by faculty and administrators who are white (81%) and male (66%) (AWA 2016).

Interesting insights surface when exploring the history of EDI initiatives and data collection practices around the diversity of post-secondary faculty, staff and students within Canadian universities. First, a literature review highlighted the fact that many EDI initiatives focused on gender parity, a practice which most often benefits white women (AWA 2018), to the exclusion of other federally-designated equity groups. Second, researchers bemoaned the paucity of available research whether through a lack of data collection or, lack of available (public) data on inclusion initiatives on university campuses (see for example, Henry et al. 2017).

Much of the data that currently exists on EDI initiatives comes from large post-secondary institutions, such as the U15. Despite human rights advocates calling for the collection of race-based data – for the purposes of illuminating inequality and developing a better understanding of the needs of racialized groups – MacDonald and Ward (2017) recently found that 82% of universities did not collect racial demographics on their students. MacEwan University was one of those institutions that does not collect and make available data on students, staff, and faculty experiences of inclusion or other demographic information associated with the federally-designated equity groups.

Scholars have called for post-secondary institutions to develop their 'data infrastructure' as a means to identify inequalities and develop intersectional solutions that work to solve issues of exclusion facing historically marginalized students and faculty (see for example, López 2016). Therefore, knowing who our students are and how best we can teach them, affords insight into building inclusive curriculum and content delivery (Lee et. al, 2014; Lombardi, Murray, & Gerdes, 2011) as well as inclusive program and institutional management (Hockings, 2010). Further, collecting data on faculty, staff and students' experiences of inclusion and exclusion will aid institutional efforts to understand those existing conscious and unconscious biases (for example, in pedagogical practices) and aid us in identifying and addressing institutional barriers (for example, increasing retention and success rates of historically marginalized students and faculty) (Henry, Choi and Kobayashi, 2012). At a teaching-focused institution such as MacEwan, the collection of such information fits well within their vision and

strategic direction of the University; namely, to help build MacEwan's capacity and reputation as an inclusive community. Such data collection efforts would also fulfill MacEwan's obligations toward their Canadian Universities commitment for 2022. An online survey, regarding faculty, staff and students' identity and their experiences of inclusion and exclusion on campus, has been scheduled for the 2019 winter semester. This survey will be disseminated and shared through MacEwan's Institutional Analysis and Planning office and serve as a benchmark for future studies of this kind.

This paper will provide a brief overview of EDI data collection and engagement initiatives from Canadian universities over the past 10 years. In particular, this paper will explore EDI initiatives at medium and small-sized post-secondary institutions, like MacEwan University, in order to understand how, if at all, size impacts the collection of data around federally-designated equity groups and the implementation of EDI initiatives.

This historical overview of policies and practices will help contextualize the findings of MacEwan's recent climate surveys and add to a burgeoning body of research regarding EDI initiatives, intercultural competency work, and applied anthropology initiatives within post-secondary institutions.

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Kristine Peace and Eric Legge (MacEwan University), Fantastic "Memories" and Where to Find Them: Understanding False Memories from the DRM Paradigm to Implanted Experiences

Conceptualizations of memory have ranged from fixed reproductions of sensory experience (early Greek and Roman scholars) to malleable reconstructions of both experience and imagination (Danziger, 2008; Cubitt, 2007). Classically, memory has been viewed as a passive spatial imprint, despite those who argued that memory was an 'active power of the mind' (Plotinus, c. 244 AD/1962). Dissenting reconstructive views were not acknowledged by scholars until psychologists began to scientifically study memory distortions in the 1900s. From Binet's writings in *La Suggestibilité* (1900) and Stern and Von Liszt's (1901) reality experiments, it was increasingly recognized that memories were susceptible to change. For example, in Bartlett's classic work *Remembering* (1932), he found that memory morphed into schematic representations over repeated recalls. However, it wasn't until Deese's (1959) examination of patterns of recall that false memories were systematically tested and recorded using what became to be known as the Deese-Roediger-McDermott (DRM) paradigm (Deese, 1959; McDermott, 1996; Roediger & McDermott, 1995; Underwood, 1965).

Early methods of studying false memories tended to rely on recall tasks that consistently demonstrated false and distorted recall of thematically related target words (Gallo, 2010). While it was understood that some false memories could arise from these "normal" memory processes (i.e., forgetting items in a list), it wasn't until the 1980s and early 1990s that

psychologists began to study a categorically different type of "fantastical" false memory, based on imagination and implantation, that arose from claims of historical abuse (Loftus, 2005; Loftus & Pickrell, 1995). As a result, researchers developed methods of testing false memories of a dramatically different scope, including those that mimicked therapeutic techniques (i.e., guided imagery, hypnosis, bibliotherapy and journaling, and suggestive questioning) which had the potential to implant memories of experiences that never occurred (e.g., Ceci & Bruck, 1993; Clancy, McNally, & Schacter, 1999; Hyman & Pentland, 1996; Lindsay & Read, 1994; Loftus, Coan, & Pickrell, 1996; Lynn et al., 1989; Schachter, 1995). In the last two decades, empirical studies have been able to implant partial/complete false memories of emotional childhood events using photographs, narratives, and interviewing procedures (e.g., Garry & Wade, 2005; Lindsay, Hagen, Read, Wade, & Garry, 2004; Porter, Yuille, & Lehman, 1999; Strange, Gerrie, & Garry, 2005). As such, this historical analysis traces our understanding of the nature of false memories, as well as the methods used to scientifically test them. Further, we will examine the divergence of memory testing procedures for "normal" versus "fantastical" false memories.

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William Smythe (University of Regina), Some Personal Recollections of the Center for Advanced Study in Theoretical Psychology in its Later Years

In this paper I share some of my experiences working at the Center as a post-doctoral fellow from 1985 to 1992, a critical period of its development that includes the last five years of the Center's official existence as a funded institute. I offer some general impressions and reflections on the spirit and culture of the Center during this time as well as some more specific recollections of participating faculty, visiting speakers and topics of our weekly seminars. The themes of our collective deliberations were characteristically diverse and wide-ranging, including issues in the psychology of language, scientific explanation and lawfulness, epistemology of the discipline, the architecture of cognition, psychological aesthetics, and hermeneutic inquiry, to name a few. Although our discussions of these issues never resulted in any clear consensus or definitive conclusions, their impact on me as a young scholar was nonetheless profound. In retrospect, and consistent with the general orientation of this symposium, it is natural to conceive of the Center as an innovative but ultimately failed attempt at theoretical integration in psychology. Nonetheless, the Center had two great virtues as an intellectual institution. The first was to open up a space for critical dialogue concerning foundational issues in the discipline; the second was to provide avenues for interdisciplinary engagement and inquiry into a wide range of psychological topics. I conclude with some

remarks on the continuing importance of both critical dialogue and interdisciplinarity for a viable human science of psychology, with a particular focus on how both are now severely undermined by the institutional structures and economic pressures impacting the modern university and how critical inquiry in psychology nonetheless continues beyond these limitations.

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Danielle Stubbe (Vanderbilt University), George W. Stocking, Jr.'s Culture Concept: The History of Anthropology and Disciplinary Myth-Making, 1942-1975

"If I were to characterize certain aspects of your thought as neo-Boasian," wrote the historian-anthropologist George W. Stocking, Jr. in a 1971 memo to a colleague in the Department of Anthropology at the University of Chicago, "would you: a) know what I meant (i.e. what I was referring to in your work) [or] b) object violently?"[1]

This paper reconstructs the dynamic—and often-convoluted—approaches to and theories about *culture* offered by practitioners of cultural anthropology in the United States following Franz Boas's death in 1942. This is not a new project. Indeed, anthropologists by the early postwar era had begun to muse over the loose intellectual ties they shared with colleagues. But why had *culture*, which Boas's first student A. L. Kroeber and Harvard University's Clyde Kluckhohn described in their 1952 treatise on the subject as "one of the key notions of contemporary American thought," become so elusive?[2] Did twentieth-century anthropologists ever labor under what Stocking would later label the Boasian *culture concept* in his *Race, Culture, and Evolution* in 1968? And how did Stocking's interpretation become hegemonic among not just historians of anthropology but anthropologists themselves?

I use letters, lectures, essays, articles, and manuscript materials from midcentury anthropologists, both published and not, to weave together an intellectual narrative of two distinct approaches to cultural-anthropological work following Boas's death. The first engaged in projects about so-called personality within national culture groups. As Peter Mandler has written recently about Margaret Mead and her colleagues in Manhattan, these cultural anthropologists often worked as state agents for operations during the Second World War. Their vision of culture further permeated the national attitudes of the United States in the postwar era, Susan Hegeman and John S. Gilkeson have since argued. Intellectual proponents of what Jamie Cohen-Cole calls the open mind adopted it in order to portray themselves as modern. And it led, in part, toward a fostering of new institutions founded to patronize American cultural achievement like the National Endowment for the Humanities in 1965. The

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[1] George Stocking to David Schneider (June 11, 1971), David Schneider Papers, Box 31, Folder 7, Special Collections Research Center, University of Chicago Library.

[2] A. L. Kroeber and Clyde Kluckhohn, *Culture: A Critical Review of Concepts and Definitions*, Harvard University Peabody Museum of Archaeology & Ethnology, Papers (1952), 3.

second approach engaged with culture groups on a smaller scale. Academic anthropological channels continued to value fieldwork meant to preserve the customs of indigenous groups in the United States and abroad. In fact, short-term employment for non-tenured cultural anthropologists, often female, also emerged outside of academia in municipal governments of the 1950s and 60s. They conducted small population studies throughout the country, considered the behavior of groups like Spanish-speaking indigenous people in the Southwest, and advised on localized welfare systems.

Both approaches offered limited opportunity to theorize about the nature of cultural work in anthropology, however. This paper suggests that it would not be until the 1960s and 70s, when anthropologists began to grapple with the colonial legacies of their research abroad and thereby took interest in the history of their discipline, that a singular vision of cultural anthropological work became necessary. From the archives of Stocking's colleagues, I demonstrate that his *culture concept*—rather than, for example, the neo-evolutionary vision of the University of Michigan's Leslie White—spread because of his academic positioning. Stocking presented his *culture concept* at his doctoral advisor Pete Hallowell's original conference on History of Anthropology in 1962. His locality to the Boas archive at the American Philosophical Society during his graduate years at the University of Pennsylvania allowed him first-access to Boas's papers, and therefore centered Boas in his narrative. And his tenured role between history and anthropology at Chicago beginning in 1968 offered him influence over colleagues from Sol Tax to William H. Sewell, Jr. during the late-twentieth century.

Further, Stocking's *culture concept* allowed his contemporaries to eschew the historical racial implications of their work throughout the twentieth century. The concept thereby offered resonance with the social movements and cultural politics of the 1960s. It undergirded Tax's engagement with American Indian leaders in Chicago, and it connected Melville Herskovits's *The Myth of the Negro Past* to many contemporary black activists and organizers from the Black Panther Party. Here this paper is indebted to recent writing from scholars of indigenous history on the origins of anthropological theory. As the contributors to Ned Blackhawk and Isaiah Wilner's *Indigenous Visions* demonstrated last year, the seemingly groundbreaking work of many anthropological actors often borrowed their theories about culture, behavior, and the universe from their indigenous subjects.

For Stocking's *culture concept*, I argue, the language of cultural politics in the 1960s was critical. His emphasis on the significance of cultural rather than racialized groups to anthropological work, which he attributed to Boas's turn-of-the-century writing, provided anthropologists both a coherent narrative for their own twentieth-century history and a contemporary project with their subjects in the present. Indeed, to Stocking, a cultural anthropologist in the 1970s should not have objected to being called a neo-Boasian: His *culture concept* had provided the validating disciplinary narrative.

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Gerald Sullivan (Collin College), From Fear to a Steady State: Gregory Bateson's Narratives of Bali as an Anticipation of the Double-bind

Between 1937 and 1949, Gregory Bateson, the Anglo-American polymath, published a "New Myth" (1937), *Balinese Character* (1942 with Margaret Mead), "Form and Function of the Dance in Bali" (1944 with Claire Holt) and "Bali: The Value System of a Steady State" (1949). Each of these pieces, in one way or another, addresses matters related to the character of the Balinese, i.e. the organization of their habits, the ethos of their society, and the organization of permissible emotion. These pieces, however, also witness a shift from an ethnographic examination of matters described psychologically to a highly schematic and second-order cybernetic exposition couched in the terms of Morgenstern and von Neumann's game theory. This change in theoretical orientation accompanies a shift in what counts narratively as an adequate explanation. This paper will examine this process of development by examining not only Bateson's published work but also pertinent portions of Bateson's papers and fieldnotes.

"An Old Temple and a New Myth" (Bateson 1937), initially presented at a conference then published, recounts events around Batson and Mead's primary fieldsite, the highland

community of Bayung Gede. The *pamangku* (village priest) from the nearby community of Katung arrived in Bayung Gede telling of trances during which Katung's people received instructions to reestablish their relations with the spirit who descends upon (*bhatara*) the main temple of Bayung Gede. A series of ceremonial trances followed leading to the further reestablishment of an old temple within a *banua* (ritual domain; see Reuter 2002a & b) centered on Bayung Gede. This reestablishment of ceremonial relations did not lead to new stories concerning relations joining communities. Bateson concluded that Balinese do not see worldly change as an ideal. Rather, Bateson suggested that Balinese prefer following known patterns as a means of avoiding fear. This essay and the fieldnotes are Bateson's only work on Bali not influenced by cybernetics.

Balinese Character (Bateson and Mead 1942) used a combination of still photographs and texts of several sorts to illustrate the processes by which ordinary Balinese as they lived their daily lives came to embody Balinese culture. This work relied extensively upon a notion of deutero-learning, or learning to learn, developed by Bateson after he and Mead returned to New York from Bali (see Bateson 1942). Bateson's notion owed debts to both Kurt Lewin and I. G. Pavlov, but it also grew out of his encounter with the idea of feedback. The volume constitutes an initial and not very developed statement of second order cybernetics, in which organisms interact with and learn from their environments, with inversely those environments being understood as the contexts of the organisms' lives together.

The article on dance returns to the notions of embodiment and bodily activity developed in *Balinese Character* as presented initially to a seminar through film and then written up. But Bateson's emphasis moved from fear, as noted in both the proceeding works, to the apparent emotionlessness of Balinese dancers.

The final essay appeared in a volume of essays presented to A. R. Radcliffe-Brown provided by scholars affiliated with institutions in various parts of the English-speaking world. Bateson sought to explain why he and Mead had not found sequences of events in which emotions intensified towards an explosive climax, so-called schismogenic sequences. Inverting the structure of a zero-sum game, Bateson postulated that Balinese social organization tended away from maximizing any single variable over others. Further, elements of Balinese life emphasized avoidance of possible conflicts rather than fear as such.

Bateson's vision of Balinese society as a self-organizing steady state anticipated his later idea of a double-bind constituted in the communication of a paradox which inhibits emotional climax. His thought moved from understanding Balinese as resistant to change and desirous of known behavioral pattern, to a second-order cybernetic circumstance with inversions of zero-sum games resulting in conflict avoidance. This paper will trace this change in Bateson's thought by looking closely at how Bateson's narratives in these four pieces changed.

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Jessie Swanek, Courtney Krentz & Kristine Peace (MacEwan University), The "Unscrupulous:" Historical and Modern Conceptualizations of Psychopathy from Deviant to Functional

Writings from Ancient Greece were some of the first known sources to characterize groups of individuals as "unscrupulous;" reflected by behaviours such as shamelessness, self-seeking affability, and being wilfully disreputable. In general, it was thought that these unscrupulous persons suffered from deficiencies in their moral characters (Theophrastus, *Characters*; translated by Edmonds, 1929). Interestingly, this early description of the psychopath did not solely identify them as criminal men, but recognized some of the functional components of this disorder (i.e., "he is apt, also, to become an inn-keeper or a tax-farmer"). From these ancient descriptions to the influence of spiritualism, any form of deviance and criminality became associated with demonic possession (Wesselmann & Graziano, 2005). While ideas of morality were heavily shaped by religious standards of the time, it wasn't until the early 1800s that psychopathy became increasingly linked to mental illness rather than the reflection of evil. Preliminary descriptions of psychopaths by medical professionals and psychiatrists (i.e., Pinel, 1801; Rush, 1812; Prichard, 1835) tended to classify these individuals as having a 'syndrome' of criminality, encompassed by terms such as *manie sans delire* (mania without delirium), moral derangement, and moral insanity (see Hervé, 2012). All reflected the notion that psychopaths were impaired in emotional, ethical, and moral functioning but yet their intelligence and minds remained intact (Millon et al., 1998; Rafter, 1997). Building on the idea of the disordered psychopath, scholars such as Koch (1888), Krafft-Ebing (1879), and Kraepelin (1907) all proposed that psychopathy was a biologically based psychiatric problem, associated with changes in one's brain and thus organic causes for this form of 'moral insanity' (see Hervé, 2012). The early 1900s brought about a reconceptualization of the psychopath as a wide range of deviancy, and focused its roots back in a tradition of clinical disorder, mental deficiency and insanity (i.e., Healy, Glueck, and Spaulding; see Kiehl & Hoffman, 2011; Lilienfeld

et al., 2015). The functional aspects of this disorder were buried in views of the psychopath as a delinquent who could not help nor constrain their criminal behaviour (e.g., Healy, 1915; Rafter, 1997).

It wasn't until Cleckley's book *The Mask of Sanity* (1941) that the modern psychopath was defined according to his clinical and functional characteristics. Stemming from this, Robert Hare began his life's work (late 1960s) dedicated to accurately identifying and diagnosing psychopathy as a distinct disorder (Arrigo & Shipley, 2001; Hervé, 2012; Millon et al., 1998). From clinical and empirically rooted conceptualizations to the 'successful psychopath', Hare and many of his successors have continued to evaluate the role of psychopathy in terms of criminal behaviour and everyday functioning (Patrick, 2018). Several scholars actively promote the view that the psychopathic personality (i.e., traits without criminal behaviour) may be evolutionarily adaptive and allow for better life success (see Dutton, 2012). As a result, history seems to have come full circle in recognizing that the 'unscrupulous' psychopath may be both criminally and socially adept, with traits that can lead to high functioning behaviours. (Word Count 491)

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Alan C. Tjeltveit (Muhlenberg College), Interpreting the Boulder Conference III: Changing Normative Visions of the Science-Practice Relationship in Clinical Psychology

Participants engaged in the ongoing debates about the optimal relationship between psychological science and psychological applications often make historical appeals to two key

1940s documents: (a) The Committee on Training in Clinical Psychology's (1947) "Recommended graduate training program in clinical psychology" (often referred to as the Shakow Report), and (b) Raimy's (1950) *Training in Clinical Psychology*, which reported on the Shakow-dominated 1949 Conference on Graduate Education in Clinical Psychology at Boulder, Colorado (often referred to as the Boulder Conference). That clinical psychologists should be trained as scientists and as practitioners was a key recommendation of both.

Some assert that, in the Boulder Conference reports, psychology *promised* that clinical practice would be based on science alone (Hayes, 1989), that professional psychology *should* be based on science alone, or that the conferees advocated for the *integration* of science and practice (Belar, 2000; LeJeune & Luoma, 2015; Overholser, 2007. The historical reasons for an emphasis on science in clinical psychology have been well-documented (Baker & Benjamin, 2000; Benjamin & Baker, 2000, 2003; Capshew, 1999; Farreras, 2001, 2005, 2016; Farreras, Routh, & Cautin, 2016; Frank, 1984; Routh, 1994; Shakow, 1965, 1969, 1976, 1978). However, the Shakow Report, adopted by attendees at the Boulder Conference, advocated for a much broader epistemic framework for clinical psychologists than science alone: the ideal background for psychologists including undergraduate coursework in the humanities, including history and "psychology as revealed in literature" (Committee on Training in Clinical Psychology, 1947, p. 542). Conferees also recommended that graduate coursework include cultural anthropology so clinical psychologists could understand the "influence of culture on personality" (p. 550). This emphasis on training in history, other disciplines from the humanities, and culture disappeared in most later accounts of "the Boulder Model."

This paper is a progress report in an ongoing line of research investigating historical factors that have led to divergent interpretations of the Shakow Report and Boulder Conference. In past presentations, I examined all sources that cited Raimy (1950) between 1951 and 1955 (inclusive) and found that, with one exception[3], most stayed close to what was reported in that book. However, I also discussed what I documented more thoroughly in a second paper: The now familiar way of thinking about and interpreting the Boulder Conference—its advocacy for a particular *model* of training, of the clinical psychologist, or of both—did not, in fact, date from the conference, but from nine years later, when Cook (1958) introduced the term "models" into the discussion, doing so in a way that addressed several issues the Boulder conferees didn't address, based in part on the experience of the new Ph.D. clinical programs attempting to follow the Boulder recommendations. The now-ubiquitous phrase "Boulder Model" was not used until 1963 (Jones & Levine, 1963, p. 220), 14 years after the Boulder Conference, with a phrase often used as a synonym for that phrase, "scientist–practitioner model," not appearing until 1964 (Basowitz & Speisman, 1964, p. 48).

In this presentation, I will focus on interpretations of the Conference in published citations to Raimy (1950) in the years between 1956 and 1967, the year after the crucial Chicago Conference on the Professional Preparation of Clinical Psychologists (Hoch, Ross, & Winder, 1966) and the year in which the controversial Clark Committee (formally, APA's Committee on the Scientific and Professional Aims of Psychology, 1965, 1967) gave its final report, as some participants advocated for openness to alternative approaches to training clinical psychologists (Crawford, 1992; Peterson, 1992; Stricker & Cummings, 1992). My

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[3] Merrill's (1951) interpretation of the Boulder Conference hinted at later divergent interpretations.

rationale for stopping with 1967 is my working hypothesis that, by then, model language became solidified, and with it interpretations of the Boulder Conference stabilized somewhat.

I will also explore the role that the expectations and requirements that came from a major funder of the post-war explosion of clinical psychology—The Veterans Administration—and from the APA accreditation process, played in shaping both the relationship of science and practice in clinical psychology and psychologists' interpretation of the Boulder Conference.

Both with regard to the sources citing the Boulder Conference and evidence concerning the VA and APA accreditation, I will pay particular note to the language used and arguments employed, especially developments in the moral language used concerning the ideal, proper, appropriate, desirable, and/or obligatory relationship between science and practice.

In these ways I will explore some ways in which the Boulder Model and Shakow Report have been used and interpreted in service of competing visions of clinical psychology and of the discipline of psychology.

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[4] Submitted by Ernest R. Hilgard, E. Lowell Kelly, Bertha Luckey, R. Nevitt Sanford, Laurance F. Shaffer, & David Shakow, Chairman. Often referred to as the Shakow Report.

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Leila Zenderland (California State University Fullerton), From Propaganda to Psychological Warfare: The Changing Terminology of Interwar Interdisciplinary Social Science

This paper will explore a change in terminology adopted by American social scientists between the first and second world wars. Specifically, it will focus on the transition from studying "propaganda," the term largely employed during and after the First World War, to the much broader concept of engaging in "psychological warfare," a phrase popularized during the Second World War. It will show how new institutions promoting interdisciplinary cooperation and cross-fertilization among social scientists, established during the interwar decades, helped to shape this transition. Among these were the creation of the Social Science Research Council in 1923, which first brought together seven disciplines; the series of interdisciplinary conferences it sponsored in Hanover, New Hampshire, in the years between 1925 and 1934; the international Seminar on the Impact of Culture on Personality run by anthropologist Edward Sapir and social psychologist John Dollard in 1932-33, which incorporated work by more than 40 social scientists from multiple disciplines; and the interdisciplinary syntheses attempted by researchers at Yale's Institute of Human Relations, established in 1931. By the time the U.S. entered the Second World War, these collaborative endeavors had effectively broadened the ways that social scientists understood what they were doing—a fact evident in the gradual adoption of new language.

This paper will explore this transition from studying "propaganda" to analyzing "psychological warfare" by focusing on three seminal texts. The first is University of Chicago political scientist Harold Lasswell's influential 1927 study, *Propaganda Technique in the World War*. Lasswell explained his indebtedness to political scientist Charles Merriam, founder of the Social Science Research Council; equally influential, however, were the ideas of two close friends, linguistic anthropologist Edward Sapir and interactionist psychiatrist Harry Stack Sullivan, whom Lasswell regularly met at the SSRC's annual conferences in Hanover. As defined by Lasswell, "propaganda" involved attempts to control opinion by manipulating "significant symbols, … stories, rumours, reports, pictures, and other forms of social communication." More broadly, his study combined political, linguistic, and psychoanalytic concepts in analyzing the emotionally charged ways that states "spoke"—both to themselves and about their enemies—during wartime.

A second key text is Leonard Doob's 1935 study, *Propaganda: Its Psychology and Technique,* largely inspired by his experiences as a psychology student in Germany between 1930 and 1932. By 1935, Doob was employed at Yale's Institute of Human Relations, where he worked closely with social psychologist John Dollard, one of the former leaders of Yale's interdisciplinary Seminar on the Impact of Culture on Personality. Doob emphasized efforts to

control "the attitudes and, consequently, the actions of groups of individuals through the use of suggestion." Although he named his study *Propaganda*, he in fact focused on five subjects that Joseph Goebbels had identified as under his control through Germany's "Ministry of Public Enlightenment and Propaganda"—that is, on "propaganda, radio, press, motion pictures, and the theatre." Linking psychological theories with sociological structures, Doob's work anticipated the new interdisciplinary field that came to be called Communications Research.

The third text suggests an even broader interdisciplinary synthesis; it was also the first in which social scientists called what they were studying "psychological warfare." Published in 1941, *German Psychological Warfare* was produced by the American Committee on National Morale, an organization that brought together "some 100 outstanding specialists in the different relevant fields: psychologists, sociologists, political scientists, educators, historians, physicians, psychologists, psychiatrists, propaganda and public opinion analysts, publicists, radio and motion picture experts, students of foreign affairs, military scientists, social workers, national economists, etc." "The title of this book," its opening line stated, "is used in the broadest possible sense of a phrase which is becoming increasingly 'popular.'" Edited by researcher Ladislas Farago (who later became an intelligence officer), it included an annotated bibliography of 561 German war-related publications. Its organization, however, differed from Doob's, since instead of focusing on media, it was divided into three sections: propaganda techniques, officer selection methods, and *Völkerpsychologie*—a German discipline that these authors translated as studies of "comparative national character." "The present war is, in large measure, ideological," this volume declared. Prior to military rearmament, Germany had begun the "psychological rearmament" of its people; as a result, all aspects of Nazism now merited psychological attention. Also crucial were new interdisciplinary theories linking culture to personality. The Nazi cultural system was trying to "construct a personality that revolves around violence, aggression, and a doctrine of racial superiority," stated one of its contributors, social psychologist Kimball Young. He then addressed what Americans would have to do to counter such new weapons of war.

In exploring these seminal texts, produced in 1927, 1935, and 1941, this paper will consider how propaganda research became a part of a much broader wartime enterprise. It will also show how new interdisciplinary syntheses led to new understandings of the nature of contemporary warfare.

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