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BOOK OF ABSTRACTS



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ABSTRACTS

Examining a decisive moment in feminist research on gender in 1970s American psychology **Ann Johnson and Elizabeth Johnston (University of St. Thomas)**

Stephanie Shields has proposed that we gain valuable insight into the history of feminist psychology when we interrogate “particular moments – specific publications or situations in which an individual or group sparked advancement of the field” (Shields, 2015, p. 143). Our study is shaped with that intention. An important hub of gender-focused research during the 1970s was Stanford University, where Eleanor Maccoby and Sandra Bem held faculty positions in the psychology department and Carol Nagy Jacklin worked with Maccoby as a post-doctoral fellow. These three published two groundbreaking studies, both appearing in 1974: Bem produced her gender-destabilizing classic, “The Measurement of Psychological Androgyny” (Bem, 1974) and Maccoby and Nagy Jacklin launched their critical analysis of the existing literature on gender difference, *The Psychology of Sex Differences* (Maccoby & Jacklin, 1974).

An interesting aspect of the geographic overlapping of these three women psychologists is its intergenerational nature. Eleanor Maccoby was not included in the cohort of second generation women examined by Johnston and Johnson (2008); her 1950 PhD completion date placed her outside the 1945 cutoff date for that analysis. But she was born in 1917 and shares many of the characteristics of the second generation cohort; Maccoby had a long career, actively participating in her discipline until her death in 2018. Bem and Nagy Jacklin entered the field later with PhDs earned in 1968 and 1972, respectively.

Analysis of oral history interviews and memoirs produced by these three women psychologists, and by others whose lives and careers intersected with them, reveals fascinating insight into their motivations for doing gender-based work and their beliefs regarding the role of research in the context of an emerging feminist agenda in psychology. As historical work on the origins of feminist psychology demonstrate, progress occurred through the forging of productive collaborations and often cross-disciplinary connections, and was also marked by moments of productive friction, as women scientists disagreed at times about methods and explanatory frameworks for moving their new field forward.

At times, their disagreements mirrored generational conflicts we have uncovered in prior research. Second generation women psychologists typically did not question their commitments to traditional positivist and empiricist research approaches while women coming after them actively challenged that tradition, proposing a number of feminist “postpositivist” alternatives (see Eagly & Riger, 2014; Rutherford & Pettit, 2015). Another focus of generational friction was the desired pace of change and need for feminist challenging of the status quo for women in psychology. At a number of moments in psychology’s history younger feminist women have agitated for confrontation while the older generation counseled caution and a gradualist approach to change – “evolution is generally better than revolution,” in the words of second-generation child psychologist Florence Goodenough (see Johnson & Johnston, 2010, p. 319).

For this presentation we will report on the initial phase of our project which focuses on examination of oral history records and memoirs produced by Bem, Nagy Jacklin, and Maccoby, as well as others whose lives and careers intersected during those years. Their pivotal interactions offer a concrete way to examine their emerging ideas. For the three women we focus on, we will attempt to trace how those interactions throw light on their later published work and career trajectories. In doing so, we hope to contribute to the project of finding and sharing “unsung transformational moments” in the history of feminist psychology (Shields, 2015, p. 143).

The “progressive” mother: Leta S. Hollingworth and feminist eugenics **Michael Stead (York University)**

In 1915, readers of *Forecast: A Magazine of Home Efficiency* could read the lyrics to *A Modern Lullaby*. Sung to the tune of “Rock-A-Bye, Baby”, parents could learn lines like, “Before your eugenic young parents were wed, they had decided how you should be fed” and “Mamma's scientific she knows all the laws, she kisses her darling through carbolized gauze” (Apple, 2006). Unsettling by today’s nursery rhyme standards, these lyrics highlight the pervasiveness of eugenics in early 20th century America, during what is now referred to as the Progressive era.

Broadly defined, the Progressive era lasted from the late 1890s to the early 1920s, with the United States undergoing immense socio-political change to address perceived corruption in public life and relieve social and economic distress, often through what were considered “progressive” reforms (Kennedy, 1975; Rothbard, 2017). Reforms driven by what Robert Wiebe (1967) describes as a new middle-class consciousness that organized itself into the strong and weak. As these lyrics indicate, this era also saw the birth and development of scientific motherhood, which invoked expertise from the new scientific psychology. Both unfolded concurrently with the cresting of first wave-feminism in the United States. In the historiography on women in psychology that began to appear in the 1970s, one of the first women – and feminists – to be recovered from this period was Leta Stetter Hollingworth. Perhaps not surprisingly, given the celebratory aims of this recovery project, the role of eugenics in the lives and beliefs of these early women pioneers has gone relatively unexplored. It is to this more unsettling, and even troubling, aspect of this historiography that this project attends. Scholars who have explored the eugenic ideology of Leta Hollingworth have established that a tension must have existed within her, citing the seemingly contradictory beliefs she held concerning women’s right to choose whether to have children, and woman’s role in saving the race (Kasper, 2003; Klein, 2002). Exploring Hollingworth’s various publications, personal correspondence, and academic relationships, this project argues that Hollingworth’s seemingly tense ideological intersection was instead a *cohesive* ideology known as feminist eugenics. A common thread amongst feminist eugenicists was that economic, sexual, social, and political equality for women was the key to preventing the degeneration of the race (Ziegler, 2008). This historical project was accomplished using the physical and digital archives graciously provided by the *Cummings Centre for the History of Psychology*.

In addition to addressing and filling a historical gap, the proposed research will speak to an uncomfortable issue in contemporary biotechnological debate: that seemingly contradictory views like eugenics and feminism can coexist within the work of, by all accounts, “progressive” thinkers. Based on the projects findings, the proposed paper could serve as a cautionary tale, warning 21st-century advocates of developing biotechnologies – namely, assisted reproductive technologies (ART) - that views placed under the “progressive” banner of feminism are not necessarily immune to the influence of eugenic policy and that the rhetoric of “progress” should always be interrogated. Progressive for whom? And at whose expense?

Untold Lives of a Third-Generation Woman in Psychology: Evalyn F. Segal (1932-2017) **Ed Morris (University of Kansas)**

Evalyn F. Segal (1932-2017) was among a third generation of American women psychologists (cf. Johnston & Johnson, 2008; Scarborough & Furumoto, 1987) and was arguably that generation’s most eminent woman in the field of behavior analysis (i.e., the discipline and practice of B. F. Skinner’s science and system; see Michael, 1984). In that field, Segal was trained in its basic science -- the experimental analysis of behavior (e.g., Segal, 1978; see Honig, 1966) – and published extensively in its most prestigious journals (e.g., on schedules of reinforcement, adjunctive behavior; e.g., Segal, 1972). In the 1970s, she was the second woman appointed to the Board of Editors of the field’s first journal for basic

science – the *Journal of the Experimental Analysis of Behavior (JEAB)*, 1958- present) -- and was the first to serve two terms. In the 1980s, she was the second woman to serve as its Associate Editor. In 2021, she was the first inductee into the Hall of Fame in the organization: Women in Behavior Analysis.

This was Eve Segal's life, but she had other lives, too – untold lives. She described these in an unpublished 1982 invited address to the American Psychological Association's (APA) Division 25 for the Experimental Analysis of Behavior that was co-sponsored by APA Division 9 the Society for the Psychological Study of Social Issues. It was titled "Women, Borscht, Gays, Laboratory Animals, and Nuclear Extinction" (Segal, 1982). Drawing from a copy she gave me, I consider her lives in the 1950s and 1960s.

First, Segal's presentation and title. For an invited APA Division 25 presentation, they were unconventional. Her colleagues advised her to speak about her science. Instead, she spoke about her field's science as the behavior of its scientists, some of it unseemly. More specifically, she spoke, in part, about "how it's been to be a gay woman behaviorist..." Some colleagues boycotted her address.

Second, Segal's education. Formally, it included a bachelor's degree in psychology from the University of Minnesota and graduate studies at Radcliffe/Harvard with Skinner in the 1950s. Informally, it included sexism and sexual assault at Harvard. In subsequent psychotherapy at Radcliff, she confided about being gay, was diagnosed as psychopathic, and was expelled. This is consistent with other accounts of Harvard's hostile environment for female psychology students.

Third, Segal's early career. She completed her doctorate at Minnesota with Kenneth MacCorquodale (Segal, 1958) and secured a tenure-track faculty position at what would become San Diego State University, with several stints elsewhere (e.g., University of North Carolina-Greensboro). During the 1960s, she struggled with her public identity – and shunned being gay -- even as the women's movement was taking hold in psychology. She resolved these struggles.

Segal had other lives as well, for instance, in complementarities between Chomsky and Skinner (Segal, 1977); animal welfare, even as she was an animal researcher (Segal, 1989); and protesting nuclear extinction (Segal, 1982). I consider these more briefly. In summary, Segal's untold lives in the 1950s and 1960s illustrate cultural, social, and individual barriers faced by a third generation of American women psychologists. I conclude by considering how they relate to those faced by the first and second generations of these psychologists.

Interwar American psychiatry and the rise of therapeutic self-help literature

Matthew McLaughlin (University of Toronto)

This presentation examines the production, development, and dissemination of self-help literature written by two psychiatrists in interwar America. By the interwar period, Americans were living in a new urban-industrial society that had transformed their everyday experiences in the private and public spheres. Overwhelmed by these changes, people sought guidance on how to navigate life in the 20th century. American psychiatrists promptly responded to these demands, and the discipline professionalized so that everyday thoughts, emotions, and behaviors became psychiatric concepts (McLaughlin, 2023; Starker, 1989). As the discipline broadened its professional domain to include all aspects of human behavior and mental life, psychiatrists sought various ways to engage with the public beyond the mental hospital (Lunbeck, 1994; Pols, 2010). An analysis of the books and articles written by the psychiatrists Abraham Myerson and Karl Menninger, published from 1920 through 1937, will illuminate how these psychiatrists affirmed their professional legitimacy beyond the mental hospital and framed everyday problems as subjects worthy of psychiatric intervention.

Examining the self-help literature published by Myerson (1920, 1925) and Menninger (1930, 1937) will reveal how they translated expert theories and knowledge for non-expert readers while instructing them on how to prevent and treat mental disorders. These texts reveal what thoughts, emotions, behaviors, and everyday experiences these psychiatrists perceived as psychiatric and how they informed non-expert readers to understand and care for themselves accordingly. Additionally, comparing the

psychiatric knowledge and corresponding advice offered by Myerson and Menninger will provide insight into the variegated state of American psychiatry at the time. Myerson remained committed to a somatic approach to psychiatry throughout his career while Menninger enthusiastically embraced psychoanalysis. Identifying the similarities and differences between their self-help texts will underline the diverse approaches that characterized interwar American psychiatry while drawing attention to their common goal of legitimating psychiatric authority over the everyday. In doing so, this presentation will illustrate that the self-help literature produced by Myerson and Menninger demonstrates how psychiatrists expanded their professional role beyond the mental hospital and into the community. Such an analysis will illuminate how these two legitimated personal and everyday concerns as ones requiring psychiatric attention and intervention.

Furthermore, to trace the development and dissemination of these self-help texts, this presentation evaluates the response of non-expert readers to the knowledge, recommendations, and professional claims to authority of the everyday made by these psychiatrists. Examining correspondence with the authors and non-expert reviews in popular periodicals will reveal the reaction of non-expert readers to these therapeutic self-help texts. This will establish what aspects of the literature were accepted, adapted, and rejected by readers. Uncovering how the average reader responded to the claims of two psychiatrists that their everyday concerns, thoughts, emotions, and behaviors were best understood and addressed through psychiatry will provide necessary insight for understanding the professionalization and expansion of psychiatry in the United States at this time.

Analysis in Extremis: The Crisis of the Ego and the Politics of Analytic Therapy Between the Wars **Phillip Henry (University of Chicago)**

In the aftermath of the Great War, Freudian psychoanalysis underwent profound transformation as both a psychological theory and a therapeutic practice. Once primarily a science of the repressed unconscious, psychoanalytic thought came to revolve around the ego, the mental agency responsible for psychical defense and social adaptation. At the same time, the standard model of analytic therapy that Sigmund Freud had outlined in his prewar case studies and technical papers would be supplemented and partially supplanted by an array of new therapeutic techniques. This paper explores the relationship between these two developments. It argues that the turn to the ego plunged the practice of analytic therapy into crisis, all but demanding a rethinking of its basic principles and procedures. Through the many-sided investigation of the ego, interwar psychoanalysis would see the emergence of a new idea of the subject, and a new subject required a new analytic therapy.

What accounts for this sudden explosion of new theoretical perspectives and therapeutic methods in the wake of the Great War? While recent decades have seen the publication of many fine historical studies of interwar and midcentury Freudianism, to adequately understand the remaking of psychoanalysis between the wars, this project returns to an older vein of scholarship that emerged from the work of Carl E. Schorske. As Schorske famously argued in *Fin-de-siècle Vienna: Politics and Culture*, Freud's decision to embark on a self-analysis in the mid-1890s – the experience from which the foundational discoveries of psychoanalysis would emerge – was motivated by a political crisis that dramatically undermined the fundamental assumptions of the liberal political tradition in which he had been raised and with which he identified. The arrival of the Great War two decades later, however, would prove incomparably more destructive. While the *fin-de-siècle* crisis had destroyed liberal political power in Habsburg Vienna, the war destroyed an entire European liberal bourgeois civilization. Born amid one crisis of liberalism, psychoanalysis would be reborn in the aftermath of another, far graver crisis that transformed the world in which Freud and his followers worked.

By plunging psychoanalysts into a far more uncertain and insecure world from the one they had known, total war undermined the psychological verities and sociocultural assumptions that had previously underpinned the practice of analytic therapy. Before the war, analytic therapy had presupposed a patient

possessed of certain psychical attributes, ones that Freud identified with a relatively strong, experienced, reflective ego. In the conditions of generalized insecurity created by the war, however, such attributes, and thus such patients, appeared increasingly rare. The psychoanalytic study of the ego, the single most important development within interwar Freudianism, would further undermine the idea of a secure, durable, mature ego. Once the taken-for-granted seat of normalcy within the psyche, the ego thus increasingly figured as a site of crisis. This paper argues that the crisis of the ego was inseparable from a crisis of analytic practice: absent its essential psychological presupposition, the standard (classical) practice of analytic therapy came to many Freudians to appear detached from the clinical challenges they faced and in need of far-reaching revision. After surveying the impact of the war and its significance for the Freudian subject, this paper considers how the crisis of the ego ushered in a crisis of the classical – *liberal* – politics of analytic therapy.

From “Anaclitic Object-Choice” to “Attachment Security”: The Development of a Psychological Concept in Late Modernity

Bican Polat (NYU Shanghai)

Attachment theory stands as one of psychology's most cited theories, offering explanatory concepts for studying parent-child relationships. The theory was formulated by John Bowlby between 1958 and 1969. Equally significant to the formulation of the theory was the research program pioneered by Bowlby's colleague, Mary Ainsworth, at Johns Hopkins University during the 1970s. Ainsworth designed a novel observational procedure, known as the Strange Situation, which facilitated the categorization of individual differences in parent-child relationships along a continuum from "secure" to "insecure" types, formalizing the notion of "attachment security." Building on this exemplar (Kuhn 1970), Ainsworth's successors integrated attachment theory into a statistical framework, enabling correlations between its constructs and other quantifiable indicators of psychological well-being. In this talk, I track the development of Bowlby's concept of attachment from its origins in psychoanalytic thought to its reconstruction within Ainsworth's research program. Through textual and contextual analysis, I show how the formalization of "attachment security" necessitated a positivist reduction of the intentional meaning inherent in the psychoanalytic notion of "anaclitic object-choice" (Freud 1914; Brentano 1874), a hypothetical process resistant to objective measurement. I describe how this immeasurable construct underwent a series of transmutations upon entering attachment research, as it was adapted to fit available investigative techniques and prevalent standards of validation. I argue that the semantic reduction of the meaning of this psychoanalytic construct within attachment research was driven by a pragmatic shift in the normalizing techniques of Anglo-American psychiatry, from their origins in asylum practice to the contemporary risk-management procedures characteristic of late modernity.

Comparisons between the Subconscious (not Unconscious) and the development of Modern Physics as a model of Science for Psychology during the late 19th and early 20th century

Robert Roff (clinical psychologist)

I referred in a prior presentation to those I have called the Old Masters of the Subconscious (Frederic Myers, Pierre Janet, Albert Binet, William James, Morton Price, Boris Sidis, Theodore Flournoy, and Carl Jung) as comprising a separate Theoretical Orientation in early Psychology (Roff, 2022). In the last few decades, many of the Old Masters have been discussed as significantly influencing each other's work (Taves, 2004, 2009; Shamdasani, 2003; Taylor, 2010), with some Old Masters experiencing a renaissance in modern areas of Psychology such as Neuroscience, Trauma/Dissociation, and being Creative/Growth-Oriented (Tinnin, 1990; Bargh, 2019; Taylor, 2010; Siegel, 2018; Craparo, Ortu, and van der Hart, 2019), and some even identify them as a separate group in psychodynamic theories (Taylor, 2010).

Going beyond the previous presentation, in this paper, I contend that the Old Masters kept up with the changes in what became known as Modern Physics, due to their emphasis on the Subconscious. I contend most of early Psychology had an insufficient understanding of the Subconscious, due to using mainly Classic Physics and the Freudian Unconscious (Boring, 1950). Freud's Unconscious (1912, 1915), used a single consciousness model, a pathological Unconscious, and did not depend on Alterations of Consciousness. Freud's concepts have been compared to Classic and not Modern Physics (Capra, 1982; Lucas, 2016), and most of early Psychology based its conception of Science solely on Classic Physics, until about the last two-thirds of the 20th century (Boring, 1950; Capra, 1982; Schultz and Schultz, 2004). The Old Masters used the Subconscious (not Unconscious), which consisted of multiple centers of consciousness often organized into higher and lower systems of consciousness (Janet, 1889 and 1907; James, 1890; Binet, 1896; Baldwin, 1913, Taylor, 1983). They also used Altered States of Consciousness as a method of research and treatment (not just "Hypnosis") (Myers, 1903; Prince, 1914, Sidis, 1909; Taylor, 1983), to identify the normative, growth-oriented/creative, and pathological aspects of subconsciousness. I will contend in this paper the Old Masters identified hidden phenomena not explainable by Classic concepts, and/or by our usual sense of consciousness. Some said the Subconscious changed Psychology in much the same manner as the discovery of radioactivity changed the conception of Classic Physics as a model for Science (e.g., Prince, 1921; Jung, 1946). Many considered the Subconscious/Subliminal as a means to reconcile Spiritualism with more traditional (Classic) concepts of Science (Gurney, Myers, and Podmore, 1886; James, 1902; Myers, 1903; Flournoy, 1911). Some made comparisons to X-rays, Ether, or subatomic particles (Myers, 1903; Sidis, 1909; Prince, 1925). All of this would culminate in Jung's work on topics such as Individuation, the Observer Effect, and Synchronicity (Jung, 1946, 1952; Shamdasani, 2003).

Most modern Psychologists, in the last half-century have understood the limitations of Classic Physics to define Psychology as a Science (Schultz and Schultz, 2004; Kelly, et al 2007; Siegel, 2018), by including Subjectivity, Creativity, and Growth-Oriented Dimensions of the Psyche (Kelly et al 2007; Taylor, 2010; Bargh, 2019), and comparing Spirituality/Religion with Modern Physics concepts, but often with incomplete or no reference to the Old Masters' work (Lyon, 2012; Siegel, 2018). I contend the Old Masters kept closer to the ideal of using Physics as a model of Science than most of early Psychology. By placing the Old Masters of the Subconscious back into the history of Psychology as a group, it may help with misinterpretations of the Old Masters' work, identify important areas overlooked by early Psychology, and perhaps be used as an additional Theoretical Orientation in the modern era.

Mental energetics and Psychophysical Parallelism: A study on the reception of energy conservation in German Experimental Psychology

Leonardo Niro (University of Essex)

The years 1850 and 1900 saw the process of institutionalisation of psychology as a discipline in German universities; this process was simultaneous and to a large extent conceptually underpinned by the emergence of psychophysical parallelism (PP) – the theory holding that the mind and body are correlated without any causal interaction. Following Gustav Fechner's original articulation of PP in *Elements of Psychophysics* (1860), parallelism would receive nearly ubiquitous acceptance amongst psychologists. Physiologist Ewald Hering for one argued that PP consisted in the 'condition *sine qua non*' for any research in psychology (Hering, 1878), while Wilhelm Wundt held that 'parallelism is not a fundamental principle, but *the* fundamental principle of psychology. It is the only principle at its disposal' (Wundt, 1896).

Despite the great variability amongst the different versions of PP (cf. Wegener, 2009), one reason that made it such a predominant position to the age-old mind-body problem at the time was its apparent solution to the problem of the causal closure of nature (*geschlossene Naturkausalität*). This consisted in a problem that, albeit not new, had been reframed due to the recent "discovery" of the law of

conservation of energy. Researchers involved in the formulation of energy conservation had emphasised the impossibility of a *perpetuum mobile*: nothing is created, nothing is lost. Nature can be reduced to a quantifiable exchange from one form of energy to another, and whichever forces act upon a body must be accompanied by an equivalent conversion from another form of energy. Only matter acts on matter, leaving no room for the causal effect of forces that cannot be reduced to attraction and repulsion. For proponents of PP, however, mental processes were neither caused nor constrained by physical processes in the body but merely simultaneous to them. Psychological and physiological events along those lines run in parallel, always accompanying one another but never interacting, thus allowing psychological phenomena to be investigated independently of their simultaneous physiological processes and, therefore, also allowing the autonomy of psychology as a science with its distinct object of study.

The final decade of the century, however, saw the emergence of a controversy regarding the status of parallelism and of the consequences of the energy law for psychology. Initially launched by Carl Stumpf's address during the Third Internal Congress of Psychology, held in Munich in 1896, criticisms to PP soon proliferated in a series of publications (Becher, 1908; Busse, 1900; Klein, 1906; Schwarz, 1897; Stumpf, 1897, 1903; Wentscher, 1896). The controversy grew to such an extent that philosopher Ludwig Busse would make the case that the mid-century *Materialismusstreit* had by the turn of the century been replaced by a *Parallelismusstreit* (1903, p. 2). As will be argued, the campaign against psychophysical parallelism reflected the exhaustion of the model by which psychology had been institutionalised until that time, and which had in the doctrine of psychophysical parallelism one of its foundational theoretical pillars. Further, I will explore some of the different ways energetics were used as framework that justified the various criteria of demarcation of psychology – both by authors attempting to reduce the mind to the underlying material exchanges, as well as by various anti-materialistic projects. As will be argued, the question at the time was not one of whether or not to use the concept of energy in psychology, since the consequences of energy conservation were simply too prevalent to be ignored by an emergence science that still felt in need to justify its philosophical and institutional foundations, but rather of how exactly to employ energetics towards different psychological projects.

Empowering Through Art: Mary Kathleen “Mickie” McGraw and the Art Therapy Studio *Christian Lewis and Jennifer L. Bazar (Cummings Center for the History of Psychology)*

Only months after Ohio became the eighth state to approve art therapy licensure, local art therapy pioneer, Mary Kathleen “Mickie” McGraw, passed away at the age of 82. For more than 50 years, McGraw fought to define the boundaries of the field and establish recognized credentials for those providing art therapy services. A founding member of the Buckeye Art Therapy Association and early member of the American Art Therapy Association, McGraw was central in establishing the Art Therapy program at Ursuline College, one of the first of its kind in Ohio. She is perhaps best known, however, for co-founding the Art Therapy Studio (ATS) with Dr. George Streeter, which remains one of the longest-running independent art therapy centers in the United States. For her local leadership in the Cleveland area, McGraw was recognized in 2010 with the Cleveland Arts Prize Award.

The Art Therapy Studio was established in 1967 at Highland View Hospital (today, MetroHealth Rehabilitation Institute of Ohio). Originally located in Warrensville Township, an eastern suburb of Cleveland, the Hospital specialized in rehabilitation for those experiencing physical disabilities due to stroke, neuromuscular disease, or injuries; the Art Therapy Studio was therefore envisioned initially as a therapeutic outlet for individuals facing physical disabilities or illness (Cuyahoga County Hospital System, n.d.). This focus was further influenced by the personal experiences of its founders: McGraw, who was left wheelchair-bound after contracting polio at a young age, and Streeter, whose battle with tuberculosis had left him bedridden for two years. Both cited their experiences with the “healing qualities

of art” during their respective hospitalizations as key to the approach that shaped the Art Therapy Studio (McGraw, 1995, p. 167; see Patton, 2010; Spaeth, 2014).

Opened at a time in the United States when the field was largely defined by a few select individuals who were using art therapy within institutional settings, the Art Therapy Studio was novel in its early adoption of a studio-based approach. Building on the work of art therapy pioneers, including Mary Huntoon, Georgette Powell, and Edith Kramer, a studio-based approach prioritizes a self-directed artmaking experience, emphasizing the individual’s freedom and personal expression (Moon, 2010). The Art Therapy Studio was similarly designed with the act of creation at its core, making supplies readily available and encouraging an open-door policy that extended beyond clients to include the participation of family, friends, and community (McGraw, 1995). Over time, the Art Therapy Studio has served an increasingly diverse group of individuals, ranging from children to the elderly, who face a range of physical, mental, social, or emotional challenges. McGraw herself was known for modifying the available supplies to eliminate barriers to participation; the most innovative of these methods being the creation of “wheel art,” a technique that enables wheelchair users to paint by using the wheels of their chairs as paintbrushes (McGraw, 1995).

Despite the significant and lasting contributions made to the field over the course of her career, McGraw’s name only appears in passing within histories of art therapy in the United States (e.g., Junge, 2010). Drawing on the archival records of the Art Therapy Studio, local-area media coverage, and published sources, this paper seeks to integrate the pioneering contributions of Mickie McGraw and her co-founder George Streeter into the tapestry of histories that reveal the development of art therapy in the United States.

Transforming the Boulder Conference: Continuity and Discontinuity in Interpretations of the Science–Practice Relationship in Clinical Psychology

Alan Tjeltveit

Participants engaged in the ongoing debates in the United States about the optimal relationship between psychological science and psychological applications often make historical appeals to the 1949 Conference on Graduate Education in Clinical Psychology at Boulder, Colorado (the “Boulder Conference”; Raimy, 1950). Boulder Conference recommendations, themselves built on previous training recommendations (e.g., Brown et al., 1935; Shakow, 1942), were interpreted and either praised or critiqued.

A common interpretation of the Boulder Conference is that, through that conference, 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). The historical reasons for an emphasis on science in clinical psychology have been well-documented (Baker & Benjamin, 2000; Capshew, 1999; Farreras, 2001, 2005, 2016; Frank, 1984; Shakow, 1978). However, that emphasis doesn’t do full justice to actual conference recommendations. Boulder conferees advocated for an epistemic framework for clinical psychologists broader than science alone: The ideal background for clinical psychologists included undergraduate coursework in the history of culture (“history of civilization, comparative literature, comparative religion, philosophy, etc.,” “psychology as revealed in literature” (Raimy, 1950, p. 215), and cultural anthropology (p. 229), plus “non-academic experiential background ... involving close relations with both ordinary and unusual persons in field, factory, institution, or laboratory” (p. 214-215). However, the liberal arts breadth that the Boulder conferees endorsed, with its concomitant epistemological breadth, gradually, if fitfully, disappeared, in discussions of clinical psychology training and in most later accounts of “the Boulder Model.”

By the early 60s, dissatisfaction with clinical psychology training resulted in the formation of the prestigious Clark Committee (formally, APA’s Committee on the Scientific and Professional Aims of

Psychology, 1965) and the 1965 Chicago Conference on the Professional Preparation of Clinical Psychologists (Hoch, Ross, & Winder, 1966). The relationship between the Conference and the Committee—operating in parallel and in tension—has not been adequately explored. Clearly, however, psychologists passionately advocated for divergent views. As Kelly (1966) noted,

the months before the [1965 Chicago] Conference had been characterized by dissent in the now massive psychological community. The dates of the Conference had even had to be set back several months to allow tempers to cool down and give the normal processes of professional intercommunication a chance to function. (p. 99)

This presentation will be a progress report in a line of research investigating historical factors contributing to divergent interpretations of the Boulder Conference. I will focus on interpretations in published citations to Raimy (1950) between 1956 and 1967, the year after the report of Chicago Conference was published and the year in which the Clark Committee gave its final report. In reviewing that literature, I will give particular attention to fidelity with the Boulder recommendations, and to different interpretations of clinical psychologist training, clinical psychologist roles, the science-practice relationship, and the role of extra-scientific contributions to training.

By 1967, the view that clinical practice should be based on science alone was increasingly equated with the “Boulder Model” and the “scientist-practitioner model.” That model was sharply contrasted with other models of clinical psychology, even though the Boulder conferees actually endorsed epistemological breadth and educational experimentation (Peterson, 1992) and promulgated no canonical understanding of the science–practice relationship. Those emphasizing science and those emphasizing practice interpreted the Boulder recommendations differently. I will explore this history of changing interpretations (and rhetorical uses) of those recommendations in the pre-1968 intra-professional disputes about the science–practice relationship, about valid and ideal training models, and about the definition of the clinical psychologist.

The Feminine Mask and Gendered Behavior in the Novels of Gillian Flynn ***Olivia Kurylo (The University of Akron)***

Male expectations for female behavior and emotional expression are consistently present in daily life, and the extreme effects of such are predominantly explored in feminist literature written by women spanning from the early twentieth century to the present day twenty-first century. Female characters in modern American literature written by women find themselves subjected to mental illnesses such as depression, anxiety, suicidal ideation, several personality disorders, and even psychopathy as specifically outlined in Antisocial Personality Disorder. On the other side of such effects lies deviant behaviors including criminal, harmful, and fatal actions. Female characters in feminist literature display such illnesses and actions through their masquerade and manipulation of male psychology in order to further their goals and promote their images to the public. In novels by female writers such as Gillian Flynn, psychologically complex female characters commit crimes that are socially stereotyped as belonging to the male gender and male capabilities while presenting in a feminine nature. In doing so, female characters such as Amy Dunne and Amma Crellin from Flynn's novels *Gone Girl* and *Sharp Objects* manipulate society into assuming their innocence by playing into society's image of a woman and their expected conformance to societal expectations established by males and the male gaze. Women's literature demonstrates females who murder like males but present as traditionally feminine and thus destroy the socially constructed barrier between gendered capabilities, all while simultaneously illustrating how psychologically destructive conformance to the male-gaze is to the female psyche.

Throughout Western history, women have been chained to behavioral and emotional expectations perpetuated by a society conditioned by patriarchal social synthesis. In such, women are forced to conduct themselves in accordance with the desires of males, and thus select the feelings that they

express based on what the male-dominated society finds appropriate. In order to save themselves from social ostracization and humiliation, women must select their behaviors and emotional responses so that they may maintain a status of desirability to the male gaze, and ultimately to the community in which they reside. Strict conduct and control of oneself, psychological processing, and emotional understanding which conforms to gender-restrictive expectations debases female autonomy, as well as splits the female identity into two separate entities that operate separately of one another. Such a rift in a person's identity of themselves proves detrimental in their conceptual understanding of themselves as a whole, and for women, have showcased a range of effects from mental illnesses to deviant behavior. In my presentation, I will examine the behaviors and psychological conditions of female characters that manifest as a result of a gender-restrictive Western society present within Gillian Flynn's literary works. I will use Flynn's work as my test case paired with additional research into how mental illnesses and deviant behavior present within females. The topic of this paper addresses societal definitions and stereotypes that allow psychologically complex female characters to manipulate their communities and the male populations that control them. The effects of such an emotionally restrictive society are explored through the examination of female behaviorism detailed within twenty-first century feminist literature produced by women writers in The United States of America.

Marx in London and 'Ethnographic Seeing': Reflections on his maturing philosophical anthropology
Robert Williams (The University of Akron)

On the heels of mercantilism came the Industrial Revolution and its attendant upheaval of the existing socioeconomic order that, in turn, gave birth to the academic discipline we know as sociology. Suddenly, land was more valuable for mining or for factories than it was for growing food. Sociologists wanted to study what was happening, why it was happening, and what could be done about the explosion in massive social inequality. A German journalist living in England, Karl Marx was witness to the subsequent machinations of a voting 3% to maintain hegemony over the remaining disenfranchised 97% of the English and British populations who suffered their laws and were unable to vote against them. Along with his wife and children, Marx lived in London during the 1840s – less than three decades after the English garrison state violently quashed four years of Luddite liberation actions in Huddersfield and surrounding villages of West Yorkshire.

The development and application of any philosophical anthropology is intertwined with distinct features determined from an economic order – namely, ideology and social history. This paper queries the impact of acculturation in London upon the philosophical anthropology of a newly immigrated Marx. His subsequent newspaper articles published in the *New York Tribune* revealed his useful 'ethnographic seeing' of London's sociocultural milieu. This paper locates 'ethnographic seeing' - conceptualized as a set of dissonant cultural logics that query culturally unfamiliar and disparate scenes - as a useful portal for critical investigations into sociopolitical thought. As a participant observer in London, Marx had more than a ring side seat.

By placing the immigrant Marx in his wider London social context, this paper explores the impact of post-Luddite garrison state influences in England upon the German scholar's maturing philosophical anthropology and sociopolitical thought. As a father of seven children during his acculturation, the likelihood and extent of a cultural transformation should be at the core of any ideological discussion of the maturing Marx. A focus on the acculturation of Marx in England through rapidly changing material, sociocultural, and technological contexts not only illuminates new features in the development of his conflict perspective of the world but opens new ways to appreciate his critical methods, philosophical anthropology, and complex sociopolitical perspective. Marx's writings rallied the social sciences from armchair conjecture towards the conceptual tools and historical methods of what came to be known as the conflict perspective in

sociology. Many anthropologists were also swayed by Marx's critiques, especially those criticisms of a state ruled by a mere voting 3% as an illegitimate superstructural protection of social inequalities perpetuated by greed. Many of his insights into the socioeconomic conditions and attending political issues remain central to sociology's conflict perspective.

This paper begins by exploring the maturing philosophical anthropology that Marx developed in London to ethnographically see power, prestige, and property in the secure hands of a mere voting 3%. It then discusses Marx's anthropological observations of the main ways that the powerful and wealthy employed their resources to maintain their class position and power. Next, the impact of 'ethnographic seeing' of London's material, political, and social inequalities on Marx's philosophical anthropology is emphasized. Finally, this paper considers the extent that Marx's acculturation in London contributed to his notion of false consciousness – a condition experienced by the suffrageless to accept hostile social inequalities through the fostering of ideological conformity. Analytical and descriptive methods are employed to explore these influences.

Slaves of Rockefeller: Progressivism, “Scientific” Education, and the New York City School Riots of 1917

Christopher Green (York University)

Today, most people think of the Gary Plan for school reform as a cornerstone of early 20th-century American progressivism. What could be more normal than having high school students move from room to room over the course of the day to take advantage of rooms that housed specialized equipment, taught by staff with expertise in the topics to be learned? Historians with maps to teach history, scientists with test tubes and Bunsen burners to teach science, coaches with sports equipment to teach gym, musicians with instruments to teach music, etc. Surely, every parent who had come from a one-room schoolhouse would be overjoyed to know that their child was to be afforded such educational luxuries. It was not always so. On October 17, 1917, the parents of New York City – especially Jewish immigrant parents – protested so vehemently against the reformist mayor's efforts to implement the Gary Plan in Gotham that the city was forced to call out the police to quell their disturbance. But the protests did not stop. They grew, day after day, spreading from their point of origin in the Upper East Side up to the Bronx, then across the river to Brooklyn and down Manhattan island to the Lower East Side. The protesting parents were soon joined by school children. As the protests grew in size – one newspaper claimed 10,000 were involved – they also became increasingly violent: from shouting and chanting that the Plan would make their children into “Slaves of Rockefeller,” the crowds soon moved on to throwing bricks and stones and bottles at school windows, and eventually to throwing the same projectiles at the police themselves. Hundreds were arrested, both parents and children.

Sensing an opportunity, opposition politicians soon became involved as well. The election for mayor was already underway at the time. The Socialist candidate, a Jewish labor lawyer named Morris Hillquit, made opposition to the Gary Plan a campaign priority, second only to his opposition to American entry into World War I (which had been declared by Woodrow Wilson several months before). James McKeen Cattell, who had been fired from his Columbia professorship just two weeks before the protests began, ostensibly for opposing the War, publicly endorsed Hillquit. He then went on to declare that the Gary Plan would facilitate the imposition of mandatory military training on all the city's sons, with the aim of delivering them ever more efficiently into Europe's “meatgrinder.”

After two weeks of public disorder, the protests were forcibly brought to an end. In the election, held a just a week later, on November 6, the mayor who had promoted the Gary Plan, John Mitchel, was soundly defeated by the Tammany Hall candidate, John Hylan. Hylan immediately rolled back preparations to implement the Gary Plan. The very day after the New York City election, the Bolsheviks seized power in Petrograd and the communist revolution in Russia began.

Defining a "Harvard Approach" to Social Science: The Cabot, Henderson and Parsons Seminars, 1926-1950

Lawrence Nichols

The paper summarizes and compares three "interstitial" efforts, in the second quarter of the twentieth century, to define what can be characterized as a distinctive "Harvard approach" to sociology and other social sciences. In each case, an intellectual entrepreneur with strong internal networks, in response to a perceived need, recruited faculty for a programmatic seminar. Thus, Dr. Richard Clarke Cabot, a professor of medicine and chair of the Department of Social Ethics, coordinated a two-semester graduate seminar from fall 1926 through spring 1927. Cabot enlisted the widest range of participants, including faculty in the humanities and social sciences as well as in social work, business and divinity. What distinguishes this event are Cabot's set of organizing questions that emphasized social amelioration, and the participation of two women. Physiologist-sociologist Lawrence J. Henderson's initiative (1932-1942) began as a graduate seminar on "Pareto and methods of scientific investigation," and it developed into an undergraduate-graduate course entitled "concrete sociology: a study of cases." Henderson recruited widely, but he asked participating faculty to apply. In their research presentations, the conceptual approach of the Italian engineer, economist and sociologist, Vilfredo Pareto, in accordance with his own summary of it. This project might be viewed as a "guerilla movement," by outsiders without formal training in sociology, to demonstrate how that field might become a "genuine science." Talcott Parsons, finally, limited participants in his seminar (September 1949 to January 1950) to faculty in several closely related fields: sociology, social and behavioral psychology, clinical psychology and cultural anthropology. These, Parsons argued, were the "basic social sciences" on which others such as economics and political science depended, and they were also gradually "converging" in a major intellectual movement. Parsons's project is also distinguished by external funding from the Carnegie Corporation.

A crucial and ongoing contextual factor motivating the organizers was serious doubt among Harvard administrators, faculty and students regarding the validity and credibility of certain social disciplines and organizational units. Cabot faced a crisis in which the administration of President A. Lawrence Lowell had lost confidence in social ethics as a field of concentration and was moving toward closing the department. Cabot therefore explored the possibility of creating a new, interdisciplinary department of "social science." Henderson's case is more idiosyncratic, in the sense that he was under no external pressure and was not only in good standing as a renowned scientist but also served as a valued advisor to the administration. Henderson's reformist effort is largely attributable to a personal encounter with Pareto's 1916 treatise on sociology, an epiphany that turned him into a passionate advocate for what seemed to him the first truly scientific approach to the controversial discipline. Parsons organized his seminar three years after the establishment of the Department of Social Relations at Harvard, which he chaired, when there was widespread criticism of the new unit as lacking intellectual coherence. It can thus be considered a post facto search for intellectual identity and organizational legitimacy.

All three seminars had significant outcomes. Each organizer intended to bring out an edited volume that would serve as an exemplar for others in sociology or in social science more generally, though only one appeared. Presenters' unpublished papers from the Cabot seminar are preserved in the Harvard University Archives, along with transcripts of discussions among participating faculty. Soon after this event concluded, however, the administration created an interdisciplinary Committee on Sociology and Social Ethics, to which some members of the seminar were appointed, and which recommended the establishment of a Department of Sociology. When that occurred (1931) several seminar participants became members or "interdepartmental" members in Sociology. The Henderson seminar stimulated published studies by participants that explicate or apply Pareto's "systems" approach, in sociology, anthropology, history and management. Another noteworthy result was the spread of Henderson's epistemology of "conceptual schemes" (similar to Kuhn's later "paradigms") both within Harvard and

beyond. Henderson had planned to bring out a “concrete sociology casebook,” and papers he selected are kept at the Harvard Graduate School of Business Administration. But Henderson’s sudden death prevented this. The Parsons seminar, finally, generated a collective statement of principles along with a set of illustrative essays, *Toward A General Theory of Action* (1951), from Harvard University Press. Its model of “three interpenetrating systems of action” and multi-level analysis influenced teaching and research at Harvard, and in social science more generally, for decades. The Social Relations experiment, moreover, remains an object of scholarly research.

An overall historical lesson might be that these three seminars mark the end of an era in which Harvard faculty and administrators could comfortably assume that their institution “would naturally lead the way.” Today, it seems far less likely that those at a particular university, no matter how eminent, would presume to attempt anything comparable to the ambitious social science projects that Cabot, Henderson and Parsons pursued.

The History of Social Psychology in 100 Years

James Blair (Elmira College)

The Social Psychology field has had many updates to its terminology and content since it first began. Terms such as prejudice, conformity, cognitive dissonance, obedience and attraction are some of the foundational ideas to understanding this subject. However, Social Psychology hasn’t had these concepts since its creation. Throughout its time, there have been many changes to how people perceive these ideas, the controversial experiments and the general notions that it has established.

This project looked into mainly introductory psychology textbooks published about every 25 years supplemented with social psychology textbooks and other introductory textbooks over the span of about 100 years. Starting from 1908 to 2008, key ideas, vocabulary, turning points and experiments were recorded.

In the early days of Social Psychology, scholars seemed to only focus on the self, habit and personality. The idea of the environment and culture is peppered throughout these books and how it influences personality. It wasn’t until after WWII when people started to study why people acted the way they did. After the experiments of the 1960’s and 1970’s, it became more widely accepted that Social Psychology was the topic about other people influencing a person or how a group of people influences another group. It was after this era of fundamental experiments that Social Psychology began to take shape of what it normally looks like in contemporary introductory textbooks.

Throughout multiple textbooks from different years and authors, the subject has been split into subtopics of social influence, attitudes, social behavior, interpersonal attraction, social relations, social thinking or a variation of this categorization. Even with the large variety in how these authors want to discuss Social Psychology, it comes down to the two general understandings of what people do to influence others and how people process and react to that influence.

Learning from students: A novel way to study research programs

Chabrian Tanguay and Alex Beaujean (Baylor University)

We often learn about scholars’ research programs by examining their corpora of publications. Sometimes, however, we cannot rely on the corpora, or at least not rely solely on it. This is often the case when there is a discrepancy between how scholars describe their research programs and the scholarly corpora they leave behind. In these situations, we have to rely on other evidence to understand the research programs (e.g., letters to other scholars, testimony of close associates). In this poster, we discuss a new method for acquiring evidence about scholars’ research programs: investigating their students’ scholarship.

Our method is based on a relatively weak assumption: to the extent scholars give some direction to their students' research projects, a systematic review and analysis of the students' work will allow us to infer scholars' research programs. Yet, there is no explicit guidance in the historical or scientific methods literature on how to conduct such reviews and analyses. To some extent, this is understandable because acquiring this information is far from being straightforward. For example, biographical works may list some prominent students, but seldom list all the students with whom a scholar worked, so it can be extremely difficult to just develop a population frame. Yet, the neglect of such a methodic work leaves a gaping hole for studying the work of particular scholars.

The method we have been developing integrates techniques from other historical and scientific methods (e.g., systematic literature review, historiometrics, qualitative meta-analysis). In our poster we will discuss aspect of this technique, but do so in the context of studying Charles Spearman's research program. Spearman is an ideal candidate for working out our method because there is a notable discrepancy between how most historians of psychology discuss his research program and how he and his students discussed his program.

The First Psychological Autopsy: Lydiard Horton's 1929 "On College Disappearances: The Analysis of a Case."

Hendrika Vande Kemp

Edwin Shneidman and Norman Farberow introduced the phrase "psychological autopsy" into the suicidology literature at the 1959 meeting of the American Psychological Association, defining it as "a detailed reconstruction of the last hours of suicide victims" (see also Shneidman 1994, Leenaars 1999). The suicidologists developed this notion—complete with a detailed outline—in ignorance of the first psychological autopsy published in 1929 by Boston's pre-eminent consulting psychologist, Lydiard Horton (1879-1945), who presented the case of Frances St. John Smith to the Massachusetts Medico-Legal Society—the professional association for medical examiners and coroners—on October 3, 1928. Frances Smith, a freshman at Smith College in Northampton, Massachusetts, disappeared on the stormy night of January 13, 1928. The ensuing "girl-hunt" cost \$100,000, equivalent to more than \$1,787,000 in 2023. The story ran on the front pages of newspapers in all 48 States and in Canada, and eventually was sensationalized in *True Detective Mysteries* (Norris 1929). Frances's remains were discovered in the Connecticut River on March 29, 1929. Identification was based on dental records, the death ruled accidental.

Horton was brought into the case on January 27, 1928, by his college friend, the journalist Stanley Washburn (1878-1950), who had a daughter at Smith College and served as the Smiths' liaison to the Press. On January 27th Horton participated in a conference in Northampton with the Smiths, Boston psychiatrist Douglas Armour Thom (1887-1951), and neurologist/psychiatrist John Alexander Houston (1839-1937), Superintendent of Northampton State Hospital. Horton, who had a detailed approach to "The Education of the Emotions," undertook a "scientific inquiry into the 'intimate past' of Frances St. John Smith," an application of [William McDougall's] "purposive psychology," to discover her internal emotional conflicts and the resulting fatigue which demanded positive action rather than self-destruction. Horton held a press conference, which he described in a letter to the writer Gene Fowler (1890-1960), emphasizing the investigative experience he acquired while working for the Chicago & Alton Railroad and in recent consultations to industry. Horton interviewed the people who knew Frances in New York and the family's summer home at South Amherst: relatives, friends, and caretakers such as the governess, travel chaperones, and the family physician, shedding light on "her education, temperament, state of health and emotional attitude from childhood till the day of her return to college at the end of the Christmas vacation." He investigated her school years at the Milton Academy, including her writing and her relationship with a host family. At Northampton he interviewed Smith College President William A. Neilson (1869-1946), the Dewey House matron, the Registrar, other Smith faculty and staff, and Frances's

friends and classmates. He also walked the campus to explore potential routes to South Amherst on bridges crossing the Connecticut River.

Horton's lengthy report—containing virtually every element of the later formal psychological autopsy—addressed Frances's mental state from a bio-psychological perspective, and primarily in terms of the expectable adjustments to college. He took into account the cessation of her menses and external descriptions of her trance-like state, and the unusual atmospheric condition of a January thunderstorm, concluding the death was accidental.

Disappointed that the Smiths did not allow him to share his results with the Press, Horton turned to his colleague, the neuropathologist Myrtelle May Moore Canavan (1879-1953), who invited him to speak at the Massachusetts Medico-Legal Association. There, attendees engaged in a lively discussion included in the 1929 publication. Soon thereafter Horton began 12 years of teaching Biopsychology at the Boston University School of Medicine. He published his ideas on the mental health of college students in 1932, and from 1936 through 1939 became the primary counselor for students at The Massachusetts Institute of Technology.

Memory center of the institute of psychology (UFRJ): rescue and preservation of old laboratory histories

Arthur Arruda Leal Ferreira (Federal University of Rio de Janeiro)

This work presents the establishment of the Memory Center of the Institute of Psychology (CMIP) at the Federal University of Rio de Janeiro (UFRJ/Brazil), which was founded in 2021 by using materials from the former laboratory of this institute, created in 1924 and closed in 1990. This former laboratory was initially proposed to be located within a mental institution, the Psychopath Colony of Engenho-de-Dentro, based on the proposal of its director at the time, the hygienist physician Gustavo Riedel (1887-1934). Waclaw Radecki (1887-1953), a Polish psychologist who had previously worked with Édouard Claparède (1873-1940) in Geneva and who traveled to Brazil in the early 1920s, was appointed to lead this laboratory. It operated until 1932 when it was transformed into an institute and later closed by the dictatorship of Getúlio Vargas. Subsequently, in 1937, the collection of this laboratory was incorporated into the University of Brazil (later called UFRJ) and formed the basis of the current Institute of Psychology (IP). In the history of this laboratory, there were a series of quite significant changes over time due to its institutional purposes (e.g., purely research or primarily teaching), regulatory legislation, and the researchers who worked there, not to mention that the original collection was added to other equipment that was subsequently acquired until the year 1990, when the laboratory was closed.

In 2012, at the initiative of Josiane Pawlowski, a project for the registration and recovery of this material was developed and carried out until 2014. Subsequently, this project was resumed by the authors of this work in 2018. It resulted in a project called the Memory Center of the Institute of Psychology (CMIP/UFRJ), which was incorporated into the IP in 2021 with the inauguration of its permanent exhibition. It resulted from a long process of articulation between research groups, master's dissertations, doctoral theses, and the collaboration of different groups. This present work intends to delineate a brief history of this process, drawing attention to the main actors involved in its establishment and the logistical, institutional, and legal challenges, as well as aiming to disseminate the current efforts of the group that leads the organization of the CMIP/UFRJ. By rescuing instruments from the old laboratories of the IP at UFRJ, from its various phases and historical moments, a permanent exhibition was assembled in the institute, where some of the rescued and partially restored collections are available to the public. This exposition presents not only the instruments but also the history of the institute to the public. The masterpieces of the center are the old laboratory instruments (Würzburg museum) and our expertise in identifying them, but it also has collections of old psychological tests, bibliographical collections of old research projects in Brazil, a site (a portal focused on the history of psychology) with entries about institutions and prominent people of Brazil, and a small publishing company.

Currently, a new interdisciplinary team has been formed which focuses on cataloging, emergency conservation efforts such as cleaning and packaging, and the restoration process of other collection items, which exhibit various degrees of preservation since they were stored for decades in the IP. This work aims to disseminate the CMIP/UFRJ collection in an initiative that involves historical research, scientific dissemination, and conservation/restoration, in addition to preserving the institutional memory of UFRJ. While not the first of its kind in Brazil, the creation and maintenance of CMIP/UFRJ has the distinction of partnering with the Conservation and Restoration undergraduate course of the School of Fine Arts at UFRJ to help with the restoration of the items in the collection. The primary goal of this initiative is to serve as an inspiration for other institutions that house old psychological laboratory instruments to conduct similar exhibitions, fostering a dialogue with the community of historians of psychology, restorers, and conservators, as well as other related fields that participate in the process of creating and maintaining historical collections and preserving institutional memory. In this aspect, the second purpose of this project is to facilitate the creation of an international network of museums and memory centers of psychology, considering the place of this meeting (The Cummings Center) and the presence of possible papers interested in psychological collections.

“Kodak Girls” and “the frequent role of the officially non-scientist wife”
Arlie Belliveau (York University)

Psychology’s early women filmmakers exist at the intersection of inter-war amateur filmmaking and psychological measurement. Their presence reflects both the increase of white women entering applied child development and comparative psychology fields (Pickren & Rutherford, 2010; Haraway, 1989), and the deliberate promotion of motion picture filmmaking to that same demographic (Swanson, 2003). This paper is an attempt to reconcile the social-material culture of motion picture filmmaking, with the ladenness of class and gender expectations and psychology’s scientific processes.

Donna Haraway noted the story-, theory-, and value-ladenness of scientific processes when discussing the politics of being female in primatology (1989). Established stories of western women documenting travel and family life with still photography normalized their interest in amateur motion picture cameras (Zimmermann, 1988; Cooper, 2024).

“Kodak Girls” appeared in Eastman Kodak camera advertisements from 1893 to the mid-1920s. Images of young middle class white women promoted the still-image cameras for independent travelers, savvy gifters, and diligent family documentarians (Cooper, 2024; Swanson, 2003). Eastman Kodak Research Laboratories developed their 1923 Cine-Kodak moving-picture camera and 16mm reversal safety film with the Kodak Girl in mind. Features such as smaller size, simplified development processing, lower cost, and flame resistance made the system marketable for home and amateur use (Swanson, 2003). In these new ads, “Kodakers” were portrayed as both trusted story keepers and playful opportunists. They were young adventurers and mature homemakers (Swanson, 2003; Zimmermann, 1988; Cooper, 2024). So, how might the stories, values, and theories attached to the Kodak Girl translate into expectations for psychology’s women filmmakers?

While there are many women who appear in psychology films as participants – particularly child development films where their children are the subjects (Cameron, 1934), or where parenting techniques are filmed (Gesell, 1934) – it was rare for a woman to act both as an active experimenter and coauthor. However, there are several fascinating examples. Haraway (1989) pointed to Ada Yerkes’ work on *The Great Apes* (1929) as an “officially non-scientist wife, who contributed substantially to the production of the primate text” (p. 298). My paper will elaborate on three women in psychology who had a similar impact: Luella Dorothy Agger Kellogg, Sarah Geraldine Longwell Doll, and Myrtle Bevan Firestone. Just as there is a striking “prominence of married couples publishing together in primate literature” (Haraway, 1989, p. 297), so too were there a number of women filmmakers publishing with their psychologist husbands.

Haraway (1989) noted determinants for successful women field primatologists including: class, race, travel, family money, an academic parent role model, an independent career, tolerance for discrimination, and the ability to delay degree completion while raising children. A case study of Luella Kellogg's films demonstrates areas of suffusion between the Kodak Girl and non-scientist wife roles.

Kellogg was credited as coauthor for her research contribution working 84-hour weeks over a nine-month period at the Yerkes Laboratories of Primate Biology in Orange Park, Florida. Raising their infant son alongside a young ape named Gua, she produced four reels of film with her husband, psychologist Winthrop Niles Kellogg (Benjamin & Bruce, 1982; Kellogg, 1933; Kellogg & Kellogg, 1933).

Luella Kellogg fits among both Eastman Kodak Girls and Haraway's officially non-scientist wife in several ways. She completed a Bachelor of Arts degree from Indiana University where she studied Spanish, was a member of Delta Zeta foundation, wrote for the Arbutus yearbook, sat on the Women's Athletic Association, and participated on the swimming, soccer, field hockey, and basketball teams (Haus et al., 1918; Larm et al., 1919; Alsmann et al., 1920; Newton et al., 1921; Tolle et al., 1922). She was a participant in the films on and off camera and presented as a young fit mother ready to film this next challenge.

The stories of Luella Kellogg, Sarah Doll, and Myrtle Firestone are situated temporally, thematically, and literally in previous research – such as Schultz-Figueroa's (2023) work on the male psychologists who filmed primates in Yerkes' lab, and Swanson (2003) and Zimmerman's (1988) characterizations of women amateur filmmakers as "dilettantes". However, their story and the work that they conducted, these overlooked Kodak Girls of psychology, is substantially richer than those accounts reflect.

Panel: Psychology Takes Flight: Exploring the Role of Psychologists during World War II

The Second World War was a turning point for the field of psychology in the United States. Psychologists – both enlisted and civilian – were involved in every branch of the military, conducting original research that responded to a variety of needs. Drawing on military reports, research briefs, printed tests, and apparatus held within the collections of the Archives of the History of American Psychology, this panel dives into specific examples of these contributions, focusing on the role of psychologists within the United States Army Air Forces (AAF). The first paper brings a new layer of complexity to the history of the testing batteries developed for the selection and classification of aircrew personnel by unpacking the collaborative process of translating the tests for use by the French Air Forces. The second paper questions the recorded "stability" of the apparatus tests that formed part of the selection, classification, and training of AAF cadets by tracing the adjustments, modifications, and eventual replacements of the Complex Coordinator Test and the Two-Hand Coordination Test. The third paper steps beyond the realm of testing to explore the development of human factors psychology by tying the field's emergence to research into the relationship between aircraft design and pilot errors. Taken together, these three papers offer new insights into the role of psychologists in the United States Army Air Forces. Research for each paper has been completed as part of preparations for a new public exhibition, *Psychology Takes Flight*, scheduled to open in 2025 at the National Museum of Psychology in Akron, Ohio.

Qualities of a « bon pilote »: Exploring U.S.-French Collaborations to Select and Classify Aircrew in WWII

Cassidy Kuhar and Jennifer L. Bazar (Cummings Center for the History of Psychology)

The roles of psychologists in the war effort expanded exponentially in the Second World War, with over 1,000 psychologists in the United States working directly with the Army, Air Force, or Navy, and another 500 contributing as civilians (Faye, 2011; see Capshew, 1999). The vast majority of these numbers were involved with the Army Air Forces (AAF). They conducted research, designed apparatus and equipment, and trained administrators and instructors. The earliest – and most-focused – area of research involved the development of testing batteries for the selection of aircrew personnel and their classification into the positions of pilot, bombardier, navigator, mechanic, and gunner. The evolution of this program has been well-studied, but international collaborations that emerged from it have been

largely left unexplored. This paper presents a case example of these collaborations, focusing on the co-creation of a psychophysical selection and classification program by the French Air Force and U.S. psychologists.

With the reactivation of the French Air Forces in 1942, French cadets were sent to Allied countries for flight training. The Institute de Psycho-Technique et de Biométrie [*Psychotechnical and Biometric Institute*] was founded towards the end of the year at the University of Algiers and was directed by Jean Malméjac, a physiologist and flight surgeon (Neverre, 1976). The Institute was tasked with researching and developing tests that would aid in the selection and classification of French aircrew candidates (Malméjac et al., 1946; see Timbal, 2008). Malméjac's work peaked in 1944 as the result of a conference where psychologist John C. Flanagan, a Colonel in the AAF Psychology Division, shared the results of aptitude testing among French and U.S. candidates with members of the Joint Air Commission at the North African Theater of Operations (DuBois, 1947). Malméjac moved forward with his work at the Institute by formally requesting aid from the United States. Philip DuBois, an American psychologist working with the AAF, was selected for the mission due to his familiarity with military testing and knowledge of the French language (see DuBois, 1944; 1947). Over the course of five months in 1944, DuBois and Malméjac worked collaboratively to translate and implement the U.S. testing battery for French use.

This process was not a simple one-to-one translation. Adaptations were required that accounted for cultural and experience differences between U.S. and French Air Force candidates. These cultural adjustments applied both to written tests, such as the General Information Test, Reading Comprehension Test, and the Biographical Data Blank, as well as instructions provided for apparatus tests (DuBois, 1947; DuBois et al., 1945; Malméjac et al., 1946). Once established, both men continued to conduct research on the newly translated battery. For his part, Malméjac continued to adapt and fine-tune the psycho-physiological testing battery for use across all French Air Force personnel recruitment and reclassification efforts. State-side, DuBois used the new battery with French candidates who had been eliminated from the pilot program in order to reclassify them as bombardiers, navigators, or gunners.

Drawing on English and French archival and published sources, this paper will explore these international collaborations that led to the development of the selection and classification battery used by the French Air Force. Sources include declassified U.S. military reports, photographs, archival training and orientation films from the AAF and War Training Service, bilingual versions of written psychological tests, and psychomotor tests, including artifacts from the AAF School of Aviation Medicine held by the Archives of the History of American Psychology. World War II-era published sources prepared by French and U.S. psychologists, physicians, and military personnel have also been consulted.

Melton's Contradiction: The Evolution of Three Apparatus Tests used in the Army Air Forces Psychology Program during World War II

Rita Sausmikat (The University of Akron)

During World War II, the Army Air Forces (AAF) employed psychologists to develop methods of selecting, testing, and training aircrew in several Psychological Research Units (Alluisi, 1994, p. 12-13). One method developed by the newly formed Army Air Force School of Aviation Medicine included a battery of twenty standardized tests, which included six psychomotor tests using apparatuses. The apparatus tests were designed to break down complex performance tests into individual components, such as coordination and speed of response (Melton, 1947, p. 985).

Arthur W. Melton, one of the lead psychologists of the AAF Psychology Program, stated that the apparatus tests remained structurally "stable" throughout the war (1947, p. 4). However, Melton contradicts himself throughout his *Apparatus Tests Report No. 4* by noting several revisions to most of the apparatuses, with a few even being completely replaced. Can an apparatus truly be considered

“unchanging” or “steady” if it has experienced corrections, modifications, and replacements? Like Theseus’ paradox, is the apparatus the same object if most or all of its parts were replaced? This research explores the life and evolution of three apparatus tests --the Two-Hand Coordination, the Two-Hand Pursuit, and the Complex Coordinator tests— to contradict Melton’s statement of stability. The Complex Coordinator Test and the Two-Hand Coordination Test were employed in the AAF School of Aviation Medicine from the beginning of the program since they showed the highest validity of the battery tests (Melton, 1947, p. 4). Derived from earlier apparatuses (O’Rourke in 1926 and Mashburn in 1931), the Complex Coordinator matured through five different models and one name change, referenced as the S.A.M. Complex Coordination Test starting in 1941. Models B, C, D, and E experienced changes to the design and structure of the apparatus, including but not limited to the elimination of spring tensions, the total replacement of the selector switch assembly, the redesign of the chair adjustment assembly, and the reconstruction of the chassis in bronze versus wood. These models all differed in characteristics that affected the scoring and the validity of the tests, so regular improvements were vital to the test’s continued reliability.

The Two-Hand Coordination Test was used in the standard classification battery from July 1942 till June 1945 when it was replaced by the SAM Two-Hand Pursuit Test (Melton, 1947, p. 177). Based off an earlier design, the SAM Two Hand Coordination Test (CM101A) was constructed by Carl Heinrich for the AAF Psychological Research Unit #2 in 1942. Structural characteristics of the Heinrich CM101A apparatus made this model unsuitable for mass testing due to its flimsy construction, including jerkiness in the operation of the control handles. The resulting revised CM101B model notably changed the apparatus structure for durability and assembly-line production (Melton, 1947, p. 184). The B Model also had improved control smoothness due to the development and installation of the SAM Smoothness of Control Measure recorder onto the chassis of the Coordination Test unit. By June 1945 however, the Two-Hand Coordination Test was completely replaced by the Two-Hand Pursuit Test, which appeared to have slightly higher validity and reliability than the former test. The design of the Two-Hand Pursuit Test was essentially a stripped-down form of the Two-Hand Coordination Test, using the same lathe style controls. An experimental Pursuit Test model (CM810A) was developed by L. L. Thurstone in 1942, though only one standard classification model (CM810B) was used in the AAF School of Aviation Medicine (Melton, 1947, p. 247).

This research uses Melton’s own reports and references on the Complex Coordination Test, the Two-Hand Pursuit Test, and the Two-Hand Coordination Test to disprove the belief that these tests remained stable and relatively unchanged during WWII. Examining the evolution of each apparatus test reveals stories of modifications, alterations, and succession.

Applied Psychology Takes Off: The Birth of Human Factors

Joseph M. Kaltenthaler and Jennifer L. Bazar (Cummings Center for the History of Psychology)

During the 1940s, aircraft design saw drastic advancements in both engineered complexity and mechanical capability. The speed at which these machines were improved sometimes came at the expense of the pilots who could not always keep up with such quick leaps in technology. Due to this unforeseen consequence, pilot error incidents were common and widespread within the United States Army Air Forces (USAAF) (Fitts, 1947a; 1947b; Fitts & Jones, 1947a; 1947b). Research on the relationship between equipment design and pilot error was initiated by psychological divisions within the Armed Forces during the Second World War. In particular, the Psychology Branch within the U.S. Army Air Forces’ (AAF) School of Aviation Medicine carried out a series of studies on problems resulting from equipment design (see Fitts, 1947a). But later studies demonstrated a persistence of the problem. In 1947 a survey found that nearly all pilots were recorded to have made control errors regardless of experience or skill (Fitts & Jones, 1947a; 1947b). Due to this substantially high rate of errors recorded amongst pilots, the USAAF turned again to psychologists to assist in the engineering of aircraft with a focus on the human-machine causes of pilot error. Research from this period drew

particularly on discrimination tests to assess a pilot's ability to differentiate between control systems in the cockpit under various conditions in order to resolve design elements that hindered pilot performance (e.g., Jenkins, 1947a; 1947b). The results of this work represent the origins of what is today human factors psychology (Fitts, 1947b). This paper will explore the field's development by examining psychologists' influence on aircraft design during and immediately after the Second World War.

At the close of the Second World War the USAAF shifted their approach on the application of psychologists in the service. Throughout the conflict, psychologists were mainly regulated to assess and train personnel until late 1945, when much of the research began focusing on the interaction between pilot and equipment (Pew, 1994). This began the Army Air Force Aviation Psychology Program, widely considered the birth of human factors psychology (Pew, 1994). At the forefront of this program was Paul M. Fitts (1912-1965), today considered one of the founders of human factors psychology. His work was instrumental in the inaugural research that addressed this impact between design and human performance. Fitts joined the war effort in 1941, becoming Director of the Psychology Branch of the Aero Medical Laboratory at Wright Field in Dayton, Ohio when it first opened four years later (Pew, 1994). This saw the joint efforts of psychology, engineering, and other fields enter into the design and refinement of aviation technology. This work led to the creation of the Human Factors Society (HFS) with some sources even attributing the naming of *human factors* to Paul M. Fitts and his colleagues (Pew, 1994). Since the field's establishment in 1941, human factors psychology has rapidly expanded and become one of the largest sectors of applied psychology (Pew, 1994).

Information regarding the development of human factors psychology, as revealed through pilot-aircraft research, will be informed by a mix of archival and published sources from the Army Air Forces Aviation Psychology Program and the Psychological Branch of Aero-Medical Laboratory available through the Archives of the History of American Psychology.

The Beginnings of the American Space Program: Research Contributions from Human Physiology and Comparative Psychology

Patrick Drumm (Ohio University Lancaster)

In the spring of 1946, a stripped-down B-17 bomber powered by modified engines took off into the stratosphere. In the rear fuselage, John Paul Stapp, M.D., Ph.D., conducted experiments at sub-zero temperatures. He was studying the effects of high-altitude flight using himself as a subject to determine the parameters of human survival there. Could humans function physically and rationally, or even avoid incapacitation from the bends (i.e., generalized barotrauma)? The answer he found entailed breathing pure oxygen. Stapp's next assignment, to study the physiological effects of acceleration and deceleration, required developing a rocket sled. Its engineering demanded several iterations. Stapp himself rode the final versions in multiple tests (Ryan, 2016). But before he did, nonhumans occupied those rocket sleds as they were safety tested. Many did not survive (Burgess & Dubbs, 2007).

The aftermath of World War II saw the parallel development of rockets to carry payloads and soon organic passengers into space. Fatalities accompanied those efforts as well (Burgess & Dubbs, 2007; Dubbs, 2003; Gray, 1998; Reuter, 2012). Eventually advances in rocketry allowed two chimpanzee astronauts to ride rockets into space in 1961. Those chimpanzees, Ham and Enos, paved the way for successful launches of human astronauts in the space race between the United States and the Soviet Union. Ham's January mission preceded the sub-orbital flights of Alan Shepard, Jr., and Gus Grissom. In November of 1961, Enos circled the Earth twice before splashing down near Bermuda. Shortly thereafter, in February of 1962, Colonel John Glenn, Jr. orbited the planet three times, surpassing Soviet cosmonaut, Yuri Gagarin's record. Gagarin, the first human in space, orbited our planet just 71 days after Ham's suborbital flight (Cassidy & Davy, 2008; Burgess & Dubbs, 2007).

Ham and Enos were but two of many chimpanzee participants in the Holloman Air Development Center colony. Most had been collected in Cameroon, and they initially rode the rocket sleds before the Project Mercury launches, all deemed necessary to evaluate the physical risks humans would face in space (Meeter, 1967). Chimpanzees' intelligence made them ideal subjects for behavioral training, and they became more than mere passengers. They learned to perform tasks in their capsules to gauge the effects of space flight on mental functioning. Ham, for example, learned to operate a lever when a flashing blue light appeared. Failure to do so resulted in punishment (i.e., a mild shock delivered to his feet) but success earned him a banana chip reward. Enos learned oddity problems with three levers positioned below three symbols, two being identical. He needed to press the lever below the unique symbol. Failure to choose correctly resulted in a shock delivered to his feet. In summary, the training of both chimpanzees entailed avoidance conditioning. Unfortunately for Enos, who performed well during training, his console malfunctioned in orbit. The same oddity problem repeatedly appeared on his display, for which pressing the middle lever was the correct response, and the switch registering depression of that lever also failed. Thus, he received numerous undeserved shocks. Another problem involving the orientation of his spacecraft necessitated an abort of the mission after two orbits. His capsule landed well beyond the original target landing area. Confined in the floating capsule for over three hours as the recovery vessel steamed to reach him, he destroyed some physiological sensors attached to his body and damaged his cabin (Madrigal, 2011).

From an early 21st century standpoint, two issues of concern arise from the foregoing account. One is that most learning theorists even at that time considered reward more potent than punishment in conditioning behavior. Perhaps a military mind set, in which inappropriate behavior gets punished, prevailed among their Air Force trainers. Also, the ethics that apply to nonhuman research subjects have dramatically changed.

Although overshadowed by their human successors in space, Ham and Enos enjoyed a period of celebrity. Ham retired to live 23 years beyond his successful flight, but Enos died of bacillary dysentery nine months after his. The story of their behavioral training, arguably misguided by contemporary standards, remains an interesting footnote in psychology's history.

Controversy at the Boundaries: The Instinct Controversy c. 1890–1930

Caitlin Mace (University of Pittsburgh)

The Instinct Controversy c. 1890–1930 (sometimes referred to as the 'instinct wars' (Burghardt 2009) or the 'instinct debate' (Hampton 2004, 2006)) was a series of interrelated and multifocal debates regarding what instincts are, in what form they exist, what kinds of creatures have them, how to study them, and, ultimately, whether to study them. In the end, the question at the heart of the instinct controversy was this: does 'instinct' have a place in a scientifically legitimate study of humans and non-human animals? The answer appears to have been in the negative for psychologists studying humans in the late 1920s. But the reason for this repudiation of instinct isn't clear in the literature. The main reason for this shortcoming is that the complicated nature of the instinct debates has resisted an overarching narrative—the debates took place in multiple developing fields with few disciplinary bounds and at the fringes of other philosophical, theoretical, and scientific debates, without any standardized use of the term 'instinct'. Despite this, many historical diagnoses have been suggested as to why appeal to instincts in science went into decline in the 1920s. To provide some clarity, I use a data-driven approach to characterizing the decline of 'instinct', showing that the decline and much of the controversy was specific to psychology in the United States. The use of 'instinct' declined through the 1920s generally, particularly after 1926 in American psychology and since 1911 in comparative psychology. Moreover, there was an increase in articles engaged in the negative project of arguing against use of 'instinct' in American psychology and comparative psychology in the 1920s. But the use of 'instinct' did not decline to the same extent or at all in other scientific fields or in some areas of psychology. These data are consistent with the

consensus judgment in the historical literature that instinct was not considered to be a worthwhile and legitimate focus of scientific study by the end of 1920s. Further, I suggest that this outcome of the instinct controversy was not due to a resolution of controversy by compromise, sound argument, or consensus. I argue that the decline of use of ‘instinct’ signals a loss of interest in the instinct controversy among American psychologists. But rather than loss of interest due to generational change or death (see Kuhn 1970), this loss of interest was a result of working out scientific legitimacy issues at the boundaries of psychology. I begin by characterizing the decline via data from a full corpus search for use of ‘instinct’ in major psychology, physiology, and zoology journals. Then, I provide an overarching narrative of the instinct controversy, highlighting the aspects of instinct psychology that were controversial. Finally, I show how the instinct concept became associated with five non-scientific or illegitimate features of some parts of early psychology, which include dead-end research programs, non-experimental methodologies, or non-empirical posits. I conclude that the rejection of instinct as a scientifically legitimate concept and subject of study was a means to secure scientific legitimacy in American psychology.

The Demise of Exceptional Subjects in Psychology: The Case of Clever Hans ***Daniela Barberis (North Central College)***

This paper will discuss a parallel transformation in animal and human performers as they were turned into subjects of psychological study—into appropriate scientific “subjects.” This transformation entailed the removal of their agency, their reconceptualization as machine-like entities who went from ‘acting’ to ‘behaving.’ While this sort of transformative reduction has been discussed by various authors, the fact that the change was analogous across humans and animals seems to have gone by without comment. I will focus on the famous case of “Clever Hans,” a performing horse. We will see how Hans was transformed from a wondrous performer into a purely reflex machine, where the deprivation of agency is the alleged “price” to pay for “objective” knowledge.

The interpretation of Clever Hans’ case has gone through a series of reversals. Eileen Crist has shown that Hans’s case, as written up by psychologist Oscar Pfungst in 1911, is interesting not just because it is “one of the most seminal works of behavioral psychology of this century,” but because it presents a reversal of assessment of the animal’s performance from answering questions to emitting responses to stimuli. In her work on Clever Hans, philosopher Vinciane Despret has pointed out that the “Clever Hans effect” has become the name of a cardinal scientific sin: the experimenter effect in which researchers inadvertently give their subjects the answers to their questions. She argues, however, that this accusation has been overstated and that there are multiple, and multi-directional forms of influence between observers and observed, some of which can be salutary and revelatory. Hans’ cleverness was turned into an ironic epithet, an accusation of stupidity by Pfungst, but it was recast by Despret as an alternative intelligence, a remarkable capacity to be affected by the handler. By looking at the reversals of “what was really going on” during these performances, I will reflect on what we can learn from these cases as to what it means to “do” science.

The Rise of Null Hypothesis Significance Tests as the Gold Standard of Statistics in Psychology Textbooks ***Spencer Arshinoff (York University)***

The ongoing replication crisis has compelled psychologists to take a closer, more critical look at statistical methodology. Null hypothesis significance testing (NHST) is the most used method by far, despite a long history of scrutiny beginning immediately following its introduction into the field. Its ubiquity is such that many psychologists labour under the false assumption that NHST is objective, infallible, and fundamental to psychology itself, rather than one of a plethora of statistical tools. NHST is

usually taught as a strictly formalized procedure, with no discussions of its historical origins and originators, or the context in which it was adopted into psychology, giving a false impression that it is as old as psychology itself. In reality, NHST did not see widespread usage in psychology until the so-called “inference revolution” of 1940-55, by the end of which it had gained the near-universal status it enjoys today.

The form of NHST used by contemporary psychologists has been criticized as a hybrid. While its origins primarily lie in the null hypothesis testing of Sir R.A. Fisher, the pre-eminent statistician of the 20th century, it also incorporates concepts from an opposing model of statistical inference: Neyman-Pearson decision theory. Neither Fisher, Neyman, nor Pearson were psychologists, but their work was picked up by textbooks used in psychological statistics instruction in the 1940s. Textbooks have taken some of the blame for the pervasiveness of this hybrid model, as the majority were written by psychologists, not statisticians. Several recent reviews have been conducted identifying errors relating to NHST in contemporary textbooks used for psychological statistics, but less work has been done on the “first wave” or textbooks, which would have carried the message of NHST.

I reviewed nine statistics textbooks that I identified as frequently used in psychology departments during the period of the inference revolution, as well as every English-language review of these textbooks published in academic journals, which totaled to 70 reviews. Most textbooks were authored by psychologists rather than statisticians, though several had a significant background in statistics. Many of the textbooks presented aspects of both Fisher and Neyman-Pearson theory in their explanation of null hypothesis testing, though it was often attributed to Fisher alone. Several of these textbooks contained fallacies or factual errors regarding NHST, such as J.P. Guilford’s *Fundamental statistics in psychology and education* and Quinn McNemar’s *Psychological statistics*. Others were vague or confusing, or left out crucial information, such as the reasons that 0.05 is typically used as the cut-off value for significance. However, none explicitly laid out the hybrid model of NHST that is used today, suggesting that the textbooks are not the sole culprit for its uptake.

The textbook reviews I analyzed rarely took authors to task, focusing primarily on their usefulness in the classroom rather than the validity of their content, suggesting that ease of use played a larger role in textbook selection than the content itself. As such, the instructors who selected these textbooks were no less important in the spread of NHST than the textbooks themselves. Since a common criticism of NHST is its oversimplification of statistical inference, and the lack of critical thinking required to use it, its uptake in psychology can be attributed to the same sort of intellectual indiscretion that keeps it so common today.

How Do Intellectuals Write About Intellectual Disability?

Heather Murray (University of Ottawa)

This paper explores the seeming paradox of intellectuals writing about intellectual disability. While community collaboration scholarship that is accountable to its research “subjects”, as well as alliance building, have been central in the historiography of some vulnerable populations, most notably Indigenous people, this research demeanour arguably has not been as explicit or robust for historians of the social sciences. How have historians writing about intellectual disability acknowledged their subject positions when writing about this area? Are conversations about community collaboration scholarship happening in other sub-disciplines portable to thinking about cognitive disability? How can historians avoid the potential hubris in the act of “giving voice” to people with intellectual disabilities, and how can they avoid extractive practices in their research? How can historians respect these often vulnerable populations that they are studying while still being critical and is being critical itself a research demeanour that could be problematized or re-imagined?

In this paper, I first explore how it is that historians have acknowledged their subject positions as intellectuals when writing about these issues, often in the form of mentioning a family member with a disability. I then discuss how complicated the prospect of community collaboration scholarship might be when people with intellectual disabilities are expressly excluded from most university environments.

In addition to my historiographical aims, I explore a deepening perception, over the course of the post-WW2 period, that intellectuals are inherently aloof and cold-hearted, taking humanity to the brink of destruction, while those with intellectual disabilities are uniquely kind and sensitive. To do so, I analyze an array of primary documents related to children with intellectual disabilities in postwar America, from organizations geared towards them and psychiatric/psychological perceptions of young people with intellectual disabilities in the archival papers of Grover Francis Powers, a pediatrician who helped found the Southbury Training School for Mentally Retarded Children in Connecticut, Lisa Kuhmerker, a children's educator and moral psychologist who studied instances when children with intellectual disabilities were not chosen for play, as well as the Association for Retarded Children of America, and state-level versions of these organizations, such as the Pennsylvania Association for Retarded Children and the Minnesota Association for Retarded Children. These official organizations, and even psychiatric observers, often advanced a "special citizens" rhetoric by suggesting that children with intellectual disabilities are often in a good mood, kind, and oblivious to the problems of the real world, such as competition and capitalism, that bedevil their parents and teachers. In these, I analyze how it is that this simultaneous sentimentalization and ethereal-ization of children with intellectual disabilities came about and how it is that these children came to embody what was good about human nature vs. what was considered quite rotten about it. I also explore the origins of the concept of "emotional intelligence" as it manifested in children with learning disabilities. Finally, I link these primary sources to my original historiographical questions in pondering whether it is the case that historians writing about intellectual disability have inadvertently reproduced these deeply ingrained postwar logics about their good and pure subjects with intellectual disabilities who have an emotional wisdom that they do not have, positioning themselves as in need of absorbing their virtue.

Elizabeth Scarborough Lecture

The Entanglement of Scientific Racism and Organized Antisemitism In the Career of Roger Pearson

Andrew Winston (University of Guelph)

The death of anthropologist Roger Pearson in 2023 at age 95 went unnoticed in the mainstream press and academic circles. But on the websites of white nationalists, neo-Nazis and anti-immigration groups, his death was featured as a major loss of an important leader. Historians of scientific racism, such as John P. Jackson Jr. and William Tucker have described Pearson's role as a Nordacist organizer and publisher of extreme antisemitic material in the 1950s and 1960s, and as editor of *Mankind Quarterly* in the 1980s and 1990s. Pearson transitioned from neo-Nazi activist to academic anthropologist and then to independent publisher of three quasi-academic journals and numerous "race-realist" books. He did so while steadily enlisting and collaborating with prominent mainstream psychologists and academics of other disciplines who shared an interest in race. How these transitions and collaborations were managed needs further analysis. I describe how Pearson became a self-proclaimed champion of academic freedom despite having years earlier replaced existing anthropology faculty at the University of Southern Mississippi with three colleagues devoted to neo-Nazi ideas and projects. His promotion of antisemitic themes continued, but with a more subtly coded and cautious discourse, using anti-Marxism as an effective screen. I stress the role of lies, personal charm, and

“collaborative circles” in understanding Pearson’s success in harnessing fears of immigration and demographic change in the late 20th century.

In my own email correspondence with Pearson in 2005, he denied many aspects of his early projects, but inadvertently revealed that he had not entirely abandoned his 1950s position on the Third Reich. He hoped I would correct my alleged misrepresentation of his work. Pearson’s case illustrates broader questions in the historiography of scientific racism, including the sanitizing of careers, the production of “alternative” histories, and the neglected role of “background figures” who organized and facilitated the revival of 19th century racial ideas. I situate the issues of impression management in the context of the mythologized vision of a value-free psychological science that gained increasing influence after WWII and the McCarthy era. This normative climate made inquiry into the ideological commitments and activities of Pearson and his co-workers both unnecessary and unsavory, allowing for a collective disciplinary ignorance and denial that has continued to the present.

Mind Your Boundaries!: The Emergence and Flourishing of a form of Psychological Life

Jill Morawski (Wesleyan University)

In his 1969 APA presidential address, George Miller called for giving away “one of the most revolutionary intellectual enterprises ever conceived” for human betterment (1969, 1065). Fearing the public’s self-conceptions of human control being promulgated by behaviorism Miller instead promoted an image of human nature assumed in the emerging cognitive psychology: rational, creative, ingenious, and self-directed. Giving away this new science, however, would not be psychologists’ responsibility. Rather, “The people at large will have to be their own psychologists, and make their own applications of the principles that we establish” (1071). Non-psychologists’ practice of this psychology “will inevitably change people’s conception of themselves and what we can do.”

Encamped in the world of experimental psychology and academic liberalism, Miller overlooked the revolutionary enterprise long ago underway: psychologies produced outside the academy. Underestimated by him was the enthusiastic, omnivorous uptake of these psychologies by an eager public. Now, use of psychology as an explanatory tool in everyday life is ubiquitous: turbo charged, transported, and mutated via social media. No longer (if it were ever the case) can one readily distinguish popular psychology produced by scientific experts from the popular psychology produced by others — despite psychologists’ early efforts to distinguish their own popularizations from the so-called psychologies proffered by so-called “quacks” (Morawski & Hornstein, 1991). Scholars of recent self-help psychology industry have documented its circulation, proliferation, impacts, and ideological tones (for example, Kaminer, 1994; Greenberg, 1994; Binkley, 2014). They have found complex, nonlinear mediations between psychological knowledge, therapeutic enterprises, and the public (Pettit & Young, 2015; Stam, 2014) and have queried the canonical distinction between ‘psychologists’ and non-psychologists that privileges PhD psychologists (Devonis, 2021). Such studies illuminate the status hierarchy of psychological knowledges and indicate the importance of taking the so-called self-help industry more seriously as an extensive, heterogeneous, and impactful enterprise.

This paper traces the emergence, proliferation, and wide-ranging impact of one popular theory: boundary psychology. First elucidated in book form in the early 1990s (Whitfield, 1993; Katherine, 1994), boundary psychology since has expanded tremendously; it now includes over 175 books, 24 published in the first 3 months of 2024. Its psychological premises and how-to instructions now appear across social and print media and are even incorporated in institutional practices. This psychology proceeds with the premise that individuals have inadequate psychological boundaries, indicating a fragile self, and that they need to build and vigorously protect their boundaries. Given its successful proliferation, many people now understand psychological boundaries and their protection as common sense (for example, Urban, 2022). The paper’s title, “Mind your Boundaries” gestures our attention to this surprisingly enduring project that

instructs people on constructing and guarding their psychological boundaries. The paper also evidences the dissolving if not already collapsed disciplinary jurisdiction that Miller aimed to preserve. Offering a ‘history of the present’ the paper “targets an aspect of contemporary forms of life that can be seen as particularly disconcerting and at the same time indicative of what is ethically and political at stake in the present moment” (Koch, 2021, p.3). Historical analysis of the boundary psychology genre finds it constructed through a bricolage of concepts and theories of the self. Further, contained in nearly all boundary psychology writings and media reports is a core logic, an entwining of psychological and moral logic. Scrutiny of this logic illuminates how boundary psychology offers recourse to perceived threats of contemporary life and simultaneously sustains certain political-economic conditions.

Achieving Procrastination: Constructions of the Working Subject *Ian Davidson (Concordia University of Edmonton)*

The psy-disciplines have long played a role in sustaining and enhancing labour performance. While the general Western political-philosophical tide turned towards neoliberalism in the 1970s and onward, the positive power of self-governance made the entrepreneurial quality of psychological health seem all the more obviously true. Still today, popular critiques point to this arguably now totalizing view of the working subject expanded into the whole subject, whether those critiques be from antiwork movements, or popular philosophical takes on the excessive positivity and consequential burnout endemic to our achievement society (e.g., Han, 2015). Within various psychological research during the latter-twentieth century, a focus on the expansive concept of self-esteem gave way to multiple forms of self-regulation (Pettit, 2020). Concurrently, experimental investigations into decision-making processes famously dismantled the traditional political economic view of a rational, self-maximizing human nature, leading to a behavioural economics.

Towards the millennium, the new logic of rationality rested on restoring the self-governing enterprising virtues of perseverance and responsibility. Several strands of psychological research, from social to newer forms like positive psychology, would chart atlases of our cognitive limitations and moral failings. Suggested models of ideal human subjects, and interventions for growing towards the ideal, were often bound up in the already established formulation of employment as a right, expectation, and moral salve to most problems. Work is the meta-project containing all other self-projects within the working subject. Loss of employment was now a psychological issue that devastates the “full motivational commitment to that series of minute projects that constitute the life of the person” (Miller & Rose, 1986, p. 156). For the working subject, all of life is projects of self-enterprise, modelling the forms of personal success and failure on what would be understood as success and failure within the context of work.

Psychologists were expanding and revisiting ideas within these political and cultural changes. For example, while research on learned helplessness first emerged within a comparative behavioural context as an environmental explanation for lack of activity or effort in the organism, it was extended into the human subject as a possible model of depression. By the 1980s, a cognitive revision of learned helplessness refashioned the problem as one of cognitive style and predisposition—setting up the path toward an initial positive psychology of learned optimism, perseverance, grit, and other character virtues. Here we can see the move towards self-regulation/self-control, but also the achievements of the working self becoming the normal and desirable template for personal wellbeing.

This paper will focus on related set of psychological concepts folded under the label “procrastination.” While psychologists and psychiatrists had long been interested in self-destructive behaviour, like patterns of behaviour destroying relationships, in the 1980s psychologists began reconfiguring this notion within the working subject’s framework of production and achievement, such as a fear of success (e.g., Pappo, 1983). Social psychologists defined self-destructive/defeating behaviour as having any “negative effects on the self or on the self’s projects” (Baumeister & Scher, 1988, p. 3). Here procrastination was proposed as a “likely candidate for study” among the major self-defeating

behaviours (Baumeister & Scher, 1988, p. 18). More recently in popular psychology and culture, procrastination (alongside its purported causes and solutions) is widely understood as a primary form of self-sabotage that thwarts the very potential of achievement through inactivity (e.g., Anderson, 2011; Rosner & Hermes, 2006; Stephenson, 2009). In this historical survey, I will attempt to trace the shaping of procrastination as a problem of (un)employment and other self-projects, as well as any research touching on modifications to the problem, such as the rebalancing act of “productive procrastination” or more genuine resistances to the self as worker.

Feminist Psychologists and Therapeutic Culture: A Transatlantic Conversation on the Psychologization of Feminism

Stéphanie Pache (Université du Québec à Montréal)

The political critique of psy sciences oscillated in postwar America between the denunciation of psychiatric power and the expansion of a therapeutic culture that would be the doom of society and democracy. The different critical assessments of the role and power of mental health professionals and psychological knowledge form a complex set of ideas that cannot be mapped systematically in neat political boxes: both progressives and conservatives have expressed worries about the expansion of psychology’s authority. In the United States, from the 1960s to the 1980s, criticisms of therapeutic culture were often considered conservative. Early critiques of the “therapeutic society” (Rieff, 1966; Szasz, 1961), or of the narcissism and individualism that it would engender (Lasch, 2018[1979]; Sennett, 1977) could either emphasize institutional arrangements -as in the case of Szasz and his critique of the judicial uses of psychiatry- or constituted a cultural and moral critique that paradoxically borrows the vocabulary of psychoanalysis (Rieff, Lasch).

Retracing the history of “therapeutic culture” represents a challenge since the expression has been conceived as a contemporary critique of specific phenomena. This expression labelled negatively processes related to the expanse of psychology and therapy in society, which many of these critical authors associated with the rise of neoliberalism and its individualistic ethos (Castel, 2011; Rose, 1996). Documenting the phenomenon, particularly historically, is difficult and often confuses several aspects to be distinguished: extension of the field of application of psychotherapeutic practices; extension of psychotherapy expertise to objects previously assessed by other disciplines; dissemination of psychotherapy language; hegemony of psychotherapy language/expertise in everyday life or other areas of activity; etc. This paper will examine the history of “therapeutic culture” and the notion of psychologization through an analysis of a debate among feminist psychologists on feminist therapy, the psychologization of feminism, and the role of psychology in feminism.

The examined exchanges took place in the early 1990s. The publications of three preeminent feminist psychologists serve as the main empirical sources: on the North American side of the Atlantic, the American Laura S. Brown and her support to a politicized psychology and therapeutic tools (e.g. Brown, 1994, 2010); on the European side, Celia Kitzinger and Rachel Perkins, British feminist psychologists who accused feminist therapy of depoliticizing feminism and called to reject psychology (Kitzinger & Perkins, 1993). These respective positions show how complex it is to disentangle the definition of psychologization from a specific political point of view involving our conceptions of power relations and agency. They also illustrate a certain degree of confusion in this debate on the political role of psychic disciplines. By the examination of these intertwined issues, the paper will contribute to question the possibility of redefining therapeutic culture as an analytical concept without its critical and normative meaning, and, if so, what it could be and how the historical feminist engagement with psychology could be a roadmap for it.