

PERSONAL POLITICS:
THE RISE OF PERSONALITY TRAITS
IN THE CENTURY OF EUGENICS AND PSYCHOANALYSIS

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Abstract

This dissertation documents personality psychology's development alongside psychoanalysis and eugenics, offering a disciplinary and cultural history of personality across the twentieth century. Using the psychological concepts of neurosis and introversion as an organizational framework, personality's history is portrayed as one of "success:" a succession of hereditarianism and its politics of normativity; a successful demarcation of the science of personality from competing forms of expertise; and a successful cleansing of personality psychology's interchanges with unethical researchers and research.

Chapter 1 provides background for the dissertation, especially focusing on turn-of-the-century developments in the nascent fields of American psychology and the importation of psychoanalytic ideas. It ends with a look at Francis Galton's eugenicist and statistical contributions that carved a key path for psychological testers to discipline psychoanalytic concepts.

Part I details the rise of personality testing in the USA during the interwar years, while also considering the many sexual and gender norms at play. Chapter 2 tracks the varied places in the 1920s that personality tests were developed: from wartime military camps to university laboratories to the offices of corporate advertisers. Chapter 3 takes stock of popular psychoanalytic notions of personality alongside the further psychometric development of personality testing. These developments occurred at a time when American eugenicists—including psychologists—were transitioning to a "positive" form that emphasized marriage and mothering.

Part II partially strays from a strict chronicling of the Big Two's development into

traits—neuroticism and extraversion—to consider the broader histories of personality in the Cold War era and beyond. Chapter 4 considers the opposing legacies of Hans Eysenck and Paul Meehl to explore the development of psychometric tools that countered popular projective techniques. Additionally, it examines the multifarious connections between psychoanalysis and psychologists striving for a science of personality. Chapter 5 closes the dissertation with a look at the psychometric work that led to the Five-Factor Model’s ascent in the 1990s as perhaps the most widely accepted perspective on personality. Along the way, the conservative politics of heredity and eugenics would capitalize on cries for the “academic freedom” of racist science while justifying trait psychology’s past.

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Introduction

Putting Personality in its Place

“...all these were on the billboards around him, systematically linked in some self-referring relationship that had a kind of neurotic tightness, an inescapability, as if the billboards were generating reality...”

- Don DeLillo, *Underworld* (1997)

In March 2018, a whistleblower alerted the press to his employer’s large-scale data harvesting of over 87 million Facebook users (Cadwalladr, 2018). Cambridge Analytica Ltd. (CA), a political consulting firm and subsidiary of the private British behavioural research company Strategic Communication Laboratories, had acquired this Facebook dataset in 2015. Sensational pieces about hacking democracy, mass breaches of personal data, and extreme right-wing-funded and possibly Russian-influenced propaganda quickly flooded the press. The few details of the scandal that were made public were so entrenched in byzantine plots that journalistic outlets had to start creating landing pages and diagrams to effectively relay the story (e.g., Chang, 2018; Confessore, 2018). Yet journalists already long knew about CA’s harvesting of millions of Facebook users’ data to sell to conservative-causes, such as Republican Ted Cruz’s failed presidential campaign (Davies, 2015).

In terms of political motives, connections were easy to find: Robert Mercer, a billionaire hedge-fund investor who basically has a career as a right-wing donor, was one of the main investors of CA; directing its data analytics toward the Leave campaign leading up to the 2016 UK-EU membership referendum (or, the Brexit vote; Cadwalladr, 2017a, 2017b; Doward et al.,

2017). Steve Bannon, once President Trump's Senior Counselor and a founder of the white supremacist propaganda website *Breitbart News*, was CA's Vice President, further ensnaring the company's uses of data in pushing for Trump's successful campaign—itsself ensconced in the endless prevarications of Russia's involvement in the 2016 US Presidential Election (Cadwalladr, 2018; Illing, 2017).

What set the CA scandal aflame was its whistleblower's revelations. Those included not only the gobsmacking update about the sheer quantity of online data harvested, but the intended purpose of such harvesting: activity eerily akin to psychological warfare—a psy-ops throwback to the Cold War. In the earliest days of CA, using personality profiling to “shift America's culture and rewire its politics” intrigued Bannon. But the traditional markers that analytic firms recorded, such as voting or consumer purchases records, were not prime criteria to determine if a voter was “a neurotic introvert, a religious extrovert, a fair-minded liberal or a fan of the occult;” some of the psychological traits that most interested CA (Rosenberg et al., 2018). Hence the firm's name, data was acquired from a team of psychometric experts working at Cambridge University, who had used a loophole in Facebook's backend. They extracted voluntarily submitted data of Facebook users who installed the Cambridge University team's personality app, but due to the loophole, they also collected the data of all those users' friends (Fair, 2019; Romano, 2018a, 2018b; Schroepfer, 2018). The personality profiling of the CA project was aligned to five traits of personality: openness to experience; conscientiousness; extraversion; agreeableness; and neuroticism. As journalists and their readers would learn, these five domains constituted the Big Five within research psychology.

While some former CA employees contended that data on the Big Five were crucial in their political meddling, such as targeting voters scoring high in neuroticism with images of mass

immigration, some scandal commentators were skeptical about effectiveness of personality science in political persuasion (Gibney, 2018) or even the technical illegitimacy of Cambridge-via-CA's activity (Kavanagh, 2018). Others have noted that the CA scandal is part of a larger history of corporations persuading citizens to gladly fork over their personal data: from clicking through seemingly harmless BuzzFeed quizzes on your phone to completing self-(re)defining Myers-Briggs Type Indicator tests for upper-management at the office (Lussier, 2018b).

How did we arrive here? To better understand the world of shady companies furtively compiling millions of online data points in the hopes of mapping your personality and influencing your behaviour—including how you vote—means understanding better the world of the Big Five personality traits. To do so, we must look to the wider world of personality psychology and its development over the twentieth century. Tucked within the Big Five are what were once referred to as The Big Two: extraversion and neuroticism (Wiggins, 1968). Those traits, and allied notions like neurosis, emotional stability, or personality types, have stood out across the twentieth century as crucial zones for the development of scientific personality psychology. The Big Two, in spirit or in flesh, turn up across this landscape: whether in the most rigorously, statistically-derived personality inventories of academia or the less psychometrically-obsessed popular understandings of personality (like types, disorders, quirks, and character). This dissertation uses the once-called Big Two—neurosis and introversion; or, later, neuroticism and extraversion—as a way to cast the nebulous and overwhelming palimpsest of personality into a navigable map from past to present.

This dissertation starts with a prelude: an opening chapter that lays the foundation for what lies ahead. Chapter 1, “Seeing Double: Origins Notes on Neurosis, Introversion, and the

Other Big Two,” provides an abridged survey of the pre- and early-twentieth century history of the ideas and traditions that informed the rise of personality psychology. Beginning with some detail on the medical study of nervousness in eighteenth- and nineteenth-century Europe, the chapter expands into an introduction on some of the basic tenets of Freud’s and then Jung’s psychoanalyses—paying close attention to their respective work on neurosis and personality types. This prelude also contains background information on turn-of-the-century New England physicians who were at the crossroads of materialist neurology, psychical abnormal studies, and spiritual therapies.

Psychoanalysis was introduced to America within this psychopathologist scene and during the rise of what was called the New Psychology: an emerging scientific approach to studying the mind. Granville Stanley Hall was one of the key scholars working towards disciplining psychology into a recognizable academic field. His institutional influence, as well as his research interests in (what we would now call) developmental psychology, evolutionary theory, and civilized morality would influence many psychologists developing mental tests of ability and characteristics. The emphasis on experimentation and quantification would arguably help Jung’s approach to the unconscious become well-received, at least in its initial methodology. Finally, the influence of Sir Francis Galton—the (in)famous British polymath who helped fuse together mental testing and applied statistics with his science/religion of heredity (what he called “eugenics”)—is also introduced. Among the many avenues of psychometric innovation Galtonian-minded scholars of the mind would pursue, the statistical technique of factor analysis would prove essential to the scientific study of traits—intelligence and personality alike—from the interwar years into the present day.

Part I of this dissertation, bearing the semi-official title of “Norms,” mainly covers the

story of the Big Two during the interwar years. Alongside tracing the theoretical and technical developments within academic psychology toward harnessing the Big Two as measurable traits of personality, Part I situates that disciplinary story in the cultural changes in social norms of sex and sexuality. Building on the work of feminist historians, the two chapters on “Norms” attempt to understand the sexual politics at play during the quantification of personality.

Chapter 2, “Multiple Personalities: Testing for Neurotics and Introverts in the 1920s,” recounts the stories of several lesser-known psychologists as they grappled with psychoanalytic ideas of neurosis and personality types while developing new frameworks and tools of personality. After first looking at how many early twentieth-century experimental psychologists challenged Victorian assumptions of the intelligence differences of males and females, the chapter spends some time exploring the ideas of pioneering physician and psychoanalyst Beatrice Hinkle. She was an enthusiastic Jungian—even translating some of his major works for American audiences—who was also a feminist scholar. For Hinkle, a typology like introvert and extrovert could supersede the socially and psychologically unhelpful divide of male and female. From there, a look at psychologists’ contributions to WWI beyond the implementation of intelligence testing—a story often recounted—places Columbia University psychologist Robert Woodworth’s research on psychoneurotic army recruits as a kickoff for a 1920s boom in personality testing.

In the 1920s, several psychologists across the USA created early forms of personality tests—sometimes in the form of a diagnostic tool from separating the normal from the abnormal or neurotic; sometimes in the form of questions to demarcate introverts from extroverts. Both academic psychologists, such as Edmund Conklin at the University of Oregon, and psychologists working in industry, such as Max Freyd at the J. Walter Thompson Company advertising

corporation, thought it pertinent to domesticate psychoanalytic theory—particularly the Big Two—into their technoscientific framework of measurement, prediction, and control. As with intelligence testing, the sub-discipline of personality psychology would partially coalesce around the desires of those in varied positions of power. Therein, persons were understood through the gazes of management, marketing, and the military.

Chapter 3, “Let’s Talk About ~~Sex~~ Personality: Interwar Psychologies of Neurosis, Introverts, and ‘Positive’ Eugenics,” picks up where Chapter 2 left off: focusing on the further development of a psychologized Big Two and its binds to the politics of sexuality. In the 1930s, reflecting the increasing popularity of psychoanalysis, neurosis, and personality types (especially introverts/extroverts), these ideas were increasingly present in magazine and newspaper pieces. Psychologists, too, especially those developing tests, were still expanding their sights from intelligence to personality traits. For Midwestern psychologists like Leon Thurstone and Joy Paul Guilford—both often collaborating with their wives who were also psychologists—factor analysis, a tool then most often associated with Spearman’s work on general intelligence, was the most fruitful avenue for incorporating the Big Two into inventories of human personality. Alongside those psychometric innovations, a new generation of Euro-American psychoanalysts were shaking up the Freudian orthodoxy of innate drives and presumed sexuality. The life and work of “neo-Freudian” Karen Horney is examined: how she used the concept of neurosis in widely promoting her version of analysis that emphasized cultural determinants of personality and the potential for change; how her views on (what we would now call) gender developed into the 1930s; and, lastly, how her views often contrasted with those of American eugenicists.

The 1930s marked a revision in what historian of eugenics Wendy Kline (2001) calls “reproductive morality:” reflecting a tepid tolerance for the increasing sentiment among

scientists and scholars that the environment and culture itself play critical roles in an organism's individual and evolutionary development, eugenicists shifted toward a "positive" version that stressed the democratic duty of motherhood and the scientific expertise required to counsel racially and sexually appropriate marriages. Along the western coast, the career trajectory of intelligence tester Lewis Terman—alongside his colleagues, agriculturalist Paul Popenoe and the Australian-American race scientist Stanley Porteus—encapsulates this modified form of American eugenics. Terman's work also signified that although sex differences in intelligence might have faded in popularity, psychologists' version of the male/female binary was to become a presumed dimension of personality testing. Chapter 3 ends with an examination of psychologist Robert Bernreuter—a Terman student who created one of the earliest and most globally successful personality inventories which prominently featured neuroticism and introversion. Through Bernreuter and later personality testers, the eugenicist norms of heteronormative marriage and gendered personalities would persist.

Part II of this dissertation, "Values," admittedly strays somewhat from the strict framework of the previous chapters. Readers expecting a comprehensive continuation of how neuroticism and extraversion bloomed in trait psychology should prepare now for disappointment. The multi-site profusion and proliferation of personality did not slow down during the postwar and Cold War era. To the contrary, psychological research using neuroticism and extraversion as variables of interest rose steadily. Before and after the arrival of "the Big Five" personality traits, debates on how to best to theorize, categorize, and measure those variables added to this mountain of publications. Instead, while still being anchored to the Big Two, these chapters home in on a few key characters and moments in the development of personality psychology from the mid-twentieth century onward, including: Hans Eysenck, Paul

Meehl, the creation of the Minnesota Multiphasic Personality Inventory (MMPI), and the history of controversies during the ascent of the Five-Factor Model (FFM; the broader and more formal title for what is often, somewhat erroneously, called the Big Five). Linking these historical actors is a shared ethos of pursuing a scientific personality psychology, defending academic freedom, and championing the irreducibility and unquestionable heritability of the individual person in society.

“Boundary Experts: Hans Eysenck, Paul Meehl, & the Scientific Psyche,” the fourth chapter, looks at the intersecting professionalization of clinical psychology and refining of personality measurement around the time of WWII. It contrasts the thinking of two of the most prolific psychologists of all time—with diametrically opposed legacies in the discipline. On the one hand, there is the largely disgraced German-British firebrand, Hans Eysenck; on the other, there is the largely revered Midwestern American skeptic, Paul Meehl. Despite their legacies standing at odds in many regards, they are both remembered as crucial promoters of a scientific clinical psychology firmly rooted in validated psychometrics and evidence-based therapies.

Chapter 4 begins with an overview of the MMPI’s development, including its original assumptions about what constitutes a normal personality. The MMPI partially grew out of the creators’ dissatisfaction with the current platter of personality measurements on offer, which included projective techniques such as the famous Rorschach Inkblot Test. After a brief look at projective testing contra the developers of the MMPI, more detail is provided regarding the latter’s creation—including its presumptions of Midwestern normality. Meehl’s contributions to the inventory, one of its validity scales that was meant to deflect deceitful test-takers, is then placed within the wider history of personality testers and others trying to decontaminate the self from their self-report tools. Crucial here are the persistent tensions psychologists had with

psychodynamic explanations of test-takers' self-deceptions when answering dishonestly, reflecting both the wider tension with projective tests that embraced such psychodynamics and psychoanalytic expertise at large. The chapter ends with a more direct callback to the comparison of Eysenck to Meehl. Though, like many psychologists, they both valued philosopher of science Karl Popper's arguments against psychoanalysis (and Marxism), they had distinct brands of skepticism: for Eysenck, anything that could threaten the primacy of a natural-scientific approach to personality was necessarily divining-via-navel-gazing; for Meehl, seriously committed to a scientific psychology, psychoanalysis long-remained a vital part of his extreme, reflective skepticism.

Whereas enhancing the MMPI—an inventory still used today, assembled out of psychiatric categories including various neuroses and introversion—would only be the first of Meehl's many projects, Eysenck would pursue the Big Two (and, soon after, Three) as central to his personality scales and taxonomies for the remainder of his career. For both of them, a left-wing academic and protest culture (e.g., "political correctness") would threaten the rights of psychologists to pursue hereditarian science—even when in the company, or in the explicit pursuit, of the most egregious neo-eugenicist and pro-segregationist politics of the Civil Rights Era and beyond. Following these connections between Meehl and Eysenck leads into the fifth and final chapter: "Conserving Personality: Innate Traits Before and After 'The End of History.'" Here, the oddly meshed libertarian-social conservative value system ungirding trait psychology's indignant response to those wary of a prejudicial pseudo-science of group differences is explored in relation to controversies over race, intelligence, personality measurement, and academic freedom.

Chapter 5 begins where personality psychologists might expect a history of personality to

have already started with: a review of the factor analytic work that informed the trait-factor models of psychometric-trait psychologists like Raymond Cattell and Hans Eysenck, and ultimately led to the (re-)discovery of the FFM. This well-trodden disciplinary story is connected to the less celebrated Cold War legacy of patrons and race scientists wanting to, once again, revive eugenics as a legitimate field of inquiry—even one of moral necessity given the desegregationist policies and anti-hereditarian attitudes of the Civil Rights era. A critical part of the ascendance of the FFM, and personality psychology more generally, is its intentional distancing from these legacies in its funding sources and its own historiography. To help better understand this process, two controversies are reviewed: on the hand, the long running nature-nurture controversy as manifested in politically volatile debates in the 1970s and then again in the 1990s over the genetic determinants of racial differences in intelligence; on the other hand, the relatively politically innocuous and highly technical “person-situation controversy” that some FFM promoters situate prominently in their own historical recollections (even when the FFM’s royal status was still hotly contested among personality psychologists).

Even though the FFM’s version of personality psychology successfully distanced itself from its more unsavoury history, a commitment to an hereditarian-first approach to psychology still united trait psychologists. Hereditarian psychologists (and, often closely intertwined, neo-eugenicists) had long seen themselves as victims of a politically correct culture. Through the scandalous research of trait psychologists working within the interdisciplinary field of “behavior genetics”—once home to anti-eugenicist geneticists—an attitude of being victimized and oppressed grabbed hold of several psychologists. The final section of Chapter 5 examines the on-campus politics of academic freedom, and trait psychology’s defense of hereditarian science against the exaggerated forces of the ideological academic elite who were supposedly: Marxist,

egalitarian, irrational, and anti-scientific. Homing in on the effects of British-Canadian trait psychologist J. Philippe Rushton, who stridently promoted a bullshit and epistemologically violent science of the genetics of racial psychological differences, this dissertation ends with a consideration of how psychologists' calls for "academic freedom" reflect a commitment to a libertarian and, somehow, also socially conservative liberal democracy where criticizing a biological psychology of group/individual differences is treasonous to both science and Western civilization.

A "Closing Thoughts" addendum serves as a dénouement to this political theatre of personality. With the world of the Cambridge Analytica scandal in mind, a scandal that used the tools of scientific personality psychology to subvert dissent and maintain conservative political control, this final section considers the morality projects of some twenty-first century psychologists. Canadian personality psychologist and clinician Jordan B. Peterson, who rose to global fame by apparently defending reason and science against Federal and academic leftist oppressors, is the present-day example of this dissertation's history *par excellence*. This dissertation closes with the author's opinions on the ethical imperatives and potentials of personality psychology, psychometric tools, and psychology more broadly.

Prelude

Bloodlines

“It is of the nature of idea to be communicated: written, spoken, done. The idea is like grass. It craves light, likes crowds, thrives on crossbreeding, grows better for being stepped on.”

- Ursula K. Le Guin, *The Dispossessed* (1974)

“Each paragraph contained a fact but the truth was entirely bypassed.”

-Howard Norman, *The Haunting of L.* (2002)

Chapter 1

Seeing Double:

Origins Notes on Neurosis, Introversion, and the Other Big Two

Neurotics and introverts, The Big Two personalities, are recent categories of self-description that arguably have a deeper past. Thinking of psychology as a discipline with “a short history but a long past” is an expression that has long been associated with the field (Ebbinghaus, 1908). This historiographic truism is debatable, as it depends on the actual psychological concept under discussion. Where the personality traits neuroticism and extraversion are considered, this truism might well apply. Before neuroticism-as-trait, nervousness was long a ubiquitous ailment; before extraversion-as-trait, prototypical notions of introverts and extroverts existed in philosophical and theological works. While acknowledging the possible deeper pasts of these concepts, this chapter almost exclusively traces their short history: from neurology to psychoanalysis to disciplinary psychology. This opening chapter’s purpose is to provide some background for what lies ahead in the remaining chapters. Though, as with the rest of the dissertation, it often foregrounds the Big Two (neurosis and introversion) or allied psychological concepts, this chapter also introduces the Other Big Two that re-emerge throughout all chapters: psychoanalysis and eugenics.

This chapter first outlines how nervousness emerged within European medical practice, eventually leading to psychological theories and therapies of the illness’ etiology. It then takes a closer look at Freudian and then Jungian theory, with an eye toward neurosis and introversion, respectively. While Americans were also working out their own theories and therapies for spiritual and psychological ailments, they were constantly engaging with European approaches.

After the turn of the twentieth century, psychoanalysis became one of the most prominent approaches for Western neurologists, psychiatrists, and early psychologists when considering psychopathology. Among the group of American professionals who were central to the import of psychoanalysis was Granville Stanley Hall. Pioneering institutions and journals for the nascent science of psychology, Hall's passions for (a specific version of) evolutionary theory, child development, and moral righteousness would inform the many psychologists who grew under his wing—including prominent intelligence tester Lewis Terman—as well as feed the fires of controversy over sexuality and child-rearing.

Shortly after WWI, during the advent of psychological testing as a massive enterprise and social tool, a psychoanalytically-tinged version of neurosis was one of the main categories a blossoming disciplinary psychology attempted to adopt for their own science of the mind. During the 1920s explosion of psychological testing, a well-established Carl Jung finally published a formal treatise on personality types. His system of extroverted and introverted attitude types would influence all worlds of testing (academic and beyond), as introversion became prominent in all varieties of personality questionnaires. How disciplinary psychologists first adapted pathologies of personality—neuroses—and types of personality—usually, Jung's introverts and extroverts—into their tests will be explored the next chapter. For now, this chapter, in addition to the psychoanalytic backdrop provided, will also introduce a main artery for the origins of mental (then psychological) testing: Sir Francis Galton's methodologies of a utopian/hereditarian science; that is, the tools for his science/religion of eugenics.

This opening chapter constitutes a prelude to the dissertation. In the main, the aim is to give readers their bearings: supplying some coordinates for the century-spanning history to come. As with the dissertation at large, this chapter is in no way intended to be a comprehensive

history of early psychoanalysis or eugenics. Perhaps more than any other chapter, these introductory sections draw heavily from several years of historical scholarship on these gargantuan areas of human thought and science. The intent here is to take familiar territory and use it to ground a new take on the history of personality—specifically, the joint rise of trait psychology and personality testing technologies. The hope here is to offer insights on some of the most persistent sociological and political tensions energizing the twentieth century story of personality, including the struggle for scientific authority over other psychological experts and viewpoints.

(Pre-)Analytic Beginnings

Nervousness, in its many senses, is an old notion. Introversion, if taken in its broadest sense of turning inward to interiority or perhaps even divinity, is also arguably also an old notion. This opening section provides some background history on neurosis and introversion as concepts related to the disciplining of psychology in roughly the late nineteenth century onward. It rather swiftly covers miles of Europe's historical ground, from the ideas of Georgian era British physicians to fin-de-siècle Germanic *Kultur*.

After briefly surveying the rise of nervousness in cultural and medical discourse, this section situates Freud's development of psychoanalysis within those traditions. It also contrasts his theories of libido with that of his once-favoured protégé Jung. Finally, closer attention is given to Jung's system of personality types—especially the notion of introversion. Although there is an immense literature on psychoanalysis, including its contentious histories, there is relatively less material devoted to Jung's development of typing. As will hopefully be shown, though what became the Big Two were not exclusively psychoanalytic concepts—and certainly not exclusively ideas that Freud and Jung crafted—their particular uses and promotions of these

concepts would influence the trajectory of academic psychology, including its version of the study of personality.

The Rise of Nervousness

The words ‘neurosis’ and ‘neurotic’ have etymological roots with the shared meaning of something acting upon the nervous system. Occasionally spelled ‘neurotick,’ the latter referred to a drug or toxin that affected the nervous system. Something being a ‘neurotic’ was considered a curative for “disorders of the nerves,” as described in 1775 in the *New Dictionary English Language* (Ash, 1775). In the seventeenth century, crucial figures in the origins of neurology, like English physician Thomas Willis, began to systematically explore such disorders and treat afflicted patients—though some would date the history of nervous disorders to ancient Galenic medicine (Piñero, 1983). By the time of the eighteenth-century, nervous disorders or diseases were well-known within the medical world (Drinka, 1984).

In the Kingdom of Great Britain during the mid-to-late eighteenth century nervous diseases were well-known to physicians and the wider public alike. The popularity of sentimental literature of that time even led to nervous diseases briefly becoming desirable (Beatty, 2012). George Cheyne’s successful popular book *The English Malady* (Cheyne, 1733) was one of the most impactful pieces in the cultural understanding of nervousness. In an attempt to showcase his savviness, Cheyne betrayed the influence of a Newtonian and mechanical worldview on his writings of the maladies. Whether through content or tactics, his book was immensely popular, going through six officially published editions and one pirated version (Beatty, 2012, p. 18).

The public and respected individuals took a keen interest in Cheyne’s work, as he even received a letter from the home base of neurotic theory: a note from a University of Edinburgh librarian named David Hume regarding the “Disease of the Learned” (Beatty, 2012, p. 78) For

Cheyne, nervousness was a class problem: a mix of the upper-class's predisposed finely turned nerves, and the availability of endless unhealthy luxuries their affluence afforded them. Though Cheyne pointed toward a long list of causes—from your parents' diseases in earlier life, to various injuries, to time spent adventuring in undesirably arid locales such as deserts—he also noted the likely cause of nervous disorders and distempers among adults:

There is nothing more common ... than to hear Men ... ascribe their Distempers ... to a wet Room, damp Sheets, catching Cold, ill or under-dres'd Food, or eating too plentifully of this of the other Dish at a certain Time, and to such like trivial circumstances, being unwilling to own the true Cause, to wit, their continu'd Luxury and Laziness, because they would gladly continue this Course and yet be well, if possible. (Cheyne, 1733, p. 48-49).

Cheyne's work espoused beliefs about nervousness that were also seen in the popular sensibility literature of that time (Porter, 2001) Among the upper-class, or at least the educated, “the notion of superior nervous suffering was commonplace” (Beatty, 2012, p. 69). But eighteenth-century readers of sensibility novels were offered only a glamorized version of nervous disease. These stories were filled with upper-class heroines prone to fits of fainting—not fits of flatulence, like in many real-life cases (Beatty, 2012, p. 23). In reality, symptoms and treatments were not always as glamorous as feigning victim to the fashionable disease, or reading about them in stories of socialites.

Robert Whytt, chair of theory of medicine at the University of Edinburgh, had a lasting impact on this era's understanding of nervous diseases. Originally published in 1764 then followed with a series of revisions, his *Observations on The Nature, Causes, and Cures of Those Disorders which Have Been Commonly Called Nervous Hypochondriac, or Hysteric* is a bedrock

text for professional and public understandings of what is neurosis.

In his prefatory remarks to that publication, Whytt conceded that he was still far from a mechanical understanding of the power of nerves. He also conceded that it might seem that basically any complaints a patient may have about their body could fall under the umbrella of Nervousness, so he made it clear that was only proposing to treat disorders that are owed “to an uncommon delicacy or unnatural sensibility of the nerves ... observed chiefly to affect persons of such constitution” (Whytt, 1765, p. iv). Whytt thought that an uncommon sensibility of the nerves predisposed sufferers to nervous diseases. Here the nervous diseases were split into three kinds: simple nervousness (possible for either sex); hysteria (exclusive to women); hypochondria (exclusive to men). Whytt’s medical theories meshed well with the already existing gendered and classed ideas of nervous diseases among the European public.

Whytt’s successor at Edinburgh, William Cullen, is credited as first to use the term ‘neurosis’ in 1793 while describing a loosely connected constellation of nervous pathologies. Cullen’s attempt at Latinizing the disease’s nomenclature caused the change in terminology from traditional terms like nervous disease and nervousness to neurosis (Drinka, 1984, p. 34). Although “neurosis” did not immediately eclipse older titles for the array of afflictions, it soon would. Cullen’s definition of neurosis did not stray too far from Whytt’s conceptualization of nervousness. Cullen promoted a “tonal theory” of the nerves, speculating on the physical contraction and relaxation of the nerves. Relatedly, Cullen considered muscles a bodily continuation of the nerves, broadening the scope of ailment allowed within the neurosis category—from migraines, to epilepsy, to diabetes (Beatty, 2012, p. 30). Despite these differences, most popular understandings of nervous disease were still anchored in Whytt’s triptych taxonomy. This most likely owed to the already established cultural understandings of

masculine and feminine expressions of an overtaxed and refined citizen of sensible constitution.

At the close of the eighteenth century and into the Victorian era, attitudes toward neurosis began to reverse. Nervousness lost its glamour as sensibility gave way to a Darwinian society caught in up in technological advancements, particularly the applications of electricity (Beatty, 2012, esp. pp. 141-173). In 1807, Thomas Trotter's popular book *View of the Nervous Temperament* reported the growing number of middle-class nervous sufferers. Their symptoms were no longer a signifier only for sufferers of fashion. Trotter also noted a high rate of nervousness among naval seamen; a finding that found more support in in Cullen's work on nervous sufferers among the naval and military ranks (Beatty, pp. 94-5). Concerns over the masculinity and resilience of British men and soldiers also emerged, such as fearing British soldiers had become as effeminate as the French. Coinciding with the eighteenth-century rising fear of male hysteria, neuroses became a symptom of Britain's "failure to cope in a modern world ruled by fashion, temptation, and lost virtue" (Beatty, p. 173). The idea of neurosis as the exhaustion of neuronal energy also began to feature among professional and public thought. Exhaustion would prove especially essential in American medical work on neurasthenia.

Toward the nineteenth century, the nervous diseases consisted of a variety of ailments, and their expression reflected the cultures of Europe and later the USA (Drinka, 1984). Although physicians of that time were still using ancient categories of illness—such as melancholia, hysteria, and hypochondria—the etiology of nervous diseases had shifted. Instead of believing that vague causes of these illnesses lay below the waist in specific organs, presumed causality was migrating outward toward the nerves, the brain, and eventually the mind (Drinka, pp. 30-1). Philippe Pinel's work is an early popular example of relocating causes of neuroses from the nervous extremities to the central anatomy of the brain. Pinel is perhaps best remembered (and

revered) as another thinker at the forefront of reason and liberty during France's Revolutionary era, and his legacy is mythologized as the emancipator of shackled and jacketed lunatics from famed hospitals like the Bicêtre and the Salpêtrière (Weiner, 2008). But when considering the drafting of atlases of human misbehaviour, more pertinent here is Pinel emancipating symptoms from peripheral causes. Claiming causal links between brain lesions and symptoms fit the overall fin-de-siècle trend of brain over body.

As older nervous diseases fell out of favour and fashion, the mysterious syndrome hysteria soon became the main neurosis at the end of the nineteenth century. Charcot's work on hysteria at the Salpêtrière in France earned him the sobriquet, the Napoleon of Neurosis (Drinka, 1984). During this time there was a trend among physicians of moving away from anatomical explanations of neurosis toward physiological etiology—and eventually psychological causes. Nineteenth-century Western Europe, with its parallel and often rancorous ideologies of romanticism and positivism, its physiological and psychological etiological models, and its defiantly obstinate hysterics, was the garden from which psychoanalysis would eventually bloom.

Before Charcot, neurosis was a broad class of pathologies that included hypochondria (in its classic usage), melancholia, and epilepsy. The etiology of many of these illnesses began to be uncovered, except for hysteria. Explanations of the root cause of neuroses in general transitioned from the anatomical to the physiological. Along with Charcot's famous work in France on hysteria, the American physician George Beard was theorizing another neurosis he labelled neurasthenia. This disease gained traction with Charcot and he considered hysteria and neurasthenia the two main neuroses of his time, helping these two forms of neurosis live on into the twentieth century (Drinka, 1984; Pinero, 1983).

Hysteria itself is a fickle subject, for both past practitioner and present historian. Fittingly, even hysteria's historiography is a complex of differing perspectives, including at least five major interpretive traditions: intellectual histories (beginning with ancient medicine); psychoanalytic histories; feminist histories; histories focusing on Charcot's contributions; and social/political histories (Micale, 1995). Questions about the realness of hysteria have run parallel to its rise and fall as a psychopathological category. For example, many doubted the veracity of the hysterical symptoms Charcot saw and sketched for the world. The reality of hysteria as Charcot studied the affliction was especially suspect, as he was most interested in studying the mental states of hysterics with the use of hypnosis. Although Charcot's work helped hypnosis become a diagnostic technique, he may have only been studying an "artificial or laboratory neurosis" (Drinka, 1984, p. 135). His rival French researcher, Hippolyte Bernheim at Nancy, contended that Charcot's women (and men) were merely suggestible and even loyal subjects—though others before Bernheim had commented on the possible connection between suggestibility and hypnosis (see Gauld, 1992, for an extensive history of hypnosis). Hysteria's long cultural trajectory, particularly the affliction's male-patient variant, is important to disciplinary psychology's interest in neurosis. Charcot's fixations on trauma and hysteria in the etiology of neurosis was well-suited for the general fin-de-siècle spirit.

Though particularly potent in late nineteenth-century Germany, in much of Western Europe there was a return to Romanticism (Ellenberger, 1970, esp. pp 279-283). This neo-Romanticism was a reaction to positivism and naturalism that grew alongside these systems of thought rather than superseding them. The neo-Romantic spirit reeked of decadence, decay, and narcissistic individuality in European art and scholarship. As Ellenberger (1970) remarks, neo-Romanticism mirrored an earlier Romantic interest in workings of seemingly natural phenomena

on the human unconscious: “As the Romantic had turned to Mesmer and animal magnetism, the Neo-Romantics were now enthusiastic toward hypnotism” (p. 279). A fascination with the destructive decaying force of technological advancements was also present during this time. Although controversial, Herman Oppenheim posited traumatic neuroses that resulted from a literal shock on the nervous system when confronted with swift technological change—especially after accidents, such as railway derailments (Drinka, 1984, p. 118). French neurologist Jean-Martin Charcot was interested in these neuroses, such as railway spine and railway brain.

These ideas and techniques influenced the work of practitioners and researchers in Austria and Switzerland. While working under Brücke as a laboratory researcher, Freud met Josef Breuer before leaving to become a scientist—most likely in order to ascend social classes and gain enough wealth to marry his new love interest, Martha Bernays (Gay, 1988, pp. 32-36). During the fall of 1885, Freud worked at Charcot’s Pathological Lab at La Salpêtrière on a travel grant. Although doing strictly neurological research—microscopically examining children’s brains—he left Paris under the influence of Charcot’s theories on trauma neuroses and hypnosis (Ellenberger, 1970, p 438). Even though Freud was undecided between Charcot’s and Bernheim’s views on hypnosis (Gay, 1989, p. 51), he and Charcot maintained warm correspondence many years afterward (Gelfand, 1988). Freud became dissatisfied with both hypnosis and electrotherapy for the treatment of hysteria or neurasthenia as he moved toward psychological causes of neurosis (Gay, 1988, p. 62). Nevertheless, he would famously take on one of Breuer’s patients and deliver a preliminary paper on *Studies in Hysteria* in 1893.

Important relationships were also being made at the Burghölzli psychiatric hospital in Zurich during this time. Auguste Forel reformed the hospital, importing a hypnotic technique he acquired from his time in Nancy, France with Bernheim. Eugen Bleuler, one of Forel’s students,

would supervise Carl Jung at the Burghölzli, where Jung would learn to use the word-association test, a Galtonian tool of mental measurement. Adolf Meyer, another student of Forel, would benefit from his experience with schizophrenic patients before traversing the Atlantic and deeply influencing twentieth century American psychiatry (Ellenberger, 1970, p. 285). These psychoanalytic understandings of pathological selves would also prove impressive and attractive to the nascent scientific discipline of psychology.

Freud's Neurosis and Jung's Libido

Much has been written about Freud's life and work and the broad strokes of his encounters with neurosis are well-known. Early in life he began a career as a neurologist, publishing research in reputable scientific journals. Due to financial constraints, he opted to begin treating unfortunate patients who other doctors refused. The two touchstones often rehashed in Freud's biographies are the influence of Charcot's work on neurosis, and Freud's keen interest in his colleague's young, hysteric patient Bertha Pappenheim (formerly known only by her anonymous case name, Anna O). In adult life, Pappenheim would become a feminist activist, writer, and social worker who fought against the victimization of women (Kimball, 2000). This pairing was certainly the germ of Freud's enduring topic and method: neurosis and talk.

As Freud's research developed and his psychoanalysis blossomed, the proposed etiology of neurosis shifted from physical to psychological. Freud was theorizing neither imbalanced bodily fluids, frayed nerves, nor brain lesions as the root cause for his patients' neuroses. He rather proposed an immaterial cause, such as a damaged mind. The mechanisms of this pathology would later develop into Freud's metapsychology of intrapsychic conflict. In this theory the mind consists of the unconscious id, the conscious ego, and mediating superego

(Freud, 1923). But Freud was not an anti-materialist; he thought given enough time science would discover the biological mechanisms beneath his theory (Sulloway, 1979). During his time working with Breuer on studying hysteria, Freud was busy charting a larger atlas of the neuroses on his own (e.g., Freud, 1894).

In Freud's early system, ungratified sexuality caused states of anxiety. These came to be called the actual neuroses. Among them were anxiety neurosis and Beard's purportedly American ailment known as neurasthenia. On the other hand, there were the psychoneuroses, such as obsessional neurosis and hysteria. Initially, the etiology of the psychoneuroses was some form of "seduction" during a patient's formative years. Like many annals of psychoanalytic historiography, Freud's uncovering of abuse among his early patients and the rejection of this work from his prude contemporaries is more mythology than fact (Esterson, 1998). Nevertheless, Freud's focus on sexuality would affect both the American reception of psychoanalysis and Jung's initial attempts at creating his own psychology.¹

Within Freud's system, the theorized root causes of neuroses changed alongside their categorization. Formerly real child abuse grew into "phantasies" reflecting universal

¹ Ellenberger (1970, pp. 331-406) argues that Pierre Janet bridged the newer late nineteenth-century dynamic psychiatry with older traditions of unconsciousness. In contrast with the neo-Romantic flavour of Freud, Jung, and Adler, Janet apparently carried on the more traditional spirit of Enlightenment (p. 331). Shortly after receiving his MD, Janet worked with Charcot at the Salpêtrière—before Charcot suddenly died and the more traditionally material-focused neurologist Emil du Bois-Reymond replaced him. Despite this shuffle in authority, Janet could carry on his research. His work soon shifted from hysteria to Beard's neurasthenia, the American neurosis also of interest to Charcot and Freud. Given his many invitations to the USA (including Harvard) to lecture on neuroses, and that he was friendly with American researchers such as Morton Prince and James Mark Baldwin, he likely had a hand at the positive reception to psychoanalysis. According to Ellenberger (1970), Freud and Jung initially cited Janet in their work but the fledgling science of psychoanalysis turned against him: such as publicly disparaging his work while appropriating his ideas on the subconscious and *idées fixes*. In Janet's system there were two neuroses: hysteria and psychasthenia—the latter being an updated label for neurasthenia as to not imply neurophysical etiology (Ellenberger, 1970, pp. 374-377). Though histories can sometimes greatly influence a field's self-understanding. Ellenberger's positioning of Janet may have helped support a historical myth of Janet as the great founder of dissociation research and the study of multiples. Hacking argues this legend-making added scientific credence to a resurgence in multiple personality studies in the late twentieth century (Hacking, 1995, p. 44; see also Leys, 1994, for more on Janet and memory studies).

psychodevelopment. All prosaic actions, verbal or otherwise, were now interpretable within a psychoanalytic gaze. Even recollections of the most private of inner spaces, dreams, were now chockablock with latent psychosexual and quasi-pathological ciphers of existence—only legible with the appropriate Freudian lens. Fine (1979) argues that by 1926, with the publication of *The Problem of Anxiety*, Freud had bequeathed a bifurcated concept of neurosis. At one level, there was the individual and her associated specific neuroses. At a higher level, there was culture and the festering pathologies of civilized communities (Fine, 1979, p. 370).

Thus, even under Freud’s psychoanalysis, neurosis remained a polysemous word that captured several distinct symptomologies. But deeply buried intra-psychological conflict between desires and reality, as well as past traumas and everyday life, was the shared etiology for all neuroses. When other psychoanalysts began to disagree with this fundamental assumption is another well-known chapter in the story of psychoanalysis. Pertinent to the current project is the work of Freud’s once closest and most loyal follower, Carl Gustav Jung—the man who historian Peter Gay refers to as the “Crown Prince” of psychoanalysis (Gay, 1988, p. 198).

Jung grew up in a German-speaking area of Switzerland and was born to a family with roots in both medical and mystical practices. His grandfather was a physician who renounced his Roman Catholicism in favour of Protestantism, a move closely aligned with the rising German nationalism of the eighteenth century. He was interested in local variations of secret societies meant to connect members to their esoteric spiritual ancestry, such as the Free Masons (Noll, 1997a, ch. 1). This heritage would foster his grandson’s eventual obsession with the spiritual, mythical, and racial past that grew within a German context where “hereditary and *Kultur* and the landscape were merged with one’s soul in the timeless and deeply resonant concept of *Volk*” (Noll, 1997, p.4). While Jung was a budding physician at Switzerland’s famous Burghölzli

hospital, the main research focus was human memory. It was here that Jung had to learn tests of word-association, a psychological tool Jung would later use in his inquiries into the unconscious (Noll, 1997, pp. 46-47).

Much of psychoanalytic history focuses on the theorists' own personalities, relishing in their clashes. As the most famous example, Jung's friendship-cum-rivalry with Freud has been extensively documented. Their fateful relationship is such a well-known beat in the history of psychoanalysis that it is a staple in popular culture, such as David Cronenberg's film *A Dangerous Method* (2011) and the popular though controversial book on which it was based (Kerr, 1993). One Jung historian, who defends Jung's reputation against cultist revisionists (e.g., Noll, 1997b), legitimately points toward the more important institutional tensions between Zurich's psychoanalysts and others (Shamdasani, 2003). Scholars can agree there is no denying the immense financial aid from wealthy American analysts—such as tycoons seeking treatment from Jung for their family members—in securing the overall influence of psychoanalysis (Noll, 1997a; Shamdasani, 2003). Both Freud and Jung's versions of psychoanalysis impacted an American psychology undergoing its own disciplining. From Freud, American psychopathologists would update their therapeutic methods and ideas on neuroses; from Jung, experimentalists and testers would see the word association tests in a new light, and eventually incorporate his psychological typologies into self-report psychological tests.

The two sides of the Freud/Jung coin are sexual drives and libido. In 1912, after a successful lecture tour in the USA, it became clear that Jung's vision for libido was not restricted to only sexual drive and energy, but meant all universal energy (Gay, 1988, p. 231). The libido and its repressed memories did not only tap into an individual's psyche, but the collective unconsciousness of racial ancestors. The distinction between Freud's drives and Jung's libido is

not simply one of sexuality versus mythology, as both concepts are important but reorganized in Jung's Swiss psychoanalysis. Jung's project moved away from the sex drives of the individual toward a more universal force—perhaps comparable to the animal magnetism of mesmerists, though less concerned with scientific filigree. Eschewing relatively straightforward concepts of seduction or repression, Jung developed an esoteric and Gnostic theorization of buried memory sometimes called cryptomnesia (Noll, 1997). Though Freud's treatment of neurotics held similar assumptions on the individual containing memories of their race (Otis, 1993).

As their respective psychoanalyses grew, Freud and Jung's theoretical developments bore similarities and dissimilarities. They both worked from a "dipsychical" model to a "polypsychical" model of personality (Ellenberger, 1970, p. 147). The idea of more than one facet within a single human's personality grew in a late-nineteenth century psychiatric context where multiple personality was a central focus. This topic attracted professionals and the public alike, including psychiatrists such as Pierre Janet and Max Dessoir. Hacking (1995) argues that the French study of multiples during the late nineteenth century was part of a larger new science of pathological memory. Various theorizations—from Nietzsche to Janet—on a subconscious part of a now multi-faceted mind set the stage for psychoanalytic theories of the conscious and the unconscious. Jung and Freud both plumbed the depths of unconsciousness: While Freud would expand his metapsychology into a tripartite of the universal but individualized unconscious mind (viz., the ego, the superego, and the id), Jung's universal energy (i.e., libido) would guide him toward expanding his work on a collective unconsciousness.

Jung's analytic psychology stood on three central assumptions: collective unconsciousness, cryptomnesia, and the spiritual role of therapy. Jung took the biogenetic law of Haeckelian evolution as supporting his view of using therapy, mythology, and sexuality to

reconnect with one's racial ancestors (Noll, 1997). Although many of the earliest psychoanalysts were under the influence of large German cultural and philosophical figures (Ellenberger, 1970, pp. 271-8), Jung was particularly indebted to those of a classical German aestheticism and their intellectual descendants, such as Goethe and Nietzsche (Bishop, 2007, 2008). Among many topics that would soon become the province of psychoanalysis, Nietzsche's writings on the flow of psychic energy and drives within the unconscious mind left a deep impression on all psychoanalyses—and eventually disciplinary psychology.

Of course, Nietzsche's personal character and philosophical identity is an historical hydra. As Siegfried Mandel wrote in his preface to psychoanalyst Lou Andreas-Salomé's book on Nietzsche—an apparently rare instance of Nietzsche discussing anything with a female intellectual—the German philosopher had many versions of his life to suit different occasions (Andreas-Salomé & Mandel, 1894/1988). Nietzsche's complicated reception in America among late Transcendentalists and pragmatists, not to mention the interpretation of his work in Nazi Germany, multiplied Nietzsche's identities even further (Ratner-Rosenhagen, 2012). Jung and Nietzsche's interests in American thought overlapped, given Nietzsche's interest in Emerson's work, and Jung's identification with American spiritualist and transcendental works.

Although towering figures of German *Kultur* influenced Jung's project from the beginning, Jung's important engagements with Nietzsche's work occurred while he was more clearly constructing a psychology based on collective unconsciousness, personality types, and archetypes. A central point for both Nietzsche and Jung was the idea of opposing selves and working them toward a united self (Huskinson, 2004). Shortly after WWI, during the nascent days of Jung's new psychoanalysis, he began to write in earnest about such opposing psychological types and their connection to the libido, or the collective unconscious.

Jung's Psychological Types

Although he had mentioned ideas of personality type and introversion in places such as lectures beforehand, Jung fully articulated his theories in the 1921 book entitled simply *Psychologische Typen* [*Psychological Types*], of which an English translation was published two years later (Jung, 1923). Jung created an overview of his emerging theory of unconsciousness—one that he would elaborate upon for the rest of his life (Ellenberger, 1970. p. 673). It is an expansive text that interprets and retrofits a glut of historical and theological characters, along with other typologies. This project included the examination of Gnostic beliefs, biographies of Jesus Christ, and the purpose of communion. Jung also dealt with Nietzsche's work, particularly *The Birth of Tragedy* and the Apollonian/Dionysian typology therein. Given Jung's deep-seated interest in his heritage, mysticism, and prominent German Romantics, it is not that unusual even his more plainly clinical works like *Psychological Types* engage with such a variety of sources (e.g., Bishop, 2007). Even Freud, though he was interested in world history and anthropology for less spiritual reasons, engaged with similar sources in his psychoanalytic works. One of the overarching goals *Psychological Types* was to buttress the theoretical foundation for Jung's two broad personality types: the introvert and the extrovert.

Despite Jung's unique system, typing was not a new intellectual enterprise. In addition to the theological and philosophical systems of humans Jung connected to his typology, there had been many medical and psychological practices that linked a type of person with expected qualities of character. Disciplinary personality psychologists are fond of pointing toward ancient systems as the earliest examples of not only personality theorizing but also supporting the Big Two as quintessential traits, such as Galen of Pergamum's four temperaments based on Hippocrates' four humors (e.g., Eysenck & Eysenck, 1985; Stelmack & Stalikas, 1991). In more

modern systems, Western notions of personality types likely stem from European psychological research on mental diseases working within the assumptions of medical nosology, such as Ribot's research (Danziger, 1997; cf. Lombardo & Foschi, 2003). Unsurprisingly, Jung's attitudinal types, especially in the most extreme and dysfunctional cases, have been compared to the symptoms of Janet's psychasthenia type or Kraepelin's manic-depressive type.

Studying personality and character has long been associated with biological perspectives, though often having to do with outward physique. From reading faces to skull size and shape to body type, a person's physique was often considered critical to one's psychology—as well as visual support for a biological basis of one's personality. Growing alongside Jung's types system was German psychiatrist Ernst Kretschmer's system that explicitly connected physique and temperament—another long tradition of personality typing especially prominent in German culture (Kretschmer, 1925). Based on Kretschmer's system of three-type system of physique-temperament, American psychologist William Sheldon's constitutional psychology would become a fairly prominent research program during the interwar years (Kretschmer, 1925; Sheldon et al., 1940).

Speculations about the connection between physique and personality would also persist well into the mid-twentieth century, such as British psychometrician and controversial intelligence researcher Cyril Burt's output (Collins, 1999). British-American psychologist William McDougall, After relocating from Harvard to Duke University—where he would try to revive a neo-Lamarckian view of heredity when other scholars were moving toward interdependent view of nature-nurture (Cravens, 1978, p. 218)—McDougall would produce a “chemical theory” of temperament in which a defective secretion rate of postulated substance “X” caused introversion (McDougall, 1929). As will be seen in Chapter 3, when intelligence

testers were shifting their techniques toward measuring personality, Joy Paul and his wife Ruth Guilford included McDougall's work as an example of "objective" criteria for tests of the inchoate introversion-extraversion trait. For them, pursuing biological bases of personality traits was a promising and obvious line of pursuit, but they rejected McDougall's work given its lack of verification and demonstrated unreliability (Guilford & Guilford, 1934, pp. 379–380).

Even though physiognomy and its variants have been popular in several locales over time, Germans have been particularly enamored of the science of reading a person's soul from their body. In the late eighteenth century, Johann Caspar Lavater repositioned physiognomic interpretations as a positivistic science; though its central assumption likely resonated with a culture that already held such beliefs, even playing face reading parlour games during the German Civil War (Gray, 2004). Phrenology, a sister system of physiognomy, also proved influential in German thought. Franz Gall apparently viewed his phrenology as an improvement on Lavater's physiognomics (Gray, 2004, p. 58).

Whatever Gall's purpose or phrenology's ultimate disrepute, the popularity of reading heads strengthened the basic correlative body-mind principle of physiognomy. While Germany's fascination with physiognomy would persist well into the Nazi era of clearly racially-motivated physiognomic thought, by the twentieth century America's encounter with phrenology would mostly fizzle out. Though not before phrenology found its way into counseling, entertainment, and education (Sokal, 2001; Tomlinson, 2005). Others have argued a modified and repressed phrenology may have even continued in the USA as the New Psychology with its focus on testing (Bakan, 1966), or that physiognomic thinking more generally persists in the more recent study of the biology of personality and intelligence (Collins, 1999). Whether psychiatry or (presently considered) pseudoscience, the sorting of persons into categories based on their

characteristics—whether body or behaviour or both belied such interiorities—was a familiar mode to Europeans and Americans alike when Jung introduced his system.

Jung's *Psychological Types* (1923) begins with a quote about the differences between the philosophical systems of Plato and Aristotle, presumably as an initial template for internally- and externally-oriented attitudes. For Jung, Plato and Aristotle do not only represent two styles of philosophy, but two separate types of human nature. The inclusion of these disparate systems of Ancient Greek thought does not only foreground the introvert and extrovert. It also immediately locates the difference between these types inwardly, within the psyche (Bishop, 2007, p. 102). This fundamental difference in nature is found within the self.

Although Jung tries to link his type system to older, even ancient, systems of self-understanding, the very notion of a divided inner- and outer-space vis-à-vis the self has a history. Taylor (1989) famously argues that the modern notion of a self depends upon having a sense of inwardness, of depth within. Interiorization of the self is not a trans-historical given, as it depended on changing philosophical views over many centuries. Using major exemplary texts, Taylor thinks that this interiority grew from a revised version of ancient teachings on self-mastery: instead of finding reason as a Platonic ideal within the ordered world, reason is accessible within the mind. From Augustine to Descartes, a reflexive radical turn where the act of knowing, and the theistic proof therein, becomes itself an object of knowledge. Whether one follows Descartes' efforts toward a science of self-knowledge and its essential universe, or Montaigne's less methodically rigorous art of self-exploration in discovering your unique identity, turning inward had become an acceptable path of pursuit for increasing knowledge and enhancing ethics.

In a post-Augustinian world, where seeking God and goodness can be done within,

keeping diaries and journals became a moral act for many, from Jesuits to Puritans. This is actually in keeping with the possible etymological roots of the word. Introversion was used not only in Jung's time, but in theological writings from at least the seventeenth century. It meant the act of turning one's thoughts inwards to the mind, soul, or to simply contemplate the spiritual. Like Taylor's intellectual history of inwardness, the act of introversion could also mean a literal connection with the supreme. For example, in 1788, English Methodist and theologian Rev. John Wesley (of the Wesleyan movement) described introversion as what the Mystics called "attending to the voice of Christ within you" (Wesley, 1872, p. 451).

Turning inward to discover the sublime is a familiar trope of Jung's writings, especially his focus on connecting with one's cultural and religious heritage. Perhaps unsurprisingly, Jung classified himself as introverted thinker, aligning him with his diagnosis of Schiller and Nietzsche the Introverts, but not Goethe the Extrovert (Bishop, 2007). Therefore, Jung presumed turning inward—for the discovery of truth, spirit, and identity—was a natural inclination of his temperament. Tending toward inwardness is conducive with fin-de-siècle American culture, where such inwardness was fundamental to Puritan theology (Taylor, 2000, p. 25). Many Americans would ultimately become enamored of Jung's work, for the touchstone of inwardness as much as a shared valuing of transcendentalist-tinged mind cures.

In a chapter of *Psychological Types* summarizing the introvert and extrovert, Jung offered two important insights. First, introverts and extroverts are attitude types with a biological basis (not yet fully understood). Second, the denial of one's psychological type in early development is a source of neurosis. With these conclusions, Jung provides a new nosology of the person and a revised etiology of neurosis. Extroverts, given their propensity to lose oneself in the external world and be extremely suggestible, are most likely to develop some form of

hysteria. Introverts, given their sensitivity and chronic exhaustion, are most likely to develop psychasthenia or neurasthenia. Another way of viewing this is of course the persistence of proper and improper, or female and male, hysterias. Jung noted the damaging effects of culture, in-sync with Freud's pessimistic late output on civilization. Western thought is predominantly extroverted, providing a culture where inwardness is devalued and leading to introverts devaluing themselves. The scourge of extroverted expectations that face self-identified introverts is a martyrdom well-known to twenty-first century consumers and sellers alike.

Jung's central pair of types, the introvert/extrovert, has a muddier backstory than the rambunctious and pervasive concept of neurosis. Aside fascination with mythology and collective history, and of course his experiences with patients, other sources for Jung's introvert/extrovert dichotomy have been suggested. Historians and biographers point toward Jung's childhood and adolescence as an introverted thinker as his root interest in types, while others suggest his long period of self-exploration after the collapse of his relationship with Freud. Jung has spoken to this latter origin story on at least a couple occasions. In *Memories, Dreams, Reflections* (1989), the closest there is to a Jung memoir², he described needing to more clearly articulate his particular brand of psychoanalysis instead of Freud's "one-sided" sexual interpretation and Adler's work on power. Apparently, his colleague's projects led him to explore the "problem of typology," stating that it became "necessary to study the polarity and dynamics of the psyche" (Jung, 1989, p. 155). Jung's dissatisfaction with Freud's "one-sidedness" not only reflected his belief in an internal harmony as healthy, but also the deeper influence of Schiller and Weimar Classicism, who would agree with Jung's take on one-

² Historians and other investigators have demonstrated that although the accounts in *Memories, Dreams, Reflections* stem from Jung's dictations, it was written by Jung's secretary, Aniela Jaffé (Bishop, 2007, p. 13; Noll, 1997). Interviews and lectures might be more accurate reflections of Jung's beliefs—at least, less heavily edited reflections.

sidedness as barbaric (Bishop, 2007, p. 118).

On top of the general cultural influence of the German Romantic period on Jung's analytic psychology, German scholar Paul Bishop believes Jung's project was a direct outgrowth of German Classical Aesthetics. Jung frequently engaged with and mirrored its key figures—particularly Goethe, Schiller, and Nietzsche—throughout his career. Jung apparently claimed to be a blood-related descendent of Goethe, strategically claiming himself progeny of German *Kultur* (Bishop, 2007, p. 19; cf. Noll, 1997, ch. 1). Although Goethe's work, such as the famous *Faust*, was essential to Jung's earliest formulations of his project, along with his relationship with Freud, Schiller loomed largest during Jung's investigation of typology. Jung's typological mechanisms bore a resemblance to Schiller's types of thinking and feeling, and to the idea of cultural forces splitting the self into two types (Bishop, 2007, pp. 108-9). When later discussing trying to work the writing of Schiller, Nietzsche, and other German thinkers into *Psychological Types*, Jung noted that the book mainly dealt with “the relationship of the individual to the world, to people and things” (Jung, 1989, p. 206). Regarding origins, Jung later suggested more personal reasons as to why he started researching psychological types in the 1910s, pointing toward Freud's own typical attitudes.

In a 1959 interview with BBC, just eighteen months before he died, Jung claimed that the ultimate tension between himself and Freud was due to a clash of psychological types. Further, he suggested this very tension between types that was greatly affecting his relationship with his mentor was exactly what drew him toward creating a system of psychological types. Of course, Jung was reimagining his past anew through the lens of a fully matured analytic psychology, but his own belief in the origin of his typology is still important. In Jung's own words when speaking with BBC *Face to Face* interviewer John Freeman:

So, from the very beginning, there was a discrepancy ... There is always a temperamental difference, and his [Freud's] approach was naturally different from mine because his personality was different from mine. That led me into my later investigation of psychological types. There are definite attitudes: some people doing it in this way and other people are doing it in another *typical* way. And there were such differences between myself and Freud, too. ("Carl Jung," 1959, 17:54).³

Along with the many character systems that Jung tries to connect to his new typology in his analytic psychology tome (Jung, 1923), others have suggested the work of many contemporary psychologists as inspiration for the introvert and extravert types: from Alfred Binet to William James. But Jung's taxonomy certainly owed a debt to the theories of a particular contemporary psychoanalyst. In a chapter on the "type problem" in psychopathology (Jung, 1923, pp. 273-288), Jung engaged directly with the work of fellow psychoanalyst Otto Gross—an initially promising acolyte of the cause whose reputation within psychoanalytic circles was eventually reduced to a drug-addicted anarchist and a notoriously perverted sexual rogue. Being an outcast likely explains the absence of much historical work on Gross, when compared to the mountains of histories on more favourable psychoanalysts. Gross was born in Austria 1877, travelled to South America after becoming a medical doctor, then was treated for drug addiction in 1902 at the Burghölzli Hospital (Heuer, 2001). Shortly after this, he became acquainted with Freud and his work, setting up a future relationship with Jung. In a letter to Freud, Jung remarked that he and Gross were so similar in nature that they were twin brothers (Heuer, 2001, p. 658).

Despite Freud's initial great expectations for Gross as a productive disciple, and Jung's

³ Transcribed by me. Italicized text reflects Jung's own emphases as I heard them.

close relationship with him, Gross battled with mental illness and addiction (and those ailments' associated recklessness) for the remainder of his short life, ultimately cutting short his possible legacy. Outside of psychoanalysis, Gross interacted with and influenced a variety of German thinkers and writers, from Franz Kafka to Max Weber (Heuer, 2001; Whimster & Heuer, 1998). In 1920, after being found freezing and starving on the streets of Berlin, Gross succumbed to pneumonia. Of the few obituaries honouring Gross among fellow psychoanalysts was a memorial offered by Wilhelm Stekel, a psychoanalyst known for his work on sexual perversions.

Jung knew Gross so well because they had analyzed each other at the Burghölzli in 1908. After co-analyzing Gross, Jung became more open to the role of sexual liberation (usually in the form of polygamy) in therapy for connecting to “ancient creative energy” (Noll, 1997, p. 87). Jung would go on to prescribe transcending monogamous sexual mores to many American clients, including famed Harvard personality psychologist Henry Murray. In his 1921 book on types, Jung closely examined Gross' *The Cerebral Secondary Function* (1902) and its proposed schemata. Gross believed that cerebral action, of the chemical or vaguer energetic variety, determines psychological type.

Specifically, the speed of cell recovery determined a host of psychological dispositions. Fast cerebral cell recovery meant more frequent cell activity, leading to distractibility and superficial affect. Slower cell recovery made for an inner space chockablock with abstraction but few concrete ideas, while also ensuring an intense affect⁴. Jung compared Gross' two types to the characterological types of Furneaux Jordan, a classification of psychological types examined earlier in the book. Jordan's impassioned and less impassioned characters are mapped onto

⁴ Gross' theory of cerebral activity bears a striking resemblance to Eysenck's much later proposed theory of cortical arousal determining a person's level of extraversion. Eysenck was an ardent critic of psychoanalysis (and clinical approaches in general), and his self-proclaimed influences are experimentalists and eugenicists, such as Ivan Pavlov and Sir Francis Galton.

Gross' dual-speed cerebral types—both of which, when considering the fundamental role of a person's attitude, are reflections of the extrovert and the introvert. Whatever other typological systems had in common with Jung's, the introvert and the extrovert are different in one essential regard: they have persisted as prominent Western categories of self-understanding for nearly a century.

American Processes of Psychological Development

At the turn of the twentieth century, psychoanalysis was gaining prominence among medical professionals, newly disciplined psychologists, and the wider American culture. Its “import” into North American intellectual circles would persist over several decades, though specific figures and moments stand out—such as the much regaled Clark University conference in 1909 in Massachusetts where Freud and Jung formally presented their ideas to a “New World” audience. Yet many among that audience were already quite familiar with matters of psychopathology, including psychical etiologies and therapies. Additionally, the American import of psychoanalysis occurred shortly after a shift in experimental⁵ psychology's focus toward modeling itself after evolutionary science.

This section first observes the network of American psychopathologists—often New England physicians from wealthy families—and their work before the rise of psychoanalysis in their continent. Their *mélange* of therapeutic and spiritual interests would make a welcoming space for both Freudian and Jungian forms of psychoanalysis. Then, the famous watershed event of 1909, the Clark University conference, is considered. The evolutionary and moral psychology of G. Stanley Hall, Clark's founding president and the man who secured Freud and Jung's

⁵ In early American psychology, there was no strong distinction between laboratory methods and correlational or testing methods. Since at least Robert Woodworth's famous 1938 textbook definition of “experiment,” it became commonplace to observe these methodologies as reflecting disciplinary psychology's “two disciplines” (Winston, 1990; see also Cronbach, 1957, 1975; Flis & van Eck, 2018)

attendance in 1909, is examined. Due to his many contributions to configuring psychology as an academic discipline, Hall's life and thought is surely another important piece in understanding the American spread of psychoanalysis. But, as will be seen in upcoming chapters, introducing Hall here is also critical, as he would influence and incense many of the psychological experts crucial to developing personality as a field of study and testing.

American Psychopathology

Before and after the famous Clark conference lectures, American psychopathology was an active network of neurologists and other researchers that was growing out of the dominant somatic style within their medical branch (Burnham, 1967; Hale, 1971, ch. 2). Somaticism as a style of neurology was the standard way mental disorders were understood etiologically a physical condition, such as a brain lesion causing a disease. During the late nineteenth and early twentieth centuries, many younger neurologists worked within this style while attempting to introduce therapies and theories that leaned toward psychological causes for mental disorders.

Freud's psychoanalysis was similar in spirit to many American neurologists: a serious interest in unorthodox approaches for studying the mind and pathology without completely disavowing the fundamental materialist assumption. A century of various claims of American mind cures—from the writings of New England clockmaker Phineas Quimby, to the Midwestern Christian Science movement, to the minister-turned-physician Warren Felt Evans at the crossroads of spiritual practice and medicine in Boston—certainly influenced the types of therapies fin-de-siècle American psychopathologists were developing (Caplan, 1998; Schmit, 2018). The cultural fascination with the curative powers of thought upon thought and body reflected the broader psychospiritual traditions of mesmerism, Spiritism, and transcendentalism. Many of the early American psychopathologists, especially those within the Boston area, had

direct ties to an “an intuitive psychology of character formation, bequeathed to them by Emerson and the Concord transcendentalists” (Taylor, 2000, p. 174).

Often working at the crossroads of established institutions, the network of prominent American researchers interested psychogenic pathology and therapy work included G. Stanley Hall, William James, Adolf Meyer (a Swiss ex-pat), James Jackson Putnam, Morton Prince, and Boris Sidis (Hale, 1971, p. 74). Most of these non-traditional researchers were located along the Eastern coast, with a particularly potent confluence in the Boston area. Unsurprisingly, Boston was one of the earliest places where doctors, such as Morton Prince, were using European concepts such as trauma and the subconscious to create a pre-psychoanalytic American psychotherapy (Hale, 1971, ch. 6).

This Boston school of psychopathologists likely established the American Psychopathological Association in 1910, an achievement Freudian acolyte and biographer Ernest Jones claimed perhaps in an attempt to enhance the narrative of psychoanalysis’ seismic impact (Taylor, 1986). In actuality, American traditions of psychopathology and therapy were established decades before Freud’s ascent. The most important instance being Reverend Elwood Worcester’s efforts at fighting the neurasthenic-inducing evils of modern civilization at the Emmanuel Church, an unexpectedly successful campaign for a post-mind cure therapy that Putnam helped create (Caplan, 1998; Hale, 1971)

Nevertheless, all these researchers and their institutions were crucial for not only developing American psychopathology studies but eventually raising the profile of psychoanalysis. Each researcher’s particular connections and impact vary⁶. Among them, James Jackson Putnam was one of the most respected neurologists in the country, who founded the

⁶ Burnham (1967), Caplan (1998), and Hale (1971) offer more complete accounts of American mind cure practitioners and psychopathology researchers around the turn of the twentieth century.

American Neurological Association and, despite his initial qualms, ultimately relented to experimenting with Freudian therapy methods on his patients. Like many others in turn-of-the-century New England, the claims and promises of Transcendentalism and mind cures intrigued Putnam (Hale, 1971, p. 135). Though important, much of Putnam's sway was within American medicine rather than the blooming science of psychology. Similarly, Adolf Meyer's reactions and promotions of psychoanalysis would matter most for the nominally nascent field of psychiatry. Like Putnam, Meyer would also enhance the American reception of psychoanalysis.

But Putnam's intimate experiences with Freud and Jung during their 1909 American expedition stands as a nearly mythical moment in psychoanalytic history. He famously had the European duo as guests at his campsite near the Adirondack Mountains of northeastern New York State—a camp that has witnessed many other prominent friends and family members as guests, such as Emersons, Cabots, and Jameses (Prochnik, 2006). Yet Putnam was an exemplary psychopathologist not only for his close ties with New England transcendentalists and the possible goings-on at his camp. Putnam's position might also suggest the often overlooked importance of Jung's research in how American psychopathologists likely initially encountered psychoanalysis. Jung shared an interest with Americans, like Putnam, in many mystical and philosophical topics even as early as his dissertation work, from spirits to Swedenborg. On top of these shared interests, the Boston medical network had open channels with Swiss physicians, such as the Burghölzli's Auguste Forel, quite a few years before the Clark University conference. Thus, many interested Americans approached Freud's work (even when visiting Europe) via their Swiss contacts, and Jung's association tests research were already well known. Taylor (2000) goes so far as to argue that before WWI, if there was such as a thing as an Americanized psychoanalysis, it necessarily viewed through a Jungian lens.

Despite Putnam's key role in how American psychopathologists (and the wider medical field) reacted to psychoanalysis, Morton Prince is likely more essential when considering this reception story alongside the disciplining of psychology. Morton Prince's work is an important example for understanding early American therapy. Like many physicians becoming interested in psychology, Prince was born into a prominent Boston family in 1854—his father was the city's mayor for three terms. After graduating from Harvard Medical School, he received further clinical training during the traditional tour of Europe. Prince returned to Europe, this time Paris, in the 1880s while seeking treatment for his neurotic mother (Murray, 1956). Prince was a crucial physician within the Boston school of psychopathologists and therapists, helping not only to popularize pathology with his popular books, but his founding of the *Journal of Abnormal Psychology* and the Harvard Psychological Clinic cemented an institutional foothold for psychopathology in American medicine and scholarship.

Miss Beauchamp, a multiple personality sufferer, was one of Prince's most famous cases. He outlines his trials and findings while trying to treat Beauchamp in his book *The Dissociation of Personality* (1906b). As an attempt to popularize American psychopathology, the book succeeded in garnering much attention in the public press (Marx, 1970). If one assumed ideas associated with a dynamic or depth psychology were not found in American thought until later in the twentieth century, a cursory scan of that book's table of contents would reveal a counterpoint: suggestion, dreams, amnesia, and intrapsychic conflict are all there. Trauma, memory, and the possible role of sexuality in the formation of neuroses were common topics in American psychopathology before the advent of the psychoanalysis cause. Nevertheless, the intriguing dispatches from Vienna and later Zurich helped stoke further interest in psychogenic etiology and their associated therapeutic methods.

In the final chapter of Prince's book, after he finally procured the real (or 'realest') personality within Miss Beauchamp, he outlines the gradual gains made with hypnotic therapy. After one evening when one of Miss Beauchamp's selves experienced a powerful hallucination, Prince tried to conjure a different Beauchamp so as not to re-experience the hallucination. He hypnotized her, "with the aid of a little ether and suggestion" (Prince, 1906b, p. 516); when she awoke, she seemed to be a much more cohesive self than expected. After further attempts at commanding certain personalities to arise under hypnosis, leading to a more synthesized version of Miss Beauchamp, her mental health was on the mend. Signs of abnormality, such as depression, were disappearing. Prince (1906) concluded that to maintain a healthy and harmonious Miss Beauchamp, the patient still needed "regular intervals to offset the wear and tear of her life" (p. 524).

Also in 1906, Prince founded *The Journal of Abnormal Psychology (JABP)*, a venue that would prove key to developing disciplinary psychology's still recognized subfields. His new journal was meant as a single place for neurologists, psychiatrists, and psychologists interested in the new psychotherapies and psychopathologies. Alongside Prince as the main editor, the initial associated editors included fellow New England colleagues such as Meyer, Putnam, and Sidis. Also among the editors was Hugo Münsterberg—an enterprising German emigrant and Wundt student hired at Harvard by William James—who was taken to applying several fresh psychological advances to many facets of life, from lie detection to psychotherapy (Hale, 1980). Seemingly able to master all facets of psychology, Münsterberg's editorial position came from his explorations in psychotherapy, later collected in a book (Münsterberg, 1909). As important as cross-pollination was, such as applying psychopathology to other psychological fields, most major contributors to *JABP* set their sights forms on treatment for abnormal patients—and what

such work could reveal about the normal person.

Taking a brief glance at *JABP*'s early output is analogous to taking the pulse of early twentieth-century American psychopathology. To be sure, important reports and reviews on psychopathological and psychoanalytic topics can also be found in medical outlets such as *The Journal of Mental and Nervous Disorders*, or in primary venues for early disciplinary psychology such as *The American Journal of Psychology*. But focusing momentarily on *JABP* will provide some pertinent insight without risking information overflow. *JABP* is also a psychopathology journal in a unique position as a key institution for early boundary work not only between psychology and psychoanalysis, but within psychology's emerging personality, social, and clinical sub-disciplines (Davidson, 2018). *JABP*'s dual role in fracturing and fusing psychologies reflected the simultaneous disjunctions and unions of a growing discipline.

For now, a simpler look at even the inaugural issue of *JABP* reveals some of the main paths on which American psychopathology was set. Pierre Janet begins the issue with an article exploring the multiple roots of impulses among neurotic and psychotic patients (Janet, 1906). This is followed by an article reviewing the basics of hypnosis, including the contrasting understandings of Charcot and Bernheim (Bechterew, 1906). In light of his colleague's monumental collection of lectures *The Varieties of Religious Experiences* (James, 1902), Morton Prince reports on the hypnotic-like states of powerful experiences causing religious conversion—speculating on the role of the subconscious and memories (Prince, 1906c). Finally, James Jackson Putnam recounts his experiment of applying Freud's methods of catharsis on a few hysteric patients at the Neurological Clinic of the Massachusetts General Hospital (Putnam, 1906). Putnam is intrigued but unsure of forcing patients to confront “often revolting” (p. 41) sexual memories that might not be as important to the neurotic symptoms as the neurologist

assumes.

In the first issue's offerings, a few observations can already be made. Given the inclusion of Janet and the fascination in hypnosis, a connection to present and previous European research is made obvious. Prince's contribution indicates trying to reconcile American psychological research with what European and American psychopathology has learned about hypnosis, memories, and the subconscious. Finally, Putnam's cautious approach to Freud's psychoanalysis, mainly due to the essential role of sexuality in Freud's neurotic theory, is emblematic of many American's initial (and, for some, current) reaction to Freud: possibly important; certainly prurient. A few years later in the same journal, in reaction to Freud's lectures at the 1909 Clark University conference, a much more enthusiastic Putnam comes to the defense of sexuality in Freud's work (Putnam, 1910). Such growth toward a warmer reception could indicate he was relenting to the importance of Freud's work, or it could indicate the softening of strict civilized mores during the early twentieth century (Hale, 1971). Whatever the reason, Putnam admits that Janet had many ideas similar to Freud, but the French doctor's work was descriptive whereas the Austrian doctor's work actually pointed toward a causal model of pathology (Putnam, 1910, p. 293). On the untoward and possibly morbid method of mining sexual memories, a cure some professionals thought possibly worse than the disease, a reformed Putnam professed his updated convictions:

It is a delightful task to lead our invalids to the mountain top and urge them to look out over the splendid fields around them, waiting for them to till. But it is cruel to attempt this when they must drag thither a heavy burden under which they are forced to stagger, pale and panting, to find themselves, at the summit, unable to proceed further. The real mountain top is always within the mind, and outward activity, which is so much prized, is

of little value unless it is the symbol of an inward harmony and peace. At every feast Truth should have the first place. (Putnam, 1910, pp. 307-308).

Stepping back for a wider view of *JABP*'s content during its first few years, the influence of psychoanalysis is clear. Signaling holdover topics from the late nineteenth century, articles on hypnosis, hysteria, Janetian psychasthenia, Beardian neurasthenia, and dissociated personality were often published in its first few years (Anonymous, 1908; Dana, 1907; Donley, 1906b; Gerrish, 1909; Prince, 1906a; Sidis, 1906b). Exploring the subconscious was a fresher staple venture, found in the form of book reviews, articles, and even symposia featuring less frequent yet prominent contributors to the venue, such as Joseph Jastrow and Théodule Ribot (Donley, 1906a; Hart, 1910; Janet & Prince, 1907; Münsterberg et al., 1907). In addition to Putnam's article mentioned above, many others were reporting and opining on the emerging role of psychoanalysis in therapy and theory (Jones, 1909, 1910; Prince, 1909; Walter D. Scott, 1909).

With the aid of several psychoanalysis acolytes-in-the-making, who happened to be frequent *JABP* contributors, the content's focus began switching toward distinctly Freudian takes on pathology: anxiety and the other psychoneuroses (Brill, 1910; Coriat, 1912; Prince & Coriat, 1907; Sidis, 1911); the psychopathology of everyday life (Jones, 1908; Sidis, 1906a); and dream interpretation (Onufrowicz, 1910; Prince, 1910). According to a report on the first proceedings of the American Psychopathological Association published in *JABP*, topics were similarly Freudian: dream, sex symbols, and anxiety neurosis (The Editor, 1910). But Jung's research on association test experiments and dementia praecox were also receiving due attention during these first few years of the journal (Brill, 1908; Coriat, 1908; Jung, 1907; Ricksher & Jung, 1908). At that time, psychoanalysis was co-evolving with American psychopathology, while both were leaving a deep impression on a fledgling disciplinary psychology.

Psychoanalysis' increasing presence in *JABP* did not grow without controversy. In contrast to Putnam's (and psychiatry's) gradual yielding to Freudianism, Prince (and psychology) harboured uncertainty and derision toward the cause. Disagreements and attacks were printed, such as Prince and Ernest Jones disputing dream work (Jones, 1911; Prince, 1911), while Boris Sidis was still doubting the accuracy and importance of sexual experience in the psychoneuroses (Sidis, 1911). Putnam's correspondence with Prince and Ernest Jones reveals that Prince saw Jones as a nervous and self-centred acolyte, and Jones saw Prince as a belligerent old dog. Despite the journal almost teetering toward a psychoanalysis embargo (or collapsing altogether), Putnam helped keep Jones and Prince collegial—and *JABP* kept publishing psychoanalytic articles (Hale, 1971, pp. 282–285). Due to hang-ups about sexuality in psychoanalytic theory, and American medicine's unusually close relationship with popular culture, Americanized versions of psychoanalysis tended toward an optimistic vision minimizing sexuality and underlining the role of environment. As the twentieth century proceeded, such American psychoanalytic outgrowths were quite unique: from the discussions among Harry Stack Sullivan, Karen Horney, and other “Neo-Freudians” at the elite New York City's secret dinner society, the Zodiac Club, to comparative mythologist Joseph Campbell's popular writings on the hero's journey in storytelling.

“The Great Expedition”

Among United States physicians and neurologists, opinions about psychoanalysis varied from its introduction onward. Yet Americans were ultimately receptive of Freud's system of psychopathology, especially after Freud and Jung's famous lectures given at Clark University in 1909. In the matter of a few years, American professionals interested in psychopathology went from never entertaining the idea of being a “psychoanalyst” to being able to join two professional psychoanalytic associations (Hale, 1971, p. 171).

But the Clark conference was not exactly the watershed moment that pro-psychoanalytic historians might present (Rosenzweig, 1992). While the transatlantic visit and the subsequent publication of the lectures undoubtedly cemented and promoted psychoanalysis as a legitimate approach—eventually, a cause—American scientists and doctors were not ignorant to most psychoanalytic concepts. This is plainly due to psychoanalysis drawing heavily from prominent European figures well known to North Americans, such as French giants of psychopathology like Janet and Charcot. Perhaps the key difference between what various Americans were already doing with hypnosis and other therapies for neuroses, and what Freud was formally presenting, was organization. Freud had a much clearer and complete vision for a science of psychopathology.

Despite the other factors in play, it is difficult to entirely downplay the Clark conference lectures. Early American psychologist and Clark University's founding president Granville Stanley Hall initiated a twentieth anniversary conference for his University in 1909 (Ross, 1972). Initially, Freud and Jung declined, though Freud later changed his mind. Freud brought along Jung and Hungarian psychoanalyst Sandor Ferenczi for his transatlantic voyage. Writing to Jung about this invitation, and poking fun at such a young university celebrating at twenty years, Freud wrote that Hall expected "my lectures to give a might impetus to the development of psychotherapy over there. The occasion: the twentieth (!) anniversary of the founding of the university" (as reprinted in Benjamin, 1993, p. 133). Though institutionally important, the 1909 Clark University Freud and Jung lectures might be overplayed in the history of the American reception of psychoanalysis. For example, Eugene Taylor points out that the conference's programs and newspaper reports of the event featured Herbert Spencer Jennings much more frequently and prominently (Taylor, 2000, p. 211).

G. Stanley Hall is, as Hegarty (2013) puts it, an “occult shadowy figure” (p. 29) in the historiography of psychology—remembered for his many firsts in establishing the discipline, such as its first journal and first society (the American Psychological Association). Yet he is often positioned as a stepping stone to disciplining psychology not worth examination in and of itself. He was born in 1844 to a Protestant farming family in rural Massachusetts (Ross, 1972). As a child Stanley was taught the virtues of self-control necessary to achieve manly character; character, for Hall’s parents, was a muscle needing “a program of moral calisthenics” (Bederman, 1995, p. 79). After training under William James at Harvard, where he became the USA’s first Psychology Doctorate earner, and becoming Wundt’s first American student while in Leipzig, Germany, Hall would eventually develop Clark University’s inaugural graduate programs.

At Clark, Hall would more seriously study child development, beginning a large-scale questionnaire program; in 1891, he also founded *Pedagogical Seminary* (later named *Journal of Genetic Psychology*; White, 1992, p. 29).⁷ In such early examples of questionnaire research in American psychology, the intent was not to eradicate the individual from multitudes while also arduously striving for some kind of psychological composite (Young, 2020). Hall’s persistent promotion of his pedagogical work to educational, child study, and other audiences—and the ensuing reports across the nation’s newspapers—helped cement his own and the discipline’s authority on topics of development (Young, 2016). Evolutionary theory coloured Hall’s views on child development, but so did the presumed morals of masculinity. As nineteenth-century medical wisdom blamed the taxing effects of modern civilization on patients’ (masculine) virile energies and ailments like neuresthenia, Hall thought fostering the primitive nervousness of

⁷ “Genetic” at this time did not have to do what we think of as “genes” in a post-Mendelian world. Though occupied with evolutionary theories, “genetic” here more so connoted biological (and moral) development.

young boys would temper them against the pressures of civilization—again, moral calisthenics for the muscle of character (Bederman, 1995, pp. 84–92)

Arguably, Hall and other American psychologists' evolutionary framework reflected the rise of functionalism: a radical attempt at re-starting scientific psychology in light of Wundt's stagnating program and model it after evolutionary theory (Green, 2009). Hall's mentor William James, in 1872, had joined the Metaphysical Club alongside renowned semiotician and logician Charles Sanders Peirce. Members would discuss controversial scientific issues, like evolutionary theory, and the club's brief existence would deeply influence not only functionalism but also pragmatism philosophies of James, Peirce, and John Dewey (Menand, 2001). Hall's interests in evolutionary theory was not in the natural selection of Darwinism, but in Darwin's contemporary Ernst Haeckel and his theory of recapitulation (Green, 2015). Within Haeckelian theory, stages of embryonic growth literally reproduced the evolutionary past of a species; Hall extended this process in adolescence, arguing children recapitulated the "primitive" races that were in his civilized race's evolutionary past. His view on Darwinian progress was an ordered, progressive one; making it comparable to the Hegelian process of historical development of which he learned about early in his academic career (Green, 2019, p. 58)

Whatever his status as psycho-evolutionary theorist, Hall was an indisputably important institutional creator and connector for early American psychology—establishing venues like *The American Journal of Psychology (AJP)* in 1887 that helped define the nascent discipline of psychology while introducing psychoanalysis to an American readership. Although the *AJP* was purportedly meant as a place for new scientific work within an experimental tradition, digital analyses of the journal's first two decades suggest the methods and topics featured in *AJP* were quite diverse (Young & Green, 2013). This plurality no doubt reflected the sheer variety of

psychologies during the turn of the century; nevertheless, articles often conformed to an experimental discourse. As one of the oldest and most visible outlets for early American psychology, *AJP* was a prime real estate for another new science of psychology: psychoanalysis. Hall published the Clark conference lectures in full in *AJP*, presumably with approval of iconoclastic co-editor Edmund Titchener.

The lectures, as published in *AJP*, are wonderful primers on psychoanalysis—at least as the cause stood in 1909. Freud, after immediately highlighting his discomfort with lecturing for members of the “New World” (Freud, 1910, p. 181), provides five lectures outlining the development of psychoanalysis. Beginning with his work with Breuer on hysteria, to abandoning hypnosis in his therapy, to parapraxis and dream interpretation, to the sexual nature of the pathogenic wishes of neurotics, and finally ending with neurotic mechanisms that support an erotic etiology, such as transference. In his fourth lecture, Freud maintained that one of the main findings in his psychoanalytic treatment of neurotics is a connection between healthy and pathological humans:

[N]euroses have no peculiar psychic content of their own, which is not also to be found in healthy states; or, as C.G. Jung has expressed it, neurotics fall ill of the same complexes with which we sound people struggle. It depends on quantitative relationships, on the relations of the forces wrestling with each other, whether the struggle leads to health, to a neurosis, or to compensatory over-functioning. (Freud, 1910, pp. 214-15).

In his lectures Jung also formally introduces the idea of introversion to his New World audience, though as a psychological mechanism similar to a Freudian defense mechanism rather than a psychological type or disposition. When detailing the case of a four-year old girl disgruntled at the birth of a younger brother, certain new behaviours and symptoms are explained

as introverted love that is no longer extending outward toward her parents. Her love for her now distrustful parents turn inward, expressed in reveries, poetry composition, and melancholic attacks. Her introversion, or “converted libido,” can be also be sublimated, making neurosis even more likely (Jung, 1910, p. 258). Thus, two of the most important lessons Jung had for the burgeoning discipline of psychology can be found in his famous lectures: neurosis requires deep analysis but is partially measurable with ostensibly simple psychological tools like an association test—and neuroses can stem from certain types of intrapsychic mechanisms, such as persons tending toward introversion. As will be seen in upcoming chapters, a presumed connection between neurotics and introverts would persist into the heyday of psychological testing, when psychologists were gradually converting these clinical categories to normal traits (e.g., Thurstone & Thurstone, 1930).

Psychologists Measure

Prince, Putnam, and other American psychopathologists and their organizations undoubtedly fomented the rise of psychoanalysis among professional and popular American thought. In Prince’s case, he may have also aided his research project’s own undoing. As Hacking (1995) remarks on the shifting theories of stateside neurosis: “Freud’s repression swamped Prince’s dissociation as a cardinal tool of the trade” (p. 154). Although multiple personalities and hypnosis—a technique most associated with Charcotian hysteria—were falling out of favour as psychoanalysis took root during the first quarter of the twentieth century, the masculine form of neurosis persisted. Sometimes shamefully called male hysteria, or hypochondria even earlier, Charcot and other European neurologists were aware of the disastrous effects shocking trauma of modern civilization could have upon the male body: from civilian railway spine to, eventually, infantryman “shell shock.” As will be shown in Chapter 2,

America's approach to male hysteria among its troops during WWI, along with its fascination with Jungian types, would jumpstart psychology's enterprise of psychological testing beyond intelligence.

But first, this section will provide information to better understand both Jung's influence on psychologists and the ideology undergirding psychologists' quantification of the self. Jung's work on unconsciousness was initially promoted through a widely-recognized tool of experimental psychology: the word association test. Through this methodological commensurability, along with a spiritual harmony among Jung's collective unconsciousness and the spiritual beliefs and therapeutic of American elites, faith in the word of Jung would coalesce.

Another faith in the measurability of human psychology had given purpose to turn-of-the-century experimental psychology laboratories and its various applied counterparts. Aside from the promises of extending lab techniques of physiology into psychological phenomena, the British tradition of anthropometry would also inspire psychologists on either side of the Atlantic. Lying within the tenet of measurability is a commitment to hereditarian science, including the egregious policies of eugenics. As detailed near the end of this section, the joint causes of psychometrics and eugenics would guide the politics and methodology of intelligence and personality psychologists for the foreseeable future.

[On the Legibility of Jungian Thought](#)

There are several ways to understand Jung's success in the USA. As mentioned above, historians point not only to the 1909 Clark Conference but Putnam's vetting of the masters of the new psychoanalysis at his rugged campsite. Qualities of Jung's novel collective unconsciousness aligned with the passé Transcendentalist movement. One could also look at the more grounded role of patronage. Jung was treating the neurotic members of elite American families, such as the

McCormicks. The stalwart convictions of tycoons provided tremendous boosts, in both funds and reputation. Overlapping with this was the Jung-worshipping personality psychologist Henry Murray at Harvard, who would also help secure Jung's influence over American psychologies and societies (Noll, 1997a, 1997b; Taylor, 2000).

After WWI, shortly after Jung more formally sets out his system of psychological types, even non-academic Americans were adapting his typologies for religious and vocational self-understanding. Katharine Briggs' amateur passion for Jung set into motion one of the most profitable psychological tests in the world, the Myers-Briggs Type Indicator (MBTI) —perhaps rivaled only by Scientology's Oxford Capacity Analysis personality test. Shortly after having read Jung's *Psychological Types* (1923), Katharine Briggs—a housewife by occupation, but an inquiring psychologist in her own right with a late-nineteenth century upbringing part religious and part scientific—began working toward what would become the MBTI enterprise that still stands today (Emre, 2018; McCaulley, 1987; Paul, 2004).

From her correspondence with Jung, it becomes clear that Briggs (like all other American importers of Jung) had a rebranding and repurposing in mind. Jung's distinctly European and (Aryan⁸-)mystical scholarship was retconned into an unmistakably American and (post-New Thought) Christian mission of the sorting of individuals by character and aptitude (Emre, 2018). Still today, sometimes unbeknownst to the test-taker, the MBTI (and its varieties) can help a manager decide on whether to hire or fire; it can help an academic counselor suggest career-paths most suited to an unsure student; it can help consumers, willfully taking the MBTI, decide who among their acquaintances is worth striking up a conversation, working alongside, or even

⁸ For this argument, see Noll (1997a, 1997b). Consider also the counter-arguments of, for example, Shamdasani (2003).

being their child's parent.⁹ The psychometric evidence and expert opinion of psychologists has meant little to the cultural trajectory of the MBTI, even enduring more recent years of journalistic critiques (e.g., Stromberg, 2014).

These connections are undoubtedly essential for assessing Jung's influence in psychiatry and popular culture, but it is important to note that early on Jung himself was associated with a particular tool: the word association test. Historians often position questionnaire-style psychological tests, as replacing older mental measurements rooted in experimental laboratory methods. Mühlberger (2017) argues that this has neglected the role of the word association test, holding it as a common root for experimental psychologies stemming from Wundt and correlational psychologies stemming from Galton. Even French psychologist Alfred Binet used a form of word association testing; his work being the basis of American interpretations culminating in Lewis Terman's 1916 Stanford Revision of the Binet-Simon Scale (Fancher, 1987, p. 140).

Other key uses of association tests include the famous Kent-Rosanoff. While still a psychology graduate student, Grace Kent worked at the King Park State Hospital in New York where she developed this test aimed at studying insanity (Kent & Rosanoff, 1910; Shakow, 1974). Aaron Rosanoff was a Russian-born American psychiatrist and eugenicist, who would later often research the heredity of psychopathologies using twin data—including criminality and "mental deficiency" (Rosanoff et al., 1933, 1934, 1935, 1937). A deep impression was also left on psychiatry pioneer Adolf Meyer, a Burghölzli alumnus (like Jung) who went on to work in

⁹ The Education Testing Service (ETS) once chose the MBTI as a potential personality tool to be administered alongside their Scholarly Aptitude Test (the famous SAT). Isabel Briggs Myers, Katherine's daughter, was at that point the primary proponent of the MBTI (a torch later carried by Mary McCaulley). Myers and the ETS did not have a great relationship (Emre, 2018; Paul, 2004). Whether it was Myers' own personality or the properties of her test, their relationship eventually cracked. One of the technical hurdles the MBTI could not vault at the ETS was the expectation that a binary personality type would show evidence of bimodality in test-takers (e.g., test scores are either in an Introvert range or an Extrovert range).

the New England area, saw the potential of word association tests a few years before Jung's Clark conference lecture (see Leys, 1985).

Psychologists were also interested, such as comparative psychologist Robert Yerkes' 1908 research at Harvard—a decade before he would find himself standing at the dawn of widespread psychological testing during WWI intelligence research (Yerkes & Berry, 1909). Association tests were well-known and used among early American experimental and applied psychologists, such as James McKeen Cattell and Hugo Münsterberg. Even before then, German researchers, such as Cattell's advisor Wilhelm Wundt, were aware of these tools when creating their own experimental measurements (Mülberger, 2017). Cattell, himself associated with mental measurements and experimental techniques of the New Psychology, studied under both German and British traditions before coalescing his brand of association tests (and other techniques) in the USA.

The “New Psychology” emerging among American experimental psychologists during the late nineteenth-century field, as John Dewey (1884) had called it, owed great debts to physiology and biology—and the aim of applying the methodologies of the physical sciences to psychological phenomena held the field together. From the 1880s into the interwar era, rapidly evolving technoscientific ideals of organization and control motivated American psychologists in standardizing their scientific tools—from the standardized inputs/outputs in the age of brass laboratory instruments to the eventual abandonment of introspective techniques in favour of tools like intelligence testing (Coon, 1993). Thus, when Jung showed the deeper meanings of a results from word association tests, it was well within a believable framework for many psychologists around the world, including America. From his earlier research results to his Clark University lecture in 1909, Jung was presenting psychoanalysis in a way understandable to most any

researcher of the mind: neurologist, *New Psychologist*, or even “newer” psychologists.

Jung’s lectures, as presented in the *AJP*, are quite distinct from Freud’s. Sharply contrasting with Freud’s provocative yet sagacious five lecture saga on the entirety of psychoanalysis, Jung homes in on three applications of the word association test. Jung’s published lecture appears much more in the tradition of scientific psychology, with several tables of reaction times, painstakingly drawn multi-textured line graphs, and even colour-coded histograms that would not look much out of place in a present day conference presentation or research article. Given its wide uses in mental measurement, Jung argued that the word association test actually has no use for the “purely intellectual, but rather only into the emotional processes” (Jung, 1910, p. 236). Yet these emotional processes, such as a neurotic’s resistances to confronting a buried memory, is not out of the reach of scientific measurement.

Jung demonstrated that psychoanalysis can be harnessed. Memories, dreams, reflections, all subsumed by unconscious forces: These can conform to an epistemology of measurable and individual knowledge, with a tool used in both the soon-to-be split experimental and applied branches. Despite the many decades of twentieth-century psychology devoted to a behaviourism devoid of mentality—let alone an unconsciousness—Hornstein (1992) has shown experimentalists’ coded repression of the taboo psychoanalytic concepts they wished to study. Alongside that story is the blossoming of applied psychology’s testing tools, and the beginning of social, clinical, and personality psychologies that also used these tools. Psychoanalytic ideas and psychiatric practices could be disciplined. Psychologists could render them intelligible within their particular positivistic discourse. Of course association tests, as with many early mental tests, likely originated in late nineteenth-century British anthropometric work, such as the work of pioneering English polymath and eugenicist Sir Francis Galton (Fancher, 1987, p. 39).

Galton's "Kantsaywhere" and the Twinning of Heredity/Testing

Galton was the seventh child born to a wealthy English family in 1822, whose paternal lineage included founders of the Quaker religion (Fancher, 1987, pp. 18–19). Though he was certainly a polymath, his desultory interests marked the comforts of the elite British class. Among his many contributions to everything from meteorology to forensic science, he is likely most remembered (and, for many, most notorious) for his development of hereditarian science in service what he termed “eugenics.” This would include one of the first detailed attempt at using twin data estimate the relative impact of nature and nurture over psychological characteristics; though the classical twin method based on then-unknown, Mendelian genetics would obviously not be developed until much later (Galton, 1875; Michael Bulmer, 2012; Waller, 2012). Additionally, in positioning Galton’s research as forerunner to the twin method, histories and scientists will often neglect other major European contributions—particularly the prominence of twin research in eugenicist Nazi ideology (Teo & Ball, 2009).

Despite his hereditarian legacy, Galton—cousin to evolutionary theorist Charles Darwin—was not a naturalist or even interested in biology and natural history in his early life (Fancher, 2001, p. 4). Content with writing on geography and travel, a younger Galton traveled the world. Still a devout Anglican, missionary journeys inspired his trips, especially his African exploration. Understanding “race” in terms of overall humanity but also in the arbitrary ethnic-categorizations of the time, Galton’s observations on such travels would inform his desire to better *the* race while noting the savagery of *some* races (Fancher, 1982, 2004; Tucker, 1996, pp. 41–50). Enamored of the statistical writings of Belgian mathematician Adolphe Quetelet on extending the calculus of probabilities, averages, and Gaussian (or normal) distributions to social matters (see Stigler, 1986, ch. 5), Galton began an empirical pursuit of racial betterment.

Starting with a magazine article on “Hereditary Talent and Character” (1865), Galton’s major work on hereditary science, *Hereditary Genius* (1869), was received as a valuable work on the “new evolutionary discourse” (Fancher, 2001, p. 5). A statistical analysis of familial eminence among prominent English families, his work would become the basis for eugenics and intelligence testing. While historians have biographically explained Galton’s attraction to empirically demonstrating hereditary eminence, such as an inferiority complex given familial pressures on his presumed genius, others have noted nineteenth-century British stock-breeders, horticulturalists, ethnologists, and medical practitioners (including alienists) widely promulgated notions of heredity (Fancher, 1998; Waller, 2001). More recently, Porter (2018) suggests the data collection of alienists and asylums on patients/inmates informed geneticist projects.

Having more firmly established himself within the network of Victorian scientific naturalists who sought to eradicate “supernatural modes of explanation” with the “emerging findings and theories of empirical science, Galton had found in his adoption of Social Darwinist thinking space for a new religion (Fancher, 2001, pp. 5–6). In an unpublished piece describing his utopian vision of invasive examinations and guided breeding, the future eugenicist world of “Kantsaywhere” was a place where “they think much more of the race than of the individual ... [where a person is] far more important as a probable progenitor of many others (more or less like to him) than as a mere individual” (Galton, 1911, pp. 193–194). Wanting the principles of eugenics to stand in for the outmoded religious tenets of a civilized race, Galton’s “secular religion” demanded a “scientific priesthood” that would lead to a well-being of the nation by design of that very nation’s individuals (Fancher, 2001, p. 18).

Galton’s moral science of lineages would inspire decades of British and American eugenicists, including journalist Albert Edward Wiggam who would cover developments in

psychological testing. In addition to his popular newspaper column (Benjamin, 1986), Wiggam was an ardent promoter of eugenics as consonant with a social policy infused with an updated Christian morality including a revised set of holy commandments beholden to evolutionary science (Kevles, 1995, p. 68). After many visits with geneticists and to Charles Davenport's Eugenics Records Office, Wiggam infused his Chautauqua lectures with eugenics—distributing blank family-record sheets among audiences (Kevles, 1995, p. 59). His best-selling book, *The New Decalogue of Science* (Wiggam, 1925), conveyed an unwavering conviction in the powers of hereditary science and psychological testing in moral tools for transforming the world¹⁰. In his *Decalogue*, among the new Ten Commandments, Wiggam listed the fourth as “The Duty of Measuring Men.” Wiggam argued that the power of eugenics lie in the ability to bypass the lengthy suffering of natural selection; eugenicists could improve on nature's selection, helping in the evolution of a healthy race. Tools for measuring a person's traits were an absolute necessity in this endeavour: “unless you can measure men you can not select them. If you can not tell who possess excellence, you can not weave it into the protoplasmic fabric of the race ... the increase of health, sanity and energy ... are so clearly matters of heredity” (Wiggam, 1925, pp. 136-7).

Perhaps overlooked among Galton's interests is his use of the word association test for exploring the unconscious (Ellenberger, 1970, p. 313). In his *Inquiry into Human Faculty*, Galton speculated that word association experiments indicate the “multifariousness of the work done by the mind in a state of half-unconsciousness” and give reason to believe in a “deeper strata of mental operations, sunk wholly below the level of consciousness” (Galton, 1883, pp. 202–203). Galton imagined that before connected ideas fully entered consciousness, there was an “antechamber of consciousness” where lie a series of related ideas. This antechamber was

¹⁰ Though in late 1927, after a brief fascination with endocrinology and the power of glands over personality, he decided the glandular enterprise was being overhyped (Pettit, 2013b, p. 1075)

“situated just beyond the full ken consciousness”, therefore its contents were not within conscious control (Galton, p. 203). Many researchers of mental processes who used word association tests, from Galton himself and onward, did not further explore this unconscious antechamber bursting with allied thoughts. Jung was, of course, the exception.

But Galton’s emphasis on the heredity of genius and the pedigree of greatness, and supporting it with statistics of averageness, would guide generations of statisticians and psychologists—British and American. A faith in the realness and usefulness of measurement—from the anthropometry to biometry to mental measurement to intelligence and personality testing—enlivened Galton’s passion. As evolutionary biologist and critic of psychometrics Stephen Jay Gould put it, “[q]uantification was Galton’s god” and he its “apostle” (Gould, 1996, pp. 107–108). When writing on the topic of psychometric experiments in an early issue of *Brain*, Galton averred a “branch of knowledge” needed its phenomena be rendered unto “measurement and number” for it to achieve the “status and dignity of a science” (Galton, 1879). His faith would live on in the attitudes of his American acolyte James McKeen Cattell, who would in turn impress this tenet onto his own student Edward Lee Thorndike. The latter’s psychometric *credo*, “[w]hatever exists at all exists in some amount. To know it thoroughly involves knowing its quantity as well as its quality,” is an oft-(mis)quoted fundamental axiom of what some still today consider a philosophically logically-flawed enterprise (Thorndike, 1918, p. 16; Michell, 2020).

Galton’s research on the inheritance of intellect (and any other psychological characteristic), along with his pioneering use of statistics in describing the association and regression of heritable qualities, would inform the work of his student and disciple: the famous statistician Karl Pearson (Fancher, 1987, p. 39). Pearson would help promote and mathematically refine Galton’s statistical methods for studying heredity, culminating in the ubiquitous

correlational and regression techniques known to nearly any scientist (psychologist or not) today.¹¹ For example, before the most commonly used correlation coefficient became Pearson's Product-Moment, it was briefly "Galton's function" (Pearson, 1895, p. 241).

Broadly speaking, Pearson's statistical innovations are perhaps an origin point for factor analysis—a regression-based technique that would eventually become the de facto method for discovering and confirming distinct aspects of intelligence and personality (e.g., Pearson, 1901). Due in part to the historical revisionism of British psychometrician and intelligence researcher Cyril Burt, the origins of factor analysis are contested (Gould, 1996, pp. 266–269). It is now widely acknowledged that fellow British psychologist Charles Spearman's (1904) work on General Intelligence was a key contribution to the method, marking "the beginning of the quantitative investigation of latent variables" (Cudeck & MacCallum, 2007, p. 1; see also Lovie & Lovie, 1993). For Pearson, Spearman's technique and his theory of general intelligence was one of the many irritable examples of "faulty statistical methods" (Porter, 2004, p. 270).

Factor analysis is an advanced mathematical technique of which a precise definition, perhaps best explained through a geometric interpretation of the core linear algebraic modeling, is beyond the scope and need of this dissertation.¹² Conceptually, all that is needed to be understood here is that the technique can use correlations among data—such as a responses to items on a questionnaire—to group the data in a way determined by the strength of those data's shared covariance. The point: it can reduce vast arrays of correlational data into a relatively smaller number of possibly meaningful (but theoretically indeterminable) statistical groupings

¹¹ Much as Galton was in no way the sole originator of (what he named) eugenics, he and Pearson were far from the only person developing tools of correlation and regression (e.g., Yule, 1897).

¹² For an older but concise textbook on multivariate analysis that supplies geometric examples see Green and Carroll (1976). There are many series that provide non-mathematical explanations (e.g., Kline, 1997; Fabrigar & Wegener, 2012). Many of the finer points of debate among factor analysts that depend on a deeper understanding of the methodology—e.g., types of rotation—are largely set aside for the purposes of this dissertation

known as “factors.” This opened the door for factor analysts to explore and dispute the taxonomy of ability and character: such as a single or multiple factors of intelligence; or three-, sixteen-, or five-factors of personality.

Arguing for epistemological consonance between factor analysts and Kantian thought, Mulaik (1987) also suggested the tendency for psychometricians (especially British) to be extremely exploratory in their investigations reflected Pearson’s own penchant to “describe, discover, and summarize” rather than test substantive hypotheses (p. 296). In the realm of factor analytic work on intelligence, Spearman’s work on General Intelligence factor helped further unify “mental tests to mental abilities within a quantitative theory” (Michell, 1999, p. 93). Further factor analytic work such as that of Burt or Chicago psychologist Leon Thurstone, implementing updated techniques meant to explore for multiple factors, would contribute to insider-debates about the categorization of intelligence (Gould, 1996, pp. 326–350). As will be show in upcoming chapters, despite debate over specific technique or interpretation, and despite tepid admittances of environmental causes of intelligence and personality factors, twentieth-century factor analysts (alongside more traditional mental testers) would largely remain committed to an assumed innate and heredity nature of psychology.

Conclusions (in Preparation for the Upcoming Chapters)

In the view of psychologists, psychoanalysis was generating promising concepts that were not being studied correctly. Psychologists’ ultimate repudiation of personality typologies, and the allied rise in psychometrically-discovered trait research, reflected an ongoing twentieth-century process of boundary work. Psychology was acknowledging the importance of psychoanalytic concepts while admonishing all aspects of psychoanalysis and psychiatry. The Big Two—neurosis and introversion, as (broadly speaking) types; later, neuroticism and

extraversion, as traits—provide a vantage point for observing some of this process. Beginning with Freudian neurosis and Jungian types, as received in the trans-disciplinary and European-informed field of American psychopathology, neurosis and introversion would feature prominently in the post-WWI psychological testing boom, discussed in the next chapter, and into the “Big Five” or five-factor models (FFM) of late twentieth and current centuries, which will be discussed in the final chapter. During and in between those moments are the machinations of interwar and Cold War personality psychologists and testers at once ashamed and celebratory of their psychoanalytic and eugenicist legacy.

By the 1920s, psychologists’ expertise on human capacity and characteristics—or intelligence and personality—was becoming well-established. In 1922, a contribution originating from the *London Daily Mail* showed how personality types could help discriminate where intelligence fell short. Running with the headline “Men of Genius Not ‘Smart,’” the short piece was reprinted in various small newspapers around the USA, including Arizona, Minnesota, and South Carolina.¹³ Apparently, a Professor T.H. Pear from the University of Manchester spoke to an educational conference about how geniuses are rarely smart¹⁴; that is, the genius “distinguishes himself from the merely smart man, who lives up to the external demands, by refusing to accept the surroundings.” Pear explained that people could be divided into two types: extroverts and introverts. The contribution ends with a pithy explanation: whereas a great fighter pilot might be a healthy extrovert, the mathematician behind the plane’s design would be a healthy introvert.

This popular report on Pear’s unsmart geniuses, spreading from London’s news and

¹³ For example: *The Bemidji daily pioneer*. (Bemidji, Minn.), 21 Feb. 1922. *Chronicling America: Historic American Newspapers*. Lib. of Congress. Retrieved from <https://chroniclingamerica.loc.gov/lccn/sn86063381/1922-02-21/ed-1/seq-5/>

¹⁴ Consideration of intelligence, gender, and personality will be the focus of Chapter 3.

across America, conveyed to readers many ideas about intelligence and personality types that would become commonplace. First, personality added a layer of complexity in understanding an individual's inherent mental ability. An intelligent person could be smart or otherwise, depending on how they directed their attention. Relatedly, there was a normal degree of potentially pathological types, such as being introverted—in fact, a healthy and adaptive version could be an introvert. Secondly, the news piece's airplane example demonstrated how differently intelligent people should be appropriately assigned their occupation. Though both intelligent, the pilot and the engineer are not suited for each other's jobs due to their personality type.

As previously noted, many testers would long presume a hereditarian position. One of Thurstone's efforts toward scaling intelligence measurements was locating the origin of intelligence scores, meaning an absolute zero value, as several months before a person's birth (Thurstone, 1928). Thus, during the early days of the interwar hereditary-environment controversy among American natural and social scientists, Thurstone seemed to side with a preformationist view of evolutionary development. Decades later, when reflecting on how he was initially uncertain of this finding, he recalled comfortably bowing to the authorities of biological and medical expertise around him: "My neurological friends assured me that such a finding could be justified" (Thurstone, 1952, p. 304).¹⁵

As for Galton's legacy, Terman (writing in his mentor Hall's *AJP*) would use Galton's letters to scientifically enshrine his intelligence quotient (IQ) as that belonging to a child prodigy (Terman, 1917). As will be show in the next two chapters, the interwar transition from intelligence to personality testing was ensnared with eugenicist-infused beliefs in heredity,

¹⁵ Thurstone would revisit the topic of heredity and intelligence a few years later with the publication of the Galton-flavoured book *Order of Birth, Parent-Age, and Intelligence* (1931) co-authored with recent medical student Richard Jenkins whom Thurstone met while working at Dr. Herman Adler's Institute for Juvenile Research (Thurstone, 1952, p. 305).

sexuality, and racial health while also influenced by the rising popularity of psychoanalysis.

Though now seen as distinct from the aims of psychometricians, Freudian orthodox psychoanalysis—which included ideas of innate psychological mechanisms rooted in biological drives and instincts—would also receive pushback from analysts wanting to incorporate environmental and cultural influences into their theories.

Part I

Norms

“It is not a dream of motor cars and high wages merely, but a dream of social order in which each man and each woman shall be able to attain to the fullest stature of which they are innately capable, and be recognized by others for what they are, regardless of the fortuitous circumstances of birth or position.”

-John Truslow Adams, *The Epic of America* (1931)

“Yeah, I'm a tramp, and who's to blame? My Father. A swell start you gave me. Ever since I was fourteen, what's it been? Nothing but men! Dirty rotten men!”

- Barbara Stanwyck as the promiscuous woman adrift, Lily Powers,

Baby Face (1933)

Chapter 2

Multiple Personalities:

Testing for Neurotics and Introverts in the 1920s

On the rising tide of a rapidly industrializing consumer society, America's 'roaring' 1920s is famed for many changes to everyday life, including changes in how womanhood was perceived and enacted. Since the 1890s, American magazines and newspapers were writing about "the New Woman" who flaunted an unusual degree of social independence and physical athleticism (R. Rosenberg, 1982, p. 54). Often paired with sketches of Gibson Girls, the New Woman was bold; she maintained many hobbies outside of the home, though without necessarily shirking her domestic duties. By the end 1920s, with further progress made in feminist and suffrage movements, and a mounting science of sex differences that often challenged the—sometimes theistically-justified—sex roles of the departing Victorian generation, there was a new New Woman: one who was truly independent, often deliberately set apart from home life as daughter, mother, or wife; at worst, she was feared to be "adrift" in society and knowingly promiscuous.

These changes left a Mrs. Margaret Katermann of Schuylkill Haven, Pennsylvania—a borough under a hundred miles from Philadelphia—incensed. After having read a recent article in *Harper's Magazine* on marriage by a "Beatrice M. Hinkle, M.D.," Mrs. Katermann took pen to paper to voice her concerns. In her letter to the editor of the *New York Times*, Katermann claimed to have worked for several years at a private detective agency, where she learned a great deal about failed marriages and its root causes: as Katermann had "come in contact with all the phases of the marriage problem in all walks of life," she felt she could "state from actual

experience that Dr. Hinkle has considerable to learn about matrimonial tangles, even though she has made a study of the matter” (“Equality in Marriage,” 1926).

She maintained that smart women’s fantasy of “marriage on an equal plan” is only maintained in times of peace. Additionally, she believed rising divorce rates had little to do with wives’ dissatisfaction. Quite the opposite: she claimed most divorces stem from the husband’s dissatisfaction with the marriage. Scoffing at the perspective of Hinkle and supposedly liberated women, Katermann rhetorically asked: “If good, sensible women of the past were not wedded on an equal basis with a man, what chance does the present harebrained, half-nude type of female (masquerading as modern woman) have of doing so?”

Hinkle, an accomplished American physician (to whom this chapter will return to shortly), was a well-known Jung-devotee and English translator of his work. She wrote about the sexes in academic and popular venues throughout the 1920s. At the very opening of the decade, she wrote in the pages of *Psychoanalytic Review* about how being “feminine” differed vastly across place and time: comparing today’s connotations with the inverted sex roles of Ancient Egypt, as Herodotus recounted (Hinkle, 1920, p. 15). She argued that studying characteristics of the two sexes was simply confusing sex with type. She shifted focus to Jung’s two main “extravert and introvert” types that had “an entirely opposite approach to life” (Hinkle, 1920, p. 25).¹⁶

Nevertheless, unlike how some writers would point to personality differences in explaining marital problems, Hinkle thought the struggles of modern marriage instead reflected a nobly “changed social attitude” toward matrimony itself (Hinkle, 1925, p. 6). Women, whom she

¹⁶ In this article, Hinkle also wrote about what she saw as different types of types—like the “emotional introvert.” This would become a persistent taxonomic problem for personality psychologists; see, for example, Chapter 4 of this dissertation and the incorporation of (Social) Introversion in the Minnesota Multiphasic Personality Inventory (MMPI).

saw as the long-suffering and falsely seconded sex, were not entirely abandoning marriage and feminine ideals. Feminist reconsiderations of womanhood and marriage now afforded women the freedom to find new extensions or versions of themselves: “Marriage is no longer considered as the substitute for an occupation, nor does it take exclusive place in their thoughts” (Hinkle, 1925, p. 8).

New research and ideas on sexuality, gender, and personality were enacting, influencing, and being influenced by various publics of the 1920s (Pettit & Young, 2017). The reading public were one of the many publics grappling with how the novel schematic of introverts and extroverts meshed with the already-challenged binary of male and female. Additionally, neurosis, particularly the psychoanalytic varieties that emphasized sexuality and child development, had only continued to gain traction after a devastating World War whose violence afflicted soldiers with the neuroses (wrongly) assumed to affect only women or male cowards. All the while an unusual collection of psychologists were trying to subsume neurosis, and then introversion, into something legible to their disciplined preferences. Many of the psychologists most crucial to the project of appropriating the Big Two—from (relatively) discrete psychoanalytic and psychiatric classifications into necessarily continuous psychological traits—were working well outside the walls of the research laboratory.

This chapter examines the spread of personality psychology and testing across several cultural spaces of the 1920s. It will first explore how the Big Two were put to work affecting notions of femininity and masculinity: from the popular writings of Beatrice Hinkle to the efforts at grasping and defending against wartime neurosis (e.g., “shell shock”). How testing for neurotic personalities and then introvert/extrovert types in industrial spaces grew will then be explored. Alongside the development of questionnaires for industrial uses, many academic

psychologists of the 1920s were also theorizing how to measure personality. While neurosis and introversion were continually re-envisioned through the efforts of psychoanalysts, popularizers, and applied psychologists, these processes and the very publics they set out to discern were in co-creation (Pettit & Young, 2017; c.f., Hacking, 1986).

The Intellect and Personality of the Sexes

As the discipline of American psychology continued forming into the WWI era, younger psychologists were moving away from a previous generation of conflated intellectual and moral influences. After the war it would seem that some aspects of psychology, psychoanalysis, and an evolving feminist movement challenged many traditions; especially socially conservative and often Christian norms around the capacities and social roles of the sexes. Though building on the intellectual pursuits and movements of an earlier generation—like evolutionary theory, pragmatism, psychoanalysis, and mental testing and experimentation—newer experts were pushing the boundaries of the previously “separate spheres” of male and female. Crucial to those developments was an increased focus on personality; marking a confluence of experts writing on types, temperaments, and traits.

This section first outlines some of the key psychological research that challenged the assumed sex differences—and background Victorian values—in intelligence research. One of those psychologists, Leta Stetter Hollingworth, was a founding member of the Heterodoxy Club: a New York City space for feminist scholars, and like-minded artists and activists. The Heterodoxy Club would serve as one of physician and Jung-devotee Beatrice Hinkle’s main venues for developing and promoting her feminist version of psychoanalysis. In Hinkle’s view, the sex binary of male and female were arbitrary and unhelpful; she would promote her preferred introvert and extrovert binary. Finally, this section closes with a look at how expertise on

nervousness and personality types was being communicated in the press and shaping the psychodisciplines' research on the devastating "shell shock" battleground soldiers.

Overlapping Spheres

In the early twentieth century, academic psychologists like G. Stanley Hall were establishing the New Psychology in universities (new and old) and journals. The common challenges of industrialization that burgeoning American metropolises and surrounding areas posed—like New York, Chicago, Baltimore, and Boston—would impact academic psychologists establishing their work as part of a legitimate field of scholarship and science (C. D. Green, 2019). Still, many other psychologists were promoting the presumed social usefulness of psychological tools for industry and government.

The nascent American disciplinary psychology was rooted in functionalism, varying versions of evolutionary theory, and the virtues of experimental and empirical investigation (Shields, 1975). Despite this shared philosophical grounding, psychologists had deep disagreements about sex differences—reflected in generationally disparate attitudes toward topics like co-ed campuses. At places like Columbia University and University of Chicago, where younger male academic psychologists welcomed the experimental investigation of sex differences to put the matter to rest, more opportunities were available for female psychologists (Rosenberg, 1982).

Much like human intelligence per se, and eventually personality, the investigation of sex differences was enmeshed with prescriptions of normality and causality. Many researchers pursuing sex differences research shared a faith in the power of the situation over individual psychology—though they still assumed nature delimited the bounds of individual differences. Biological causes of male and female temperament, such as the maternal instinct, was conjoined

with the long-running variability hypothesis (Shields, 1975, 1984). Leaning on the virtues of evolutionary theory, the older generation of scholars assumed that variation in ability was an essential evolutionary mechanism in the survival of the species. Consequently, this meant males, being the stronger and more progressive sex, were also the most variable. Coupling this hypothesis with a statistical definition of normality—a standardly symmetrical bell-shaped curve of density probabilities for a population’s trait—boded well for men: within a normal distribution, greater variability meant a greater possible range (Shields, 1975, p. 748). Alongside historical evidence of eminent male scholars and leaders, it seemed obvious to many that the height—or extreme—of intelligence was an exclusively penile parameter.¹⁷

Some psychologists and scholars saw opportunity to depart from the previous generation’s Victorian-stained psychology. In Chicago, pragmatist philosopher and educational pioneer John Dewey received Darwinism in a very different way than the older G. Stanley Hall. At the very end of the previous century, Hall’s worries about the weakening effect of civilizing manhood had led him to a Chicago-area scandal. Partially stemming from his Lamarckian beliefs in the recapitulation of the evolutionary path from embryos into adolescence, his 1899 recommendation to kindergarten leaders to foster the innate racial “savagery” of young boys (in order to temper them against the feminization of civilized, White, male adulthood) was sensationalized in the press as advocating barbaric violence amongst toddlers (Bederman, 1995 ch. 3). For Hall, his reading of Darwinism legitimated the ongoing divergence of sex roles as a feature of a properly evolving civilization. Dewey, who had no problem with “mixed audience” courses or female researchers, viewed Darwinism as a liberation from social assumptions of the

¹⁷ Not all who valued statistical normality had faith in the variability hypothesis of the sexes. The famous statistician Karl Pearson—Galton’s student, biographer, and eugenicist acolyte—was one of the main critics of the “hypothesis” as pseudoscientific (Pearson, 1897).

natural order of the sexes (Rosenberg, 1982, p. 60).

At Columbia in New York, Leta Stetter Hollingworth was mounting a research program built on the findings of other female psychologists like Helen Thompson Woolley that challenged such assumptions about the sexes. Hollingworth was previously a schoolteacher in Nebraska, but married women were barred from teaching in New York (Shields, 1975). Instead, alongside gaining a graduate education, in 1913 she began working as a mental tester for the city's Clearing House for Mental Defectives. A year later, she earned the city's first Civil Service position of "psychologist;" then, in 1916, she became Chief of Psychology at Bellevue Hospital.

Hollingworth's work at these institutions were in the very early days of intelligence testing, where she used American adaptations of Binet's tests. As Hollingworth amusingly recalled, when she was initially transferred to Bellevue's Psychopathic Service in 1915, a medical doctor asked her what her occupation was. After replying, "a psychologist," it was clear that the doctor did not know what she meant. Hollingworth clarified: "I give mental tests" (as quoted in Harry L Hollingworth, 1943, p. 102). Psychologist-as-tester seemed a legible role to established professionals; especially for women who were often assumed to be working within the margins of professional hierarchies (Furumoto, 1987).

One of Hollingworth's main research projects challenged key assumptions about sex differences, such as the effects of women's menstrual cycle on their mental ability, and the conjoined variability hypothesis and normal distribution of mental traits assumption. Though her research seemed consonant with the era's feminist politics, she did not agree with types of feminism that relied on the unique physical differences between men and women or that saw women as superior (Rosenberg, 1982, pp. 110-1). Nevertheless, Hollingworth was an active

suffragist with the New York Women's Suffrage Party, and in 1912 she became a founding member of the Heterodoxy Club of which she remained an active member for a quarter century.

Beginning in the bohemian Greenwich Village in 1912, the Heterodoxy Club brought together several educated women who wanted to discuss feminist theory and promote the self-discovery and self-emancipation they saw central to modern feminism (Wittenstein, 1998). Members of the all-women club included feminists, communists, and proponents of birth control. As its name suggests, the Heterodoxy Club was a space devoted to the "discussion of unorthodox opinions" (Rutherford et al., 2012, p. 282).¹⁸ Mabel Dodge, the "mystic of Greenwich Village," was a club member who greatly reflected a heterodoxy that incorporated knowledge that questioned gendered subjectivity, including psychological research, social reform, occultism, and psychoanalysis (Hustak, 2013). Seeking advice from students of Mary Baker Eddy, Dodge, like other women investigating life and their roles within it, drew on the body-soul holism of Christian Science. Given the diverse interests of female scholars and activists at this time—often vying for a depth of knowledge through alternative systems of faith and subjectivity—psychoanalysis was understandably welcome. Especially Jung's explicitly occult-tinged analytic psychology.

During the first year of the Heterodoxy Club's existence, Jung visited the avant-garde group while he in New York; Beatrice Hinkle was escorting him around the city (Sherry, 2011, 2013). Hinkle was an American physician among an initial group of Jungians who helped

¹⁸ Within or without the Heterodoxy Club, pluralistic female scholars were keen to investigate personality and gender from many angles, including newer psychoanalytic frameworks. For example, June Downey, most famous for her work in graphology and temperament analysis, wrote about how temperamental differences, particularly introverts versus extroverts, could shed light on failed marriages in fiction (Downey, 1925). Though located in Wyoming by the time the Heterodoxy Club was established in faraway New York, Downey was quite variable in her abilities and interests. She was a playwright and poet whose written word often dwelt on knowledge and spirit: "The Soul-Beyond! Scarce realized, wholly known; Sought in the dusk of sight, the dumb of tongue" (Downey, 1904, p. 52).

professionals and the public alike become familiar with analytic psychology. Jung was known among American psychopathologists even before the spread of Freudianism, though he was a talented and tireless promoter of psychoanalysis in the USA. He spent much more time touring the country and boosting the cause than did Freud—before and after Freud’s single visit to the New World in 1909. Though many American disciples of Jung would emerge later in the twentieth century, knowing Jung as an elder and mystical sage, there was an earlier generation of Jungian analysts, including Hinkle (Noll, 1997b, p. 274).

Hinkle Trades Sex for Type

Beatrice Hinkle (née Moses) was born likely in 1870 in San Francisco. After having two children, she decided to become a medical doctor in 1899 at Cooper Medical College, a medical school later bought by Stanford University (Wittenstein, 1998). After relocating to New York City once her husband died in 1905, Hinkle became interested in the various forms of mind cure therapy permeating American culture. Like the minister-physician Warren Felt Evans—one of the earliest New England “mind cure” practitioners—Hinkle was drawn to movements steeped in Spiritism, mesmerism, and connected to notions of Transcendentalism, such as Quimby’s New Thought and his student Mary Eddy Baker’s Christian Science (Caplan, 1998; Schmit, 2018). As with both the network of mainly male New England medical professionals interested in abnormal psychology and the all-female members of the Heterodoxy Club, Hinkle was interested in the latent curative and potentially liberative powers of the inner-self.

In 1911, Hinkle made the long voyage across the Atlantic Ocean to attend the Third International Psychoanalytic Congress held in Weimar, Germany. Apparently having personally met both Freud and Jung—the latter then president of the International Psychoanalytic Association—Hinkle returned to the USA determined to open a private practice using

psychoanalytic techniques (Wittenstein, 1998). From her return onward, Hinkle worked toward translating and promoting Jungian psychoanalysis, eventually becoming a provocative conduit between Jung and American audiences.

Reporting on Hinkle's answers to questions at an international conference for female physicians at D.C.'s YWCA headquarters, an anonymous reporter highlighted how Hinkle's ideas went against Hall's assumptions on essential sex differences. Hinkle argued for using the introvert and "extravert" instead of male and female as the two fundamental types of personhood. Accepting this would broaden women's employment opportunities, while also taking away taboos of feminine work when a man might be suited for it depending on his personality type. As the reporter succinctly explained, Hinkle's two types were true regardless of one's gender: "There are no such things as 'masculine' men and 'feminine' women ... if you belong to the 'extravert' type of humanity, you are aggressive, self-confident; while if you are an 'introvert' you are thoughtful, hesitant, less aggressive" (clipping and citation in **Figure 1**).

That article presumably chose Hall as the best authoritative representative of status quo positions on the psychology of the sexes; despite the current generation of male and female psychologists who challenged Hall's Victorian-Haeckelian understandings of racial and sexual development. The piece featured a cartoonish sketch meant to portray (or lampoon) the possible social ramifications of Hinkle's shocking take on gender. The cartoon features a confused and sweating plump man using a washboard, with women doing traditionally male manual labour on either side of him and a large question mark in front of him. Oval portraits of G. Stanley Hall and Beatrice Hinkle hang above the perplexed and portly cartoon man.



Figure 1. Clipping from *The Perth Amboy Evening News* (New Jersey), November 7, 1919, p. 10, *Chronicling America: Historic American Newspapers*. Lib. of Congress. The same report appeared in other regional newspapers, such as Bisbee, Arizona and Corpus Christi, Texas. Retrieved from <https://chroniclingamerica.loc.gov/lccn/sn85035720/1919-11-07/ed-2/seq-10/>. The Library of Congress believes that the newspapers in *Chronicling America* are in the public domain or have no known copyright restrictions.

Personality typing was already at play in a 1912 *New York Times* interview with Jung, where he explained the psychodynamics of American neurosis: “When I see so much refinement and sentiment as I see in America, I look always for an equal amount of brutality. This pair of opposites—you find them everywhere” (“America Facing Its Most Tragic Moment,” 1912).¹⁹

¹⁹ Although this *NYT* interview with Jung was published without naming the interviewer, a Hinkle historian (a

Brutal and sentimental types were a familiar binary to Americans. Jung's personality types were building on several previously suggested typologies of personhood. One such system was William James' tough- and tender-minded character types, a binary that could map onto stereotypes of men and women. Some older American psychologists viewed Jung's introvert/extrovert as interchangeable with James' tough/tender, such as Joseph Jastrow did when answering an *Evening Star*'s reader's question about whether extrovert and introvert was accepted professional terminology (Jastrow, 1929). Much as nervousness, especially hysteria, tended to be associated with womanhood, being an introvert was also becoming the dual abject position of pathological and womanly.

Jung's new personality types began appearing in many other literary locales during the 1920s. The terms began appearing in fiction, such as the 1925 *Harper Monthly*'s piece "And They Call It Science," about a suicide victim who was (among other triggers) taunted for being an introvert (Bennett, 1925). In that same issue, in an opinion piece floating somewhere between worried sociological think-piece and cantankerous drivel, one writer complained about the insidious individuals and institutions emerging in America that limited freedom simply because they have the "drive" (England, 1925). Mocking "modern psychologists," the author believed these limiters of freedom were looking to channel an "unsatisfied sex-urge, which is forced to find outlet in some such channel or turn inward and produce introverts or nervous wrecks" (England, 1925, p. 763). Whether an introvert or neurotic, they were both pathological results of a broken psyche.

Mirroring Jung's confrontational public assessment of America's neurotic psyche,

rarity) suggests an author: During Jung's 1912 Fordham University lectures, Hinkle arranged for Charlotte Teller to interview Jung for *The Times*. Given that this is Jung's only interview in the *Times* during that time, Teller might have been the interviewer—making Hinkle the producer of the interview (Sherry, 2013).

Beatrice Hinkle would use the introvert and extrovert to critique intersected assumptions about intelligence and gender. Shortly after, further promoting Jung among Americans, Hinkle began translating his works. Besides from Jung's translated academic essays or lectures, Hinkle's 1916 translation of *Psychology of the Unconscious* (C. G Jung & Hinkle, 1916), later revised and titled *Symbols of Transformation*, was Jung's major text among English-speaking audiences, until a translation of 1923's *Psychological Types*. Hinkle's work translating and promoting Jung was a prelude to her own contributions.

In her original works, Hinkle not only criticized Freud's theories and methods of female psychosexual development, but she also dismissed the experimental study of sex differences (Morawski, 1997, p. 29). Her feminist project, while critiquing Freudianism, still trusted Jung's analytic psychology as an emancipatory tool for women. Her provocative writings centrally featured Jung's system of personality types, helping to promote the introvert/extrovert dichotomy among public and professional audiences while Jung's formal system was in its infancy. Even before the publication of her first book, *The Re-creating of the Individual: A Study of Psychological Types and Their Relation to Psychoanalysis* (Hinkle, 1923), Hinkle's project of undoing sex roles gained some public notice.

Hinkle would continue to garner attention in the press, though mostly in the form of positive reviews of her *Re-creation of the Individual* book—what Noll (1997) calls “one of the very first Jungian ‘how-to’ manuals” (p. 269). Two reviews of her book in the *New York Times* helped further promote not only the feminist uses of Jungian psychology, but the centrality of Jung's axes of personality: introverts and extraverts (Clark, 1924; Thorne, 1924). Above mental ability, above sex, and above any other dubiously conceived and measured differences, Hinkle championed Jung's types as the most essential. Through her other encounters with the public,

such as pieces on modern marriage and women's independence for *Harper's Monthly* (Hinkle, 1925, 1932), or a piece for *The Nation* that discussed psychoanalysis' role in modern feminism (Hinkle, 1927), Hinkle continued to help cement Jungian psychology as part of the emerging lingua franca of psychology.

Finally, if psychologists like Helen Thompson Woolley and Leta Stetter Hollingworth had challenged assumptions about manliness and intelligence, personality offered a space to reconsider the psychological abilities of men and women. The impending rise of psychology's version of personality testing helped bolster an assumption adjacent to the variability of the sexes' mental ability: the complementarity hypothesis. The assumption of complementarity held that different strengths or temperaments of men and women made them suited for distinct roles within society (Shields, 2007).

Nervous Wives and Hysterical Men

Because Jung's types were introduced and popularized much later than nervousness (or the Freudian-tinged "psychoneurotic"), neurosis was industry's first introduction to measuring a worker's personality. Early in the twentieth century, alongside its promotion in psychopathology studies, psychoanalysis began to be featured in the American popular press. According to Hale's (1971, p. 397) survey of magazines, as early as 1911 psychoanalysis' presence grew as older mind cures and other therapeutic traditions, such as the Emmanuel Movement, vanished. From 1914 toward the close of the decade, articles about psychoanalysis were appearing in women's magazines like *Good Housekeeping*, *Ladies' Home Journal*, and *Cosmopolitan*, and in general interest magazines such as *McClure's*—the latter venue also being a favourite for self-promoting psychologists, such as Hugo Münsterberg. This period even saw the beginning of psychoanalytic novels, such as James Hay, Jr.'s *Mrs. Marden's Ordeal* (1918). In the opening chapter of that

novel, Dr. Doyle explained the uncomfortable process of analysis: “You will have to tell me what you believe about religion, the most intimate things about your life with your husband, the big things and the little things, sex things and all. You may keep nothing back from me” (Hay, 1918, p. 5).

While the days of American neurasthenia, and its spa- and electro-therapies, were receding into the past, psychoanalysis was helping to repackage nervousness with ongoing concerns over children and mental hygiene. Concerns over child development was also spurring on philanthropic funding, such as contributions from 1925 until 1940 from the Laura Spelman Rockefeller Memorial (Lomax, 1977). Soon the parent education movement afforded female researchers training and jobs—leading to some egalitarian adjustments to the traditional ideas of mothering and family life in the writings of some child experts (Johnson & Johnston, 2015). Echoing nineteenth-century theories of neurosis stemming from an unnatural urban environment, and perhaps Hall’s presumptions of child development, one 1923 article in *Ladies Home Journal* featured a psychologist and physician warning of the nerve-wracking effects that a restrictive and repressed city life can have on children (Walsh et al., 1923).

Conveying the weight of this medical advice, future President Herbert Hoover provided a foreword to that *Ladies’ Home Journal* article, in which he extolled past medical accomplishments of eradicating “old scourges” (e.g., smallpox). Yet he suggested a lower degree of American accomplishment with “more subtle troubles.” Hoover reflected on the country’s predicament: Though they had rejected the men found to be mentally unfit for service from the army, they had unwisely “let them drift back without question into our industrial life. There they remain today, a serious handicap in progress, because in their youth no one corrected their remedial defects” (Walsh et al., 1923, p. 36). Reflecting the country’s more general occupation

with nervousness, under the promotional guidance of psychiatrists and psychologists, similar pieces can be found in *Ladies' Home Journal*. Like a harbinger of horror, American professionals were writing on the symptoms of the nervous housewife, and the fine line between pleasing your child or making them a spoiled neurotic, as Freud apparently warned (Chapin, 1922; Myerson, 1920).

For a society that was worried about being nervous, now refashioned in psychoanalytic terms of deep-seated and repressed desires, their easy adoption of Jung's personality types is unsurprising. The introvert and the extrovert not only added a layer of self-understanding, but helped the public distinguish a person's temperament from their mental ability, as well as delimit the normal and the abnormal in light of ubiquitous psychopathology. Though American psychologists were quick to adapt and promote Jung's two types, without a doubt Jung's psychiatric disciples debuted his concepts for the public.

At this same time, the psy-disciplines were committed to their wartime efforts of assessment. After tentatively convincing the US military to allow it, psychologists developed and administered the Army Alpha and Beta tests on recruits to assess their intelligence (e.g., Carson, 1993; Gould, 1996; Kevles, 1968a; Samelson, 1977). Comparative psychologist and then APA president Major Robert Yerkes led a committee on recruit intelligence with other leading experts, Lewis Terman and Henry Goddard. Details about psychologists' various testing efforts on recruits would be uncensored and released to the public shortly after the war ("Secret Mind Tests of the Army," 1919), while Yerkes would publish other controversial results, such as the average mental age of recruits being a bit over thirteen years old (Fancher, 1987, pp. 127–132).

Though the implementation and results of army testing would embolden both the mental hygiene movement and the psychological testing enterprise, they tended to overshadow the

parallel work of psychiatrists on testing for and treating neurotic soldiers. The spectre of male hysteria among soldiers was a threat to belligerents on either side of the Great War—and disciplinary psychologists would launch their own testing project on psychoneurotic recruits. As with the many tracts on nervous disorders and hysteria, research on the mental effects of combat was largely an Old-World export. Harking back to the traumatic effects of railway wrecks on the late nineteenth-century spine, brain, and psyche, the awesome and shocking new technologies of trench warfare during the Great War were transforming men on either side into, in the eyes of contemporary observers, hysterical women and helpless children. Throughout WWI the nomenclature for such a demoralizing and disquieting potpourri of neurotic symptoms among soldiers was quite diverse, including: shell shock; war strain; gas neurosis; buried alive neurosis; soldier's heart; war neurasthenia; and anxiety neurosis (Micale & Lerner, 2011, p. 17).

Among these, “shell shock” most securely sunk its roots in military and public discourse. Shell shock—a term that the inexperienced yet leading battlefield physician and psychologist Captain Charles Myers most formally introduced as medical terminology—was initially meant as a literal description of the phenomenon: the physical force of nearby exploding artillery shells were shocking soldiers' nervous systems (Myers, 1915). In addition to physically assessing the afflicted senses (e.g., blindness), Myers's work was unlike more traditional, materialistic physicians doing similar battlefield medicine. The young field doctor was willing to explore memories using hypnosis and discuss dreams with shell shocked soldiers.

Even early on, physicians who were open to psychogenic explanations of symptoms, like Myers, wondered about the actual role of explosives in producing symptoms. For instance, Myers thought it odd, given how deafening the thunder of nearby shell explosions were, that “hearing should be (practically) unaffected, and the dissociated ‘complex’ be confined to the

sense of sight, smell, and taste (and to memory). The close relation of these cases to those of ‘hysteria’ appears fairly certain” (Myers, 1915, p. 320). By 1916, as physicians like Myers encountered cases where there was no nearby shell explosion, military doctors tried to substitute shell shock with more accurate terms like war neurosis. Discerning etiology and nosology of war neurosis became even more arduous with increasing use of gruesomely fatal shells filled with poisonous gas. Yet the public found the vagueness and physicality of the “shell shock” label reassuring, as it corresponded with deep-seated values.

Before the British Regular Army was established in 1914, the Old Army had been steeped in virtues of austerity and self-control for many generations. From the point of view of veterans, senior soldiers, and many of the public, the arithmetic of war was simple: men were divided into heroes or cowards (Shephard, 2002). The craven desertions, bizarre gaits, and physically incongruent complaints of neurotic soldiers bore too much of a similarity to a most abject form: hysterical women. Some feminist historians have even compared shell-shocked men to female hysterics as a continuation of internalized protest against the Victorian era’s gender norms (e.g., Showalter, 1987).²⁰

Despite Britain’s discomfort with fully confronting the psychological aspects of warfare injury, the four-month Battle of Somme in 1917 left the army’s tented ‘hospitals’ so overwhelmed with shell-shocked soldiers it was an impossible threat to ignore. To help accommodate these undesirable yet unavoidable circumstances, the British War Office began a “crash programme in psychological medicine” at Maghull Hospital near Liverpool the following year. Scottish psychiatrist and Jung-devotee Maurice Nicoll welcomed this news, seeing it as

²⁰ Although, as scholars of the history of trauma make clear, assuming ontological equivalence among the various incarnations of trauma-associated symptoms denies the particular historical-cultural milieu necessary to produce each unique instance and understanding of the phenomenon (Micale & Lerner, 2011).

psychological perspectives officially replacing a dated and defeated medical orthodoxy (Shephard, 2002, p. 109). In the meantime, Nicoll's own work was introducing both sides of Atlantic to Jung's introvert and extrovert types, such as in a journalist's commentary on his latest book for the *New York Tribune* ("Life to the Introvert," 1918). Reports and eventually medical movies of shell shock would also make its way across that ocean as the USA entered the war.

As the National Committee for Mental Hygiene was already established, it was perhaps less difficult for American psychiatrists to enter their country's military efforts. Thomas W. Salmon had already gained experience working for the National Committee at Ellis Island during massive influxes of immigrants—potential carriers of culture-threatening physical and psychological disease (Salmon, 1913a, 1913b). Salmon was a key psychiatrist in the American efforts to understand and treat shell shock. Another American project for detecting the mentally unfit and neurotically predisposed, also often elided from the story, is disciplinary psychology's brief but methodologically fruitful efforts led by Columbia University professor Robert Woodworth.

Testing the New Normal

If the 1920s began with psychologists eager to define and classify this thing called "personality," they surely closed with a dizzying assortment of tests, articles, and theories (e.g., Allport & Allport, 1921; Allport, 1921; May et al., 1928, 1929, 1930). The numerous tests and reviews of personality research reflected a wider interwar trend of moving away from studying the "intellectual traits." That trend coincided with the beginnings of personality psychology as a recognized (though still pluralistic) sub-discipline with its own journals, monographs, and societies (Parker, 1986). Intense criticism of intelligence testing, like Walter Lippmann's debate

with Lewis Terman, might have encouraged intelligence testers to shift toward personality, or at least expand their work to also incorporate non-intellectual traits. While editorials in the *New York Times*, covering topics like mental tests, were generally enthusiastic earlier in the century, by the time of WWI all the way to the 1940s, those editorials became frequently critical of psychologists' legitimacy and supposed expertise (Dennis, 2002).

Yet intelligence testers' move toward personality also paralleled a wider change in cultural values. The early twentieth century rise of corporate bureaucracy transformed labourers' expectations and self-understanding—moving emphasis from the moral-sounding qualities of character to the scientific-sounding importance of personality: from a culture valuing production to a culture valuing consumption. Commercialization demanded personal magnetism over personal diminution (Susman, 1979). On the other hand, the terms “character” and “personality” were often used interchangeably long before and after the apparent shift from agrarian to corporate values. Similarly, emphases on self-improvement and personal magnetism existed before the shift to consumer culture and do not necessarily indicate a joint change in American conceptions of personhood (Heinze, 2003). Even though Gordon Allport, a crucial promoter of the blossoming field of personality psychology, ostensibly promoted a scientific approach of personality over character that was harmonious with the aims of corporate capitalism, his work was also infused with Victorian ideals of character such as the depth of individuality (Nicholson, 1998, 2000b, 2002). During the 1920s and onward, moral connotations both old and new, and of both character and personality, persisted in academic psychology.

This section picks up on where the last left off: shell shock in WWI. Specifically, it considers the work of prominent Columbia University psychologist Robert Woodworth in developing an inventory for assessing the psychoneurotic tendencies of army recruits. His work

is often pointed to as the beginning of personality questionnaires. Several psychologists in the 1920s would adapt Woodworth's questions in creating their own tools for measuring abnormal personalities. In the case of Colgate University's Donald Laird, a once well-known popularizer of psychology, Woodworth's psychoneurotic inventory would pave the way for an instrument meant to measure both abnormal and normal personalities. Consonant with their cultural pervasiveness, measuring Jung's introvert and extrovert types would constitute an entire schedule within Laird's inventory—marking an important point in the introvert/extrovert's transformation into a quantifiable, psychological trait.

Woodworth Worries about Normality

Born in 1869, Robert Sessions Woodworth grew up across New England as his father's work as a minister kept his family relocating. While in Ohio, his mother was a founder and principal of women's seminary at Lake Erie College, teaching many topics including mental philosophy (Winston, 2012). Woodworth wrote tongue-in-cheek about his childhood through a psychoanalytic perspective: "So you would probably diagnose me as an oldest child—Alfred Adler says it shows plainly in my 'style of life'" (Woodworth, 1932, p. 360). He went on to deny any "mother-fixations." Though Woodworth spent some time as a mathematics teacher in Kansas, he ultimately went to Harvard in 1895 to study psychology and philosophy. While at Harvard earning his Master's degree, his teachers included William James and Josiah Royce, and he began a long friendship with Edward Thorndike. In 1897, Woodworth decided to earn his doctorate at Columbia University under pioneering mental tester James McKeen Cattell; though Woodworth also studied statistics and anthropometry under anthropologist Franz Boas (Winston, 2012, p. 53). On the issue of sex differences and the causal effects of culture, Boas and Cattell differed quite widely (see Rosenberg, 1982).

Perhaps best remembered for his textbooks rather than any particular psychological theory, Woodworth helped establish a strong sense of psychology as a natural science that uses experimental methods. Given his doctoral advisor, Woodworth's work would emphasize the pairing of experimentation with the study of individual differences (Winston, 1990, 2012). Woodworth recollected Cattell as "the chief of all my teachers [at Columbia] giving shape to my psychological thought and work" (Woodworth, 1932, p. 367). Cattell, whom Woodworth referred to as his "master," prepared him for his two largest mental testing projects: association testing and psychoneurotic testing. Psychoanalysis loomed large in both these endeavours, though often implicitly, as association test experiments and the study of nervousness were well within the purview of American psycho(patho)logy. Leading up to these two major testing endeavours, Woodworth would first have an important experience at the 1904 St. Louis World's Fair.

Properly titled the Louisiana Purchase Exposition, Woodworth was appointed head of the fair's Anthropology department. Woodworth, and his assistant Frank Bruner, ran anthropometric tests on white visitors along with the fair's many members of "exotic" races who were on display. Besides museums, anthropologists would always capitalize on grand events like a World's Fair to raise awareness and legitimacy of their field and its findings (Troutman & Parezo, 1998). The 1904 fair in particular was used as an exhibition of abnormal subjects: from asylum superintendents displaying the crafts and writings of their "idiotic and feebleminded" inmates, to the education-on-display of "primitive races" like the people of Philippines (Trent, 1998). At the 1904 fair, Woodworth tested 1,100 individuals on a series of anthropometric tests, including simple tests of mental ability involving placing shapes on a "form board." Thus, at a national fair showcasing anthropology and medicine's role in the study and (re-)education of

mentally and culturally abnormal persons, Woodworth was ambassador and spectator for the blossoming discipline of scientific psychology. Though psychologists had promoted their science a decade earlier at the (in)famous 1893 World's Fair in Chicago, where they were unhappily grouped under "Department M, Ethnology" alongside anthropology and neurology (Shore, 2001, p. 71; see also Green, 2019, pp. 194–202; Larson, 2003).

Six years after St. Louis' 1904 fair, Woodworth reflected on his experiences with mental testing hundreds of subjects from different races (Woodworth, 1910). He doubted the value of typologies for a scientific psychology, whether it represented nationality (e.g., a Frenchman), a locality (e.g., a Bostonian), or a vocation (e.g., a scientist).²¹ Woodworth offered much caution when considering the relationship between the typical group member, the average group member, and the actual group member: "you may have to search long for an individual who can be taken as a fair example ... What we should like is some picture or measure of the *distribution* of a given trait throughout the members of a group. (Woodworth, 1910, pp. 787–788, his italics)

Before Woodworth was assigned to study shell-shocked soldiers in the latter-half of the Great War, he was also no stranger to psychoanalysis. While fully committed to fashioning an experimental psychology, Woodworth was always an eclectic researcher, studying topics such as reflexes, imageless thought, and time perception (Winston, 2012, p. 54). His wide interests in psychological experimentation allowed him to explore many popular methods, including a large review of the many types and uses of association tests (Woodworth & Wells, 1911). The association test review was part of a report of the larger American Psychological Association's Committee on the Standardizing of Procedure in Experimental Tests. Woodworth and his

²¹ Arguably, his reflection anticipated not only personality psychology's critique of personality typing and favouring of theorizing trait continua in order to capture the normal subject, but also the still ongoing internal critiques of interindividual trait research methods (e.g., Allport, 1962; Lamiell, 1981; Molenaar, 2004).

colleague Frederic Lyman Wells formed the sub-committee on association tests (Winston, 2012; Woodworth & Wells, 1911, p. 1).

In their review, Woodworth and Wells identified three main purposes for association tests: measuring the speed of formation of new associations; mental diagnosis; measuring mental alertness. When writing about the test's use for mental diagnosis, the authors conceded a psychoanalytic perspective could be used not only for diagnosing disturbing ideas or uncovering repressed knowledge, but simply showing the "lines of thought with which an individual is conversant, and the sort of relationships along which his mind habitually moves" (Woodworth & Wells, 1911, p. 5). Additionally, when discussing using the test as a measure of mental alertness, the authors explain how periods of confusion in a test subject likely stemmed from the "emotional value of certain stimulus words," as Jung and "his school" highlighted (Woodworth & Wells, 1911, p. 6). Despite these psychoanalytic perspectives and uses, the reviewers doubted the potential for standardizing association tests for such purposes given how deeply individual complexes and emotional values were for each test subject.

Association tests of all kinds had already infused many locales of American psychology, including some of its earliest and most contentious research on sex differences. Pioneering psychologist Mary Whiton Calkins, who established a research laboratory at Wellesley College and successfully defended her PhD dissertation at Harvard on the association of ideas despite not being allowed to receive the degree, entered a dispute with Joseph Jastrow about the use of association testing in investigating sex differences (Furumoto, 1979, pp. 352–353).

The debate over the appropriate interpretation of their results was published in an early volume of *Psychological Review* (Calkins, 1896; Jastrow, 1896a, 1896b; c.f., Tanner, 1896). Both Jastrow's and Calkins' research indicated that when women were prompted to write down words

that came to mind, they tended to write words associated with interior furnishing and food. Yet the researcher's parted ways in their interpretations of the experimental findings. In Jastrow's eyes, the women's associations clearly revealed the innate preferences of the female mind. Calkins thought differently; to her, the women's associations came from exposure and habit, not necessarily an innate predisposition for domesticity.

Whereas Woodworth's diverse career would only occasionally return to testing and pathology, Wells' career would continue along this route. Born in Beacon Hill in 1884 to a family with connections to the affluent and scholarly class of Boston, Wells ultimately earned his PhD at Columbia University—where his teachers included Woodworth (Shaffer, 1964). Among his many medical, clinical, and military appointments, Wells was appointed Assistant in Pathological Psychology at McLean Hospital in Waverley, Massachusetts. It was during his time at McLean, at the age of twenty-six, he collaborated with Woodworth on association tests for the APA committee on Experimental tests. Though younger than the earlier generation of Boston psychopathologists, Wells was just as keen to engage with psychoanalysis. In particular, Wells could be considered an early example of clinical psychology's attempts to fit psychoanalysis in with psychology's twin commitments: mental hygiene and adjustment (Pietikäinen & Clark, 2017, p. 25; c.f., Napoli, 1981).

During his time at McLean Hospital, he produced several articles on the themes of tests and hygiene in clinical contexts. These were building toward one of his more substantial contributions, *Mental Adjustments* (Wells, 1917). Covering the gamut of topics in psychopathology, from association to dissociation, with an eye toward the ideal of mental adaptation, Wells' *Mental Adjustments* seemed to survey all he had learned up to that point. The book is also an early example of a disciplinary psychologist incorporating Jung's personality

types, understood here as a state or process of introversion. In Wells' view, introversion was a wasteful process of seeking pleasuring within when confronted with an unpleasant environment. Such a "short-circuiting of electric energy" could lead to serious maladjustment and disease (Wells, 1917, esp. pp. 61–65).

Like Wells, and many others, Woodworth was also interested in Freud's cause (e.g., Woodworth, 1917)—especially as psychoanalysis continued to gain popularity alongside a gradual turn toward behaviouristic discourse in psychology. Woodworth's critique of psychoanalysis was multifaceted, but the limits of psychoanalysis' methodology was central. Given the weakness of psychoanalytic methods, and the preoccupation with only certain aspects of human personality (i.e., sexuality), Woodworth saw psychoanalysis as not totally devoid of value but certainly worthless for a scientific psychology. Nevertheless, during the same time Woodworth was working on his critique of Freud, he was unavoidably engaged with psychoanalytic perspectives on neurosis while working for the military.

Once America entered the First World War, the APA assigned many psychologists to wartime efforts, including a middle-aged Woodworth. He was assigned to create a test for "emotional stability" (Woodworth, 1932, p. 374). Woodworth realized that the army's susceptibility to shell-shock or war neurosis was as grave as the lurking scourge of latent feeble-mindedness. He decided that pursuing the symptoms that psychopathologists, neurologists, and psychiatrists were elucidating in their neurotic cases would be the most productive avenue of inquiry. After pouring over hundreds of clinical case histories, Woodworth had hundreds of possible symptoms and "threw them into the form of a questionnaire which could be applied to a group of subjects at a time" (Woodworth, 1932, p. 374). Albert Poffenberger, a fellow Columbia psychologist and later advertising researcher helped in writing up the test's first version.

After some question-pruning once determining to which questions normal subjects frequently provided affirmative answers, a revised version was applied to “one of the camps” of troops (Woodworth, 1932, p. 374)—apparently with the help of E.G. Boring (who also greatly helped with Yerkes’ intelligence research on recruits). Woodworth then applied the test to a sample of diagnosed patients. In his autobiography, Woodworth claimed that a recruit’s quantitative score on his test could serve as a filter and complement to further psychiatric assessment for those prone to psychoneurosis (c.f., Winston, 2012). Though these results were shortly after presented at a conference for the Surgeon General, resulting in the decision to trial Woodworth’s test in the recruitment process, WWI ended. Woodworth’s journey in creating his Psychoneurotic Inventory, what soon became relabeled as the Personal Data Sheet, was a brief detour in an academic career built on eclectic content and stringent methodology.

Woodworth and other enlisted psychologists reported their wartime efforts at the 27th meeting of the American Psychological Association in Baltimore, for which a report was published in *Psychological Bulletin* the proceeding year (Langfeld, 1919). The 1918 meeting occurred at Johns Hopkins University in affiliation with the American Association for the Advancement of Science, and “men in service” made up a considerable portion of the sessions’ audience members. Special joint sessions included “Scientific Personnel Work in the Army,” “Scientific Contributions of the Educational Survey,” and symposium on “The Future of Pure and Applied Psychology” led by “Major” Robert Yerkes and “Mr.” Edward Thorndike. Reflecting the rising demands and patronage of psychological science during wartime, the speakers seemed to acknowledge the pure and applied psychology split while championing both approaches virtues. Apparently Hall commented: “We should keep the science pure, but not so pure as to get our feet off the earth and thus not be able to help mankind. Our motto should be

‘service’ in the best sense” (Langfeld, 1919, p. 34). Nearly all of the research presented that year was on some aspect of warfare and soldiering: from personnel selection and special aptitude tests, to intelligence testing, to the effects of war on troop morale.

In the *Psychological Bulletin* report, below Stanford University’s Lewis Terman’s report on methods of intelligence test for literate and illiterate men, and directly after Columbia University’s Leon Thurstone’s report on using psychological tests for the selection of telegraphers, was Woodworth’s report on the “Examination of Emotional Fitness for Warfare.” In this initial report to the APA, as in his autobiography years later, Woodworth saw the work of psychologists and psychiatrists being clearly delineated. The psychologist could administer an initial quantitative mental test, such as the list of neurotic symptoms he compiled. The “neuropsychiatrist” would conduct an individual examination of any abnormal soldiers. After achieving success on draftees at Camp Upton, Woodworth decided that his list of questions was best used in a similarly twofold manner: “Qualitatively, such a list of questions can be used to furnish clues [sic] to be followed up by oral questions [by a psychiatrist]. Quantitatively, the plan is ... to base the question of further examination on the total score” (Woodworth as quoted in Langfeld, 1919, p. 59).

Woodworth theorized, along with other psychoanalytic therapists and psychological testers, that a mild presence of some symptoms was perfectly normal for an otherwise psychologically healthy person. Exactly where the threshold demarking the normal from the abnormal person lay remained undetermined, though Woodworth had some general notions given his testing experiences. Going off his experiences with identified neurotics at Camp Upton, along with shell shocked soldiers at other locations, neurotics appeared to answer positively to at least twenty to thirty of the hundred symptom list. Even though Woodworth had excluded

original symptoms that were too commonly found among normal subjects for this final version of the psychoneurotic inventory, healthy individuals still responded positively to a number of them. The difficulty in distinguishing the normal from the abnormal in psychological testing would quickly be considered an advantage. For one, psychologists would promote Woodworth's inventory and its adaptations as a tool for discriminating patients from persons (H. E. Garrett & Schneck, 1928). Additionally, tests that adapted or incorporated Woodworth's inventory could be promoted as not only a mental hygiene tool, but a tool for measuring the personality of normal subjects, such as their vocational interests (Laird, 1925a). The twofold potential of personality testing represents two of testing psychology's major promises of the twentieth century: a measure of a person's defectiveness and a measure of a person's social utility.

By the 1930s, after his inventory had become bedrock for more than a decade of psychological testing efforts, Woodworth seemed generally unimpressed at the path his test had taken: "Since the War, quite a number of psychologists have used the questionnaire or modified forms of it, and, though the results have never been striking, it still seems to have possibilities of usefulness" (Woodworth, 1932, p. 374). Despite Woodworth's subdued estimation of his inventory's importance, it is now remembered as likely the earliest self-report personality questionnaire—and certainly one of the most pervasive and enduring measurement tools in the discipline's history. Though industrial tools for measuring neurosis would grow numerous from the 1920s onward, and fuse with tools for measuring introversion in the 1930s, many share their source in Woodworth's psychoneurotic inventory (Gibby & Zickar, 2008).

[Donald Laird: Forgotten Popularizer](#)

Although psychopathologies would encroach upon normal psychological life in American culture WWII, especially with advent of the "growth industry" (Herman, 1995), the

post-WWI era saw many efforts at linking clinical categories to traits of a normal personality. In addition to Freud's efforts of extending his psychology to all normal subjects, psychologists straddling established areas—like psychopathology—and emerging areas—like personality psychology—were building their own bridge between the abnormal and the normal. All the while psychological testers were imbuing the normal personality with a quantitative perspective that offered continuity between pathology and normality.

Quickly after Woodworth's initial research and psychoneurotic inventory, his measure was repackaged as the Woodworth Personal Data Sheet (WPDS). This tool helped establish a viable route for psychological testing that grew alongside intelligence testing: the testing of temperament. While educators and eugenicists were interested in a person's inherent (and likely inherited) mental ability, factory and other business owners were more concerned about an employee's inherent emotional stability. Several tests adapted from the WPDS, alongside competitors like the Humm-Wadsworth Temperament Scale, were marketed and sold to businesses looking to boost efficiency by filtering out neurotic (or emotionally unstable) employees (Gibby & Zickar, 2008; Lussier, 2018a). As with the illegitimate uses of feeble-mindedness and intelligence testing to target, institutionalize, and often sterilize, a variety of social outcasts, measures of emotional stability, it was believed, could be used to weed out political dissenters. Eventually, psychological tests based on the WPDS were implicitly marketed as a tool for circumventing the legal protections of labour unionists against discrimination (Zickar, 2001).

Whether they were used with genuine concern for a worker's predisposition to neurosis, or as an anti-organized labour filtering mechanism, tools for measuring psychoneuroses became paramount to personnel selection. A prime example of this work is Donald Laird's update and

expansion of the WPDS. Laird was a prominent popularizer of the discipline from the 1920s until his death from lung cancer a few days before Christmas, 1969. Laird sustained an eclectic career researching everything from the effects of city noise pollution and odours on human psychology, to sleep patterns and efficiency, to business leadership and managing women in the workplace (“Dr. Donald Laird,” 1969).²²

Taking no issue promoting his diverse psychological research, many of Laird’s projects resulted in books blurring the line between the academic and popular (e.g., Laird, 1927, 1935; Laird & Laird, 1942). His work was often reported in *The New York Times* (e.g., Associated Press, 1933; Kieran, 1933; Laird, 1925e). On top of his popularity in the press, Laird maintained his diffuse output in various professional outlets, such as *Industrial Management*, *Factory and Industrial Management*, *Office Economist*, *Medical Record*, and *Nation’s Health* (Laird, 1921, 1925c, 1925d, 1928, 1933). Like Hollingworth and Woodworth, Donald Laird gained experience working alongside medical professionals during the 1910s. For Laird, working as a psychological tester during wartime would not only inform the testing needs he saw on the horizon, but the job also helped him meet his future romantic and academic partner.

The Lairds met during WWI while Donald was working as a psychological examiner at the Great Lakes training station, where his soon-to-be wife Hilda Drexel was working as a psychiatric nurse (“Dr. Donald Laird, Popularizer of Psychology, Dies,” 1969). Throughout Donald’s career, Hilda was apparently crucial for the psychological experiments at Colgate University in upstate New York—he referred to her as a “silent partner” (“Mrs. Donald A. Laird, Psychologist’s Wife,” 1938). She was eventually credited as co-author later in their previously

²² Information also from “Donald A. Laird tribute” written by F. Kenneth Berrien, 1970. Found in A1132, Department of Psychology Records, Special Collections and University Archives, Colgate University. For Berrien’s obituary, see Bochner (1971).

unacknowledged joint career. In 1925, Donald became the director of Colgate University's Psychology Department and converted what was a college chapel on the second floor of Colgate's Alumni Hall to a research laboratory. Known as "Doc," Donald held audiences of students for seminars, but also evenings meant for faculty, where "Doc" apparently entertained them with Gilbert and Sullivan operas sometimes in apropos garb, such as a "Japanese kimono for the *Mikado*."²³ Catching a glimpse of Laird's endless enthusiasm, *The New York Times* reported on his work on the history of Colgate University's psychology department ("Psychology Start Hailed at Colgate," 1936).²⁴ His prominence within the NYT endured, as even his resignation from Colgate University warranted a mention ("Dr. Laird Resigns at Colgate," 1939).

No doubt reflecting his experiences as a psychological examiner during WWI, one of Laird's most impactful projects was developing a personality inventory. Laird's inventories borrowed heavily from Woodworth's original list of dichotomous questions on psychoneurosis or emotional stability. Laird worked with psychologists across the country—from Wyoming to Connecticut—in trying "various devices" for detecting psychopathology (Laird, 1925b, p. 128). Among his (often unnamed) collaborators, Laird seems especially appreciative of Kansan psychiatrist Karl Menninger's cooperation.

Colgate Test Separates the Wheat from the Chaff

Menninger, along with his physician father and brother, had recently founded the soon-to-be famous Menninger Clinic in Topeka, Kansas. Karl obtained his medical degree at Harvard in 1917 specializing in psychiatry, and after some time working at the Boston Psychopathic Hospital, returned to Topeka to establish his family clinic (Carney, 1993). Possibly due to his

²³ From Berrien's memorial. Found in A1132, Colgate University's Department of Psychology Records.

²⁴ This *NYT* article reported Laird as being Department Head since 1927, conflicting with their own obituary for Laird.

exposures to psychoanalysis, including a 1934 meeting with Freud, or the dismal financial straits of the family clinic during the Great Depression era, Karl Menninger strongly promoted the New Psychiatry at the Menninger Clinic. The Clinic's decisive movement to a psychoanalytic direction entailed opening its doors to several psychoanalytic emigrants—though many would soon leave for the more refined locales of the eastern seaboard (Carney, 1993).

Additionally, during the Depression era, Karl was using public venues, such as *Ladies Home Journal*, to extol the virtues of psychiatric intervention for greater mental hygiene (K. Menninger, 1930a, 1930b, 1931a, 1931b, 1931c, 1931d, 1931d, 1932). This series of “Mental Hygiene in the Home” articles covered topics of parenting, love, and happiness. They mainly consisted of readers writing Dr. Menninger for advice on problems in their lives. Given Menninger's aims of expanding a psychoanalytically-grounded psychiatry into a general family practice, his cooperation with Laird is unsurprising. Laird's first attempts at a creating a personality test grew out of a need to expand mental hygiene efforts beyond intelligence tests.

Tentatively calling it a “Test of Abnormal Mental Traits,” and later calling it the Colgate Personal Inventory, Laird wrote that it belonged to the many other attempts at filling “the lacunae in non-intelligence tests” (Laird, 1925a, p. 419). Though Laird's initial instrument was developed for as a more objective and efficient diagnosis, he presumed an axiom shared among many disciplinary psychologists during the burgeoning days of psychological testing: “the traits which are characteristics of mental ill-health are but exaggerations of traits of behavior present in all humans” (Laird, 1925b, p. 128). He also noted that developments in psychiatry supported this assumptions, perhaps alluding to the impact of Freud's *The Psychopathology of Everyday Life* (Freud & Brill, 1914), or merely with his probable correspondence with Menninger. To that end, Laird's test of abnormal mental traits was promoted as doubly useful: It was at once a test of

mental hygiene and a vocational test.²⁵

Laird and his colleagues tweaked Woodworth's original list of questions. For reasons not explicitly stated, any questions about childhood were removed; questions about bowel movements and urination were omitted as unsuitable for group testing. Laird's tool was separated into schedules that followed psychiatric categories. Schedule B1 was a catch-all for many neuroses and even psychotic conditions: psychasthenoid, neurasthenoid, hysteroid, and schizophrenoid. The qualifying suffix attached to these otherwise familiar categories indicated a person's tendency toward these pathologies (Laird, 1925a, p. 419).

Looking at the actual questions of early tests like Laird's can elucidate not only how psychologists at that time were curating symptoms, beliefs, and behaviours as indicators of the Big Two. The content of the questions also reflects the implicit values that delineated a normal subject from an abnormal subject, often entangled with capitalistic values about the proper conduct of efficient and self-sufficient men. Some questions from the 1925 version of the B1 schedule included: "Have you been afraid of responsibility?"; "Have you ever experienced a great mental shock?"; "Have you been able to do good work while people are looking on?"; "Have you been rattled easily?"; and "Has work tired you quickly?" (Laird, 1925b, pp. 131-4).

On the other hand, Schedule C1 of the Colgate Personal Inventory was a collection of questions intended to assess a subject's level of introversion. Despite the attempt at distinguishing introversion from the previous list of neurotic indicators, it is apparent that Laird

²⁵ Laird incorporated a graphic ratings scaling technique in his test in order to obtain its dual status as a test that could discern normal from abnormal subjects while serving as a way to determine a worker's vocation. The questions were adapted from Woodworth's inventory items, which had questions followed with a simple YES/NO answering format. In Laird's adaptation, the questions were followed with a series of possible answers and a dotted line running above them. The person filling out the questionnaire had to check above the answer they felt best described themselves, then the test scorer would use a stencil to quickly gauge where the checkmarks lay. The stencil for mental hygiene purposes blocked the middle 50%, only exposing the extreme checkmarks; the stencil for vocational purposes did the opposite.

and his team could not fully untangle the two. While there are many questions about social relations like conversations, public speaking, working with others, and interactions with the opposite sex, there were also questions such as: “Have your moods changed without apparent cause?”; “Have possible troubles bothered you?”; and “Have your feelings been easily hurt by remarks or actions referring to you?” (Laird, 1925b, pp. 135-8). These kinds of questions suggest the pathological dysfunctions of mood and nervousness identified in neurotics were also assumed to be present in introverts. Consonant with Laird’s commitment to a psychology applicable to the needs of commerce and industry, some of the C1 questions considered the test-taker’s economic worth: “How have you been about making loans?”; “What sort of work have you liked best?”; and “How have you been at selling things” (Laird, 1925b, pp. 135-8).

Thus, within a few years of the 1923 English translation of Jung’s *Psychological Types*, early personality testers saw introversion on an equal footing with the classical neurotic entities. In the 1930s and throughout the remaining twentieth century, personality testers and theorists became practically incapable of studying one without the other. As neurosis had found its way into the management of workers’ inner-lives, Jung’s personality types were gaining attention in a bustling commercial expansion. In fact, Laird likely borrowed his introvert and extrovert questions from a psychologist working in advertising named Max Freyd.

Those Corporate Advertising Types

The 1920s was a time when early forms of personality testing—tests of neurosis and introversion chief among them—were entering many capitalistic venues. During the interwar era, a newfound zeal for personal efficiency under the watch of Taylorist scientific management buoyed a rapidly accelerating corporate industry. Green (2019) argues that the earlier organizational revolution, especially the transformation of American institutional structures

within its rapidly expanding cities, was “single most significant aspect of the socio-economic environment in which American psychology took root” (p. 191).

Though there were many other professionals interested in studying labourers and consumers—like engineers and sociologists—some of disciplinary psychology’s earliest instances of applying its science to industry came from former students of Wundt: James McKeen Cattell and Hugo Munsterberg. During the turn-of-the-century rapid expansion of industry, applied psychology journals in general, like the *Journal of Personnel Research*, would become more and more common (Koppes & Pickren, 2007). With major companies like Macy’s Department Store or Procter & Gamble increasingly employing psychologists, some were leaving the universities entirely—either through their own branching out to businesses or vice versa (Vinchur & Koppes, 2007).

Advertising was also keeping pace with the new efficiency and an exploding production of goods. Ads were not only ensuring the ratcheting up of consumption to match production; they were themselves becoming a massive and lucrative industry devoted to selling, fulfilling, and creating desire. National advertising, both feeding and feeding off an emerging culture of consumerism and therapeutics, strengthened new notions of self-realization (Jackson Lears, 2000). Advertisements as a medium were an extension of earlier visual media, represent old desires of abundance told as new fables (Jackson Lears, 1994). Though they have always served a function—sometimes purely informative; sometimes persuasive—advertisements can be read for the cultural values, fears, and notions of selfhood therein. And although in visual legend and folklore earthly and spiritual abundance were often entangled, by the seventeenth and eighteenth centuries abundance also meant commercial expansion and establishing trade.

This section examines the little-known contribution of Philadelphia psychologist Max

Freyd. While briefly working in corporate advertising in the mid-1920s, Freyd drew on Hinkle's translation of Jung to create perhaps the most influential list of questions for discerning introverts from extroverts. Of course, like any other tool created in the human sciences, its manifest and latent content belies the morals of personality during that place and time. For example, extreme introverts lacked leadership, nerve, and their aloofness suggested sympathy for radical politics. The section ends with a broader sampling of the many psychologists hard at work throughout the decade trying to domesticate Jung's personality types at the same time as expanding the theories and tools of personality traits. The 1920s researchers recounted throughout this chapter, all working toward conceptualizing and quantifying (ab)normal personality testing, would inform the next decade of (often factor analytic) research on personality traits

Behaviour in Advertising

In the early twentieth century, Northwestern University professor of psychology and commerce Walter Dill Scott had been publishing articles that culminated in his influential *The Psychology of Advertising* (Walter Dill Scott, 1908)—though he largely repackaged standard marketing wisdom as scientific classifications (K. W. Buckley, 1989, p. 138). Scott was one of the discipline's earliest and most successful industrial psychologists, eventually building on his experiences with psychological testing during WWI to form the Scott Company in 1919. His Company would work with many major clients, like Westinghouse Electric and Manufacturing Company, and would lay influence personnel management and evaluation (Lynch, 1968).

Scott's psychology of advertising depended on the assumptions of an irrational and suggestible consumer, much like a patient under hypnosis (Kuna, 1976). After WWI, public relations pioneer Edward Bernays—famously Freud's grand-nephew²⁶—was formulating and

²⁶ Much—perhaps too much—has been made about Bernays' relation to Freud, his application of psychoanalysis to marketing, and the long-term cultural effects therein (e.g., Curtis, 2002).

implementing indirect modes of persuasion (Bernays, 1923, 1928). Though drawing from principles of psychopathology, the idea of suggestion in advertising usually carried an associationist view of causality—a view compatible with the classical conditioning of

By the 1920s in the USA, advertisements were coded with new sources of abundance and plenitude: industrial efficiency and mass production; and the complementary pairing of personal efficiency and mass consumption. Stanley Resor, the new president of soon-to-be advertising giant the J. Walter Thompson Company (JWT), understood these newer economic emphases. A Yale graduate, and an early example of a college-educated man to ascend the advertising industry, Resor made sure to hire likeminded intellectuals (K. W. Buckley, 1989, p. 135). First, he hired Paul Cherrigton, a Professor of Marketing at the Harvard Business School and disciple of Frederick Winslow Taylor, to use the principles of scientific management within marketing: essentially to quantify the buying habits of consumers. To further achieve this, Resor, like many other businessmen of that time, looked unto the discipline of psychology for answers—specifically the latest behaviourist version of the field: “[W]e were in hopes that [behaviourism] could formulate into actual laws some of the hunches that we had found to have worked out too often to be mere chance or even coincidence.”²⁷

To that end, Resor hired the ever-enterprising behaviourist John Broadus Watson. The JWT were initially concerned about Watson’s intellectual integrity, in light of his recent firing from Johns Hopkins due to having an affair with one of his students (Buckley, 1989, p. 132). Many of Watson’s colleagues came to his defense in letters of support for his candidacy—even E.B. Titchener, a strong critic of Watson, sent such a letter (though not without a peppering of

²⁷ Stanley Resor. Letter to Henry T. Stanton. July 13, 1923. Found in Personnel Records Box 10 (Max Freyd), J. Walter Thompson Company Personnel Records (hereafter JWT-PR), David M. Rubenstein Rare Book & Manuscript Library, Duke University.

caveats regarding Watson's temperament). Resor's hope for Watson within JWT was to unveil the heretofore invisible laws of consumer behaviour to ultimately shape their buying habits. But before Watson could achieve this, he had been moved to a different position within the company. behaviourist psychology (Jackson Lears, 1994, p. 208).

Watson was no stranger to applied psychology. As early as 1921, he joined the Psychological Corporation's board of directors, a company a few other Columbia University psychologists, including McKeen Cattell, established after WWI in light of the success of the Scott Company. Watson's new position as an advertisement executive was a much more affluent but riskier path than continuing his academic career. While at JWT, Watson's advertising techniques promoted style over substance, such as the use of indirect testimonials that associated mundane products with exciting symbols to stimulate desire rather than appeal to reason (Buckley, 1989, pp. 139–141). Although there was much skepticism among consumers and advertisers alike about testimonials earlier in the twentieth century, a certain credibility had been restored through the use of celebrity endorsements (especially those of elegant actresses endorsing products from the cosmetics industry). Rather than a psychological innovation courtesy of Watson's associationist techniques, this advertising strategy was in-keeping with JWT president Resor's views on the consumers tuning into the advice of individuals superior in knowledge and taste (Schweitzer, 2005, p. 270).

Acknowledging that the only chances of Watson ever fulfilling his initial nomological task would be "by burning the midnight oil," Resor charged Watson with finding a replacement psychologist.²⁸ It was perhaps through this new appointment at the Psychological Corporation that Watson became aware of Max Freyd. Upon hiring him in 1923, Freyd was a recent graduate

²⁸ Resor to Stanton. July 13, 1923. Found in Personnel Records Box 10 (Max Freyd), JWT-PR.

from Pittsburgh who had experience with both classic and newer forms of testing, such as association tests and personnel selection. In his first year at JWT, the head of its Chicago office reached out to Resor (in New York with Watson and Freyd) to commend the new hire: Freyd had identified historical inaccuracies in a recent campaign that ran with an incorrect foundation date of Philadelphia. In the most Freudian of parapraxes, the Chicago office had written Freyd's name as "Freuyd." Whether it was his experience, or the suggestive psychological astuteness of his name, Freyd was likely JWT's second psychologist within its ranks.²⁹

Freyd was born in 1896 in Seattle, Washington, and earned his Bachelor's and Master's degrees at the University of Washington. In 1920, he traversed the continent to study in Pittsburgh at the Carnegie Institute of Technology (now Carnegie Mellon University) on a Division of Applied Psychology fellowship. While there he worked in some capacity with the Bureau of Personnel Research, "an organization which is maintained by a number of corporations of national scope."³⁰ As with many other psychology graduates of the early twentieth century, Freyd's experiences with psychological testing intersected with a formative experience working alongside medicine's approach to abnormal psychology. For a summer job in 1921, he researched in the Department of Psychology at the Boston Psychopathic Hospital, associated with Harvard Medical College. After receiving his doctorate in 1922, he spent that summer researching personnel selection methods for the Federated Engineers Development Corporation.

Just a few days after New Years on 1923, John Watson reached out to Freyd on behalf of JWT, offering him the position of Psychologist at a starting salary of fifty dollars per week

²⁹ Stanton to Resor. July 9, 1923. Found in Personnel Records Box 10 (Max Freyd), JWT-PR.

³⁰ Freyd's handwritten resume. Undated (likely between 1923 and 1925). JWT-PR.

(around \$750 USD, adjusted for inflation).³¹ When he was offered the position, Freyd was in the midst of his first academic year as Instructor in psychology at the University of Pennsylvania. Freyd was associated with that university's Psychological Laboratory and Clinic, where pioneering clinician and eugenicist Lightner Witmer held his position as Director. While there he apparently designed personnel tests for the Collins Service in Philadelphia to reduce their high turnover rate of salesmen, basing his tests on work he had developed for the Federated Engineers.³²

Additionally, while teaching “ninety Wharton School seniors”, he had also submitted them to an association test exploring the relationship between generic products and specific brands. For example, providing a stimulus word like “rubber heels” and asking for a brand as their response. Freyd wondered whether the JWT would be interested in such research.³³ In fact, Watson was interested if association tests could be adapted for “consumers rather than college students or advertising men” Despite Watson’s dependence on associationism in his own work, and the congruence with psychoanalytic association techniques like Bernays, Watson seemed to doubt the validity of any verbal associations consumers might make generalizing to their actual purchases. Watson sarcastically wrote Freyd that his associations with breakfast food might lead him to say a product like “‘Force’ or ‘Sunny Jim’—yet I would not eat it on a bet.”³⁴

Coincidentally, during the year of Freyd’s hiring at JWT, Donald Laird was exploring the very question of how useful association tests were in advertising research. Laird viewed advertising as “one of the first fields to be invaded by the applied psychologist,” and considered introducing the association test as obvious as the tool was one of the “chief bulwarks” of

³¹ John B. Watson. Letter to Max Freyd. January 5, 1923. JWT-PR.

³² Freyd to Watson. January 22, 1923. JWT-PR.

³³ Freyd to Watson. January 22, 1923 JWT-PR.

³⁴ Watson to Freyd. January 26, 1923. JWT-PR.

classical experiments. (Laird, 1923, p. 357). Confirming Watson's suspicions, Laird found little congruence between a brand a participant thought of and a brand they actually purchased. The finding was initially more pronounced in women participants, but then that reversed in a four-month follow-up. Relegating the association test to armchair psychology, Laird concluded it might not be suitable for the purposes of advertising research, despite how invaluable the test was "to the structuralist in ascertaining how the mind is composed" (Laird, 1923, p. 365).

In the initial letter offering Freyd his new position, Watson (at the request of President Resor) asked Freyd to begin his research straight away. Specifically, Resor wanted Freyd to "keep an eye" on a few key products they were advertising or preparing to advertise. They wanted Freyd study how they were sold, from daily tactics like "whether window displays and trims are often given" to long-term trends such as "whether sales are increasing month by month or dropping."³⁵ All the products Watson listed were aimed at either improving bodily function or appearance: Pebecco was a toothpaste advertised to prevent tooth pain and ugliness; Pond's Cold Cream and Vanishing Cream was essentially a facial beauty cream; Odorono was a harsh yet effective deodorant; Cheramy was an elegant woman's perfume; and Sloan's Liniment was meant to reduce muscle pain in workers. A few years later, Watson informed a JWT staff meeting that consumers were willing to apply Odorono, now a best-selling product, to their body despite it being caustic enough to hurt their skin and damage their clothing. By 1930s, ads were running with illustrations of people's faces disgusted by smelly sneakers or filthy hands; odor was a consumer obsession (Lears, 1994, p. 172).

Having come from the academic world, Freyd seemed unsure about the point of collecting so much information on the requested products through such haphazard methods. He

³⁵ Watson to Freyd. January 5, 1923. Found in Personnel Records Box 10 (Max Freyd), JWT-PR.

was likely used to more precise and self-directed forms of research, but as academic scientists became industrial researchers in the twentieth century, their managers would have to inculcate them with a new moral compass. Rejecting the burdensome individual genius for manageable teams of average workers, industrial research managers extolled personal virtues in recruits that were compatible with this vision, such as initiative, energy, loyalty, and cooperativeness (Shapin, 2010, esp. pp. 183-5).

Likely recognizing Freyd as a teamwork tyro, Watson let him know: “you will increasingly have to gather such information for no particular purpose in order to get a reservoir of information ... Apparently there is no information ... which is not useful at one time or another to an advertising agency”.³⁶ Comically, Watson added even more products to Freyd’s research pile: Formamint (its slogan: “The Germ-Killing Throat Tablet”), Dr. King’s New Discovery (bitters for coughs colds, and all “lung troubles”), and Dr. Bell’s Pine-Tar-Honey Compound for coughs and colds (“Like the Sun’s Rays through a Cloud”). Freyd’s tenure at JWT was fleeting—he resigned in May of 1925.³⁷

Introverts: Dissident Men of Inaction?

After his brief time at the JWT, Freyd continued building on his experiences working as an industrial psychologist. He began working at the Personnel Research Federation in 1925 and his personnel research culminated in a book on *Procedures in Employment Psychology* (Bingham & Freyd, 1926). Freyd’s co-author on that publication, Walter Van Dyke Bingham, was one of the more established industrial psychologists of the era. After 1928, Freyd would relocate again to work with the Retail Research Association. His time spent as an in-house psychologist at JWT, though a brief tenure, undoubtedly helped Freyd market himself and

³⁶ Watson to Freyd. January 24, 1923. Found in Personnel Records Box 10 (Max Freyd), JWT-PR.

³⁷ Freyd to Resor. May 14, 1925. JWT-PR.

psychological research as essential for effective hiring, managing, and selling practices. His time spent at JWT would also produce one of Freyd's most noteworthy contributions to psychology: his list of introvert descriptions. Applied and personality psychologists would continue to draw on Freyd's list when constructing the earliest psychological tests for the Big Two.

Freyd had experience in designing psychological tests. During his busy time at the University of Pennsylvania, Freyd was working on what would become a highly-cited methodology review article on the process of scaling an individual's degree of whichever psychological trait was intended to be measured (Freyd, 1923). His review focused on a newer method, the graphic rating scale: a combination of asking the subject to draw a check on a horizontal line representing a range of a trait and asking the subject to indicate which phrase(s) corresponds most closely with their own (or the subject they are judging's) level of the trait.

Freyd likely first encountered the graphic rating method during his summer working for the Bureau of Personnel Research at Carnegie Institute of Technology, as he mentioned its use there, though he noted the method originated in the personnel research laboratories of the Scott Company. Taking inspiration from Galton's work on mental imagery, Freyd demonstrated possibilities for a graphic rating scale, supplying example questions like that might indicate his interest in introverts and extroverts: "Is he quiet or talkative?" (Freyd, 1923, p. 92). Freyd expanded that, depending on the directional wording of the phrases under the trait-rating line, the distribution of raters' checks along the line will change—with a symmetric distribution cresting in the middle, resembling a bell, being the goal. Freyd listed many possible uses for this scaling method, including personality and vocation tests.

In 1924, while still associated with JWT, Freyd published his list of introvert and extrovert questions in *Psychological Review* (Freyd, 1924). He was deeply steeped in both

psychoanalytic writings and psychological testing research. Freyd understood the two “complementary types of personality” as originating in Jung, pointing to *Psychology of the Unconscious* (likely Hinkle’s translation) as their genesis (Freyd, 1924, p. 74). Freyd tried to include the viewpoints of various personality researchers in his review of various research on introverts and extroverts, including that of Jung popularizers like William White, Maurice Nicoll, and Beatrice Hinkle; the racial work of William McDougall; as well Allport brothers’ work on traits and June Downey’s work on measuring will and temperament.

In what would become a leitmotif in reviews of introversion studies, Freyd remarked that writers before him rarely attempted a precise definition of the introvert and extrovert personality types. Rather, they have so far been happy with a list of descriptions of such a person fitting their respective type. Freyd proposed two definitions based on previous writings: an introvert was an “individual in whom exists an exaggeration of the thought processes in relation to directly observable social behavior,” while an extrovert possessed “a diminution of the thought processes in relation to directly observable social behavior” (Freyd, 1924, pp. 74-5).

Freyd’s list of questions, written while at JWT, lived on as the fifty-four traits of introverts and extroverts (Freyd, 1924, pp. 78–79). The content of Laird’s Schedule C2 questions suggest he or his research team drew their influences from Freyd. Either way, both Freyd and Laird’s work would inform future personality questionnaires. In addition to the examples of Laird’s questions mentioned above, Freyd’s list of introvert tendencies belied cultural assumptions about being an effective and enterprising man (Freyd, 1924, pp. 79-80). An introvert apparently “avoids leadership at social affairs” and “shrinks from actions which demand initiative and ‘nerve’.” Even worse, an introvert “limits *his* acquaintances to members of *his* own sex,” “is effeminate (if a man),” and “believes in ‘mind’ cures”—one could maybe add

an introvert for Freyd was Heterodoxy in social and spiritual beliefs. Freyd's descriptions of an introvert also conformed to the ways the neurotic or emotionally unstable labourer was being construed. An introvert "resists discipline and orders," and is "a radical; wants to change the world instead of adjusting himself to it." Apparently, the introvert was an uncooperative and pathologically maladaptive masculine invert.

Freyd (1924) explained that he collected these traits from many "psychologists of standing" (p. 78), along with graduate students, who answered what they thought were the main traits of the introvert—although he does not detail specifically which psychologists he asked. Freyd's list of traits, like Woodworth's original questionnaire items for neurotics, became a communal well for any psychologists interested in making their own personality inventories for the rest of the decade. Even as psychometric methods became much more sophisticated (or complicated), older test items often stuck; variants of them would often endure, even when historically-culturally inappropriate.

In addition to the impact and longevity of Freyd's list of introverted traits, his article revealed psychologists' difficulties in folding Jung's types into their statistical outlook. Building on Thorndike's work on measuring mental abilities, Freyd echoed the field's doubts about the universality of extreme psychological types. Distributions of abilities, and presumably most traits, "are almost invariably unimodal, arguing for the existence of but one type—mediocrity" (Freyd, 1924, p. 85). Freyd thought that psychological types, or at least the notion of extremes, could be retained as theoretical or rare individuals; most normal persons might tend toward one or the other, or lie squarely in between a trait's opposing poles, such as introvert-extrovert. Though Freyd valued reconciling psychological concepts with "statistical facts," he thought that the initial theoretical matter to settle was the actual "constellation of abilities" associated with the

introvert-extrovert poles: “Since the number of such oppositions is practically limitless, the thing may easily be reduced to absurdity. These matters of theory should be resolved in clear-cut terms and not in the verbiage of the psychoanalyst” (Freyd, 1924, p. 86).

Freyd, like many other psychologists, found the psychoanalysts’ writing and methods convoluted and unhelpful for the scientific study of a normally distributed trait. Despite these deep-seated misgivings, Freyd still wanted to engage with psychoanalytic and psychiatric types ostensibly meant for diagnosing abnormal subjects, converting categories like neurosis and introvert into quantifiable traits of the normal personality. Psychoanalysis was Americanized in many ways, leading to homebred psychoanalyses that lessened their grips on sexuality and moved toward social functioning. Throughout the 1920s, academic psychologists participated in this greater American adaptation project.

(Ab)normal, Social Personalities

Before testing for emotionally unstable workers and neurotic students became fully intertwined with measuring introversion, a handful of disciplinary psychologists attempted to wrangle Jung’s two types themselves. Psychologists working within the mental hygiene movement, or those at least researching and teaching at the crossroads of abnormal psychology and mental hygiene, recognized Jung’s types as important early on. Even before the English translation of *Psychological Types*, notable social psychologist and eugenicist William McDougall talked about Jung’s fundamental types in his widely read *Outlines of Abnormal Psychology*. Shortly after, in his fearmongering *Is America Safe for Democracy* (McDougall, 1921), a book on the state of America’s mental hygiene, McDougall used introverts and extroverts as another level of analysis when considering differences (and defects) among countries and their racial stock. Yet these examples were superficial and brief borrowings of the

latest fashionable Jungian lexicon. Psychologists were also beginning to harness Jung's types in earnest, rendering them legible to a still insecure discipline that valued mental measurements as a matter of course.

As described in Chapter 1, Morton Prince was a central member of the Boston School of psychopathology, especially for disciplinary psychology. Having founded the *Journal of Abnormal Psychology*, a bastion of psy-disciplinary research on pathology, Prince (however reluctantly) helped establish psychoanalysis as a viable and important form of theory and therapy. After the Great War, and with the recommendation of his colleague William McDougall, Prince sought to expand the scope of his journal to now include social perspectives on psychology (Barenbaum, 2000). In 1921, for the sixteenth volume of his journal, Prince brought on board a cooperating editor with expertise in social psychology: Floyd Allport. The journal's new title reflected the content's expansion, as the journal would now be titled *The Journal of Abnormal Psychology and Social Psychology* (*JASP*; later shortened to simply *Journal of Abnormal and Social Psychology*). As editors changed, from Floyd Allport, to Henry T. Moore, and then back to Harvard under Floyd's brother Gordon Allport, the journal's coverage would adjust, eventually minimizing its medical focus and increasing content on the new forms of social and personality psychology. Despite these changes, the journal would long be tied to the diagnosis and treatment of the abnormal individual (Smith, 2013).

JASP and allied journals during disciplinary psychology's transformational interwar years published several research articles on Jung's introvert and extrovert types. As Woodworth's work had generated tools for detecting abnormal and normal levels of neurosis, psychologists saw introverts/extroverts as another psychoanalytic category for the taking. These projects would promote the potential usefulness of measuring an individual's degree of

introvertedness for the general study of human personality to more specific applications, such as seen in the works of Laird and Freyd above. Alongside the creation of multiple introversion measurement tools during the post-WWI psychological testing boom, psychologists were struggling for conceptual clarity when appropriating psychological types generally associated with the abnormal subject and extending it to the normal person.

The Allport Brothers' works during the 1920s into the next decade, within *JASP* and without, were essential for fostering an individualized and normalized social and personality psychology—related fields and viewpoints that were incubating within American psychopathology studies (Barenbaum, 2000; Davidson, 2018; I. A. M. Nicholson, 2000a). In the inaugural year of *JASP*'s upgrade, the siblings published two key articles in orienting disciplinary psychology toward claims of a scientific psychology of personality. Gordon reviewed what he saw as the major threads of personality research, along with his distinctions between terms such as personality, temperament, and character (Allport, 1921). For example, if the study of personality was to take place within a Watsonian view of personality as an individual's responses to personality, character is all that which is “evaluated according to prevailing standards of conduct [the study of which] belongs rather to social ethics” (Allport, 1921, p. 443; c.f., Nicholson, 1998).

In this early review of personality psychology, Allport noted that psychologists, either engaging with or borrowing from psychiatry and psychoanalysis, were hard at work on a “schedule of human traits which will express in the most economical fashion a true analysis of personality” (Allport, 1921, p. 444). On the topic of questionnaires, he saw Woodworth's list of questions as benefitting from being “couched in ‘behavioristic’ language” (Allport, 1921, p. 452). Taking an American view of Jung's typology, Allport saw the introvert and “extravert” as

“two important types of adjustment”, also noting the important work of Columbia’s Wells (Allport, 1921, p. 446).

Another detailed review of personality, this time authored by both Allports and focusing more precisely on classifying and measuring personality traits, appeared in *JASP* that same year (Allport & Allport, 1921). Their initial schedule of personality traits consisted of four major areas: Intelligence; Temperament; Self Expression; and Sociality. In explaining their third category, Self Expression, the brothers seemed to be mixing psychoanalysis with American presumptions of mental health: “loosely related traits indicating the subject’s most general type of ‘ego-expression’ and adjustment” (Allport & Allport, 1921, p. 12). “Extroversion-Introversion” fell within this category of traits, and the brothers saw its lineage as Freudian psychology via McDougall and Jung.

In part, that review likely grew out of Gordon’s dissertation work, where he experimentally examined the related Self Expression traits extroversion-introversion and ascendance-submission. The demands of employers and administrators in an industrial culture, and the practical service psychologists could provide (such as choosing the right worker), likely helped Gordon Allport settle on these two traits for his dissertation (I. A. M. Nicholson, 2002, pp. 87–88). The Allports viewed previous efforts to detect extroversion-introversion as dubious due to ambiguities when observing a subject. For example, was an absence of a reaction a true absence or repression? Nevertheless, they saw the IE distinction as opening up “a remarkable vista for the understanding of humanity” (Allport & Allport, 1921, p. 12).

A bibliometric analysis of the articles and books cited in *JASP* from 1925 to 1942 suggested several active research communities working within the sub-fields of psychology during that era (Davidson, 2018). Many of these communities reflected the boundary work

within interwar disciplinary psychology, particularly the process of claiming or undoing psychiatric and psychoanalytic concepts within a quantitative and behaviouristic framework. The intention of these appropriations, aligning with the Allport brothers' visions of social and personality psychology, respectively, focused on creating a science of the normal, individualized personality. One of these communities centred on introversion studies.

During the 1920s, introversion studies was a blossoming area of research that reflected both the changing interests of psychologists publishing in *JASP* and allied journals, along with the inescapability of psychoanalytic pathologies. Little-known Oregonian psychologist Edmund Conklin (**Figure 2**) was chief among initial academic psychologists grappling with Jung's two main personality types. Born in 1884, he earned his Bachelor's degree at Springfield College in 1908. Shortly after, he earned both his Master's and Doctorate at Clark University during a time when "G. Stanley Hall was the dominant figure there" (Seashore et al., 1943, p. 393). Perhaps due to his alma mater and the influence of Hall, Conklin maintained an interest in what the study of personality types and traits could elucidate for genetic psychology.

After joining the faculty at the University of Oregon in 1911, Conklin wrote and lectured on a wide variety of topics to both academic and lay audiences, including the psychology of religion. Known to some students simply as "Ned," he established the University's Psychological Laboratory, and became the head of their Psychology Department in 1913. At Oregon, Conklin taught a wide variety of psychological courses, including Abnormal Psychology, a History of Psychology Seminar, Methodology, and Principles of Psychoanalysis. A course description, possibly supplied by Conklin, described his Abnormal Psychology course as covering: "Traits and theories of hysterical phenomena, insanity, and the borderland phenomena."³⁸ Alongside his

³⁸ Taken from University of Oregon's 1921-22 Year Schedule and Announcement of Courses (p. 124). Retrieved from <https://scholarsbank.uoregon.edu/xmlui/handle/1794/12749>

colleague Kimball Young, he also taught a course on the emerging topic of Social Psychology.



Figure 2. A photograph of Edmund Conklin. Used with permission from the University of Oregon’s Special Collections and University Archives expressly for this dissertation.

Into the 1920s, Conklin also ran a course on a quintessential topic of that era’s applied psychology: mental hygiene. A course description suggested that undergraduates taking this class would learn “the conditions of efficient mental activity ... the methods and results of the studies of mental fatigue, the hygiene of the emotions and of the intellectual processes.”³⁹ From these descriptions it is clear that, at least in Oregon, abnormal psychology was still associated with the most recently famous specific neurosis, hysteria; while mental hygiene was a topic thought to cover “mental fatigue” (such as Woodworth’s research on psychoneurotic military recruits) and emotional hygiene (such as the adapted applications of Woodworth’s research in the workplace).

From 1923, the year of release for the English translation of Jung’s *Psychological Types*,

³⁹ See Fn. 38.

to 1927, Conklin published three articles on the classification and measurement of introverts, extroverts, and his newly proposed third type: the ambivert (Conklin, 1923, 1924, 1927b). Even though Conklin eventually published on and long taught abnormal psychology at the University of Oregon, he recognized, along with other psychological testers, the potential for extending clinical concepts to capture normal personalities (Conklin, 1927a). In his first piece, Conklin thought psychoanalytic writers, pointing out Freud and Jung, viewed personality through a morbid and pathological lens.

He also thought in these writings that the introvert was the pathological type, implying and prescribing the extrovert—though perhaps this best reflected American psychoanalyst William White’s views on the two types. Regardless of his true influences, Conklin pleaded to his readers’ common sense: Using a mix of literary and everyday characters, he reminded them that normal personalities did not often fit into a single type (Conklin, 1923). As a solution, Conklin began promoting a normal personality category: the ambivert. Though he often slipped into trait-talk as psychologists were doing with other Jung’s psychological types as well as neurosis (e.g., “ambiversion”, “ambiverted”, and “ambivert” being used nearly interchangeably by Conklin and others).

Deliberately using psychoanalysis as a starting point to shift toward studying the normal personality, Conklin viewed his ambivert as a normal and healthy individual—meaning adaptable, flexible, and effective. The influence of American functionalism on Conklin’s writings and theorization is quite evident, working with Darwinian views of evolution and adaptation (Davidson, 2017). Green (2009) argues that functionalism can be viewed as American psychology’s first revolution: its ideas and promoters planting the seeds for both behaviourism and the rise of psychological testing. If so, then Conklin was an exemplary “revolutionary”

whose work on personality testing bridged Hall's genetic psychology with Watson's behaviourism.

Firstly, he aimed to incorporate a developmental perspective, similar to Hall's work on adolescence and development (Conklin, 1924). Secondly, he aimed to use Watson's conditioning to explore why abnormal persons could not adapt to environments and adjust their degree of introversion like a healthy ambivert or ambiverted person, as Watson had previously explored mental disease broadly (Conklin, 1924; Watson, 1916). Near the end of the decade, coinciding with many efforts to develop tests for neurosis and introversion, Conklin unsuccessfully contributed his own personality testing method meant to expand on a central tool of intelligence testing: the ambivert quotient (Conklin, 1927).

Alongside Conklin's failed ambivert quotient technique, many other psychologists were publishing research and developing tools for measuring introversion. For example, Edna Heidbreder's research used Freyd's list of introvert descriptors (Freyd, 1924; Heidbreder, 1926). Heidbreder was a graduate of the University of Wisconsin and then Columbia, making connections with a venerable cast of American psychologists, such as behaviourist and mathematical psychologist Clark Hull, eugenicist and early social psychologist William McDougall, mental tester and experimentalist Edward Thorndike, and the pluralistic Robert Woodworth⁴⁰. Much like Woodworth, Heidbreder is perhaps best remembered for her book about psychology's methods and schools, *Seven Psychologies* (Heidbreder, 1933), in which she included Freudian approaches as one of the main schools within the field.

Before that book's publication, Heidbreder interest in psychometrics was sparked while she was an instructor at the University of Minnesota (Rodkey, 2010). Using a copy of Freyd's

⁴⁰ Heidbreder actually wrote one of Woodworth's obituaries (Heidbreder, 1963).

introvert questions, Heidbreder produced statistics for a stunningly large sample of nine hundred general psychology students—a taxing prospect in an era without computers. Her research project’s guiding question was: How useful were Freyd’s questions for discriminating introverts from extroverts? Heidbreder listed 54 “traits” (as she referred to each separate item) from highest discriminative power to the least (Heidbreder, 1926, p. 129). At the top of these results: “Limits his acquaintances to a select few. (This may be beyond his control.);” at the bottom: “Is conscientious” (Heidbreder, 1926, pp. 130–131). Among her conclusions, Heidbreder viewed Freyd’s list as a statistically justified instrument for distinguishing personality types.⁴¹

Heidbreder soon expanded her introversion research, this time investigating sex differences. Believing that psychologists had failed to demonstrate difference between men and women on intelligence tests, “the question of possible sex differences in *temperament* has taken on additional importance” (Heidbreder, 1927, p. 52). Citing Laird’s research as suggesting a sex difference in introversion and extroversion, with women tending toward introversion, Heidbreder attempted to replicate his work (though using Freyd’s list of descriptions directly). The replication failed: the differences were “so slight as to be practically negligible in determining an individual’s position on an introvert-extrovert scale” (p. 52). Heidbreder noted that the differences that were found seemed less indicative of introversion-extroversion than masculinity-femininity. For example, she posited that “in a country where masculine prowess is so closely

⁴¹ Heidbreder also provided some general warnings, ultimately echoing Freyd’s concerns about the generality of extreme personalities. She argued that as there was no external criterion (e.g., a previously validated and well-established test of introversion), her results said nothing as to whether her sample of undergraduates were actually introverts or extroverts. All the results had done was support Freyd’s questions as being able to differentiate two groups. Though her results also indicated that Freyd’s descriptions constituted “a fairly definite general attitude” (Heidbreder, 1926, p. 134). In other words, even though Heidbreder could not conclude whether a group of students were truly introverts or extroverts, her findings suggested that the introversion trait was within the providence of normal personality. Much like Freyd, Heidbreder concluded that the concepts were not actually distinct types but extreme ends of a single trait-dimension, and that personality questionnaire items do not diagnose so much as indicate the direction of an individual’s personality: toward introvert or extrovert.

associated with business success, it is not surprising that men must impress themselves and their associates as being thrifty ... and not disliking or avoiding any process of selling” (Heidbreder, 1927, p. 58).

More to the point, Heidbreder (1927) argued that the sex differences and introversion-extroversion differences were “independent variables” (pp. 60-1). A single list of trait descriptions—recall, she was using Freyd’s JWT list of introvert tendencies—was able to bring out differences in introverts and extroverts, as well as separate differences in men and women (however minute). These two sets of differences “did not run parallel to each other” (p. 60). Heidbreder was pointing not only to how trait descriptions were often, unknowingly to their creators, interwoven with other psychological traits such as masculinity-femininity. She was also arguing that sex traits and personality traits were distinct in kind. Heidbreder could not say whether sex differences found were “the causes of the effects of the social standards they resemble” (p. 59). While psychoanalytic popularizers like Hinkle were also grappling with these topics in the 1920s, debate about the role of personality alongside gender, would continue into the 1930s—whether in the psychoanalytic work of Karen Horney or the “positive” eugenicist framework of marriage compatibility, which are both covered in the upcoming chapter.

Another “Measuring Introversion and Extroversion” article appeared in *JASP* in the late 1920s, written by Edwin Ray Guthrie from the University of Washington. Guthrie was a Nebraskan who began his career in mathematics and philosophy, though would become a somewhat prominent learning theorist among the so-called neo-behaviourists during the 1930s (Clark, 2005). Apparently known for his brutally parsimonious theories and matching personal disposition, Guthrie’s disdain for both psychoanalysis and fellow psychologists without the adequate rigour is palpable in his paper on introversion and extroversion (Guthrie, 1927).

Guthrie was obviously dissatisfied with the state of research on introversion: “We are told that President Wilson was an introvert and that Lloyd George was an extrovert ... that hysterics are extroverts and neurasthenics are introverts ... [it is] decidedly reminiscent of the accounts of the phrenological faculties” (Guthrie, 1927, p. 83).

The spectre of phrenology, and the wider field of physiognomy, was certainly at the forefront of Guthrie’s mind when considering the usefulness and validity of personality typing in a scientific psychology. All the same, as with many psychologists before and after him, psychoanalysis held a certain attraction as a well of possibility. While Guthrie thought “it is surprising that some of the more prosaic psychologists have not made an effort to establish the objective existence of these types,” in almost the same breath he complained that “this vagueness should be found in the Freudian accounts is not surprising” (Guthrie, 1927, p. 83). Clearly, psychoanalysis was a case of promising ideas held in the wrong hands.

Among Guthrie’s (1927) measurement tools were Laird’s C2 Introversion Schedule and a “Jung Association test” based on a list of one hundred words from *The Measurement of Emotion* (1922) written by British parapsychologist Walter Whately Smith—later renamed Walter Whately Carington. Guthrie found the strongest correlation between the C2 and the association test: a “negligible” .12. He remained uncertain about the uses of Jung’s types for describing normal personalities. Likely mocking the utility of typing, Guthrie argued that the opposing mental habits of an “Esquimau” and a “dementia precox [sic] patient” offered an obvious difference between two types of persons in which Jung’s types might be applicable. He concluded that though “[w]e may all find some personal pleasure in the use of Jung’s types as occasional descriptive epithet [their] application to normal persons should be avoided until we are much more certain of our ground” (Guthrie, 1927, p. 88).

Finally, shortly after the *JASP* publications of Conklin, Heidbreder, and Guthrie on introversion studies, the Clarence Neymann and Kenneth Kohlstedt's Diagnostic Test (NKDT), a popular diagnostic tool for "introversion-extroversion" was introduced in the same venue (Neymann & Kohlstedt, 1929). In keeping with the broad interests in abnormal and personality psychology, the NKDT came from an appropriately interdisciplinary collaboration: Neymann was located at Northwestern University's Department of Psychiatry and Kohlstedt was at that school's Department of Psychology. The duo believed grasping introversion and extroversion was crucial to understanding allied psychiatric ailments. They rejected many other techniques for assessing personality, such as Hermann Rorschach's projective inkblots tests, and viewed their test as a simple alternative.

Yet Neymann and Kohlstedt balked at the scaling gradations gaining popularity in disciplinary psychology, such as the graphic rating scale, in order to connect psychopathological categories to traits of both the normal distribution and the normal personality. They instead opted for a simple binary response scheme (Yes/No) for their diagnostic test—harking back to Woodworth's simplistic psychoneurotic filtering tool meant strictly for identifying potential problem recruits during WWI. Deciding on fifty statements preceded with a general "Do you like or dislike?" question, example items from their list included: "Stay at home during a social affair"; "Often meditate and think about yourself"; "Pay little attention to details"; and "Act on the spur of the moment" (Neymann & Kohlstedt, 1929, pp. 483–484). To support their test, they acquired scores from one hundred manic-depressive patients (the supposed extroverts) and one hundred schizophrenic patients (the supposed introverts) from psychopathic and state hospitals in and around the Chicago area. In their results, they included a graph showing an expected bimodal curved distribution and their obtained frequencies roughly fitting within the curves.

They also indicated their tests on normal subjects support such a bimodal grouping of types, though without providing much in the way of details.

The NKDT, along with the several other introversion measures covered above, would influence further disciplinary efforts toward a quantitative measure of the Big Two as psychological testing expanded in the 1930s. Much like general news, notes, and mentions of neurotics and introverts were appearing in popular press venues, the promise of these simple tests for sorting one type of person from another was being publically touted. An Associated Press piece, picked up in *The Evening Star*, reported on the NKDT even before its debut in *JASP* (Associated Press, 1928). Apparently, members of Northwestern University—Drs. Neymann and Kohlstedt, along with their colleague Dr. John J. Morgan—were promoting their diagnostic tool as a way for employers to determine suitable, meaning “temperamentally and mentally qualified,” job applicants.

The NKDT was being peddled to the reading public and the employers within it in the same way that adaptations of Woodworth’s psychoneurotic inventory had been. In order to demystify the psychological jargon for the reader, the short piece of journalism on the NKDT included a cultural key to decipher Jung’s still quite fresh personality types. Bearing a striking resemblance to how astrological signs are still explained today with exemplary celebrities: “Theodore Roosevelt, Mussolini, William Howard Taft, and Gov. Alfred E. Smith are extroverts ... Col. Charles A. Lindbergh, President Coolidge, President-elect Herbert Hoover and Woodrow Wilson were named as examples of introversion” (Associated Press, 1928). In an era of increasing credibility of celebrity endorsements for products meant to cleanse clothes, smother bodily stench, and sculpt facial appearance, famous and experienced figures of American and world history were unknowingly being put to work exemplifying (and endorsing) the country’s

new personality types.

Chapter 2 Conclusions

At the close of the 1920s, the Big Two had begun to coalesce in both disciplinary and popular culture. Psychopathology, especially varieties of neurosis, was an ever-prominent topic within the academic journals, newspapers, magazines, and likely behind the closed doors of factory operators and office managers. After WWI, the sanitation and organization of citizens' psyches weighed heavy on the minds of Americans. Whether questioning emotional stability, daydreaming, psychosis, or personality types, people worried about the psychological quality of their racial stock. And whether in academic journals or in *Ladies Home Journal*, it was nearly unavoidable to view these topics through a psychoanalytic lens—whether through the works of Freud, Jung, or Americanized adaptations and extensions.

The immediate post-WWI era was also a time of critical maturing for disciplinary psychology. As a consequence of implementing their testing expertise during wartime for filtering the feeble-minded and the constitutionally nervous, and given the industrial demands of increasing production and sales, psychologists were rapidly creating and peddling their wares. Industry needed tools for understanding, organizing, predicting, and controlling behaviour. Factory owners wanted to deter emotionally unstable workers (often conflated with workers sympathetic to organized labour); retailers wanted to boost their sales and advertising agencies wanted to better sway consumers; the army and universities wanted to know how to delimit the normal from the abnormal person, while also yearning for a way to understand an individual's innately best-suited vocation. Psychologists were co-creating the publics they studied, tested, wrote about, and wrote to (Pettit & Young, 2017).

But the psychological market featured stiff competition from the medical field, the

broader schools of psychoanalysis, and eventually profit-seeking outsiders and hucksters. While emerging disciplinary venues, such as *JASP*, were deliberately setting up spaces for nascent subfields like personality psychology to grow, they were also spaces for psychologists (especially testers) to stake a claim in the American psyche. Such boundary work required dual appropriation and differentiation. Friction between psychologists and psychiatrists would begin in their cooperation as emerging psychological experts. All the psychologists explored here had some degree of exposure to medicine's treatment of psychopathology.

Leta Stetter Hollingworth, legally barred from teaching and attempting to eke out a living as a female psychologist, joined and rose in the ranks of New York City's mental testers for differently abled children even before earning her doctorate. Woodworth, a fellow Columbia University psychologist, experienced the mental testing of exotic and "normal" subjects at 1904 World's Fair, and then genuine and potential neurotics at the tail-end of the Great War. Though comparatively young, Woodworth's colleague in reviewing association tests Frederic Lyman Wells had a deep interest in psychoanalysis and served as Assistant in Pathological Psychology at McClean Hospital in Waverley, Massachusetts. Donald Laird and Edmund Conklin both actively incorporated psychoanalysis to expand testing beyond the non-intellectual traits. Finally, one of Max Freyd's many gigs as an entrepreneurial and young graduate was working at the Boston Psychopathic Hospital. Outside the discipline, female scholars, like Beatrice Hinkle, were successfully folding personality psychology into the ongoing discourses about femininity in society. As Shields (1975, 2007) has noted, the statistically normal distribution being applicable to all psychological traits strengthened the variability hypothesis undergirding presumed sex differences in mental ability—explaining the long-line of male geniuses.

In the 1920s and 1930s, the increasing rise of personality testing and typing, joined with

statistical work on multiple intelligences and personality traits, would serve the variability hypothesis' joint notion of the complementarity of the sexes. If sex differences in intelligence could not be pursued, then studying in what ways the personality of the sexes differed would suffice—affording an ostensibly new mode of explaining differences in the sexes' best-suited occupations. The popularity of personality and its newest paradoxically fluid binaries of introvert/extrovert and normal/neurotic would supplement and for some even supplant the male/female binary, as with Hinkle's writings or Heidbreder's research in the 1920s. Though, as with ambiversion or androgyny (Davidson, 2017; Morawski, 1985), psychologies and their publics would often reify rather than synthesize the established dialectic even when offered the middle ground. Given the sustained activity within the introversion studies of the 1920s, along with the various tests that moved within neurosis' wide sphere of influence, a slightly different ilk of testers—intelligence researchers interested in eugenics as well as factor analytic methods—soon joined the sandbox.

Chapter 3

Let's Talk About Sex Personality:

Interwar Psychologies of Neurosis, Introverts, and “Positive” Eugenics

In a 1934 piece of romance fiction for the *Washington D.C. Evening Star* called “Psychology is So Wonderful” readers were introduced to Mary Virginia, a college student in the process of constructing her own introvert-extrovert questionnaire—with the unexpected help of a young red-haired man (see **Figure 3**). At first Mary Virginia is oblivious to the young man’s incessant staring, as she was busy looking inward and swooning over psychology: “It was remarkable. Fifty questions, and she would tell by the way a person answered them whether he was—well, one thing or another. Psychology made you feel so powerful”. After leaving the library, the man follows her, until she finally confronts him, leading quickly to administering her test on him. Mary Virginia needed a genuine introvert for her “haunted-house experiment,” and once she gleans that her stalker-stranger is “100 per cent introvert” she swiftly recruits him.

Shortly after, she runs into her beau, Bill, who having learned about her new recruit was concerned, asking if it was a “[f]ellow or girl?” Indignant, Mary Virginia retorted: “Really, Bill I don’t divide my subjects into males and females, but into extroverts and introverts.” Upset by her interests in psychology, but perhaps more so by her aspirations of filling an empty position at the department in their own university, Bill reminded Mary Virginia that “a girl” had many other options, such as marriage. Once again, Mary Virginia shot back a retort bursting with cultural sentiment: “Who wants to be a wife!” Surprising no one, a jealous and suspicious Bill follows her to the “haunted-house” and ruins her experiment. While walking toward her car, seemingly in the span of seconds, the aspiring female psychologist and the still-nameless red-haired

introvert discover they are in love: “Experimentally she lifted her arms to his shoulders and tilted her head. Experimentally he kissed her. Both of them agreed, scientifically speaking, of course, that the experiment was a success.”

12 THE SUNDAY STAR, WASHINGTON, D. C., NOVEMBER 18, 1934.

Psychology IS SO WONDERFUL

By HARRIET BALL

“A RE you self-conscious,” wrote Mary Virginia in her efficient small script, “when a member of the opposite sex stares at you intently?”

And, lifting her eyes slowly past the young man with reddish hair, she gazed out the window of the college library. “Thank the powers of environment,” she thought, “I’m not an introvert, so it doesn’t make any difference whether he looks at me or not. In fact, I’ve hardly noticed him.”

But at least the young man with reddish hair had supplied her with the last question for her extrovert-introvert test. Mary Virginia returned her eyes to her notebook. It was remarkable. Fifty questions, and she could tell by the way a person answered them whether he was—well, one thing or another. Psychology made you feel so powerful.

She closed her book and opened her vanity. Above its rim her eyes collided with the eyes of the young man at the next table, withdrew, and settled on her own satisfactory reflection. Now, why on earth was he staring like that? she wondered, pushing back a curl of daffodil-colored hair, frowning at her soft mouth.

The mirror offered a clue. However, Mary Virginia merely erased it with a snap of the lid and walked to the library door. It was opened for her from behind.

“Thank you,” she said, and glanced up. There he was. Any one susceptible to the combination of light hair and dark eyes, she decided, might consider him good looking. She herself had always preferred dark-haired men.

SHE looked ahead firmly and descended the steps of the library. He came after her. She cut across the campus behind the biology lab. So did he. She passed the tennis courts, the women’s gym, the psychology building, and still

to a dance tonight at Duncan Lake? Red Hot and his Hotchas.”

“I’m sorry. I can’t. I’ve just found somebody else for my experiment. We’re working on it tonight.”

Bill coughed in an effort to be nonchalant. “Fellow or girl?”

“Really, Bill I don’t divide my subjects into males and females, but into extroverts and introverts. He’s an introvert.”

“He!” snorted Bill, abandoning nonchalance. “Who is this guy anyhow?”

“I don’t know who he is. I just met him a few minutes ago.”

Bill’s face grew red and he began to wave his arms.

“You mean you’re going to take this guy out to the same place you took me? Alone? That deserted old house?”

“Now, Bill,” soothed Mary Virginia, “you know perfectly well that’s the only way I can conduct my experiment. And it’s got to be accurate, it’s got to be as good as I can make it, because

“Yes, I know. Because next year you want to come back here and teach. Well, that’s all off. Ginny. They’ve already got somebody to fill the vacancy. I heard about it yesterday.”

“Oh—but—oh, Bill—”

Bill tried to look sad, for her sake.

“Gosh, I’m sorry. But there’s plenty of other things a girl can do. Like setting married and being a wonderful wife

But so is love--- and a haunted house is no place for scientific experiments

Illustrated By JOE KING

She sat on the ground, leaning against his knee. Bill said, “Gosh, I’m sorry if I scared you, but this guy twisted somp’n in my neck.”

Figure 3. Clipping from *The Sunday Star* (Washington, D.C.), November 18, 1934, p. 12. Shows story “Psychology is So Wonderful” by Harriet Ball. Retrieved from <https://chroniclingamerica.loc.gov/lcn/sn83045462/1934-11-18/ed-1/seq-86/>. The Library of Congress believes that the newspapers in Chronicling America are in the public domain or have no known copyright restrictions.

Harriet Ball's strange story of science and love on and off the campus quad is a forgotten puff piece with a finale promising true love between compatible personalities. Yet the text also suggests how common not only introverts and extroverts were becoming as ways of categorizing people, but also how disciplinary psychology's scientific approach of experiments and questionnaires was becoming recognized as a scientific framework for psychoanalytic concepts. On top of that, barring the romantic ending, Mary Virginia is a stand-in for the New Woman, or the Woman Adrift, of the late 1920s discussed in the previous chapter in relation to Beatrice Hinkle's writings. Echoing Hinkle's faith in Jungian typology as an indispensable tool for feminist projects, the fictitious Mary Virginia thought the introvert and extrovert binary was a much more pertinent and truer way to organize people than "fellow" and "girl."

This local newspaper story of psychological romance, published in the late autumn of 1934, also reflects how psychology and psychoanalysis were restructuring ideas of personality, sex, and gender. During the 1930s in American psychoanalysis, Karen Horney was shifting from a feminine psychology to a more universal psychology of personality anchored not in Jungian types but the even broader concept of neurosis. Concurrently, American psychological testers were frequently shifting or expanding their work from intelligence to emotional and moral qualities of personhood in the form of "non-intellectual" or personality traits. Likewise, here there was shift in the sexual politics among eugenicists researching and utilizing personality psychology, such as Lewis Terman at Stanford University. Introverts and extroverts were especially popular in the emerging reproductive morality of American eugenicists.

Into the 1930s, several prominent American psychological testers began homing in on personality. Many were former schoolteachers, failed engineers, or both. As the network of 1920s psychologists dealing with intelligence, psychopathology, and psychoanalysis—such as

Max Freyd, Donald Laird, or Leta Stetter Hollingworth—had gained experience working with psychiatric populations, many of this batch of testers shared background experiences as engineering students and teachers. Their work was often tethered to neurosis (in its broadest sense), introversion, or both. From the early 1930s onward, psychometric work on personality traits would begin noting the inextricable link between neuroticism/emotional stability and (various forms of) introversion. This “imbrication,” as it would be designated, of the Big Two developed within a more complicated interplay between the Other Big Two: eugenics and psychoanalysis.

While sex differences in personality could be confirmed or discredited with research, the assumed binary sex was a built-in feature of early American personality psychology. And even though psychoanalysts like Horney might have championed neurosis as more important than gender and sexuality per se, or a eugenicist like Paul Popenoe (partially echoing feminist psychiatrist Beatrice Hinkle) might have suggested introversion as an immutable and inborn trait crucial to marriage counseling, writings on sexuality and sex differences did not dissipate but proliferated in modified form within a new arena of research. Just as personality had to be grappled with earlier to better manage and control soldiers, patients, labourers, students, and consumers, personality now had to be addressed to analyze and authorize heteronormative marriages.

The sex binary itself was becoming another personality trait in this process. Binary sex was stabilized into an implicitly binary, gendered trait with an androcentric anchor that valued heterosexual couplings as normal. Even if a psychologist examining personality was not especially interested in or did not find any significant sex differences, excluding the binary altogether seemed unfathomable in light of the established realness-via-measurability of

masculinity(/femininity). When testers continued to expand their jurisdiction to personality traits, psychological sex differences shifted from something one chose to examine—as in earlier intelligence testing of the sexes—toward an immutable, built-in feature of psychological measurement.

This chapter first examines how psychologists continued to hone their measurement tools and incorporate personality traits—specifically, neuroticism and extraversion. The Thurstones and the Guilfords are two expert psychometrician couples whose work on measuring the Big Two will be explored. Though they would build on the 1920s research covered in Chapter 2 (such as Freyd’s, Laird’s, and Woodworth’s personality research), American Midwestern psychometricians in the early 1930s would apply the factor analytic tools previously used when developing measurements of intelligence and ability. Then, Karen Horney’s psychoanalytic work on neurosis is used to see how personality was changing as public concept that emphasized the force of culture on psychological development. Horney’s work is then positioned in the shifting tone of American eugenicist ideology. Eugenicists such as psychological tester Lewis Terman and colleagues were taking personality seriously while building a “positive” eugenics that enforced normative marriage and mothering. The sex binary would begin its stabilization as an incontrovertible fact of personality within such work.

The final section of this chapter takes a wider view of the eugenicist history of the Californian (and West Coast) context within which Terman and colleagues’ science of personality and gender blossomed. Understanding the Californian roots of eugenicist personality testing and marriage counseling is crucial to positioning a key site of progress in establishing the Big Two as fundamental traits within the universal taxonomy of human personality. Eugenicist ideals of gender, coupling, and personality would shape the career and legacy of Terman’s star

student, Robert Bernreuter from Hawaii to Pennsylvania. Early in his career, he would expand his dissertation work to develop his own multi-scale personality inventory—the Bernreuter Personality Inventory (BPI)—which became one of the most popular personality tests around the globe; until the creation of the Minnesota Multiphasic personality Inventory (MMPI) in the 1940s, a development picked up on in the next chapter.

Normal Neuroses

In the 1930s, the Big Two personality categories were continuing to grow in prominence within popular culture as well as disciplinary psychology. Though the previous chapter discussed publics in the plural during 1920s confluence of psychological testers, analysts, and various popularizers of personality, it was rare for contemporary thinkers and testers to actually conceptualize the public as such. Alongside Walter Lippmann’s debate on the validity and ethics of intelligence testing with Lewis Terman, Lippmann was engaged in a debate with John Dewey about the role of the public in a democracy in which rapid technological transformation had rendered society unprecedentedly complicated. For both Lippmann and Dewey, a public was not a particular social community already organized to take care of social issues. Distributed social issue calls members into becoming a public—seeming to suggest a multiplicity of possible publics (Marres, 2005). This aspect, at least, of their ideas was not reflected in the practices of experts’ public-creation during the remaining interwar years.

As Igo (2007) demonstrates, social scientists and opinion poll surveyors were converging on the idea of *the* public: a knowable, singular, statistical, and normal public. Though their methods and samples were often local and particular, even in aggregate. Within academic psychology, the 1930s saw further refinement in the statistical methods of psychometricians like Leon Thurstone and Rensis Likert. Their shared methodological move toward standardized

questionnaires with unique scaling designs on social attitudes was paired with a shift toward often researching only university students. Ironically, the new and efficient form of assessing the public's attitudes toward social issues often excluded much of the wider public, unlike the sampling done for opinion polling (Young, 2017).

Citizens, whether called into an emerging public depending on the issue or deciding how they sized up against what was considered normal, wanted to know about their personality for its implications in many facets of life: from career and marital success to, eventually, vaguer notions of self-fulfillment. Experts armed with psychological knowledge, whether they were eugenicists or psychoanalysts, were eager to inform *the* public about *the* normal personality. In the 1930s, psychological testers were caught at this busy intersection of intellectual influences and political commitments. Neurosis and introversion, by now widely known to publics and experts alike, stood as pillars to organize their versions of personality around. And factor analysis, the British-American tool of the intelligence testing trade, was flourishing alongside simpler forms of personality questionnaire development.

Neurosis Gets Factored In

In the early 1930s, rather than leaving personality only to generalist or applied psychologists interested in abnormal psychology and simplistic mental testing, some psychologists were extending Spearman's development of factor analysis for the study of intelligence to the study of personality. Louis Leon Thurstone is the principle example here. Thurstone, a past APA president (1922-23), was a prolific researcher interested in making psychology a scientific discipline through quantification. One colleague remembered him as the main torchbearer of quantitative psychology after the nineteenth-century German physicist Gustav Fechner (Guilford, 1957). Born to Swedish parents with the family name Thunström in

Chicago in 1887, the young Louis was educated across varied regions: from Illinois, to Mississippi, to Sweden, and finally to Jamestown, New York where he finished his high school education (Thurstone, 1952, p. 296). The rest of his academic career would be similarly varied.

Since high school, Thurstone had a penchant for the sciences, especially physics and geometry. Though he went to Cornell University to study civil, then electrical, engineering, his work as a student in the basement of Rockefeller Hall—such as designing new a motion picture camera and projector to reduce flicker—usually stemmed from his love of both physics and photography. Reflecting on his time at Cornell near the end of his life, Thurstone wondered: “Perhaps I should have majored in physics, instead” (Thurstone, 1952, p. 297). Irrespective of his chosen major, his work on motion picture machinery eventually led to an assistantship in Thomas Edison’s East Orange Laboratory immediately after his graduation in 1912. To his time around Edison, soaking in the habits and behaviours of a famed invention-machine held in human body, he owed the realization that there were disparate forms of fluency in problem-solving—and perhaps intelligence more generally. Less than a decade after Thurstone’s stint at the East Orange lab, Edison himself designed a forty-eight-item questionnaire meant to assess the mental ability of industrial chemist applicants. After hitting many news venues, the Edison Questionnaire’s mass publicity made it a cultural touchstone of testing: raising both public awareness and skepticism about mental testing during the early 1920s (Dennis, 1984).

Soon after, pursuing his side-interests in human learning and machine interaction, Thurstone began his graduate career at University of Chicago under the guidance of James Angell. With an attitude toward other psychologists that he would continue to carry, Thurstone did not trust the methods and lessons of psychology students: “I was sure that engineers had higher standards of intellectual honesty” (Thurstone, 1952, p. 300). Among his graduate

colleagues outside of psychology was Beardsley Rumel, who would become a leading expert on economics and statistics. Among his many achievements, Rumel would go on to direct the Laura Spelman Rockefeller Memorial during the 1920s. The Spelman Memorial was a short-lived fund John D. Rockefeller established in 1918 in memory of his wife; it had the mission of stimulating empirical and socially relevant social science research (Martin Bulmer & Bulmer, 1981; see also Gordon, 2015). As with many social scientists associated with University of Chicago's Social Science Division and allied networks of researchers, Thurstone would soon be a frequent recipient of Rockefeller dollars.

Armed with the new technique of multiple factor analysis (MFA) that he developed in researching intelligence, Thurstone expanded to the personality traits that he had previously seen as largely restricted to the purview of psychoanalysis. Describing personality as one of his “principal interests in psychology,” he devoured as much literature on abnormal psychology and psychoanalysis as he could after obtaining his doctorate (Thurstone, 1952, p. 318). After spending a term of graduate school with Elton May at the Philadelphia General Hospital—an opportunity granted, once again, by U. Chicago chum Beardsley Rumel—Thurstone became convinced that “no one should ever receive a doctorate in psychology without such an experience, no matter what his major field might be” (Thurstone, 1952, p. 319). But after the 1920s, a decade that witnessed a boom of psychological tests for psychopathological categories like neurosis and introversion, Thurstone worked with his wife Thelma using MFA to examine what he had previously deemed as unintelligible aspects of human psychology.

By 1930, with the debut of *The Journal of Social Psychology*, the Thurstones, who were still honing quantitative measures of intelligence, officially widened their gaze to

personality.⁴² In its inaugural issue *Louis*, one of the journal's associate editors, and Thelma, published the modestly titled "A Neurotic Inventory" as its lead article. For the journal's first article, the Thurstones' had a deceptively simple objective: to design a reliable index of neurotic tendencies, in this case among university freshmen (Thurstone & Thurstone, 1930). Their inventory's initial list of 600 items included Woodworth's psychoneurotic inventory, Laird's schedules, and Freyd's introvert/extrovert list. The original items were put on separate index cards, sorted, and then pared down in a subjective process. The initial list had 60 items for introversion and extroversion. The Thurstones found that some of the introversion items, and "a few" of the "extroversion" items, captured some aspects of the neurotic personality. As they remarked, the neurotic personality was quite similar to an introverted personality, "probably what one would expect" (Thurstone & Thurstone, 1930, p. 18).

Though the Thurstones did not explain why they initially included any introvert or extrovert questions, they were likely following suit with the past decade of psychological testing. Previous tests of neurosis and abnormality, like Laird's adaptations of Woodworth, incorporated questions about introversion. Likewise, there was a string of reviews and questionnaires about measuring introversion throughout the 1920s, like Max Freyd's famous list. As Jung's personality types were gaining increased attention in a culture already worried about nervous housewives, anxious children, neurotic soldiers, and emotionally unstable workers, the introvert seemed the next main psychoanalytic and psychopathological subject.

⁴² Originally subtitled "Political, Racial, and Differential Psychology," the new journal was promoting social psychology in the widest sense—mirroring the many social psychologies of cultural anthropologists, neo-Freudians, and more individualistic disciplinary psychologists and psychometricians. This journal was one of the earliest sites to feature a wider disciplinary shifting away from an individual differences approach to the mental differences between races toward the measurement and study of racial relations, social attitudes, eventually the social psychology of prejudice (Samelson, 1978, p. 268) Its editorial board also reflected these varied and evolving perspectives, with ardent culturalists like anthropologists Franz Boas counted alongside ardent eugenicists like psychologist William MacDougall as its associate members.

In addition to the Thurstones, the Guilfords (Joy and Ruth) were another productive husband-wife duo using factor analysis to explore mental ability and personality. Just as with Leon Thurstone, Joy Paul Guilford went to Cornell University to pursue his graduate degree in Psychology—though beginning in 1924, the same year Thurstone became assistant professor at University of Chicago. Born in Nebraska in 1897 to a family of farming heritage, Guilford followed an indirect but not unusual path to graduate studies: teaching in rural schools, pursuing a degree in organic chemistry at the University of Nebraska, and working as an assistant to psychology professors to fund his undergraduate degree—this even leading to taking charge of the university’s psychology clinic in late 1922. Among his many positions at different universities, a brief visiting position at Northwestern University allowed him to attend Thurstone’s evening seminars and even visit Charles Spearman at the University of Chicago to discuss factor analysis (Guilford, 1967, p. 180).

Whereas the Thurstones began with neuroticism and wound up including introversion, the Guilfords would pursue this path in opposite direction. Joy Guilford was already working on applying factor analysis to Jung’s introvert and extrovert types, especially as William MacDougall had received them in the early 1920s: “Common American conceptions were in general agreement that Jung’s types should be regarded as opposite poles of a continuous dimension” (Guilford, 1967, p. 182). Working with his wife Ruth, their investigations of personality—beginning with Jung’s types—would lead to the identification of multiple factors of personality and the eventual creation of two measurement tools: first the Nebraska Personality Inventory in the 1930s, then the Guilford-Zimmerman Temperament Survey in the 1940s (Comrey, 1993, p. 202). For Guilford, factor analysis was the obvious method to investigate whether introversion was a single- or multi-dimensional phenomena.

Guilford's first attempt at wrangling Jung's concept, and the consequential slew of psychologists' tests and lists of questions, was in a *Psychological Bulletin* piece (Guilford & Braly, 1930). This review signaled a few assumptions about a productive personality psychology. First, quantitative methods—meaning psychological tests as a continuation of the Americanized combined Wundtian/Galtonian approach via McKeen Cattell but also the British approach of factor analysis as used by Spearman—were essential. Psychology's quantitative methods had “slowly stripped [extroversion and introversion] of the poetic terminology which has been used to describe them” (p. 96). Second, extroversion and introversion were a “pair of traits of personality” (p. 96). Thus, introverts and extroverts are distinct personality traits without being personality types—though this would quickly slip into conceiving them as opposing poles of a single trait. Third, the theoretical distinction between the two traits (or opposite poles of the single trait) was not easily classified as they seemingly encompassed all aspects of the human personality: intellectual, emotional, and social.

After a few years of research, Guilford returned his attention to introversion in two major papers—one for *JASP* and one for *Psychological Bulletin*. Though now, the joint “introversion-extroversion” terminology, or its abbreviation “IE,” began to appear (Guilford, 1934; Guilford & Guilford, 1934). What were previously separate types and traits were now the single IE trait. The *JASP* article, co-authored with his wife Ruth Guilford, followed up exactly what had been recommended upon previous diagnosis: the factor analysis treatment. Noting that previously proposed measurements of IE, such as Laird's or Neymann-Kohlstedt's, tended to not highly correlate neither with each other nor with the few possibly objective (i.e., biological) criteria for IE, the Guilfords decided it was best to start anew. The couple would apply Spearman's established technique of analyzing item correlations for detecting general and group factors, such

as in the general ‘*g*’ and specific factors of intelligence. They also used Thurstone’s much newer technique, MFA, to better understand the number and extent of any group factors. With the great strides made in conceptual clarity with only one article, the Guilfords believed that, assuming valid measurement tools, “Spearman’s or Thurstone’s or any other accepted technique ... offer a fruitful approach to a very intricate problem” (p. 397).⁴³

Using Thurstone’s MFA, four important factors (of eighteen recovered in total) grouped their items and were given tentative labels of social inwardness or introversion; emotional sensitivity; impulsivity; and self-interest (Guilford & Guilford, 1934). All except the emotional factor correlated highly with the *g* factor thought to represent overall IE, as with the *g* of intelligence. For the Guilfords, the failure for the questions on emotional aspects of personality to represent IE explained some of the previous researchers’ failures in correlative studies. Here it was becoming apparent that questions of emotionality were statistically distinct from other aspects of IE, perhaps deserving of their own factor or trait categorization—like had been done with neuroticism or emotional stability.

From an initial list of 75 items, the Guilfords whittled their test down to 35 dichotomous response (Yes/No) questions. The married psychometricians openly admitted that there was no real criterion for choosing their items other than their own judgement and the suppositions of the introversion researchers from which they borrowed. Moreover, there was no real criterion for supposing any *g* factor found during their analysis represented the IE all these introversion researchers were writing about, other than to try to settle on common judgements of the trait.

⁴³ Personality as a problem to be solved, such as a puzzle, is a sticky metaphor that still attracts psychologists today—and is still thrust upon Psychology sophomores with the bestselling textbook series *The Personality Puzzle*, currently in its eighth edition (Funder, 2019). I also recommend Hacking’s (2002) chapter “Five Parables” (esp. pp. 35-39), where he briefly historicizes the framing of philosophy as problem-solving. (He “blames” early twentieth-century English-speaking scholars, including William James.) Also relevant to science-as-solving is *Working Knowledge*, especially its sections on Thomas Kuhn’s time at Harvard (Isaac, 2012).

When it came to using clinical patients as some form of criterion, such as people with dementia praecox as extreme forms of the IE trait, the Guilfords rejected the notion more anecdotally:

We also know, from personal experience ... how little dependence can be put upon their [psychopathic patients] answers, even in the most seemingly normal cases ... But what is an even more serious matter, the cultural level of the average patient in a public hospital is so remote from that of the college student, for whom the typical test is devised, that he cannot interpret all questions in the same way ... additional spontaneous remarks or response [to a Yes/No question] reveal that the subject may have entirely missed the point. (Guilford & Guilford, 1934, p. 380)

The reasons the Guilfords rejected using abnormal participants in their factor analytic research on personality traits are layered. At the more abstract level, it made little sense to compare their findings from a normal sample to the extreme scores of an abnormal participant on the same IE scale. Rather than validating their tool as a measure of IE (whatever it might be, in terms of statistical factor structure) for any person, it would help design a tool to distinguish the normal from the abnormal. Though this may be useful for psychiatrists or clinically oriented psychologists, it did not reflect the Guilfords' project of extending psychometric works on intelligence into the rest of human personality.

The Guilfords much preferred testing (presumed to be normal) college undergraduates. They administered their list of 35 questions they viewed as best representing the busy area of introversion studies on 930 students, though with an additional 36th question: "Are you male?" (Guilford & Guilford, 1934, p. 382). Assuming there would be sex differences among their responders—alluding to, though not citing, research where men were more extraverted and women more introverted—they included this as a simple way to explore and suppress those

effects. The rest of the questions are a wonderful snapshot of how American culture had come to understand introverts, extroverts, and their overlap with neurotics. Questions of note include: “Do you express yourself better in speech than in writing?”; “Do you generally prefer to take the lead in group activities?”; “Are you inclined to worry over possible misfortunes?”; “Do you like to speak in public?”; “Do you adapt yourself easily to new conditions?”; and “Do you like to sell things?” (p. 382). The introvert was a withdrawn worrier and thinker, whereas the extrovert was a well-adjusted leader and salesman.

Within the more arcane technical passages of their article is a wonderful microcosm of the type-to-trait boundary work. The Guilfords viewed their dichotomous (Yes/No) responses as unrealistic as it concurred with the bimodal introvert/extrovert typology psychometric psychologists repudiated. In other words, it ran against the statistical normativity at work in all traits of human psychology. Providing a series of formulae to explain their use of a corrected version of tetrachoric correlation to help work around this false dichotomy, the Guilfords assured their readers they assumed “some kind of continuous distribution” for the items and that the current response were “merely forced into two categories by the conditions of the test” (p. 384). Furthermore, the trait distributions were likely normal, as “most human traits are” (p. 385). Of course, all these considerations did not extend to pesky Item 36 on identifying a participant’s sex, which the authors thought was “obviously more like a point designation” (p. 385). Although statistical normativity implied all items, if considered human traits, should be distributed as such, this logic could not extend to the entrenched binary of male and female.

In another *Psychological Bulletin* review of IE shortly after this factor analytic work, Joy Paul Guilford noted the many characteristics likely connected to the trait, including “neurotic tendencies.” Pointing at the introverted end of the IE trait spectrum, he averred there was “almost

a universal suspicion that the *I* is inclined to maladjustment if not to more serious instability” (Guilford, 1934, p. 343). The Guilfords revisited their 1934 data two years afterward, in light of Thurstone’s more refined computational methods for MFA (Guilford & Guilford, 1936). Their original four common factors (social introversion/inwardness; emotional sensitivity; impulsiveness; and self-interest) were now three, simply titled: S; E; and M.⁴⁴ They gave the factors simple letters for names to dissuade too much unwarranted interpretation about what they actually represented; although each letter certainly did correspond to their initial idea about what each factor meant. S was about social introversion or inwardness; E was about emotionality or emotional sensitivity; and M was about a masculine ideal.

Although the Guilfords considered labelling their E factor as “emotional introversion-extroversion,” they felt the factor loadings did not support a bipolar trait interpretation (Guilford & Guilford, 1936, p. 121). On the other hand, they did see “a thread of emotional immaturity or emotional dependency” running through the items loading onto E—bringing it closer to measures of neuroticism and emotional stability. Despite the fact that in previous IE studies “the majority of investigators find males slightly more [extraverted] than females, but many find no sex difference” (Guilford, 1934, p. 341), gender—or at least an idealized form of masculinity—was the Guilfords’ third factor M.

Methodologically, the Guilfords’ work represented the future direction for much of personality psychology. They held a steadfast faith in the organizational virtues of applying the latest quantitative techniques to a disjointed science of personality. For them, personality was a

⁴⁴ Two other factors that they recovered but did not retain for their measurement development was R (“rhythymia”; care-freeness) and T (“thinking introversion”; thoughtfulness). It is difficult not to force a connection to the currently standard personality domain/dimension of Openness to Experience and Conscientiousness (Guilford & Guilford, 1936, pp. 121–122). Also, sorting out the varieties of introversion (and allied traits like surgency, sociability, etc.) would long occupy trait psychologists for decades to come. The Guilfords and many others would continue their attempts to classify these factors and their relationships with other major traits (e.g., Guilford & Guilford, 1939).

hidden structure lying within others—a structure that would only capitulate to the correct, statistical machinery. Their instrumental faith rang true for psychologists who were also studying personality but not within a factor analytic tradition. For the earlier generation of American intelligence testers like Lewis Terman, along with his colleagues and students, simpler testing techniques would (initially) suffice. Binary gender as a component of personality would also persist. Before examining that cluster of research, the next section will explore how personality—neurosis, in particular—was becoming popularized and transformed outside of disciplinary psychology and within the contentious terrain of psychoanalysis.

Horney's Neurotic Era

The second generation of psychoanalysts, or “neo-Freudians,” were often analysts who eventually emigrated from Europe to the United States whose work placed a much greater emphasis on the influence of culture on personality and impacted Western culture, mental health professionals, and social scientists through therapeutic innovation and new literary markets (N. G. Hale, 1995; McLaughlin, 1998). As before, these developments in psychoanalysis were not isolated from other disciplines and professions. While American social science sites of psychoanalytic importation, such as the “historical expedition” to G. Stanley Hall’s Clark University in 1909 (Rosenzweig, 1992), are certainly important, lesser known paths such as the Berlin via the University of Chicago after Franz Alexander’s appointment to Chicago’s medical school in 1930 are important to understanding Horney’s position in American thought (Gitre, 2010). As Alexander found more institutional support and camaraderie at the university’s Social Sciences Division (SSD) than its medical school, he was able to mount an interdisciplinary seminar that included participants like the psychometrician Thurstone and the less easily disciplined social scientist John Dollard (Gitre, 2010, p. 240).

Given the professional and private connections between those in the University of Chicago network and those working in conjunction with major patrons like the Social Science Research Council (SSRC) and the Spelman Fund, funding was provided for psychoanalytic opportunities and exposure as part of social science professionalization. Such an interest in psychoanalysis was not exclusive to idiosyncratic or maligned social scientists. Additionally, SSRC and the American Psychiatry Association, with money also from the Spelman Fund, began a Colloquium series on Personality Investigation in 1930, eventually attracting psychologists like the Allport brothers and Thurstone, and anthropologists like Margaret Mead and Ruth Benedict (Gitre, 2010, pp 257-8).

Such funding opportunities and interdisciplinary events allowed for a broader exposure of the newer “psychoculturalism” of neo-Freudians. While there were already varieties of culturalism in American social science before the migration of neo-Freudians like Karen Horney and Erich Fromm, the new work helped social scientists move beyond Freudian biological-focus on drives and instincts (Gitre, 2011). Horney’s output deeply affected understandings of neurosis, normalcy, and gender.

Her experiences as a female medical student in early twentieth-century Freiburg, Germany, who pursued her interests in neurology and psychoanalysis while fulfilling the roles of mother and housewife at home, influenced her views on illness and therapy (Rubins, 1978). In 1910, during her final year of medical school, Horney attended Herman Oppenheim’s neuropsychiatric clinic in Berlin and also began undergoing her own psychoanalysis under Karl Abraham (Rubins, 1978, p. 38). Dissatisfaction with the ideas and practices of both these men foreshadowed central aspects of her future vision for psychoanalysis. In the case of her sessions with Abraham, she ceased his aid in 1912; though she would not end analysis, relying on her

proclivity for diary-keeping⁴⁵ and “self-analysis” (Paris, 1996, p. 4). Horney’s final graduate thesis politely challenged Oppenheim’s emphasis on the organic causes of neurosis, such as trauma neurosis and later shell shock. Suggesting the possibility of non-internal sources of psychopathology so early on may have also suggested Horney’s eventual defection from Freudian orthodoxy (Rubins, 1978, pp. 46–47).

In the latter-half of the 1930s, after settling in the USA, Horney turned her attention toward a more general psychoanalysis; one incorporating the goal of human growth. Her two major books of that period, *The Neurotic Personality of Our Time* (1937)—hereafter NPOT—and *New Ways in Psychoanalysis* (1939), cemented her status among the most successful European expatriate psychoanalysts. Horney’s recent experiences with American life, quite different from her European milieu, likely informed her shift toward a form of psychoanalysis that appreciated the indelible force of culture on personality. Yet even before her move, she was starting to cite ethnographic and anthropological studies in her work and became friends with George Simmel. While in Chicago, where she and her daughter initially moved in 1932, she became associate director of the Chicago Psychoanalytic Association—apparently providing her the chance to meet the likes Howard Lasswell, Edward Sapir, and John Dollard. Finally, once relocated in New York City, Horney became friendly with anthropologists Margaret Mead and Ruth Benedict (Paris, 1994, p. 99).

After settling into New York City, Horney and her colleague (and romantic partner) Fromm became friends with psychoanalyst Harry Stack Sullivan. Horney knew Fromm during their past lives in Europe. As early as 1928, while still living in Germany, she invited him to speak at a Berlin Institute meeting, and they continued to influence each other’s work (Rubins,

⁴⁵ Though Horneyan self-analysis, as she later developed it, was much more formalized pursuit of self-knowledge than simply diary-keeping (Díez Manrique, 1984).

1978, pp. 121-2). Their friendship with Sullivan led to Horney and Fromm becoming fixtures in Sullivan's Zodiac Club, an NYC academic salon first held in a speakeasy during the prohibition era. The club's participants would eventually include prominent nodes of the social science and psychoanalysis network. (Gitre, 2011, p. 23). Within these locales and now with strong ties to American social science, Horney's work was shifting toward a reimagining of neurosis and personality more generally. NPOT in fact grew out of a 1935 course Horney taught at NYC's New School for Social Research titled "Culture and Neurosis" (Rubins, 1978, p. 100). A year before the book's publication, she seemed to have tried out her ideas in an article in the first volume of *American Sociological Review*, where she considered "uncovering ... the neurotic character structure" as a prerequisite to curing neurosis might demand some "modifications in Freud's views of the relation between culture and neurosis" (Horney, 1936, p. 221).

Horney's insistence with her publisher about her upcoming book's title, *The Neurotic Personality of Our Time*, indicated the importance of place and time for her new approach to neurosis (Paris, 1994, p. 100). It was not a simple observation of neurosis occurring during a certain time within a specific locale; it was a radical reframing of neurosis as an intransigent feature built from, and unique to, her current time and place. Horney held that neurosis could not be defined against any universal human nature as such a thing was intractable; meaning there was no typical and universal form of neurosis (Rubins, 1978, p. 225). By focusing on pathogenic conditions within an anxiety-inducing society causing complex defense mechanisms, Horney paradoxically reduced neurosis from a true universal while making it possible for anyone struggling with common anxieties. Neurosis was pluralistic, local, but pervasive; at the very least, the potential for neurosis was possible across all worlds. Though anxieties and defenses were to Horney normal within many cultures, genuine neurosis was a more extreme and rigid

form of reactions and unconscious emotional conflicts rooted in the pressures of cultural standards.

Horney's network of social scientists committed to psychocultural (and, generally, psychodynamic) explanations were approving of her repositioning of neurosis (Taylor, 2009). Ruth Benedict saw Horney's "non-biological and non-instinctive" update of psychoanalysis representing the "cooperation of students of social problems and of individual psychology" (Benedict, 1938, p. 135). Horney's apparent transgressions left Freud's acolyte Ernest Jones unimpressed. Not respecting the literary market neo-Freudians were writing for—"[t]his volume appears to be addressed to the public"—he was annoyed at how she "constantly deprecates" Freud's emphasis on biological factors (Jones, 1940, p. 240). He warned his brethren of Horney's European and American bias with a simple closing caveat to his book review: "We note that the name of no English analyst is among the numerous analytical writings referred to" (Jones, 1940, p. 241). John Dollard—an eclectic social scientist with experience in sociology, anthropology, psychology, and psychoanalysis—saw NPOT's deviations from traditional psychoanalysis as still interwoven with a deeply committed following of Freud: "The appreciation of Freud's work is so genuine that the theoretical deviations are refreshing rather than annoying" (Dollard, 1938, p. 279).⁴⁶

⁴⁶ Esoteric and dense writing was a common complaint among no-nonsense psychologists wanting to harness psychoanalysis into topics legible to their disciplinary norms. Dollard was most grateful for Horney's "rare gift for making complex matter simple" while resisting the trend of doing the opposite. In a jab at the most arcane of analysts, Dollard noted that the book's "intelligibility may be alienating to the many who delight in esoteric knowledge, but a comfort to the plain-spoken who want to get results" (Dollard, 1938, p. 280). The desire for results is unsurprising, as Yale's Institute for Human relations IHR itself was a Rockefeller Foundation-funded effort at a scientific enterprise that would "provide the basis for a rational management of human affairs" (Capshe, 1999, p. 20). Though the IHR would largely fail at achieving the kind of scientific integration, it did help build towards a formal, mathematical learning theory (Morawski, 1986, p. 220)—owing much to the work of its resident neo-behaviourist, Clark Hull. His work on hypnosis and suggestibility, along with Dollard's upcoming *Frustration and Aggression* (Dollard et al., 1939), were part of a larger network of behaviourists working toward appropriating and operationalizing psychoanalysis (Davidson, 2018, pp. 161–162; Hornstein, 1992). Nevertheless, Dollard was disappointed at both Horney's lack of engagement with the role of ethnography, as well as her apparently unclear position on Freud's drive theory—the latter a disappointment to "the social scientist who has profited much" from

In the popular press, NPOT was given a positive though misinformed review for the *New York Times*. The reviewer was adamant that the “clearly and sanely written” book would not only interest the “lay reader” but that the “psychiatric profession will profit by many of Dr. Horney’s views” (Welch, 1937, p. 85). Unfortunately, the book reviewer seemed quite convinced that Horney was a male psychoanalyst, repeatedly talking about “his” views on neurosis and “his” approach to psychoanalysis. Accepting Horney’s many controversial positions, such as current cultural standards fostering neurotic behaviour, the reviewer also captured something central to Horney’s psychoanalysis: normality.

As the reviewer understood Horney’s thesis, “[t]he distinctions between the normal and the neurotic personality he [*sic*] describes as largely quantitative” (Welch, 1937, p. 85). As Canguilhem (1966) would famously later observe, alongside the rising positivism and demand to control nature in nineteenth century, there was a historical shift away from categorizing pathology and health as heterogeneous kinds toward assuming a continuity between pathological and normal life. This implied pathology as an extension of normal processes, allowing the tracing from pathology back to health along the same continuum.⁴⁷

Using “quantitative” in describing Horney’s extension of pathology into the defining of normality, thereby undoing discrete typologies into something continuous, was quite appropriate considering the contemporary work of psychological testers. They carried a project of normality like Horney’s, though their emphasis on quantitative tools was entangled with the meaning of normality in the statistical sense. Her conception of the normal and the pathological kept her ideas partially congruent with both eugenicists and psychological testers. Though Horney’s focus

notion of drives (Dollard, 1938, p. 283).

⁴⁷ Though there was always room for contradictions, or discontinuity within the norm of continuity, such as in the work of experimental physiologist Claude Bernard who presumed qualitative differences between normal and pathological mechanisms and products of vital functions (Canguilhem, 1966, p. 71).

on the power of culture in determining one's personality—normal or otherwise—was much less consistent with the eugenics and testing.

Horney was very interested in how her still new social setting, the USA, induced neurosis. As she saw it, “[m]odern culture is economically based on the principle of individual competition” (Horney, 1937, p. 284). A competitive culture isolated individuals and engendered hostility toward all others, as they were potential competitors. Though this hostility was most obvious among individuals competing within the same occupation, Horney saw competitive hostility permeating in all aspects of life. Potent hostility was found in friendships (between same- or opposite-sexes), within sexual relationships, within school life, and certainly within the home. As Horney put it, “perhaps most important of all, it pervades the family situation, so that as a rule, the child is inoculated with this germ from the very beginning” (Horney, 1937, p. 285). Horney was soon formally ostracized from orthodox institutions for her irreverent form of psychoanalysis: in 1941 the New York Psychoanalytic Institute disqualified her from a training analyst and instructor without providing an official reason (Natterson, 1966, p. 455). Despite this, she would continue her success and prominence outside the old guard of New York psychoanalysis—especially among American audiences of her books, and magazine and newspaper articles.

Psychoanalytic ideas more generally have always been influencing both culturalist and positivist versions of American social science since the field's importation. Although psychoanalysis' impact is most often associated with the “cosmopolitan-liberalism wing of the social sciences,” its infusion in funding and professionalization also guided a modernist social science that emphasized social control: “psychoanalysis cut straight across both these visions, fueling both” (Gitre, 2010, p. 241). Even as neo-Freudianism was diverging from an orthodox

focus on biologism and, in some ways, sexuality, psychological testers were using psychoanalytic categories and sometimes theory to push ahead a hereditarian science of personality that required normative gender roles.

(Gendered) Personalities

Upon first glance, one might surmise from the trends in the psychologies of personality during the 1930s that sexuality and sex differences were fading into the past. A new generation of psychoanalysts, and their wide network of allied intellectuals, were shifting the analytical gaze away from innate drives and libido theory toward external circumstances in explaining all aspects of human personality. Even Freud's late work, as Herzog (2017) suggests, was moving away from sexuality and toward the interplay of individual and society, such as in *Civilization and its Discontents* (Freud, 1930). Meanwhile, intelligence testers were continuing their move and expansion to personality as the measurable aspect of humanity that apparently trumped all other psychological facets in terms of importance, including sexuality.

But the work of psychologists and neo-Freudians are not actually reducible to such a story. Through the research projects of Karen Horney, as well as the synchronous research of psychologist Lewis Terman, we can find a more complicated history. While Horney's work may have reframed psychoanalysis in terms of femininity instead of masculinity, eventually leaving both behind, she did not eschew sexuality. In contrast, while Terman and others seemed to be homing in on the complexities of sexuality instead of simple sex differences—especially its “inversions” and “inadequacies”—his work actually helped concretize the sex binary into gendered personality traits. How these projects converged and diverged will be explored.

The central argument here is necessarily slippery, as it depends on fixing terms that cannot—and should not—be fixed. In this section, as with the entirety of this dissertation,

language has attempted to agree with historical usage. Thus, sex is used to connote the male/female, while sex roles allude more to the particular masculinity/femininity binaries mobilized in psychologists' work. "Gender," though the word was in use, did not then carry the meanings and analytical weight as it does today.

As a discipline and science, psychology has long had a complicated relationship with gender: its practices are gendered while it theorizes gender while it produces empirical work on gender (Rutherford, 2020, p. 22). While key theoretical work has since established norms when using "sex" and "gender" in psychological research (esp. Unger, 1979)⁴⁸—sex implying something innate and gender implying something socially constructed—this section does not conform to such usage. In the ensuing years, prescribed and proscribed meanings of "gender" in academic circles and in policy did not become a settled matter, and usage of the term "gender" in academic articles has again trended toward incorporating biological meanings (Butler, 2004; Haig, 2004). When writing of gender or gendered personality here, it is simply to denote the transition from one binary to another—e.g., man to masculine. Ontological assumptions of those terms vary across historical actor and place.

This work also relies and expands on Hegarty (2007, 2013), who quite crucially problematizes the normalizations at play in psychology. While there is a coupling of statistical and moral norms, this can vary with context. A more traditional and conservative Queteletian normalization presumes normality resides around the average. In Galtonian normalization, particularly when considering mental ability, focus tends toward the positively extreme—or intellectually exceptional—allowing an "escape latch" of welcome atypicality (Hegarty, 2013, p. 16). Even though this may seem consistent with how Canguilhem (1966) viewed normativity—

⁴⁸ Although Unger's article is most often cited to support this distinction in terms, the article's argument against sex differences research (the aspect she felt more important in the article) is largely neglected (Rutherford, 2007).

that is, diversity and abnormality indicating health as it could transgress and create new norms—such a dynamic did not usually hold for psychologists writing at the intersections of personality and gender. Abnormality at those crossroads were necessarily pathological.

On the Sexless Duties of Motherhood

Before Horney fully explored the external or cultural forces behind neurosis, a series of essays during the 1920s into the early 1930s on feminine psychology constituted the first phase of her writing career (Paris, 1996). Even before that, in her diaries around 1911, Horney was trying to apply Adler's notion of inferiority and masculine protest to her own life and dreams. In pursuing the idea of many behaviours rooted in feelings of inferiority about being female, Horney recalled many instances in her childhood: from her wishes to pee freely outdoors as boys did, to cutting her hair short, to making up for physical inferiority with academic excellence. She much later in life had a dream where her husband had a vagina, which she interpreted as revealing her own latent homosexual tendencies—yet another form of seeking masculinity. In terms of her time and place's gender norms, Horney's waking life was also expressed in what contemporary analysts and psychologists would see as a masculine fashion: Pursuing medical science and indulging in a relatively sexually promiscuous life (Paris, 1996, p. 62).

Horney's turn toward a feminine psychology was targeted at exactly that kind of conception of female misbehavior as a form of masculine protest—itsself a manifestation of the presumed female castration complex (“penis envy”). In the early 1920s, Horney responded to her former analyst Karl Abraham and his take on female psychology. Horney's (1924) published reply on the genesis of female castration complex was, in more modern feminist eyes, still conventional and deferential on the feminine essentialism and norms of motherhood. Nevertheless, her work did hint toward a female sexuality that incorporated pleasure (Garrison,

1981, p. 677)—a focus that would soon feature prominently in American marriage guides. A few years later, Horney's resistance to orthodox views on femininity amplified in her since celebrated "Flight from Womanhood" (1926). Upending many Freudian principles, she reconfigured female inferiority as dogma stemming from men's unconscious fears of women—such as their (re-)absorption into the vagina. Contra Freud, hers was a psychology that foregrounded women's sexuality—clitoral stimulation, young girls' self-discovery, young boys' womb envy—and spoke of women's social disadvantages as construing womanhood as abject (Garrison, 1981).

But her work did not completely transgress the presumed innateness of sex. She argued that a mother's attitudes toward her children stemmed from the mother's own experiences with her parents and how she was taught male and female roles (Horney, 1933). One possible route for neurosis could be a woman with strong ties to her mother develops reasons early in life to be dissatisfied with the female world: "As a result of all this she emotionally turns away from her innate sexual role and develops masculine tendencies and fantasies" (Horney, 1933, p. 459). Resentment of the female role, through teaching children qualities like "men are brutes and women are suffering creatures," could lead to a mother who was intolerant of any sexuality within their own children (Horney, 1933, p. 460). The disdain of womanhood could produce "masculine" women—domineering mothers—who only strengthened the artificial cycle of self-hatred.

As American eugenicists were moving toward an updated morality project that featured appropriate marriages and copulation, and psychologists were similarly shifting their focus from racial and sex differences in intelligence to studies of prejudice and personality (the latter including gender-as-trait), Horney was leaving such topics behind her. Coinciding with her arrival in America in 1932—after passing an immigration exam in New York City—Horney

eschewed her initial phase of work on feminine psychology. In July 1935, the same year she was teaching a course at the New School on neurosis and culture and preparing NPOT, Horney gave a talk at the National Federation of Professional and Business Women's Clubs in New York City. She outlined only her misgivings of her feminine psychology. She also explained the ramifications for any science of sex differences. Titling her speech "Woman's Fear of Action," Horney's central concern was the illusory naturalness of "woman" and "femininity"—both sex and sex role. She argued that while women do tend toward positions and self-understandings of subservience, nurture, and emotionality, these qualities of womanhood only seem natural because cultural and economic factors have so long enforced the psyche of women into such characteristics (Horney, 1935, as reprinted in Paris, 1996).

She also posited that in many cases the maligning of women to "natural" roles of mother and housekeeper is to exclude women as competition for employment—with such sentiments about the sanctity of woman's nature increasing during times of economic crisis, such as the then recent Great Depression. Whether rooted in Catholicism or Freudianism, or enacted in democratic or fascist societies, attitudes about women's nature rendering them subservient and dependent are shaped to serve the economic needs of the patriarchy. She warned the audience that "[a]ny sudden increase in interest over sex differences, therefore, must be regarded as a danger signal for women" (Horney, 1935, as reprinted in Paris, 1996, p. 232).

In closing her speech, Horney offered some thoughts that not only negated the legitimacy of sex differences research, but also closed the door on her feminine psychology in the hopes of opening a universal psychology focusing on growth. Chastising researchers and writers obsessed with the nature of gender, perhaps even herself, she pleaded "we should stop bothering about what is feminine and what is not. Such concerns only undermine our energies. Standards of

masculinity and femininity are artificial standards” (Horney, 1935, as reprinted in Paris, 1996, p. 238). From Horney’s view, the sex differences that do exist between men and women were illegible. They were shrouded in the artificial distinctions propped up by cultural norms and needs. Wanting to improve the lives of women through increased self-confidence and opportunity, she called for recalibrating efforts away from theorizing femininity or masculinity and toward reaching the potentials of human personality. Perhaps not wanting to deny all hope for eventual scientific inquiry into fundamental sex roles, she added that “[p]aradoxical as it may sound, we shall find out about these differences only if we forget about them” (Horney, 1935, as reprinted in Paris, 1994, p. 238).

While Horney’s legacy might be of challenging contemporary notions of gender, it would be a mistake to construe her contributions as moving away from sexuality (Herzog, 2017, p. 53). That artificial departure is used as another layer of her break-from-Freudian-orthodoxy narrative in the historiography. Although Horney’s work certainly addresses femininity, it was accomplished through a continued focus on sexuality—on female sexuality instead of exclusively male—and its relation to topics from marriage to neurosis to self-hatred to self-realization. Flipping the causal arrow, now flowing outward to inward, did not eliminate sexuality. Horney seemed to have wanted to leave the indeterminable natures of masculinity and femininity behind without entirely abandoning sex.

Even though Horney’s work, along with neo-Freudianism more generally, deeply influenced American psychoanalysis, social science, and intellectual thought, it was not indicative of future directions. Horney, and many psychoanalysts deemed too political or sexual, were worked out of mainstream psychoanalysis like stubborn stains. Met with powerful critics like church leaders, psychoanalysis and psychiatry adjusted itself into sexually conservative

revisions of earlier ideas and newer sects like ego psychology (Herzog, 2017; McLaughlin, 1998). Into the postwar era, popular forms of psychoanalysis would tamp down and prune sexuality meanwhile enforcing normative gender roles. These trends would make for an androcentric and homophobic culture of psychoanalysis demanding feminist and gay rights resistance, peaking in the early 1970s (Bayer, 1987).⁴⁹ While American psychoanalysis would become more inclined to stave off sexual experiences and enshrine gendered selves in the postwar years, personality psychology was headed toward such a situation in the middle of the interwar years.

As the 1930s went on, the strict hereditary focus of the recently enacted sterilization policies initiated in various North American institutions began to lose favour among social scientists. Eugenicists' interests were also expanding from mental hygiene to motherhood. As witnessed at California's Sonoma State Hospital, the culture's "reproductive morality" had already shifted in the 1920s from segregation of the "moronic" to the sterilization of mostly women. More important than their actual intelligence scores, women chosen for sterilization were often institutionalized for allegedly being sexual promiscuous. Though some historians would argue that criticisms of the sterilization defense against the feeble-minded from natural scientists spelled the end for eugenics practices in the USA—with an admitted cultural lag in actually stopping sterilizations—Kline (2001) makes the case that eugenicists simply revamped

⁴⁹ The deliberately depoliticized ego psychoanalysis of the 1940s gave way to a postwar culture of homophobia where there was a converging consensus among analysts in media and books that homosexuality was pathological. American psychoanalysis becoming suddenly vocal on a sexual topic was perhaps a reaction to non-analysts claiming expertise on sex, resulting in events like a debate between Karl Menninger and Alfred Kinsey at the American Psychiatric Association in 1954 (Herzog, 2017, p. 62-4). With increasing protests against the pathologization of homosexuality, coming even from within the profession, psychiatry acquiesced. The removal of homosexuality in late 1973 from the DSM-II, after years of protest even at American Psychiatric Association conferences, signalled the difficulty of defining or removing psychopathy from psychiatry's (then incidental) manual of diseases. Paired with the anti-psychiatry movement that asserted cultural norms created categories of mental pathology, the legitimacy of psychiatry seemed at stake.

their strategy. In the hopes of restoring a moral order that interwar changes in gender, race, and class hierarchies had threatened—while also conceding the role of environment in a person’s development—eugenicists homed in on mothering and marriage. This was a new version of eugenics that separated the desires and pleasures of sexuality from the democratic duties of motherhood.

Proper mothering, and its requisite forms of social conduct, became key to repackaging eugenics platforms. Eugenicists were adept at separating motherhood from sexuality, with the former being a democratic duty associated with the fit mother and the latter merely desire associated with the unfit and promiscuous (sometimes feminist) women (Kline, 2001, p. 61). Biological fears over the mentally and morally deviant tainting of the racial stock were still associated with demographic fears of “race suicide.” Eugenicists had long warned others of plummeting birthrates among white, middle class—implying those of inferior races and class would be soon filling the gap. Whether immigrants, sexual deviants, the feeble-minded, or the promiscuous, threats to a normal society were numerous and pervasive. Such concerns over population trends served eugenicists shifting outward focus from sterilization toward messages of proper reproduction.

Eugenics became a way of defending not only traditional racial and class hierarchies, but the norms of gender and sexuality as well. Emphasizing proper pairing and rearing among the superior strata of society could be just as useful to protecting the racial stock as sterilization. To be sure, this was not a period of decline in sterilization rates. The infamous *Buck vs. Bell* case, eventually argued before the US State Supreme Court in 1927, had only recently occurred—a decision that upheld sterilization “on eugenic grounds” as legal power of the state (Kevles, 1995, p. 111). As will be touched on in the next section, in the late 1920s California was

a leading state in the cause and administration of sterilizations. Yet sterilization was not an exclusively eugenicist tool in the sense of racial purification. Across America sterilization was approached differently, and often in tandem with evolving and local issues of social welfare. Sterilization victims often included non-racialized (i.e., White) poor people. On top of this, sometimes poor women pursued sterilization as an obtainable form of birth control, especially during the Depression era (see Ladd-Taylor, 2017). For racially-motivated eugenicists, championing the literally seminal role of reproduction in building a better race provided a way of conceding a role to environment in determining the intelligence and personality of individuals—without actually revising assume innateness and heredity of psychological traits.

Terman would become an exemplary case in rebranding of the American eugenics movement toward a “positive” eugenics: one that focused on the duties of marriage and motherhood. Born to a central Indiana farming family in 1877, Terman’s time as an undergraduate at that state’s university was under the tutelage of many former G. Stanley Hall students. With their encouragement and recommendation, Terman began his PhD in 1903 at Worcester’s Clark University. Clark was a bastion for American psychologists thanks to Hall, the enterprising evolutionist who was still the university’s President. Though he learned from many prominent Clark professors, Terman was awestruck and “intoxicated” with how erudite and sagacious Hall seemed in his Monday-evening seminars (Minton, 1988, p. 23). It was at Clark that Terman began researching mental testing and intelligence among adolescents for his doctoral research. Though aware of Spearman’s work on a single factor of intelligence, Terman was more taken the work of Thorndike and the possibility of several mental abilities (Minton, 1988, p. 27).

Despite not pursuing Spearman’s tools of factor analysis as Thurstone and Guilford

would later⁵⁰, and despite his dissertation being a qualitative look at the different aspects of tests and intelligence, Terman certainly had faith in the powers of statistical approaches. His famous update of the Binet-Simon intelligence test, and the eventual implementation of versions of his Stanford Revisions on American recruits during WWI, is a well-known piece of psychology's historical tapestry that many historians have covered (e.g., Carson, 1993; Fancher, 1985; Kevles, 1968b). Terman's output from the late 1920s into the 1930s perfectly reflects many of the topics now within the purview of the new, "positive" eugenics. Continuing his work on children and pursuing a Galtonian focus on the extreme cases of superior intellect, in 1921 Terman and several female assistants began the process of recruiting the most gifted and youngest students (as measured on the Stanford-Binet) from Californian city schools (Hegarty, 2007, p. 137). Beginning a few years later, research on the chosen gifted children were published in a series of long-running monographs titled *Genetic Studies of Genius* (Terman, 1925).

The gender and sexuality of precocious children had always been of interest to Terman, even during his graduate days when publishing work suggesting that gifted children were particularly susceptible to the degeneracy and "sexual inversions" of a modernizing civilization (Terman, 1905). As Hegarty (2007, p. 136) points out, Terman's paper was published during the same time his professorial idol G. Stanley Hall was theorizing on sexual desires leading to a precarious "adolescent" stage of life where willpower and self-control were at stake until marriage, particularly for males. Terman's early writing on genius as linked to degeneracy and sexual inversion was also engaging with Victorian era ideas of the "mad genius." Conflating exceptional intellectual giftedness with desperate neuroses was particularly felt within the German Kultur, with its mad yet genius heroes like Nietzsche and Wagner (Drinka, 1984). Even

⁵⁰ For Terman's thoughts on Spearman's factor analytic research on unitary intelligence, see Terman (1932, p. 319).

though by the time Terman began his 1920s research project on gifted children he would adjust his earlier views, how “masculine” or “feminine” his studied children acted would still feature prominently in the work.

Terman had been interested in the abnormal since he was a child. Among his most memorable schoolmates, he recalled those who were exceptionally unintelligent, gifted, neurotic, and immoral. Terman considered his othering of such schoolmates as educational as his actual lessons, witnessing and forever remembering schoolyard deviants more so than the gifted:

...a feebleminded boy who was still in the first reader at the age of eighteen, a backward albino boy who was pathetically devoted to his small sister, a spoiled crippled boy given to fits of temper and to stealing, a boy who was almost a ‘lightning calculator,’ and a playmate of near my own age who was an imaginative liar and later came into national prominence as an alleged swindler and multi-murderer. (Terman, 1932, pp. 300-1).

Terman had become good friends with Robert Yerkes during their WWI efforts on intelligence testing. Afterwards, Yerkes chaired the National Research Council’s Committee for Research for Problems of Sex. In the 1920s into ‘30s, Yerkes, a comparative psychologist, funneled funds from the Rockefeller Foundation toward hormonal studies and studies of animal behaviour—avoiding any controversy in using funds for studies using what would look like sex surveys in more modern eyes (Hegarty, 2013, p. 4). An exception to this would be Terman’s new direction in his research, turning to the gendered personality of gifted children and eventually toward the marital happiness of adults. Later on, while still conducting and publishing this research, Terman was also a member of the privately funded Committee for the Study of Sex Variants; a body established to fund research on psychological and sociological aspects of “normal” sex behaviour (Minton, 1986, p. 3). That group’s only finished project was psychiatrist

Georg Henry's two-volume *Sex Variants* (1941)—a series of case studies that portrayed the inversion of sex roles as both cause and symptom of homosexuality (Minton, 1986, p. 6).

Initially, Terman's researchers noted the proportion of boys to girls among the gifted and non-gifted children's activities, leading to a masculinity index that was then used for an individual masculinity score based on the average masculinity for that child's preferred activities (Hegarty, 2007, p. 138). By the end of the decade, with the help of former student Catharine Cox Miles⁵¹, the *Genetic Studies* series began including a Masculinity-Femininity (MF) test. At stake for Terman was decoupling popular associations between precocious children and less desirable attributes like being spoiled, neurotic, or homosexual (Minton, 1986). Terman's Galtonian normalization, allowing for an abnormal but not pathological positive extreme such as being intellectually gifted would clash with other forms of precocity—guiding Terman's harsh critique of Alfred Kinsey's research that seemed to normalize homosexuality (Hegarty, 2013, p. 16). Terman helped police deviancy and uphold acceptable exception: his gifted were strictly intellectually precocious, making them exceptionally adjusted individuals who (in the long run) would not be sexually queerer than average (Hegarty, 2013, p. 90). An MF score could help ensure gifted children did not channel their precocity into transgressing their innate sex through gender-dissonant interests—sexual and otherwise.

Further work on validating the MF scale was consonant with Committee for the Study of Sex Variants' ultimate emphasis on sex role deviation. Though Terman apparently conceded sexual equality in intelligence, mirroring a larger waning enthusiasm for psychological sex differences research in the 1930s, research as presented in *Sex and Personality* (1936) intended to explore presumptive sex differences in “instinctual and emotional traits” (Minton, 1986, p. 9).

⁵¹ Though Miles's attitudes toward sex and sex research differed from Terman's, eventually leading in a falling out between them during the writing of *Sex and Personality* (Hegarty, 2013, p. 85; see also Hegarty, 2012)

Beginning as an explicit MF score, Terman's measure of masculine and feminine personality types would evolve into the Attitude-Interest Analysis Test (AIST). The test's expansion and change of name were likely to disguise the test's purpose: to see if a subject measured positively (masculine) or negatively (feminine) (Minton, 1988, p. 169). Switching from how the sexes differed in mental ability to how they differed in specific, occupational ability also reflects how personality testing of the 1920s onward helped encourage a complementarity norm of the sexes.⁵²

In an effort to further validate their work, Terman (working with E. Lowell Kelly) gained access to a crucial "non-normal" comparison group: homosexuals, or, in Terman's parlance, sexual inverts. Lowell was sent out to survey homosexuals, such as at the San Quentin prison—where one of Terman's gifted, the composer Henry Cowell, would serve four years for committing oral sex with another man. In his letter of support for an appeal, Terman portrayed Cowell as an inauthentic sexual invert—rather he was a gifted subject only delayed in his adjustment (Hegarty, 2013, p. 89). Alongside working against the acceptability of sexual precocity, binary logic also informed and delimited Terman and Kelly's work on homosexual groups. True homosexuals were passive males while the inauthentic inverts were extremely masculine yet still attracted to the same sex—making them perverted rather than truly inverted. For females, the opposite was the case: to be active (such as aggressive) was to be a true invert (Minton, 1986).

The AIST subtests included word association tests, popular with an earlier era of mental testing, and an inkblot projective test (Morawski, 1985, p. 205). Terman and Miles' book on their AIST research, *Sex and Personality* (1936), described the male and female psyches in ways

⁵² Though in the norm of multiple normativities, Terman's work with Quinn McNemar revived the older, variability hypothesis of intelligence among the sexes (as Hegarty, 2013, points out).

consonant with the Victorian notion of the separate spheres of binary sex roles (Morawski, 1985, p. 207). For example, masculinity was adventurous and object-oriented while femininity was timid and domestic oriented. The AIST would inform the inclusion of a masculinity subscale in a variety of popular psychological tests, such as Strong's Vocational Interest Blank and even into the next decade with the creation of MMPI.

While American Jung devotee Beatrice Hinkle was using psychoanalysis to promote a feminist psychology that rejected the research of sex differences and even the concepts of masculinity/femininity, psychologists were incorporating psychoanalytic concepts into an ongoing project of individual differences research. In transforming sex into a gender-trait, there was a problem: psychological traits were viewed as quantitative and on a continuum, demanding variation as the norm and typing as the abnormal. Lewis Terman developed a tool to measure one's degree of masculinity that defied this statistical normativity, just as the Guilfords had to when first confronted with their Item 36 ("Are you male?"). For personality psychologists of this era, the process of transmuting sex into a gender-trait only served to concretize the intransigent sex binary into their blossoming techniques. Even later psychological work on androgyny (Bem, 1974) may have only helped buttress the binary of masculinity and femininity (Morawski, 1985).

Measurable Wives

A certain binary logic was part and parcel of the two productively married couples who were using the intelligence test technique of factor analysis to develop personality measures of the Big Two during the early 1930s: the Thurstones and the Guilfords. In the Thurstones' work, based on previous questionnaires, questions presumed sexual inversion as a feature of neuroticism, like "Have you ever been afraid that you are sexually inferior to other men (other women)? (Thurstone & Thurstone, 1930, p.7). In their work, while they did find female students

scored higher in neuroticism than males, they offered the alternative interpretation that women were more open about their personalities than men (Thurstone & Thurstone, 1930, p. 18). Their charitable openness still maintained a distinction between masculine and feminine: trading a sex difference in personality for a gendered feature of personality.

In the Guilfords' work, sexuality was such an important aspect that they tacked it on as a final question in their measure, though confessed that—unlike all the other questions meant to measure personality traits—its binary forced-choice response did not reflect an underlying continuum. As mentioned above, appropriate gender norms was such an essential part of the Guilfords' work, and their participants' responses, M (possibly interpreted as “masculine ideal”) was among the only three factors they retained as meaningful and interpretable personality traits (Guilford & Guilford, 1936). Though race was mostly absent from these works on measuring personality, with the exception of “Jewishness,” a gender-trait, bound by binary logic and anchored by masculinity as its positive pole, was gaining ground even when sex differences were results were of little value or interest. Such a basic axis of individual difference, becoming naturalized into a personality trait itself, was wonderfully conducive to the wider rebranding of eugenics as a benevolent science of happy marriage and homemaking.

Such dynamics of gender, personality, and merit would carry over into Terman's research on marriage. Psychologist E. Lowell Kelly, who introduced Terman to the latest literature on marital advice—which stressed companionship and satisfaction—believed personality played a central role in a happy marriage. In 1931, while Kelley was working at the University of Hawaii, Terman received funding for longitudinal research on marital happiness from the same source as he had for his AIST research. Unfortunately, Kelly left Hawaii not for Stanford to work with Terman but for a position at the University of Connecticut, leaving Terman with a research

project on marriage that he did not independently conceive (Hegarty, 2007, p. 145). Instead of abandoning the research, he reached out to a fellow colleague of the Human Betterment Foundation and now director of the American Institute for Family Relations (AIFR): Paul Popenoe.

Terman's conclusion that personality, rather than female sexual satisfaction or male-female sexual compatibility, determined a happy marriage not only confirmed Kelly's beliefs but played well into the AIFR's evolving approach to marriage counseling.⁵³ Echoing Galton, Terman envisioned a future where numerous couples undergoing "extensive batteries of ability, personality, interest, and compatibility tests" at "matrimonial clinics" was commonplace (Terman, 1932, p. 329). His colleague Paul Popenoe opened the AIFR in early 1930, modelling it after 1920s-era German clinics that assessed engaged couple's mental fitness. Popenoe's career path and interests in better breeding meshed well with California's wide network of eugenicists. Formerly studying under eugenicist and founding university President David Jordan Starr at Stanford, Popenoe went on to become an agricultural explorer and plant breeder. With the financial aid of Ezra Seymour Gosney, a prominent and philanthropic eugenicist who had very recently founded the Human Betterment Foundation, Popenoe opened the AIFR to keep pursuing his passion for proper human breeding (Stern, 2005, ch. 5).

Unsurprisingly, unlike other marriage counselors of the era, the AIFR viewed heredity as crucial to marital issues, emphasizing genetic counseling. Although being appropriately masculine or feminine was part of the AIFR's foundation of advising fit couples to reproduce, whether someone was an introvert or extrovert was also crucial to their counseling (Stern, 2005,

⁵³ Though Popenoe's marital advice of the 1920s focused on sexual satisfaction (for the race and class appropriate couple), he even psychologized this in understanding good sex as intelligent strategy for monogamous coupling (Hegarty, 2013, pp. 51-2).

p. 171). As eugenicists were transitioning from pushing sterilization to newer forms of population control, the interdisciplinary field of “biotypy” blossomed. Biotypologists aimed to scientifically classify humans not by “categorical pyramids” like race, but new “gradated spectrums” (Stern, 2005, p. 152). Everything from blood sampling, dream analysis, Rorschach inkblots, and various personality tests were used for this new system of organization. Popenoe’s writings on introverts and extroverts also drew heavily from the emerging field biotypy. For Popenoe, other marriage counselors were fools to ignore inborn differences in personality among engaged couples.

A prime example of Popenoe’s public outreach is his 1937 *Scientific American* article covering “introverts and extraverts.” He began his article with a simple affirmation: these types of people exist, and anyone can observe their existence—“Look at a group of inventors, then at a parade of Shriners (Popenoe, 1937, p. 197). Clearly evident differences could be seen beyond mere occupation; people were different types of temperament and “body-build.” While these differences in personality were “not associated with intelligence” and could be influenced by “childhood training or surroundings,” they were necessarily inborn due to their close relation to physique (Popenoe, 1937, p. 197). For example, introverts had slender body types with poor circulation while extraverts had sturdy body types prone to hypertension.

For Popenoe, pointing to research at the AIFR in L.A., introvert and extravert types were relevant to marriage compatibility in perhaps an unexpected way. Although he conceded that many others had suggested “an introvert and extravert would be mismated [*sic*],” the AIFR’s findings suggested any combination of types could produce a successful marriage so long as the couple were aware of their “weak and strong points and use that knowledge intelligently” (Popenoe, 1937, p. 199). IE were to be considered aspects of normal personality—just as “most

of us are medium height or weight,” most people were in fact ambiverts (Popenoe, 1937, p.200). In Queteletian form, only the most extreme introverts and extraverts were abnormal, therefore presumed pathological. At worst, emotional problems could tip into the terrain of neurosis—though even introverts became neurotic in a different way than extraverts, becoming neurasthenic rather than hysteric (Popenoe, 1937). Popenoe, like many psychological experts, conferred a special status for knowledge of personality, improving both relationships and personal welfare. To gain this knowledge, you had to rely on the research and tools that experts like Popenoe and colleagues provided—including measures of your masculinity-femininity, itself becoming an essential personality trait.

To be sure, exploring sex roles in producing happy and fit marriages and children were not the exclusive purview of eugenicists. As explored above, the problems of modern women’s “masculine protest” and its negative effects on their roles as mothers and wives was central to psychoanalysis. Even Karen Horney’s writings on marriage that reframed the issues in feminine and sociological terms helped stabilize the sex binary into a binary of sex roles. But Terman’s work parted from Horney’s in at least two crucial ways, barring the obvious disagreement on the cultural determinants of human psychology. First, whereas Horney’s psychology focuses on the sexual inversions of dissatisfied women, masculinity and its inversions (and the promise of heterosexual rehabilitation) more often anchored Terman’s work. Second, whereas Horney argued against a patriarchal society that deliberately barred women from work, Terman’s emphasized inequality—such as the exclusion of women from paid work—as a feature among happy and compatible marriages (Hegarty, 2007, p. 146).

In the late 1920s, Horney slid from writing about feminine psychology toward an analysis of marital conflict and happiness, which perhaps reflected marital issues in her personal life

(Garrison, 1981, p. 680). Horney's earliest writings on marriage were somewhat congruent with eugenicist thought. In 1927, Horney contributed three essays on marriage to Max Marcuse's book *Marriage: Its Physiology, Psychology, Hygiene, and Eugenics – A Biological Marriage Book* (all collected in Paris, 2000, pp. 39–67).⁵⁴ While Horney thought the “medical-eugenic viewpoint” of physical fitness in determining the potential for healthy progeny was essential, she argued for the equal importance of psychological fitness (Paris, 2000, p. 40). Yet she held little faith in any psychological expertise being able to predict the development of a marriage—a complex sexual relationship between two unique individuals. Though she saw studying the psychosexual adjustment of relationships as a first step toward understanding psychological fitness for marriage, she dismissed a method that proceeded “from established constitutional types or, in a more naïve way, from certain human traits” as such types were either too physiologically oriented or too general to inform about specific marriages (Paris, 2000).

Still working within an Oedipal reading of relationships, Horney thought the pursuit of monogamous relationships in spite of widespread marital dissatisfaction was a form of wish fulfilment rather than genuine love. (Horney, 1928). Such work on marital conflict pointed toward sociological causation while still operating within Freudian biologisms of male and female instinct (Garrison, 1981). While Beatrice Hinkle promoted abandoning male and female in place of Jung's introvert and extrovert in pursuing compatibility and equality in the household, Horney's work at this point still focused the sex binary—though with an emphasis on female/femininity as seen through the lens of culturalism.

Marital sex manuals in the 1920s into the 1930s tended to promote enhancing male sexual performance in pursuit of increasing female sexual pleasure (Neuhaus, 2000). Yet in the

⁵⁴ Actual German title: *Die Eche: Ihre Physiologie, Psychologie, Hygiene, und Eugenik – Ein Biologisches Ehebuch.*

case of Terman's research on marriage, despite the book market's focus on husbands' experience and wives' bodies and sexuality, the gendered personalities of wives and husbands mattered most. Inadequate wives, those who rarely if ever orgasmed, caused inadequate marriages. Wives' inadequacies were rooted in an indecisive, conformist, and neurasthenic personality. Hence the cause of marital and orgasmic inadequacy was re-located within women's personalities rather than their marital relationships or their husbands' experiences and abilities (Hegarty, 2013, pp. 57–58). Terman's dismissal of sexual compatibility in determining marital happiness likely reflected his own normalized infidelities (Hegarty, 2013; Minton, 1988).

Unlike Terman's conclusions on personality and marriage, Horney's output in the late 1920s into the early 1930s repositioned man as the inadequate half. Terman's construal of the female as inadequate in terms of gendered personality proceeded in the vein of intelligence research rooted in the Victorian assumption of separate (and ranked) spheres of male and female. His work was also consonant with orthodox psychoanalysis' penchant toward the androcentrism of masculinity protests and complexes, as well as the heteronormativity of American psychoanalysis and psychiatry reacting against neo-Freudianism well into the Cold War era (Herzog, 2017). These norms necessitated a stable personality that was innate, bred, and measurable. In sharp contrast, Horney's work was one of self-discovery, growth, and hope—though she focused on change at an individual level rather than social change (Díez Manrique, 1984; Garrison, 1981).

For both a culturalist psychoanalyst like Horney, and eugenicist marriage experts like Terman and Popenoe, personality was something to publicize. Horney's mode was more self-oriented: she contributed widely bought and read books to an emerging non-fiction market, universalized a culturally bound form of neurosis, and promoted self-analysis as a path to

psychological knowledge. Although Popenoe certainly publicized his work, and even included a short introvert-extravert questionnaire in his *Scientific American* article explored above, personality psychology's version of publicizing was still oriented toward experts. To tap into the full extent and knowledge of personality, one had to seek expert advice of marriage counselors, psychologists, and their specialized measurement tools. Although through introversion measurements like Laird's or Bernreuter's (see below), Terman scored high in introversion as presumed most introspective-inclined psychologists would (Terman, 1932, p. 303), his scientific output was mostly extraverted and devoted to othering. The heteronormativity and binary logic of Terman's output could be due to his professional identity as an applied scientist, tied to the moral assumptions of an empiricist model of science (Minton, 1986). Perhaps more accurately, Terman's forms of normativity fit with the necessary stock for a eugenics-in-transition: gender conforming, straight, white, monogamous breeders.

“Imbrication” of the Big Two?

Among the personality questionnaires used in Terman's research (and on himself to see his introversion score) as well as at Popenoe's AIFR was the BPI: the Bernreuter Personality Inventory (BPI). Robert Gibbon Bernreuter's personality inventory began life as his dissertation, completed under the guidance of Terman at Stanford University in 1931. Despite a career of many accomplishments, Bernreuter would largely be known and remembered for BPI. As one obituary writer put it, “not many psychologists achieve immortality through their doctoral dissertation” (G. M. Guthrie, 1995, p. 41). Preceding the Minnesota Multiphasic Personality Inventory (MMPI) by more than a decade, one of the BPI's claims to fame is that it was psychology's first single list of questions intended to measure multiple personality traits. The initial four traits were neurotic tendency, introversion, ascendance-dominance, and self-

sufficiency. Research on the BPI led Bernreuter to consider the possibility that tools measuring neuroticism and introversion often correlated because the two ostensibly distinct traits may actually be one—a state he referred to as “imbrication” (Bernreuter, 1934).

The BPI is an essential but somewhat forgotten tool in the history of personality testing. As a personality measurement, historians have rightly placed it within the Woodworth-Laird-Thurstone lineage of neurotic inventories. Such tools pitched to determine a worker’s or employee’s level of emotional adjustment and stability were particularly attractive to owners and managers of industry (see Napoli, 1981). The BPI, along with the Humm-Wadsworth Temperament Survey, was a multi-trait tool successfully marketed to industry during the interwar years—despite the disjunction between test sales and multiple studies indicating a lack of test validity in industrial settings (Gibby & Zickar, 2008, pp. 170–172).

An important part of understanding Bernreuter and his inventory is situating his story within the story of American eugenics, especially among social scientists and eugenicists along the western coast and into the Pacific. Terman and Australian-American Stanley Porteus were two researchers central to the promotion of a eugenics-anchored enterprise of psychological practice. They laid the intellectual path for Bernreuter’s entire career—from the internationally used BPI to his implementation of various forms of testing in Pennsylvania State University’s Student Services that included marriage counseling. Bernreuter’s life and work, including his claim of an “imbrication” of neuroticism and introversion, speaks to the more general and ongoing fusing of psychoanalytic and eugenicist ideas within which he operated.

Personality of Western Pioneering

Bernreuter was born in Tampico, Illinois on December 9, 1901 to a father who was a Methodist minister and a mother who worked as a nurse. Two years later, his father died and

Bernreuter's mother raised him alone in Woodstock while still working. After graduate school, he enrolled in the University of California to become an electrical engineer but wound up failing most of his courses. After restarting his academic career at the College of the Pacific, Bernreuter remained anything but studious as he was much too preoccupied with life beyond the classroom. As he recalled, at College he was "majoring in extra-curricular activities ... and minored in psychology."⁵⁵ Like many psychologists who played key roles in the rise of personality testing before him, such as Thurstone's graduate term with Elton Mayo at the Philadelphia General Hospital and Max Freyd's brief stint at the Boston School of Psychopathology, Bernreuter was exposed to psychiatry during his education. During his undergraduate years, a summer position as an attendant at the California State Mental Hospital in Agnew sparked an interest in psychopathology.

California was a leading state in American eugenics and its implementation of sterilization. Social welfare institutions often became the main venues for this violent manifestation of eugenics ideals. Eugenics and the associated belief in hereditarianism have deep roots in the California's past. After a violent history of the USA annexing the area from Mexico and capturing its towns, California became the 31st state during the Gold Rush era in 1850. In the late nineteenth century, families from the settled communities of the eastern seaboard heading west to lay claim to areas in the newer state. Those in power had long been ordering their state according to principles of race betterment. For example, though it was far from a unanimous congressional decision, the Chinese Exclusion Act of 1882 severely limited Chinese immigration and precluded the possibility of gaining citizenship—rendering Chinese lives as legally lesser

⁵⁵ Taken from a biographical sketch (based on an interview with Bernreuter) titled "A Bit About Robert G. Bernreuter" written by a "Caroline Bird" (handwritten authorship note) and dated 1946. Found in Box No. M2625, Folder 2, The Robert Gibbon Bernreuter Papers (hereafter RBP), The Drs. Nicholas and Dorothy Cummings Center for the History of Psychology, The University of Akron.

(Fisher & Fisher, 2001). By the early twentieth century, a large network of scientists and reformers aimed at making hereditarianism essential to state practices. Whether through agricultural expertise, wildlife preservation, or the more obvious medical and psychological interventions, professionals holding eugenics ideals literally shaped California's spaces and inhabitants (Stern, 2005, p. 84).

Though eugenics held broad appeal for many reformists and scientists as it was often a loosely defined doctrine that numerous people of differing political persuasions touted as a possible fix to social and moral problems, it had a particular appeal to white supremacy in California. Notions of racial purity and hierarchy meshed well with the pioneering spirit of the west that hinged on white settler's divine rights of manifest destiny (Stern, 2005, p. 113). In California, and other Western states—as well as Canadian provinces and northern territories (see Dyck, 2013; Dyck & Lux, 2016)—a vision of a White political democracy for resettling families marginalized and racialized all others, such as transient male labourers. Even though much of the West's population growth was due to the national and international travel of migrants from varied backgrounds who drove many economic markets, this labour force was understood as replaceable and sub-human (Shah, 2011).

On top of their labour, the ways in which non-White denizens and transients lived and loved threatened fundamental aspects of a White democracy. The inter-generational or other non-nuclear systems of co-habitation, the non-monogamous forms of relationships, and the non-binary ways of being helped recognized citizens more clearly define the heteronormativity of a White, Christian, monogamous ideals—ideals that guided legislative and political power over racialized minorities (Shah, 2011, esp. Ch.5). As eugenicists shifted toward “positive” eugenics of controlling reproduction and prescribing proper pairing, the mere existence of non-Whites and

other moral deviants did not threaten the racial stock. Their radically alternative forms kinship, love, and rearing threatened the process of racial betterment through “scientifically” ratified marriage and copulation.

It was in this California that Bernreuter earned his A.B. in 1924 from the College of the Pacific. Like Guilford around this time, Bernreuter briefly taught children at a rural school in Capay Rancho, California, before starting his graduate career at Stanford under the wing of intelligence testing giant—and fellow former schoolteacher—Lewis Terman. By 1922, Terman became executive head of Stanford’s Department of Psychology, and during his first term he hired Edward Kellogg Strong, a psychologist Terman had met during their time working for the Army and who had built his reputation at Carnegie Tech in Pittsburgh. He created the Strong Vocational Interest Blank—still used today but renamed the Strong Interest Inventory. In his new position, Terman became successful at securing funds and became influential in the university’s affairs, cultivating Stanford as bastion for psychological testing congruent with the university’s eugenicist values (Minton, 1988, pp. 134–139). Under Terman’s guidance, Bernreuter developed a crucial tool in the transition from intelligence testing to measuring personality traits. Though Terman’s own work in the 1920s did not yet venture into the study of personality, he was expanding his scholarly focus beyond mere intellectual ability.

Before Bernreuter began working on his dissertation in earnest, Terman presented him with once-in-a-lifetime opportunity: a two-year research assistantship with an established psychologist. The place: Far west to the University of Hawaii in Honolulu. The research topic: intellectual and cultural differences among the region’s races. The grantee: prolific eugenicist, ethnographer, mental tester, and friend to the Stanford elite, Stanley Porteus. Though Bernreuter had taught children while a schoolteacher and dealt with various “deviants” while an attendant at

California State Mental Hospital, the research opportunity with Porteus was a chance to build on these experiences while strengthening his test administering skills.

Porteus was a former schoolteacher in Australia, who eventually specialized in Special Education—focusing on feeble-minded children (Porteus, 1969). He began corresponding with Terman in 1915, which helped secure his first American job offer in 1916: Director of Research at the Vineland Training School in New Jersey. Though taking over the position for Henry Goddard—a widely known feeble-mindedness researcher and promulgator whom Porteus greatly admired—was an initially awkward transition, it led to Porteus’ taking an academic position in the Pacific (Porteus, 1969, pp. 68–69). Porteus’ name had been suggested to the Dean of the University in Hawaii in Honolulu as someone who could establish and direct a Psychological and Psychopathic Clinic. Such a research clinic at the university was meant to complement the Waimano Home for the Feeble-minded, which a Dr. Arthur Andrews (after reading Goddard’s *Kallikak Family* and being enlightened and frightened) pressured the local government to recently establish (Porteus, 1969). From 1919 onward, Porteus would continue his prolific output anchored by his interest in the mentally defective, primitive races, and the further development of his non-verbal and purportedly culture-fair Maze Test of intelligence.

In 1927, Porteus and others working on racial differences at the University of Hawaii benefitted from a Rockefeller Foundation grant to continue and extend their work. The political position of Rockefeller philanthropy on race relations during that time was quite complex: while some of their philanthropy was consistent with segregation, other Rockefeller subsidiaries were mounting “action-oriented approaches” to studying race relations across the social science disciplines (Gordon, 2015, p. 53). The research project Bernreuter helped with in Hawaii was not Porteus’ only Rockefeller-funded study of racial differences during this time. Shortly after

Bernreuter returned to Stanford in 1928, The Australian National Research Council, with support from the Rockefeller Foundation, granted Porteus the funds necessary to psychologically investigate the country's Aborigines. Porteus saw this consecutive data-collection project as a great opportunity to gather statistics "on a really primitive people that would prove basic to our studies here [in Hawaii]" (Porteus, Dewey, & Bernreuter, 1930, p. 95).

Even though it had been nearly three decades since Edward Ross (1901) had warned of "race suicide" among the white middle class—a sentiment that the popular press and even the standing President of the United States, Theodore Roosevelt, echoed—Porteus, like many other eugenicists and progressives alike, was still concerned with dwindling birthrates in the interwar years. In the eventual report of the Hawaiian research conducted with Bernreuter, published in one of G. Stanley Hall's many outlets, *Genetic Psychology Monographs*, Porteus warned readers: "If the present birth-rate tendencies continue the white man's superiority may easily prove to be the white man's undoing; the more time and effort he spends in turning out motor cars the less attention he may give to turning out children" (Porteus et al., 1930, p. 101). Though explicitly about race suicide, rehashing concerns over the reproductive habits of the fit race was conducive to the "positive" eugenics shift seen in Terman and Popenoe's research on gendered personality and marriage.

Additionally, Porteus' work during this time sat well with adjusting eugenic sentiments on the role of environment. His very locale offered him a way to offer lip service to the role of environment in the determination of a race's intellect and temperament. In Porteus' view, Hawaii was "a natural laboratory for racial studies" (Porteus et al., 1930, p. 174). Sketching a dubious history of the islands—one that suggested that unlike the continental USA, slaves were never forcibly supplanted from their homes and resettled in Hawaii—Porteus saw Hawaii as a natural

laboratory for the control it offered on cultural influences. In his mind, the islands were a natural laboratory for racial differences because all non-biological variables were *ceteris paribus*.

On top of his views on the apparently rare geography of Hawaii, Porteus conceded a role for environmental influences, but also eschewed problematic verbal tests of intelligence in favour of non-verbal tests of mental performance or ability (Porteus et al., 1930, p. 106). This deflection doubled as a marketing boost for his Maze Test, a non-verbal “performance test” which he and his assistants (including Bernreuter) administered on thousands of school children from varying racial backgrounds across Hawaii. Given the role he stressed on accounting for environmental forces in the design of his tests and research, one might think this transferred to a softer position on the permanency of psychological traits. Yet, in that same report Porteus scoffed at Watson’s famous claims of a behaviouristic psychology molding a child’s identity. Porteus’ faith in the immutable, biological condemnation of degeneracy remained unshaken: “it is recognized by all who have participated in the training of defectives that the effort to make a silk purse out of a sow’s ear is, as far as these cases are concerned, entirely hopeless” (Porteus et al., 1930, p. 111).

When Bernreuter returned to Stanford in 1928 from his research trip to Hawaii, the American eugenics movement was expanding toward a “positive” form that accommodated environmental influences and tackled reproduction topics like marriage. Bernreuter dropped any racial research, though he apparently maintained a private interest in the study of racial minorities—especially the Japanese after the Pearl Harbor attacks. Bernreuter had little to say about his time in Hawaii; he kept details of his doctoral detour in the Pacific sparse or absent from his few biographical documents and resumes.⁵⁶

⁵⁶ For example, in the 1946 biographical sketch apparently written by Bird, and other CVs and forms available in the RBP.

Bernreuter Heads East

In terms of disciplinary impact, Bernreuter's dissertation and its resulting BPI earned him immense and lengthy success. A decade later, Terman was still complimenting Bernreuter's success—likely feeling proud of his own successful training of a protégé who ended up mounting his own renowned psychological test. Recalling his apparent foresight, Terman reminded Bernreuter how he told him “it was certain to arouse a lot of interest.”⁵⁷ Astounded at the hundreds of studies that had since made use of the BPI, Terman further vicariously boasted: “It is even beyond my expectations and must almost set a record ... your Ph.D. dissertation ‘rang the bell’ as very few in my experience have done.”⁵⁸ Terman's excitement was warranted. The BPI had not only become a common tool for American psychologists and sold well to industry; it was also constantly being translated for use around the globe, from the Hindu BPI to the Japanese BPI.⁵⁹

The BPI was marketed to the public even before its formal publication in 1931 after Bernreuter received his doctorate—most likely with help from Terman and his network of eugenicists. As early as 1929, Albert Wiggam, the popular psychology journalist and ardent eugenicist, had used some questions from Bernreuter's personality inventory research in reporting on Terman's marriage research for a piece in *Cosmopolitan*. In a letter to Bernreuter more than a decade later, Wiggam recalls this promotion of the BPI and asks for a “little test” to feature in his upcoming article called “The Happiest Married Couple I Know.”⁶⁰ The BPI was so successful that other psychologists were profiting off its popularity. Cornering the market in

⁵⁷ Lewis Terman. Letter to Robert Bernreuter. February 9, 1942, Box M2625, Folder 5, RBP.

⁵⁸ Lewis Terman. Letter to Robert Bernreuter. February 9, 1942, RBP.

⁵⁹ Several examples of translated BPI items, along with correspondence between Bernreuter and researchers from around the world, are held within the RBP.

⁶⁰ Albert Wiggam. Letter to Robert Bernreuter. August 10, 1941, Box M2625, Folder 5, RBP.

technology that would aid in scoring the BPI, a Professor Carlie of the Bureau of Research & Clinical Service at Butler University in Indiana put out an advertisement for his “Carlie’s Scoring Bars.” Carlie’s ad claimed that the scoring bars would allow a psychologist to score all six subscales of the BPI “in less than nine minutes!”⁶¹

Bernreuter himself market his tool’s convenience, and importance, even in academic venues. In an article detailing the BPI’s construction, he noted that other tests that only measured a single trait were too long and, worse, artificially inflated the importance of that trait—obscuring “the still significant greater significance of the total integrated personality” (Bernreuter, 1933, p. 387). In contrast, the BPI could measure four traits central to personality *in toto*. In effort to further generalize the tool, as had been done in moving from a single to multiple traits, Bernreuter also modified previous tests’ questions to preclude referring to “experiences of a special group only, such as a college classroom experiences, or to a single sex” (Bernreuter, 1933, p. 390). Nevertheless, the BPI’s items were validated using criterion groups of students⁶² from a Californian junior college, two state colleges, and, of course, Stanford University (Bernreuter, 1933, p. 391). Additionally, as the BPI was meant for both men and women, attention was paid to their differences: with females tending to be more neurotic and introverted, and less dominant and self-sufficient than males (Bernreuter, 1933, pp. 393-4).

In 1940, Robert Woodworth wrote Bernreuter asking his opinion on “some form of questionnaire [for] indicating pre-neurotics or similar poor risks among the drafted men or volunteers for the Army and Navy.”⁶³ Reflecting the changing methodological expectations and alluding to work done in Nebraska (most likely the Guilfords’ work on personality scales),

⁶¹ Found in Box M2625, Folder 11, RBP.

⁶² The students’ ethnicities were not disclosed, and perhaps not recorded.

⁶³ Robert Woodworth. Letter to Robert Bernreuter, November 17, 1940, Box M2625, Folder 5, RBP.

Bernreuter recommended using factor analysis for devising such a questionnaire.⁶⁴ Future colleague and Air Force researcher, John C. Flanagan, had factor analyzed and forever revised the BPI for his doctoral dissertation (Flanagan, 1935). His research suggested that the BPI's items were sortable into two general factors: emotionality (later labelled self-confidence) and sociability.

Meanwhile Flanagan would put both factor analysis and personality testing to work within the US Air Force. He was a Harvard graduate who was working with National Research Committee funding on a project on assessing the traits and abilities of pilots. Flanagan was crucial to the Army Air Forces (AAF) Aviation Psychology Program. Having worked with Thurstone during summer graduate courses at Yale, Flanagan brought factor analysis methods to the Aviation Psychology Program (Capshew, 1999, pp. 108–110). In 1938, while working for the AAF, Flanagan received a funding offer from prominent anti-immigration eugenicist Frederick H. Osborn who ran the Army's Information and Education Division during WWII. The patron backing Osborn's offer was the newly established The Pioneer Fund, a soon-to-be prominent financier of racially based eugenics research.

The Pioneer Fund was a private trust established just the previous year by Massachusetts textile heir Wycliffe Preston Draper. Its purpose was essentially to defend against race suicide; more specifically, to promote race betterment and provide aid in the education of children who descended from white settlers of the original 13 states or "related stocks" (Tucker, 1996, p. 173). Flanagan's research task served a simple purpose: helping ensure that white Air Force pilots, presumably some of the fittest men in the country, continued to breed a better race. The research project's purpose was firmly rooted in ongoing fears of demographic decline, or "race suicide"

⁶⁴ Bernreuter to Woodworth, November 19, 1940, Box M2625, Folder 5, RBP.

that fueled mid-twentieth century eugenicists rising interests in maintaining gender norms and appropriate marriage. Years later Roger Pearson—the noted British anthropologist, *Mankind Quarterly* editor, and white supremacist who spent several postwar years trying to mount a “Fourth Reich” to counter geographic “racial chaos” and Nordic annihilation—wrote admiringly of Flanagan’s research on Air Force pilots and their families (R. Pearson, 1997).

Flanagan’s work on the BPI was far from the only research contesting the test’s prominence. Towards WWII and beyond, due to several research articles and reports on the BPI’s poor predictive validity, the test was falling out of favour in industry and being replaced with tools like the Guilford’s Nebraska Personality Inventory (Gibby & Zickar, 2008, p. 171). While Bernreuter always presented the BPI as superior in reliability to other tests, as well promoting its ease-of-use as a shorter, all-in-one measurement for multiple personality traits, even he was not convinced of the tests’ categorization of traits. In his initial report on the BPI, Bernreuter suspected that his introversion and neurotic tendency scales were highly correlated because they were separate names “given to a single trait whose real nature has been obscured by the inadequacies of [previous] tests” (Bernreuter, 1933, p. 402). Agreeing with Edmund Conklin, deviser of the ambivert personality type or ambiversion personality trait, Bernreuter thought previous tests of introversion measured “abnormal manifestations” of the trait, explaining its link to neuroticism (Bernreuter, 1933). Yet he still differed from Conklin’s Queteletian view of introversion-as-ambiversion—that is, the most average being most healthy—as he thought the more extroverted an individual the more emotionally stable that individual (Bernreuter, 1933, p. 404).

Bernreuter would soon devote further research to the thorny issue of how many genuine traits his scales were measuring. Going by his findings, and work of Conklin and even Freud,

who he thought suggested introversion was a “way-station on the road to neurosis” (Bernreuter, 1934, p. 185), he more deeply examined the “imbrication” of his neurotic tendency and introversion-extroversion scales. Using 157 engineering students (all male), he found his scales highly reliable when compared to several other personality tests. Given the intercorrelations among tests, he also thought that a general factor could explain this imbrication of traits. Distinguishing this hypothetical factor from Spearman’s *g* factor of general intelligence, Bernreuter labeled his general factor of personality *E* represented the emotional aspects of behaviour rather than the intellectual (Bernreuter, 1934, p. 196). He intended to further research his *E* factor, including its physiological bases, but this goal does not seem to have ever materialized as Bernreuter’s responsibilities and interests led him elsewhere—and eastward.

In 1931, with both Terman and E.K. Strong’s encouragement, Bernreuter took a position at what was then called Pennsylvania State College (now University). His debut post-doctorate position included not only teaching in-class and correspondence courses, but also establishing an education psychology-focused clinic (Guthrie, 1995). His clinic set the foundation for implementing various assessments and counseling services for the College’s students and applicants. The Psycho-Education Clinic existed within the larger Psychological Clinic that provided a variety of services. In an early Psychological Clinic Manual, co-written by Bernreuter (then Director of the entire Clinic), five primary service areas were listed: student advisor service; psychological counseling service; psycho-education clinic; veterans’ mental hygiene service; and marriage counseling service.⁶⁵ In those listed services alone, Bernreuter’s varied interests and experiences are obvious; including those of his doctoral supervisor, Terman. Pennsylvania College’s Marriage Counseling Service advised and researched engaged couples

⁶⁵ Manual for the Psychological Clinic found in Box M2625, Folder 1, RBP.

for both marriage compatibility and happiness. Like Popenoe's AIFR in Los Angeles, the Psychological Clinic used a variety of psychological tools for its various branches; including, of course, the BPI.

During a yearlong leave from Penn State in 1936, Bernreuter extended his services to the state's Department of Public Instruction. Specifically, he served as Chief for the Division of Special Education. As noted in a 1968 tribute to Bernreuter held at the annual meeting of Pennsylvania Psychology Association, during his brief time as chief he helped transfer legal responsibilities from physicians to psychologists. After successfully carrying out negotiations, Bernreuter "relieved" medical doctors posted to public schools for the purpose of examining "mentally retarded children," replacing them with public school psychologists. He also prepared the standards for licensing school psychologists and psychological examiners for the State Council on Education. Institutionally, with his university clinic and work on state education, Bernreuter contributed to repositioning faith in assessment for vocation, marriage, mental illness, and education from the psychiatric and medical establishment to disciplinary psychology. Psychology's tools of assessment, though often seductive in statistical prowess, were still used alongside projective techniques and interwoven with psychoanalytic ideas on personality and sexuality.

During WWII, Bernreuter became involved with on-the-ground research. He first spent two years in Washington working for the Army Specialist Corps. After being promoted to Lieutenant Colonel and working for the Far East Air Forces for another two years—apparently he was one of the first Americans to enter a devastated Hiroshima after Japan's surrender—Bernreuter finally left the military in 1946 (Guthrie, 1995).⁶⁶ Bernreuter joined the refrain of

⁶⁶ Details also taken from Bird's biographical sketch, RBP.

normalization during the postwar era that continued the decades-long trend of likening psychopathologies like war neurosis as extreme forms of prosaic problems (see Herman, 1995). When finally leaving the Army, Bernreuter redoubled his convictions that studying the “psychological equivalents of the common cold” was more useful than spending time on the “rarer deviations from human averages.”⁶⁷ In this way going against his mentor Terman’s obsessions with the exceptional, by the end of WWII, Bernreuter more confidently than ever aimed for the psychological study of normal personality. Though this normal personality was one that valued statistical normality and defined normality-via-pathology when considering basic traits like neuroticism, introversion, or their possible underlying factor/trait of emotional control.

Once returned to Penn State, he continued to work his way up the administration’s ladder, closing his career as Vice President of Student Affairs. His work led to the university’s System of Freshman admissions that used various psychological tests to predict academic success. In his opinion, this made freshman admissions “free of all political influence.”⁶⁸ From 1946 onward he also served as Technical Director of The Klein Institute for Aptitude Testing, spreading the BPI even further outside academic research—such as on tens of thousands of “salesmen profiles.”⁶⁹

Alongside Bernreuter’s career successes, Paul Popenoe’s AIFR would continue to grow into and after WWII. Their in-house tool, the Johnson Temperament Analysis (JTA) was thought to reveal women’s masculine protests and other forms of sexual inversion (Stern, 2005, p. 173). Through measuring pairs of traits such as nervous-composed, subjective-objective, and aggressive-submissive, the JTA echoed assumptions of the gendering of neurosis and introversion while following psychoanalytic notions of sexual development and pathology.

⁶⁷ From Bird’s 1946 biographical sketch, RBP.

⁶⁸ From Bernreuter’s CV, found in Box M2625, Folder 2, RBP.

⁶⁹ From Bird’s 1946 biographical sketch, RBP.

Along with the JTA, the AIFR implemented both psychological and psychiatric tools for personality, like Terman and Miles' masculinity-femininity scale, the Rorschach, Kent-Rosanoff Word Association Test, one of Guilford's factor-analyzed temperament tests, and, of course, the BPI. Bernreuter's BPI and his clinic at Penn State also involved the transmutation of the Other Big Two: psychoanalysis and eugenics.

Chapter 3 Conclusions

While neurosis and introversion were continuing to evolve in psychoanalytic theories and exports in the 1930s, coinciding with their further popularization in everyday discourse, psychological testers continued to incorporate these categories. For psychologists, normalized psychopathology was legible only with the aid of tools meant to enshrine and enact the moral principles of a class of eugenicist experts. Along with appropriating psychoanalytic categories, psychologists maintained a hypocritical relationship with psychoanalytic theory: pushing or pulling whenever ideas fit their norms. In Terman's case, he precluded Oedipal theories of parental attachment to explain marital happiness, while discounting analysis of topics like heterosexual extra-marital affairs (Hegarty, 2013, pp. 63-4)—a transgression rendered unproblematic in the eyes of a rationalized infidelity. Yet he had no issue entertaining Oedipal explanations of "sexual inversion" or homosexuality; an explanatory framework that partially posited an environmental cause for homosexuality rather than (to eugenicists) the more important and undeniable inborn traits of intelligence and personality (Hegarty, 2013, Ch. 4).

Similarly, Paul Popenoe and the AIFR would engage with psychoanalysis when it did not threaten their pursuits. Their in-house personality test, the JTA, relied on psychoanalytically orthodox notions of masculine protest in women—aligning with sexual inversion as pathology. Despite the disciplinary norm of rebuking psychoanalysis as non-scientific, Terman and Popenoe

still held faith for its theories when it did not transgress the political values of eugenics—leaving the psychoculturalism of Horney aside. As will be explored in the following chapter, personality and clinical psychologists contesting expertise over psychoanalysis, while sometimes maintaining a respect for Freudian orthodoxy, would persist as defining feature of the discipline.

In general, personality traits were not easily placed into a single form of normalization. The extremes of introversion could be construed as both abnormal and pathological—an assumption mentioned in early factor analytic personality research (Guilford & Braly, 1930, p. 96). Additionally, as in Bernreuter's early work on the BPI, introversion could be understood in more Galtonian norms where to be very extraverted was a preferred and socially useful quality. To be introverted was to be highly neurotic, especially in Bernreuter's imbricated state where such high scores reflected a lack of emotional control. Yet introversion could be drawn in Queteletian norms where only those who could function with a certain amount of both introvert and extrovert characteristics, known as ambiverts, were within the normal and healthy region (Davidson, 2017). Given the dizzying number of behaviours personality traits could encompass, and the varying moral and social readings of those behaviours from one context to another, it was a norm for multiple forms of normalization to be at play when quantifying personality.

Much as Goddard introduced the “moron” category and blurred the line between normal and abnormal mental ability (Zenderland, 1998, pp. 103–104), or sharpened the line between moral and immoral behaviour, extending psychopathological categories into the basic traits of normal personality allowed a continuation of the eugenics project. Yet the dissonance between extreme-as-exceptional and extreme-as-pathological muddied the waters. Binary logic helped settle this dissonance, as well as undergird the uncomfortable co-existence of personality types and traits. Though trait-like, a masculinity score assumed two types: man or otherwise.

Personality psychology held both implicit and explicit qualitative distinctions between types within quantitative trait continua. As will be shown in the following chapter, figuring out how to categorize the type-yet-trait of gender—as a single trait or as separate traits—would persist in the creation of clinical psychology’s watershed personality assessment tool, the MMPI.

Despite multiple normativities at play, the desire to transmute psychoanalytic and psychopathological categories like neurosis and introversion into key traits in the scientific study of normal personality united many personality psychologists. Even when what was normal, abnormal, healthy, or pathological seemed at best murky in their theorizations. Such conceptual slippage was true for both the content and structure of personality traits. In a move that would come to long define psychometric personality psychology (and perhaps scientific psychology en masse), refuge from theoretical uncertainties was found in a devotion to methodology.⁷⁰

Factor analysis was emerging as a preferred method among trait psychologists to deal with theoretical issues by setting them aside. Even though certain key psychologists would continue to be critical of factor analysis’ leading role in the science of personality, such as Paul Meehl, even those most familiar with simpler forms of test and taxonomy construction would concede to its usefulness, such as Bernreuter did to Woodworth in light of Flanagan’s factor analytic update of the BPI. Citing eugenicist and intelligence researcher Spearman’s work (C. E. Spearman, 1927), the Guilfords believed his techniques of factor analyzing measurement data would not only be applicable to introversion and extroversion, but “may solve the riddle of personality traits in general” (Guilford & Braly, 1930, p. 105). The Guilfords’ suggested itinerary for future personality research would become the mandate for much of trait psychology:

⁷⁰ The favouring of methods over theory is a topic crucial to the rise of factor analytic personality psychology, and its apparent apolitical status, and will be revisited in Chapter 5. Several historians, psychologists, and statisticians have documented this and related themes (e.g., Abelson, 1995; Bakan, 1967; Danziger, 1985; Gigerenzer, 2004; Lamiell, 2013; Porter, 1995).

factor analysis for taxonomy; biological correlates for theory. After using such advanced quantitative methods, thereby establishing the classification of the traits and their constituent “constellations of habits, tendencies, or dispositions”, psychologists could finally begin uncovering their “physiological basis” (p. 105).

The Guilfords’ early work on personality also touched upon several studies of the “physical correlates” of introversion and extroversion, from German psychiatrist Ernst Kretschmer’s well-known work relating body types to clinical types, to William McDougall’s frankly odd experiments on the effects of chemicals on dissociation (presuming a relationship between the latter and introversion/extroversion via hypnosis). By the late 1930s, a eugenicist like Popenoe would rely on the emerging work linking personality to physique as a way to deflect the importance of environmental influence on what he presumed to be innate traits. The next part of this dissertation will explore the proceeding generations of personality psychologists committed to quantitative methods, such as Hans Eysenck and like-minded trait psychologists, who continued the hereditarian framework for psychological traits well into the end of the century.

But before exploring the rise of personality psychology as a strictly psychometric endeavour of discovering universal trait taxonomies and their biological correlates, the next chapter will lay out the mounting tensions between psychology and psychoanalysis from the later interwar into the Cold War era. Pugnacious deniers of Freudianism, like Eysenck, and tortured psychologists who still held faith in Freud’s importance in explaining personality, like Paul Meehl, would be joined in a methodological battle against psychoanalytic expertise. As explored, Meehl’s profound skepticism and eclecticism would shape the development one of the most enduring personality tests of the discipline’s history: the MMPI.

Part II

Values

“The word which will not die, should we all perish in battle. The word which can never die on this earth, for it is the heart of it and the meaning and the glory.

The sacred word:

EGO”

-Ayn Rand, *Anthem* (1947 edition)

“PARNELL: *Race-mixing!* Ladies and gentlemen, do you think anybody gives a good goddamn who you sleep with? You can go down to the swamps and couple with the snakes, for all I care, or for all anybody else cares. You may find that the snakes don’t want you, but that’s a problem for you and the snakes to work out, and it might prove astonishingly simple—the working out of the problem, I mean. I’ve never said a word about race-mixing. I’ve talked about social justice.

LILLIAN: That sounds Communistic to me!”

-James Baldwin, *Blues for Mister Charlie* (1964)

Chapter 4

Boundary Experts:

Hans Eysenck, Paul Meehl, & the Scientific Psyche

At the 1996 Annual Convention of American Psychological Association (APA), its Division 12 (The Society of Clinical Psychology) held a centenary of sorts—marking the hundred years since the founding of America’s first psychological clinic (Routh, 2010, p. 23). In honour of the occasion, the Society presented two special awards for Clinical Psychologists of the Century: one to Minnesota’s Paul Meehl and one to London’s Hans Eysenck. In a history column in the Division’s newsletter over a decade later, the recipients were remembered as clinical psychology’s “odd couple” (Routh, 2010). Both Meehl and Eysenck were extraordinarily prolific writers on several facets of disciplinary psychology.

To a clinical psychologist, they would indeed seem to be an unusual pairing, given their opposing stances on therapy, assessment, and the classification of psychopathology. Crucially, Eysenck was an ardent and voluminous dissenter of psychoanalysis, from targeting the effectiveness of therapy early in his career (Eysenck, 1952) to writing about what he saw as the just decline of the “Freudian empire” toward the end of his career (Eysenck, 1985). Meanwhile Meehl believed in and practiced psychoanalysis. Despite Meehl’s famous book on the benefits of statistical techniques over expertise in clinical prediction (Meehl, 1954) and his lifelong infatuation with philosopher of science and noted anti-Freudian Karl Popper, Meehl’s faith in psychoanalysis certainly renders him an odd bedfellow for Eysenck.

Less apparent to a disciplinary insider are Meehl and Eysenck’s many points of congruence. Although they disagreed about the value of psychoanalysis, they converged on the

value of quantification and psychometrics in developing a scientific psychology. APA's message was clear: clinical psychology is a scientific practice. While Meehl helped further refine the massively successful Minnesota Multiphasic Personality Inventory (MMPI) for purposes that held closer to psychiatric virtues of classification and diagnosis, Eysenck developed his psychometric tools and taxonomies within personality psychology's polysemous normality: pathology exists at the extreme ends of the normal curves of a normal human personality. The pair shared a postwar goal of increasing the scientific legitimacy of psychology via its methodological practice. Additionally, they both dealt with a central challenge in developing tools for clinical or personality assessment: the test-taker herself. Whether through deliberate deception, unconscious self-deception, or classic malingering, the subject of a psychological test was an inherently dishonest creature not to be trusted.

This chapter examines overlapping aspects of Meehl and Eysenck's careers to discuss the larger processes of boundary work within psychology leading into and shortly after WWII. Though boundary work is related to the philosophical problem of demarcating science from pseudoscience—a topic which itself will be addressed in this chapter—here it means the practical problems academic psychologists face in delimiting and protecting their discipline's authority (Good, 2000, p. 387; see also Gieryn, 1983) The tensions between psychoanalysis and disciplinary psychology existed even as psychoanalysis was being introduced to the nascent American psychology scene.

Once intelligence testers, abnormal psychologists, and newly established factor-analytic personality psychologists began appropriating psychoanalytic types as psychological traits, such as neurosis and introversion, a set of virtues became evident. Among these virtues was the conviction of psychological traits as measurable phenomena. During the interwar years and

onward, psychiatrists and psychologists alike were making use of projective tests like Hermann Rorschach's inkblots. To secure their status as authorities on psychological measurement, psychologists had to simultaneously convince those within and without their discipline that their personality tests were valid instruments while claiming projective tests were scientifically useless.

Deceit is central to the wider story of the Big Two and this chapter's specific story about battling expertise over personality and psyche. Throughout the history of nervous diseases into neuroses, the realness of a patient's symptoms was a growing concern. The threat of malingering lingered over nervous disorders: from Charcot's performative hypnotic-hysterics of the Salpêtrière to the neurasthenics-cum-obsessional neurotics on psychoanalytic couches. During the Great War, the shockwaves of trauma among many draftees were met with suspicion from the traditional upper brass as well as the traditional physicalist neurologists. War neurosis was blurred with abject, male hysteria: Men with wombs carrying cowardice. Early efforts at screening for "psychoneurotics" or those with the potential to become one—as seen in Woodworth's efforts at measuring the emotional stability of American recruits and draftees (see Chapter 2)—was in effort to block the flow of potential shell shock victims along with malingerers. But there was a counterpart to faking in the field: Faking on the test.

Meehl, who would become an emblematic researcher within the fight against clinical expertise, would also play an important part in developing tools to detect and deflect deceitful test-takers. In contrast to deceit-deterrent psychometrics, so-called subjective techniques like the Rorschach Ink Blot Test were in disciplinary psychology's crosshairs. Meanwhile, Eysenck would continue to work on his personality taxonomy and scales—carrying on testers' work of repositioning pathology as normal traits—but he would also become a major proponent for the

experimental testing of psychoanalysis.

With Popper's falsification in mind, psychology's most effective way of demonstrating the bogus expertise of psychoanalysis was to place it within the classic scientific framework of an experiment: a space of control where, ideally, noise is muted and signal is boosted. While Eysenck would confidently point to experimental evidence discrediting psychoanalysis, Meehl was less sure. This chapter demonstrates how two figures like Eysenck and Meehl—polar opposites in many regards—can still be remembered in tandem as defenders of a scientific version of psychological expertise. In the following and final chapter, the political beliefs that better matched the otherwise-odd couple of Eysenck and Meehl will be explored within the wider context of controversies over hereditary-oriented trait psychology and academic freedom.

Minnesota's Claim to Personality

The Minnesota Multiphasic Personality Inventory (popularly, the MMPI) would distinguish and dominate the newly solidified profession of clinical psychology from the early to post-WWII era. Additionally, alongside the personality scales and taxonomies of trait psychologists like Hans Eysenck, disciplined varieties of neurosis and introversion would persist and ossify in the scales and questions of psychological tests like the MMPI. As will be seen, though the MMPI was designed using a pathologized population and initially intended strictly as a clinical diagnostic tool, its development depended on and grew alongside American forms of normativity. During design phases of the MMPI, the “normal” comparison population harked to other major social scientific endeavours such as that of the Lynd's Middletown Studies in Muncie, Indiana (e.g., Lynd & Lynd, 1929), as well as the enforced normativity of Terman's (and other eugenicists') research. Though not necessarily representative of the average American, the Lynds' reports of Muncie's denizens—or the MMPI's Minnesota normals—

would become an idealized “average” for other Americans to compare themselves: Midwestern, White, Christian, old stock, heteronormative family structures (see Igo, 2008).

The MMPI’s major rival tools of psychological expertise were the already established projective techniques, especially the ubiquitous Rorschach Ink Blot Test. Even though projective techniques were popular among many psychologists, including those working on personality and clinical research, they would soon have to suffer a total loss of credibility in the ongoing policing of psychology’s boundaries. Consistent with their views on psychoanalysis more broadly, some key psychologists like Eysenck were adamantly against projective testing while others like Meehl were (at least initially) more open-minded. In the pursuit of demarcating a scientific form of psychological expertise, the inherently interpretive, projective tests would lose respectability to the so-called “objective” quantitative measures of personality like the MMPI. Yet alongside this success for a scientific psychology were the persistent (and suspiciously psychoanalytic) challenges endemic to self-report questionnaires: from test-takers’ self-deceptions to experimenters’ biases.

The Vision

After the Great Depression, during the New Deal recovery, federal funding would help set the stage for one of the most prevalent psychological tests of the century. In 1936, alongside an expanding psychiatric hospital system, the University of Minnesota opened its hospital (Buchanan, 1994). Within its Psychopathic Unit, staff psychologist Starke Hathaway and psychiatrist James McKinley collaborated to design what became the MMPI. Through Workers Progress Administration funding, a New Deal agency meant to increase employment and promote public works projects, Hathaway began investigating an “objective inventory” for delineating the symptoms and attitudes of normal, neurotic, and psychotic patients (Buchanan,

1994, p. 150).

Hathaway was professor of both Psychology and Neuropsychiatry, so his influence within a psychiatric setting was rather unique for a staff psychologist. Collaborating with McKinley provided access to adult populations with relative ease (Buchanan, 1994, p. 150). His knowledge of physiology was a replacement for his initial academic passion: electrical engineering. Even though Hathaway disliked the psychological tests created in the 1920s and 1930s—setting out to develop the MMPI through his own methods of test construction—he shared the backstory of a failed engineering student with many testers, such as Leon Thurstone and (most importantly) Robert Bernreuter. The Bernreuter Personality Inventory (BPI) was in Hathaway’s crosshairs when targeting the flawed testing-as-usual in disciplinary psychology. Throughout his life, Hathaway would maintain an image of himself as an inventor—perhaps clinging to his initial passion for electrical engineering—and proudly referred the MMPI’s F Scale (one of its validity scales) as his invention (Schilling & Casper, 2015, p. 90)

But before committing to the inchoate role of Medical Psychologist in Minnesota, Hathaway received funding for reconnaissance. He travelled across the country to learn how psychologists fit into medical schools. His tour included the medical schools of the University of Pennsylvania (where Bernreuter spent most of his career), Columbia University (home base for Robert Woodworth and his network of researchers), and Worcester State University, as well as the Vineland School in New Jersey, Bellevue Hospital, and the Boston Psychopathic Hospital (where a young Max Freyd briefly worked before his time at the J. Walter Thompson Company). During his visits, Hathaway met many psychologists who he considered mental testers—technicians working under physicians, many of them women who were “handicapped by the stereotype of the nurse’s role” (Hathaway, 1978, p. 111). There were exceptions: Frederic

Lyman Wells (then at Harvard), Robert Woodworth's former collaborator on a 1911 investigation into association tests, had earned respect from both the medical and psychological departments. As Hathaway put it, Wells was a "powerful, albeit unique, figure" and his book *Mental Tests in Clinical Practice* (Wells, 1927) still seemed the "only practical and relevant one available for medical applications" (Hathaway, 1978, p. 111).

Despite his high esteem for Wells, Hathaway saw a template for Minnesota's nascent project of joining psychology and medicine in a productive manner. At Worcester State Hospital in Massachusetts, Hathaway saw David Shakow and his at work on their schizophrenia research project. It was here that he saw "an environment that was more of a model of what I felt we should have at Minnesota" (Hathaway, 1978, p. 112). Shakow's place in the history of clinical psychology is often as the "architect" of the scientist-practitioner model of training for clinical psychologist. The APA initially appointed Shakow to a committee on developing clinical training in 1946, in response the massive expansion and mobilization of applied psychologists during and after WWII (Benjamin, 2005; see also Capshew, 1999). Two years later, with the encouragement of the Veterans Administration, the VA held a now famous conference in Boulder, Colorado with the intent to solidify a clinical training model (Cautin, 2006, 2008). Alongside an emphasis on scientific research in their training, psychologists' versions of psychotherapy and assessment would eventually come to dominate clinical settings like the VA (Benjamin, 2005; Pickren, 2007). Hathaway and team's MMPI, designed in a research setting modelled after Shakow's pre-Boulder Conference work, fit the expectations of newly professionalized clinicians quite well. Such work was in deliberately stark contrast to the projective techniques of psychoanalysts and psychiatrists, especially the Rorschach.

Disciplining Projection

The Rorschach Ink Blot Test was the first famously used projective test. The Rorschach's set of ink blot cards is a ubiquitous material object that by the mid-to-late twentieth century could be found in the hands of counselors and lawyers, purportedly revealing an indivisible nexus of psychological interiority—aspects others might divide into imagination, memory, ability, or emotion (see Galison, 2004). Getting people to describe what they see in an ambiguous image like an ink blot was not a new idea. Within Germanic culture, ink blot reading was once a parlour game, akin to games of physiognomic face-reading (Gray, 2004). And within psychological testing, Alfred Binet incorporated ink blot interpretation into his research on children that would ultimately lead to Americanized versions of intelligence measurement (Lemov, 2011).

Though there is indeed precedent for using ambiguous images like ink blots in interpretative games and even tests, Herman Rorschach's 1921 *Psychodiagnostik* is generally considered the first formalized system. The box of cards, each precisely covered in seemingly chaotic splashes of amorphous ink, was no longer a mere toy (c.f., Young, 2015). These cards were now being read with the understanding that one's reading was a coded message from the psyche's depths of interiority.

Rorschach was a Swiss psychiatrist who earned his medical degree at the prestigious University of Zurich during the first decade of the twentieth century. Though he never actually worked at the university's associated hospital, the Burghölzli—where Jung first devised his version of word association while assisting the hospital's director Eugen Bleuler—Rorschach attended Jung's lectures and even found an academic advisor in Bleuler. After spending a time treating patients in the isolated hamlet of Herisau, Rorschach suggested that an ideal

psychological test “would somehow combine and supersede the word association test, Freudian free association, and hypnosis” (Searls, 2017, p. 112). Coincidentally, in 1917 he had just briefly met one of Bleuler’s recent graduate students, Szymon Hens. Hens had written his dissertation on ink blots and their various interpretations by schoolchildren, normal adults, and mental patients. Though Rorschach was initially interested in how schizophrenic patients interpreted his cards—and how such interpretations aligned with the normal thought latently schizophrenic—Rorschach also explored how introversion (a notion Jung had already introduced) fit into the perception of his ink blots. Jung’s *Psychologie Typen* was published the same year as Rorschach’s *Psychodiagnostik* (1921), so Rorschach sought to distinguish his meaning of introversion from Jung’s—however exaggerated such a distinction may have been (Searls, 2017, pp. 155–161)

Rorschach’s ink blot technique, though wordless, was harmonious with previously established measurement tools like the Kent-Rosanoff word association test popular among American psychologists. Given his education, and the basic idea that a person’s response is latent with a deeper, unconscious meaning, Rorschach’s ink blots was a distinctly psychoanalytic method (Buchanan, 1997, p. 175). First introduced in the American scene by psychiatrist David Levy in the 1920s, the United States would continue to be a central site of intensive research and promotion on the Rorschach test—through proponents like Columbia University’s Samuel Beck and Bruno Klopfer, the latter a German émigré who fled rising Nazism in 1934. The many Rorschach followers splintered into five competing sects—the growth of factions being another commonality with psychoanalysis—though a popular, purported integration was proposed in the 1970s (Exner, 1974; Lemov, 2011).

Rorschach’s test, along with the general idea of projective testing, gained prominence

even among psychologists. Like E.G. Boring's commitment to analysis, behaviourist B.F. Skinner created a verbal version of projective test early in his career (Rutherford, 2003). In the realm of clinical and personality psychology, Henry Murray and Christina Morgan's Thematic Apperception Test (TAT), a "homegrown variant" of the Rorschach test, is worth noting (Buchanan, 1997, p. 175). Taking over the directorship of Harvard's Psycho-Clinic (formerly held by Morton Prince) in 1935, Murray was a former biochemist who turned his attention to psychology. He and his partner Morgan were affluent Boston Brahmins who, like many of New England's wealthiest families, were enamoured of Carl Jung's individual psychology (Robinson, 1992). Though in their case, Murray and Morgan may have been even more deeply committed to the ideas of an increasingly bohemian and mystical Jung. In the late 1920s, Jung suggested Murray try polygamy instead of divorcing his wife, engaging in a three-way relationship with his wife and his mistress (Morgan). Murray would continue sexual explorations and other markers of Jung's paganist practices of cultural pride from there on (Noll, 1997a, 1997b).

The Rorschach and the TAT were, and remain, the two most salient examples of projective testing. Ink blots would permeate all facets of culture, from the assessment tools of educators or insurance companies to symbols of psychological expertise and inquiry in entertainment. Despite projective testing's initial popularity with psychologists, and their indelible mark on a psychologized public's self-understanding, after WWII the MMPI offered a distinguished professional identity: one rooted in psychiatric categories while eschewing the subjective qualities of interpreting someone's interpretations of art (Buchanan, 1997).

The MMPI was a unique clinical tool that partially came about due to co-creator Starke Hathaway's dissatisfaction with the current contents of the psy-discipline's toolbox. This meant not only a dissatisfaction with projective techniques, but also self-report personality

questionnaires—especially how they were being implemented. As far as Hathaway was concerned, tests like Bernreuter’s Personality Inventory were used in a pointless fashion in clinical settings:

It seemed a little silly during a case conference on a patient in the psychopathic hospital to say that the Bernreuter neurotic scale showed him to be neurotic; for many patients, that would be the first thing they themselves would say. I felt that many psychologists played a sort of game: when a patient was referred to them for testing, they went through a routine of acquiring test data that artificially iterated what the ward nurses easily observed. (Hathaway, 1978, p. 114).

For Hathaway, psychological measures of neurosis and introversion that grew out of the 1920s testing boom held little promise in evening the playing field between physicians/psychiatrists and psychologists. With his unsubtle comparison between the knowledge psychologists and their tests with the common sense of nurses, he was pointing at the secondary and feminized position of menial labour current measurement tools afforded psychologists. Given that projective testing symbolized psychoanalytic expertise, it is no surprise that Hathaway had little tolerance for such tools.

One of Hathaway’s students, Paul Meehl, had initially presented the topic of studying projective techniques for his dissertation. Hathaway convinced him to work on the problem of deception in the MMPI instead. Although Meehl claimed he did not feel pressured to do study the MMPI over projective testing⁷¹, he later recalled his advisor’s deep-seated distrust of all things Freud. Claiming that Hathaway had “a pathological bias against Freud,” Meehl sympathized with one of this biases like causes: a commitment to “Minnesota empiricism, which

⁷¹ UMA, p. 34.

I share.”⁷² Ever the devout Freudian, Meehl suggested a deeper etiology of Hathaway’s pathological bias: sexual Puritanism.⁷³ Whether Meehl’s analysis of his mentor was accurate, it certainly touched on broader, cultural and professional changes in psychoanalysis that occurred during and after WWII.

As psychoanalysis gained steam among American professionals and consumers alike, its core tenets and functional framework morphed with the religious and political politics of Cold War America. As historian of sexuality Dagmar Herzog recently noted, psychoanalysis “has turned out to be only and always iridescent” (Herzog, 2017, p. 15). A resurgence of religiosity mixed with jingoist zeal demanded a psychoanalysis that rejected the political transgression of culture-attuned neo-Freudians and critical theorists, along with the sex-laden theories of Freudianism. In the 1940s, figures like Karl Menninger, Meehl’s introduction to psychoanalysis, helped downplay fractures within psychoanalysis—essentially excluding vocal dissenters like Karen Horney. Shortly after, American psychoanalysis bent to critiques from church leaders and adjusted itself toward a de-sexualized yet sexually normative form (see Herzog, 2017, ch. 1).

In 1944, while working as Hathaway’s teaching assistant at the University Hospital, Meehl took an interest in learning how to administer the TAT more so than the Rorschach. After learning to administer the TAT from a physician-in-the-know, Meehl abandoned the use of any standardized manual. He instead fell back on analysis, treating the material produced from administering the TAT as more fodder to guide his interpretation of the patient—essentially, as more talk. Although Meehl considered his time using TAT as a graduate student on patients as fun, he concluded it was an exercise “wealthy patients could afford” and he held “grave doubts

⁷² Meehl to Peterson, January 7 1999. Printed in Peterson (2005, p. 82).

⁷³ ⁷³ Meehl to Peterson, January 7 1999. In Peterson (2005).

as to just how much the payoff was in terms of incremental validity.”⁷⁴ Once again echoing Boring’s extensive and expensive trials with analysis, Meehl did not completely lose faith in projective techniques after his experience in graduate school. Moving away from the TAT, he still “spent time and money (when I hadn’t much of either early in my career) learning Rorschach with [Columbia’s] Samuel Beck and Bruno Klopfer.” Learning from Hathaway’s blind spots, Meehl wanted to be certain his “Minnesota skepticism” was not biasing his take on the usefulness of projective testing. Looking back on his second voyage into the projective depths, Meehl saw it as wasted time and blamed his concern for being “intellectually fair” to the dubious method.⁷⁵

Normality, Introversion, and Masculinity

The original MMPI was a tool meant to determine degrees of pathologies, treating them as though they were psychological traits. But those pathologies were mapped onto types of pathology, thus joining the usually distinct traditions of assessment (Buchanan, 1994). This furthered the ongoing polysemous norming in psychological testing: abnormality was an extreme normality both theoretically and statistically. As with many testers before them, the team behind the MMPI found it difficult to avoid the consideration of neurosis when distinguishing the most abnormal from less abnormal (McKinley & Hathaway, 1943). In addition to enrolling psychiatric patients, there was also a normal comparison group to help in the design of the MMPI. These “normal Minnesotans” were hospital visitors but were not under the care of a physician. Markers of normality included being married, being between the ages of 26 to 43, having a minimum eighth-grade education, being middle-class, and being at least somewhat religious. The normal test-takers were White and usually of northern European descent (Schilling & Casper, 2015, p.

⁷⁴ UMA, p. 31

⁷⁵ UMA, p. 81.

83).

A selling point of the MMPI to medical professionals was that most of its scales were based on psychiatric nosology, likely of Kraepelinian influence (Buchanan, 1994; Schilling & Casper, 2015). Kraepelin's systematic approach of clinical observation in classifying disease entities still impacts the conceptual map and practice of European and American psychiatry today—though through partial understandings (and re-discoveries) of his varied work (Jablensky, 2007). Nevertheless, the pursuit of psychiatric classification and its physiological underpinnings became much more emblematic of the future practice of psychiatry than psychoanalytic approaches.

Of long interest to physicians and psychiatrists was to distinguish severe, or perhaps genuine, mental illness in patients from the more common forms of psychoneuroses. The MMPI had distinct scales for many psychiatric categories, such as schizophrenia and psychopathy, but it also included more grey-area categories like hysteria and psychasthenia—constellations of symptoms long associated with neurotic problems of emotionality and anxiety. Even in the earliest days of the MMPI's development, when it was intended solely as a tool of an emerging medical or clinical psychology, it quite casually unified psychiatric typology with psychometric trait modeling.

The MMPI's scales of neuroses and other psychopathologies also included a measure of introversion. Scale 0, as it was denoted, was specifically a scale to measure Social Introversion. Initially, work on this scale schematized introversion into three separate forms: thinking, social, and emotional (TSE). The Minnesota TSE Inventory was developed at the University of Minnesota, but within the Department of Educational Psychology and apart from any of Hathaway and McKinley's work on the MMPI. Demonstrating that the TSE scales could

discriminate between groups of college students in expected ways—such as Physical Education students scoring much higher in extroverted thinking than English Majors—researchers assumed the scales were a valid way to diagnose and counsel college students (Evans & McConnell, 1941). Another researcher at the University of Wisconsin, Lewis Drake, expanded on this work and introduced his own scale and scoring key just for Social Introversion (Drake, 1946; Drake & Thiede, 1948). Although Drake intended to do the same for Thinking and Emotional Introversion, his Social Introversion scale alone was incorporated into the MMPI (Buchanan, 1994).

Initially, Drake computed separate norms for males and females in his work on social introversion, but decided to combine those averages as they were very similar (Drake, 1946, p. 53). But the MMPI's suite of scales did not escape the issue of sex differences. Now conceptualized as a trait itself, building on researchers like Lewis Terman, the MMPI would include Scale 5: a measurement of masculinity-femininity (MF). Indeed, several of the questions in Hathaway and McKinley's MF scale were taken directly from Terman and Miles' (1936) Attitude-Interest Analysis Test (Morawski, 1985, p. 209). Even the development of the MMPI's version of measuring gender as a trait likely copied Terman and Miles' procedure.

In creating the new MF scale, the responses of heterosexual men and women were compared to a group of homosexual men (i.e., the abnormal or criterion) in operationally defining gender. As with all other MMPI scales, Scale 5's original use was to identify pathology, such as identifying abnormal expressions of gender like homosexuality (Hathaway, 1956). Hathaway later confessed not being comfortable with grouping masculinity and femininity together; he once desired a separate FM (femininity) scale strictly for use on women (Hathaway, 1956, p. 110; Martin & Finn, 2010, p. 64). In the inventory's eventual major revision, the MMPI-

2, Scale 5 was retained but two separate gender scales were added GM and GF (Butcher et al., 1989)—seemingly clarifying nothing. The MMPI's MF scale was a popular tool for assessing gender for many decades to come, and the overall inventory relied on gendered norms all its scales. In the wake of the 1991 Federal Civil Rights Act outlawing any consideration of race, religion, or gender in hiring practices, this should have posed a problem to employers using the MMPI. But even recent researchers, who concede that the inclusion of gendered norms has little effect on most of the MMPI-2's scales—echoing Lewis Drake's abandonment of them for his Social Introversion scale in the 1940s—cannot excise gender: “it seems advisable for most test users to continue to refer to the traditional gender norms” (Martin & Finn, 2010, p. 80).

Even though Hathaway and McKinley saw the MMPI as a prototypical tool, one that needed to be adjusted or even replaced with improvements, the inventory's entry into the wider market of test-givers hindered this ideal (Buchanan, 1994). Once the Psychological Corporation began publishing the MMPI in 1947, its promise as a valid, widely applicable, and clinically useful tool held much appeal. By 1966, the Psychological Corporation was distributing about 500,000 copies of the inventory, along with its test sheets and interpretative guides, per year (Schilling & Casper, p. 90). The MMPI and its variants continued its wide success even into the 1980s, during the rise of computerization in taking tests, scoring them, and even aiding in clinical interpretation. The MMPI became one of software companies' most sought-after psychological tests, likely due to its expansion well beyond a diagnostic tool in the clinic into a personality test for personnel selection and screening in various companies starting in the 1950s (Lussier, forthcoming).

During the early dissemination of the MMPI, postwar American culture had been grappling with issues of privacy and freedom. While many citizens feared the authoritarian

statecraft of the USSR and its infiltration into locales symbolic of American life, like the suburb, one of the main ways to combat that control came at the cost of personal privacy. Worries about the invasion of privacy on the part of authorities—such as police wiretapping—expanded not only into domestic spaces, but also psychological interiors. With the popularity of psychoanalysis, there was long an impulse towards a deeper knowledge of one's own interiority; now there was also an emerging suburban and corporate norm of legible, conforming citizens (Igo, 2018, p. 105). But such a state of affairs was met with critics. For example, in the realm of marketing research on consumer habits, the Freudian-soaked “depth interview” and other ways of exploring (or manipulating) a consumer's unconsciousness was met with popular criticisms like *The Hidden Persuaders* (1957). Psychological experts were likewise met with suspicion.

Although psychological testing in various forms had been proliferating in social institutions of American culture for many decades, the expansion of all things “psy” after WWII was quite salient to citizens worried about mental invasion. In the 1960s, unlike in previous years, moral outrage and protests of invasive psychological tests resonated with both conservative and liberal groups. In 1960 alone, an estimated 130,000,000 psychological tests were administered on students (Igo, 2018, pp. 130-3). Concerned parents and citizen groups were especially worried about tests that did not seem to measure scholastic abilities, but invaded more private aspects of the psyche (Buchanan, 2002, p. 288).

Critics were especially vocal against the MMPI within corporate workplaces. As its often pointed out, William H. Whyte's popular *The Organization Man* (1956) included an appendix devoted to cheating personality tests—with his central message being to feign normality, always. Yet the MMPI and newer psychological tests came equipped with validity scales and lie scales, meant to squash any dreams of a test-taker circumventing or distorting her psychological

assessment. In addition to a selling point, test-makers' focus on this technical aspect helped deflect questions of morality in the MMPI's public use. Due to the psychopathological content in several of the MMPI's items, the test was mocked and critiqued as improper and invasive (Igo, 2018). The clamouring over the MMPI even led to a Congressional investigation over psychological testing. But the public outrage over invasive testing was occurring during an ongoing technical debate among psychological experts about response style—an extension of long-held critiques of test-takers malingering and lying.

As Buchanan (2002) explains, debates over method and validity allowed psychologists to address anti-test critiques while sidestepping the political and moral outrage. On the one hand confronting the test-taker as a reactive agent with her own motives was a progressive move of cooperation with the tested public. On the other, citizens (including corporate employees) were now unsure of how to deflect invasive testing that came equipped with less crude and more sophisticated techniques—creating fears of an inscrutable power of experts over others. Despite testing's growing anti-deception features, psychologists continued to struggle with how to deflect, or even define, a test-taker's deception.

Doubt and the Psyche

Before the rapid growth and professionalization of clinical psychology during and after WWII, and the ascendance of the MMPI, clinical researchers and therapists used a variety of tools besides personality inventories and projective tests. As in many other areas of research, word association methods were still a prominent option. One of the earliest tests clinical psychologists used was the Kent-Rosanoff Test (Benjamin, 2005). Created through the collaboration of American psychologist Grace (Helen) Kent and Russian-born psychiatrist Aaron Rosanoff, the 100-word test was initially meant for use on the insane but, as with the MMPI

much later, expanded to the normal population (Kent & Rosanoff, 1910).

Even in an obituary of Kent, 74 years after the test's introduction, David Shakow, noted architect of the Boulder researcher-first model for clinical psychologist, asserted that Rosanoff's name being on the test's title only reflected his rank as Senior Physician rather than his actual involvement (Shakow, 1974). Nevertheless, as will be seen, Meehl would draw on Rosanoff's work when first working on the MMPI's validity scales; scales that were meant to detect and deflect test-taker deception. The continuing sense of resentment psychologists had for psychiatrists and psychoanalysts seemed to underline the tense role of expert physician versus menial labourer in the relationships of psychiatrists and psychologists during much of the twentieth century.

When attempting to control the self in self-report questionnaires, psychologists would implicitly draw from psychiatric and psychoanalytic mechanisms. The dual denial and acceptance of test-takers' contradictory interiority is perhaps one of the most indelible impressions psychoanalysis left on the very methodology of psychological test-makers. The intractable self would persist in both testing critiques and their methodological innovations: From simpler concepts of malingering, honesty, and frankness to more complex mechanisms of self-deception and auto-illusion, then finally to framing deception as a motive or desire toward socialization and conformity—both brimming with Cold War anxieties of losing oneself to the masses.

The Dishonest, and the Honestly Dishonest

In 1926, experienced intelligence tester Mark May and ordained professor of Religious Education Hugh Hartshorne began a series of bibliographies on personality and character testing to help bring some clarity to the rapidly growing industry (May & Hartshorne, 1926). Hartshorne

and May's work initially focused on what would become longstanding methodological concerns in the science of trait psychology: How can we measure interior attitudes and motivations? What exactly are we measuring? Can we trust the subject being tested to not deceive us? Their work on character measurement was part of the Character Education Inquiry that Thorndike oversaw at Columbia's Teachers College, a study of moral growth and education which the Institute of Social and Religious Research ultimately funded from late 1924 until 1929.⁷⁶ During this still early era of personality testing, character and personality were still in the process of being deliberately separated. Nevertheless, questions about one's character, personality, or degree of honesty, held moral implications; it was expected for test-takers to engage in deceit. Hartshorne and May's initial volume detailing their work on measuring character, *Studies in Deceit* (1928), is a clear point of integration within psychological methodology—locking the expected deception (a deflection) on part of the subject with the requisite deception (a counter-deflection) on part of the researcher (see Pettit, 2013, ch. 6)

Into the 1930s, there continued to be research on the honesty of test-takers for various popular personality tests. At San Diego State College, there was an investigation into whether one could fake occupational interests on the Strong Vocational Interest Blank (Steinmetz, 1932).⁷⁷ Meanwhile, on the continent's opposite coast at NYU, another psychologist investigated how "self-interest" would impact scores on Allport's test for Ascendance-Submission (Manzer, 1933). It soon became clear that anonymity, such as not including a signature when administered the Woodworth-Matthews Personal Data Sheet (Olson, 1936), encouraged more honest

⁷⁶ The actual details on funding, committees, and institutional roles of Hartshorne and May were more complicated than I might impress (see the foreword of Hartshorne & May, 1928 for finer details)

⁷⁷ Steinmetz (1932) apparently based his study on a "manuscript" prepared by E. Lowell Kelly for Lewis Terman and Catherine Cox Miles where he investigated test-takers' ability to influence scores on the "Stanford Masculine-Feminine Test" (p. 123). I have not been able to find any such published work.

responses. One psychologist pointed out that insuring “frankness” on the part of the test-taker was not an issue with intelligence testing and seemed unique to personality measurement. Perhaps invoking Freudian metapsychology by accident, he found that test-takers who had conflict between their self-description and their ideals were more likely to deceive—rendering any objective measurement of one’s conflict level necessarily invalid (Spencer, 1938).

Research noting discrepancies between personality measurement scores and observed behaviour also pointed toward dishonesty’s major ramification: invalid self-report questionnaires. One team noted the low correlations between students’ scores on the Bernreuter Personality Inventory and counselors’ observations of those students’ behaviour (Jarvie & Johns, 1938). An early-career Abraham Maslow working in comparative psychology noted a discrepancy in dominance as expressed and dominance as observed (Maslow, 1937). Though still a form of deceit, test-takers who were not in a clinical or military setting were being dishonest about their deficit of pathology rather than faking its presence. For personality testers not pursuing diagnosis, there was a shift in the test-taker’s tactics: from faking bad to faking good.

Many of these studies and issues were covered in Goodwin Watson’s review of “personality and character measurement” near the close of the decade (Watson, 1938). Watson, co-founder of the Society for the Psychological Study of Social Issues (SPSSI), noted two obvious facts about personality measurement: that self-report was the most common method, and that a “‘good’ score on a neurotic inventory may be due to deceptive pretense, and may represent distressing maladjustment” (Watson, 1938, p. 270). From the vantage point of a normal distribution, to fake bad or good were both pathological extremes. Watson went on to make a key distinction between deliberate and accidental fakers. When it comes to describing one’s own personality, a test-taker might simply be unaware and “in all honesty vigorously deny those

failings which they unconsciously resent in themselves” (Watson, 1938, p. 270).

Historians have pointed out the expansion of plain dishonesty in psychological subjects into honest dishonesty toward the tail-end of the interwar years. The shift can be understood as an improvement in technique of handling reactive test-takers (Zickar & Gibby, 2006), coupled with increasing theorization on response styles and biases (e.g., Buchanan, 2002). Placing this development in the wider history of deception as both the subject and method of psychological inquiry, Pettit (2013a) focuses on the deceitful self blossoming into the deceivable self. Nearly a decade after Goodwin Watson’s review, prominent clinical psychologist Albert Ellis—famed for his Rational Emotive Therapy technique though at this time still practicing psychoanalysis—offered his own opinions on the state of personality measurement. Although the “Woodworth-Thurstone-Bernreuter type of questionnaires” had long been met with strong criticisms, he saw that they maintained popularity among Americans (Ellis, 1946, p. 388). Restating Watson’s demarcation of the faker from the honestly dishonest—or the deceitful self from the deceivable self—Ellis saw obtaining “a high degree of accuracy and truthfulness” as a basic goal and obstacle for personality questionnaires (Ellis, 1946, p. 414).

Of course, in Ellis’ (1946) review of personality questionnaires he was most interested in their clinical validation: Can they delineate the neurotics from not; introverts from extroverts; or submissive persons from dominant persons? Though this was the promise of Woodworth, Laird, Bernreuter, and other test-makers, there was a body of research highlighting the inability of questionnaires to make the basic distinction between normal and not. For example, Carney Landis at Columbia University would twice examine the validity of the BPI—including its neuroticism and introversion sub-scales (Landis & Katz, 1934; Landis et al., 1935). With the disappointing results of personality questionnaires’ ability to differentiate normal from not in

hand, Landis thought attention in measurement design ought to be turned from validation methods like internal consistency toward empirical examination of symptoms and behaviour.

Around that same time Landis received funding from the Committee for Research in Problems of Sex (CRPS), the funding group which Terman's testing colleague Robert Yerkes chaired, to study the presumably "untouched" sexuality of physically disabled women (Serlin, 2012).

In the earliest days of developing the MMPI, Hathaway also recognized the limitations of personality inventories like the Humm-Wadsworth Temperament Scale (HWTS) and the BPI (Hathaway, 1939). In shifting focus away from the scale's construction via its item content and toward the testimonies of patients and the response patterns of an agentic test-taker, the MMPI had to grapple with the known varieties of mendacity (Buchanan, 2002, pp. 151-3). The MMPI initially had two validity scales: The Lie Scale and the F Scale. The Lie scale was modeled on that work of Hartshorne and May in the late 1920s on studying deceit (Buchanan, 2002; Hathaway & McKinley, 1940, p. 251). It was meant to detect when someone was presenting themselves favourably—faking good—whereas the F scale was meant to detect atypical response patterns that suggested either illiteracy or a plain unwillingness to cooperate. As a former student of electrical engineering, Hathaway preferred to view himself as an inventor of technology; the F Scale being an invention in which he took particular pride (Schilling & Casper, 2015, p. 90). Conversely, the K Scale grew out of an advisor-student collaboration between Hathaway and his eclectic student Paul Meehl.

Though Meehl's initial idea for a dissertation topic was an investigation of projective technique, such as the Rorschach or Henry Murray's Thematic Apperception Test, Hathaway persuaded him to tackle something more tenable. Instead of an expectation that Meehl would work on the MMPI for his dissertation, the work came out of his shared interest in "false

positives” on the MMPI.⁷⁸ Looking to the HWTS for inspiration, Meehl began theorizing how self-regulation might fit into responses on the MMPI. The HWTS was grounded in psychiatrist Aaron Rosanoff’s temperament theory, which Humm was familiar with because Rosanoff was his doctoral supervisor at the University of Southern California (Lussier, 2018a, p. 82). In Rosanoff’s theory, temperament was divided into seven components, one of which was the Normal component (Rosanoff, 1921). Normality was a functionally foundational component within human temperament, as it integrated and balanced the other components—such as stabilizing emotions.

Meehl set off creating his normality scale, or N scale, by analyzing all the MMPI’s items on psychiatric patients matched with profiles of the “Minnesotan normals” previously used in developing the inventory. In light of the results, Meehl decided he was not measuring anything like a normalizing or stabilizing mechanism of the psyche; rather, he was developing a measure for “test-taking attitude.”⁷⁹ Switching his framework from Rosanoff’s normalization component to Paul Horst’s (1941) idea of suppressor variables, he continued to work on the measurement of test-taking attitude. After further research and refinement, including the use of factor analysis, Meehl and his advisor named their phenomenon as simply “K”—at once a statistical factor, a suppressor variable, and the name of its associated scale for measurement (Meehl & Hathaway, 1946).

Although Meehl used factor analysis in this case, he never held the method in any high regard as a means of discovery. Having only ever used it throughout his career a handful of times as a way to reduce the number of items when constructing scales, Meehl was “readily squelched

⁷⁸ UMA p. 34.

⁷⁹ UMA p. 34.

by ‘experts’ who swear by it” as a way to infer the existence of latent variables.⁸⁰ Meehl’s irreverence contrasted sharply with Eysenck’s faith in the multivariate method foundational to the psychometric trait research enterprise. After updating his Maudsley Personality Questionnaire to the Eysenck Personality Inventory, a hidden scale was included for scoring the inventory: the Lie Scale (Eysenck, 1947; Williams, 1969). Like Neuroticism and Extraversion, Lying was (re-)discovered through factor analysis and would long persist into further updates of his personality inventory as well as research on dealing with deception in test-taking.

In the same year as his first major book was published, Eysenck wrote in *The Eugenics Review* that one of the main impediments to psychology being taken seriously—and therefore being able to widely use valid psychological assessment in pursuit of Galton’s eugenics utopia Kantsaywhere—was a “mushrooming of pseudo-scientific, ‘intuitive,’ subjective, literary ‘depth’ psychologies” that had stolen the public’s attention away from scientific psychology (Eysenck, 1947, p. 103). Unsurprisingly, unlike Meehl’s K Factor, Eysenck’s Lying was strictly traditional and eschewed the possibility of any psychoanalytic interiority or “depth.” The test-taker’s deception was intentional, surface-level dishonesty.

Reflecting his persistent interest in finding harmony between psychology and psychoanalysis, Meehl eventually understood the true nature of his K factor as lying between test-taking attitude on the surface and psychodynamic machinations of repression and denial underneath.⁸¹ Even in his paper with Hathaway introducing the K Factor (as opposed to his earlier reported N Scale), he takes care to include not only personality tests’ susceptibility to faking and lying, but also their “even greater susceptibility to unconscious self deception” (Meehl & Hathaway, 1946, p. 525). They argued that MMPI’s initial validity scales, F and L—

⁸⁰ Meehl to Peterson, December 11, 1999. In Peterson (2005, p. 138).

⁸¹ UMA p. 35.

which were based on earlier techniques of combating dishonest test-takers—were generally effective at detecting “extreme distortion” in responses (p. 561). But it was clear to them that the K factor was more sensitive to subtle distortions, including unconscious processes.

Self-Deception and the Psychodynamics of the Repressed

Although there are mentions of unconscious processes, Meehl and Hathaway (1946) do not mention any psychiatric or psychoanalytic work, such as the works of Aaron Rosanoff. Yet they do briefly mention a crucial research study by Else Frenkel-Brunswik on self-report ratings: “Mechanisms of Self-Deception” (Frenkel-Brunswik, 1939). Published in a SPSSI bulletin of the *Journal of Social Psychology*, she reported on “experimental investigations of illusions about oneself” (p. 409). While still at the University of Vienna, before relocating to University of California in Berkeley, she began her research on forty doctoral students, seeking statements about themselves while also observing their behaviour. Four judges who worked in different areas of the university, such as its library, and were familiar with the chosen students were asked to give general descriptions of each student.

Frenkel-Brunswik’s hope was to uncover whether a statement about oneself was trustworthy, or if it had “symptomatic value of a functional status quite contrary or otherwise related to its overt meaning” (1939, p. 420). In other words, her question was whether remarks about one’s own personality could be read into for deeper (and possibly contradictory) meaning. Among one of the more amusing findings was a likely unexpected obstacle: a student without a personality. In perhaps a case of being too honest with a psychologist, judges described an unfortunate student as “not very much of a personality ... lacking personal style ... so colorless [and] not a personality” (p. 410). These assessments were certainly indicative of the use of the word “personality” in a more colloquial fashion, likely tied to talk of someone being with or

without character. Nevertheless, the notion of personality being a single trait (or factor) along which persons can be graded and compared, much like unitary theories of intelligence, would become a (rather preposterous) topic of research in the twenty-first century.⁸²

Frenkel-Brunswik (1939) found striking discrepancies between subjects' and judges' reports of behaviour, especially when being asked about "shortcomings of the subjects" (p. 411). She claimed to have observed a series of "defense mechanisms" that stopped aspects of one's personality from entering conscious thought. The crudest mechanism was "distortion into the opposite," an "auto-illusion" especially prevalent for socially negative traits (pp. 411-2). More subtle defense mechanisms included omission, justification, and varied tactics of camouflaging and evasion. Such mechanisms of self-deception, like distortion into the opposite, was much more common among neurotic students. While a degree of incorrectly reporting one's own behaviour seemed a normal occurrence—these students had an average of 27%—there was certainly an upper extreme. In their most extreme case, a psychiatrist diagnosed a student whose behaviour was reported more than 50% incorrectly as having dementia praecox. As Frenkel-Brunswik (1939) opined: "Lack of insight into one's own personality reaching such a high degree must indeed be considered pathological" (p. 414).

Whether consciously or not, psychological subjects misrepresenting themselves in self-report techniques of personality measurement was not subsiding. Meehl's contributions to the MMPI would influence the next iteration of detecting deception in test-takers: social desirability. Initially, this line of research would be pursued at surface level, much like Meehl's K Factor purported to capture behavioural patterns indicating test-taking attitude. But once these ideas entered the arena of the social psychology experiment in the 1960s, questions of motive and

⁸² Race psychologist J. Philippe Rushton would be the main proponent of the General Factor of Personality (see the concluding section of Chapter 5 of this dissertation).

desire (re-)emerged. It was difficult for even experimental psychologists to avoid inferences about the possible psychodynamics at play beneath the observed patterns of test-item responses.

By the mid-1950s, the MMPI had garnered much attention from researchers interested in personality and clinical assessment. Several had pointed out how issues of personal or social desirability could be affecting how test-takers—or at least college students, their main sampling demographic—respond to the inventory’s various scales. Building on previous research on deceit and deceivability, psychologists noted how test-takers were claiming to have characteristics desirable in the eyes of others, such as being extraverted and generally behaving in a magnanimous manner. The opposite was true for traits assumed to be seen as undesirable to others, such as being neurotic or pathological in any way (e.g., Fordyce, 1956; Rosen, 1956a, 1956b).

By the late 1950s, with Cold War anxieties over conformity at a high-water mark, plainer interwar notions of frankness and complex inward-facing notions of self-deception were receding in favour of a newer concept: social desirability. Allen Edwards’ work on social desirability crystallized and further popularized the topic. Though Edwards is far from a well-remembered psychologist, the popularity of his scales, especially his Social Desirability scale, has ensured his place as one of the discipline’s most highly cited psychologists of all time (Haggbloom et al., 2002). The seed for his work on social desirability was planted in 1951, when collaborating with Leon Thurstone on psychometric work examining comparative judgement and response scaling (Edwards & Thurstone, 1952). While working on this project, Edwards examined numerous personality inventories “designed to measure such traits as neuroticism, emotional stability, inferiority feelings, emotional maturity, neurasthenia, anxiety neurosis, hysteria, paranoia, hypnomania, depression, and the like” (Edwards, 1957, p. vii). Swimming in

a sea of pathologies, Edwards justly wondered about tools that could measure what he thought of as normal personality variables.

With the idea of measuring normal personality traits in mind, Edwards quickly saw the contaminating effect of social desirability. Even though a statement or question in a personality could be designed to measure a variety of traits, it was possible and crucial to “describe each one in terms of its position on the social desirability continuum” (Edwards, 1957, p. 3). Using items from the MMPI’s validity scales, he set to work on designing a distinct measure for Social Desirability. In this way, Edwards’ work folded earlier psychometric traditions of focusing on the face validity of an item’s content with newer traditions of focusing on response patterns or style. All test items fell somewhere along a continuum of social desirability; all test-takers had an individual tendency of giving socially desirable answers to items. In Edwards’ view, once the desirability of items has been determined, individual differences in tendency toward giving socially desirable answers could be mapped (Edwards, 1957, p. 28). The logic behind this conceptualization was subtle but crucial: an item’s inherent desirability held constant across test-takers—only a person’s tendency to respond desirably varied.

Edwards pursued the measurement of normal personality, designing the once popular Edwards Personal Preference Schedule. Basing it on the theories of Henry Murray, an American Jungian and creator of the Thematic Apperception Test (TAT), Edwards saw various needs and motives making up the normal personality. All had been created with social desirability in mind, but social desirability itself was not considered a unique motive. Nevertheless, Edwards considered social desirability (or tendency towards it) a measurable trait—an interpretation preferable to psychodynamic inferences. In response to a factor analysis of the MMPI (Kassebaum et al., 1959), in which the research team interpreted their first factor as “ego-

strength vs. ego-weakness,” Edwards remarked that while such a label “may be more satisfying from the viewpoint of personality dynamics . . . we believe it more reasonable to interpret the factor as ‘Social Desirability vs. Social Undesirability’” (Edwards & Heathers, 1962). Likely unknown to Edwards were the (Neo-)Freudian perspectives on the fundamental need to belong or to maintain interpersonal bonds—a body of research social psychologists only much later would seriously organize (e.g., Baumeister & Leary, 1995).

Among psychologists, social desirability seemed to shift test-taker deceit away from psychopathology and, ostensibly, away from unconscious machinations. To want to be fit in with society was a normal, surface-level need. At the same time, there was a sinister Cold War connotation to this natural propensity toward conformity. Throughout the twentieth century “socialization,” as an interdisciplinary concept, often had psychoanalytic dynamics built into its theorizations. Moving away from a socially determined self, an emerging cognitive psychology of the 1960s suggested a successful form of socialization that could resist totalitarian obedience and enemy mind-control (Morawski & St. Martin, 2011). With both anxious and tranquil notions of socialization in the American psychological imaginary, research on social desirability in personality tests soon merged with research on conformity in a very different testing situation: the social psychology experiment

Also befitting the Cold War era of experimental social psychology were Stanley Milgram’s (in)famous experiments on obedience—reflecting the cultural anxieties over foreign psychological manipulation and the dissolution of traditional American values (e.g., Nicholson, 2011). Within the epistemology of the bunker, a political economic space of permanent war, suspicions over the authentic and suggestive selves bounded the work of psychologists (Lutz, 1997). In the early Cold War era, psychologists were also becoming uneasy and uncertain about

the segmentation of laboratory experimenter and laboratory subject (Morawski, 2015). In this climate of uncertain boundaries between expert and subject—and truth and lies—social psychologists Douglas Crowne and David Marlowe expanded on Edwards’ social desirability work.

The duo created an immensely successful string of experiments exploring what they eventually labelled approval motive. Crowne and Marlowe’s initial work distinguished their scale as separate from Edwards’ and the MMPI—though using many of the same items—because previous measurements had a focus on pathology. Even Edwards’ social desirability scale, in their view, was “in the very restricted sense that high SD scores imply that it is bad or undesirable to have or admit to symptoms [they compare Edwards’ understanding of it to, of course, an understanding of neuroticism]” (Crowne & Marlowe, 1960, pp. 352–353).

Instead, Crowne and Marlowe claimed that their social desirability scale captured something apart from psychopathology—and something located within the test-taker, not the test items. As Crowne later remarked, this early work suggested that test-takers who respond in a “socially desirable manner are more conforming, cautious, and persuasible [*sic*], and their behavior is more normatively anchored ... [they] also have a problem with aggression, tending to repress hostility, and they also engage in self-protective measures to avert anticipated threats to self-esteem” (Crowne, 1983, p. 22). This work would evolve into a long series of varied psychological experiments using multiple methods, including the irrepressible word association test, to investigate their experimental subjects need for approval from others and continue validating their own scale—culminating in their citation-classic, *The Approval Motive: Studies in Evaluative Dependence* (Marlowe & Crowne, 1964).

Crowne contended that his work with Marlowe on the approval motive tapped into a

socially undesirable type of person: “the approval-motivated person should be susceptible to social influence, compliant, and conforming” (Crowne, 1991, p. 18) During a debriefing, a participant in an early experiment—“a nearly complete conformer,” Crowne later noted—enthusiastically explained her conformity as teamwork (Crowne, 1991, p. 18). Rather than more noise to be decontaminated from personality testing, Crowne and Marlowe’s work explicitly reframed desire in test responses as itself a personality trait that involved “the use of repressive defenses” (Crowne, 1991, p. 18).

Edwards was impressed with the quantity and variety of their experimental work, but he doubted the entire enterprise. Rather than supporting their hypothesis of an underlying approval motive at play, he did not think their results precluded “the liar hypothesis.” In a review of their monograph, Edwards also suggested experimenter bias was at play (Edwards, 1965). By suggesting that Crowne and Marlowe were simply seeing what they wanted in their results, he continued to deflect the possibility of depth, drive, and motive within the psychological subject. Ironically, though in keeping with psychologists’ concomitant use and dismissal of psychoanalytic ideas when enforcing their discipline’s boundaries, Edwards seemed to be suggesting an interior motive or unconscious bias in Marlowe and Crowne (and all experimental psychologists).

Crowne and Marlowe’s view of social desirability as itself a substantive aspect of personality rather than mere response style would influence even researchers expanding on Eysenck’s factor analytic trait scales early versions of the Five-Factor Model (FFM; McCrae & Costa, 1983). Meanwhile, explicit psychodynamic tools like projective testing would be largely excluded from consideration within the dominant psychometric-trait approach to personality psychology—though the presumed mechanisms of self-deception and problems of expert

interpretation would persist indefinitely in both psychological testing and experimentation.

Demarcating Skeptics

For psychoanalysis, as historian Nathan Hale puts it, the advent of WWII raised the tradition to “a position of precarious prominence in American psychiatry” (Hale, 1995, p. 187). As with WWI and its scourge of shell shock, war neurosis continued to be a debilitating problem for American troops (Herman, 1995). The demonstrable success of psychotherapy helped push psychoanalysis’ prominence within the psychiatric profession, as the clinical method was becoming most often associated with analysis. Despite gaining ground in professional and public acceptance, what psychoanalysis actually meant was still as mutable as ever. As Jung and Adler had famously broken off from Freud’s theories of human psychology, and as a second generation of analysts like Horney and Reich had pushed beyond libidinal psychoanalysis into the culture and politics of life, American psychoanalysis circa WWII was reorganizing its principles. A more socially conservative form of psychoanalysis was coalescing, as seen in the Karl Menninger’s push against culturalist-analysts or the rise of the ego psychologies of David Rapaport and Heinz Hartmann (Herzog, 2017).

Disciplinary psychologists upheld their antagonistic fascination with psychoanalysis. Well into the postwar era, they continued to place psychodynamic concepts and mechanisms under the spectroscopy of experimentation and measurement. Sometimes peaceful relationships between psychologists and psychoanalysts occurred, such as the 1964 monograph by Hungarian-born David Rapaport (a former staff member of Topeka’s Menninger Clinic) and prominent American clinical psychologist David Shakow, *The Influence of Freud on American Psychology*. After reviewing many Freud’s central theories, and psychological tests of those theories, the co-authors concluded that psychologists should continue to study and test Freud’s theories to build

on them, as “Freudian thinking is part of man’s conquest of nature—the understanding of human nature” (Shakow & Rapaport, 1964, p. 202).

Their statement about psychology as a research venue for the pursuit and expansion of psychoanalytic theory resonates with psychologists’ output during the twentieth century. Even Lewis Terman, in a long, telegraphic list of “credos,” opined that “Freud’s concepts, even when their validity has been discounted about 100 per cent, nevertheless, constitute one of the two most important contributions to modern psychology, mental tests being the other (Terman, 1932, p. 330). That psychoanalysis was demonstrably incorrect about human psychology, yet unimpeachably important to the understanding of human psychology, were two incompatible beliefs that preoccupied psychoanalytic believers and (to a paradoxical degree) dissenters alike. When considering human personality, psychoanalysis was as inescapable a mode of thought as eugenics—even for psychologists who truly loathed Freud and continually pointed to study after study falsifying his theories (Hornstein, 1992).

Popper: Psychology’s Favourite Philosopher

Both Eysenck and Meehl engaged with philosopher of science Karl Popper’s views on psychoanalysis when critiquing or defending Freud, respectively. Indeed, Popper (at least in time) and his concept of falsification is common currency among scientists’ understanding of the philosophy underpinning their work. Even recently, among psychologists pushing for methodological reform largely in reaction to an apparent crisis in replication published research findings situate Popper (as well as Meehl) as a guiding light for best research practices (Derksen, 2019, c.f. Flis 2019). Popper was a Viennese scholar who, though originally was associated with the Vienna School of logical positivists or empiricists, would eventually reconfigure scholarship on the criteria of an activity being scientific. Dissatisfied with the works of Wittgenstein and

others on demarcating science from not, Popper transitioned from the problems of logic—such as induction and the truth-value of statement—to a new arena of concern starring the rules of methodology (Gattei, 2009, pp. 26–33).

Pointing out the illogic lying beneath pure empiricism’s way of constructing scientific theories was one of Popper’s most crucial ideas. The problem of induction, originally posed by David Hume in 1739, can be interpreted most radically as “not being entitled to *any degree of confidence whatever* ... in any predictions regarding what we have not observed” (Lange, 2011, p. 43, *their italics*). Considering this problem of logic as both unsolvable and representing the pitfalls of empiricism, Popper would instead radically reframe the issue with a methodological-epistemological suggestion. Worried that previous efforts in logically discerning the scientific character of statements was too harshly eliminating all metaphysics, including natural laws, while mistakenly retaining pseudoscience, Popper eventually developed the epistemological concept of falsification (e.g., Popper, 1935). If a theory was to be scientific, it must be put to a test, especially in unfavourable conditions, with the necessary possibility of failure. Any theories that could only be confirmed and never hold the possibility of being disconfirmed, no matter the conditions or phenomena, were distinguished as pseudoscience.

In addition to progress in the burgeoning field of philosophy of science, personal experience fuelled Popper’s deep concern for a mechanism that could distinguish scientific theory from not. He was a student in interwar Vienna, a place and time he described as “tumultuous” (Popper, 1974, p. 24). Once the Austrian Empire collapsed after the Great War, “Red Vienna” became a hotpot of revolutionary thinking and movements, like many other parts of Europe. At that time, an adolescent Popper was apparently fascinated with a few key ideas: Einstein’s theories of relativity; Marx’s theory of history; and the psychoanalyses of Freud and

Alfred Adler. While Einstein's work would continue to preoccupy Popper's thoughts as he developed his own scholarship, he would quickly become disillusioned with the theories of Marx, Freud, and Adler.

While still a teenager, Popper had begun attending meetings of socialist students—both university and secondary school. In 1919, he was entirely “convinced by their propaganda” and identified as a Communist for a few months (Popper, 1974, p. 25). But then, on the edge of merely seventeen years old, Popper bore witness to a protest gone awry: apparently Communist protesters had instigated a shooting, resulting in the deaths of several socialist and communist workers. Shocked, and later upset that he could have ever accepted Marxism “uncritically,” he became an ardent anti-Marxist and self-described skeptic once he was seventeen—though his hope for a redeemable form of socialism would endure for many years afterwards (Popper, 1974, p. 26).

While Popper considered his experiences with Marxism in interwar Vienna as “one of the main events of my intellectual development” (Popper, 1974, p. 27), he held his disillusionment with psychoanalysis as less personally meaningful. Nevertheless, a similar pattern of fascination with revolutionary ideas followed with umbrage at apparently being duped occurred around same time as his encounter with Marxism. While still a teenager, Popper had actually worked with Adler, helping with children and adolescents in working-class areas of Vienna where the psychoanalyst had established social guidance clinics. In a climax far less dramatic than blood-soaked streets, Adler's unyielding convictions shocked the young Popper. After reporting to Adler about a case Popper viewed as completely unsuited for Adlerian analysis, Adler had no problem analyzing the case in terms of inferiority complexes even before having seen the child in question. When Popper pressed Adler about how he knew it was a correct approach in this

case, Adler simply reassured young Karl to have faith in Adler's clinical expertise (Popper, 1965, pp. 34–5).

During his adolescent years, in the earliest days of a post-imperial Vienna, Popper thus grew to distrust alluring theories—they could lead to life-altering misdiagnosis or life-ending friendly fire. He had begun his journey of considering what makes a theory scientific and what makes a theory pseudoscientific or religious. Popper held that theories like Marx's or Freud's or Adler's were masquerading as science, though they seemed to function more as myths: “they resembled astrology rather than astronomy” (Popper, 1965, p. 34). No matter the situation, believers in such theories could verify their tenets while looking at any situation—any dissidents were simply ignorant to the truth of things “because it was against their class interest [in the case of Marx], or because of their repressions which were still ‘un-analysed’ and crying aloud for treatment [in the case of psychoanalyses]” (Popper, 1965, p. 35). Popper's deep-seated distrust of expertise and preference for omnidirectional skepticism would greatly appeal to philosophy of science student Paul Meehl and echo in Eysenck's positions on what he called “the Freudian Empire.”

Hans Jürgen Eysenck was one of Britain's most popular and controversial psychologists, whose research topics and political attitudes would influence individual differences researchers around the world. Eysenck continually provoked controversy over his research and opinions on a variety of hot-button issues: from touting the scientific plausibility of a relationship between race and inherited intelligence to problematizing the established causal link between tobacco use and lung diseases (Buchanan, 2010). Recently, researchers have pointed to evidence of fraud in Eysenck's research linking personality traits—like neuroticism—to diseases like cancer and heart disease; with scholars noting how the credibility of Eysenck's work was raised and ignored

years before (Marks, 2019; Marks & Buchanan, 2020; Pelosi, 2019). Eysenck, as framed in his memoir *Rebel with a Cause* (1993), saw himself as a lifelong non-conformist who valued the power of scientific inquiry over all questions of politics and ethics.

Like Popper, Eysenck's indefatigable skepticism—or at least his carefully self-curated image as a scientific skeptic—partially stemmed from his experience with extreme (left- and right-wing) politics in Europe. Echoing Popper's youth in Vienna, Eysenck's formative years were in interwar Germany. Eysenck's skepticism would rarely turn inward to the dubiousness of some of his own work. For that, other researchers and historians would have to examine the veracity of his scientific process and bold claims. Following in the footsteps of his advisor Cyril Burt, a British psychometrician with a notorious legacy of using fraudulent data to support his claims on the heritability of intelligence, Eysenck's own legacy is continuing to unravel (Hearnshaw, 1981; Joynson, 1989). In a quest to legitimate psychology as a natural science, and perhaps to draw attention from his own questionable process of political- and monetary-driven inquiry, Eysenck had a long-harboured distrust of psychoanalysis.⁸³ He particularly despised the influence that Freud and his followers had over psychiatry and psychology.

Yet Eysenck owed his major career opportunities, as well as his work on personality, to psychiatry and clinical work more broadly. During WWII, when Aubrey Lewis, director of London's Maudsley hospital—named after famed alienist Henry Maudsley—hired Eysenck, he provided Eysenck a unique status among British psychologists. Eysenck, like Phillip Vernon and John Raven, would be one of the few psychologists to work within psychiatry and have access to an adult population instead of being limited to children as psychologists were most often with educational research (Buchanan, 2010, p. 78). By and large Eysenck's available adult population

⁸³ I refer readers to Chapter 6 of Buchanan's Eysenck biography (2010) for greater details on this issue. His is an especially comprehensive and thoughtful overview of Eysenck's lifelong "clinical partisanship."

were psychiatric patients—a feature that would delimit his research, even when investigating the fundamental dimensions of the purportedly normal personality.

Even Eysenck’s first project at Maudsley—technically, one of its satellite hospitals, Mill Hill, during its wartime closure—was placing psychiatric mainstays under scrutiny: suggestibility and its relationship to neurosis. Having decided to use his limited time and resources toward testing (and possibly debunking) the contents of popular psychiatry textbooks that were on hand, Eysenck began an academic programme which many of the psychologists he idolized (such as Clark Hull) would likely admire: the “harvest of accepted [psychiatric and psychoanalytic] wisdom with a view to adjust or undermining it” (Buchanan, 2010, p. 85). After WWII, Lewis tasked Eysenck with constructing a clinical psychology curriculum for the newly established Institute of Psychiatry.

The “Maudsley Clinical Course” would educate the first generation of formal British psychologists-practitioners, and while Eysenck did not teach or practice, he helped choose staff and influenced the programme’s theoretical and professional commitments (Buchanan, 2010, p. 181). A crucial work in his career-long campaign against psychoanalysis was his article “The effects of psychotherapy: An evaluation” (1952), a crucial critique of therapy’s effectiveness—especially psychodynamic varieties—and an article that helped foster a zest (especially in the USA) for evidence-based forms of therapy (Buchanan, 2010, p. 195; c.f., Meehl, 1954).⁸⁴

Though Eysenck would be a major player in the pursuit for a mathematically derived universal taxonomy of human personality, this was not sufficient for his vision of a scientific psychology. Taken with the experimental tradition of Russian physiology, especially the work (or at least scientific image) of Ivan Pavlov, Eysenck sought causal models rooted in biology—

⁸⁴ This is to say nothing of Eysenck’s initial impact on and campaign for behaviour therapy—and his eventually awkward relationship with it. Again, see Buchanan (2010, ch. 6)

implying an incontestable, physical reality for statistical factors interpreted as personality (super-)traits. His research program was unlike many others in Britain before WWII, as both behaviourist psychology (including Pavlovian work) and physiological research had very little influence over British psychology; as ever, Eysenck was drawn to the unpopular (Buchanan, 2010, pp. 122–123).

Building on concepts of inhibition found in Pavlov's work (especially on experimentally-induced neurosis in canines) but also American neo-behaviourist Clark Hull's mathematically obsessed work, Eysenck's initial output on the biology of personality was overly ambitious work on cortical processes and its relation to psychopathology (Eysenck, 1955; Eysenck, 1957; see also Smith, 1992). Through the hard work of the talented students he often depended on to maintain his publication levels, including Jeffery Gray, Eysenck's biological explanations for his personality models would be expanded and guide the work of a small cluster of biological personality researchers for the rest of the century—a network to which will be briefly returned later in this chapter during a discussion of trait psychology's funding sources. Currently, some psychologists not entrenched in the traditions of the psychophysiology of personality are much less accepting of the persistent theories of the neurological substrates beneath psychological traits (e.g., Yarkoni, 2015)

Eysenck's obsession with burying Freud for good (alongside psychoanalysis more generally) would prove one of the most continuous threads of his lengthy and prolific career. In typical fashion, Eysenck was so combative with psychoanalysis that even during the 1980s—a time that he and others saw as the beginning of psychoanalysis losing grip on the psychiatric profession—he posited Freud's legacy as the main hindrance to developing a human science. Rather than the psy-disciplines growing into sciences of normal and abnormal behaviour in the

image of the natural science, Eysenck thought it likely that “Freud has set back the study of these disciplines by something like fifty years or more” (Eysenck, 1985, p. 202). For Eysenck, the past half-century or more of a culture and its disciplines saturated with psychoanalytic thought was a travesty—a regrettable detour within the history of science. Eysenck completely rejected the defense of psychoanalysis as a hermeneutic tradition that works alongside the sciences and the humanities. Likening Freud to Marx, he saw the psychoanalysis-as-hermeneutics argument as a corruption of Freud’s original intent of creating a science—just as Marx’s followers all apparently misunderstood his own intentions (Eysenck, 1985, p. 10).

Here Eysenck’s opinions on psychoanalysis diverge considerably from Popper’s positions.⁸⁵ For one, Eysenck thought Popper was incorrect in arguing that psychoanalysis, Marxism, and astrology were untestable theories. His reasoning was tautological but likely acceptable to a self-fashioned, hard-nosed empiricist: Popper was “mistaken” because psychology has continually put psychoanalysis to the test; it failed (Eysenck, 1985, p. 14). In fact, Popper believed the political predictions of Marx and his followers were testable but already falsified enough to be discarded. The real problem was the resistance to effective falsification through endless re-interpretation of Marxist theory (Popper, 1965, p. 37). On the other hand, Popper held both Freud and Adler’s as “a different class” of pseudoscience: Psychoanalysis was genuinely untestable and irrefutable as “there was no conceivable human behavior which could contradict [the theory]” (Popper, 1965, p. 37). We can demarcate Eysenck (and disciplinary psychology) from Popper on this point of psychoanalysis’ testability.

⁸⁵ Eysenck (1985) cites Adolf Grünbaum (though spelling his name incorrectly as “Gruenbaum”) to support his positions against psychoanalysis. Grünbaum was a German-American philosopher of science who worked within an early variant of American logical empiricism. Like Eysenck, he was a critic of psychoanalysis and hermeneutics, but was also a critic of Popper’s philosophy.

Meehl vs. the Psychologists

Eysenck and Popper also differed on the point of psychoanalysis' potential validity. Popper's more nuanced positions on psychoanalysis were much more congruent with the relatively sparse writings Paul Meehl provided on the matter. For such a major component of his personal and clinical life, the usually verbose and prolific Meehl seemed to somewhat shy away from writing about Freud or psychoanalysis except in asides, defenses of his beliefs, or his autobiography. Given the largely negative sentiments toward psychoanalysis within academic psychology, especially among evidence-based clinicians and psychometric-trait psychologists—two of Meehl's major research communities—his relative reticence certainly fits.

Meehl has secured a lasting impact on disciplinary psychology across several research domains. Research citing his major works especially increased in the 1980s and are still commonplace today—though perhaps ritualistic in some cases, such as his landmark paper on construct validity written with Lee Cronbach (Cronbach & Meehl, 1955; see also Cronbach, 1992). A visualized citation analysis of articles citing Meehl's work conveys his multiple areas of influence: research on decision making, clinical judgment, and evidence-based therapies; debates on statistical practice and other methodological concerns; research on measurement validity, including the problems of self-report inventories; as well as various research on the nosology and etiology of clinical categories, especially schizophrenia, psychosis, and allied psychopathologies (see **Figure 4**).

Though the surface topography may suggest a desultory research program, Meehl was consistently concerned with problems of decision-making and interpretation—whether on the parts of scientists, diagnosticians, or everyday people. From his early interests in the philosophy of science scene blossoming at the University of Minnesota to his contributions on construct

validity and critiques of disciplinary research practices, to his development of validity scales for the MMPI and work on clinical expertise, or even to his longstanding interests in schizophrenia and his development of “taxometrics,” Meehl has led many scholarly lives and held many audiences. But his tireless skepticism and Midwestern, no-nonsense attitude was a career constant.

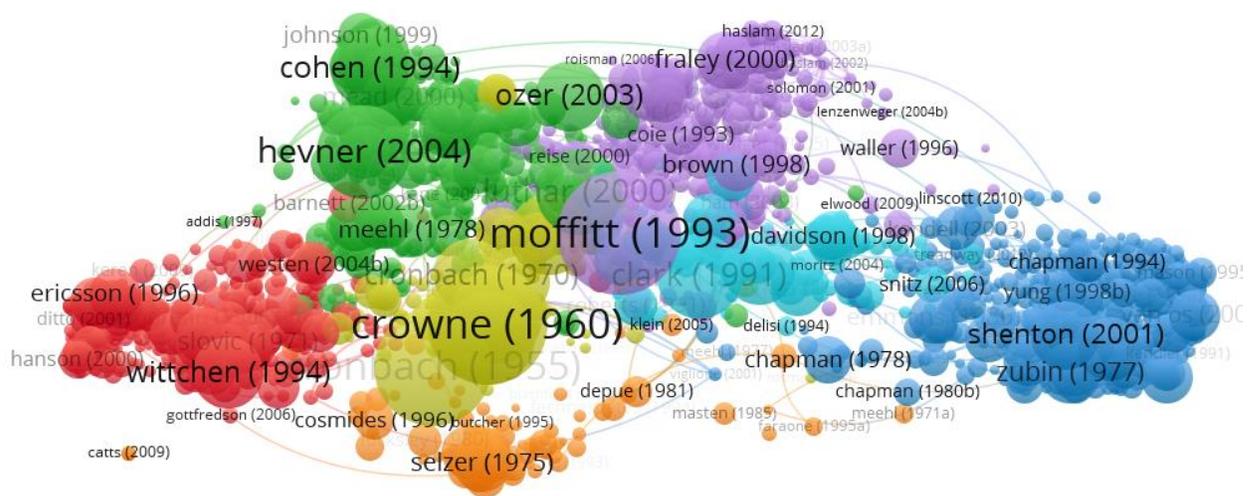


Figure 4. A citation-driven network of 3,185 (originally 10,845) articles citing Meehl’s works. Citation data was downloaded from *Clarivate Analytics Web of Science*. Reference lists were visualized with *VOSviewer* software, which used a bibliographic coupling algorithm to determine similarity of articles’ reference lists and clustered them based on this similarity. The software suggested 7 distinct clusters (which could be interpreted as 7 research communities or a similar categorization). Articles citing the citation-classic Cronbach & Meehl (1955) were intentionally left out of this network and set aside for separate analysis. This research has been presented elsewhere (Davidson & Flis, 2018, August).

Though Meehl and Eysenck may have seemed aligned in their emphasis on skepticism, Meehl bristled at most of Eysenck’s beliefs in methodology and psychology. Toward the end of their careers, Meehl’s long-time colleague and former student Donald Peterson recalled an unsuccessful attempt at replicating the research of one of Eysenck’s students. While working with trait psychologist Raymond Cattell on developing tests of “extraversion,” Peterson procured

the data from Eysenck. Peterson's replicated results were "garbage" and he came to suspect the data as "either concocted or cooked."⁸⁶ Further recalling that Eysenck cited the original research without reservation, and did not mention anything in their correspondence, Peterson "never quite trusted [Eysenck's] work after that."⁸⁷ Meehl surely disliked Eysenck because he "spits on Freud," but he most seriously doubted Eysenck's credibility as a researcher: "I'm suspicious of him. He says dogmatic things about nosology ... He's unscholarly in this respect. It shocks me."⁸⁸

Meehl obviously held different beliefs about psychopathology and methodology than Eysenck, such as the existence of schizophrenia and the purpose of factor analysis. But the pair's diverging reception of psychoanalysis went well beyond what they saw Freud fitting with disciplinary psychology. Meehl's career as a psychologist began in the interwar years; during a time when the disciplinary emphasis of university researchers was on experimentation and behaviourism. Psychoanalysis afforded Meehl a vantage point into the unspoken but presumed depths of the psyche—even while experimental psychologists and behaviourists were working hard to appropriate and discredit psychoanalytic theories (Hornstein, 1992).

Even as defenses of a scientific psychology opposed in principle to psychoanalysis gave way to a behaviourist psychology that ignored while covertly reworking psychoanalytic ideas, many devout scientific psychologists still saw personal value in analysis. Even Harvard's E.G. Boring, who worked to preclude psychology outside of the laboratory as part of the discipline's history and identity in his landmark *History of Experimental Psychology* (1929), went through 168 sessions of analysis over 10 months starting in 1934. That someone so committed to a

⁸⁶ Donald P. Peterson. Letter to Paul E. Meehl. April 24, 1995. Printed in Peterson (2005, p. 23).

⁸⁷ Peterson to Meehl, April 24, 1995, in Peterson (2005).

⁸⁸ Meehl to Peterson. May 3, 1995, in Peterson (2005).

positivist vision for psychology could have “a childlike faith” in the personal transformative powers of psychoanalysis was likely not novel (Hornstein, 1992, p. 257)

In the case of Meehl, a name often associated with the wide worlds of construct validity and evidence-based clinical practices, his commitment to psychoanalysis—especially what he termed its traditional form (meaning Freudian)—is initially surprising. But like many individuals before and after him, including the most ardent of scientific psychologists, Meehl saw therapeutic promise in psychoanalysis. Growing up as self-described, precocious child who voraciously read on any topic, he borrowed a copy of Karl Menninger’s *The Human Mind* (1930) from a friend’s father’s library.⁸⁹ Meehl was just entering his teenage years, and his father recently “drove the car into the garage and left the motor running.”⁹⁰ Shortly after, his mother began experiencing what felt to her like heart attacks, which her physician unhelpfully explained away as a “‘functional’ heart disorder.” With the threat of a second parental loss looming, compounded with an unclear psychiatric connotation to his mother’s symptoms, Meehl recalled that reading Menninger’s popular book on psychodynamics “was a kind of Damascus experience, and it had an immediate and—I must insist to skeptics—a permanent therapeutic effect.”⁹¹

Throughout his career, Meehl would have to acknowledge and justify his faith in Freudian psychoanalysis. Although received and remembered as a book promoting actuarial or statistical processes of clinical diagnosis as more dependable than a clinician’s expertise, Meehl recalled his landmark *Clinical versus Statistical Prediction* (1954) as more even-handed—even

⁸⁹ Information on Meehl’s personal life is largely from a 2015 unedited version of Meehl’s 1989 autobiography (hereafter denoted as UMA). See Meehl (1989) in References for original publication. The unedited version is available at <http://meehl.umn.edu/sites/meehl.dl.umn.edu/files/139autobiography.pdf>

⁹⁰ UMA, pp. 8.

⁹¹ UMA, pp. 8-9.

including “pro-clinical examples.”⁹² Meehl was not seeking to entirely preclude the importance of clinical expertise; rather, he thought those skills were misapplied to predicting clinical outcomes. However each reader took his argument, Meehl’s blended identity certainly helped with maintaining the optics of a fair critique (Buchanan, 1997, pp. 187–188). With his interests in psychoanalysis and the disciplinary culture strongly against it, Meehl confessed experiences “an intense cognitive dissonance and a strong, persistent need to resolve it”—such as in his monograph on diagnosis.

After giving his presidential address to the Midwestern Psychological Association in 1956, in which he promoted “formalizing” clinical interpretation, he was invited to a hotel room for drinks. One group of psychologists commended him for standing up to the clinicians, though once it came to light that Meehl’s recent book had arguments against actuarial method, he could sense the quashing of comradery. Psychoanalysis itself helped frame Meehl’s recollections of meeting resistance to anything hinting of depth psychology among many fellow psychologists. Meehl long remained uncertain whether the disdain such experimental psychologists had for psychoanalysis “is mainly a matter of lack of exposure, or the kind of defensive mechanism that Freud and other analysts have used *ad hominem* from time to time.”⁹³

More importantly, psychoanalysis offered Meehl a framework for much of his personal life and relationships. As with E.G. Boring, who had sunk a massive sum of money into his own analysis, in 1989 Meehl estimated he had spent \$50,000 on his own analysis. Convinced of analysis’ powers of personal growth and also its educational potential for curious clinicians, Meehl valued individual experience over composite statistics when it came to analysis: “there is a psychological difference in the impact between experiencing it on the couch oneself and

⁹² UMA p. 36, see also p. 57.

⁹³ UMA, p. 37.

knowing some statistics.”⁹⁴ His personal convictions would guide his own work as a therapist.

Meehl offered his services as a therapist on the University of Minnesota’s campus. Sometime during the 1958-9 academic year, Meehl began treating Saul Bellow, a star of the literary world.⁹⁵ Bellow, who was there for a one-year position at the university, was having sexual problems with his recently wed wife, Sondra “Sascha” Tschachbasov. Apparently, Meehl (perhaps unhelpfully) suggested he begin treating Sascha as well (Menand, 2015). Although he would ultimately adapt his own therapeutic technique to accommodate more contemporary, cognitive approaches such as Albert Ellis’ Rational Emotive Therapy, Meehl would retain flavours of a psychodynamic therapy (including the ubiquitous signifier of The Couch).⁹⁶ Nevertheless, Meehl still held the “classical” form of therapy as experientially superior—claiming a “slight feeling of haziness or even dizziness on arising from the couch which often follows a classical hour” but did not follow other forms of therapy.⁹⁷

Even in his celebrated article “Theoretical Risks and Tabular Asterisks: Sir Karl, Sir Ronald, and the Slow Progress of Soft Psychology” (1978), Meehl included an addendum defending his “positive assessment of Freud” (p. 829). His former student Thomas Bouchard, now head of the Minnesota Twin Study (more in Chapter 5), apparently confronted Meehl with the incompatibility of advancing Popperian argument for statistical practices in psychology—the linchpin of Meehl’s article—with his belief in psychoanalysis. After revisiting points made elsewhere, such as the actual analysis session being the most suitable site for studying psychoanalysis and that certain facets of the technique were “not presently researchable” (Meehl,

⁹⁴ UMA, p. 70.

⁹⁵ Meehl no doubt took notice of the shared Saul-Paul names of therapist-client. Meehl had previously alluded to “his namesake,” the Disciple Paul (formerly Saul, before his transformative religious experience on the road to Damascus, according to Christian scripture). Meehl also occasionally alluded to his brief allegiance to Lutheranism. (see, for example, UMA).

⁹⁶ UMA, p. 71.

⁹⁷ UMA, p. 72.

1978, p. 830) due to technological and theoretical limitations, Meehl seemed to retreat into a defence of psychoanalysis as an article of faith: “All of us believe a lot of things that we would not have the vaguest idea how to express as a probability value (*pace* strong Bayesians!) or how to compute as an indirect test of statistical significance” (Meehl, 1978, p. 831). Ultimately, Meehl argued that betting on psychoanalysis, and waiting until adequate tests were created, was a “personalistic” prediction. Meehl evaded the question of when and how such adequate tests of psychoanalytic theory would emerge. He believed the previous waves of critiques and experiments were not sufficient.⁹⁸

As with Eysenck, Meehl’s admiration of Popper did not ensure total agreement. Late in life, proving himself the inveterate, Midwestern empiricist he spoke of, Meehl made a list of thirty-six positions he saw in Popper’s works. He only agreed with about a third of them. Among Meehl and Popper’s points of convergence was that science “is often advanced by daring conjectures.”⁹⁹ Relatedly, Meehl firmly disagreed that the theories of Freud and Adler, along with Marxism and Darwinism, were untestable and thus irrefutable. As noted above, Popper’s actual views on the falsifiability of psychoanalysis and Marxism were more nuanced: he considered Marxism as already refuted through tests of history; he considered Freud and Adler’s theories of a separate class of pseudoscience that survives testing through equivocation (Popper, 1965, p. 37).

In fact, Popper expanded further on his view of psychoanalysis, matching Meehl’s “personalistic prediction” of the value of psychoanalysis. With an surprising reverence for something he classified as its own calibre of irrefutable pseudoscience, Popper himself had “no

⁹⁸ See also Meehl to Peterson. February 23, 1999: “In the 1920s, academic psychologists tried to test Freud ‘quantitatively’ by dumb studies ... This shows quantification is not *sufficient*. But I contend it is necessary.” In Peterson (2005, p. 94).

⁹⁹ From Leslie Yonce’s endnote in Waller et al. (2013, pp. 500 - 501).

doubt that much of what [Freud and Adler] say is of considerable importance, and may well play its part one day in psychological science which is testable” (1965, p. 37). Whether a sympathetic skeptic like Meehl, a derisive skeptic like Eysenck, or a disciplinary touchstone of boundary work between scientific enterprises and not like Popper, psychoanalysis was a cultural and professional phenomenon difficult to repress.

But for the sympathetic psychologist like Meehl, working under a graduate supervisor who rejected psychoanalysis out of principle (as many psychologists did, at least outwardly), there were ways to reconcile his self-described cognitive dissonance. Meehl’s first major contribution to disciplinary psychology, stemming from his doctoral research, would be a crucial addition to the professionalizing occupation of clinical psychologists: essential work on the MMPI’s validity scales. These covert scales, intended to detect deceitful test-takers, would set the MMPI apart from both previous surface-only personality inventories like the BPI as well as depth-infused projective tests like the Rorschach Inkblots. Outwardly, validity scales marked an expansion of psychometric technique to deflect all varieties of dishonesty and deflection within a test-taker. Yet tucked below the manifest content of the anti-deception mechanisms of Meehl’s and others lurk the psychodynamics of unconscious self-deception and the need to belong.

Perhaps a way to demarcate a skeptic like Eysenck from a skeptic like Meehl is their degree of reflexivity. Meehl’s omnidirectional skepticism—something he identified as partially rooted in childhood trauma—demanded a turning inward. On most issues, he appeared to maintain a curiosity and was willing to entertain personally meaningful psychoanalytic notions that authority figures within his discipline did not. Meehl’s was a deep reflexivity rarely seen among psychologists committed to a scientific form of psychology (Morawski, 2005). On the other hand, Eysenck held psychoanalysis in low regard for several reasons: one being his

disinterest in deliberate and measured examination of self and method. Quickly moving from one article or book to the next, Eysenck's skepticism was arguably less sincere than Meehl's. Being skeptical was more like a badge of (dis)honour to flash when others complained of his penchant for controversial research topics.

Put to the Test

It seems that for Meehl, when it came to therapy, though psychoanalysis had its drawbacks it was still fundamentally worth believing in. Whereas with personality measurement, both the evidence against and his own experiences with projective techniques was enough to discount psychoanalysis. Maintaining the boundary between psychiatrists and psychologists, and ensuring the preferred tools were on the side of his home discipline, places Meehl in dual position. While he had deep disagreements with Hathaway, Eysenck, and much of personality and clinical psychology about psychoanalysis, he converged with them on the value of psychometrics over projective testing.

Eysenck, in keeping with his derision for how a psychology as unscientific as psychoanalysis had become so pervasive, discredited projective techniques as more of the same. Despite this attitude, Eysenck was often compelled to address projective tests throughout his academic and popular press books. In his first book in 1947, *Dimensions of Personality*, Eysenck sought a standardized format for the Rorschach. Yet by 1955, Eysenck began challenging clinicians he knew to use the Rorschach to discriminate between neurotic and normal patients (Buchanan, 2010, p. 210). This was also the year that the Institute of Psychiatry at Maudsley, Eysenck's place of work, stopped using the Rorschach altogether. Soon afterward he began publicly criticizing the Rorschach (e.g., Eysenck, 1959). Unlike Maudsley, the psychologists and psychiatrists at the Tavistock Institute were associated with a small network of Rorschach

enthusiasts who continued postwar use of the ink blot test. British projective researchers were quite often women researching children and adolescents; their work and lives have also suggested a lesbian- or queer-affirmative legacy (Hubbard, 2017; Hubbard & Hegarty, 2016, pp. 154–156).

Even in his 1993 biography, Eysenck was still contending that a test like the Rorschach was “not the perfect mirror of personality it had been portrayed (Eysenck, 1993, p. 99). Much earlier, in his popular press book *Sense and Nonsense in Psychology* (1958), Eysenck talked about them as “a group of tests which have blossomed during the last twenty years to a surprising extent” (p. 218) and warned the reader of the crucial difference between psychometric and projective tests: “In an objective test there is a correct answer, a right and wrong way of doing things, or at least a numerical measure of success and failure. In the projective test all this vanishes” (p. 219).

Two decades later, in his popular press book *You and Neurosis* (1978), Eysenck reiterated how the Rorschach and the TAT are unreliable and invalid measures. Perhaps most crucial for Eysenck, whose research programme centred on discovering the biological and genetic bases of psychological differences among persons and peoples (more in Chapter 5), projective tests were apparently the only type of personality measure that found no differences between the correlations of mono- and di-zygotic twins. But given their psychometric impression, these tests “cannot be used to prove anything regarding the importance of heredity or environment” (Eysenck, 1977, p. 102).

Personality psychologists who held similar goals and values as Eysenck were also initially charitable to projective tests. Their thinking seemed to be that perhaps such techniques could someday be formatted properly, in the image of a psychometric test. British psychometric-

trait psychologist Raymond B. Cattell, while working at Harvard in the 1940s, wrote a review and critique of the many projective techniques, claims, and some of the mixed findings up to that point (Cattell, 1944). Though he had previously done research on projective tests while in Britain, Cattell likely witnessed expert use of the TAT during his time at Harvard as he was writing from this review from Henry Murray's Psychological Clinic.

Much as Meehl, while a graduate student, quickly abandoned any standardized use of the TAT in favour of using it as another form of free associative talk for analysis, Cattell noticed that techniques like the TAT, or story completion, or musical reveries, had become “nothing more than an excuse for setting rolling a long train of free association and free phantasy which no longer has any connection with interpretation of a situation” (R. B. Cattell, 1944, p. 189). He acknowledged that while psychologists were “naturally interested in exploring all the possible highways and byways of the unconscious,” they first needed to identify and study each unconscious mechanism independently and scientifically (p. 189). Unlike Eysenck, Cattell thought that if projective techniques were improved in various ways, such as clearly hypothesizing and operationalizing the purported phenomenon of interest in each technique, they could have potential as tools. Whereas he claimed objective tests like intelligence measurements had an obvious function, Cattell saw the focus on projective techniques was currently stuck on “the elaboration of the art and ritual of the test” (p. 191).

Psychologists' fascination with projective techniques as route into the unconscious—a fascination that developed into methodological critique—coincided with a broader movement for such techniques. Having also captured the imagination other social scientists, like anthropologists, the use of projective tests on peoples from around the world proliferated in the 1940s to the 1960s. In broadening the usual types of peoples assessed in psychological inquiry,

actors in the projective test movement aspired toward a dense psychological database of all people (Lemov, 2011). As previously mentioned, despite psychologists' psychometric criticisms of reliability and validity, projective tests (especially the Rorschach) would also become commonplace in North American everyday life. The post-WWII proliferation is one of the ways in which the historical trajectory of projective testing mirrored psychometric testing.

Another way in which the twain did meet was their shared struggle for legitimacy in the eyes of disciplinary psychologists. Just as intelligence testing in the American setting was largely implemented to root out the abnormal—such as the feeble-minded threatening the integrity of racial stock—that launched as a tool of social control in the assessment of WWI recruits, Woodworth's Psychoneurotic Inventory developed with draftees and recruits is often pointed to as key origin for the psychometrics of personality. A problem for psychometric intervention on a reactive subject was the multiple deceptions afoot, including basic malingering. Just as psychometric tests incorporated deceit as a feature of their tool—something itself to be measured and controlled, often all within the same personality measurement tool—Klopfer's version of the Rorschach test was used as a tool to detect malingering in WWII. Echoing the Alpha and Beta group tests of intelligence in WWI, group-administrations of ink blots became a tool to suss out confessions of sexual psychopathy in the form of homosexuality among male troops. While the Rorschach shifted from a tool for detecting sexual inversion to a tool for investigating prejudice, the entrenched norms and partial knowledge of heterosexuality persisted (Hegarty, 2003; c.f., Samelson, 1978).

As the MMPI ascended to clinical psychology's preferred method of personality assessment, one that afforded them a scientific legitimacy and clinical expertise distinct from psychiatric and psychoanalytic projective techniques, keepers of the Rorschach took notice.

Through the 1970s work of John Exner, there was a briefly revived professional credibility of the Rorschach. Exner had even previously taken aim at the MMPI's validity scales, producing research that indicated they were useful to detect malingering but not cases of faking good (Exner et al., 1963). The partial comeback of the Rorschach (via Exner's system) hinged on conforming to psychometric virtues of standardization. Yet, in response to pressures of becoming more efficient like psychological inventories, other Rorschach teachers argued for the use of intuition over actuarial technique (Buchanan, 1997). An explicit move toward subjectivity in projective testing proved alarming to devout promoters, like Exner, as it rehashed the central critiques of psychometric personality and clinical psychologists—even ones who waffled in self-admitted and agonizing cognitive dissonance over the problem of expertise, like Paul Meehl.

Chapter 4 Conclusions

In this chapter some of the familiar beats of the history of professionalization in psychology—the interwar into postwar developments of the MMPI and projective techniques—were considered with a focus on expertise. Who can know a personality? Psychologists have always kept this cardinal query in lockstep with answers to other questions about process: How can we know a personality? By what means? For psychologists most devoted to the vision of a scientific psychology in the mold of physics and chemistry, like Eysenck, the question of what constitutes personality was answered through the pursuit of scientific-seeming methods. In the case of Eysenck, he had apparently always dismissed the possibility of a psychoanalytic depth beneath the surface of behaviour—under which swam drives, desires, and defenses in post-hoc predictable chaos. In place of these psychological explanations of personality, Eysenck would mount an enduring and international research project pursuing another kind of depth: the biological structures and their functions that explained individual and group differences in

personality traits on a one-to-one scale.

Yet many other psychologists found it difficult to deny the possible depths of the psyche; this was even the case for behaviourists and psychologists disciplined during behaviourism's ostensible hold on the discipline. Befitting a story of psychoanalysis, psychologists conflicted with and within themselves—hedging, prevaricating, translating, and rationalizing the discomforts of denial. Nowhere is this tension more pronounced than in Paul Meehl's prolific exegeses. While he would downplay his stance, or modify his techniques, or even work towards the undoing of clinical expertise's authority, his core faith in a psychodynamic personhood persisted. Meehl's adjustments coincide with the methodological imperatives of disciplinary psychology, but (as will be shown in the next chapter) his obstinacy on many issues also suggests the latent politics of trait psychology.

From the perspective of demarcation and methodology, psychoanalysis lost credibility among psychologists most concerned with becoming a scientific discipline: a budding trait-focused personality psychology and a newly reformed clinical psychology. Being beholden to psychometrics was quickly becoming a *sine qua non* for these fields. The psychometric tenet held true even through harsh critiques about the accuracy of responses from test-takers. The most prized solutions to the problem of a deceitful test subject was to work within psychometrics and seek improvements. Indicative of psychology's unbreakable tethering to psychoanalytic thought, psychodynamic explanations were often assumed in explaining a test-taker's deceit (especially self-deceit). Whether psychologists like Eysenck or Edwards on the side of lying-without-depth on the one hand, or psychologists like Meehl or Crowne on the side of deep-deceit on the other, the problem of misrepresentation within psychometrics was simply folded into the framework of psychometric-trait psychology. In all cases, whatever the form of deceit, the distortion itself was

rendered as a measurable trait to be either muted or independently explored. The central threat to personality measurement was no longer bug, but a feature of either the tests or the test-takers or both.

Meehl abandoned his interests in projective techniques in favour of working on the MMPI for various possible reasons. One reason is he, like many other psychologists, simply believed in the importance of scientific measurement. Though would always be less inclined to concede experimental findings against psychoanalysis; ones that Eysenck happily conducted, collated, or disseminated throughout his career. While there was room to contest the conduct, meaning, and appropriateness of experimentally investigating the psychodynamic person, this was not the case for psychometrically investigating this form of personhood. Instead of arguing whether psychometric technology could yet adequately put psychoanalysis to the test, as even Popper had said of experimental technology, psychoanalysis now had to be imported into the very conduct of the psychometric method. Now, instead of a simpler appropriative process of disciplining psychoanalytic concepts—like neurosis into neuroticism and introversion into extraversion—the very methodology of this process had to be modified with covert Lie, K, or Social Desirability scales.

Although projective techniques, especially the Rorschach, persisted across the psy-disciplines, other professional institutions, and became markers of psychological expertise in popular culture, Koopman (2019) suggests an intriguing reason as to why a tool like the MMPI is less precariously situated in our systems of information. The very formatting of a psychometric instrument affords it a place within the proliferating informatic technology of the mid-twentieth century and onward. Supplementing and developing alongside traditional forms of power (sovereign, disciplinary, and biopolitical), there is an infopower distributed among the

standardized techniques of information and the persons constituted therein. The infoperson is bound to and delimited by established formats of forms, tests, certificates, and databases—we are tethered to the modes of personhood provided. Whether one agrees with Koopman’s political and historical argument, in perusing the questionnaires, charts, and graphs in Allport’s work (Allport, 1937), he makes an astute observation. While Allport perfunctorily included psychodynamic modes in his graph ways of researching personality, he omitted it from the actual chapter on personality research methods (Koopman, 2019, p. 101). Apparently, depth psychology did not offer the psychologist a worthwhile technique, deeming it unscientific.

Yet it is not simply a matter of technique. Throughout this chapter, and this dissertation so far, a focus has been on the boundary work via methodological. While Gordon Allport no doubt had reservations about the validity of Freudian expertise (Allport, 1968; see also Nicholson, 2002, esp. pp. 68-71 on Allport’s meeting with Freud), he also continually held reservations about the advancing techniques of psychometrics (Allport, 1966). Likewise, Meehl did not hold factor analysis in high regard. For him, it was not a tool of taxonomic trait discovery; it was a tool for reducing statistical clutter. Though expressed differently, what links Allport and Meehl’s hesitance to fully commit to the psychometric framework they helped design and legitimate is their politics of personhood. Whether in Allport’s persistent research into idiographic methods, or in Meehl’s refusal to disavow his Freudianism, there is a central concern: losing the individual.

One aim of connecting Meehl to Eysenck in this chapter was to reconnect the story of clinical psychology with the postwar rise of psychometric trait psychology. The Big Two, conceived as psychological traits (and eventually statistically analysed factors necessitating traits or super-traits) were crucial for both clinicians developing their own tools and personality

psychologists demarcating psychological science from other psy-disciplines. Whereas Meehl might have personally preferred couch analysis for revealing the deeper truths of personality, statistical analysis—merely one in his impressive stable of hobbyhorses—would triumph. Meehl and Eysenck will continue to be key players in the next chapter, as Meehl's political beliefs led him to publicly defend (yet another resurgence of) controversial race science during the 1990s more commonly associated with a disgraced figure like Eysenck.

Chapter 5

Conserving Personality:

Innate Traits Before and After “The End of History”

The December 13, 1994 issue of the *Wall Street Journal* ran an editorial entitled “Mainstream Science on Intelligence” (later reprinted in Gottfredson, 1997). The open letter sought to correct the misinformation on the science of intelligence that had been spreading within the press in light of the recently published and highly controversial book, *The Bell Curve: Intelligence and Class Structure in American Life* (Herrnstein & Murray, 1994). Co-written by Richard Herrnstein, a Harvard psychologist (deceased by the time of publication), and Charles Murray, a conservative policy analyst who had previously written on the threats of the welfare society (Murray, 1984), their tome on American economic inequality included arguments for forms of policy rooted in the argued reality of heritable differences in cognitive ability among races. Drawing evidence from dubious scholars, such as notorious British-Canadian psychologist J. Philippe Rushton—who also had a book on the science of heritable, racial differences out that year (Rushton, 1994a)—public controversy over *The Bell Curve* apparently misrepresented the science of intelligence; or, at least, fifty-two people believed so.

The “Mainstream Science on Intelligence” editorial included a list of fifty-two signatories. Many of them were involved in the transdisciplinary field of behaviour genetics (BG), and several were trait psychologists with a long involvement in the controversy over intelligence and race. Yet other signatories did not seem to have much of a connection to either BG or race science. For example, alongside expected signatories such as Raymond Cattell, Hans Eysenck, Arthur Jensen, Richard Lynn, Robert Travis Osborne, and Rushton, Paul Meehl had

provided his signature in support. Meehl was not the only Minnesotan signatory; his former student Thomas Bouchard—longstanding director of the Minnesota Center for Twin and Adoption Research—also signed in agreement.

Though Meehl’s signature could be interpreted as support for the hereditarian scientific projects of his former student and other colleagues, his signature suggests deeper motives at work. As will be shown, two of Meehl’s major intellectual commitments, hereditarianism and libertarianism, better explain his willingness—indeed, his need—to defend the academic freedom of fellow psychologists under what he perceived as persecution by conceited academics and activists who promoted an egalitarian vision for humanity. While the “Mainstream Science” editorial’s statements on the biological causes of individual or group differences were equivocal enough to seem scientific, other psychologists and scholars understood it as a PR ploy to justify racist science: The statement was “a sociological one about what some experts believe, not about what science proves” (Panofsky, 2014, p. 5). Though Meehl did not likely share some of the more hateful beliefs of his fellow signatories¹⁰⁰, his commitment to a biological basis for psychology (in the case of his work, psychopathology) and his enthusiasm for the contemporary conservative decrying of political correctness run amok, reflected the wider attitudes of personality psychology at the close of the millennium.

As *The Bell Curve* dominated the public perception of psychology in the American press, a new consensus was emerging among personality psychologists: the five-factor model (FFM). Every version of the FFM featured neuroticism (sometimes labelled emotional stability) and extraversion as prominent domains of personality. The FFM would eventually come to dominate psychometric-trait models of personality; it is now commonly used and expanded on across

¹⁰⁰ For example, as shown in Chapter 4, Meehl did not trust and seemed to dislike Eysenck.

psychology's sub-disciplines. In the 1990s, FFM researchers would incorporate a previous controversy into their own version of disciplinary history: the person-situation controversy of the 1970s. While largely a technical debate over the predictive validity of personality measures and the cross-situational consistency of behaviour, the stability of "the person" here also represented traits as heritable and genetically determined. Ironically, this disciplinary skirmish over methodology occurred alongside the political controversies and bitter debates over the genetics of race and intelligence spurred on by Arthur Jensen's research on race, IQ, and educational policy (Jensen, 1969).

As the stability/heritability of intelligence become politically (and, for many, scientifically) controversial, trait psychology would once again find refuge in the stability/heritability of the "non-intellectual" traits: personality. In this final chapter, the taxonomic work of personality psychologists is briefly reviewed, leading into the received history of the FFM. An examination of the FFM historiography, as psychologists have previously portrayed it, will then be supplemented with a look at how FFM ostensibly diverged from the political quagmires of trait psychologists associated with race science—such as Eysenck or Cattell, whose undesirable legacy looms large. The person-situation controversy in personality psychology of the early 1970s will be seen alongside the concurrent controversy over race and intelligence in BG, as they shared both trait psychology's methodology and several of its researchers. Crucially, in casting the FFM's history within the person-situation controversy— isolated from the political maelstrom of intelligence and race—and in pursuing more acceptable lines of research funding, the psychometric approach to personality psychology could carry forth hereditarian presumptions without being associated with the more unsavoury past of trait psychology.

Next, the juncture of hereditarianism and political conservatism among trait psychologists will be examined in a way that will allow for a new understanding of the politics of personality psychology—including shedding light on how a scholar as skeptical and open-minded as Meehl would come to the defense of racist psychologists. In the 1980s and 1990s, hereditarian researchers tapped into the wider conservative-libertarian discourse of academic freedom and political correctness; a version of academic freedom mobilized in the defense of racist science. This chapter's final section includes a close examination how the race psychologist Rushton refracted criticism through the conservative prism of left-wing oppression and academic freedom. Fear of losing academic freedom to oppressive Marxists would reel in many libertarian-leaning scholars, including psychologists, not necessarily devoted to crypto-eugenicist causes. For neoliberal political writers, prominent psychologists among them, a process of historical revisionism echoing the FFM's historiography would persist before and after the collapse of the USSR.

New Branch; Old Tree: Personality Set Apart from Its Past

As psychometric (often meaning factor analytic) researchers began merging with—or co-opting—the foci of other personality researchers, including Gordon Allport's trait psychology, during the interwar years and onward, a new vocation for personality psychologists began to form: that of taxonomists. Through several decades of statistically analyzing personality questionnaires, as well as data on English language descriptors of persons, competing factor/trait models of personality became a central arena of personality expertise. Pioneering this work were psychologists who would continually stoke controversy for their work on topic such as intelligence—like Eysenck—or who would consistently support a neo-eugenicist ideology but such support would not come to light in full until the 1990s—specifically, Raymond Cattell. As

this work ultimately led to the FFM, the zenith of personality taxonomy, younger generations of personality psychologists benefitted from distancing their work from their disciplinary lineage.

This section first briefly overviews some of the main developments in the factor analytic pursuit of a fundamental taxonomy of human personality. The moments and researchers therein are often recounted in internalist histories of personality psychology (especially its lexical approach), and this section connects them with the ultimately successful taxonomy: the FFM. While the FFM has its own internalist history, which will be considered in the next section, this section considers what the FFM research project shares with personality's unsavoury "past." During and after WWII, governmental granting agencies funneled an unprecedented amount of money toward the social sciences pursuing the mold of natural science, including psychology (e.g., Capshew, 1999; Herman, 1995; Solovey, 2004, 2013). Despite these funding sources, many personality psychologists would turn to patrons evoking anti-governmental and anti-regulatory values such as the Pioneer Fund or various tobacco corporations. In the latter case, two key FFM researchers would initially be entangled with the racist legacies and tobacco funding of an earlier generation of personality psychologists. Making the move to larger and more honourable funding sources would be one way of isolating personality from the wider political controversies of trait psychology.

The Big Five: The End of Personality

The 1980 publication of the *Diagnostic and Statistical Manual III* (DSM-III; shortly thereafter DSM-III-R, 1987) nearly expunged the very words "neurosis" and "neurotic" from psychiatric usage. Although allied terms for neurosis lingered in the pages of the DSM-III, they were devoid of their original meaning. Neurosis, once the psychoanalyst's catch-all for various non-psychotic psychopathic syndromes even in the pages of previous DSM editions, implied a

psychodynamic etiology (e.g., Freudian intrapsychic conflict anchored in childhood experiences), a lengthy psychoanalytic treatment, and an extreme outcome of a universalized quality of modern human existence. In the words of psychoanalytic psychiatrist reviewing the recent history of the DSM-III in anticipation of the fourth edition: “With the advent of DSM-III, time has shrunk from a lifetime to a moment, from the extended evaluation to the 45-minute cross-sectional interview” (Wilson, 1993, p. 408).

In 1974, in an effort to revise the DSM-II, the American Psychiatric Association formed a Task Force on Nomenclature and Statistics. Initially the chair of the Task Force, Robert Spitzer, and his colleagues intended to excise neurosis. But when faced with the DSM-III not being approved by the association’s Board of Trustees, the Task Force compromised: inserting the word “neurosis” parenthetically after the word “disorder” (Mayes & Horwitz, 2005, p. 262). But in the spirit of the DSM-III, the word was a mere descriptor devoid of any causal implication for any disorder. The exclusion of psychodynamic etiologies in the new symptom-checklist styled DSM held true for all new categorizations except for one: post-traumatic stress disorder (Herzog, 2017; Young, 1996). The traumatic shock of warfare (now expanded to any traumatic event)—the neurotic syndrome that had initially drawn the apparatus of psychological testing to neurosis—could not be excluded as a causal factor in such cases.

Neuroticism had a very different trajectory within psychology. Although Strand (2011) argues that while psychiatrists moved toward classification while clinical psychologists focused on behaviour, this was not the case for personality psychology. The psychometric technique personality and clinical psychology was increasingly bound up in the pursuit of a universal taxonomy of personality traits. The Big Two were, and are, chief among the proposed cardinal planes of personhood. While neurosis may have taken a dive in common usage from the 1970s

onward (see **Figure 5**), research on the personality trait “neuroticism” only grew in disciplinary psychology (see **Figure 6**). By the 1990s, according to some, personality psychologists had reached a taxonomic consensus: there were five big traits or domains of personality (or, at the very least, consistently recoverable statistical factors). As with other perspectives on personality, some would even begin proposing the FFM as a way to organize and diagnose personality disorders (Costa & McCrae, 1992).

Google Books Ngram Querying “Neurosis”



Figure 5. Google Books Ngram Viewer search for the word “neurosis” in English (2012) language from 1900 to 2010. A marked decline in usage in 1973. Retrieved from https://books.google.com/ngrams/graph?content=neurosis&year_start=1900&year_end=2010&corpus=15&smoothing=3&share=&direct_url=t1%3B%2Cneurosis%3B%2Cc0#t1%3B%2Cneurosis%3B%2Cc0

Though Gordon Allport is often remembered as trait psychology’s progenitor, his particular vision for personality psychology was not carried through to the mainstream of future research. The unsuccessful continuation of Allport’s project is especially true for personality psychologists who, in the vein of intelligence research, adopted factor analytic methods as an integral tool for the discovery and validation of trait models—a methodology Allport would only

consent to, without much enthusiasm, near the end of his life (Allport, 1966). Nevertheless, one of Allport’s projects on the language of personality is a frequently noted touchstone for histories of psychometric-trait psychology.

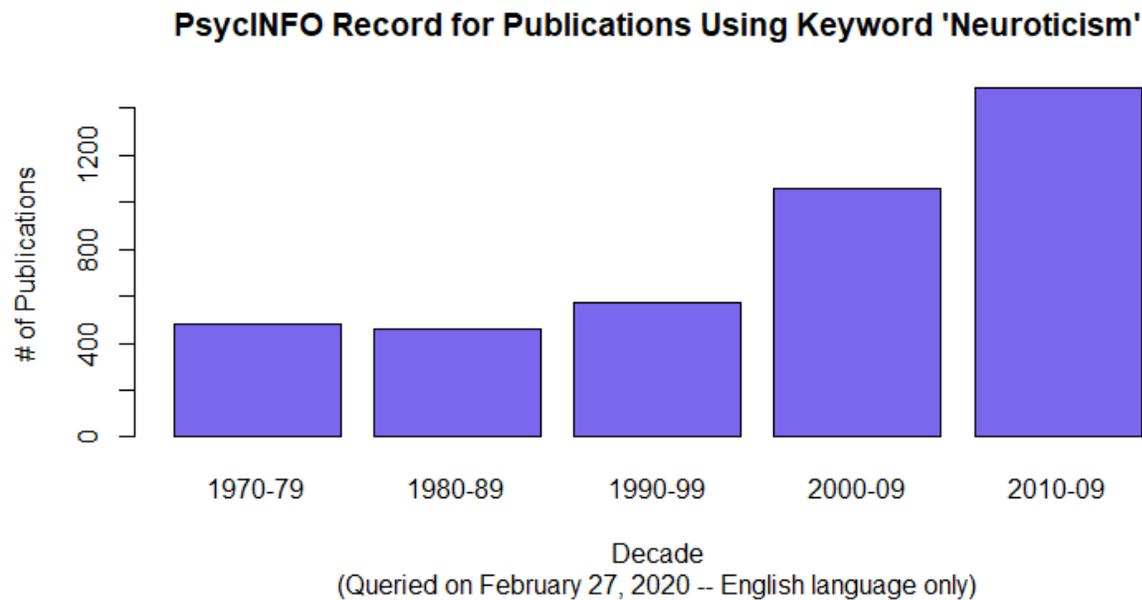


Figure 6. PsychINFO query for keyword “neuroticism” in English language publications from 1970 to 2019. This does not include terms that have long been used alongside or in place of neuroticism, especially “emotional stability.”

Allport’s 1936 monograph written with Henry Odbert on “Trait-Names: A Psycho-lexical Study” stands out as a crucial site of trait taxonomy and methodology progress in both trait psychology’s disciplinary recounting (e.g., John et al., 1988) and classic criticisms alike (e.g., Gould, 1996). Stemming from German characterological work (Baumgarten, 1933)—influenced by the lexical hypotheses of Ludwig Klages—Allport and Odbert gathered nearly 18,000 descriptor terms from a *Webster’s* dictionary to begin the process of winnowing down the main categories of trait descriptions found in the English language. Acknowledging the ontological

slipperiness of pursuing labels-for-people found in written language instead of observing behaviour in the laboratory or clinic, they held no illusion about the partiality of their work as list-makers: “trait-names are symbols socially devised [that cannot be accepted until correspondence] with a true trait has been experimentally or clinically established. Traits cannot be called forth by fiat; they must be discovered” (Allport & Odbert, 1936, p. 20).

A decade later, British-born University of Illinois psychometrician Raymond B. Cattell—a quasi-rival to Eysenck in terms of a distinct trait-taxonomy model and a comparably gargantuan research output—would use the Allport and Odbert adjective set in designing his own model and scale. After much research using reduction techniques, Cattell went from 4,500 trait descriptors to 35 variables—which were factor analyzed into 12 of the factors in his 16 Personality Factors questionnaire (e.g., Cattell, 1944; Cattell et al., 1970; see also John et al., 2008, p. 118).

Meanwhile, Eysenck’s work on personality taxonomy signaled what would become evergreen practices of psychometric-trait personality psychologists from the postwar era to the present day: theorizing personality traits within evolutionary biological and genetic frameworks; valuing factor analytic methods as arbiters of trait taxonomies; and anchoring the research program on the cardinal domains (factor analyzed super-traits) of personality. For Eysenck, his personality inventories would develop from a 2-factor model (Eysenck & Eysenck, 1964) featuring his famed Big Two; then, soon afterward, to a 3-factor model: his PEN (Psychoticism-Extraversion-Neuroticism) Model that now included psychoticism as another domain of normal personality rooted in the clinic (Eysenck & Eysenck, 1975). While the lexical analysis of Allport-via-Cattell and onward would inform a major research tradition in personality taxonomy, both the work of Eysenck and Cattell would serve as the groundwork for the tradition of factor

analyzing scores across questionnaires rather than wordlists—and seeking hereditarian and evolutionary frameworks to put taxonomies to theoretical work.

By the early twenty-first century, the publication trend became clear: research using the FFM or Big Five trait models greatly outweighed the number of publications using previously popular models like Eysenck's or Cattell's (John et al., 2008, p. 116).¹⁰¹ Within the lexical tradition, work growing from Warren Norman's taxonomic research (e.g., Norman, 1963) would lead to a five-factor solution which lexical, Oregon-based Lewis Goldberg would coin as the "Big Five" (Goldberg, 1981, 1990) Alongside the lexical approach, researchers comparing questionnaires were pursuing an integrative pursuit of the ultimate trait taxonomy. The famous FFM research and resulting inventory (the NEO-PI) of Paul Costa and Robert McCrae Though that data was generated using Cattell's 16PF scales, they saw two of the three resulting clusters conforming to Eysenck's Big Two domains. The duo interpreted their final and less immediately identifiable cluster within a Jungian framework of psychological functioning and individuation (Costa & McCrae, 1976, p. 569).

Of course, outside of academic and government-funded research, Jung's legacy had inspired an entire market was growing around one of the most widely recognized, proprietary personality systems in the world: the Myers-Briggs Type Indicator (MBTI). Even FFM researchers, likely acknowledging the cultural cache the MBTI held (and still holds today), went about early on in their FFM research to view it from a "five-factor perspective" (McCrae & Costa, 1989b). They would also incorporate Harvard Henry Murray's Jung-influenced list of needs—which had previously informed other psychometric scales—into an FFM perspective

¹⁰¹ Owing to the rapid growth in FFM research and related personality research, the authors also mentioned how "a comprehensive and detailed review of most of the available research is no longer possible" (p. 116). I deeply sympathize.

(Costa & McCrae, 1988). Based on Jung’s original work on personality types, the MBTI’s sixteen types—and archetypal elaborations thereof—have entered the parlance of many office-workers, managers, life coaches, marketers, and online forum posters alike.¹⁰²

Towards the millennium, the FFM ascended towards a status quo for personality research. Though not without competing models of personality, as well as research traditions that did not conform to the presumed epistemological pre-eminence of quantitative methods, the FFM became a standard structure to which other taxonomic models old and new was compared against. For example, the HEXACO model is a six-dimensional model that mostly agrees and converges with established FFM structures (Ashton & Lee, 2007). Personality psychologists became busy researching questionnaires and adjective-lists in pursuit of validating a finer-grained taxonomy—filling in the hierarchy of personality factors residing above and below the level of the Big Five. The controversies of the past, and the search for personhood, had seemingly ended.

Five-way Personhood & the Optics of Going Straight

The comparative speed and ease of the Big Five consensus in the 1990s, considering traits had a long history in psychology, one mired in both political and methodological controversy. The stability of personal attributes carried deep connotations of the innate, the inherited, and the genetic concepts which resonated with psychology’s earlier entanglement with eugenics. The Pioneer Fund—recall, a private trust established in 1937 originally to promote race betterment and deter “race suicide”—kept alive certain interwar eugenic concerns. Despite its small budget compared to federal government grants, the Pioneer Fund served as a crucial patron and social network for a branch of biologically oriented trait psychologists well into the

¹⁰² For the Myers & Briggs Foundation’s official summary of the measurement tool and their sixteen-type structure, see <https://www.myersbriggs.org/my-mbti-personality-type/mbti-basics/>

postwar era. Its money continued to fund and guide hereditarian psychological research throughout the Cold War and afterwards—whether the work of Cattell, Eysenck, Rushton, or Bouchard’s major Minnesota Twins project.

However, the hard-fought victories of the 1950s Civil Rights movement did much to revive organized, scientific racists in the United States and Europe. In 1960, the recently formed and boldly named International Association for the Advancement of Ethnology and Eugenics (IAAEE) established a journal to promote their view, *The Mankind Quarterly* (MQ). Simply put, the association and its allied groups are a prime instance of misusing human science to promote neo-fascist ideology (Winston, 1998). MQ would publish the opinions of segregationist spokesman and eminent Columbia University psychologist Henry E. Garrett. Seeing the scientific facticity of the genetic inferiority of black Americans under scrutiny, Garrett was compelled to point out the “equalitarian dogma” of social scientists allied with social reformers and humanitarians (Tucker, 1996, pp. 153–157).¹⁰³

Stanley Porteus (the Hawaii-based colleague of Terman and early mentor of Bernreuter) sat on the Executive Committee of the IAAEE, alongside a cadre of notorious eugenicists and like-minded academics, such as Robert Kuttner, Garrett, and Robert Osborne. In the back pages of the MQ’s first volume, the IAAEE’s State of Aims and Objectives was included. Among their aims were “encouraging a free flow of information between scholars [across various disciplines] and in restoring freedom of inquiry to those areas (particularly the study of race and race relations) where extraneous political and philosophical predispositions have frequently terminated discussions to the general detriment of the social and biological sciences.”¹⁰⁴ MQ served as a perfect vehicle for someone like Porteus to once again set his sights on racial

¹⁰³ Garret’s “Equalitarian Dogma” would quickly be republished to wider audience (Garrett, 1961).

¹⁰⁴ Taken from one of the copies of *Mankind Quarterly* held in the SPP.

differences and the study of aboriginal peoples

Modulating his terminology, as he had done before when sliding from ‘intelligence’ to ‘mental performance’ in his 1930s (Porteus et al., 1930), Porteus published an MQ piece entitled “Ethnic Group Differences.” In this piece, Porteus bemoaned how Nazi Germany’s massive extermination campaign based on their “untenable” views of eugenics long suppressed the more scrupulous scientist who believed in the existence of racial differences. Porteus joyously wrote about the eugenics and racial research renaissance he observed. In a call-to-pens for eugenics’ apparent return from cultural hibernation, Porteus assured readers that because “Now that the emotional nausea [of the Nazi’s atrocities] has subsided, a new determination to sift available evidence on this controversial subject has arisen. Scientists should welcome this tendency, particularly in relation to evaluating proofs of certain mental and temperamental traits of importance to human adjustment” (Porteus, 1961, p. 187).

Notoriously, Stanford University physicist William Shockley—co-winner of the Nobel prize for inventing the transistor—fully supported a revived eugenics; briefly adding an unfortunate scientific credibility to racist science. The like-minded Berkeley psychologist Arthur Jensen thought Shockley was “a man of absolute integrity” with “sheer intellectual horsepower” but having a “rather unlikeable character”¹⁰⁵ The offensiveness of eugenics in a post-Holocaust world would continue to fade away for many psychologists, especially those defending hereditarian psychology. Facing scorn from mainstream science, and looking to drum up more financial and public support for the cause of race betterment, Shockley formed the Foundation for Research and Education on Eugenics and Dysgenics (FREED) (Tucker, 1996, pp. 193–194).

¹⁰⁵ Jensen to Cattell. December 22, 1972. Found in Box M5311, Cattell Correspondence folder, The Arthur Jensen Papers (hereafter AJP), The Drs. Nicholas and Dorothy Cummings Center for the History of Psychology, The University of Akron.

Raymond Cattell, also on MQ's editorial board, would extend his trait psychology research into a moral system meant to guide society: Beyondism. Though Cattell had distanced his work and reputation from his 1930s support of German Nazism—a time when he also began his pursuit of a utopian science that divided races, such as the Jewish people (e.g., Cattell, 1933, 1938)—his postwar career (aside from his 16-factor personality model) was devoted to creating a book, a society, and a newsletter for Beyondism. The depth and longevity of Cattell's relationship with postwar "revived" eugenics did not become well known until the 1990s (Tucker, 2009). Noting how the most radical right-wing eugenicists promote and celebrate Cattell's Beyondism, one critic regarded it as a "neo-fascist contrivance" (Mehler, 1997). When considering a political economic system best supported by genetic and psychological evidence, Cattell overall favoured a form of capitalism, as it assured a competitive free market where undesirable groups of people would die out: "a stupid people controlled by a wise elite would outlive an averagely intelligence democracy ... this would be an overall genetic loss" (Cattell, 1972, p. 323). Cattell hoped his book promoting Beyondism would reach a wide audience, as he believed its "restructuring of values ... would do more than anything to make the scientific pursuit of behavior genetics fully acceptable."¹⁰⁶

Cattell's Beyondism work would influence many neo-fascist writers, such as Wilmot Robertson (real name Humphrey Ireland¹⁰⁷) whose works included *The Dispossessed Majority* (Robertson, 1976). A work firmly rooted in "race realism" and fears of white Nordic-American race suicide and loss of traditional norms, Robertson concluded his "racial history" tome warning: "[E]verything hangs on the fate of the American Majority. If its dispossession is not stopped ... there will soon be no America ... the dying fall of the American Majority is the dying

¹⁰⁶ Cattell to Jensen. April 19, 1973. Box M5311, Cattell Correspondence folder, AJP.

¹⁰⁷ Possibly. See Tucker (2009, p. 229, note 105).

fall of America itself” (Robertson, 1976, p. 559). In correspondence with intelligence researcher Arthur Jensen regarding the academic and student backlash to Jensen’s work on race and IQ, Cattell mentioned that “a Mr. Wilmot” had sent him a copy of *The Dispossessed Majority* which he found “very exciting.” Wilmot also suggested sending a copy to Jensen; Cattell agreed that Jensen would certainly “enjoy it.”¹⁰⁸

Though some psychologists may still today defend Cattell for his (undeniable) technical contributions to multivariate analysis and psychometric methods, it is difficult to brush aside his interests in eugenics and racial segregation.¹⁰⁹ Especially when considering when Cattell was 90 years old, he interviewed with *American Renaissance*—an extreme right-wing and white supremacist outlet that had previously celebrated Cattell’s *Beyondism* book (Cattell, 1972). The piece celebrates Cattell’s core Beyondist beliefs as unchanged: the need for a moral system that affirms radical inequality and promotes evolutionary competition among races; the destructive force of welfare; the need for education on heredity; and the necessity of segregating races.¹¹⁰

Although Eysenck would often accept research money from the Pioneer Fund, he would go to great lengths in his autobiography to dispel any suspicions of ethnic prejudice, especially anti-Semitism (Eysenck, 1990). Though he certainly had little sensitivity about racial terminology behind closed doors. During the 1970s, he mentioned in correspondence to Jensen that he was interested in comparing “blacks and whites ... and yellows” on intelligence scores.¹¹¹ Faced with Eysenck’s nonchalant attitude toward where his funding came from, his true beliefs (if he held such things) seem indeterminable.

¹⁰⁸ Cattell to Jensen. December 5, 1972. Found in Box No. M5311, Cattell Correspondence folder, AJP.

¹⁰⁹ Even Jensen, in correspondence with Eysenck, would denounce racial segregation. Jensen to Eysenck, May 28, 1975, Found in Box No. M5311, Folder 12, AJP.

¹¹⁰ Archived interview available here (if you want to provide the domain with hits): <https://www.amren.com/archives/back-issues/october-1995/#article1>

¹¹¹ Eysenck to Jensen. December 11, 1972. Found in Box No. M5311, Folder 12, AJP.

Cattell, though a rival in terms of warring trait models (16 vs 3 factors), assumed they shared core beliefs but was never entirely sure of Eysenck's true colours: “[Eysenck] realizes these sharp, technical debates between us in the limited area of the scientific domain do not affect in the least our substantial agreement on a number of probably more important things. At least I hope he does.”¹¹² Despite his persistent pursuit of the physiological correlates (and implicitly causes) of personality, Eysenck claimed to embrace a “biosocial” view of psychology— Eysenck's students apparently admired his ability to “transcend” the never-ending nature versus nurture debate (Buchanan, 2010, p. 161).

In addition to seeking non-governmental funding from the eugenicist Pioneer Fund, Eysenck also sought funding from tobacco corporations. Scandals over Eysenck's links to American and British tobacco corporations in funding his research on personality disposition to anxiety and lung cancer rates is well-known to historians; though the fraudulent practice of Eysenck and his colleagues is still becoming known to the scientific community (e.g., Buchanan, 2010; Rose, 2010; Smith, 2019). Phillip Morris would also continue to fund generations of Eysenck's students pursuing psychophysiological research (e.g., EEG laboratories) on nicotine, personality, and intelligence, including Marvin Zuckerman¹¹³, while others like Robert Stelmack and his colleagues and students would work alongside RJ Reynolds Tobacco Company scientists well into the 1990s.¹¹⁴

In this regard, the career trajectory of Costa and McCrae proves significant. At the start

¹¹² Cattell to Jensen. December 13, 1972. M5311, Cattell Correspondence folder, AJP.

¹¹³ Zuckerman's “Grant Proposal: Smoking and Sensation Seeking...” found in Box No. 1014, Philip Morris Records (Master Settlement Agreement). Retrieved from <https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=nhyb0114>

¹¹⁴ An example is the internally distributed draft report of “Can smoking speed cognitive processing?” co-authored by two employees of the R.J. Reynolds Tobacco Co. Found in Box No. RJR50777, Case: US Comprehensive Request 124, the RJ Reynolds Records (Master Settlement Agreement). Retrieved from <https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=kfjd0093>

of their career in the mid- to late-1970s, the since disbanded (but rebranded and reconstituted) Council For Tobacco Research (Schick & Glantz, 2007) partially funded Costa and McCrae's early research using gerontological data.¹¹⁵ These research data were of male veterans from the Normative Aging Study. This study, receiving governmental funding from the National Institute of Health (NIH), occurred at Boston's VA Outpatient Clinic in 1963—a decade before Costa and McCrae's analysis. The duo were interested in using lifespan data to explore the stable factor-trait structures undergirding human personality (Bell et al., 1972; Costa & McCrae, 1976).

Then, in 1978, Costa received an appointment as head of a new Section on Personality, Stress and Coping at the Gerontology Research Center, Baltimore, MD. What had been a tenuous tap into federal funding had now turned into full-time employment. Around this time, and with no indication of re-applying for funding, Costa submitted a Final Report for the Council for Tobacco Research 1979 in which he seemed to indicate that much of the funding was used to further validate their questionnaire findings in pursuit of their own model and scale; as to the specific research hypothesis, he opined one interpretation of the negative results: “enduring personality dispositions” likely outweigh any temporary benefit to well-being smoking might offer.¹¹⁶ Nevertheless, tobacco companies would continue to note Costa and McCrae's publications that resulted from their funding—as well as revised versions of their NEO-PI, likely for their own research purposes.¹¹⁷

¹¹⁵ For example, Costa's 1977 grant renewal application for the project “The Relations Between Smoking Motives, Personality, and Well-being.” Found in Box No. 122, Grant No. AP01085R1, the Council for Tobacco Research Records (Master Settlement Agreement). Several grant applications, progress reports, and draft publications are available to the public at the main URL domain. This particular document retrieved from <https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=pzxd0215>

¹¹⁶ Costa's 1979 Final Report available in Council for Tobacco Research Records. Retrieved from <https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=hkcv0070>

¹¹⁷ An internal copy of Costa and McCrae's 1992 “Revised NEO-PI-R” Item Booklet-Form R (Women) found in Box no. 1253, Philip Morris Records (Master Settlement Agreement). Retrieved from <https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=ynxp0117>

Although Costa and McCrae soon discontinued their relationship with Big Tobacco—unlike Eysenck and some of the small network of biopsychological researchers—it is important to note that they were well within an “Eysenckian” research route outside of their one-time funding sources. Unlike the lexical work of Goldberg (stemming from Norman, Fiske, and Cattell’s expansion on Gordon Allport’s work) and his eventual Big Five model and scale, Costa and McCrae worked from a questionnaire approach to get to their version of the FFM. As previously mentioned, their initial models (their Big Three) were from data using Cattell’s 16PF as well as Eysenck’s scales. In their interpretation of the data, Costa and McCrae suggested a different third factor: Openness. This led to the development of their widely recognized personality inventory: the NEO-PI (Neuroticism, Extraversion, and Openness). With regards to Eysenck’s legacy, Costa and McCrae would even recently sidestep controversy in favour of his empirical contributions—especially organizing trait psychology around neuroticism and extraversion, bedrock for their version of the FFM (Costa & McCrae, 2012).

Younger generations of personality psychologists, including FFM proponents, indeed owed a debt in methodology and topics to the established research landscape. In addition to the psychometric work of Eysenck and Cattell, Costa and McCrae’s initial FFM research rested entirely on the advent of longitudinal data—echoing psychological tester Lewis Terman’s work—a method they would continue once relocated from Boston to Baltimore. To seek stability over a lifespan was to support a hereditarian position, even if one was open to incorporating the environmental influences on the presumably inherited traits.

Something superficially resembling intelligence research has also persisted within FFM researchers. When Costa and McCrae examined Norman’s 1963 research themselves, they concluded that the fifth factor of the FFM was best labelled Openness to Experience rather than

Intellect or Culture as others had interpreted it. Disputes about the fifth major factor—and the adjoining lexical versus questionnaire methodologies—would continue long afterward: is it intellect, imagination, creativity, openness? (e.g., Trapnell, 1994). More recently, researchers maintain that there is a lack of association between the fifth domain of personality and cognitive ability (DeYoung et al., 2014). In other words, personality traits—even this one resembling something intellectual—are in no way to be confused with the original trait: intelligence. Just as personality psychologists are in no way to be confused with the original trait psychologists: intelligence researchers.

Nevertheless, modern day personality trait research tends toward a hereditarian position—having grown out of the traditions of psychological testing that more often than not attempt to locate innate and stable characteristics of organisms, it would be difficult not to. For most FFM researchers, the model has long been positioned within evolutionary or genetic frameworks (Goldberg, 1993). Even more recent (and quite sophisticated) attempts at wrangling in the FFM for the purposes of creating a grand meta-theoretical framework for human personality and development hark back to the Cold War aspirations of behavior genetics. The FFM is argued as a way to tap into the potential explanatory power of neuroscience, evolutionary biology, genetics, and cybernetics (e.g., DeYoung, 2015).

Of course, diverging from the least desirable parts of personality psychology's heritage has long been of interest to FFM researchers. For psychologists who view their work as a branch of the sciences, Eysenck is especially problematic. For perhaps even worse than his association with neo-eugenicists and race scientists, and his general propensity toward stoking controversy with slapdash research projects and books on anything he found interesting, was his openness to fraud when it ultimately served a higher truth. Fraudulent science was unimportant to Eysenck so

long as the cooked numbers supported the correct worldview. When considering the charges of fraud lodged against his mentor Cyril Burt, Eysenck pulled a whataboutism, pointing to the fraudulent research of egalitarian, Marxist scientists: “The damage they do is far greater than done by Burt” (Eysenck, 1990, p. 210)

Instead of a conceptually and politically complicated account of how research on personality taxonomy moved toward the Big Five, official FFM histories offer a simpler account of methodological progress, delayed by cultural attitudes, and finally allowed to reach a theoretical consensus. For one psychologist, the slow movement toward and acceptance of the FFM was much like previous great discoveries of human history—such as the slow acceptance of plate tectonic theory in geophysics (Digman, 1996). Hans Eysenck and wife Sybil thought similarly: aligning their fight for “heterodox” science with Ronald Fisher’s struggle in publishing his 1918 work on Mendelian heredity, which was “too novel to be acceptable to lower mortals” and “[p]racticioners of ordinary science” (Eysenck & Eysenck, 1992, p. 395).

The mid-1990s FFM narrative of past dissonance and present harmony—framed as an inevitable outcome—was consonant with a global narrative among conservative commentators of that moment. In light of the ongoing collapse of the Soviet Union, conservative commentators were celebrating a triumph of consensus: the best (and obviously ultimate) form of society was emerging across the globe. Best represented in political scientist and conservative philosopher Francis Fukuyama’s “The End of History?” (1989)¹¹⁸, it was suddenly clear that the world had witnessed “the total exhaustion of viable systematic alternatives to Western liberalism” and was now entering “the end point of mankind’s ideological evolution” (pp. 3-4). It appears that the

¹¹⁸ Later expanded on in Fukuyama (1992). More recently, Fukuyama has postponed the end of history—blaming identity politics and political correctness (Menand, 2018). Interestingly, Fukuyama has also reconsidered his “end of history” in an era of new, genetic technologies (e.g., Fukuyama, 1999).

presumed end of history coincided with the presumed end of personality: an inevitable five-factor model of personhood emerged victorious in an inevitable world of free-market democracies. Uncertainty and upset were base emotions of an irrational past.

Situated Scandals: On the Selective Histories of the FFM

Often pitched as a technical, methodological debate contained within the discipline psychology, the person-situation controversy of the late 1960s and 1970s reflected how personality psychologists grappled with the vexed concepts of heredity and environment in the aftermath of eugenics. Many disciplinary accounts purposely evade this political history (Digman, 1996; Goldberg, 1995). In this section and onward, after briefly sketching these controversies and psychologists' involvement, it will be made clear that personality was viewed as a safe disciplinary space for race scientists to both deflect and promote their intertwined ontological and political values: hereditarianism and libertarianism.

Around the time of WWII, the earlier hereditary-environment controversy among American natural and social scientists was largely settled with an answer that would be sustained—sometimes superficially—in personality psychology's miniature version of the same debates. An interactionist and interdependent model of causality was repeatedly the solution to problems of nature versus nurture, heredity versus environment, personality versus situation, or (at its most abstract) internal versus external forces on human evolutionary development. These controversies, having a family resemblance, spanned much of the twentieth century and ran across professional spaces: from geneticist Thomas Morgan's fruit fly research program, to the earliest days of transdisciplinary field of behaviour genetics (BG) to the severely treacherous back-and-forth on the relationship between race and intelligence.

Gene X Environment in Behaviour Genetics

In the earliest days of the postwar landscape, there were clear signals that the hereditary-environment controversy of the past few decades had subsided across the American disciplines. In 1947, a leading geneticist and anthropologist jointly published an article in the prestigious journal *Science* titled “Natural Selection and the Mental Capacities of Mankind” (Dobzhansky & Montagu, 1947). Their article reiterated the by then widely accepted view of heredity and environment working as interdependent variables in the puzzle of human evolution. They also claimed the virtual non-importance of genotypic differences in personality traits for studying either individuals or groups (Dobzhansky & Montagu, 1947, p. 590). Though their emphasis was accommodating the complexities and plasticity of human phenotypical development and that process’ relationship with cultural evolution, the intent was not to discredit biological understandings of humanity. Montagu would help shape the famous UNESCO statements on race and science, providing a mainstream rebuttal to Nazi scientific racism and future support for the Civil Rights movement (Brattain, 2007). As historian of evolution Cravens (1978) pointed out, the postwar sentiment leaned toward understanding humanity’s unique genotype as what allowed for culture and its interlocking mutable human phenotypes (p. 158).

Among psychologists who were not committed to hereditarian and eugenicist frameworks, more complex and interactionist views on development were appearing very early on in the heredity-environment controversy. For example, Harry Hollingworth’s interactionist update on Hall’s genetic psychology (Hollingworth, 1927). By 1957, psychometrician and then American Psychological Association President Anne Anastasi—famed for her inclusion of cultural concerns in the methodological development of differential psychology—addressed the APA on the topic from a place of humility (Anastasi, 1958). However, the variables of heredity

and environment may be conceptualized, controlled, or measured—even if considered interdependent—it did not sidestep a central problem of unavoidable ignorance. The kinds of questions and precision of answers psychologists wanted about the genetics of human intellect, personality, and behaviour were currently unintelligible and necessarily unanswerable.

In the postwar era, behaviour genetics (BG) grew as an interdisciplinary field that had its own academic association and journal. Various researchers were interested in the genetic basis of behaviour, and a transdisciplinary network like BG allowed further mining of already-interdisciplinary areas like neuroscience, cybernetics, and medical genetics (Panofsky, 2014). During this postwar golden era for BG, leading figures were able to avoid the rancorous politics and ungovernable controversies that would later come to define the field. From Dobzhansky's role in developing the field, to his student Jerry Hirsch's efforts in strengthening the research network, BG was an anti-eugenicist specialty interest largely within the purview of animal researchers. Its openness to ideas and initial ability to overcome controversy was soon supplanted—a cadre of psychologists and patrons with vested interests in a devoutly hereditarian framework would soon render BG into a field that fed off the clamour of political and ethical controversy.

Many trait psychologists, despite their claims of being interactionist—and their assertions of critics being wholly environmentalist—proved (uniquely) resistant to the postwar interactionist orthodoxy within behavior genetics. They worked hard in advancing claims for the strong hereditary basis of intelligence and personality. For many, a symbolic heritability of their own intelligence was of paramount importance. Like their intellectual father, Galton, many intelligence researchers were fascinated with the success and achievements of their kin. As Hegarty (2013) rightly observes, “[p]sychologists who are most invested in the category of

giftedness and genius have a long history of ontologizing themselves and each other as paradigmatic of the high intelligence that they describe” (p. 16). Empirically showing who were the most published and cited psychologists was important—especially if they were of your own lineage or “tribe.” Arthur Jensen was very keen on affirming his mentor Eysenck’s prolific output; he noted Eysenck’s citation count in the *Social Science Citation Index* was only outweighed by Freud and Marx, his two main nemeses (Jensen, 1997). Even interactional psychologist Norman Endler’s only real collaboration with Rushton were a series of articles on the “productivity” and impact of psychology departments in Canada, USA, and the UK (Endler et al., 1978; Rushton & Endler, 1977, 1979). Rushton would later use such methods to continue the constant honoring of Eysenck’s scholarly prowess (Rushton, 2001).

Arthur Jensen, perhaps Eysenck’s most controversial protégé, was a California-born, onetime hopeful musician and briefly a social worker who then went to Columbia University’s Teachers College to earn his PhD, received in 1956—where he mostly researched psychoanalytic topics, such as projective testing (Fancher, 1987, pp. 185–189). After discovering Eysenck’s popular writings and completing a two-year post-doctoral position with him in London, Jensen’s interests and aims changed. After the publication of “How Much Can We Boost IQ and Scholastic Achievement?” in the *Harvard Education Review*, written from his baccalaureate-granting and professorial home of UC Berkeley, he would be forever associated with post-Civil Rights scientific racism (Jensen, 1969). In his article, largely a literature review, Jensen portrayed intelligence as a unitary and neurologically-based psychological trait that is stable across time and within races; in other words, fighting against claims of intelligence’s plasticity. Among his arguments, Jensen contended that, despite “conclusive proof,” heritability was more important in determining intelligence than environment (Jensen, 1969, p. 29).

Jensen's paper roiled many into action: from student protests to the coining of "Jensenism" as a term essentially standing in for scientific racism (Fancher, 1987, pp. 197–201). His focus on the genetically-determined intelligence gap between White and Black students marked the end of BG's golden age (Panofsky, 2014). Despite bemoaning a misrepresentation of his true views in public, Jensen would confide to his mentor Eysenck that "[m]y views on heredity and IQ are intolerable to a large segment of the Berkeley community, including the Negro president of the Board of Education"¹¹⁹ In lieu of a balanced research agenda and membership, trait psychologists within BG would push the nascent and interdisciplinary field from a neutral space to politically involved and strongly hereditarian.

For Jensen and his colleagues, politics, meaning to allow emotions to override reason, stood in the way of legitimate racial science—even when those political ideologies and the associated emotions are a reasonable reaction to racist science. Jensen considered members of SPSSI to be "a strange bunch, very fussy-minded psychologists ... If one wants to find the extreme environmentalists, SPSSI is the place to look."¹²⁰ With regards to protestors, they were simply "the campus nuts."¹²¹ Critics were rewarded with sobriquets, like "Prof. Richard 'New Left' Lewontin."¹²² Eysenck noted some of his former students were becoming critical, especially during the scandal over Burt's fabricated data: "oddly enough they were all members of the Communist party They were all quite bright but apparently have carried their ideological prejudices around ever since."¹²³ Ideology, emotionality, and socialism had mitigated their former intellectual brightness.

¹¹⁹ Arthur R. Jensen to Hans J. Eysenck. July 7, 1969. Found in Box No. M5311, Folder 12 "Eysenck Correspondence, AJP.

¹²⁰ Jensen to Eysenck. December 22, 1972. Found in Box M5311, Folder 12 "Eysenck Correspondence," AJP.

¹²¹ Jensen to Eysenck. February 27, 1973. AJP.

¹²² Jensen to Eysenck. July 9, 1973. AJP.

¹²³ Eysenck to Jensen. November 12, 1976. AJP.

Claims of rampant egalitarianism during the 1970s controversy over race and intelligence were likely exaggerated. While a strong anti-hereditarian and egalitarian spirit was certainly present in the immediate aftermath of WWII's atrocities, by the 1970s hereditarian and internalist positions were far less maligned. In early post-Holocaust moments, such as the 1950 UNESCO statement on race (and its revisions), a strong anti-racist attitude tended to support an egalitarian stance among scholars. How successful such statements were in shaping human science and undoing reified notions of race is debatable (Brattain, 2007). Even anthropologist Ashley Montagu, often positioned as a cultural relativist of the most extreme, held hereditarian views on human capacities (Weidman, 2019). While a robust scholarly commitment to anti-racism and egalitarian values may have been true during the initial postwar moment, by the 1970s and after Jensen's controversial article, race research had shifted again from "socially destructive" to "socially important"—especially under the guise of academic freedom (Panofsky, 2014, p. 71; see also Lewontin, 1996). Yet an unreasonable zeitgeist that emphasized egalitarian values, and eschewed the heredity and stability of psychological traits, was the apparent impediment for both intelligence and, as will be shown next, and personality research of the 1970s.

Person X Situation in Personality Psychology¹²⁴

The 1970s controversy over the genetic determinants of racial intelligence would parallel the less politically simmering person-situation controversy (PSC) occurring within disciplinary psychology. The PSC (along with the alleged, general egalitarian and anti-heredity/trait sentiments of the 1970s) would be framed as a major impediment to the FFM. While Jensen's (1969) paper can be argued as a tipping point for the debate on race and the transformation of

¹²⁴ This section draws on research previously reported in Davidson (2015).

BG's own politics, personality psychologists point to another allegedly villainous publication that derailed their own research program: Walter Mischel's *Personality and its Assessment* (1968). And just as many psychologists, alongside most natural and social scientists, had long been pushing for an interactionist or interdependent view on heredity and environment, a push for interactionism within personality psychology would become the official solution to the PSC. But even psychologists incorporating an "interactional psychology" to their taxonomic and biological research of psychological traits would lean toward an hereditarian stance—and others would leave questions of "situations" and "interactions" behind at the dawn of the FFM.

In the standard historiography of the FFM, there is often a "pre-history" to which proselytizers point toward as evidence for two crucial aspects of their history (Digman, 1996; Goldberg, 1995). Firstly, the FFM was previously discovered before the work of psychologists in the 1980s and onward; hence, the true underlying structure of personality traits was lying in wait. Secondly, although researchers were well on their way to further validating and publicizing their great discovery, an egalitarian social and campus zeitgeist severely impeded scientific progress.

One of the key works in this FFM "pre-history" are technical reports on the stability of an FFM-like factor structure in personality ratings produced in the US Air Force's Personnel Laboratory (Aeronautical Systems Division) at Lackland Air Force Base, Texas (Tupes & Christal, 1958, 1961). Factors in that technical report would include Emotional Stability (comparable to Neuroticism) and Surgency (comparable, arguably, to Extraversion); they would also include a fifth factor named Culture (comparable to Goldberg's Intellect/Culture, and Costa and McCrae's Openness). Other "pre-history" findings include, as mentioned before, Warren Norman's research and Donald Fiske's research. Another early discovery is attributed to the research of psychologist Donald Fiske, both published in the pages of the sub-disciplinary-

defining *Journal of Abnormal and Social Psychology* (Fiske, 1949; Norman, 1963; see also Davidson, 2018). In fact, one of the main internalist histories of the FFM is featured in a festschrift honouring Fiske (Goldberg, 1995).

If researchers were homing in on the FFM of personality this early on, possible impediments to the model's ascendance were necessarily posited. So, as Goldberg (1995) asked in the title of his FFM history, “what the hell took so long?” Answers included laying blame on individual psychologists, the ethos of the discipline in the postwar era, the legacy of behaviourism, as well pointing toward the general egalitarian politics of academia during the American counterculture moment and afterwards. For FFM promoters, the 1960s and ‘70s was an “era of skepticism”—in which research exploring a person’s stable predisposition toward certain behaviour (i.e., “personality traits”) demanded the utmost scrutiny (Digman, 1990; Digman, 1996). Other disciplinary historians would concur that the 1970s and early 1980s was “a period of dormancy” for personality research until a supposed rediscovery of factor analytic methods (John et al., 2008, p. 119). Even FFM dissenters like Eysenck still very much agreed that the cultural climate was not conducive to personality research: “there was always in the United States an unreasoning dislike of individual differences. The notion of ‘equality,’ carried to irrational extremes as suggesting biological rather than social equality, created a Zeitgeist hostile to the study of traits and types” (Eysenck, 1990, p. 98)

Walter Mischel—and his pointed but largely apolitical, technical critique of trait psychology—served as an antagonist who represented all these sources of FFM-discovery delay. Mischel’s *Personality and Assessment* (1968) is an unassumingly short book that provided critiques for both the trait approach to personality but also the psychodynamic approach. For the author, it was written in the tradition of a “genuine integration of the knowledge of behavior that

is emerging” from experimental and measurement research on personality (Mischel, 1968, p. *viii*). One of the most stinging and oft-recited parts of his critique was “the personality coefficient:” a weak, average correlational relationship between self-report inventories and observed behaviour. While Mischel argued that the low personality coefficient plaguing the psychometric-trait approach suggested personality structure was “more subtle than broad unitary trait theories,” (Mischel, 1968, p. 101) the book is far from a vicious attack on trait psychologists. Perhaps giving more grief to the psychodynamic approach to personality—which would align him closely with a trait researcher like Eysenck—Mischel’s book was a vehicle to promote his own social-behavioural models of personality; in which he positioned his approach to the Big Two of personhood: psychoanalysis and heredity/traits.

Yet Mischel’s methodological critique was a stand-in for the broader threat of critiques against strict hereditarianism—and as a rebellious force, Mischel and the counterculture “situationism” he represented would prove to be a paper tiger in the exaggerated fight over personality psychology. Mischel’s (1968) initial criticism certainly prompted much response, and psychologists produced research on the predictive validity of trait research, on cross-situation behavioural consistency, and on interactional approaches well into the 1980s. Yet Lawrence Pervin, an expert in the PSC—and someone who had pointed out the uselessness of such controversies the same year as Mischel’s book—contended that the supposition that psychologists truly believe persons behave completely consistently or inconsistently across situations was patently false (Pervin, 1968, 1989).

Nevertheless, as one psychologist who was involved with PSC early in his career, observed the debate had generated a proverbial “cottage industry” mainly devoted to discerning “what Mischel did and did not actually say” (Funder, 1983, p. 63). Research on the PSC,

produced in an already dizzying sea of personality research, quickly amassed. Even the controversial race psychologist Rushton dreaded a potential project reviewing the insurmountable literature on personality stability: “What a quagmire—a real test of my organizational abilities more than anything else.”¹²⁵ But besides from the few early examples of FFM research, like Donald Fiske’s or Warren Norman’s taxonomic research and its continuation into Goldberg’s Big Five, not all personality psychologists took a great interest in pursuing strictly taxonomic research. Even by the 1990s, even after the mass collection of evidence supporting the cross-instrument and -language validity of the FFM structure, there was still much inter-generational dispute about its adequacy in describing human personality (Block, 1995; Costa & McCrae, 1995; Eysenck, 1991; Eysenck, 1992; Goldberg & Saucier, 1995; McAdams, 1992).

A 1968 review of research on personality structure mentioned Mischel’s book mentioned alongside the many other recent developments in personality psychology (Wiggins, 1968). In that same review, noting the ever present and prominent research circling the traits of neuroticism and extraversion, Wiggins deemed them the Big Two—a pair easily associated with Eysenck. While the research within the purview of the Big Two was common, especially among psychologists developing personality scales and taxonomies, researchers pushing for a five-factor solution to personhood would not become normative for years to come. Though many personality psychologists were contributing to the PSC, established and prolific figures like Eysenck and Cattell were still pushing their own factor models—as well as pursuing varied other research and personal interests.¹²⁶

¹²⁵ J. Philippe Rushton to Norman Endler. January 30, 1981. Found in Box 17 of The Norman Endler Papers (hereafter NEP), Call Number 2003-040, The Clara Thomas Archives and Special Collections, York University, Toronto, Ontario, Canada.

¹²⁶ Another “big two” in personality research was also emerging in Wiggins’ own work on the circumplex model of

If anything, Mischel's 1968 book likely raised the profile of Norman's taxonomic work and the proposed five-factor solution. Instead of only focusing on popular models like Eysenck's 2-factors or Cattell's 16-factors, Mischel would frequently use Norman's work as a departure point for his critique. Long after the dust had settled on the PSC, and the FFM was gaining ground, debate among personality psychologists about the right number of factors for an appropriately adequate taxonomy of human personality's underlying structure would continue (e.g., Block, 1995; Eysenck, 1992). Even Donald Fiske, whom Goldberg (1995) and others position as an early discoverer of the Big Five, was unsure about the rising FFM consensus. Writing in his own festschrift in which Goldberg contributed his account of the FFM's history, Fiske favoured explanations of persistent truth over zeitgeist-impediments: "rarely have so many personality psychologists agreed on any substantive proposition. To be more cautious, never have those concerned with personality measurement seemed to agree so closely" (Fiske, 1995, p. 354).

The PSC may have felt like a delay in progress among FFM researchers, but it seemed to have little effect on the already present and persistent disagreements among personality psychologists already committed to an "internalist" and factor-analytic framework. The genuine threat of Mischel and his critique was not delaying scientific progress, but its challenging of the hereditarian position held among many personality psychologists. The PSC was a miniature, contained, and addressable version of the wider controversy with which trait psychologists interested in race, intelligence, and BG were contending. Mischel and the "situationist" stood in

personality, which had partial roots in the interpersonal research of Harvard exile and newly self-fashioned countercultural guru Timothy Leary (e.g., Freedman et al., 1951; Leary, 1957; see also Devonis, 2012). Interest in this model would persist well into the end of the century. Costa and McCrae, in the same year they published a piece framing the MBTI within a FFM perspective, would also validate Wiggin's circumplex model as complementary to their five-dimensional scale (McCrae & Costa, 1989a).

for the outraged (and, to race psychologists, outrageous) critics and protestors. At the end of the 1980s, when Rushton was facing the beginning of backlash to his research on racial differences, he thought back to the PSC: “I am becoming convinced that much of the 1960s opposition to ‘trait theory’ was ideological in origin,” chalking up opposition to the non-scientific and political concerns of “left-liberal and egalitarian ... social activists” of the past and present.¹²⁷

In an echo of the way eugenicists and associated psychologists perfunctorily adopted an environmental view, or even an interdependent view, on the causes of human development during the 1930s shift toward a “positive” eugenics, trait psychologists conceded interactionism as an obvious answer to the PSC. Even before Mischel’s apparent salvo, clinical and personality psychologist Norman Endler—who would lead the charge for an “interactional psychology”—saw interactionism as obvious and any dispute about persons versus situations as inane: “[The dispute] over whether the main source of variation in behavior is in situations or in persons turns out to be a pseudoissue” (Endler & Hunt, 1966, p. 337; see also 1969).

Given the preceding decades of debate among natural and social scientists, and the eventual adoption of heredity and environment working in interaction as necessarily interdependent causal variables, it is unsurprising that Endler would have such a position at the ready before the PSC even heated up. Even Mischel, in part responding to some of the rabbleroising reactions to his book, would soon reaffirm the obvious: interactionism was necessary for a unified and accurate account of the complexities of human personality (Mischel, 1976, 1977). The interdependence of gene/environment, or the interaction of person/situation, was a postwar consensus. The unspoken controversy of the PSC may lay elsewhere—likely in the resuscitation and maintenance of intelligence research and the wider trait psychology project.

¹²⁷ Rushton to Marvin Zuckerman. May 31, 1989. From Box 17, NEP.

Asides from Endler's research program on interactional psychology, prominent trait psychologists did little to adjust their course: the rudder's angle maintained its tilt toward an hereditarian explanation of personality. Even Eysenck had always claimed a "biosocial model" of human psychology, though in his view the inherited aspects of psychology seemed to be the genuine part while the social were simply governmental limitations upon a person: "Man is a biosocial organism ... determined equally by his biological factors ... and by social constraints enforced by government" (Eysenck, 1990, pp. 63-4). By the late 1980s, when defending his research on race and intelligence, Rushton would echo the importance of a biosocial model that "integrates both evolutionary and genetic components with the more salient environmental features" as a means to "solve individual and social problems" with any serious "adequacy."¹²⁸

Much as Panofsky (2014, p. 6) points out that BG scholars should not be understood as having a single political position¹²⁹, it is worth noting that even trait psychologists directly involved with neo-eugenicist research programs had varied beliefs about hereditarian and environmental influence. Nevertheless, the default position among many trait psychologists, even those like Eysenck who ostensibly promulgated a "biosocial" and interactionist framework, was hereditarian with a varied tolerance for how much of psychology they allowed environmental explanation to encroach. By the 1980s, other psychologists who were once keenly interested in the problems of situations and interactions were switching gears. For example, Lawrence Pervin admitted to Endler that he had "dropped my research on situations" in favour of exploring "affect & personality."¹³⁰

In Mischel's case, even though he and his students would continue modelling personality

¹²⁸ Rushton to Behavioral Genetics Associations Membership. April 27, 1989. From Box 17, NEP.

¹²⁹ Though the most ardently conservative and hereditarian were often trait psychologists like Jensen and Rushton.

¹³⁰ Lawrence Pervin. Letter to Norman Endler. January 4, 1984. From Box 17, NEP.

while respecting the complexities of internal and external causal influences (e.g., Mischel & Shoda, 1999), he would achieve his great fame with a distinctly internalist experiment on self-control which he had begun long before his review of personality trait stability. The Stanford Marshmallow Test is by now a cultural signifier: understood as a litmus test for a child's future successes and failures in school, career, and life in general (Konnikova, 2014; Mischel, 2014). Although Mischel promoted his experiments as demonstrating how flexible persons are—changing how one frames a situation can affect one's self-control—they have been criticized for taking an American conceptualization of willpower and “putting a cute toddler's face on a terrifyingly austere interpretation of economic class and social reproduction” (Moreton, 2014, p. 30). The Marshmallow Test would influence research on “emotional intelligence” and rampant self-help literature that emphasized the primacy of impulse control in developing character. Far from an enemy of internalist personality psychology, portrayals and interpretations of Mischel's self-control experiments would shift away from situational factors as essential to situational factors as dispensable in understanding willpower (see Staub, 2018, ch. 4).

As with Costa and McCrae's eventual moving away from Eysenck and Cattell — in funding sources and in factor model— personality psychologists understood their own history as separate from political controversies. The PSC—framed as an issue of behavioural consistency or abstractly as internal versus external causes of behaviour—was understood as distinct from the long-running and racially charged controversies over nature and nurture. In fact, the decoupling of person/situation from nature/nurture was part of personality psychology's formal self-organization as a discipline. In Pervin's *Current Controversies* series, the PSC is always treated as an issue entirely separate from the nature-nurture controversy (Pervin, 1978). The implication here is that questions about the stability of personality traits—such as Mischel's

dreaded personality coefficient—ought to be understood as separate from questions about the stability, heritability, unity, and ethicality of intelligence.

But this rhetorical division was porous in real life. Aside from the obvious growth of personality testing from the established intelligence testing in the 1920s already explored, many of the psychologists interested in personality (including interactionist research) were either directly involved with ongoing research on intelligence—such as Eysenck, Cattell, or Rushton—or associated with groups that saw such research as fundamental to a psychometric psychology. Someone like Norman Endler, who does not appear to have had any direct or vested interest in intelligence research, was a member of groups like Eysenck’s International Society for the Study of Individual Differences (ISSID)—its summer conferences were a place he could catch up in-person with colleagues like Rushton.¹³¹ This is not an example of “guilt” by association. It points to a broader disciplinary norm among many personality, developmental, and clinical psychologists. Before the fallout of the Bell Curve debates, psychometric and evolutionary research on intelligence was simply a part of apolitical individual differences research in scientific psychology (despite the literal protests of the 1970s). FFM researchers were wise in separating early on—and in reinforcing this separation in their historiography.

The PSC history narrative, though truthful in its details, was a way to divert attention from these less than desirable associations and place blame on all things “social”—from social approaches to personality to social attitudes of the era. Indeed, it can be cast as a manifestation of the uneasy sub-disciplinary marriage of social and personality psychology (c.f., Barenbaum, 2000; Davidson, 2018; Lanning, 2017). Although FFM researchers would grow out of the trait research of psychologists like Eysenck and Cattell, they would smartly move away from the

¹³¹ Endler to Rushton. February 5, 1987. From Box 17, NEP.

normalized controversies that certain psychologists and their eugenicist patrons had foisted upon BG. Even though the rise of the FFM in the late 1980s and into the '90s would coincide with yet another major controversy over race and intelligence—the Bell Curve controversy—the five-dimensional model and its researchers had diverged from its legacy of scandalous research.

But the FFM would continue the project of hereditarianism found in earlier trait psychology. By the late 1980s, Costa and McCrae would put their longitudinal research methodology to work in settling the lingering disputes over trait stability over time: using their NEO-PI, results suggested that time and “aging itself has little effect on personality. This is true despite the fact that the normal course of aging includes disease, bereavement, divorce, unemployment, and many other significant events” (Costa & McCrae, 1988, p. 862).¹³² The FFM assured a person remained themselves in the face of even dire life events—traits were innate and, usually, unchanging. As even the Eysencks noted in an editorial on peer review in *Personality and Individual Differences* (Eysenck & Eysenck, 1992), while that journal was established in part to have a venue for research out of vogue within the USA, its content had since become “largely interchangeable” (p. 398) with that of the prestigious *Journal of Personality and Social Psychology* (*JPSP*).

Academic Freedom for the “Dispossessed Majority”

Central to the politics held among hereditarian-leaning psychologists, including those studying personality traits and psychological disorders, is a conviction of being the oppressed victims of a morally righteous, left-wing, egalitarian academic system and a wider politically correct culture. Imagining runaway, or what some would identify as “frenzied,” egalitarianism,

¹³² Costa and McCrae are still working today to navigate the research corpus on trait stability and change, convinced that a trait perspective should be a priority even when studying development (Costa, McCrae, & Löckenhoff, 2019).

as oppressing the academic freedom of psychologists and others held true for the “reform” eugenicists during the Civil Rights era—such as Cattell, Porteus, and Garrett; the pro-hereditarian psychologists during the backlash against Jensen’s research on race and intelligence; the pro-hereditarian psychologists during the Bell Curve and associated Culture Wars; and, much more recently, the pro-hereditarian psychologists currently claiming to uphold the (economically and individually) liberal democratic virtues of an open and free society—such as Jordan B. Peterson and Steven Pinker.

Victimhood identity is within the province of both left-wing and right-wing politics. Most recently, positioning yourself and your group as victims has become a primary way of “doing” politics (Horwitz, 2018). Victimhood politics emerged from the Civil Rights movements, flowing into other minority rights and women’s movements (and their internal dynamics), as well as reactions against those movements. Once American programs were put in place to address structural inequalities, such as the Equal Employment Opportunities Commission, “white ethnics” such as working-class Catholics resented not being included. As a 1980s Reagan-Thatcher style neo-liberalism supported individualistic solutions to all social maladies, conservatives saw rights-based interventions like Affirmative Action as smacking of communistic social engineering that interfered with the free market of self-reliant individuals with distinct capacities and preferences (Horwitz, 2018, p. 563).

At the same time, it was becoming a common tactic among conservative commentators to target political correctness (PC)—an artificially homogenous group of activists and policies aimed at policing identity and values in the aim of increasing the (in conservatives’ minds) falsehoods of radical equality (Fairclough, 2003). For conservatives, PC was an epithet lobbed at leftists looking to end the apparent discrimination of the majority class. The most authentic (and

certainly most resentful) victims, it was and is argued, turned out to be the victims of victimhood—members of the norms of American society who were losing systemic privileges to PC censoring and egalitarian interventions (Horwitz, 2018, p. 564). Within the wider 1990s “culture wars” of the neo-liberal and Christian Right—such as Newt Gingrich—defending American traditions against the threat of multiculturalism, PC was another weapon of the New Left (Bruno, 2019; Hunter, 1991; Peters, 2008).

In this section, debates over academic freedom within psychology are situated within the wider political landscape of conservative politics of identity, victimhood, and self-marginalization. The controversies described above—the 1970s controversies over intelligence and the interlinked PSC— and those of the 1990s are best understood within the context of conservative values and argumentative tactics. Ironically, much like the concurrent PSC and Jensen-prompted controversy of the 1970s, the FFM’s ascendancy and historiography was being written in the 1990s alongside yet another controversy over race and psychology: the *Bell Curve* and Rushton’s research. In fact, due to Herrnstein’s death just prior to the *Bell Curve*’s publication, the press relied on Rushton as the main psychologist and scientific authority defending race science (Panofsky, 2014, pp. 2-4). His uses of the conservative discourse of victimhood and egalitarian oppression will be explored below.

Within the writing and correspondence of Rushton and like-minded scientists and scholars, there was a persistent presence of “social” oppressors—social activists; social psychologists; social anthropologists; socialists; situationists; egalitarians; Lysenkoists—impeding the purportedly apolitical psychological science of intelligence and personality. During the Cold War era, neo-liberalism had also emerged as primary form of subjectivity within economically-“developed” and -liberal North American and European countries (Teo, 2018; see

also Rutherford, 2018). The conservative-led charge protecting academic freedom, and implicitly liberalism itself, from PC appealed to many hereditarian-leaning psychologists over the past few decades—especially those, like Paul Meehl, who had long pushed for epistemic openness within the discipline.

Campus Battlegrounds

As the politics of victimhood grew into a conservative-dominated discourse, ideas of academic freedom also transformed. Initially a platform for enlightened scholars and scientists to fight against the residual moral prudence and social values of polite society, by the 1980s academic freedom as a cause had expanded to include social conservatives and race scientists. They were the genuine majority victims of an oppressive victimhood politics originating from minority rights. A theme within conservative writing since at least McCarthyist-era fearmongering of communism infiltrating the academy (Buckley, 1951), in the 1980s once again immensely popular books like *The Closing of the American Mind* (Bloom, 1987) sounded the conservative klaxon over PC-led oppression on American campuses. Critics of campus PC culture threatening academic freedom included even the most extreme of conservative writers, like Indian-born provocateur Dinesh D’Souza—who would later propose repealing the 1964 Civil Rights Act in favour of colour-blind and pro-business policy (D’Souza, 1992, 1995).

Central to the discourse of conservative victimhood is the alleged oppression of their right to free speech (Batchis, 2016, esp. ch.3). As one expert on the long-running scholarly and political debates over academic freedom notes, the “increasing tendency to treat academic freedom as synonymous with free speech” is a symptom of successful conservative and libertarian ideologies of individualism and privatization in which the university is positioned as a marketplace of ideas rather than a space in service of the public good (Scott, 2019, p. 10).

Academic freedom was an ongoing issue for psychologists, and others interested in BG and sociobiology, during the 1970s controversy over race and intelligence. During the uproar over Jensen's research, how politics fit with science was perhaps an even more critical conflict than the left versus right or heredity versus environment arguments (Panofsky, 2014, p. 84). Looking to quell things, and distance the field of BG from a political effort, invoking academic freedom was a road to common ground with critiques of hereditarian approaches. For many psychologists and scholars on either side of heredity/environment debates, there was worry that "a common academic value was under threat when protestors protested or threatened scientists" (Panofsky, 2014, p. 85). To wit, a mix of BG researchers and those defending their right to study the inheritance of human capacity and behaviour signed a resolution published in the prestigious *American Psychologist* (Page, 1972). Alongside the expected signatories—Cattell, Eysenck, and Jensen—were (at that time) well-respected scientists like Nobel laureate Francis Crick, as well as psychologists like Harry Harlow, Stanley S. Stevens, Robert Thorndike, and, of course, the inveterate skeptic Paul Meehl. The SPSSI would respond with outrage, as they were accustomed to associating hereditarian stances with eugenics, racism, and legitimated inequality (Pettit, 2011, p. 99).

Yet some of the "environmentalists/egalitarians" and left-leaning critics of Jensen's work were initially sympathetic to the indisputably noble cause of academic freedom. In a more recent interview with psychologist Leon Kamin, author of works intensely critical of hereditarian research (e.g., Kamin, 1974), he claimed he entered the controversy seeking to defend the rights of scholars like Richard Herrnstein to speak without interference from student protestors.

Chalking it up to an erroneous bias of his youth, he has since developed a more refined notion of

academic freedom.¹³³ When controversy over race and intelligence would re-ignite near the end of the century with the work of race scientists Herrnstein and Rushton, a more genuinely corrupted praxis of academic freedom emerged.

Despite worries of anti-equality, Eysenck—whose true beliefs seem nebulous at best—ostensibly endorsed equality as much as academic freedom. Like many race scientists—and the chorus of conservatives claiming PC oppression—he thought protestors wanting to block his lectures in the name of academic freedom was the apex of irony (Eysenck, 1990). Late in life, Eysenck would emphasize he fully supported social equality (while biological equality was egalitarian propaganda), such as claiming adherence to feminist causes. Though he would clarify his own tolerance limits on social equality and activism: “I am not an advocate of the shrill lesbian overtones of some man-hating feminists ... [I prefer] sane and rational feminism” (Eysenck, 1990, p. 23). Eysenck’s dual faith in libertarian personal freedoms and socially conservative versions of equality would colour his encounter with the psychology of authoritarianism. His views reflected a broader distrust and hatred of all manifestations of supposed left-wing oppression in race science and more general hereditarian psychologies.

Of course, psychologists researching racial differences and heredity like Jensen were faced with true resistance from student protestors, faculty, politicians, and activists condemning racism masquerading as science. Eysenck thought American student protestors against Jensen’s research were violent agitators. In a piece written for the CIA- and M-I6-funded British magazine *Encounter*, Eysenck thought that “[h]owever ridiculous some of these ‘militant’ English students” might be, they were at least not as disruptive and violent as Jensen’s less polite American student protestors (Eysenck, 1972, p. 88).

¹³³ Interview with Panofsky (as recounted in Panofsky, 2014, p. 260, fn. 51).

Not a year after that magazine article, Eysenck encountered a less polite crowd of English students. As recounted in his autobiography, the student protest in 1973 at the London School of Economics, when he was meant to give a lecture, broke out into a scuffle that included Eysenck getting punched in the face (Eysenck, 1990). In reaction to the assault on Eysenck at the LSE, Jensen opined that “the opposition” were becoming “more extreme and virulent.”¹³⁴ Future race scientist Rushton, then a graduate student, witnessed the assault on Eysenck. When faced with his own protestors and dissenters nearly two decades later, Rushton recalled how Eysenck did not strongly pursue race differences research after his attack, and considered the “taboo on race” to be one of the severest moralistic impositions across the span of all human history: “There is no parallel like it in history. Not the Inquisition, Not Stalin, Not Hitler. Nowhere else has there ever been for so long a time such a taboo topic”.¹³⁵ Of note in Rushton’s outrageous litany is his ordering of dictators: Stalin outranking Hitler in atrociousness is emblematic of hereditarian psychology’s focus on the terrors of left-wing fascism

Patriotic Psychology vs. Left-Wing Fascism

During the early days of rising fascism in Europe that would lead to the atrocities of WWII, many neo-Freudian and Marxist scholars like Wilhelm Reich and Erich Fromm became interested in explaining the political developments in Germany (Samelson, 1986). Members of Frankfurt’s Institute for Social Research were recognizing that, despite rising poverty, people were either aligning with anti-Communist leaders or becoming politically apathetic instead of creating a revolt against the capitalist system (Baars & Scheepers, 1993). After the Institute relocated to the USA in 1934, and members such as Theodor Adorno and Max Horkheimer secured funds to study anti-Semitism, Berkeley-area research would eventually lead to the

¹³⁴ Jensen to Eysenck. May 17, 1973. AJP.

¹³⁵ Rushton to Zuckerman. May 31, 1989. Box 17, NEP.

hugely influential 1000-page tome *The Authoritarian Personality* (AP) (Adorno et al., 1950).

AP was landmark of integrative research—bringing together psychometric methods, projective testing, and clinical interviewing for socially relevant theory (Samelson, 1986, p. 194). The project implied an uncomfortable question: is America susceptible to fascism as seen in Europe? Though very popular among psychologists, especially as American social psychology at the time was a welcome area for studying prejudice and eventually conformity (e.g., Samelson, 1978), AP received legitimate methodological critiques, along with follow-up research and debates from varied psychologists (some of which was collected in Christie & Jahoda, 1954). Crucially, AP was published during a turning point in American politics: McCarthyism and the Red Scare was entering the university system. For example, in 1949 the University of California began requiring employees to sign anti-Communist oaths. In the case of M. Brewster Smith, an AP sympathizer, he was subpoenaed and blacklisted from the National Institute of Mental Health (Roiser & Willig, 2002, pp. 77–8).

Mandated anti-Communist positions began shaping reactions to AP. Political scientist E.A. Shils, who had initially praised AP, reinvented himself—like other intellectuals—from a liberal thinker to a neo-conservative proponent standing against the threat of Communism (Roiser & Willig, 2002, p. 80). Though a British psychologist, Eysenck joined the roiling debates and published research strongly contesting AP as a one-sided look at right-wing authoritarians—they had forgotten about the even worse fascists on the political left. Drawing from unpublished doctoral research of one of his students (a method for his research production) claimed English Communists demonstrated more potentiality for authoritarianism than English Fascists. Additionally, harking back to the personality typing of William James and Joseph Jastrow, Eysenck claimed a tough/tender-minded bipolar personality factor better accounted for the

phenomenon at hand (Eysenck, 1954). Though he used behaviourist explanations for this and the adjoining radical-conservative attitudinal component, he would use his extraversion dimension as way to explain the inherited predisposition toward tough- and tender-mindedness (Roiser & Willig, 2002, p. 83).

Eysenck's work on the psychology of politics would mark yet another area of highly controversial and contested research within his "rebellious" oeuvre (see Buchanan, 2010, ch. 7). He understood AP as a work consistent with the egalitarian Zeitgeist that also incorporated his nemesis of psychological expertise: It was a work written by "refuges from Nazi Germany combining neo-Marxism ... with a rather old-fashioned type of psychoanalysis" (Eysenck, 1990, p. 86). He also recollected that critiques of his book on political psychology was his first encounter with "the hostility and hatred of the militant left" (Eysenck, 1990, p. 85), pointing out that economist Friedrich Hayek had argued a similar point about Fascists and Communists having a common authoritarian bond.

Hayek's libertarian political writings, along with the similarly coloured epistemological writings of his colleague Karl Popper, certainly impressed psychologists fearing leftist authoritarians and its oppressive PC culture. Popper, particularly his work on falsification and the demarcation of science, has long been a touchstone for psychologists (see Chapter 4). Less acknowledged are Popper's libertarian views on social change and the adjoining distrust of Marxism (e.g., Popper, 1945). For Popper, especially with rising left- and right-wing authoritarianism within Austria and Europe, guiding people toward freedom and democracy precluded directly changing individuals. Better suited toward maintain personal freedom was piecemeal form of social engineering that did not sink into the pitfalls of Marxist engineering and Utopian thinking (Derksen, 2017, pp. 105–107). One of Popper's earliest writings along

these lines was published in Friedrich Hayek’s journal *Economica* (Popper, 1944a, 1944b, 1945).¹³⁶

Hayek, Popper’s likeminded Austrian colleague and architect of neoliberalism, held similar views against any social interventions impinging on individual freedoms and consequently interfering with a free market. Echoing Popper’s stance against Marxist prophecy and historicism, Hayek thought centrally-planned economies failed because “the totality of resources that one could employ in such a plan *is simply not knowable to anybody*” (Hayek, 1989, p. 85). In a time of rising conservative discourse over the socialist PC cesspools that American universities had become, Hayek’s held congruent views about the intelligentsia: “The higher we climb up the ladder of intelligence, the more we talk with intellectuals, the more likely we are to encounter socialist convictions” (Hayek, 1989, p. 53). As controversy over race psychologist Rushton’s work—and the wider Bell Curve scandal of the 1990s—broke out, hereditarian psychologists would continue to amplify the conservative distortions of victimhood and academic freedom in the face of a “socialist” culture and an elite, presumably Marxist academy.

Rushton and the Corruption of Academic Freedom

As the Bell Curve scandal and the wider culture wars over PC ideology roiled on, a Canadian-based psychologist became the public representative of the scientific soundness of race research. J. Philippe Rushton¹³⁷, as previously touched on, took on the role of public figure in

¹³⁶ To which Hayek apparently made substantial edits (Derksen, 2017, p. 106).

¹³⁷ Not much in the way of trustworthy scholarship on Rushton’s personal life and career exists. The only published book-length treatment (closer to a cheaply printed pamphlet) is Dutton (2018). It is a bizarre biography, often portraying Rushton in a very harsh light—insinuating and exploring aspects of his sexual life, mental health, and (racial) ancestry. Unlike most insider biographies of fellow colleagues and institutions, this book is—in the words of Richard Lynn, a noted Pioneer Fund defender and one of the few to provide Dutton’s book with a quote for the blurb—a “warts and all” biography. Dutton apparently had access to an unpublished memoir of Rushton’s, in Lynn’s sole possession. Given rumours of Rushton’s embezzlement of Pioneer Fund money while he was Director, and Rushton’s general proclivity for bad press, Dutton’s biography appears to be more so an example of half-

representing BG and *The Bell Curve* position after the death of that book's co-author, Harvard psychologist Richard Herrnstein. But Rushton had already stirred up a significant amount of clamour over his own research. Some of his earlier work on evolutionary and sociobiological approaches to race and psychology had gone mostly without incident. Contrary to the public backlash Canadians would unleash a decade later, his 1970s work was welcome—especially within the field of theoretical psychology. Rooted in the projects and initiatives of psychologists like Joseph R. Royce, the University of Alberta had become a home base for theoretical psychology and its wide-ranging topics.

During the 1970s, meetings in Alberta among major psychologists from North America and Europe resulted in theoretical psychology conferences and publications. As one of Royce's many research interests included BG and factor analytic methods, theoretical psychology welcomed psychologists like Cattell, Eysenck, and Phillip Vernon (Dawson et al., 2019, pp. 94–95). Just before the storm of controversy swept in around him, Rushton would feature alongside Jensen and Vernon in the *Annals of Theoretical Psychology*, where he wrote about viewing questions of race and genetics in the new framework of sociobiology (Rushton, 1984). Popularized in the work of E.O. Wilson, sociobiology and its adaptations persists as a controversial neo-Darwinist meta-theory encompassing human biology and social science (Caplan, 1978; Wilson, 1975). Though contemporary critics viewed its introduction as another hard-leaning hereditarian framework to promote the genetic determination of human capacity, later critics would reflect on its position in the wider history of “biosocial” discourses and allied ideology, as well as Wilson's use of expertise rhetoric (Lyne & Howe, 1990; Perry, 1980; Rabinow, 1999). Defenses of sociobiology and related fields persist in academia and the popular

hearted, crypto-eugenicist public relations job than a genuine work of historical scholarship.

press, where authors fall back on segregationist fictions of an oppressive “equalitarian fiction” (Wade, 2014; c.f., Roseman, 2014).

The tipping point for outrage over Rushton’s research, in his view, would not happen until after a presentation on his r/K theory of race differences given at a symposium on evolutionary theory at an early 1989 meeting of the American Association for the Advancement of Science (AAAS).¹³⁸ Rushton’s research incorporated both cranial and genitalia measurements to support his claims of differences among (arbitrarily defined) races in inherited intelligence and mating habits. Unsurprisingly, Rushton was interested in expanding his r/K systematization of racial differences into other fundamental traits—namely Eysenck’s three-factor model, paying most attention to neuroticism and extraversion in explaining differences among “Caucasians,” “Mongoloids,” and “Negroids” (Rushton, 1985).

The press picked up his conference talk, and Rushton began his ride on the road to being ostracized. While the President of the University of Western Ontario (Rushton’s employer) insisted on protecting Rushton’s academic freedom, Rushton bemoaned that: “social activist groups organized sit-ins and demonstrations” insinuating his support of South African Apartheid; many of his colleagues “preferred not to speak to me, averting their eyes as they passed in the hall”; four of his graduate students found new advisors; he was bought out of being a continuing co-author on an introductory psychology textbook series; and the Ontario Provincial Police [OPP] was investigating him “believe it or not.”¹³⁹

Rushton, much like Eysenck or Shockley, did not shy away from the public spotlight. He accepted geneticist David Suzuki’s challenge to a public debate held in front of a crowd of 2000

¹³⁸ Rushton to Zuckerman. May 31, 1989. NEP. Rushton’s timeline of events is also recounted in detail in a letter to Andre Ryerson (November, 27, 1989), found in Box M5312, Folder 28, AJP.

¹³⁹ Rushton to Zuckerman, 1989, AJP.

at Rushton's school and aired live on television for 2 hours.¹⁴⁰ He took very little of his criticism seriously and thought the entire hubbub about genetically determined racial differences to be a rejection of evolutionary science en masse. Regarding his OPP investigation, Rushton thought if it ever went to trial it would be Ontario's Scopes Monkey Trial and make the province look "as ridiculous as Tennessee looked in the 1920s."¹⁴¹

By the time he agreed to appear on *The Geraldo Rivera Show*—alongside others like William Shockley, anti-eugenicist BG psychologist Jerry Hirsch, and a "black man whose name I forget"¹⁴²—colleagues were becoming critical of his daring public pronouncements. Once five BGA officers wrote an open letter to their membership about Rushton's "dubious" theories that only "fuel the fires of prejudice," Rushton had decided he had few he could still trust to uphold academic freedom.¹⁴³ As he put it: "One expects it from 'social activists' and 'social anthropologists' but not from those supposedly committed to scientific values of truth above those of 'compassion' and 'social justice' and other catch-phrases often used nowadays."¹⁴⁴

With criticisms coming from all sides, Rushton thought his persecution was "becoming a witch-hunt."¹⁴⁵ Even fellow Canadian psychologists, who seemed to briefly tolerate his work—especially as it pertained to the promising theoretical insights of a grand framework for human development such as sociobiology—were turning on him in academic venues (e.g., Weizmann et al., 1990). Rushton bemoaned the liberal-academic defense of a writer like Salman Rushdie meanwhile they were "eerily silent about the political debacle surrounding me."¹⁴⁶ He viewed

¹⁴⁰ CBC News *The National* archive clip, Feb 8, 1989, available here: <https://www.cbc.ca/archives/entry/the-rushton-suzuki-debate>

¹⁴¹ Rushton to Zuckerman, May 31, 1989. NEP

¹⁴² Rushton to Zuckerman, NEP.

¹⁴³ BGA officers (DeFries, Fulker, Vandenberg, Carey, & Wilson). Open letter to the Behavioral Genetics Association Membership. February 17, 1989. Box 17, NEP.

¹⁴⁴ Rushton to Zuckerman, May 31, 1989. NEP.

¹⁴⁵ Rushton to Zuckerman, NEP.

¹⁴⁶ Rushton to Zuckerman, NEP. (Yes, Rushton seemed to believe his situation was comparable to that of celebrity

himself as a victim of “extreme egalitarianism” and “reverse-McCarthyist tactics.”¹⁴⁷ Indeed, Rushton would soon after explicitly invoke the “equalitarian dogma” of pro-segregationist and past APA president Henry Garrett (Rushton, 1994b). The idea that elite, Marxist, and PC academics had mounted a New McCarthyism against the right was a popular notion in the early 1990s (Robbins, 1991). With hurt persecution and stinging resentment at an all-time high—two central emotions within the politics of victimhood—Rushton and likeminded scholars blamed all things “social” and “equal.”

In the case of wider neoliberal and conservative sentiments, the economist Hayek also held a deep disdain for the very word “social” and anything related—a derision several trait and hereditarian psychologists would also adopt. Labelling it a “weasel word,” Hayek saw “social” as a “sort of guide-word for rationalist morals intended to displace traditional morals” (Hayek, 1989, p. 14). He also thought that the phrase “social justice” was one of the worst uses of the word as it represented a “distributive justice” irreconcilable with a competitive market and individual freedom (Hayek, 1989, p. 118). Such an “anti-capitalist ethic” was the province of those who were “[p]retending to be lovers of freedom” (Hayek, 1989, p. 119).

Similar claims of leftist ideology clouding the minds of hereditarian critics coloured the wider 1990s controversies over race and intelligence. At the 1995 annual meeting of the Behavior Genetics Association, the association’s President Glayde Whitney¹⁴⁸ organized a symposium on Group Differences including the work of Rushton and Jensen. Despite some objections within BG to Whitney’s encouragement of research pursuing race differences research, Whitney simply claimed that anyone who disagreed with his position was suffering

writer Salman Rushdie and that he deserved similar adoration as a victim of intellectual oppression.)

¹⁴⁷ Rushton to Zuckerman, NEP. (It’s a long letter.)

¹⁴⁸ Notoriously, Whitney would provide a foreword to the autobiography of Ku Klux Klan Grand Wizard David Duke released in 2000 (a book not worth citing unless necessary).

from “Marx-itis” (Holden, 1995; Panofsky, 2014, p. 2).¹⁴⁹ Individual differences psychologists thought a (presumably Marxist) ideology of equality blinded critics to the reality of genetically determined race differences in psychological traits—even if a critic was a respected zoologist or geneticist. The allegedly dubious politics of critics were synonymous with runaway emotionality and socialist-sentimentality; they valued humanity over the sterile scalpel of the rational scientific enterprise (Jackson Jr, 2006).

In the common parlance during the 1990s debates over PC and academic freedom, the varied epithets (like egalitarian, equalitarian, environmentalist, situationist, socialist, Marxist, Lysenkoist, situationist) for any critic who was being too political for scientific debate were often reduced to (or coded as) “egalitarian.” Of course, egalitarianism had long been a target for both hereditarian and eugenicist scholars, especially in the sense of a fictional equality among individuals and races (Winston, 1996). Echoing the long-running tensions between the constitutional equality and idealized meritocracy of American democracy, as psychological testing long has (Carson, 2007), Rushton rejected egalitarianism in favour of differential psychology because “people must be judged on their merits.”¹⁵⁰ But it had spread out toward the broader conservative discourse over PC and academic freedom, including in the thinking and writing of Paul Meehl.

Near the end of his life, the ever-open-minded Meehl had seemed to become fully convinced of the conservative warnings of the oppressive leftist PC culture. He had likely long been exposed to deeply libertarian systems of thought, such as his long-running fascination with

¹⁴⁹ Panofsky makes the crucial point that in order to have had a controversy within the BGA in the 1990s, there had to have been a plurality of political positions among its membership. As this dissertation focuses on the ardently hereditarian trait (intelligence/personality) psychologists within BG, such a plurality is less evident in that cross-section of BG’s history.

¹⁵⁰ Rushton to BGA membership. April 27, 1989. NEP.

the writings of Karl Popper. Among the many areas where Meehl and Popper concurred were that society benefitted most from “piecemeal social engineering by incremental steps than by violent revolutions or grand schemes.”¹⁵¹ Like many Americans, Meehl also lost faith in his government after the Vietnam War. After the war he never voted in a presidential election again and campaigned for others to follow suit (Peterson, 2005, p. 35). He recalled with disgust that the government “lied and lied and lied, killed 58,000 young men for nothing except their own *libido diminandi*. Yech!”¹⁵² Nevertheless, when faced between his corrupt capitalist government and the communist alternative, Meehl asserted: “I much prefer those clowns in Congress to what I am currently reading about the growth of terror & suppression in USSR from 1917 on.”¹⁵³ In the 1960s and ‘70s, an era of destabilizing racial and sexual hierarchies, anti-statism (once within the purview of the Left) was unified with other anticommunist and emerging New Right sentiments, such as economic liberalism and the social conservative protection of traditional values (McGirr, 2001).

Indeed, much like his changing views on government vis-à-vis the Vietnam War, Meehl—echoing the recollections of Popper and even Ronald Reagan—recalled naïvely associating with socialists in his youth: “when the fact showed socialism wasn’t efficient, I abandoned it—a purely rational change ... I did not join the right-wing Christers.”¹⁵⁴ Like Popper before Meehl, seeing socialism as a vapid part of youthful idealism was also true for Eysenck. While Eysenck apparently supported socialism for “powerless workers” in pre-Hitler Germany, he was on the side of “bosses” against trade unions in 1970s Britain (Eysenck, 1990, p. 22). In his later career, Meehl would find refuge not in the Evangelical Right, but certainly

¹⁵¹ From Leslie Yonce’s endnote in Waller et al. (2013, pp. 500 - 501).

¹⁵² Meehl to Peterson. November 9, 1996. From Peterson (2005, p. 34).

¹⁵³ Meehl to Peterson. December 15, 1998. From Peterson (2005, p. 78).

¹⁵⁴ Meehl to Peterson. April 25, 1999. From Peterson (2005, p. 102).

among conservative writers. He had worked with James Q. Wilson—a think-tank political scientist and popular author who promoted hereditarian explanations of criminality (Wilson & Herrnstein, 1985; also, Novak & Wilson, 1986)¹⁵⁵—and thought he was “one very smart, clear-headed man with a sensitive shit-detector.”¹⁵⁶

Meehl also attributed his admiration for mainstream conservative ideas and their proponents like Wilson to his self-described “state-phobia.”¹⁵⁷ His libertarian and anti-statist values, galvanized by the Vietnam War, meshed well with the conservative flurry over PC and egalitarianism. His long-time colleague and correspondent, Donald Peterson, worried that not being able to publicly oppose Affirmative Action due to “reverse discrimination” would lead to a “backlash” where “all will suffer.”¹⁵⁸ Meehl believed “reverse discrimination” was a main cause of racism, and he strongly opposed it on libertarian grounds, claiming it “[v]iolates the statute ... violates the XIV amendment [and is an example of] Bad social engineering.”¹⁵⁹ Meehl’s libertarian values and distrust of PC initiatives would carry over into his final unfinished and unpublished major work, *The Seven Sacred Cows of Academia*.¹⁶⁰ Resembling the conservative rantings of a cantankerous and victimized conservative academic, *Sacred Cows* includes Meehl’s recollections of encountering pro-diversity student initiatives on the University of Minnesota’s campus in section entitled “College for Everybody:”

I recently saw a huge poster on a college bulletin board that said, “CELEBRATE DIVERSITY.” I understand what it means to celebrate the Fourth of July, to celebrate Mass, or even one’s birthday. But given that people have different colored skins or eat

¹⁵⁵ The former of which was co-authored with *Bell Curve* writer Richard Herrnstein. Eysenck also took notice of Wilson’s widely acclaimed book on morality (Eysenck, 1994).

¹⁵⁶ Meehl to Peterson. March 26, 1997. From Peterson (2005, p. 53).

¹⁵⁷ Meehl to Peterson. March 26, 1997. From Peterson (2005, p. 53).

¹⁵⁸ Peterson to Meehl. November 27, 1991. From Peterson (2005, p. 9).

¹⁵⁹ Meehl to Peterson. December 12, 8. 1991. From Peterson (2005, p. 11).

¹⁶⁰ Currently available here: <http://meehl.umn.edu/publications/seven-sacred-cows-academia>

different favorite foods, that this is an occasion for celebration puzzles me. Nor do those who talk about it make clear whether people should make an active effort to think and act like those who differ from them in ethnic, class, or other origin; or whether they should cultivate their own subgroups' distinctive features and take pride in them; or perhaps both? Who knows. (Meehl, 1997/2008, p. 78).¹⁶¹

For Meehl, the only form of diversity worth celebrating “forces itself upon the attention of any observant, rational person and which cuts across such biological and social variables as race, sex, geography, language, or class” (Meehl, 1997/2008, p. 78).¹⁶² He bemoaned that “we don’t usually celebrate” the diversity of group differences which “some muddleheads prefer to sweep under the rug” (Meehl, 1997/2008, p. 78). Meehl had long pursued the biological aspects of psychological traits and disorders. For example, his well-known work on schizophrenia. Unlike less reflective figures associated with BG and hereditarian psychology, like Eysenck or Rushton, Meehl spent a significant amount of time and energy trying to unravel the ontological knot of psychological categories (e.g., Meehl & Yonce, 1994). Yet his libertarian politics would ever shine through in those works (Meehl, 1992)—piquing the interests of Meehl’s colleague and Big Five researcher Lewis Goldberg, who wondered about the realness status of Meehl’s “frenzied egalitarianism” category.¹⁶³ Even though Meehl did not accept factor analysis as an adequate method for inferring “real latent entities,” he would accept *g* (general intelligence) due to the “converging evidence [like] twin data, even the recent brain size & other physical correlates”¹⁶⁴—the latter evidence source perhaps alluding to Rushton’s work.

¹⁶¹ See *Sacred Cows* footnote 158.

¹⁶² See *Sacred Cows* footnote 158.

¹⁶³ Lewis Goldberg. Letter to Paul Meehl. March 31, 1992. Available here: <http://meehl.umn.edu/files/goldberg1992letterpdf>

¹⁶⁴ Meehl to Peterson. December 11, 1999. From Peterson (2005, p. 138).

Despite Meehl's elaborate theorizations and deep soul-searching over the ontology of psychological categories, his admiration and interest in hereditarian science alongside his libertarian politics would ensure Meehl's place among the long-term public supporters of the academic freedom of race psychologists. Jensen regarded Meehl as a (relatively) silent ally: "Meehl is very sympathetic to the recognition of genetic factors in all kinds of human differences," but "he refused to write anything in this area on the grounds that the opposition to genetical thinking is so idiotic and militant that Meehl wants to be able to carry on without having his peace disturbed."¹⁶⁵ Seemingly having no genuine interest or belief in eugenicist agendas, Meehl would still have a mild Nazi-apologist moment later in his life. While proclaiming Jung as a "clever muddle-head"¹⁶⁶ who might occasionally perceive a "deep truth," Meehl's relentless intellectual openness also prompted him to think that perhaps even Hitler's *Mein Kampf* contained "some social truths that many sane people missed."¹⁶⁷

Meehl had long defended the academic freedom of hereditarian psychologists: from adding his signature alongside respected psychologists in the 1972 resolution on hereditarian approaches (Page, 1972) to adding his signature alongside a less reputable group of psychologists in the *Wall Street Journal* in 1997. Shortly after the *WSJ* editorial, the controversy over APA recognizing Cattell with a Lifetime Achievement Award prompted Meehl to pen a piece on scientists, ideology, and legacy. He claimed "these days of political correctness" demanded such reflections (Meehl, 1998, p. 1123; see also Tucker, 2009).

Academic freedom (even that of racist psychologists) was the leitmotif of the numerous letters of support sent into the University of Western Ontario in defense of Rushton's work.

¹⁶⁵ Jensen to Eysenck. January 18, 1974. Found in Box M5311, Folder 12 "Eysenck Correspondence," AJP.

¹⁶⁶ A favourite insult of Meehl's that he lobbed at "unintelligent" scholars (i.e., those who disagreed with him).

¹⁶⁷ Found in Peterson (2005, p. 142).

Defenders of Rushton's rights as an academic included: Thomas Bouchard; Raymond Cattell; Hans Eysenck; James Flynn; Jeffrey Gray; Richard Herrnstein; Arthur Jensen; Richard Lynn; Robert T. Osborne; Roger Pearson; Robert M. Stelmack; James Q. Wilson.¹⁶⁸ Also among the supporters was sociobiologist E.O. Wilson, who had also reached out to the National Association of Scholars (NAS) over his concerns that Rushton was "being punished for his ideas, and his ideas were being effectively silenced."¹⁶⁹ The NAS is one of several conservative advocacy groups that claim to fight for the values of academic freedom.

Those holding any real power over Rushton's employment as an academic largely supported Rushton's freedom to conduct racist research. Even in his 2012 University of Western Ontario (UWO) staff obituary, Rushton's was portrayed as a martyr for the academy's integrity: "Distressingly, many interested parties, even faculty members themselves, seemed oblivious to the essential role that academic freedom plays in the life of scholarly work in general."¹⁷⁰ Not until the summer of 2020 would UWO recant their statements on Rushton's academic freedom and formally denounce him as "the largest recipient of Pioneer Fund grants at the time of his death" who conducted "racist and flawed studies, sometimes without appropriate ethics approval."¹⁷¹

Many psychologists with vested interests in the freedom to pursue hereditarian, genetic, and sociobiological lines of inquiry have formally supported such academic freedom groups. In Canada, likely in reaction to the controversy around Rushton, his departmental colleague Donald

¹⁶⁸ All letters of support collected in a custom-made bounded document entitled "On Rushton, Race and Academic Freedom: Responses from the International Academic Community" found in Box No. M5312, Folder 28 "Rushton," AJP.

¹⁶⁹ Edward O. Wilson. Letter to Stephen H. Balch. November 6, 1989. Found in Box No. M5312, Folder 28, AJP.

¹⁷⁰ Available here:

<https://web.archive.org/web/20190430025846/http://psychology.uwo.ca/people/faculty/remembrance/rushton.html>

¹⁷¹ See <https://psychology.uwo.ca/people/faculty/remembrance/rushton.html>

Jackson helped establish The Society for Academic Freedom and Scholarship (SAFS).¹⁷² SAFS's American counterparts include the conservative-leaning Heterodox Academy (HA)¹⁷³ and the ostensibly non-partisan FIRE. Posts on the HA's website include a blog identifying the “cosmic egalitarianism” endemic PC critique, noting Rushton among other unfairly persecuted psychologists.¹⁷⁴ Among the Advisory Council for Fire sits Canadian-American evolutionary psychologist and psycholinguist Steven Pinker.

Pinker's fame is directly related to conservative psychology's pushback against “frenzied egalitarianism.” Within his immensely popular book *The Blank Slate: The Modern Denial of Human Nature* (2002), Pinker positioned a Standard Social Science Model that necessitated an extremely egalitarian past—ideas which historians and other scholars have contested (Levy, 2004; Winston, 2006). At its worst, cognitive and evolutionary psychologists defend racialization by arguing it requires further research within their fields—that social scientific and humanities accounts of race must be supplemented with an essentializing human scientific framework (Jackson Jr, 2017). Outside his psychological research, Pinker's historical accounts bear a striking resemblance to the narratives around the PSC: situationist and environmental psychology getting in the way of trait psychology's progress, including its discovery of the FFM.

Though Pinker identifies as a political liberal, his work promotes a vision of social and economic liberalism most often within the purview of conservative writers (Flaherty, 2019). In addition to railing against oppression at the hands of cultural relativists, Pinker has used his platform to assert his own variation of the “end of history.” Even in the face of a complex and precarious post-9/11 world facing extreme environmental and political crises, Pinker is more

¹⁷² Their website: <http://www.safs.ca>.

¹⁷³ HA's publicly listed donors include the conservative think-tank The Triad Foundation: <https://heterodoxacademy.org/our-donors/>

¹⁷⁴ See: <https://heterodoxacademy.org/a-social-science-without-sacred-values-part-2-of-an-article-summary/>

interested in demonstrating that suffering and death has only decreased (strictly, quantitatively speaking) and enshrining the rationality of a mythologized Enlightenment (Pinker, 2012, 2018). Echoing criticism lodged against Fukuyama’s work, scholars have sharply criticized Pinker’s popular books on the history of violence and the Enlightenment for omitting the costs of “progress” (Bell, 2018; Goldin, 2018, 2018).¹⁷⁵ More broadly, post-9/11 scholars have come to view previous visions of an “end of history” as a falsehood—seeing Fukuyama’s “naïve triumphalism” as an ideology of progress resembling “the missionary projects of past empires” (Wright, 2004, pp. 6–7). Pushing against the conservative championing of progress with evidence of economic and social instability, others argue for a “return of history” (Welsh, 2017). Whether in matters of political economy or psychology, finality in the present presumes a past of continuous and incontestable progress.

Chapter 5 Conclusions

Throughout the end of the twentieth century into this one, conservative versions of academic freedom were often woven together with broader libertarian disdain for social intervention, statism, and the PC overthrow of the academy. As Panofsky (2014) points out in his history of controversy in the transdisciplinary field of BG, resting on arguments of academic freedom helped legitimate hereditarian projects—even those few who were deliberately (or uncaringly) involved with the postwar “reform” eugenics. As a younger generation of personality psychologists expanded their taxonomic work into the FFM, they would benefit from the apolitical and disinterested scientific status that previous trait psychologists had argued for their (often explicitly racist) work. FFM researchers could do so while simultaneously downplaying or

¹⁷⁵ Controversy over Pinker—truly one of the present day’s highest profile psychologists—has recently grown given his association with the notorious Jeffrey Epstein (Flaherty, 2019).

severing ties to their disciplinary heritage.

In this way, the move from previous taxonomic systems—like Eysenck’s Big Two then Big Three—toward the FFM helped personality psychology inflect from a lineage of extreme right-wing and often eugenicist-inclined trait psychologists toward an ostensibly respectable science of personality. A science of the person that still operated on long-established principles of quantification, classification, and validation. Within the contests over academic freedom in the realm of race and intelligence research, the study of all other traits along other axes of difference seemed harmless—even worthwhile. Personality provided an avenue to continue researching minority groups: “unlike most cognitive measures, personality scales tend to have little differential impact on protected groups, and thus they are less prone to raise discriminatory concerns” (Goldberg, 1993, p. 32).

Whether the FFM—packaged with an origin myth of consensus, apolitical status, and the end of personhood—won out among personality models is a secondary issue to the true success story underneath. This history is really the continued success of hereditarianism and its concomitant, conservative politics of the academic freedom to study individual and group differences—moral and social consequences be damned. While interactionism seemed to resolve the internal PSC, interactionism or interdependence of gene/environment and person/situation was already the common postulate before Mischel’s apparent salvo. And, for the most extremely hereditarian trait psychologist, interactionism (before and after the PSC) seemed to be a token acknowledgement of environmental impacts on the person, echoing the “positive” eugenics of the 1930s.

Amazingly, within academic outlets and venues committed to a dubious version of academic freedom, there is still space for the most extreme of psychologists. Despite years of

research discrediting the scientific study of race and psychology, it persists in varied guises (Winston, 2020). During the last leg of his career, Rushton published several articles arguing for a single dimension of personality: The General Personality Factor (e.g., Rushton et al., 2008; Rushton & Irwing, 2009). His argument for a monolithic personality super-super-trait was largely taxonomic: scaffolding on years of previous personality research into the FFM and the gradations of statistically-discovered factors (actually, meta-traits, facets, aspects, and other theorized components presumed to represent real latent variables) lying above and below the central five dimensions of personhood.¹⁷⁶ Rushton’s work on a general personality factor loaded with evolutionary explanatory power was consonant with the still-running research on the general factor of intelligence (*g*). For Rushton—like Cattell—inequality (ensuring meritocracy) was simply another “manifestation of a universal hierarchy in intelligence, personality, civilization achievements, family stability, and progressing to social order” (Panofsky, 2014, p. 1).

Joining earlier conservative commentators like James Q. Wilson, and conservative trait psychologists like Cattell, there is today another iteration of rationally-grounded morality projects: Steven Pinker, Sam Harris, and most recently (in a development eerily reminiscent of Rushton’s rise to fame) Canadian psychologist Jordan B. Peterson—our current generation’s defender against the PC oppressors of research enshrining gender (rather than race) realism. Even though evolutionary and trait psychologists have worked hard to discredit critiques of their work as irrational and political, their public outreach projects have been deliberately prescriptive. In the vein of Galton’s religion-via-science, or Cattell’s morality-via-science, popular writers

¹⁷⁶ In a numerological twist, some personality researchers have once argued for a Big Two; though this time, these “meta-traits” lay above and grouped the FFM. The Big Two are labelled alpha and beta, or plasticity and stability (e.g., DeYoung, 2006; DeYoung et al., 2002; Strus et al., 2014). Others doubt the psychological reality of such higher-component factors or meta-traits, just as Rushton’s GFP has received technical criticism.

(often famous psychologists) hold a steadfast faith in reason and science alone shaping personal and societal prescription.

Yet, much like invoking Popper in delineating scientific practice from not, these science/morality projects share a certain degree of engagement with a deeply libertarian argument against Marxism and all social engineering. At the same time, this personal liberalism and its adjoining principles of equality is often symbiotic with its counterpart meritocratic vision that depends on inequality. Measure of merits—whether in the form of intelligence or personality testing—can both help a meritocratic republic organize while further legitimating its violent ideology of human inequality (Carson, 2007; Teo, 2008, 2010). Among the current batch of celebrity psychologists and other commentators, beneath the skin of their innocuous and logical prescription for heeding scientific research when considering the moral life of individuals and society, there resides the double-helix of hereditarian science and conservative values: a combination predisposed to cancerous consequences.

Closing Thoughts

On the Political Methods of Personality

One evening in the early spring of 2019, which happened to fall on the Christian holiday of Good Friday, 3,000 ticket-holding audience members—some who had reportedly paid \$1,500 for a single ticket—gathered inside the Sony Centre for the Performing Arts in Toronto, Canada. The high-paying audience members were not in attendance to see a musician or a comedian or panel of podcasters. They were there to witness what had been billed “the debate of the century” on the topic of “Happiness: Capitalism vs Marxism” (Marche, 2019). Purportedly on the side of Marxism was Slovenian philosopher Slavoj Žižek, who has written on some of the most provocative figures in history, from Lenin to Lacan. He is a rarity among philosophers, as Žižek’s name and unmistakable oratorical style is known well beyond the pages of academic journals. More certainly on the side of Capitalism stood an academic whose overnight celebrity was at its apex: Canadian psychologist Jordan Peterson.

Leading up to the debate, Žižek and Peterson’s slated sparring was hyped across the world: from local papers, to the international free press, and even to Russian news outlets with more opaque motives (Anonymous, 2019; Fraser, 2019; N.B., 2019). After the debate ended, a clear champion remained unclear; the audience, deflated. One reviewer in *The Guardian* sympathized with Peterson’s apparent lack of familiarity with Marxist literature, even its core text *The Communist Manifesto*: “I’ve been a professor, so I know what it’s like to wake up with a class scheduled and no lecture prepared. It felt like that” (Marche, 2019). On the other hand, a review in the conservative *National Review* claimed that Peterson in his first thirty minutes “dismantled *The Communist Manifesto* [and] cited the atrocities of the 20th century ... as

evidence of Marx being a ‘narcissistic thinker’” (Kearns, 2019). Still, that reviewer agreed, with perhaps a touch of crestfallenness, that because the debaters kept stressing how much they agreed with each other it “scarcely qualified as a debate.” An attendee who wrote about their experience at the debate agreed with many journalists: “discussion about happiness was displaced by the pessimism that both authors expressed ... both agreed that they were victims of a victimizing system, and that they should double down on their rejection of political correctness” (Serpe, 2019, p. 97).

As historian of fascism Timothy Snyder recently noted, among both Russian and (Russian-influenced) American politics, there is a “politics of eternity:” a perfectly looping, historical fiction of jingoist innocence facing off with the evils of the other side (Snyder, 2018). Apparently, perceiving oneself as a victim both connects and divides. Even Zizek and Peterson agreed on the fundamental terror and stupidity of political correctness. Whereas thirty years before their debate, the debate between the then-famous Canadian psychologist J. Philippe Rushton and still-famous geneticist David Suzuki featured no offerings of olive branches, Peterson versus Zizek was a piece of high-priced theatre performed by the (unexpectedly) politically well-met.

Nearing the end of this dissertation, for those who know him, it might be hard not to think about Peterson. While these closing thoughts have no critical analysis of Peterson’s New Right politics, as others have already provided that, the global stardom of a clinical/personality psychologist must be noted. Peterson is exemplary of several of this dissertation’s topics and themes. Firstly, his lectures and books—ranging from mythology to meaning-making—often invoke or stem from Jungian thought. Secondly, his academic publications (where he most often served as a secondary or tertiary author to others, such as his students) include several

psychometric articles refining the taxonomy of the FFM, examining the biological correlates of personality and intelligence, the interaction of bio-traits with drug dependence, and even the personality psychology of political beliefs (e.g., DeYoung et al., 2002, 2006, 2014; DeYoung, Hasher, et al., 2007; DeYoung, Quilty, et al., 2007; Hirsh et al., 2010; Peterson & Carson, 2000). He is a Jung-grounded clinical/personality psychologist who embodies the conservative politics of the hereditarian-trait approach.

As with most personality research, his publications were non-controversial. His popular press self-help books were also, initially, not read and criticized the world over—not to the degree they would become. Peterson’s rise to fame came in 2016, after his critique of the Canadian Federal Government’s proposed Bill C-16 appeared on his YouTube channel in a series of videos titled “Professor Against Political Correctness” (*Bill C-16*, 2017; Lynskey, 2018). Bill C-16 proposed adding gender identity and gender orientation to the Canadian Human Rights Act, in order to legally protect oppressed groups from discrimination on grounds of their identity. Peterson saw it as socialist oppression: PC culture, once again, run amok and censoring scientific truths. Even though journalists and others from Canada and beyond have explained away Peterson’s unfounded critique—even the professor who hired him at the University of Toronto regretted his decision and called Peterson “dangerous”—nothing seemed to stop Peterson’s soaring stardom (Cumming, 2016; Schiff, 2018). From appearing on immensely popular podcasts, along with news and late-night talk shows, for many people around the world Peterson became the face of reason in an age of “social justice warriors” and their terrorizing identity politics. Within the past year, Peterson’s first biography came out. The title: *Savage Messiah: How Dr. Jordan Peterson is Saving Western Civilization* (Proser, 2020), and you can easily purchase it online and likely from your local bookstore

For many psychologists, trait psychology depends on the traditional psychometrics of individual and group differences—despite the recurring critiques of the very logical and statistical soundness of their methods as standardly implemented (e.g., Borsboom et al., 2003; Hussey & Hughes, 2020; Lamiell, 1987; Michell, 2008, 2012; Molenaar, 2004, 2013). For a psychologist like Peterson, with racial categories having been (for most psychologists) discredited—or disallowed—the binary form of gender, rooted in biological justifications as race once was, is potentially the next “natural” group to be taken away. As described above, during the discipline’s shifting away from studying racial differences of any kind and sex differences in intelligence, personality afforded a space to secure the sex binary as a central axis of difference—even encasing gender itself in the pantheon of psychometric-traits. Presuming gender to be an uncomplicatedly “natural” and stable binary willfully ignores not only the feminist and historical scholarship someone like Peterson would wave away as toxic, leftist egalitarianism; it also ignores the critical work of scientists, including psychologists, on gender identity, genetics, and sexual development (e.g., Bem, 1974; Fausto-Sterling, 1999; Fine, 2017).

Yet, for Peterson, losing the designations of Man and Woman—and his right to denote anyone however he pleases—marks more than denying the grooves of nature carved at her joints. And it marks more than his alleged loss of freedom of expression and thought. His concerns reflect the worries of many other conservative thinkers and rabble-rousers. At stake for Peterson is a loss of humanity itself: individuality, tradition, hierarchy, order, and the right type of civilization.

Peterson’s politics are not hidden; he freely provides them on his website. Among his list of “Great Books,” Steven Pinker’s *Enlightenment Now* (2018) is now included.¹⁷⁷ His blog posts

¹⁷⁷ See <https://www.jordanbpeterson.com/great-books/>

include “Equity: When the Left Goes Too Far,” where he coined the acronym DIE: “Diversity, Inclusivity, and Equity.” For Peterson, DIE is the “mantra ... of the radical left types that dominate the humanities and social sciences.”¹⁷⁸ In light of the New Left gone mad with social control, Peterson even considered running as leader of Ontario’s provincial conservative party (Read, 2018). In another blog post, “The Great Ideological Lie of Diversity,” he claimed only the truly prejudiced look toward celebrating and prioritizing the “immutable characteristics that typify different groups, including race, sex, gender (because that is distinguished by those same ideologues from sex).”¹⁷⁹

This dissertation proposed a vantage point for seeing the history of personality psychology, particularly what became the dominant psychometric-trait approach. The Big Two—neurosis and introversion—as an organizational frame of the enormous mass of research and popular literature on personality was initially meant as a conceptual route to the story of the Big Five. Over the course of the twentieth century, Psychology disciplined itself through a process of domesticating psychoanalysis into something legible, controllable, measurable, testable, and (ideally) incontestably scientific (Davidson, 2017, 2018; Hornstein, 1992; Richards, 1987). Trait psychology and its psychometrics were essential to domesticating psychoanalytic notions—incorporating those notions into a tradition of hereditarian and eugenicist science that presumed the primacy of statistically-determinable, stable, innate traits.

While personality psychology, especially after the rise of the FFM, succeeded in the optics of distancing itself from its eugenicist heritage, the dynamics of sex and race have merely been repressed into the most latent of variables. Peterson belies this heritage. Personality psychology in the twenty-first century is supposedly an apolitical and disinterested human

¹⁷⁸ See <https://www.jordanbpeterson.com/political-correctness/equity-when-the-left-goes-too-far/>

¹⁷⁹ See <https://www.jordanbpeterson.com/blog-posts/lie-of-diversity/>

science. As we have seen, personality's history of the present suggests otherwise. Even without historical evidence, the obvious counter to the value-free vision of a science of the person would be that any field devoted to creating and discovering knowledge of humanity itself is necessarily, categorically, a moral and political project. When reform for the sake of social justice provokes someone like Peterson to deploy a rhetoric of reason and science to legitimate systemic prejudice, the ostensibly disinterested science of personality reveals its political undercurrents.

Personality psychology's implicit politics are not a foregone conclusion: Unlike inflexibly stable traits, a discipline's goals are plastic, fluid, and changeable. Hereditarian-trait psychology, which encompasses much of personality psychology in the present day, might seem irreversibly intertwined with conservative values. Can a science of difference persist in an ethical form? This dissertation suggests a reconsideration of personality's tacit values, such as whose "freedom" we are defending and what for. Decoupling reason, a genuine virtue, from social conservatism and libertarianism—the tensely twinned tenets of meritocracy—is necessary. Psychologists have fought for social justice before, and several continue to do so. Psychometrics have helped psychologists and many other practitioners understand and guide persons of varying abilities and needs. Like any scientific technology, the tools of personality and trait psychology need to be directed toward the promotion of humanitarian values. Unfortunately, these tools have too often served to promote the values of the powerful. The Cambridge Analytica scandal suggests the existential horrors of psychometrics-for-the-powerful: deliberately using tools of personality to covertly guide our political agency. Salient to that scandal too are the persistent psychoanalytic and unconscious aspects of personality—in this case, its persuasion.

Within psychology, prioritizing epistemic modesty over grandiosity—a reversal even the recent, wider calls for methodological reform embody—could provide a new guiding virtue

(Teo, 2019). Critical reflection is one route toward epistemic modesty; this dissertation has been my own contribution. The motivations of a critical history like the one you have just read are not dissimilar from any other scholarly or scientific inquiry: We want to reflect, explore, challenge, create, discover, and (in the best of cases) see the contemporary world anew. All that separates is process—and, maybe, our personalities.

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