

WHEN PSYCHOLOGISTS WERE NATURALISTS:
QUESTIONNAIRES AND COLLECTING PRACTICES IN EARLY AMERICAN
PSYCHOLOGY, 1880-1932

JACY L. YOUNG

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Abstract

This dissertation reshapes our understanding of the earliest years of American psychology by documenting the discipline's methodological plurality from its very inception. In tracing the use of questionnaires over the first half century of the discipline's existence as a science, I argue that a natural historical orientation, wherein collection, analysis, and categorization are central to the scientific enterprise, has been a persistent facet of the field. Manifested in a recurrent interest in collecting information on mental life, this natural historical perspective facilitated a moral economy of data, wherein the discipline's affect-laden norms and values sanctified the objects and practices of mass data collection. This in turn lent itself to the adoption of statistical analyses as a central component of psychological science. Although, at first glance, falling outside of the bounds of the mechanically objective practices that characterized the new psychology's laboratory endeavours, with their use of standardized instrumentation, projects with this orientation adhered to this form of objectivity in their own way. Seeking precise accounts of mental life, including information on its physical correlates, these enterprises engaged the public in collection practices in the field. Taking up subjects with widespread interest outside of purely scientific spheres – including child study, psychical matters, and dreaming – questionnaire projects had broad appeal. Undertakings with less popular allure deliberately and necessarily confined themselves to more restricted university populations. Issues of social relevance remained mainstays of this kind of research, but by the 1920s the public's relation to questionnaire research shifted so that they were no longer active participants in collecting activities. Instead, questionnaires were circulated in more restricted circumstances and their findings served as the basis for broad claims about the state of the public's mind. To do so effectively, I argue, practices of collecting with questionnaires shifted from thick to thin

description; no longer were rich descriptive accounts of mental life the aim of these endeavours. Rather, increasingly restricted ranges of information were accumulated, a process that culminated in the development of numerical Likert scales and the use of more sophisticated statistical analyses. Scales of this kind continue to dominate questionnaire research today.

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Introduction

Questionnaires are ubiquitous. Regular components of contemporary psychological research, they have also moved beyond scientific spheres to feature regularly in our everyday lives. Whether used to assess perfectionism or attitudes toward political candidates, these instruments now pervade the many domains of our existence. How is it that the questionnaire is now our preferred means of gathering information on any and everything? When and why did psychology adopt this mode of inquiry? What shape did early research with the method take? And what are the intricacies of practice that attended the use of this method?

This project tracks the use of questionnaires within American psychology over the first half century of the discipline's existence as a science. In tracing the questionnaire's early trajectory, I argue that a natural historical orientation, wherein collection, analysis, and categorization are central to the scientific enterprise, has been a persistent facet of the field from the very beginning. Manifested in a recurrent interest in collecting information on mental life, this natural historical perspective facilitated a moral economy of data, wherein the discipline's unspoken affect-laden norms and values sanctified the objects and practices of mass data collection. This in turn lent itself to the adoption of statistical analyses as a central component of psychological science. Although, at first glance, falling outside of the bounds of the mechanically objective practices that characterized the new psychology's laboratory endeavours, with their use of standardized instrumentation, projects with this orientation adhered to this form of objectivity in their own way. Seeking precise accounts of mental life, including information on its physical correlates, these enterprises engaged the public in collection practices in the field. Taking up subjects with widespread interest outside of purely scientific spheres – including child study, psychical matters, and dreaming – questionnaire projects had broad appeal. Undertakings with

less popular allure deliberately and necessarily confined themselves to more restricted university populations. Issues of social relevance remained mainstays of this kind of research, but by the 1920s the public's relation to questionnaire research shifted so that they were no longer active participants in collecting activities. Instead, questionnaires were circulated in more restricted circumstances and their findings served as the basis for broad claims about the state of the public's mind. To do so effectively, I argue, practices of collecting with questionnaires shifted from thick to thin description; no longer were rich descriptive accounts of mental life the aim of these endeavours. Rather, increasingly restricted ranges of information were accumulated, a process that culminated in the development of numerical Likert scales and the use of more sophisticated statistical analyses.¹

In documenting the collecting practices of early American psychologists, this study provides a counterpoint to the extensive historiography of the psychological laboratory and its role in legitimating the new psychology as a science (e.g., Capshew, 1992; Coon, 1993; Kroker, 2003; Morawski, 1988b). The rhetoric of experimentation and the locating of psychology as a laboratory-based science were crucial for the establishment of the field, and have been amply documented in disciplinary histories. The new psychology coalesced around the ability to render mental functions observable, notably innovations that allowed for the measurement of the speed of various mental processes (Benschop & Draaisma, 2000). Together with psychophysical investigations that directly linked physical stimuli with the psychological experience of such,

¹ This project is self-consciously not a history of each and every questionnaire-based research project in first decades of American scientific psychology. Hundreds of questionnaires were constructed, distributed, and compiled during the time period covered in this project. A complete accounting of each of these projects is not simply infeasible, but would not, in and of itself, prove particularly informative. Consequently, I address only a fraction of these questionnaire-based investigations and focus instead on the range of subjects inquired into, as well as the nuances of practice associated with these kinds of instruments.

psychology emerged as a science rooted in laboratory practice. Despite this self-conscious positioning of their work within the laboratory, psychologists retained interest in mental functions that could not easily be investigated via reaction time experiments or psychophysical studies. Although methodologically suspect to some, questionnaires provided a means of studying these otherwise unobservable mental states beyond the confines of the single, anecdotal case.²

Spanning the final days of Gilded Age America, continuing through the reform oriented Progressive Era, and ending in the socially turbulent times of post-World War I America, this account of questionnaire research shows how the discipline regularly sought to study topics of widespread interest. Although the laboratory is often emphasized as the defining feature of the discipline's establishment this was by no means the only site of scientific practice. Taking place in the field, broadly conceived, questionnaire research was especially well-equipped to address culturally popular issues of the day. Much of the questionnaire-based research I engage with is situated in the final decades of the nineteenth century, during the rise of the Progressive era. In part, the shape of scientific psychology at this time is a reflection of this period's overarching concern with social reform and widespread faith in science as the means of achieving a better world.³ This is seen most clearly in psychology's involvement in educational matters from the very beginning of the discipline. The field billed itself as the key to scientifically reforming educational practices in a way that would produce social benefit (e.g., Cattell, 1890). In doing so, it ensured the discipline a place in the national conversation and established the roots of the later applied psychology that took shape in the early twentieth century. Even in those instances where

² For an illuminating discussion of the role of the case in the psy-disciplines, especially the psychoanalytic case history, see Forrester (1996).

³ On this era in American history see Hofstadter (1955), Lears (1981, 2009), and Wiebe (1967).

direct intervention in prominent social issues was not the aim, as was the case with child study, those adopting the questionnaire method nonetheless engaged with subjects of popular interest and their attendant publics. The co-opting of cultural practices for scientific investigation is not unique to the American context, as Alexandra Bacopoulos-Viau (2013) documents a similar process in France with respect to the adoption of automatic writing as an object of psychological study.⁴ Psychology consumed the cultural practices surrounding it, making clear the porous boundary between cultural and scientific practices more generally.

To date, relatively little has been written on the history of the questionnaire within psychology, especially in its very early forms.⁵ Even for those individuals whose work has been amply documented – G. Stanley Hall, William James, Joseph Jastrow, and Mary Whiton Calkins – very little attention has been paid to the specifics of their questionnaire research.⁶ This kind of psychological research was oriented around practices of collection. Addressing this gap in the historical record, this dissertation investigates the myriad of ways in which collecting figured into the practice of psychology at the end of the nineteenth and into the early twentieth centuries. This history of collecting practices within psychology takes the questionnaire as a prominent collecting technology employed by the discipline during this period. Using questionnaires involved enlisting into the data collection enterprise a wide swath of individuals, most notably those who fell outside the privileged realm of the educated and elite. These individuals were charged with accumulating data via questionnaires crafted, publicized, and printed by other, nominally expert individuals. The success of these kinds of investigations was predicated upon

⁴ Bacopoulos-Viau (2013) also addresses the subsequent adoption of automatic writing into another cultural milieu, that of literary expression.

⁵ The most notable exceptions to this are Bordogna (2005, pp. 107–8), Danziger (1990, p. 75), Gault (1907), and Young (2012a, 2012b).

⁶ Among the most relevant sources on each are Bordogna (2008), Scarborough and Furumoto (1987), Pettit (2013b), and Ross (1972).

the labour of the former group. Yet, within this endeavour there remained a clear delineation of responsibility between the often socially diverse body of lay individuals involved in data collection and the experts charged with interpreting the mass of facts they accumulated.⁷ An expansion of our understanding of who the participants in early psychological research endeavours were, the positions they occupied, and the practices of their involvement is a central aim of this project.

Collecting information was, of course, only the first step. Following the amassing of a bulk of data on mental life was the challenge of sorting, classifying, and analyzing this material. This process, unlike collection activities, was largely confined to trained experts. Constructing a synthetic whole from what were initially masses of descriptive accounts was a difficulty not fully surmounted for decades. This depended in large measure on changes in the construction of questionnaires, as well as the development of tabulating technologies that could process large masses of newly numerical data. Underlying the adoption of the questionnaire method was the, not always clearly articulated, conviction that large amounts of data were scientifically valuable. Masses of information held an inherent appeal for many. Investigators employing questionnaires during the first decades of American psychology faced an incompatibility between the nature of the data they were collecting and their aspirations for its use. Accumulating rich, thick descriptions of personal mental experiences, they endeavoured not to understand this material at the level of the individual, but rather in terms of the mass. This aspiration left many overwhelmed in the face of reams of descriptive responses and ambivalent about the method's value to the discipline. Only with developments in the construction of questionnaires that allowed for the collection of directly numerical data was this difficulty surmounted.

⁷ A historical account of the category of the “expert” is provided in Gooday (2008).

Danziger (1985) has termed this orientation toward studying populations the Clark model in recognition of the preponderance of this kind of research at Clark University in the 1890s. In many respects this is misleading as these kinds of endeavours date earlier and involve a larger contingent of the psychological community than heretofore recognized. Missing too from this discussion is any recognition of the role of intermediary figures in the data collection process. Danziger's aim of explicating the relationship between subject and experimenter, while sufficiently constitutive of much psychological research, obscures the contributions of other kinds of individuals including the educators so much a part of the Clark model of research. At the same time, there are unremarked upon continuities between the Clark model and the laboratory-based Leipzig model, particularly in terms of emphasis on training and expertise as a necessary part of the scientific process, as well as the continued assignation of responses to specific individuals in at least some of the reports of questionnaire research. The restriction of "statistics" in his analysis to techniques for manipulating numerical data also obscures the discipline's long preoccupation with statistical studies, in the broader sense of inquiries which accumulated large quantities of information of whatever form. The division of psychological practice according to Danziger's tripartite scheme of Leipzig, Paris (or clinical), and Clark models, while valuable in terms of explicating the social dynamics of the research situation, if adopted uncritically, obscures both other forms of difference as well as commonalities across these forms of practice. This is especially apparent in this inquiry into early questionnaire research.

The Nature of Questionnaires

By the 1880s questionnaires were a well-known method of investigating psychological matters, referred to variously as circulars, blanks, schedules, syllabi, and questionnaires, in

addition to the now familiar term questionnaires.⁸ Even given longstanding popular awareness of this mode of inquiry, questionnaires remain surprisingly difficult instruments to define. They are at once incredibly simple and decidedly complex. We know, almost intuitively, what a questionnaire is, but describing their characteristic features can be challenging. At base questionnaires are both material objects and a set of rules for collecting information (see Gundlach, 2007). As instruments they comprise no more than a list of questions, which is reproduced and circulated with the aim of amassing information.⁹ Depending on the circumstance, the set of questions might be printed individually or, for expediency in distribution, reprinted within the pages of popular and scientific periodicals. However disseminated, placing questionnaires in the hands of a multitude of individuals is central to the effort to have large numbers of individuals respond to a set of questions and provide this information to an investigator. Only once this process of distribution, circulation, and return is completed is the questionnaire fully constituted.

As items used to produce scientific data, questionnaires fall within the larger category of scientific instruments (Gundlach, 2007; Sturm & Ash, 2005). Although not laboratory apparatus of the type often associated with the early years of psychology as a distinct discipline, questionnaires are nonetheless psychological instruments. As such questionnaires facilitate the adoption of specific scientific and social practices. From the earliest iterations of these instruments within psychology, the practices attendant to their use influenced their very form. Placed in the hands of inexpert individuals, the specificity of questions and the degree of detail provided in instructions to those collecting data were critical. At the same time, alterations to the

⁸ The latter term, questionnaire, is unique in its use by Edward Titchener and clearly aligned by him with the work undertaken by Galton and Hall (see Titchener, 1901, pp. 387–8).

⁹ The more general role of lists in science has also been addressed (Delbourgo, 2012; Delbourgo & Müller-Wille, 2012; Müller-Wille & Charmantier, 2012).

composition of questionnaires allowed them to be recalibrated for various ends by investigators possessing different interests and agendas. As printed lists of questions questionnaires also operate as a kind of “paper tool” (Kaiser, 2005; Klein, 2003), facilitating not only the description of pre-existing knowledge about the world, but creating objects and order where none existed before.¹⁰ In some respects, it was only in the 1930s that questionnaires were fully realized as paper tools. The emergence of Likert scaling provided a systematic means of ordering and representing aspects of mental life often lacking in earlier efforts with these kinds of instruments.

The form of psychological questionnaire we are most familiar with today – where agreement or disagreement with particular statements is indicated via selection from a numerical scale – marks the very end of my story. Decades before the development of scales of this type, questionnaires were well-worn instruments in psychology. Occasionally involving the collection of information along discrete dimensions, including items to be completed with simple yes or no responses, early questionnaires more often consisted of open-ended questions to which individuals were free to respond with as little or as much descriptive detail as they desired. Depending on the investigation, descriptions could be procured directly from the individual whose experience was being reported, from observations made by others of the individuals, or from accounts gleaned from hearsay or read about in books. The aim, often, was simply to amass as many accounts of experience as possible. It was only decades into the twentieth century that the shape of questionnaires changed. No longer were descriptive accounts the mainstay of these instruments. Instead, the construction of questionnaires was radically altered so that only a restricted range of answers was permissible.

¹⁰ Extensive discussion of research tools in scientific practice is provided in Clarke and Fujimura (1992).

In their earliest iterations questionnaires served, in some respects, as means of collecting multitudes of case studies all at once. As Forrester (1996) notes in his discussion of psychoanalytic case histories, cases are often divorced from the individual, circulating as disembodied dossiers of information. To some degree, responses to questionnaires operated in the same manner. These instruments produced extensive descriptive accounts of personal experience and information in responses was rarely associated with an identifiable individual. Even in instances where some characteristics of the respondent were attached to their data, readers would be hard pressed to identify a subject from these pieces. This is, in part, a reflection of perhaps the most glaring distinction between case studies proper and information collected via questionnaire. The former are far more interested in producing, as far as possible, a full account of an individual's given circumstances, while the latter often attended only to a small subset of this personal experience. Generally targeted toward the collection of information on specific topics, questionnaires necessarily reduced individuals to pieces in order to construct a larger whole. Only in doing so could an account of aggregate functioning with respect to a particular topic be produced.

The practice of collecting information on various aspects of human experience via lists of questions, circulated among those with access to these experiences, was not a development unique to psychology. Decades before psychology began to take shape as a discipline, this mode of data collection was well established among European anthropologists who printed queries intended to serve as ethnographic instructions to collectors (Sera-Shriar, 2014; Urry, 1993). In this field questionnaires were effective means of gathering information regarding peoples in far-flung locales. In the earliest years of the new psychology, the boundary between psychology and anthropology was itself porous. This is most evident in undertakings like the Torres Strait

expedition, which was as much a psychological as anthropological inquiry (Herle & Rouse, 1998; Martin, 2013), as well as in the positioning of the psychology exhibit at the 1893 World's Columbian Exposition within the Department of Ethnology (Shore, 2001). Ultimately, psychologists were less interested in putting these tools to work in amassing information on foreign peoples, but they nonetheless found questionnaires beneficial in the accumulation of masses of data on psychological life. In adopting the method, previous anthropological uses of questionnaires were invoked as evidence of the practice's legitimacy, especially the use of lay individuals as data collectors.¹¹ Similarly invoked was work in this vein by Charles Darwin. More important still, was Francis Galton's use of this method, as well as studies undertaken by German pedagogical societies, both of which served as direct models for some of the earliest questionnaire-based inquiries in American psychology (see Chapters 1 & 3). As these lines of influence suggest, psychological questionnaires were by no means unique to the American context and investigators within the United States were well aware of similar undertakings occurring elsewhere.¹² This project is restricted to work with psychological questionnaires within a singular national context, leaving the use of psychological questionnaires in other locales for future researchers.

As collecting technologies, questionnaires have much in common with surveys and polls. There exists a rich literature on the history of both social surveys and opinion polling (Bales, 1996; Bulmer, Bales, & Sklar, 1991; Converse, 1987; Greenwald & Anderson, 1996; Groves, 2011; Igo, 2007; Katz & Sugrue, 1998; D. J. Robinson, 1999; Yeo, 2003), but little has thus far been written on the history of the questionnaire. Though similar, questionnaires are distinct from

¹¹ See, for instance, Hall (1895a, p. 36, 1924, p. 392).

¹² In addition to British and German investigations, questionnaires were employed in psychological research in France. Discussion of Galton's questionnaire-based inquiries can be found in Burbridge (1994, 2001).

these other approaches in part because of the function to which each are put. Questionnaires are employed with the aim of amassing facts that science might then interpret. In contrast, social surveys and opinion polls, while also used to collect large amounts of data, are more the domain of those with, respectively, reformist and commercial or political interests. Surveys, in particular, often target specific populations, seeking to reveal details unique to their social existence.¹³ Psychological questionnaires, in contrast, are often distributed to any and all who might provide responses with the goal of amassing information on the human condition on a broad scale. While both may employ a list of questions to gather data, their aims are decidedly different. Even still, the line between methods can be hard to locate. Consider historian Robert Brain's (2001) work on Max Weber's early twentieth century investigation of workers' lives. Brain switches easily between the use of the term questionnaire and that of survey to describe Weber's investigation, which he argues was informed not just by efforts to extend the measurement practices of the laboratory to mass investigation, but also an understanding of social measurement drawn not from the natural, but from the historical sciences. A distinctly sociological project, Weber's questionnaires are put to a very different use than those of psychologists. Even within more psychological realms, the term questionnaire has at times been employed rather loosely, such as in the Edison Questionnaire, a series of questions devised by Thomas Edison and administered to job applicants in the 1920s. Despite its title, this instrument falls more within the category of mental tests, as its intent was individual evaluation rather than mass description (Dennis, 1984).

Styles of Scientific Thinking

The adoption of the questionnaire in psychological investigations by a multitude of individuals is clear evidence of the methodological plurality of the new psychology as it emerged

¹³ Along similar lines, undercover investigations of into the lives and conditions of particular social groups were also undertaken (see Pittenger, 2012).

in the United States.¹⁴ In addressing the nature of early questionnaire-based research, this project draws from the literature on styles of scientific thinking. Alistair Crombie (1994), Ian Hacking (1982, 2002), and John Pickstone (2001) have each articulated a similar series of recurrent styles of reasoning in science. The questionnaire method, with its aim of collecting, classifying, and analyzing masses of information, operated within what has been identified as a *natural historical* or *taxonomic* style of scientific reasoning, in contrast to the *experimental* style that characterized those psychologists whose work involved experimentation within laboratory spaces.¹⁵ Historian Katherine Pandora (1997) has detailed the role of this kind of perspective within the field from the 1930s, particularly in the work of psychologists Gordon Allport, Gardner Murphy, and Lois Barclay Murphy. Extending this characterization, I document the presence of a natural historical orientation within the discipline from its very inception. Those employing questionnaires did not reject the idea of psychology as a science, nor did they reject the importance of laboratory spaces or the role of specialized training. Instead, they drew on a different brand of scientific epistemology for authority, explicitly aligning their work with natural history. Promoting a broader understanding of the scientific basis of the field, one that included the accumulation of information from diverse, non-specialist sources, they adopted multiple styles of scientific reasoning and a more accommodating understanding of objectivity.

Experimentation and work with questionnaires involved different styles of scientific reasoning, but at the level of discourse no definite distinction between the two forms of practice was made. The recognition that experimental investigations may reveal causal relations, while

¹⁴ Further discussion of the breadth of the new psychology's methods can be found in Pettit (2008).

¹⁵ In many respects, this characterization is in line with Danziger's (1990) distinction between Galtonian and Wundtian traditions in psychology. A now classic account of experimentation as a form of knowledge production can be found in Shapin and Schaffer (1985).

commonplace today, is missing in late-nineteenth century debate over the use of questionnaires in psychological research.¹⁶ As a means by which psychological data might be generated questionnaires, like the category of mental tests, were often understood as experiments (see Winston & Blais, 1996). This reflects the fact that for psychologists at the time the experiment was an incredibly broad category, encompassing nearly any undertaking that produced data on mental life. Even still, there was a clear difference between the experimental work conducted in psychological laboratories and that undertaken with questionnaires. The kind of knowledge obtainable through each mode of inquiry was unmistakably different. Laboratory-based experimental studies in the Wundtian tradition, pursued knowledge of the generalized normal, white, adult, male mind (Danziger, 1990). In contrast, questionnaires offered an avenue by which to document and dissect information on a wider range of human mentality. Although this project documents the role of the questionnaire within the new psychology, the history of this particular psychological instrument is not told in isolation. Within the methodologically diverse territory of the new psychology, investigators were not necessarily committed to a singular mode of inquiry. Instead, the same investigator at times adopted different methods, and in some instances employed multiple methods within a single research project. Similarly, some subjects of inquiry were subject to multiple investigations by different psychologists, each adopting different kinds of investigatory practices. Just what these various practices afforded researchers is explored in the context of a variety of questionnaire-based projects.

¹⁶ The breadth of the meaning of the term experiment in early psychology, and the 1930s standardization of the term to refer to investigations involving independent and dependent variables capable of revealing causal relations, is discussed in Winston (1990; 1996). Throughout this project, I engage the term experiment as a means of juxtaposing questionnaire inquiries with contemporaneous endeavours that were explicitly non-questionnaire-based and that often took place within laboratory spaces. Although cognizant of the potential anachronism of employing this narrow definition of experiment in a period where this was not yet the established meaning, the expository simplicity of doing so has, ultimately, prevailed in the face of this concern.

Still more distinguishing features between these two modes of inquiry, as articulated at the time, were the places and people involved in each. Those associated with questionnaire-based research were seen as questionable sources of scientific knowledge. Research with questionnaires largely shunned the laboratory in favour of the accumulation of information in the field.¹⁷ The standardization of laboratory spaces and their associated instrumentation allowed laboratories to serve as “truth-spots,” what Thomas Gieryn (2002) has characterized as spaces in which scientific claims that transcend the bounds of place may be generated. It was the laboratory as site of psychological practice that was crucial to the discipline’s assertion of its status as a science (Capschew, 1992; Morawski, 1988b).¹⁸ For naturalistic collection projects employing questionnaires, place was crucial in a very different sense. Data were to be collected not in the sanctified spaces of laboratories, but in the unregulated environment of the field. The legitimacy of data, in this case, was predicated upon the very naturalness of settings in which information was amassed, as it was only in these spaces that unadulterated mental life could be found.

Not only was the field, by definition, unstandardized, so too were collectors and the instruments of collection employed therein. Much of the early questionnaire research in psychology relied on the assistance of untrained individuals in data collection efforts. Their participation was, for many, antithetical to the discipline’s efforts to claim dominion over the territory of the mind. Questionnaires themselves were piecemeal productions, largely unstandardized across investigations, unlike the instrumentation of the laboratory which was

¹⁷ On the relation between laboratory and field in biology see Kohler (2002). On the role of fieldwork in science, particularly its role in anthropological investigations, see Kuklick (2011), Kuklick and Kohler (1996), and Sera-Shriar (2014).

¹⁸ The ideal of the laboratory was by no means unique to psychology. For discussion of the laboratory’s role in American sociology see Owens (2014).

specially calibrated in an attempt to ensure precise measurements (Benschop & Draaisma, 2000; Coon, 1993; Kroker, 2003). The flexibility of the questionnaire method, in terms of who, where, and what might be used to collect data, was appealing to many as it allowed for the study of a diverse array of psychological topics. At the same time, the lack of standardization of space, instrument, and user was problematic. Employing untrained individuals in scientific practice, even if only in highly circumscribed roles, was seen by some as a threat to the professional standing of trained psychologists, putting in jeopardy the entire enterprise of scientific psychology. Seeking scientific and social standing, these psychologists were unsympathetic, if not outright dismissive of, the use of questionnaires to generate psychological knowledge.

In venturing out of the laboratory and including the public in the research process, questionnaire projects violated standards of objectivity central to the discipline's scientific identity. These kinds of investigations did not conform to the mechanical objectivity central to laboratory-based research. This kind of objectivity involved the production of scientific knowledge by machine-like means, something that could involve mechanical devices either literally or metaphorically (Daston & Galison, 2010; T. M. Porter, 1995). Psychologists pursued this kind of objectivity through the use of standardized instrumentation in laboratory spaces, but also through specialized training that in effect transformed individual psychologists into mechanical operators of the mind (Coon, 1993; C. D. Green, 2010; Kroker, 2003). The inclusion of untrained members of the public in the research process, and as sources of information about the mind, undermined psychologists' own claims to expertise, ones largely grounded on specialized training in the field.¹⁹ By involving untrained, and therefore what some

¹⁹ Much like Green (2010), I do not see the emphasis on training in early American psychology as an adherence to Daston and Galison's (2010) epistemic virtue of trained judgment. Within this scheme expertise is to include specific individual aptitudes such as skilled

contemporaries considered as untrustworthy, individuals in the research process, the resultant knowledge was suspect. The social nature of knowledge production in science is predicated on trust, which for psychologists was rooted in training.²⁰ Mechanical objectivity and expertise were not completely absent from questionnaire-based research, but their presence was far more circumscribed than in other forms of psychological practice at the time.

Collecting Practices and Data Gathering

This study of the questionnaire in early American psychology builds upon a growing literature on collections and collecting practices (e.g., Crane, 2000; Fabian, 2010; Findlen, 1994; Kohler, 2006, 2007, 2008; Strasser, 2012a). As historian of science Robert Kohler (2007) observes,

...collecting is not a simple act of gathering but an unusually complex social and cultural practice. At the core of each collecting science are procedures for finding, selecting, extracting, recording, and transporting objects from field to storage vault. No less necessary are strategies of designing and assembling collections to serve some definite scientific purpose; methods of ordering and classifying objects for ready retrieval or public display; and the arts of preserving and curating that make collections of fragile objects permanent. All scientists are finders (in one way or another); only collecting scientists are also keepers. (p. 432)

As Kohler recognizes, collecting is a part of most, if not all, sciences but there are some sciences and some scientists for whom collecting is a much more central concern. Within the new

observation and interpretation. In contrast, psychology's emphasis on training is more closely aligned with mechanical objectivity, wherein psychologists are themselves to be specially calibrated and machine-like in their observational activities, rather than act as interpreters of intermediary representations of their subject matter.

²⁰ The social nature of scientific knowledge production, and the key role of trust in this process, is discussed in Shapin (1994).

scientific psychology it was those who adopted the questionnaire as a methodology who made collecting most central to their science. Recourse to standardized measurement and laboratory spaces as validating was impossible in these undertakings and it was, instead, the collection of masses of information that lent these endeavours authority.

In his call for other historians of science to pay greater attention to the collecting sciences and their attendant practices, Kohler enumerates a number of characteristics he sees as unifying these sciences. For Kohler (2007), (1) “collecting is place specific” (p. 445). It involves a (2) “social diversity of...practitioners” (p. 445) who take on (3) “composite professional roles” (p. 446). These sciences have also (4) “retained distinct vestiges of the vernacular cultures from which they sprang” (p. 446) and are (5) “ambivalent about their dependence on objects and collecting” (p. 447). Collecting sciences also (6) face the challenge of keeping collections and the emphasis on collection results in a (7) “split professional identity” (p. 448). Finally, the (8) “dependence on found objects engenders a distinctive moral economy of data” (p. 449).

Kohler’s criteria for identifying and describing the collecting sciences provide a starting point for my discussion of collecting within early American psychology. That said, these criteria are not absolute, especially in the case of psychology, a science for which the objects it seeks to collect are necessarily ephemeral. The fact that psychology collects accounts of phenomena, rather than phenomena themselves, suggests that its practices of collection may have more in common with data gathering than with collecting proper (e.g., Sepkoski, 2013; Strasser, 2012a). For Kohler (2007) “reportorial data ‘gathering’” (p. 431), of the kind undertaken by anthropologists and ethnologists, is not strictly speaking collection as “it is this ‘thing-y’ particularity of found objects that demarcates the collecting sciences as a natural group. It shapes the distinctive practices of these sciences, distinguishing them from the data gathering that is

common to all empirical science” (Kohler, 2007, p. 432). Mental events are not material, or “thing-y,” in the way identified by Kohler as typical of the collecting sciences. Rather, psychology’s collected objects are in fact proxies for the original, intangible phenomena that are the real object of interest. It is accounts of mental events, and the behavioural manifestations thereof, that are collected rather than the events themselves. In the collection of this kind of immaterial material the questionnaire served as an invaluable tool.²¹

Kohler’s understanding of data gathering as apart from collecting has not gone uncontested as a recent piece by historian Bruno Strasser (2012a) on role of collecting in the life sciences makes clear. Unlike Kohler, Strasser (2012a) is interested in data collection, rather than the collection of material objects, and is “...focused on a specific cognitive and material practice centred on the constitution and use of collections...” (p. 305). In doing so, Strasser argues that rather than experimentation overtaking natural history in the biological sciences, the two practices have converged.²² For the purposes of this project the collecting undertaken by early American psychologists is situated somewhere between the kind of collecting of material objects discussed by Kohler and that of the collection of data discussed by Strasser. Psychologists may have collected data, but they did so only because their object of investigation was itself immaterial and therefore inaccessible. Psychological experimentation in laboratory spaces itself involved a particular set of practices, but it is the collecting practices that characterized questionnaire research that are most glaringly absent from the historical record. Places like the psychological laboratory may well have served as “truth-spots” that legitimized the knowledge produced therein, but methodologies and their associated practices also offered a means by

²¹ Paper proxies were also collected in biological sciences, such as in Georges Cuvier’s “paper museum” of specimens see Rudwick (2000).

²² For more on the historical relation between collecting and data practices in biological and life science research see Strasser (2010a, 2010b, 2011, 2012a, 2012b).

which truth might be produced. It is some of the extra-laboratory practices of this period, and their relation to other efforts at scientific knowledge production, that this project documents.²³

Despite not meeting Kohler's criteria of materiality, many of the characteristics of the collecting sciences are also typical of the collecting done by early American psychologists. As a result, a number of these points guide my discussion of early questionnaire research. In establishing psychology as a science the laboratory played a key role, as it allowed psychologists to situate themselves within a place that bore the imprimatur of science. (1) With respect to collecting via the questionnaire method, place was also important; it was not in the laboratory, but in the field that accounts of mental events were to be amassed. It was often in schools and homes that the questionnaire was put to use. (2) As a consequence of the objects of psychological interest existing outside the confines of the psychology laboratory, psychologists relied on a "social diversity of...practitioners" (Kohler, 2007, p. 445) in order to assemble their collections, largely by enlisting the public, particularly parents and educators, in their research projects. These individuals possessed unique forms of "residential" (Kohler, 2006) or "experiential" (Vetter, 2011) knowledge derived from the experiences of their daily lives. This kind of knowledge was a crucial component of questionnaire research projects, as it was information that was otherwise inaccessible to psychologists, whose own knowledge bases were more decidedly "cosmopolitan" in nature, in that they were "more global and theoretical" (Kohler, 2006, p. 157) in orientation. These involvements of lay individuals reveal the importance of social relationships and networks of collectors for the success of questionnaire projects. (3) A diverse pool of participants also encouraged the construction of "composite professional roles" (Kohler, 2007, p. 446) in which some of those out in the field collecting were

²³ On practices in the human sciences see Isaac (2012).

psychologists and some of those who began as collectors struggled to become psychologists in their own right. (4) With respect especially to those who sought to study children's minds, psychology as practiced with the questionnaire method retained a tendency to venerate the child. (5) The difficulties of collecting ideal specimens, that is full, detailed accounts of mental phenomena, also led to an ambivalence about the value of collecting for many of those who adopted the questionnaire method and others in the field. (6) When completed questionnaires were returned en masse psychologists also faced the challenge of what to do with such abundant returns. (7) For those psychologists who adopted the questionnaire as part of their research on the child, the involvement in the child study movement resulted in a "split professional identity" (Kohler, 2007, p. 448) in terms of their status as both psychologists and as educational experts, for whom expertise was predicated on their scientific knowledge of the child. (8) Kohler's (2007) final point, that the "dependence on found objects engenders a distinctive moral economy of data" (p. 449) is also true of the early adopters of the questionnaire method within psychology. These psychologists clearly privileged data, particularly in abundance, but were at times not entirely sure how to articulate what data on this scale afforded the discipline.

In attending to the practices of collection at the centre of questionnaire-based research, Danziger's now canonical analysis of the ways in which psychological knowledge was constructed in the first decades of the field's existence is complicated. This project does not challenge his characterization of the psychology's development, but rather offers a more nuanced account of just how the "triumph of the aggregate" (see Danziger, 1990, pp. 68–87) he identifies came about. It is the locations in which the mass assemblage of data occurred, the diverse participants in this research process, and the struggles of investigators to make sense of accumulated information that are the centre of my analysis. Danziger, despite his focus on the

role of the subjects in psychological research, pays little attention to exactly who the subjects of questionnaire research were, or to the practices of data collection. Specifics of subject and investigator in questionnaire research are absent. Instead it is asserted that these participants were children, and therefore problematic sources of psychological data. As this project documents, this is an oversimplification. Although child study questionnaires were among those most abundantly circulated, this mode of inquiry was used to investigate a variety of topics with participants who ranged from children, to college students, and to the adult public more generally. At the same time, involvement in questionnaire-based research extended beyond simply the psychologists initiating research projects and those whose information was recorded on questionnaires. Crucial to many of these enterprises was the participation of non-expert collectors as intermediaries between psychologists and respondents.

Work with questionnaires was both a consequence of and a contribution to a moral economy of data. Moral economies, as outlined by Lorraine Daston (1995), are the affect-laden norms and values that sanctify certain objects and practices.²⁴ Facilitating the emergence of this moral economy within psychology was the broader rise of quantification and measurement in the nineteenth century and its influence on the social sciences.²⁵ The use of quantification and measurement practices was part of, and spurred on by, changing conceptions of causality, certainty, determinism, and objectivity. These kinds of changes in the very understanding of how the world might come to be known established quantification and measurement as valid means of knowing the world, but were also themselves brought into being, at least in part, as a result of the

²⁴ For a very different understanding of “moral economy” see Thompson (1971).

²⁵ There is an extensive historiography on the ascent of quantification and measurement in society, as well as the import of such for the development of the social sciences (Anderson, 1988; Bannister, 1987; J. Brown, 1991; Cohen, 1982; J. Cole, 2000; Desrosières, 1998; Gigerenzer et al., 1989; Hacking, 1990; T. L. Haskell, 1977; Poovey, 1998; T. M. Porter, 1986, 1995; Ross, 1991; Rusnock, 2002).

rise of a quantified worldview. As part of efforts to manage citizenry, nations collected increasing amounts of data from as early as the seventeenth century, but into the nineteenth century developments such as an ever-increasing population, in ever more urban environments, roused government efforts to keep track of the populace as never before. To do so, they resorted to a heavily quantified system of state-gathered numbers to track various features of this society (Gigerenzer et al., 1989; Hacking, 1990; T. M. Porter, 1986), including the institution of national censuses (Anderson, 1988; Curtis, 2001; Levitan, 2011). Efforts to manage an increasingly unwieldy society created an “avalanche of printed numbers” (Hacking, 1990, p. 2) in European states in the early nineteenth century. In this system, the populace was not only quantified, but also classified, with data gathered on various kinds of social deviancy including suicide, murder, and disease, among others. These seemingly innocuous practices of counting citizens and their attendant behaviours were followed by more sophisticated quantification technologies, and more importantly the application of these technologies to a wide range of fields of inquiry, one of which was psychology.

The prevalence of questionnaire-based research in American psychology at the end of nineteenth century was part of a larger move toward quantification within the discipline. These projects were self-consciously fashioned as “statistical” in nature as a function of their aim of amassing and analyzing large quantities of information.²⁶ That said, the data collected by early adopters of questionnaires were largely descriptive, rather than numerical. For those adopting a naturalistic style of scientific thinking, the accumulation and use of masses of data was central to the scientific standing of their work. As Danziger (1990) notes, for these individuals “there was a belief that mass data, gathered by these means, constituted a valid basis for psychological

²⁶ Discussion of the statistical nature of early social survey work can be found in Bulmer (1991).

knowledge" (p. 75). Within the emergent moral economy of data psychologists could not only collect information via experimentation, but also through widespread questioning conducted outside of laboratory spaces by non-experts.

Without recourse to standardization and training, questionnaire research relied for its scientific authority on the mass accumulation of data. Inherent in this practice was the belief that large quantities of information, whatever their form, held scientific value. To be sure, the collection of masses of information was not unique to questionnaire research.²⁷ Experimental projects undertaken psychological laboratories could also result in large quantities of information, but in questionnaire-based projects masses of material served as the central marker of authoritative knowledge as this mode of inquiry could neither rely on the legitimacy afforded by laboratory spaces, nor on directly numerical results such as those produced in reaction time studies. Seeking to align their descriptive inquiries with the physiological and perceptual interests of those in the laboratory, if only speculatively, physical states associated with mental phenomena and sensory and perceptual experiences themselves were recurrently asked after. In some instances, questionnaires contained items that asked individuals to engage in specific forms of self-experimentation. Similarly, efforts were made to transform masses of descriptive information into numerical values. Data of easily quantifiable form, however, remained elusive in many projects.

Eschewing a singular truth-spot in favour of diverse, multiplicitous data questionnaires were able to do what the experimental methods of the day could not. Mass data collection efforts held the possibility of documenting difference. Variation was antithetical to the initial aims laboratory psychology, especially the standard Wundtian paradigm. Nonetheless, it soon

²⁷ It would not be until several decades into the twentieth century that statistical sampling techniques were established as an alternative means of obtaining credible data (Converse, 1987).

surfaced as an abiding interest among psychologists influenced by Darwinian notions of evolution, fostering the emergence of an American system of functionalist psychology (C. D. Green, 2009; O'Donnell, 1985; R. J. Richards, 1987). In contrast to the homogeneity aimed for in the psychological laboratory, questionnaires explicitly sought out diversity through the accumulation of large quantities of material on mental life from the masses. Only in this form of scientific practice could the rich variation inherent in the human populace, as well as similarities across groups, be fully revealed. These kinds of undertakings – which aimed to reveal differences on the basis of sex/gender, nationality/race, and age – were part of a growing discourse on difference within both science and broader society (Bederman, 1995; Noon, 2005a; Russett, 1989; Shields, 1975). An increasingly diverse American populace, and the increasing visibility of these differences within urban centres, also served to increase the import of describing variety in its many forms. Doing so, spoke to social concerns surrounding pervasiveness of groups deemed deficient and fears of degeneracy more generally (Pick, 1993).

The use of psychological instruments such as the questionnaire was part of a broader trend toward understanding the aggregate by collecting mass information on the shape of the social world.²⁸ Common to early research projects using questionnaires were efforts to de-limit the “normal” functioning of the mind by documenting the full range of human experience. Over the course of five decades of questionnaire use, the relation of work with this method to understandings of normality shifted considerably. As Ian Hacking (1990) notes,

the normal stands indifferently for what is typical, the unenthusiastic objective average, but it also stands for what has been, good health, and for what shall be, our chosen destiny.

²⁸ Twentieth century efforts to produce an aggregate understanding of the social world, through the application of social surveys and opinion polls, are ably discussed by Igo (2007).

That is why the benign and sterile-sounding word ‘normal’ has become one of the most powerful ideological tools of the twentieth century. (p. 169)

Similarly, Peter Hegarty (2013) has recently called attention to the necessity of attending to the division between the visions of “normal” inherent in Adolphe Quetelet’s vision of the aggregate and that in Francis Galton’s conception of the ideal when discussing psychology’s construction of normal.²⁹ This division is most notable when comparing Hall’s work on children with latter scaled attitude questionnaires. Hall’s undertakings were very clearly in the tradition of Galton, as he amassed information on child life in an effort to produce an idealized vision of normal childhood. Other investigations, especially with twentieth century developments in the scaling of questionnaire items, pursue information on normality in line with Quetelet’s notion of the aggregate. Statistical averages across large groups of individuals spoke not to an ideal state, but rather to the current mental state of the aggregate public.

The reliance in many questionnaire projects on public assistance with data collection meant subjects of investigation were often of popular interest. Inquiries into child life, psychical phenomena, and dreaming all relied on the participation of a public that was itself already captivated with the topic at hand. Securing the cooperation of large numbers of individuals in scientific projects was predicated on such interest, and in cases where less broadly engaging subjects were investigated, respondents were necessarily obtained from more circumscribed populations. Studies of mental imagery and mental association, for instance, relied not on the public at large, but rather on members of college communities, particularly students. Easily accessible populations of students would continue to form the basis of questionnaire research in later years. Questionnaires in the 1920s and 30s were also geared toward topics with broad social

²⁹ On the notion of the “normal” in psychology see Carson (2003).

relevance, but explored these through the assessment of attitudes toward various issues. The cooperation of a diverse public was no longer a necessary component of this work, as the circulation of questionnaires was largely limited to university populations. Questionnaires were now wielded not by untrained members of the general public, but by psychologists. Rather than amassing information from any and everywhere, data were gathered from students. The public was no longer engaged in scientific practice, but was instead the target of work with questionnaires as investigations resulted in pronouncements regarding the aggregate public's position on social issues.

From Thick to Thin Description

Efforts to construct a meaningful whole from collected descriptive accounts were never fully successful. Statistical aggregation of assembled information was simply impossible despite stated aspirations to produce something akin to a composite portrait (see Hall, 1883a, p. 252, 1893b, 1924). This difficulty engendered a particular ambivalence about the method among its early adopters. Those employing questionnaires in the final decades of the nineteenth century were drawn in by the seductiveness of masses of data, convinced that within a mass of material was bound to be a bounty of knowledge. Yet, without a means of synthesizing large quantities of descriptive information this knowledge remained ever elusive. It was only with the advent of techniques that allowed for the collection of information in a more directly numerical form that this circumstance changed. Description was abandoned in favour of numerical values that permitted collation and manipulation in ways impossible with diffuse descriptive accounts. Numbers were simply more amenable to reductive techniques. No longer was experience open-ended. Rather than being free to depict experience in any way they so chose, individuals were instead restricted to a predetermined range of possibilities with corresponding numerical values.

Only in this way was the whole of a body of data able to be treated in unison, allowing for distinct conclusions on the state of the aggregate.

This transition from masses of descriptive data to the collection of numerical proxies of mental experience illustrates a shift from thick to thin description within the field.³⁰ As historian Theodore Porter (2012) notes, the benefit of thin description is that it “offers outsiders the opportunity to act and to choose, relying on knowledge without deep understanding. For insiders it signifies self-denying objectivity: forfeiting, if only in principle, the right to interpret” (p. 222). Thin accounts offer straightforward bases for decision making in a way impossible with nuanced thick description that lacks a singular, conclusive result. In the context of attitude research this kind of thin description of the aggregate’s state of mind on issues had, at the very least, the potential for definite social action. Providing simple, concrete findings on social matters allowed for the dissemination of results outside of scientific spheres, but necessarily reduced the complexity of social life. Often of numerical form, information like this served as immutable mobiles (Latour, 1986) capable to circulating broadly in government and corporate contexts. At the same time, work in this vein operates with a guise of objectivity that early investigators in the thick description tradition were hard pressed to claim. As the shift from thick to thin description in questionnaire research demonstrates, neither characteristic is intrinsic to a field (T. M. Porter, 2012). And it is not that thin description wholly replaced thick, as there are still those who seek detailed, nuanced accounts of personal experience.

³⁰ My use of “thick description,” and its counterpart “thin description,” as a way of thinking about changes in questionnaire research draws from the use of the term by historian of science Theodore Porter (2009, 2012). As Porter (2012) acknowledges, the term was previously employed by philosopher Gilbert Ryle (1949, 1968) and then anthropologist Clifford Geertz (1973), whose respective uses of the term were necessarily somewhat different, but who both highlighted the importance of meaning for understanding.

The thick description of early questionnaire research produced an abundance of paperwork. Little of this, however, remains. Even among those with particularly rich archival collections, such as William James, documents pertaining to questionnaires are scarce. Outside of limited correspondence relating to data collection and the difficulties of interpreting information, the bulk of the papers generated in questionnaire-based projects are lost to the historical record. In a few instances, archives house copies of the blank forms that were circulated to collect data.³¹ Glaringly absent are the replies submitted by the public to psychologists in response to questionnaires, as well as records of the more personal interactions of the public with the research process.³² As a result, accounting for the experiences of the public in their engagement with psychology is difficult. Evidence of these encounters is always fragmentary, never fully capturing the subjectivities of participants. At the same time, as historian of the human sciences Michael Pettit argues “archival silence and presence are in themselves suggestive evidence that can illuminate the social history of expertise” (Pettit, in press).³³ That kept and that disposed of are marked signs of, respectively, what is valued and

³¹ For instance, the collection of Topical Syllabi housed in the Clark University Archives (Topical Syllabi, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

³² Only two completed questionnaires were located in archival collections over the course of this project. One is William James’s answer to James Pratt’s 1904 questionnaire on religion (William James. [Answers to J. B. Pratt’s questionnaire on religion]. Printed sheet with autograph annotations; [n.p., 1904]. William James Papers, 1803-1941, b MS Am 1092.9 (4474), Houghton Library, Harvard University, Cambridge, MA). The other is the 1930 response of Rensis Likert’s father’s to his son’s “A Survey of Opinions” (April 26, 1930, Box 21, Folder 1930, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI). Neither is an example of the kind of answers the public at large generated in response to questionnaires.

³³ In a similar vein, Pettit (2013b) has also recently proposed that Hacking’s notion of looping effects be used as a methodology, in an effort to document the feedback process inherent in the public’s engagement with psychology. He argues “historians should become more attentive to how the public’s interactions with these sciences generated what might be called “therapeutic archives” of self-talk created in conversation with experts. The twentieth century saw the proliferation of such archives, whether recordings of psychoanalytic sessions, responses

what is not. The lack of remaining material from questionnaire projects, which necessarily generated masses of paperwork, is itself telling. Individual accounts were not themselves esteemed. It was only the collected whole that might reveal scientific truth. Value was located at the level of interpretation, after which raw data was simply superfluous. In the end individual reports were no more than ephemera, easily disposed of, either by researchers themselves or by subsequent individuals faced with a profusion of what were often undoubtedly seen as redundant documents. That much of the correspondence with the individuals charged with undertaking the labour of data collection is also absent, speaks to the relationship between investigator and collector. Those engaged in the work of collecting were often no more than a means to an end, a route to data on a scale that was otherwise impossible to achieve. As a consequence of these factors we are left with a distinct lack of information on both collectors and respondents, and their particular engagements with the research process. This makes difficult, if not impossible, a precise accounting of the questionnaire as a device of the public, defined by Marres and Lezaun (2011) as the “forms and modes of participation that are irreducibly material, and that recognizes that the political value of objects, devices and settings is itself established during the emergence of an idiosyncratic public” (p. 491; see also Marres, 2007). The questionnaire undoubtedly helped shape conceptions of the public, but exactly how the public engaged with these instruments and how they understood that engagement remains unclear. Similarly, the extent to which questionnaires operated as “technologies of elicitation,” or instruments “designed to generate lay views on the issues at hand, and feed those opinions into the policy process” (Lezaun & Soneryd,

to psychological tests, or inquiries made to celebrity scientists. These are not strictly medical records; they derive instead from a broader (psycho)therapeutic culture oriented around the cultivation and examination of an autonomous, deeply feeling self” (Pettit, 2013b, p. 1056). Unfortunately, these kinds of materials are largely lacking in the context of early questionnaire research.

2007, p. 279) is difficult to discern. What is clear, however, is that early questionnaire research was predicated on a distributed form of scientific practice among members of the public. These kinds of engagements undoubtedly shaped this public, and helped craft its very existence, in particular ways (Igo, 2007; Kelty, 2005; Latour, 2004).

A number of historical works have sought to address the role of participants in social scientific research. Most recently Frans Lundgren (2013) has discussed public involvement in Galton's anthropometric work, though largely sidestepping the public's experience of this work in favour of documenting Galton's concerted effort to create particular kinds of selves via this participation. In contrast, Sarah Igo's (2007) exploration of the contested construction of a mass public in relation to social scientific surveys and opinion polling offers a far richer account of the public's experiences of their interactions with experts.³⁴ Sociologist Mike Savage (2010) has provided a unique insider's perspective on the encounters between subjects and researchers documenting changing social class identities in Britain during the mid-twentieth century, drawing heavily from the rich accounts provided by participants. In her recent history of autism, Chloe Silverman (2012) attends to the work of non-experts, highlighting how the caregiving and research efforts of parents have proved crucial to scientific developments in the field. Like these projects, this account of the history of questionnaire research attempts to capture, to the extent possible, the contributions of lay individuals to early psychological research practices. These subjectivities are difficult to grasp given the paucity of the historical record, yet deserve to be attended to as important aspects of psychology's history. In the chapters that follow I attempt to reveal the role of the public to early psychological research by reading the traces that remain of

³⁴ The employment of lay individuals in statistical studies in New Deal America has also been documented (Didier, 2011).

their contributions in published accounts and fragmentary archival records, while also seeking to glean something of the subjectivities of those engaging in this work.

Chapters 1 and 2 trace the influence of Francis Galton on the American context. Incipient biologist Henry Fairfield Osborn's questionnaire-based psychological work, some of the earliest in the nation, was directly modeled on Galton's research. Inquiring into the subject of mental imagery via questionnaire provided emerging eugenicist Osborn, much like Galton, with the opportunity to document various forms of group differences in mental capacities. Although Galton's influence on American psychology is often associated with the mental testing work of James McKeen Cattell (Sokal, 1987a), Osborn's work offers an alternative entry point. Subsequent work on mental associations, discussed in Chapter 2, also built on earlier inquiries undertaken by Galton. Investigations of number habits and mental associations were part of attempts to counter reports of psychical phenomena by documenting how common mental proclivities could account for the kinds of coincidences that were often interpreted as supernaturally meaningful. At the same time, questionnaire research on mental association reveals that just as questionnaires were able to depict differences between groups they were also uniquely capable of documenting similarities across groups.

In Chapters 3 and 4 I turn to some of the most abundant questionnaire-based research in early American psychology, that conducted on child-life. Central to this kind of research was G. Stanley Hall who, together with his students and associates, produced a plethora of questionnaires from the 1880s through the 1910s. Chapter 3 discusses Hall's data collection efforts in terms of the involvement of a network of female educators in this work. Although psychologists like Hall purposefully restricted their role in questionnaire research to certain forms of practice, for these educators involvement in scientific child study was a means of

obtaining a form of authority over child-life and educational practices. I then turn in Chapter 4 to debate among psychologists over the scientific value of questionnaire-based child-study work given its reliance on untrained data collectors. Unable to appeal to the quality of the material amassed as a marker of this work's authority, quantity served as a convenient stand in. At the same time, seeking to root his research in dominant scientific ideals, Hall sought to frame his questionnaire-based research as mechanically objective, both literally and metaphorically, while also highlighting the role expertise at the point of interpretation. Seeking to produce a composite portrait of child-life, Hall struggled to synthesize his masses of descriptive material and to invoke the growing authority of numbers in his project.

Next to research on child-life inquiries into psychical phenomena were some of the most abundant questionnaire-based projects in early psychology. In Chapter 5 I discuss efforts on the part of the American Society for Psychical Research to collect large quantities of information on psychical phenomena through appeals for public assistance. William James, in particular, undertook projects in this vein, including an inquiry into consciousness of lost limbs and a much larger census of hallucinations. The labour associated with the latter project, in particular, was enormous as it accumulated large quantities of information. Just as in child study, data from the untrained public, however extensive, was suspect and required careful consideration by trained experts able to evaluate the validity of responses rooted in personal experience. Chapter 6 takes up efforts to interpret the data amassed in the census of hallucinations and the larger debates over the reality of psychical phenomena in which this work was embedded. Central to this discussion was the invocation of various probabilities as forms of evidence. Disagreement over the reality of psychical phenomena, and the value of particular forms of evidence, reveal the differing scientific styles and methodological commitments of early American psychologists. At the same

time, the non-psychical explanations offered for purportedly psychical phenomena positioned information gained from untrained members of the public, the central component of questionnaire research, as inherently untrustworthy.

Chapter 7 takes up the various investigations of dreams and unconscious mental life undertaken by American psychologists in the 1880s through to the early twentieth century. Like child study and psychical phenomena, these subjects were of popular interest. Appropriating popular cultural practices of dream interpretation and the keeping of dream books, the discipline sought to document the full range of the mind's ability, as part of a continuing effort to naturalize mental proclivities often characterized as psychical. Involving a variety of methodological approaches, many of them statistically oriented, work on dreams reveals the messiness of methodological distinctions during this time.

In the final chapter I move ahead in time to the 1920s and 30s to discuss changing approaches to the construction of questionnaires. Following developments in mental testing, particularly as a result of large-scale intelligence testing during World War One, the very form of questionnaires became increasingly standardized. At the same time, the public's relationship to questionnaire research changed. Instead of involving the public in the research process, questionnaires served as a means of speaking about the state of the public's mind. In the context of socially oriented research on attitudes, the work of first Louis Thurstone and then Rensis Likert was central to the redesign of questionnaires along increasingly numerical lines. This, I argue, constituted a move from thick to thin description, a shift that irrevocably altered questionnaire research in psychology.

Chapter 1

Verifying Variation: Collecting Accounts of Mental Imagery³⁵

One of the earliest questionnaire-based investigations in the United States was undertaken at the College of New Jersey (later Princeton University) in 1880. In June of this year, a young American biologist wrote to British polymath Francis Galton (1822-1911) requesting a copy of the mental imagery questionnaire he was circulating. The biologist, Henry Fairfield Osborn (1857-1935), sought to distribute Galton's questionnaire in the United States and hoped, in so doing, to make a "small contribution to Psychological service."³⁶ The psychological research Osborn conducted was initially structured around the research objectives outlined by Galton, but he soon adapted Galton's questionnaire research methodology in order to pursue his own burgeoning psychological interests.³⁷ Better known in history of science scholarship for his biological work in late-nineteenth and early-twentieth century American vertebrate palaeontology, for his administrative leadership during a twenty year long tenure as president of the American Museum of Natural History (AMNH) in New York City, and for his prominent role in American eugenics (Clark, 2008; Kevles, 1986; Rainger, 1991; Regal, 2002) Osborn was also an early American psychologist, albeit a temporary one.

For Osborn, psychological research "was taken up ... in the leisure hours not occupied by other professional work" (1884c, p. 440) and, by his own admission, pursued only "as far as my

³⁵ This chapter, in slightly different form, has previously appeared in print (Young, 2012b).

³⁶ H. F. Osborn to Galton, June 14, 1880, Galton, F. (1612-1926). Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

³⁷ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

other work will permit.”³⁸ Notwithstanding this subsidiary position in his professional life, this questionnaire research provides an early link between the only just emerging American discipline and British psychological practices, particularly those of Galton.³⁹ The project sought to amass information on individual experiences of mental imagery or visualization; that is, visual representation experienced in the absence of direct external stimulation, be it the memory of previous sensory experience or a constructed internal representation of some not previously experienced image.⁴⁰ For the purposes of his project, information on mental imagery was obtained by having individuals self-report, in an open-ended fashion, on their past and present visualizing experiences. And questionnaires were the means of collecting this data. As would be the case with subsequent investigations using the method, questionnaires provided Osborn with a means of collecting information on various forms of difference. Once located in his specific set of respondents, differences were generalized as applying to wider populations, including groups never directly investigated.

³⁸ H. F. Osborn to Galton, August, 19, 1880. Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

³⁹ Given the nascent state of scientific psychology in the United States at the time Osborn undertook his research, and his lack of further work in the field, it is perhaps unsurprising that the project attracted little attention at the time (see Chapter 7).

⁴⁰ Images, taken to be fundamental components of consciousness, would later figure prominently in introspection’s decline within American psychology. The so-called imageless thought controversy, catalyzed by disparate findings from German psychological laboratories (see Kusch, 1995, 1999), comprised disputes over the possibility of thought occurring in the absence of imagery (Danziger, 1980, 1990; Thomas, 1989). The inability of the introspective method to yield consistent results was evidence of the failure of attempts to standardize introspection so as to bring the method in line with the period’s techno-scientific ideals (Coon, 1993; Kroker, 2003). Within American psychology, the early twentieth century debate over the veracity of imageless thought led to disillusionment with idea of mental imagery as a guiding psychological concept, and with the study of consciousness more generally. This, in part, led John B. Watson to propose the abandonment of consciousness, including imagery of the type investigated by Galton and Osborn, as an object of psychological study in favour of a system of psychology he termed *behaviorism* (see Watson, 1913a, 1913b; Wozniak, 1993).

In this chapter I argue that Osborn's work offers an alternative avenue for the Galtonian influence on American psychology. This orientation has largely been attributed to the mental testing work of James McKeen Cattell begun in the late 1880s (Danziger, 1990; Sokal, 1987a). Osborn's research took place years earlier and was directly Galtonian in character, bringing with it an interest in individual and group differences. This enterprise foreshadows the later obsession with mental ability that would overtake the discipline in the twentieth century. At the same time, it served to further questionnaire-based research within the new field of scientific psychology with later undertakings building upon this mental imagery work, but recasting it in terms of the more culturally popular phenomena of dreaming (see Chapter 7).

Henry Fairfield Osborn (1857-1935) and Psychology at Princeton

From birth onward, Henry Fairfield Osborn lived a life of privilege as a member of New York City's social and financial elite. The son of a railroad magnate and the grandson of a successful New York City merchant, Osborn was also the nephew of prominent American financier J. Pierpont Morgan (1837-1913), the founder of the hugely successful J. P. Morgan financial institution. The Osborn family's wealth allowed Osborn to attend New York City's best preparatory schools before his departure from the city in 1873 to attend the College of New Jersey (Rainger, 1991; Sloan, 1980). At Princeton, Osborn's interest in science was fostered through contact with Presbyterian minister, Scottish Realist philosopher, and College president James McCosh (1811-1894),⁴¹ as well as geology professor Arnold Guyot (1807-1884), both of whom instilled in Osborn the conviction that science and religion are valid means of knowing the world, and thus are complimentary endeavours (Maier, 2005). The professed acceptance of the

⁴¹ McCosh's relationship to the "new" psychology has been explored by Rodkey (2011).

theory of evolution by some at the College, including McCosh, furthered Osborn's own interest in the biological sciences.⁴²

Under McCosh, the College was also a bastion of discussion on, though not research in, the new physiological psychology emerging in Germany. Charged with teaching mental and moral philosophy at the College, McCosh incorporated findings from German physiological psychology into his courses in the early 1870s and later in the decade Wilhelm Wundt's *Grundzüge der Physiologischen Psychologie* made its way into his teaching (G. Richards, 1995, 2004).⁴³ The presence of both the doctrine of evolution and discourse on the new physiological psychology at the College influenced Osborn, ultimately shaping not only his professional development, but also his personal views. Within the institutional context of the College of New Jersey, where the College president himself endorsed evolutionary views, including for a period Darwin's doctrine of natural selection (Livingstone, 1987; Moore, 1979; Wertenbaker, 1946), variation was a viable subject of inquiry, one that Osborn took up in his questionnaire research.⁴⁴ His longstanding interest in the nature of human variation, which first manifested in his mental imagery research, would eventually lead him to endorse social intervention in human breeding practices. In the early twentieth century, as a prominent American eugenicist, he helped found the eugenic-minded Galton Society with fellow eugenicists Madison Grant (1865-1937) and Charles Davenport (1866-1944) and served as president of the second International Congress of

⁴² The acceptance of the theory of evolution by McCosh and others at the College was subject to criticism from some members of the Princeton community. One of McCosh's foremost critics was Charles Hodge, Principal of Princeton's Theological Seminary, who equated Darwinism with atheism (Hoeveler, 1981; Marsden, 1994).

⁴³ McCosh's interest in the new psychology was at least partially a consequence of its emphasis on sense perception, which fit well with his Realist philosophy (Maier, 2005).

⁴⁴ McCosh's acceptance of Darwinian evolution was tempered by his view that evolution by natural selection operated on variations that only appear to be random, but are in fact of God's design (Marsden, 1994).

Eugenics (Clark, 2001; Kevles, 1986; Osborn, 1932; Regal, 2002). Not only did Osborn adopt Galton's mental imagery questionnaire, he also eventually adopted the eugenic ideas that Galton championed.

Osborn graduated from the College of New Jersey with an undergraduate and later a graduate degree, as part of the first cohort of American students specifically trained in the emerging discipline of biology, and in 1880 accepted an appointment as the College's first professor of biology (Hoeveler, 1981; Maier, 2005; Rainger, 1991; Wertenbaker, 1946). Within his role as Professor of Biology Osborn almost immediately exhibited an interest in the new physiological psychology. In 1881 he, along with two of his colleagues, historian William Sloane (1850-1928) and vertebrate palaeontologist William Berryman Scott (1858-1947), established a "Wundt Club" at the College. The club aimed to keep abreast of new physiological psychology findings, particularly those emerging from Wundt's newly institutionalized physiological psychology laboratory at the University of Leipzig. At weekly meetings the club's members, including McCosh and various students of the institution, one of whom was future developmental psychologist James Mark Baldwin (1861-1934), discussed new scientific developments and partook of related anatomical and physiological demonstrations. Spurred on by the success of the club, McCosh instituted a new course specifically on physiological psychology (Hoeveler, 1981; Maier, 2005; Rainger, 1991; Wetmore, 1991). The course was taught through the combined efforts of McCosh, Osborn, and Scott from 1883 until 1886, at which time Osborn and Scott were joined by Baldwin, who seemingly took on McCosh's portion of the course.⁴⁵

⁴⁵ Osborn, H. F. (1886). *Physiological psychology*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 7. American Museum of Natural History Archives, New York City, NY.

The Questionnaire Method at Princeton

Research into mental imagery began at the same time that physiological psychology was gaining a stronger foothold at the College. In line with his, by this time longstanding, interest in the developing field, McCosh published on the results of the mental imagery investigations Osborn undertook (McCosh, 1886; McCosh & Osborn, 1884),⁴⁶ but was not himself involved in carrying out the research. McCosh's interest in the project arose nearly a year after Osborn first contacted Galton about the possibility of distributing his questionnaire in the United States when Osborn presented on the project before the College of New Jersey's Philosophical Club.⁴⁷ Despite McCosh's support of scientific psychology, for him psychology would always be “the science of the soul” (McCosh, 1886, p. 1), rather than a modern science of the mind.⁴⁸ In his view, science was to proceed by the Baconian inductive method (Maier, 2005) and, within this scientific orientation, psychology was to “gather in (*induco*) facts, but always with a view of discovering an order among them and arranging them” (McCosh, 1886, p. 1; see also Rodkey, 2011).⁴⁹ McCosh never actively engaged in the kind of empirical research that characterized the new psychology, but was supportive of Osborn's questionnaire-based mental imagery research,

⁴⁶ Although both McCosh and Osborn authored this article, the article itself is comprised of three distinct sections (I, II, & III), each indicated as authored *solely* by either McCosh or Osborn. McCosh's contribution to the article is a discussion of “the general laws and characteristics of our mental imagery” (McCosh & Osborn, 1884, p. 58), and it is only in the section of the article authored by Osborn that the questionnaire research on mental imagery conducted at Princeton is discussed. At places in this paper, quotations from the McCosh and Osborn article will be prefaced with attributions of the quoted material to either McCosh or Osborn, in accordance with the section of the article from which the quotation originates.

⁴⁷ H. F. Osborn to Galton, April 9, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁴⁸ This definition of psychology appeared in McCosh's textbook *Psychology: The Cognitive Powers*, which received a scathing review from G. Stanley Hall (1887b) who concluded that, “from a scientific standpoint... little that is good can be said of the book” (p. 147).

⁴⁹ McCosh's endorsement of Francis Bacon's inductive method of scientific inquiry was common amongst Protestant theologians (Maier, 2005).

with its use of introspection conducted en masse to gather facts on mental life. For him, this work was acceptable as it “proceeds on the method which Bacon held in view” (McCosh & Osborn, 1884, p. 71). At this time he also spoke favourably of recent questionnaire-based psychological research undertaken by Hall (1883b), which appeared in the pages of the *Princeton Review*, insisting that it similarly conformed to the Baconian method (McCosh & Osborn, 1884, p. 72).⁵⁰

Osborn recognized that the questionnaire research he was engaged in was an effort at introspective psychology and he was moved to argue that, “introspective research is becoming somewhat overshadowed, in the present day, by the brilliant progress of the experimental school, yet it deserves to be put upon a continually sounder and wider basis” (Osborn, 1884c, p. 449). This defence of a type of introspection more aligned with the British philosophical tradition, wherein introspection involved consciousness scrutinizing consciousness, was not a denial of the value of the emerging German experimental introspection, but rather an attempt to elevate the scientific status of non-experimental introspection.⁵¹ Osborn’s adoption of a more philosophically-oriented form of introspection – one in keeping with McCosh’s views – was tempered by his explicit attempts to link descriptive accounts of mental imagery with corresponding physiological states. This attempt is consistent with the entrenched scientific aims of the new psychology that was only just coalescing at this time. That his psychological research adopted a decidedly naturalistic orientation is in line with his status as a biologist, one who would soon establish a successful career in vertebrate palaeontology collecting and analyzing

⁵⁰ Hall (1883b) himself referenced Galton’s visualizing work in his questionnaire research. Further, some of Hall’s first published success on the educational front came in the pages of the *Princeton Review* (Hall, 1882a, 1882b).

⁵¹ For more on the differences between the British and German introspective traditions see Danziger (1980, 1990).

fossil specimens. Osborn's work on mental imagery illustrates the apparent ease by which qualitative, introspective research could become, at least superficially, physiologically grounded.

Constructing Questionnaires and Categorizing Responses

Osborn's decision to undertake research into mental imagery was partly a consequence of his psychological interests and partly the result of an apparently chance encounter with Francis Galton the previous year. In 1879 Osborn travelled to Britain where he studied embryology with Maitland Balfour at Cambridge University and comparative anatomy with Thomas Henry Huxley in London (Rainger, 1991; Regal, 2002). While in London, he was introduced to Galton at a Royal Society dinner.⁵² Upon his return to the United States, Osborn happened upon a published account of Galton's mental imagery research, wherein Galton himself solicited the accumulation of information on mental imagery by others (Galton, 1880b, 1880c, 1883). These encounters, first with Galton himself and soon after with Galton's mental imagery research, prompted Osborn to contact Galton about obtaining a copy of his mental imagery questionnaire so that he might distribute it to individuals in the United States. Regarding the possibility of this research, Osborn wrote to Galton,

If you think it worth your while I will undertake to obtain a number of sets of answers from the most intelligent men and women I know personally or by reputation. They may be interesting in comparison and conjunction with those of your English correspondents. If you have not completed your inferences from the answers already obtained I shall

⁵² H. F. Osborn to Galton, June 14, 1880, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

enjoy very much undertaking this small contribution to Psychological service and forwarding the results to you.⁵³

Osborn's initial intent in undertaking this research was merely to gather information on mental imagery for Galton. His letter to Galton two months later makes his research agenda explicit: "I appreciate your suggestions regarding the collection of answers to your 'questions' and will endeavor to obtain answers ... in the spirit which you desire."⁵⁴ In its initial conception, the research was to fulfill Galton's aims and not Osborn's own, though this soon changed as Osborn developed his own mental imagery related research interests.

At the time Osborn contacted him about his mental imagery questionnaire, Galton himself had only recently begun research into the subject. The project began at some point in 1879 or 1880, when Galton casually questioned his acquaintances about their powers of mental imagery.⁵⁵ A more formal questionnaire investigating individual habits of visualization soon followed (Galton, 1880c).⁵⁶ As he described it, he set about studying "...the degree and manner

⁵³ H. F. Osborn to Galton, June 14, 1880. Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁵⁴ H. F. Osborn to Galton, August 19, 1880. Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁵⁵ In his initial informal investigation of imaging ability many of those Galton questioned were his scientist friends (Galton, 1883). This attempt to ascertain the imaging abilities of scientists is understandable given changing views on the role of imagination in science during this period. As Lorraine Daston (1998) has outlined, it was only in the mid-nineteenth century that a type of scientific objectivity arose that sought to relegate subjectivity and its companion, the imagination, to the artistic, rather than the scientific realm. In keeping with these efforts to banish the imagination from science, Galton's mental imagery research found that most scientists had no familiarity with the notion of mental imagery (Galton, 1883). While mental imagery is not synonymous with imagination, it is an aspect thereof, and Galton's investigations of the imaging ability of men of science fits within the discourse on the contested role of the imagination in science that Daston has documented. For more on the history of scientific objectivity see Daston's collaboration with Peter Galison (Daston & Galison, 2010).

⁵⁶ The questionnaire was a method Galton employed in a number of other prominent investigations in the latter half of the nineteenth century. These research endeavours include investigations into the qualities of English men of science and the characteristics of twins

in which different persons possess the power of seeing images in their mind's eye" by "collecting a large and growing store of materials, partly of verbal answers made by friends to my inquiries, but principally by means of written replies to a printed list of questions that I am distributing" (Galton, 1880c, p. 252). Requests for information from individuals were geared toward the collection of

well-marked and properly-authenticated instances of persons who are able to recall, or represent to their imagination, with great vividness, either sights, sounds, smells, or tastes, and to obtain information that may throw light on the peculiarities of the representative faculty in different families and races. (Galton, 1880c, p. 256)

The latter aim would prove central in the interpretation of findings from both his and Osborn's imagery research.

The questionnaire Galton developed to assess mental imagery was entitled, *Questions on Visualising and Other Allied Faculties*. The second version of the questionnaire, itself based upon an informal first questionnaire (Galton, 1883), instructed respondents as follows:

Before addressing yourself to any of the Questions on the opposite page, think of some definite object – suppose it is your breakfast-table as you sat down to it this morning – and consider carefully the picture that rises before your mind's eye.

1. Illumination. – Is the image dim or fairly clear? Is its brightness comparable to that of the actual scene?
2. Definition. – Are all the objects pretty well defined at the same time, or is the place of sharpest definition at any one moment more contracted than it is in a real scene?

(Fancher, 1983a, 1983b; Galton, 1874, 1883; Godin, 2007). For a contemporary critique of Galton's statistical study of mental imagery see Bain (1880).

3. Colouring. – Are the colours of the china, of the toast, breadcrust, mustard, meat, parsley, or whatever may have been on the table, quite distinct and natural?⁵⁷ (Galton, 1880b, pp. 301–2, 1883, p. 378)

As this segment of the questionnaire indicates, all of the items on Galton's mental imagery questionnaire were open ended; the respondent was merely asked to elaborate on various aspects of their mental imagery skills. Galton then ranked the responses he received according to the precision of an individual's mental imagery, with higher ranks indicative of greater ability. For the questions reproduced above, top ranked responses included, “Brilliant, distinct, never blotchy” and “Quite comparable to the real object. I feel as though I was dazzled, *e.g.* when recalling the sun to my mental vision” (Galton, 1883, p. 89). Individuals characterized as moderately capable of mental imagery offered responses such as, “Fairly clear, not quite comparable to that of the actual scene. Some objects are more sharply defined than others, the more familiar objects coming more distinctly in my mind” and “Fairly clear as a general image; details rather misty” (Galton, 1883, p. 90), while those with low ability provided responses such as, “Dim and not comparable in brightness to the real scene. Badly defined with blotches of light; very incomplete” (Galton, 1883, p. 91). In total, Galton's mental imagery questionnaire consisted of fourteen items. The remaining eleven questions asked about a variety of other aspects of visualization including characteristics such as the “distance of images,” “persons,” “scenery,” “comparison with reality,” and “numerals and dates” (Galton, 1883, pp. 378–9), as well as other sensory experiences.

⁵⁷ A variation of this question was used as part of the battery of physical and mental tests administered to the students of Columbia College by James McKeen Cattell in the mid-1890s (see Cattell & Farrand, 1896).

Undertaking research via questions posed to masses of untrained individuals was a valuable means of investigating mental life, so far as Galton was concerned. It was his contention that

although philosophers may have written to show the impossibility of our discovering what goes on in the minds of others, I maintain an opposite opinion. I do not see why the report of a person upon his own mind should not be as intelligible and trustworthy as that of a traveller upon a new country, whose landscapes and inhabitants are of a different type to any which we ourselves have seen. It appears to me that inquiries into the mental constitution of other people is a most fertile field for exploration, especially as there is so much in the facts adduced here, as well as elsewhere, to show that original differences in mental constitution are permanent, being little modified by the accidents of education, and that they are strongly hereditary. (Galton, 1880c, p. 256)

Much like Osborn, Galton considered mass introspection via questionnaire as a profitable means of understanding the mind's terrain. Especially valuable, given the ability of questionnaires to collect information from a multitude of individuals with relative ease, was the promise the method held of revealing the great diversity of mental life. Crucially, it was this kind of variation that was central to the evolutionary scheme introduced by his cousin, Darwin, decades earlier (Darwin, 1859; see R. J. Richards, 1987).

Although Osborn initially used Galton's *Questions on the Visualising and Other Allied Faculties* as his means of assessing visualization ability, he soon found this questionnaire inadequate for the investigation of the aspects of visualization he found most interesting. In 1882, Osborn solicited information on the visualizing faculties for a second time, this time using a heavily modified version of Galton's questionnaire, one he termed simply *Questions on the*

Visualising Faculties.⁵⁸ On this modified Princeton Mental Imagery Questionnaire,⁵⁹ Osborn offered an explanation of the research he was undertaking:

In the Spring of last year I sent to Francis Galton, the English psychologist, a collection of answers to his *Questions upon the Visualising and Allied Faculties* ... In acknowledgement he wrote me that they would prove of considerable value to him ... the friendly tone of Mr. Galton's letter[s] have encouraged me to continue the subject this year.

The following series are taken in part from Mr. Galton, and were partly suggested by the replies to the last year series.⁶⁰

Rather than solicit further responses to Galton's mental imagery questionnaire, Osborn opted to pursue his own line of research, one informed by his previous use of Galton's questionnaire, but not determined by the content of such. The questionnaire originally implemented to measure mental imagery was modified so that, of thirteen questions that comprise the questionnaire, the first four were taken without modification from Galton and of the remaining nine only three bore any resemblance to the original items on Galton's mental imagery questionnaire.⁶¹ Thus, in his second endeavour into psychological research, Osborn significantly altered Galton's mental imagery questionnaire so that he might investigate the aspects of mental imagery he took to be of

⁵⁸ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

⁵⁹ The phrase "Princeton Mental Imagery Questionnaire" has been adopted to distinguish Osborn's questionnaire from Galton's early versions.

⁶⁰ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY. See also McCosh and Osborn (1884, p. 58).

⁶¹ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY. See also McCosh and Osborn (1884, p. 60).

interest, in particular: the physical correlates of memory formation and later retrieval, the phenomenon whereby novel events strike individuals as familiar, and the relationship between aging and visualization.

Among the new items Osborn included on the 1882 mental imagery questionnaire are a number of questions that inquire as to the physical states that accompany memory formation and subsequent visualization. Question 10 on the modified Princeton Mental Imagery Questionnaire reads:

- (a.) Are the most vivid mental images that you can now bring up associated with any definite physical state at the time you formed them, such as active exercise on horseback or in running, or such as comparative repose in reading or study? (b.) Do you seem to be able to call up mental images easier at one time of day than another, or in an active rather than a quiet state of body? (c.) If you have ever experienced any impairment of vision how has this affected your visualising power at the time or subsequently?⁶²

This series of questions was Osborn's attempt to ascertain the physical states that most often accompany memory formation and subsequent retrieval. What is described as a fairly typical, if above average, response to this question reads, "Mental images, so far as I have been able to observe, come most in a passive state of body, and are not associated with any particular exercise" (McCosh & Osborn, 1884, p. 61). Question 11 on this revised questionnaire, "What was the state of mind in which the most vivid mental images you can now recall, were formed?

⁶² Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

In what states of mind can you recall mental images most readily?"⁶³ elicited responses similar to that of Question 10: "When I am engaged in any kind of mental labor my visions do not come. When I am in a comparatively pensive state with no particular mental object in view, I am most apt to have images" (McCosh & Osborn, 1884, p. 61). By inquiring into the physical correlates of memory formation in questions like these Osborn was able to link qualitative inquiries into mental imagery with particular physical states. Such a connection, between descriptions of memory formation and retrieval and their associated physical states, allowed Osborn to frame his qualitative research in physiological terms and to more decisively link his biological and psychological interests.

In published accounts of his mental imagery research, Osborn discusses the specific physiological states thought to accompany initial memory formation and later mental imagery. He asserts that, "physiologically speaking, the memory of an event is a revival, a repetition of the original brain changes accompanying that event" (Osborn, 1884a, p. 480), and further that,

...in order to renew the visual signal, that is to recall our friend's face, the disturbance at the terminal station must be repeated. Our imagery then, in so far as it depends upon physical causes, consists of a literal revival of the former disturbance in the brain.

(Osborn, 1884c, p. 441)

Ascribing a connection between the purely descriptive data generated by his questionnaire research and possible physiological accompaniments of mental imagery, provided Osborn with a means of grounding, if only speculatively, his mental imagery findings in the biological science in which he had been trained.

⁶³ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

In addition to investigating the physiological states associated with mental imagery, the revised version of the Princeton Mental Imagery Questionnaire contains an item that Osborn characterized as “particularly interesting.”⁶⁴ In March 1882, Osborn informed Galton that he had “distributed a second series of questions, with several added to your original list & have been receiving some very full and interesting answers. Testimony upon the last question brings out interesting points.”⁶⁵ Osborn identifies the phenomenon he sought information on in the final item of the Princeton Mental Imagery Questionnaire as having previously been discussed by others under the terms “false,” “unconscious,” and “illusory” memory (Osborn, 1884a, p. 478), as well as “double perception,” “double presentation,” and “double thinking” (Osborn, 1884b, p. 274), and describes this phenomenon as,

a curious psychical or psycho-pathological experience which is alluded to by many writers upon psychology, and is not infrequently met with in general literature. It is that vague sentiment of familiarity we sometimes have upon entering a new experience, best expressed in the words, ‘I have seen or known all this before.’⁶⁶ (Osborn, 1884b, p. 274)

Elsewhere, Osborn asserts that his attention was drawn to this phenomenon, by a Southern gentleman; he had experienced them only in connection with scenery, and attributed them to the supposed uneven action of the nerves supplying the eyes, one side of the brain thus receiving the image before the other, and causing the second image to appear as a familiar repetition of the first, in this way giving rise to a deception. (1884a, pp. 479–80)

⁶⁴ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

⁶⁵ H. F. Osborn to Galton, March 22, 1882. Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁶⁶ This phenomenon might today be identified by the term *déjà vu*.

This Southern gentleman's ascription of a physiological cause to his experience of familiarity when encountering new scenes is in line with Osborn's previously discussed research objective: the uncovering of the physiological states associated with mental imagery. Seeking to discern the physiological cause of visualization, Osborn distributed a new questionnaire, one written to specifically to ascertain not only the physiological accompaniments of visual memory formation and retrieval, but also the physiological cause of this "interesting" phenomenon.

The modified Princeton Mental Imagery Questionnaire sought to investigate this phenomenon through the addition of a new question. The item that sought information on the nature of the experience whereby a newly encountered scene or event strikes us as familiar, reads:

Have you come suddenly upon an entirely new scene and while certain of its novelty felt inwardly that you had seen it before – with a conviction that you were revisiting a dimly familiar locality? Mention if you can an instance or two in which this has occurred. Has any satisfactory explanation of this experience ever suggested itself to you?⁶⁷

Preoccupied with providing an account of this phenomenon, Osborn (1884b) later reprinted the question in an issue of *Science*, requesting that the journal's readers forward onto him information on these kinds of experiences. In reprinting this item, he also expanded its content, adding the following questions:

How frequent is the experience in your case? Was it more frequent in childhood than at present? How soon do you usually become conscious of the deception? Does it occur

⁶⁷ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

more frequently in connection with some kinds of experience than with others? (Osborn, 1884b, p. 274)

The phenomenon Osborn was seeking information on was familiar to those completing his questionnaire. As one individual asserted,

I have experienced this feeling quite frequently. Sometimes I dream what seems at the time to be familiar, and yet different from anything dreamt before. The only explanation at all satisfactory was that I had seen some thing similar in a painting or drawing.

(Osborn, 1884a, p. 479)

While the Southern gentleman who initially inspired Osborn's research interpreted his experience of the phenomenon in terms of an underlying physiological process, this respondent interpreted the phenomenon as the result of simple frailty of memory. A further possible explanation of this phenomenon explored by Osborn was that this sense of familiarity arises as the result of "the scenes or conversations of early life – memories so distant that they cannot be brought into consciousness, yet sufficient to arouse this sense of early experience" (Osborn, 1884a, p. 481).⁶⁸

Along these lines one of the items Osborn added to his modified imagery questionnaire sought to ascertain the relationship between childhood, aging, and mental imagery ability.

Headed "*Childhood and age*" this item asked:

Have your powers of visualising varied much in your recollection? At what age were the earliest mental images formed which you can still recall. [sic] State if you can what is the

⁶⁸ "Double memory-consciousness" was later invoked by Josiah Royce (see 1888, pp. 247–8) as an alternate, non-psychical explanation for telepathic phenomena (see also Chapter 4).

character of those images, do they seem as bright as those formed in later years. [sic]

What events of your home life in childhood do you recall most readily or vividly?⁶⁹

In this question Osborn sought to discover the developmental trajectory of mental imagery ability, as well as the types of memories from childhood most likely to be recalled in later life. In responding to this item, one individual asserted,

My visualising power I feel is not so good as it was a few years ago. I do not remember when I did not have mental images. I recall very vividly my sister falling into the water and how she looked when she was rescued, tho I was not over four or five years old at the time. (McCosh & Osborn, 1884, p. 61)

As Hall would in subsequent years (see Chapters 3 & 4), Osborn used the questionnaire method as a means of obtaining information on childhood. On the basis of this research, he came to conclude that, “the images formed in childhood are with most persons clearer, brighter, and more numerous than those of later years” (McCosh & Osborn, 1884, p. 66). Osborn’s interest in the relationship of aging to mental imagery ability was part of his more general interest in variation in mental imagery ability. The investigation of the developmental aspect of the visualizing faculty is representative of one aim of mental imagery research: to discern not only individual differences in mental imagery, but also group differences in this faculty.

Interpreting Mental Imagery and Determining Difference

The specifics of Osborn’s questionnaire-based psychological research provide evidence both of his long standing interest in human variation and his pervasive concern with the quality of the American populace. Although Galton and Osborn differed in the particular aspects of

⁶⁹ Osborn, H. F. (1882). *Questions on the visualising faculties*. Henry Fairfield Osborn Papers, MSS.O835, Box 82, Folder 5. American Museum of Natural History Archives, New York City, NY.

mental imagery they each found interesting, and thus the aspects of mental imagery they sought information on in their questionnaire research, their rationales for conducting research into mental imagery were markedly similar. Both were certain that individual differences in mental imagery would be detected and sought to collect and categorize their data in such a way as to detect group-based differences in mental imagery as well. As Osborn asserted at the end of his mental imagery research, “... no one could have foreseen the extent of individual variation which this investigation has disclosed” (McCosh & Osborn, 1884, p. 62).⁷⁰ Similarly, Galton’s aim in undertaking mental imagery research was, “to define the different degrees of vividness with which different persons have the faculty of recalling familiar scenes under the form of mental pictures, and the peculiarities of the mental visions of different persons” (Galton, 1880b, p. 301). In doing so, he hoped to delineate the boundaries of normal mental imagery. Osborn’s interest in mental imagery stemmed from similar concerns. In his view,

Our mental images seldom attain the full force of reality; when they do, they are not far removed from hallucinations. The more one studies this faculty the more credit one gives to the subjective truth of visions. This transition is an easy one from a vivid image which we know to be an image, to an image which we believe to be real. (Osborn, 1884c, p. 447; see also 1884a, p. 485)

A shared concern with individual differences in mental imagery ability, and more particularly with defining the extent to which imagery is normal, rather than pathological, was central to the mental imagery questionnaire research undertaken by both Osborn and Galton.

Establishing norms of mental imagery also extended to an investigation of race and sex differences in ability. Galton hoped that his research would, “elicit facts that shall define the

⁷⁰ For discussion of group-based differences in mental imagery ability see Osborn (1884c).

natural varieties of mental disposition in the two sexes and in different races, and afford trustworthy data as to the relative frequency with which different faculties are inherited in different degrees" (1880b, p. 301). He recognized the potential existence of both individual and group differences in the ability to form mental pictures, and expected to find both types of differences in his mental imagery research. In his view

the peculiarities of visualisation, such as the tendency to see Number-forms, and the still rarer tendency to associate colour with sound, is strongly hereditary, and I should infer, what facts seem to confirm, that the tendency to be a seer of visions is equally so. Under these circumstances we should expect that it would be unequally developed in different races, and that a large natural gift of the visionary faculty might become characteristic not only of certain families, as among the second-sight seers of Scotland, but of certain races, as that of the Gipsies [*sic*]. (Galton, 1881a, p. 739, 1881b, pp. 653–4)⁷¹

Osborn similarly sought to ascertain sex and race differences in mental imagery. In the fall of 1880, he informed Galton that,

Acting upon the suggestion offered in your letter I have relied chiefly upon educational institutions, and accordingly have sent copies to Vassar (female) Princeton, & Columbia and more recently to Hampton College Va. You may know the latter institution by having heard of its singers who have been all over Europe. It is for colored students and of late they have added a department for Indians. Replies from the latter College may therefore prove interesting.⁷²

⁷¹ On number forms and coloured hearing see Chapter 2.

⁷² H. F. Osborn to Galton, November 7, 1880. Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

As Osborn indicates, Galton's questionnaire was to be completed not only by Princeton's male students, but also by female students at Vassar College and by "coloured" students at Hampton College in Virginia.⁷³ In the context of the objectives of the mental imagery research, responses from these groups were vital as they were to provide the needed material out of which to discern sex and race differences in mental imagery ability. While later investigations into topics with pervasive cultural engagement could call upon the public for assistance, Osborn's inquiry into the less popular subject of mental imagery was restricted to those at educational institutions who might be personally induced to provide responses for the project.

As this attempt to obtain responses to his mental imagery questionnaire illustrates, data collection with questionnaires was largely an opportunistic process. Within Osborn's own institutional milieu, data could be amassed through direct personal appeals for information, such as his request that those in attendance at his presentation before the College of New Jersey's Philosophical Club complete the circular.⁷⁴ Collecting information from those in one's immediate environment was relatively straightforward. Successful data gathering beyond this realm required more effort and necessitated securing the assistance of sometimes-distant individuals, something that often required leveraging existing personal relationships. In the case of efforts to obtain responses outside of the Princeton context acquaintances were called upon to collect data in their given locale, but such cooperation was not guaranteed. For instance, responses from female students at Vassar College were obtained through the assistance of the

⁷³ H. F. Osborn to Galton, April 9, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁷⁴ H. F. Osborn to Galton, April 9, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

institution's president Samuel Caldwell,⁷⁵ but other efforts to obtain data were unsuccessful. As Osborn noted

to my considerable disappointment I got no answers from Hampton Sidney [sic] College.

Nor from friends in the South (Louisiana) who expected to do something for you among the colored people so my returns are all from Americans & mostly from students.⁷⁶

In addition to a lack of responses from Hampton Sydney College, and the South more generally, information also failed to come in from Columbia College.⁷⁷ Appeals to friends for research assistance were by no means assured of success. Osborn also sought to obtain responses by enlisting Hall's aid in distributing the circular to students at Harvard Medical School (McCosh & Osborn, 1884, p. 58), though whether Hall in fact solicited responses and forwarded the data on to Osborn is unknown. It is also unclear to what extent this contact influenced Hall's (1883b) decision shortly thereafter to undertake his own questionnaire-based research (see Chapters 3 & 4).⁷⁸

From the responses to his mental imagery questionnaire Galton drew several conclusions. Firstly, he observed that both children and women possess a strong visualizing faculty, and further that women are more prone to detailed imagery than men (Galton, 1883). In his view,

⁷⁵ H. F. Osborn to Galton, April 9, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁷⁶ H. F. Osborn to Galton, April 9, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

⁷⁷ H. F. Osborn to Galton, May 10, 1881, Francis Galton Papers, 152/6B, University College London Special Collections, London, England. The responses Osborn collected to Galton's original questionnaire were eventually forwarded on to Galton and 60 of these responses – 34 from male Princeton students and 26 from female Vassar students – were later included in Galton's volume, *Inquiries into Human Faculty and its Development* (1883). Significantly, given Osborn's later social views, this is the volume wherein Galton first introduces the term "eugenic" (p. 24).

⁷⁸ Galton also considered school children as an excellent population in which to exercise the questionnaire method (see 1880c, p. 252).

“the mental difference between the two sexes seems wider in the vividness of their mental imagery and the power of introspecting it, than in respect to any other combination of mental faculties” (Galton, 1880c, p. 252). From this result he concluded that a well-developed visualizing faculty is an impediment to intellectual achievement, as “an over-ready perception of sharp mental pictures is antagonistic to the acquirement of habits of highly-generalised and abstract thought” (Galton, 1883, p. 88). Those individuals who were both successful intellectually *and* in possession of a well-developed visualizing faculty, had in fact to suppress this faculty in order to achieve intellectual greatness (Galton, 1880c, 1883). He concluded, “there is no doubt that abstract thought is best carried on without the aid of this concrete imagery, and that a natural tendency to indulge in it is liable to be repressed by vigorous brain-workers” (Galton, 1881c, p. 85). Finally, he determined that peculiarities of mental imagery are hereditary. It was this conclusion that led Galton to further conclude that differences in mental imagery between races must exist, as he reasoned that, “since families differ so much in respect to this gift, we may suppose that races would also differ, and there can be no doubt that such is the case” (Galton, 1883, p. 100).

Osborn unreservedly endorsed the conclusions Galton reached from the mental imagery research, asserting that this research brought to the fore “...the obscure influences of heredity and environment, or nature and nurture, upon our ordinary mental operations” (McCosh & Osborn, 1884, p. 58). He further voiced his conviction that, as Galton argued, women, children, and savages possess superior mental imagery skills:

This faculty of visualization is inherited: talent or deficiency in forming images is found to run in families. As regards sex and age, among women it is more keen and better developed than among men; many adults have lost the power of imagery and will tell you

they have never possessed it, but this is contradicted by the fact that among children imagery is almost universal. Savages, as far as our knowledge goes at present, are remarkably endowed with this faculty. (Osborn, 1884c, p. 444)

As is typical of late-nineteenth century discourse on difference, Galton and Osborn both characterized the mental capacities of women, children, and savage races as equally inferior (see Bederman, 1995; Noon, 2005b; Russett, 1989; Shields, 1975, 1982), although in this case inferiority was equated with the possession of a well-developed visualizing faculty, rather than with poor mental imagery ability.

The characterization of women, children, and savages as possessing a distinct range of mental imagery ability is reflected in the particulars of Osborn's questionnaire research. The discernment of sex, race, and age-based differences in mental imagery are all ingrained assumptions of his project. Not only did he deliberately set out to gain responses from both female and "colored" students, but developmental differences in mental imagery were also explicitly asked after. Although not always successful at obtaining information from these groups, questionnaires were nonetheless valuable instruments in efforts to obtain information from diverse bodies of individuals. In the context of Osborn's later eugenic commitments, his early interest in group differences in mental capacity is telling. Even in the early 1880s, more than two decades before his eugenic views became public, Osborn was explicitly concerned with detecting difference in society. This aim is evident in his assertion, within a discussion of his mental imagery research, as to "how much would be gained if we could trace the influences of heredity, of race, of cultivation or neglect, – results such as can be obtained only by pushing out inquiry among large numbers of persons" (Osborn, 1884c, p. 450). In adopting Galton's

questionnaire-based research, Osborn was able to investigate en masse just those facets of society that concerned him most.

Interested in documenting the relation of children's mentality to that of savages, Osborn set out to undertake a further psychological investigation. In June 1882, having completed his work on mental imagery, Osborn wrote Galton a final letter proposing a new research project:

At the close of the May term here I am more at leisure for two or three months and propose to start a circular paper which may interest you. The idea is to obtain the child's development in art by collection of a large number of drawings of figures made by children. What I have already done in a tentative way has proved interesting by comparison with the drawings of our American savages.⁷⁹

Like in his mental imagery project, Osborn sought to investigate the child's mind, this time even more explicitly with the aim of equating the child's mentality with the savage's. This, again, was very much in line with Galton's (1883) understanding of the mental life of primitive peoples, whose drawings he considered evidence in favour of their superior imaging abilities. Whether Osborn's project ever came to fruition is unknown.

Although both Galton and Osborn concluded that particular characteristics of mental imagery are hereditary, the more general ability to mentally represent was thought to be educable. One of Galton's reasons for undertaking research on mental imagery was in fact his conviction that a study of the visualizing faculty would be of benefit to educational practices, as "a serious study of the best method of developing the faculty of visualizing is one of the many pressing desiderata in the new science of education" (Galton, 1880a, p. 324). As a result of his questionnaire research on mental imagery, Galton concluded that, "there is abundant evidence

⁷⁹ H. F. Osborn to Galton, June 4, 1882, Francis Galton Papers, 152/6B, University College London Special Collections, London, England.

that the visualising faculty admits of being developed by education" (Galton, 1883, p. 105, see also 1880a, p. 322). Similarly, Osborn held that,

This faculty deserves far more attention from educators than it has yet received.

Visualization is inseparable from the entire mental activity of children, yet we have barely begun to take advantage of this fact or to recognize the invaluable aid it may render in enabling children to bring and retain clearly before the mind's eye such objects as geographical maps, columns of figures in mental arithmetic, or pages of music. As a matter of fact, children are constantly taking visual notes in this way; but the faculty might be brought to a much higher degree of usefulness by special practice. (Osborn, 1884c, pp. 448–9)

Despite concluding that a well-developed visualizing faculty served as a hindrance to intellectual achievement, Galton and Osborn encouraged the development of certain features of this faculty through education, interpreting imagery skills as advantageous in various technical and artistic endeavours.⁸⁰ Galton, in fact, concluded that, "the highest minds are probably those in which it [i.e., the power of mental imagery] is not lost, but subordinated, and is ready to use on suitable occasions" (1883, p. 88). Emphasis on the pedagogical value of their questionnaire-based projects aligns well with Hall's soon-to-be initiated program of child study research and also foreshadows Osborn's later commitment to public science education through his role as AMNH president (Osborn, 1911, 1912; Pauly, 1991; Rainger, 1991), as well as his more general interest in the quality of American education (Osborn, 1892, 1903, 1906, 1910, 1925, 1927). For Osborn, an ardent neo-Lamarckian, quality education was a eugenic initiative, as it offered a means of ensuring prolonged social betterment.

⁸⁰ This is in keeping with Daston's (1998) discussion of the nineteenth century relegation of the imagination to the artistic, rather than scientific, realm.

Conclusion

By every indication Osborn completed circulating the Princeton Mental Imagery Questionnaires in the spring of 1882. Although he would continue to publish on mental imagery and the results of his questionnaire research for two more years his research in this area had come to an end. By the mid-1880s his research turned increasingly to biological topics, and by the early 1890s, he had left psychological research behind for a career in vertebrate palaeontology. In 1891 he ended his tenure at Princeton and relocated to New York City. Although he was initially offered the position of Professor of Psychology at Columbia College, Osborn turned the position down, as he was no longer working in the field. He subsequently accepted joint appointments to the Biology Department of Columbia College and the vertebrate palaeontology department of the AMNH, while the position of Professor of Psychology was filled by Cattell.⁸¹ Come the twentieth century, Osborn was one of the most influential vertebrate palaeontologists in the country, with an influence that extended beyond the scientific realm (Rainger, 1991). From his politically prominent position as president of the AMNH, over a twenty-year tenure, he became one of the most noted eugenicists in the United States, a development hinted at by the interest in human variation evident in the questionnaire research undertaken decades earlier.

Osborn conducted some of the earliest questionnaire-based psychological research in the United States, but his tenure as a psychologist was short-lived. That being said, he never lost interest in the nature of mental life and related issues. In the 1890s he, as well as Baldwin (another former student of McCosh) and British comparative psychologist Conwy Lloyd Morgan

⁸¹ It was at Columbia College that Cattell instituted his most extensive program of mental testing, which involved testing all incoming students to the College and to the Columbia School of Mines on a battery of anthropometric tests (Sokal, 1987a).

(1852-1936), each seemingly independently, developed an evolutionary theory in which consciousness plays a guiding role (Baldwin, 1896a, 1896b, 1896e; Osborn, 1896, 1897; see Young, 2009, 2013). Nonetheless, for Osborn the study of mental imagery appears to have been his only foray into psychological research.

Osborn did not pursue the subject further, but the study of mental imagery did not go idle. Nearly a decade after Osborn collected his initial data, Professor of Philosophy Andrew Campbell Armstrong Jr. (1894), of Wesleyan University, took up the project again.⁸² In 1890 he obtained 43 responses Osborn had collected using Galton's original questionnaire and added to these a further 151 responses from Wesleyan students. The latter were collected from students in his elementary psychology over the course of four successive years, from 1890 through 1893, while only 37 of the former were used in Armstrong's combined analysis as the remaining six were from non-students. By and large Armstrong's results matched those previously recorded by Osborn and Galton, but he also took this work one step further. As he reported in 1894, "within the group of Wesleyan students an attempt was made to compare the power of visualization with the standard of scholarship as tested by college grades" (Armstrong, Jr., 1894, p. 505). Here, efforts to track mental ability were extended into the practical realm of college performance in much the same manner as Cattell's soon-to-be discredited method (Sokal, 1987a). Armstrong (1894) deemed the data "inadequate" (p. 505) for definitive conclusions, but nonetheless observed the apparent trend for those with lower imaging ability to be those with the highest

⁸² Like Osborn, Armstrong was educated at Princeton, earning a Bachelor's degree from the institution in 1881. He spent the next year studying psychology with McCosh as fellow of mental science. In 1885 he was awarded a master's degree from the Princeton Theological Seminary and following several years teaching at his alma mater he joined the faculty of Wesleyan University in 1888, remaining there for the rest of his career (Hildebrand, 2005).

class standings. Similarly, the small number of responses received from women pointed toward a more pronounced imaging faculty than was the case with men.⁸³

It remains unclear to what extent Osborn's early questionnaire-based inquiry inspired the later body of work with the methodology but, irrespective of its direct influence on later projects, it introduced this mode of inquiry to the American context. In doing so, it set the stage for much of the work to come with the method. While the Galtonian orientation within American psychology has traditionally been traced to Cattell's mental testing work, Osborn's endeavours provide an earlier and alternate source of this influence on the field. Subsequent investigations undertaken by not only Armstrong and Hall, but also Jastrow and Francis Speir Jr. (see Chapter 7), are all linked to this early work in some respect. As in Osborn's investigations, questionnaire-based inquiries more generally were pre-occupied with collecting information on various forms of difference. The ability to document variance in mental ability writ large came increasingly to dominate the discipline. Armstrong's explicit attempt, in the immediate wake of Osborn's work, to orient the results of early mental imagery projects toward this aim was only the beginning of a much larger trend that would only fully take shape in the twentieth century.

⁸³ Titchener's widely adopted manual for teaching psychology, *Experimental Psychology*, published in 1901, included examples of what he termed the "Questionary Method" (1901, p. 387). In the context of a series of experiments on "Ideational Type and Association of Ideas" Titchener (1901) presented the questionnaire method as the "most complete and satisfactory upon the visual side" (p. 391) and suggested its use to investigate imagery following Galton's earlier example. This form of inquiry was subsequently taken up by investigators at Vassar College, who explicitly adopted the methodology outlined in Titchener's volume (Bell & Muckenhoupt, 1906; French, 1902).

Chapter 2

Enumerating Mental Associations

In the 1880s and 1890s a number of psychologists turned their attention to mental associations of various forms, subjects Galton (1883) discussed alongside his mental imagery studies. Unlike earlier investigations of mental imagery, however, work on mental associations often demonstrated the vast similarities in the minds of men and women, rather than pronounced differences between groups. The questionnaire's ability to reveal similarities across groups, I argue in this chapter, was as much a part of the method's appeal as its ability to discern differences. Emerging eugenicists like Galton and Osborn might focus on uncovering the latter, but this was by no means the sole power of the questionnaire. Just as laboratory-based studies of limited numbers of subjects – the prototypical scheme of the new psychology in the Wundtian tradition – could not reveal difference, neither could such studies make clear similarities in mental life of assumed disparate groups. In the context of studies of mental association data conveniently fell into discrete categories, as questionnaires sought to document the presence or absence of particularly kinds of associations, particularly those related to numbers, colours, and letters. This kind of material, unlike the more descriptive data that dominated other kinds of questionnaire endeavours (see Chapter 4), held the distinct advantage of being relatively easy to synthesize into a coherent whole. The regular invocation of particular kinds of mental associations as alternative explanations for psychical phenomena imbued these kinds of investigations with further import. Documenting the ubiquity of mental associations rendered these mental proclivities particularly viable natural, rather than supernatural, causes of the coincidences so often deemed meaningful.

Central to the conversation over possible alternative explanations of telepathy in the latter years of the nineteenth century was the invocation of one particular kind of mental association: number habits (see Chapters 5 & 6). That individuals might form strong associations with certain numbers rather than others, that there might be a general preference among individuals for some numbers, and that these associations might inform the results of research into telepathy was acknowledged by all in the foregoing debate. For at least some, this state of affairs provided a plausible alternative explanation for the apparent occurrence of telepathy, one appealed to alongside other non-psychical explanations. What remained to be explained was the exact nature of mental associations, number habits included. Efforts to do just this were taken up by investigators, who surveyed the minds of large numbers of individuals. This provided a means of documenting not only the peculiarities of mental associations, but of rendering their occurrence a normal part of mental life. Much like the work on dreams and unconscious cerebration I describe in Chapter 7, research into mental associations provided a means of accounting for psychical phenomena in terms of the mind's as yet undocumented proclivities.

Even if number habits were not explanations for the entirety of findings in psychical research, these kinds of mental habits offered compelling counterpoints to often-persuasive personal experiences with coincidences. Coincidences that could be interpreted as meaningful served to support belief in the reality of psychical phenomena. In offering an account of how apparently supernaturally significant coincidences were products of the mind's natural tendencies along certain lines, the weight of personal experience as evidence in favour of psychical matters was challenged. Questionnaires, by gathering information from numerous individuals, offered a succinct means of providing a scientific counterpoint to the authority of personal experience. In documenting the pervasiveness of the mind's propensity to form

associations of various kinds, unique patterns of association were rendered a normal part of mental life. The number habits investigated by psychical researchers were tendencies to respond in certain ways, but number forms were decidedly visual. Thus, much of the research on mental associations may also be understood as a variation on basic perceptual and sensory research, which was the hallmark of early laboratory psychology. This research also allowed for discussion of individual variation impossible in paradigmatic experimental work, which was confined to adult, male, white minds (Danziger, 1985, 1990).

The mental imagery studies undertaken by both Galton and Osborn involved inquiries into what were termed “visualised numerals” (Galton, 1881d, 1883), also referred to as number forms (see Chapter 1). These forms were unique spatial depictions of numbers characterized by Galton (1880c) as “usually fantastical and sometimes very elaborate” (p. 253) and were “by no means the same in different persons, but assumes the most grotesque variety of shapes” (Galton, 1881e, p. 87). More specifically, for those who possess number forms whenever a number is thought of, it appears in the same place on a visual diagram which is invariably called up, viewed by the mental eye, often definitely located, and which usually consists of an irregular composition of lines on which the figures appear either written or printed. (Phillips, 1897a, p. 506)

Though mental imagery and number forms were Galton’s (1883) main interest within the field of mental associations he also made mention of colour associations with letters. This work, along with studies by a number of other European investigators, were touchstones for much of the work on mental associations taken up by American investigators.⁸⁴

⁸⁴ In addition to Galton, work by Théodore Flournoy (Calkins, 1895a; Phillips, 1897a), Alfred Binet (Calkins, 1892, 1893a), and Henry Beaunis was referenced in the context of American inquiries (Calkins, 1892, 1893a).

Investigations of mental habits by American psychologists were of two kinds. On the one hand were investigators, including George Thomas White Patrick, Mary Whiton Calkins, and Daniel Edward Phillips, who sought to document the nature, variety, and commonness of mental associations through the circulation of questionnaires. This included number forms, but also alphabet forms and coloured hearing. On the other hand were efforts to enumerate individuals' number preferences. This work, most notably that of Jastrow with respect to the results of the 1880 census, used great stores of data to demonstrate number habits. Both were statistical undertakings, but each was of a slightly different orientation. While Jastrow emphasized group differences in his collected data, Patrick, Calkins, and Phillips all downplayed difference in order to frame mental associations as normal features of mental life common across groups. In each instance, the investigation of entities with a limited range possible values (numbers, colours, letters, etc.) ensured that analysis and interpretation of collected data was relatively manageable, at least along certain lines.

Joseph Jastrow and Census Statistics

In the mid-1880s Jastrow (1885b), at the time a fellow in psychology at Johns Hopkins University, turned his attention to what he characterized as “some peculiarities” of the most recent United States census (see Pettit, 2013b). The census, undertaken in 1880 under the direction of statistician Frances Amasa Walker, surveyed a national population of just over 50 million individuals in an effort to document, and thus manage, a changing American populace. Expanded census work undertaken in the late nineteenth century, led by statisticians like Walker, took on a broader social role beyond political apportionment as “business associations, reformers, and the new university men” (Anderson, 1988, p. 85) pushed for the collection of data in line with their interests. This involved the growth of social statistics to document changing

social life, including both economic changes as well as the collection of vital statistics on “defective, dependent, and delinquent classes” (Anderson, 1988, p. 85). Outside of official census undertakings, the social survey simultaneously became a popular means of documenting the particularities of American life in various locales (Bulmer et al., 1991; Katz & Sugrue, 1998).

According to Jastrow’s analysis, the individuals accounted for in the 1880 census demonstrated a marked preference for certain kinds of numbers when declaring their age on the census. The general goal of his analysis was to “shed light on the characteristics of the natural bias in favor of round numbers” (Jastrow, 1885b, p. 464).⁸⁵ In the context of contemporaneous psychical research where chance coincidences were imbued with significance, documenting this kind of particularity of mental life served to counteract superstition. Charles Minot’s earlier reference to mental habits as explanations for otherwise inexplicable correspondences between individuals was received favourably by Jastrow who saw the “neglect of the natural community and similarity of men’s thoughts” (Jastrow, 1889, p. 81 fn.) as a problem in need of addressing. In the case of especially close companions, who are familiar with each other’s thought-habits and thus able to predict their association of ideas, this was even more clearly the case (Jastrow, 1895). If anything, Jastrow, thought “knowing, as we do, how closely alike are our modern education and interests, the wonder is, rather, that these coincidences are not more frequent and startling” (Jastrow, 1887, p. 115).

In the case of the census, the American public showed evidence of pervasive number habits in particular directions. Individuals were disposed to report their age in round numbers, especially multiples of ten (20, 50, etc.), rather than the odd numbered ages immediately

⁸⁵ He went on to suggest a more practical ambition for his project: “a means of suggesting modifications in the method of questioning which would obviate these misrepresentations” (Jastrow, 1885b, p. 464).

preceding these values (19, 49, etc.). To examine the strength of this tendency, Jastrow (1885b) began by “subtract[ing] the number of persons recorded as 9 years old from the number recorded as 10, and express[ing] this excess in percentage of the number at 9 years” (p. 461) and doing the same with every age ending in 9 and its immediately following round number. Averaging the resulting 9 percentages produced what he termed the “10 exaggeration” (Jastrow, 1885b, p. 461), with each individual percentage serving as the 10 exaggeration for its multiple of 10 (i.e., the 10 exaggeration at 20, the 10 exaggeration at 50, etc.).⁸⁶ The overall 10 exaggeration for the entire population of the United States was calculated to be 71.25%, though it ranged from a low of 9.5% at 10 years of age to a high of 126% at 60 years of age.

The 10 exaggeration served as the basis for Jastrow’s discussion of race and gender based differences in numerical preferences.⁸⁷ According to his calculations, this tendency was particularly strong among the foreign born, females, and “colored” people which, for the purposes of his calculations, included Chinese, Japanese, and “civilized Indians” (Jastrow, 1885b, p. 462 fn.). To concretize what was otherwise a rather abstract discussion of the propensity for some groups to display – or over-display – a penchant for certain numbers Jastrow produced two line graphs of the 10 exaggeration for the population as a whole and each of his groups. In the first of these figures each group was further divided by gender (see Figure 1). Of the stark presentation of the data in his Fig. 1, Jastrow contended “the enormous exaggeration of the colored people is the first striking, appalling fact” (Jastrow, 1885b, p. 462). With an average 10 exaggeration of 432% the group’s tendency to prefer round numbers was more than 6 times

⁸⁶ Jastrow (1885b), at least for the purposes of this analysis, assumed that individuals were predisposed to round up to the nearest multiple of 10. The possibility that individuals might round down in the same fashion was not addressed.

⁸⁷ Jastrow’s attention to difference was by no means confined to this analysis. For instant, he contended that the propensity to hypnotization was dependent “upon the nationality, class, temperament, and so on, of the individuals observed” (Jastrow, 1889, p. 78, see also 1891c).

that of the population as a whole. A similarly patterned "5 exaggeration" was also noted, and represented in Jastrow's Fig. 1 with cross-marks on each of the 10 exaggeration lines.

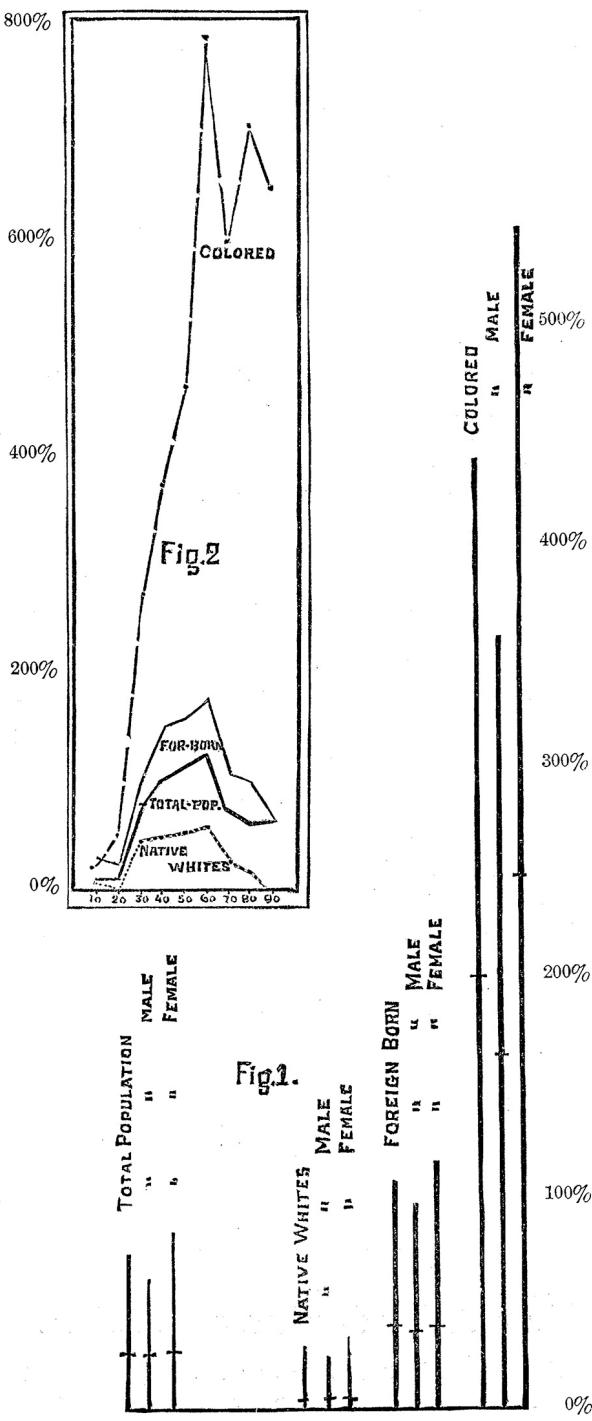


Figure 1. The 10 exaggeration for various groups. Reproduced from Jastrow (1885b, p. 462).

Jastrow singled out a number of factors to explain some of the specific peculiarities of number preferences. The greater prevalence of the 10 exaggeration in the foreign born and “colored” people was, in part, attributable to illiteracy as he had worked out that “a set of lines representing the illiteracy for the several races and sexes would closely resemble fig. 1” (Jastrow, 1885b, p. 462 fn.).⁸⁸ Illiteracy might correspond with number exaggeration, but it was not a complete explanation for the phenomena. Jastrow (1885b) noted, “the negro is the being upon whom all the various causes tending to produce this peculiar falsity of returns are the most active” (p. 462). The group next most susceptible to these causes was the foreign born. Though relatively high rates of 10 exaggeration in foreign born individuals might be explained in large part by illiteracy, as in “colored” individuals, the “doubtless misunderstandings between the foreigner and the census official, owing to a meagre acquaintance with the language, enter as an additional disturbing influence” (Jastrow, 1885b, p. 462). No such additional factor was offered to account for the even higher rates of number preference in the “colored” group. For this group illiteracy served less an explanation for their pronounced 10 exaggeration than as yet a further indicator of the group’s more general, and assumed innate, inferiority.

Both race and place were positioned as especially pernicious influences. Separating the census data by state, Jastrow noted a tendency for native white males of southern states to show more pronounced 10 exaggerations on account of what he pointedly remarked to be “too close intimacy with the ‘round-number loving’ negro” (Jastrow, 1885b, p. 463).⁸⁹ Such an influence

⁸⁸ Jastrow did not offer an explicit rationale for why illiteracy would produce a pronounced ten exaggeration. Perhaps he held the illiterate to be less aware of their exact age and thus more likely to select a multiple of ten when reporting their age, or the illiterate to be children of other illiterate individuals who were unable to accurately record birth date information for their children.

⁸⁹ Jastrow (1885b) did not explicitly consider what role education, or the lack thereof, and particularly rates of illiteracy might have played in producing these results.

was, in his view, “dangerous to statistical accuracy” (Jastrow, 1885b, p. 463). The corrupting influence of “colored” individuals was responsible for elevating the 10 exaggeration of native white males beyond the more “normal” levels displayed by, for instance, those in the New England states. Gender was similarly determinative, even within groups already singled out for their deficiency. The large number exaggeration exhibited by elderly “colored” women, nearly twice that of their male counterparts, was attributed to “colored” women being “far more forgetful of their ages than old men” (Jastrow, 1885b, p. 463).

One form of number exaggeration was, however, exclusively the domain of native white males. Though this group was the one least disposed to the 10 exaggeration that was at the centre of Jastrow’s analysis they did demonstrate a clear preference for one number: 21. This exaggeration was difficult to gauge with any degree of precision because its closest lower counterpart, 20, was itself part of the 10 exaggeration. Similarly, 19 proved inadequate for comparative purposes as it was presumed underreported given the exaggeration of 20. Nonetheless, Jastrow noted that there were more males at 21 than 19, and more so than was the case with females. The exaggerated number of males at 21 was explained purely on the basis of the social utility of the age for a white male, but not a white female, or “colored” male for that matter: “21 being the voting-age, and 1880 the year of a hot presidential campaign” (Jastrow, 1885b, p. 464).

Amassing a larger body of information on the American public than that assemble for the 1880 census was simply inconceivable. Unlike others who created questionnaires to gather information on specific aspects of mental life, Jastrow capitalized on this existing data set. An easily accessible mass of data, the census allowed him to avoid the perils of large-scale data collection and its attendant labour. At the same time, in order to ascertain anything of value

regarding mental habits, Jastrow had to extrapolate from data collected for another purpose entirely. This was both advantageous and limiting. On the one hand, the data allowed him to make grand claims regarding the regularity of certain tendencies of the mind. On the other, the project very decidedly lacked the introspective element so common in large-scale data collection with questionnaires. Using a mass of already collected data, assembled for another purpose, meant that individual experiences with the mental habit he identified was decidedly absent from the analysis. What remained, however, were the kinds of “curves and averages of surprising regularity” (Jastrow, 1895, p. 573) that Jastrow privileged.⁹⁰ This kind of effacing of the personal element was not limited to undertakings where the inclusion of such was impossible. More generally, Jastrow (1900) held the “tendency, to insist that the laws of science shall be precisely and in detail applicable to individual experiences possessing a personal interest for us, has wrought much havoc” (p. 84, see also Jastrow, 1895, p. 573) including the perpetuation of belief in psychical matters. The solution, in his view, was “to insist upon the statistical nature of the inquiry” (Jastrow, 1900, p. 84). The census and smaller scale questionnaire inquiries were one means of countering personal experience with statistical data.

⁹⁰ Jastrow’s analysis of census data was one of many such undertakings with the findings from the 1880 endeavor, largely as a consequence of Walker’s successful organizational and data collection efforts (see Anderson, 1988). One other such instance was Alexander Graham Bell’s interrogation of the enumeration of individuals classified as blind, deaf-mute, idiotic, and insane in the United States and his observation that there had been a relative increase in the number of these individuals in the populace over time (“Census of the defective classes,” 1889). Further discussion of the oddities of the census’s age statistics, in relation to race and place, was undertaken by Albert Williams, Jr. (1889) several years later. Williams (1889) was drawn to the subject by an encounter with a numerically based “so-called mind reading trick,” in the context of which he observed “the number 7, though so common among the Semitic races, is a rare one with Aryans. We have for favorite numbers 5s, 10’s, and strangely enough, 3’s” (p. 11008). It was this work, rather than Jastrow’s, that Phillips (1897b) cited in his work on number preference (see also Sanford, 1903).

As Jastrow (1887) recognized in the context of psychical research, “the human mind is not calculated to act like a die-box or a raffling-wheel” (p. 116).⁹¹ Having pairs of individuals guess numbers and comparing their matches with calculations of the probability of chance coincidences was consequently a failed research approach. Census statistics only served to further reinforce this view. In subsequent decades other psychologists continued this line of research. Both Fletcher Dresslar (1899) and Edmund Sanford (1903) undertook projects on number preferences in guessing, in each case collecting a large body of statistics of the guesses made by numerous of individuals within the context of a public guessing contest.⁹² Efforts to explain number preferences in these investigations resorted to appeals to longstanding superstition, such as Dresslar’s (1899) contention that

as lower peoples developed the need of numbers and the power to use them, certain of these numbers came to be surrounded with a superstitious importance and endued with certain qualities which led at once to numerical preferences more or less dominant in all their thinking connected with numbers. (p. 784)

Superstition could be used to explain why individuals chose certain numbers, and in the case of psychical phenomena were included as explanatory factors for the correspondence between individual’s guesses. While continuing to reference the kind of superstitious thinking invoked by

⁹¹ And more so, “to have numbers chosen is a different thing from having them drawn. In fact, it is possible to suggest a certain kind of number-preference by the framing of the question” (Jastrow, 1887, p. 116).

⁹² In Dresslar’s (1899) case material was secured from a Los Angeles clothing company that ran a contest with a one hundred dollar gold prize for those who could guess the correct number of seeds in a very large uncut squash. More than seven thousand individuals offered guesses as part of the contest. Sanford’s (1903) data was amassed as part of a guessing context, ostensibly held by a Worcester photographic supplies dealer, which offered as a prize a valuable camera. Individuals guessed the number of beans held within a five-pint jar displayed in the shop window. Nearly three thousand guesses from more than seven hundred individuals were recorded.

Jastrow within his assessments of psychical belief, later researchers also recognized the import of contextual factors. Sanford (1903) in particular noted that number habits “are not fixed and constant, as seems generally to have been assumed, but vary characteristically with variations in the conditions under which the guessing is carried out” (p. 401).⁹³ Rather than assuming number habits to be fixed characteristics of individuals or groups situational factors were appealed to as necessary features of any observed tendency of individuals to choose certain numbers over others.

Collecting Mental Association

Efforts to collect accounts of mental associations were more involved than Jastrow’s census work. Psychologists seeking to elucidate the nature of a variety of mental associations opted to employ the questionnaire method as a way of amassing their own data. Central to many of these investigations, as in Galton’s earlier inquiries, were the enumeration of number forms, but efforts in this direction did not stop with simple numerical associations. Rather, inquiries into number forms often rapidly expanded to include efforts to describe a wide variety of mental associations, including various synesthesia. From the late-1880s through the mid-1890s three projects were undertaken along these lines.

Trained predominantly as a philosopher Patrick, like a number of others during this period, expressed an interest in the emerging new psychology. In the 1880s, following three years of graduate study at Yale University, he moved to Johns Hopkins where he studied philosophy and psychology with Hall.⁹⁴ Although he was more interested in the former than the

⁹³ Notably Sanford (1903) found no difference in the guesses of men and boys in comparison to those of women and girls.

⁹⁴ Patrick (1895a) was also active in the child study movement. See also ‘National Association for the Study of Children: Preliminary Announcement;’ ‘Organization and Plan of

latter field, Patrick minored in psychology and economics. At Hopkins, he was part of a generation of students – including John Dewey, Cattell, Jastrow, William Burnham, James Hyslop, Henry Donaldson, and Sanford – who went on to make significant contributions to a number of fields including, psychology, philosophy, neurology, and education. In 1887, prior to completing his doctoral studies, Patrick was called back to his alma mater, the University of Iowa, to take up the position of Professor of Mental and Moral Science and Didactics. A year later, upon hearing Hall was leaving Hopkins for the newly founded Clark University, Patrick temporarily returned to Baltimore to complete his examination for doctor of philosophy. Shortly thereafter, in 1890, he established a laboratory of psychology at the University of Iowa.⁹⁵ In the 1890s, like many American students of the time, he travelled to Germany for further instruction. There he visited Oswald Külpe and Wilhelm Wundt, and briefly worked in the latter's laboratory (Cantor, 1991; Patrick, 1932). Ultimately, more philosophically than psychologically inclined, Patrick was nonetheless active in the field especially prior to psychologist Carl Seashore's 1897 arrival at the University of Iowa (Patrick & Gilbert, 1896, 1897).

Although at least nominally credentialed in laboratory psychology, through his studies with Hall and his time in Germany, Patrick's psychological investigations were not confined to this space. In the late 1880s, having not yet established a laboratory at his institution, he developed an interest in number forms and opted to investigate the phenomena via questionnaire. As he described, “the general character of number form [*sic*] is such that a person having one can not think of the related numbers without *seeing* them in a definite visual picture” (Patrick, 1893, p. 507). To document the existence and character of these forms, Patrick set out to gather

Work of the National Association for the Study of Children,” B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁹⁵ This was only the 10th such laboratory to open in North America (Garvey, 1929).

information on these forms of thinking from numerous individuals. Over the course of four years, several dozen examples of number forms were collected, explicitly following the line of work set out by Galton earlier (see Patrick, 1893).

Over the course of his investigation Patrick altered the form of his questionnaire. As a result, the body of data collected was not all from the same set of questions. Earlier in his studies questioning was brief and pointed: “When you think of the numbers from 1 to 100, do you mentally see them in any form, or outline? If so, can you draw a representation of it?” (Patrick, 1893, p. 505). As would be the case with both Phillips (1897a) and Calkins (1892), later questioning was expanded to include queries on a variety of mental associations. This included not only questions on numerical associations, but also ones on alphabet forms and alphanumeric colour associations:

1. When you think of the numbers from 1 to 100, do you see them in any particular form?
If so, will you write or draw it on paper?
2. When you think of the alphabet from a to z, do you see the letters in any particular form?
3. Have you any associations of color with the numbers or letters? (Patrick, 1893, p. 510)

Both sets of questions were asked of college age individuals, largely those between 18 and 25 years of age, both male and female. This included 75 individuals who responded to the first iteration of the questionnaire and a further 29 to the second extended version. Of these, 30 and 8 individuals, respectively, were female.

Toward the end of Patrick’s work on number forms Mary Whiton Calkins began her own systematic inquiry into the matter. At Wellesley College, Calkins and her students undertook extensive questionnaire-based inquiries into number forms and other mental associations, far

beyond those of other early American investigators. These investigations were part of Calkins's longstanding interest in the more general nature of association (Calkins, 1893b, 1896a, 1896b, 1898; Münsterberg et al., 1894). Trained initially in Classics and Philosophy at Smith College and hired in 1887 to teach Greek at Wellesley, she was first approached in 1888 about the possibility of teaching a course on the new psychology at the College. The position was briefly postponed, but in 1890 Calkins was made instructor in psychology at the school, under the condition that she obtain a year of training in the field. After exploring several options, including travelling abroad to study in Germany, as well as opportunities for study with John Dewey at the University of Michigan and George Trumball Ladd at Yale, Calkins endeavored to arrange for graduate training at the geographically nearer Harvard University. In addition to its proximity, Harvard had the advantage of possessing what neither the University of Michigan nor Yale University did: a laboratory of psychology (Scarborough & Furumoto, 1987).

Thanks to lobbying from her father and Wellesley's president, Calkins managed to secure from the Harvard administration the right to attend courses in psychology in 1890. As a woman, however, she was never formally admitted as a student. At Harvard, Calkins studied with both William James and Josiah Royce, though this work was largely philosophical rather than experimental, despite the presence of a psychology laboratory at the institution. To supplement these studies, she arranged to work with Sanford in Clark's psychology laboratory (see Chapter 7). As at Harvard these studies were informal, as Clark similarly did not permit female students to officially enroll at the University (see Chapter 3). With Hugo Münsterberg's arrival at Harvard in 1892, to take over operation of the university's psychology laboratory, Calkins returned to the institution for several years of further training in experimental psychology (Calkins, 1930;

Scarborough & Furumoto, 1987).⁹⁶

Throughout her studies in psychology at both Harvard and Clark, Calkins continued to teach at Wellesley. There, in 1891, she established the first psychology laboratory at a women's college in the United States. Shortly thereafter research with questionnaires was incorporated into her course on experimental psychology through the study of mental forms and associations. For Calkins, there was no necessary distinction between experimental laboratory investigations and those undertaken via questioning with circulars. Both served as central components of her teaching and therefore Wellesley students were introduced to psychology as a science that could be practiced through a variety of investigatory methods.⁹⁷ Calkins established a formal laboratory of psychology, and students were instructed in the use of the instruments within it, but there was also room for studies that did not necessitate the use of this kind of formal scientific space.⁹⁸ Work with questionnaires was one such mode of study.⁹⁹

In the fall of 1892, following a period of training in laboratory psychology and her establishment of a laboratory at Wellesley, Calkins (1892) initiated a new psychology course at the school: "Psychology, including Experimental Psychology" (p. 260). As part of this offering,

⁹⁶ This work culminated in her completion of all the necessary requirements for a doctoral degree in psychology. Although a successful unsanctioned doctoral examination was held, with James, Royce, Münsterberg, and other Harvard faculty members, Calkins never received an official doctoral degree from the university (Scarborough & Furumoto, 1987).

⁹⁷ Other undertakings on association from the Wellesley laboratory also included work explicitly following Galton's example (see Calkins, 1898).

⁹⁸ For descriptions of other non-laboratory bound research projects see Calkins (1892).

⁹⁹ Earlier in the decade Calkins was also involved in questionnaire research, as a participant rather than an investigator, as part of James's American census of hallucinations (see Chapter 5). Her questionnaire-based psychological research does not seem to extend beyond the 1890s, though she did continue to assist with data collection for others' studies into the twentieth century (see Thayer, 1905) and in 1910 undertook a questionnaire-based investigation of the state of psychology education in institutions that lacked laboratories (Calkins, 1910) as part of a larger inquiry on the part of the American Psychological Association's Committee on the Teaching of Psychology (C. E. Seashore, Angell, Calkins, Sanford, & Whipple, 1910).

students completed a number of experimental investigations in the new psychology laboratory, largely based on those outlined by Sanford (1891a, 1891b, 1892) in his “Laboratory Course in Psychology,” as well as other experimental work, including studies of attention and space-consciousness, among others (see Calkins, 1892). These kinds of experimental investigations were not the only research conducted in the course. Also undertaken was the “collection of statistics about colored hearing and number-forms” (Calkins, 1892, p. 269),¹⁰⁰ that is, an inquiry into forms of synesthesia, focusing particularly on the association of certain sounds with a given colour and the association of numbers with particular spatial arrangements (see also Calkins, 1893a). This inquiry into mental associations began in the spring of 1892 and continued into the fall of that year with wider reaching annual circular studies conducted at the College in later years (Calkins, 1893a, 1895a). As in other inquiries, Calkins’s questionnaires were easily alterable instruments and subsequently circulated versions were regularly reconfigured in light of knowledge gained from previous undertakings (see Calkins, 1895a).¹⁰¹

¹⁰⁰ Both of these subjects were later investigated at Vassar College in the first decade of the twentieth century (Rose, 1909). Experiments on single individuals with such associations were also conducted during this period (e.g., Dresslar, 1903). For an overview of some of the early psychological research on synesthesia see Campen (1999).

¹⁰¹ Also undertaken during this period were educationally oriented studies by Calkins and her students on what was termed “mathematical consciousness” (Calkins, 1894). This included a further questionnaire-based inquiry that compared those who liked mathematics and those who did not, men and women, and those who preferred algebra to those who preferred geometry on a number of separate points. As part of this inquiry 30 male students at Harvard and 87 female students at Wellesley, in addition to specifying their positions regarding the previously listed points, responded to the following questions: “Do you find it easier to remember (a) words? or (b) how things look, sound or feel?” (Calkins, 1894, p. 272), “Do you remember easily or with difficulty?” (Calkins, 1894, p. 273), “Can you carry a long calculation in the mind?” (Calkins, 1894, p. 274), “Do you naturally classify or group (e.g., notice the likenesses and differences of two declensions or of two botanical families)?,” “Do you reason out or remember a demonstration?” (Calkins, 1894, p. 276), and “Do you readily supply intervening steps in a demonstration?” (Calkins, 1894, p. 278). As part of this investigation a small body of material was also collected on the sense-type of numerals (visual, articulatory, auditory, or graphical) in children. Included in this research report was a related study comparing the degree of

Much like James McKeen Cattell's soon-to-be initiated efforts at submitting all incoming students to Columbia College and the Columbia School of Mines to his series of largely anthropometric mental tests, a practice that began in 1894 and continued into the twentieth century (Sokal, 1987a), Calkins attempted something similar with her circulars. Following the initial investigations of 1892, the questionnaires were systematically distributed to all freshmen students at the College for two years beginning in 1893 (Calkins, 1895a, see 1930).¹⁰² Unlike Cattell's attempt to construct tests that revealed something about the subject's overall mental ability, the aim of the Wellesley studies was decidedly less ambitious. Questionnaire-based investigations of mental associations were not intended to serve as tests revelatory of larger mental functioning, but were simply framed as studies of particular psychological phenomena. This limited field of inquiry – as well as the project's relatively short duration – may well have spared Calkins the kind of downfall that Cattell's project met with when his student Clark Wissler, trained in statistical methods by anthropologist Franz Boas, revealed that the Columbia mental tests did not correlate to any significant degree with academic achievement (Sokal, 1987a).

Shortly after the conclusion of Calkins's research project, Phillips undertook a further questionnaire-based investigation into numerical associations. At the time a student at Clark

imagination in children who did and did not like arithmetic according to their varying degrees of arithmetical ability (Calkins, 1894). For this investigation children's

imaginative power was tested by the request to make up a little story about the sunflower and the rose; and by the following questions:

If you were a king or a queen, what would you do?

Like what does the wind seem to you to sound?

If the wind were a person, like what would it look? (Calkins, 1894, p. 280)

¹⁰² Calkins (1895a), in the fall of 1895, expressed her intent to continue the project several years longer, though it does not appear that this in fact transpired. In her autobiographical reflections only annual investigations along these lines for 1893 and 1894 are mentioned (see Calkins, 1930, p. 35) and no further research reports were published.

University, Phillips was later appointed Professor of Psychology and Education at the University of Denver (see Phillips, 1913). Initially, he sought information on numerical associations as part of a broader circular inquiry into “Numbers and Mathematics” initiated in April 1896.¹⁰³ This undertaking was largely oriented toward pedagogical considerations, particularly improvements that might be made to the teaching of numbers and mathematics as a function of insight into children’s acquisition of this kind of knowledge (Phillips, 1897b).¹⁰⁴ Under a section in the circular headed “Psychological and Anthropological” several questions were directed to numerical associations (see Phillips, 1897b).¹⁰⁵ Most interesting to Phillips’s (1897b) mind was an item that inquired into experiences with number forms,

- (f) Cases of number forms, *e.g.*, the first 12 numbers being habitually associated with a dial or clock face, the first ten on a line, straight or curved, systems of dots, colors, etc.

Do odd seem to you different from even numbers? Draw any number forms. How do you

¹⁰³ “Numbers and Mathematics,” April 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. In 1899 at Clark E. B. Bryan, with Hall’s assistance, undertook related studies into “Mathematics in Common Schools” (Feb. 8, 1899, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA) and “Mathematics in the Early Years” (Feb. 15, 1899, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA). Phillips was not involved with this work, but did later undertake another questionnaire-based inquiry. This 1898 study of “The Teaching Instinct” served as his doctoral dissertation (Phillips, 1898a, 1898b). A later circular investigation of “The Elective System in American Education” was also conducted (Phillips, 1901).

¹⁰⁴ Phillips’s (1897b) understanding of the development of the number sense, particularly his description of the separate development of ideas of number in terms of series and ratio, was disputed by John Dewey (Dewey, 1897a; see also Phillips, 1897c).

¹⁰⁵ “Numbers and Mathematics,” April 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Most of the returns Phillips received to his circular addressed only questions under the head “Psychological and Anthropological.” Although he received more than 800 responses almost 200 of these “were found of little value in any way” (Phillips, 1897b, p. 248) and were excluded from his discussion. Williams (see Chapter 3) was once again one of the main data collectors for this investigation (see Phillips, 1897b, p. 278).

arrange days of the week or month, the musical scale? (p. 240)¹⁰⁶

For Phillips (1897a), more intense interest in the nature number forms “was first aroused by some strange and complicated answers” (p. 508) to this question. Since “this section did not cover the points of greatest interest” he set out to construct a more complete questionnaire on the subject (Phillips, 1897a, p. 508).

To do so Phillips personally questioned more than 300 normal school students on the subject. With the knowledge gleaned from these interactions, he assembled a new set of questions. Although the project began as an inquiry into number forms, over its course it expanded to document a host of other mental forms including specific iterations of number forms such as “month-forms” and “week-forms,” as well as “alphabet-forms” and forms associated with the Lord’s Prayer and songs (Phillips, 1897a, p. 509). Regarding mental forms more generally he inquired:

1. At what age did it appear? 2. How did it originate? 3. Is it useful, or troublesome? 4.

Do you see the figures on a line? 5. How large does it appear to be? 6. Where is it located? 7. Are you left-handed? 8. Do you know of any forms in your family? 9. State any peculiarities about your form or its use. 10. Do you like mathematics? 11. Give name, age, and sex. (Phillips, 1897a, p. 508)¹⁰⁷

Using this set of questions, Phillips accumulated responses from more than two thousand individuals, including 969 men and 1040 women.

¹⁰⁶ “Numbers and Mathematics,” April 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁰⁷ This syllabi is not included – as a circular distinct from the “Numbers and Mathematics” syllabi – among those in the Clark Archives, nor is it listed in Hall’s (1924) accounting of the syllabi issued at Clark during his tenure.

Surveying Students and Student Surveyors

Central to the accumulation of information on mental associations was the involvement of students in the research process. Patrick (1893) collected what he imprecisely identified as between 35 and 40 examples of number forms over the course of four years, which he noted “were collected from or by my students” (Patrick, 1932, p. 415). Left unclear was whether the same students served as both subject and investigator. If indeed the case, this kind of role exchange was well within the parameters of practice of the new psychology. What Danziger (1985) has identified as the Leipzig model, the form of practice most closely associated with laboratory investigations of the period, is characterized in large part by regular role exchanges between experimenter and subject. In the context of those teaching the new psychology, questionnaires provided a technically simple research scheme with which non-specialist students could engage.

A reliance on students, as both investigators and subjects, was also an integral feature of Calkins’s work on mental associations, though her subjects also extended to members of the larger university community. In her case, the decision to undertake questionnaire-based research was informed by her understanding of the psychology course as “a general one” that needed to be “adapted to students without especial scientific training or without particular interest in experimental work” (Calkins, 1892, p. 260). Calkins’s initial inquiry into mental associations involved questioning 543 people, most of whom (526) were members of the Wellesley College community. As she and her students discovered, 98 of these individuals experienced either or both coloured hearing and number-forms. Of these, 32 experienced only coloured hearing, 78 experienced only number-forms, and 14 experienced both coloured hearing and some kind of

number-form (Calkins, 1892, p. 269).¹⁰⁸ As a class assignment, this data was collected by students “aided by a simple set of questions” (Calkins, 1892, p. 269). Whether subjects were required to respond to these questions by themselves filling out answers on paper, or students verbally questioned individuals and recorded their responses, is unclear. Whatever the procedure for amassing information on mental associations, this procedure revolved around a set of questions that guided data collection, a procedure that Calkins later explicitly identified as questionnaire-based (Calkins, 1930, p. 35). Once again students, at least in some instances, served as both data collectors and offered self-observations of their own mental associations for the project (see Calkins, 1893a). Unlike the Wundtian Leipzig model that underpinned much laboratory psychology, questionnaire research did not necessitate that participants be trained in the new psychology. Relying instead on a Galtonian model of psychological science, this work engaged a variety of individuals as participants with an eye toward accumulating information on populations (Danziger, 1985, 1990). Even still, the role exchange of subject and experimenter characteristics of Leipzig model extended to practice in this realm as, at least in some instances, students served both as data collectors and as sources of information themselves.

The students and educators involved in Phillips’s work were of a more diverse makeup than in previous inquiries. Phillips was himself a graduate student at Clark during the tenure of his project and the data he gathered was largely from student populations of various kinds, as was often the case in questionnaire investigations undertaken at the institution (see Chapters 3 & 4). Of the more than two thousand responses collected with his second circular, over three hundred were from normal school students and nearly one thousand from Worcester school

¹⁰⁸ Only a paragraph later, Calkins (1892) specifies that 33 individuals experienced coloured hearing. This accounting of those with mental associations does not align with her more general claim that 98 individuals experienced one or both kinds of associations.

children between 10 and 16 years of age. Outside of these groups, a further 360 responses were received through the general circulation of his syllabus and 343 miscellaneous responses were obtained through personal questionning undertaken by Phillips. Of the latter group 92 were teachers and a further 41 were from the Clark University community. As in Calkins's work, both students and educators comprised the bulk of research subjects.¹⁰⁹ Additionally, the responses collected from normal school students, as in his initial inquiry on the subject (Phillips, 1897b, p. 278) and the questionnaire-based research conducted at Clark more generally, were almost certainly the result of the assistance of normal school instructors in data collection. Across these endeavors students, and the broader body of individuals associated with educational institutions, were key to the accumulation of masses of data.

Difficulties with Data Collection

Across these endeavours data collection was fraught with difficulties. Simply asking questions of individuals, while at face value an imminently straightforward undertaking, was in reality a much more challenging process. Even communicating exactly what was being asked after was problematic. Phillips (1897a) noted of his efforts to survey individuals that “those who have no form have no idea of what you are speaking of, and are often slow to comprehend any explanation, appear surprised or treat indifferently what you say” (p. 510). In much the same way, many of the individuals Patrick questioned at first reported they had no experience with the kind of mental associations asked after. Despite this initial denial, in his work at least, further questioning often revealed that these associations did exist. As he observed,

it would seem that a person having even a complicated number form might live and die

¹⁰⁹ Phillips also solicited at least some responses to his questionnaire by approaching individuals in public spaces. He at one point remarks “six men in a shop were asked if any of them has a number-form” (Phillips, 1897a, p. 510) and then went on to reproduce the number form of one of these men (Phillips, 1897a, p. 513).

without knowing it, or at least without once fixing his attention upon it or speaking of it to his nearest friends, although such a one might use his form in daily computation. It seems to him quite natural to see the numbers in that way, and the thought may never enter his mind that others should see them differently. (Patrick, 1893, pp. 505–6)

Oftentimes number forms were such an ingrained part of thought processes for individuals that they were not immediately aware of them as a distinct feature of their mental life. The necessity of undertaking additional prodding to get individuals to accurately characterize their mental functioning makes clear that questionnaires were not discrete objects bound to the printed page. Rather, printed lists of questions served as prompts to data collection that sometimes needed to be supplemented by further inquiries on the part of the researcher. In this investigative scenario questionnaires extended beyond the written word on the page to encompass more informal verbal inquiries, the traces of which remain largely elusive.

Other investigators similarly recognized the need for more personal questionning. Though Phillips (1897a) did not observe the same necessity of pushing past initial claims of unfamiliarity, he nonetheless “personally interrogated” (p. 508) a subset of his respondents. These kind of personal inquiries were one means of obtaining further responses to questionnaires from individuals who might otherwise be reluctant to take the time to write out responses to a printed circular of questions. In the case of data collected from school children, in person questionning served as a means of clarifying what was asked after. As Phillips (1897a) describes, the process of collecting data from children involved providing “a short explanation” of mental forms to a classroom of children and then requesting that they “draw whatever form, or forms, they had” (p. 509). Those who produced forms were then questioned privately in more detail. Recognizing that collecting information from children on their mental life was problematic,

Phillips (1897a) argued that the data, while certainly not error free, “cannot be far wrong” since a similar proportion of adults reported mental forms and nearly all traced their forms to the earliest days of childhood (p. 510).

Efforts to ensure the existence of mental associations relied on multiple tactics. In the case of mental forms, both Phillips (1897a) and Calkins (1892) turned to individual’s drawings of their forms as confirmation of their existence. Drawings were sufficient to authenticate mental forms, but non-visual associations necessitated other forms of verification. In the case of research undertaken at Wellesley, reports of coloured hearing were substantiated by re-questioning individuals two months after their initial responses were recorded (Calkins, 1892). Of the body of collected data Calkins (1893a) contended,

the figures given ... have been carefully verified, and every effort has been made to interpret accurately through personal interviews, or through correspondence the exact meaning of the subjects. Hundreds of letters have been written and scores of interviews have been held. (p. 439 fn.)

The reality of individual experiences of mental associations was also communicated through the inclusion of detailed case descriptions in research reports (see Calkins, 1893a). Narratives of these kinds were persuasive evidence in favour of the authenticity of personal experience, even if only a fraction of the collected cases were presented in this manner. This was similarly the approach taken in much of the questionnaire research conducted at Clark (see Chapter 4). Additionally, much like James’s attempts to verify accounts of hallucinatory experiences (see Chapter 5), Calkins and her students expended considerable time and energy in their endeavour attempting to confirm claims of mental associations.

In questioning and re-questioning individuals it also became evident that mental associations were not transient states but were rather extremely stable across the lifetime. Calkins (1895a) saw her work as providing a “virtual demonstration of the stability of the experience” (p. 91). Of nearly 200 individuals questioned for a second time about their experience of photisms (i.e., synesthesia) and mental forms, between several months and a year later, all but one were found to still possess the same association (Calkins, 1895a, 1930). This finding corroborated Patrick’s (1893) earlier contention that individuals questioned years apart would “draw from his mental picture of it a copy differing in no essential respect from the original copy” (p. 507). Similarly, Phillips (1897a) re-questioned some of the 332 normal school students a year after his initial collection of information and “found no change of any note” (p. 509) in the mental forms reported by these individuals.

Unsurprisingly the content of questionnaires was crucial in amassing data deemed valuable by investigators. Consequently, Patrick, Calkins, and Phillips each altered their circulars over the course of their data collection efforts. In Calkins’s case her 1894 circular, “Questions on Synesthesia” was explicitly altered as a result of challenges observed in previous years’ inquiries.¹¹⁰ Some items from earlier iterations of the circular were removed because of what was described as difficulties obtaining accurate responses to certain questions or near unanimous responses from those sampled (Calkins, 1895a, p. see p. 90–1). The final iteration of her questionnaire began with a set of four preliminary questions:

- I. Do you think of particular colors in connection with letters of the alphabet, or numerals, or proper names, or musical sounds, or in any other unusual connection?
- II. Do you think of numerals, or names of months, days or years or of any series of

¹¹⁰ The questionnaire is reprinted in full in Calkins (1895a, pp. 101–7).

words, as arranged in particular shapes, like circles, squares, zig-zags, or very long lines?

III. Do single numerals, letters, musical notes, etc., make you think of different shapes?

IV. a. Do you especially like or dislike any numerals, letters, etc.?

b. Do numerals, letters, etc., seem to you to be like people? (Calkins, 1895a, p.

102)

For each question individuals, on the basis of personal experience, were instructed: “Answer by ‘Yes,’ ‘Yes?’”, or ‘No.’” Do not fail to answer “Yes?” *not* “No,” if in any doubt” (Calkins, 1895a, p. 102). An answer of “Yes?” was to be given by anyone who was at all unsure about whether they possessed such associations. While Patrick sought to overcome the foreignness of mental associations for many individuals through additional questioning, Calkins chose to surmount this difficulty by providing individuals with a concrete alternative response that was neither “Yes” nor “No.” On the basis of these initial responses, those who answered “Yes” or “Yes?” to any of the items were asked to complete more detailed questions on the subject. Like James’s earlier census of hallucinations (see Chapter 5), this questionning began with a simple accounting of the prevalence of the phenomena before moving on to more detailed inquiries. Limiting responses to a predefined set of possible answers ensured that at least this subset of the data could be collated with ease.

The more specific questions on associative phenomena were divided into three topic areas – pseudo-chromesthesia,¹¹¹ forms, and personification – and filled multiple pages.¹¹² The questions were prefaced with the instruction: “Many of these questions may be answered by

¹¹¹ This term was earlier defined by Clark fellow William Krohn (1892) as a “class of phenomena in which colors are called up in the mind of the subject when certain letters or words are spoken, or seen in print or writing” (p. 20).

¹¹² In reprinting the questionnaire as part of a 1895 journal article these questions spanned 7 printed pages (Calkins, 1895a, pp. 101–7). The length of the printed version that was distributed for data collection is unclear.

“Yes” or “No,” but fuller replies are preferable. It is hoped that all questions will be answered, but the less important ones are starred” (Calkins, 1895a, p. 102). This instruction was a response to the difficulties with data collection encountered in previous years. Although more than 500 people were included in the initial 1892 investigation into mental associations, an 1893 effort to sample all incoming students at the College was unsuccessful. This failure was due to a common difficulty in these kinds of projects: many individuals sampled simply did not respond or did so carelessly. As Calkins (1895a) described, “every member of the freshman class was questioned, but the preliminary inquiry was by circular, and the traditional objection to answering statistical inquiries may be responsible for many careless, negative replies” (p. 91). To address this issue, the project of sampling the next year’s freshman class was more involved. This included addressing the class directly wherein “the purpose of the investigation was explained, and the preliminary questions were answered before the students left the room” (Calkins, 1895a, p. 91). This was followed by the more detailed set of questions, which were provided “either by circular or by personal interview” (Calkins, 1895a, pp. 91 & 93, see also 1930, p. 35). The decision to simplify the questionnaire by indicating, via the presence of an asterisk, which of its many questions were less important and therefore might be skipped in the interest of time, was a further means of addressing students’ reluctance to devote their time to the project. Those in a rush, it seems, might still provide profitable responses by focusing their attention on the asterisk-less questions Calkins deemed most valuable. Even when lengthy descriptive responses were provided to questions, the field of inquiry was confined to particular forms of mental associations. By focusing on these clearly defined categories of types of experience, the organization of messy, descriptive data on the particularities of colour, number, and alphabet associations could be streamline to some degree.

The challenges of data collection extended beyond attempts to survey Wellesley students. Calkins reports – separate from her other figures – the case of anomalous data collected by Lillie Williams of the New Jersey State Normal School in Trenton. Although Williams questioned 250 students at the school, she found only a small fraction of pupils experienced either coloured hearing (5 cases) or number forms (6 cases). This was in direct contrast to Calkins's (1895a) finding that

of every ten persons five at least have some peculiar, fixed form of mental imagery, and that of these five two are likely to have photisms and four to possess some mental form, while three must admit some other kind of apparently erratic association. (p. 93)

To explain this discrepancy, Calkins (1895a) noted that many individuals who possessed some degree of associative tendencies did not report so, presumably responding “No” rather than “Yes?” to preliminary questionning, and that nearly half of the subjects “answered the questions hastily in time taken from other work” (p. 93).

Like Patrick before her, Calkins cautioned that individuals were often unaware of the operation of mental associations until further prodding. In Calkins's case, this observation became a way of reconciling disparate findings between data collectors in different locales. Doing so was an exercise in diplomacy, especially in the case of findings produced by particularly active collectors. Williams, one of the most productive collectors of questionnaire data (Carnicom, Faye, & Baker, 2011; Hall, 1924), could not simply be written off with a few choice words regarding the poor quality of the gathered data, or left out of the discussion entirely, but had to be appeased with reference to her skill. Just as Hall regularly praised Williams's contributions to his child study investigations (see Chapter 3), Calkins (1895a) noted “this report of a careful observer is certainly worthy of consideration” (p. 93). While Phillips

(1897a), for instances, could simply express his thanks to the “authorities of the Worcester schools, to Supt. Carroll and the teachers of Worcester” (p. 527) for their cooperation with his scheme of work, such platitudes were insufficient in the case of active data collectors.

Irrespective of whether Calkins was correct to dismiss Williams’s findings as the result of substandard questionning and excise them from her larger body of data, her careful framing of the work of another investigator provides a glimpse into the personal dynamics on which data collection rested.

The Roots of Mental Associations

As in other questionnaire-based research endeavours efforts were made to connect mental associations with physiological states, if only superficially. Calkins (1893a), understood an observed “hereditary tendency of colored-hearing and of forms” as indicative of “cerebral changes” (p. 452) that accompanied the phenomena. Multiple family members who possessed mental associations were evidence of a hereditary element for Calkins, but not Phillips (1897a) who contended

I find no more signs of, nor reasons to look for, heredity here than in any highly developed power of imagination, memory, art, music, etc., all of which are much questioned since Weismann’s theory of heredity has become prominent. Two in the same family may have like forms by mere coincidence. (p. 520)¹¹³

Patrick avoided discussion of heredity, but explicitly dismissed attempts by others to link mental associations with particular cerebral centres. Responding to William Krohn’s (1892) earlier assertion that “pseudo chromesthesiae phenomena arise from some sort of cerebral work which is the outcome of the close relation of the cortical centers, which are connected by numerous

¹¹³ Phillips’s exact interpretation of Weismann’s work in this context is left unspecified.

associational fibers, notably the visual and auditory centers” (p. 38), he instead noted that most colour associations, and all number forms, could be better accounted for with reference to the usefulness of a given association (Patrick, 1893).

Calkins also speculated that the prevalence of mental associations was the result of their utility in childhood. Despite a lack of children investigated with regard to their mental associations, retrospective accounts revealed that almost all these associations dated back to childhood (Calkins, 1893a).¹¹⁴ Patrick (1893), meanwhile, speculated number forms were “the attempt or necessities of children to give a concrete form to the abstract” as numbers “are bald abstractions that the poor child must manage in some way” (p. 513). Calkins (1893a) similarly concluded that, “visualization of numerals or of word-series may be an important aid to memory, especially in a child’s first struggle with numbers. Accidental associations of this sort may then be perpetuated because of their helpfulness” (p. 450; see also Patrick, 1893, p. 514). Other mental associations, such as coloured-hearing and name-associations, were also posited as the outgrowth of useful childhood associations. Most, however, could not be confidently traced to any particular childhood experience. For instance, of twenty respondents only one affirmative response was received to the question “Can you compare your form with any pattern of wall-paper or carpet, with any crack in the plastering or with any other line which you might have seen when learning the numbers in childhood?” (Calkins, 1892, pp. 270–1).¹¹⁵

¹¹⁴ In his discussion of number forms, Galton (1880c) had previously noted “young people see forms more commonly than adults, but that their forms are less developed and sure...where they are vivid and serviceable they are much used, and insensibly grow in vividness, in definition, and in automatic character. Otherwise they decay from disuse and become forgotten” (p. 495).

¹¹⁵ On his first syllabus Phillips (1897b) similarly asked after “Cases of “eye geometry,” or automatic puzzling over patterns of carpet, wall paper, bricks on sidewalks, strong fondness for tracing the forms of decorative ornamentation, spontaneous drawing of such patterns or development of them in the use of kindergarten material” (p. 240). See also, “Numbers and

Both Calkins's and Patrick's speculation about the childhood roots of mental associations took place without any direct investigation of children's experiences with the phenomena. Calkins (1895a) noted on her circular "canvasses of men's colleges, or college classes, of associations of people in middle life, of schools of children and young people, and of the accidentally blind and deaf, would yield especially valuable results" (p. 102). Nonetheless, she did not pursue information from these populations herself. The lack of research with these groups was not a consequence of inaccessible research populations. In other instances, Calkins's students studied infants and young children directly (see Calkins, 1892, 1894) and secured data from male students at Harvard (Calkins, 1894). Some Wellesley students even sought out specialized populations for study, including one student who investigated imagination in the blind through the study of children at the Perkins Institute (see Calkins, 1892). Questionnaire-based research with children, however, was problematic. In the context of other research, Calkins and her students directly addressed the difficulties of asking children for information on their mental life and in that instance opted "to avoid asking them questions requiring an introspection which would have been artificial and difficult" (Calkins, 1894, p. 280). Although other populations were at least nominally available for research purposes, Calkins confined her work on mental associations itself to the more readily accessible population of Wellesley College women who to her mind, unlike children, were capable of unproblematic introspection.

As a consequence of the largely female community from which her subjects were drawn, discussion of gender differences in mental associations was completely absent from Calkins's work. This was not the case in other Wellesley projects, notably those into mathematical consciousness, which directly investigated gender differences and found "no important

distinctions” (Calkins, 1894, p. 279) between men and women.¹¹⁶ Patrick (1893), following Galton, observed that number forms were “perhaps a little more common among women than men” (p. 506), while Phillips (1897a) observed little in the way of difference between the genders in the presence of mental forms. The lack of these differences was in direct contrast to Galton’s (1883) earlier contention that mental forms were twice as numerous in women as in men and other contemporaneous discussion of pseudo-chromesthesiae phenomena which contended “the larger number of the subjects are women” (Krohn, 1892, p. 36).¹¹⁷ While questionnaire-based inquiries allowed for evidence to be collected with respect to gender differences in mental life this did not, of course, necessitate the discovery of such differences. In the case of investigations into mental associations gender differences, where addressed at all, were largely absent.

Like so much questionnaire-based research that into mental associations appealed to the pedagogical value of this characteristics of the mind. More generally, Calkins (1894) and her students contended “no serious psychological study can be undertaken which does not lead to pedagogical conclusions” (p. 269). Tracing, for instance, the close connection between “the number, time, and space concepts” provided number with “a wider application” (Phillips, 1897b, p. 277; see also Calkins, 1897). Along these lines Adelia Hornbrook, a teacher at Evansville, Indiana’s Classical School, promoted the view that number forms might be employed with great benefit in educational enterprises. Inspired by Galton’s work on the subject and Hall’s child

¹¹⁶ A similar absence of gender differences was found in the related inquiry into the relationship between imagination and arithmetical ability, as well as fondness for arithmetic. Here it was concluded “imagination is the basis of every psychic process and, in some form or another, of all intellectual ability; that not only the artistic, but the scholarly or the practical child is imaginative, each in his different way” (Calkins, 1894, p. 283).

¹¹⁷ Krohn (1892) went on to say that women “as a class can hardly be called *introspective*; at least they are less so than men – but they are more observant” (p. 36).

study endeavours (see Chapters 3 & 4), she attempted “to promote directly the growth of number forms in the minds of children” (Hornbrook, 1893, p. 473) through a variety of formal numerical schemes. Calkins (1895a) spoke approvingly of this approach:

the use of charts and of diagrams is in itself a suggestion of mental forms, for calendars and primer pages lie at the basis of many month, week and alphabet forms. To make these suggestions more definite, and, in particular, to impress the child’s memory, as Miss Hornbrook does, with some simple number-form, seems a reasonable, pedagogical application of these forms. Such aid to the visual imagination might not aid the essentially “ear-minded” children, but it could do no harm unless unduly pressed. (p. 100)

Although supportive of these kinds of efforts, this applied work was not something Calkins, or other investigators of mental associations, pursued.¹¹⁸

A Common Phenomenon

In surveying large bodies of individuals, mental associations were increasingly classified as common features of the normal mind. After questioning individuals closely about their mental life, and pushing past their initial claims of unfamiliarity with the subject, Patrick was prepared to revise Galton’s previous estimate of the prevalence of number forms. Rather than one in thirty adult males, and one in fifteen adult females, possessing number forms Patrick (1893) was “inclined to believe that one out of six adults would be a more accurate proportion” (p. 506),

¹¹⁸ Galton (1880d) similarly contended “I believe the forms to have been mnemonic diagrams, invented by the children when they were learning to count verbally, the sounds of the successive numerals being associated with the successive points of the forms....On this supposition we possess in these numerical forms a representation of the route along which the attention naturally travels in the mental field of view of the child. It is entirely the child’s own way of working, and therefore true to his nature; and being natural, it persists through life and offers itself in the adult for our examination” (p. 495).

with little by way of gender differences.¹¹⁹ In his view it was the failure of many individuals to immediately recognize the subject being inquired after that rendered previous estimates of the prevalence of number forms far too low. Mental associations were similarly pervasive in the populations sampled by Calkins and Phillips. Phillips, however, accounted for the discrepancy between Galton's figures and those of American investigators by noting number forms may be of numbers in general or for specific types of numbers (e.g., days of the week, months of the year, etc.). Galton considered only the former in his estimation of prevalence, while both Patrick and Calkins included the latter in their estimates (see Phillips, 1897a).

Not only were mental associations like number forms a regular feature of the normal mind, more emotionally tinged associations were as well. Calkins (1892) found that a number of respondents reported strong emotional associations and

a sort of dramatization of numbers or of colors. Thus one subject writes, “1, 2, 4, 7 and 8 are reliable quiet, well-disposed, but not brilliant numbers; 3 is a sharp shrewd, noisy and disagreeable number always making as much trouble as possible. For 13 I always had a great antipathy. It had all the disagreeable qualities of 3 added to a pertness and aggressiveness which made it repugnant to all the other numbers, with which it seemed never to associate. I never wanted to be thirteen years old.” (p. 270)

This kind of personification of numerals was also noted by Patrick (1893), who referenced two cases of women who possessed strong associations of numbers with particular types of individuals, such as “6 = a young man, plain, matter-of-fact person, slow, good; will never amount to more than the average” (p. 509). On his first circular Phillips (1897b) similarly inquired “Do the figure forms have any moral or personal character, as: 8 looks happy, 7 cross, 4

¹¹⁹ Patrick (1895b) later outlined in great detail his views on the psychology of women.

solid, etc.?" and "Describe any rare case of pupils who associate number processes with personal acts or dispositions, as *e.g.*, four is peaceable, and dwells tranquilly with eight, but with seven it feels and acts out enmity, etc." (p. 240).¹²⁰ Though these kinds of inquiries were absent from his later questionnaire, he nonetheless saw number forms as directly related to other mental association "including Pseudo-Chromesthesia, dramatization of numbers (that is, the giving to certain numbers the characteristics of certain persons, animals, or objects)" etc. (Phillips, 1897a, p. 526). More generally Calkins (1895a) found that a number of individuals, particularly those experiencing coloured-hearing, reported that their given mental association was pleasurable.

The Possibilities of Statistical Analysis

The end result of these questionnaire-based investigations was to render mental associations, broadly conceived, a normal part of the mind. Rather than position mental associations as psychical phenomena, researchers like Calkins, Patrick, and Phillips positioned these tendencies as regular characteristics of normal minds. This was in direct contrast to other contemporaneous investigations, including the census of hallucinations, which sought to amass information on abnormal mental life as a means of substantiating the reality of psychical phenomena (see Chapter 5). These kinds of associations – of number forms, alphabet forms, coloured-hearing, and more – were positioned as recurrent features of mental life across individuals, whether male or female, young or old. As Calkins (1897) noted of Phillips's work, "it tends to lure the number-form from the *terra* more or less *incognita* of the abnormal, into the familiar domain of the normal psychic life" (p. 682). Mental associations might well be

¹²⁰ "Numbers and Mathematics," April 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

idiosyncratic, but they were by no means abnormal.¹²¹ Nor were they a means of making distinctions between groups of various kinds.

Statistical undertakings held the possibility of revealing pervasive differences, but such differences – and more importantly – their meaning was not self evident. These kinds of studies allowed individuals like Jastrow to argue for demonstrable differences between men and women but they also allowed others, most notably Calkins and her students at Wellesley, to argue just the opposite. Shortly after the completion of her circular studies of mental associations, Calkins and her student Cordelia Nevers engaged Jastrow in a debate over the conclusions of his study of the community of ideas of men and women (Jastrow, 1891a, 1891b, 1894a). Having asked his students “to write out at their leisure one hundred words as rapidly as possible” (Jastrow, 1891b, p. 559) Jastrow found that female students produce a less variable set of words than male students and that their words tended to refer to concrete rather than abstract ideas. While these findings were themselves up for debate (see Calkins, 1896c; Nevers & Calkins, 1895),¹²² the idea that these kinds of findings were evidence of innate differences in the minds of men and women was the much more contentious issue.

For Jastrow (1891b) work on the community of ideas of men and women was an opportunity to obtain “material capable of yielding in objective form the results of unconscious and natural mental processes” (p. 559). It was these “natural mental processes,” he argued, that showed evidence of distinct differences between men and women. Though the minds of men and women were different, Jastrow did not contend that these differences appeared regularly in each

¹²¹ Personal accounts of mental associations were provided by a number of individuals during this period, including one on the personification of numerals offered by Margaret Whiting (1892), and another on the colours of letters from David Starr Jordan (1891).

¹²² Additional research on the subject was undertaken nearly a decade later by Manchester (1905).

and every individual. Rather, what the statistical method offered was “the data for new and suggestive generalizations” (Jastrow, 1894a, p. 152) including ones regarding pervasive differences in the sexes at large. Speaking of “the much-discussed question of the differences in brain characteristics of men and women” he offered that “the acknowledged inability of an expert anatomist to determine whether a particular brain belonged to a man or a woman” (Jastrow, 1895, p. 573) was not conclusive evidence of the absence of brain differences. The difference between men and women’s brains was not to be detected at the individual, but at the statistical level:

Give the anatomist a goodly number of fairly selected brains and tell him that all the women’s brains are in one group and all the men’s brains in another, and he will tell you which group is masculine, which feminine; and this more than offsets his failure in the former test. It establishes a statistical regularity. (Jastrow, 1895, p. 573)

It was only in the context of statistical undertakings, like those into the community of ideas, that regular and inescapable differences could be detected.

Attempts by others to explain the differences between men and women with reference to differences in experience failed to sway Jastrow (1896). Nevers and Calkins (1895) argued that generalizations from Jastrow’s findings were impossible as they were based upon “the comparison of men and women of entirely different training” (p. 366-7). While, Calkins (1896c) contended, “a statistical study may truly, if sufficiently extended, establish characteristic differences in the interests of men and women” attempts to distinguish fundamental differences between male and female minds, with respect to the community of ideas, is “futile and impossible, because of our entire inability to eliminate the effect of environment” (p. 430).

Wading into the debate, Amy Tanner (1896) pointed out that it was impossible to attribute the

differences in men and women's thought to either inherent mental differences or to differences in life experiences as the origins of these differences were in no way addressed in the studies.¹²³

Elsewhere, addressing the difficulties of ascertaining whether less variation in the mentality of inferior races was the result of innate differences, Jastrow (1898) noted

differences of life and training are so prominent ... that adequate and comparable standards of estimation are not easy to find. Yet, as man in a measure fashions his own environment, that environment itself becomes a significant index of his variability. (p. 307)

It seems that even differences in the environment were, for Jastrow, ultimately traceable to the inherent limitations of certain groups.

His statistical studies allowed Jastrow the opportunity to document what he presented as innate differences in variability of the mental lives of minds of men and women.¹²⁴ At the same time, he also proposed that the community of ideas in the public at large was becoming increasingly similar. In an effort to account for purportedly psychical phenomena, notably coincidences ascribed to telepathy, he argued that "mental community" in thought was pervasive and only becoming more so. In his view, "with the marvellously increased facilities for the dissemination and transportation of thought, the range of such mental community is correspondingly extended" (Jastrow, 1895, p. 572). The result of modern life, with its expanded circulation of knowledge, was that people's points of view, education, literature, and thoughts more generally were becoming more and more similar producing a "common life" (Jastrow, 1895, p. 572). This growth in "common life" provided an alternative explanation for

¹²³ On Tanner's contribution to the community of ideas debate see Pettit (2008).

¹²⁴ On the variability hypothesis, which proposed that men and women were differently variable in their characteristics, see Shields (1975, 1982).

coincidences otherwise marked as telepathic. Even still, women's mental community necessarily remained that much more similar than the combined community of men and women. And in the context of psychical research, this similarity was that much more dangerous. A less variable community of ideas meant that women were that much more prone to the kinds of coincidences used to substantiate psychical phenomena, making them – depending on your point of view – ideal or problematic subjects of psychical research.¹²⁵

Conclusion

Detailing the pervasiveness of mental habits provided another means of countering psychical explanations of coincidences. That the mind was predisposed to certain lines of thought, and did not operate as a “die-box” (Jastrow, 1887, p. 116), was well documented by psychologists in the final decades of the nineteenth century. Unlike the messy descriptive data that dominated other questionnaire-based inquiries, studies of mental habits were able to deal, at least in part, with clearly demarcated values. With the scope of information limited in this way, investigators were better positioned to analyze and draw conclusions from a bulk of material. While Jastrow described mental habits as a function of innate differences between groups, a series of questionnaire-based inquiries into mental associations gathered evidence in support of the normality of these mental predilections. Of course, as in other circular investigations, gathering information on mental life was a complicated process in many respects as it necessitated extended questionning and the careful navigation of personal relationships. The end result of these labours was an understanding, counter Jastrow, of this facet of men and women's minds as functionally identical. Questionnaires, though capable of describing variation, were also able to reveal similarities rather than differences.

¹²⁵ Discussion of the role of women in spiritualism can be found in Basham (1992), Lehman (2009), and Owen (1989).

Chapter 3

Interrogating the Mind of the Child:

Child Study, Collecting, and Female Educators

Science has broken into the field ... Its method is not that of the spider, that spins out of its own inwardness, but that of the ant and the bee, many individuals working together, and each content to toil long if he may bring at last a tiny stone, solid and well hewn, to the great but unfinished temple of science. (Hall, 1890, p. 18)

The new psychology, as it took shape in the United States, was almost immediately concerned with issues of social relevance. On this front child life and associated educational practices were some of the first targets of the developing discipline. Throughout the final decades of the nineteenth century a number of psychologists capitalized on widespread interest in child study and undertook research on child development, while also framing the larger growing body of psychological knowledge as educationally significant.¹²⁶ This work acknowledged that children's minds were especially elusive, particularly by way of the laboratory-based experimentation that characterized much of the rhetoric, if not always the reality, of the new scientific psychology. Unable to interrogate the child's mind in the same manner as the standardized adult, white, male mind (see Danziger, 1985, 1990), psychologists instead undertook extensive case studies and questionnaire-based projects.¹²⁷ In doing so,

¹²⁶ On the history of child study in relation to psychology see especially Brooks-Gunn and Johnson (2006), Davidson and Benjamin, Jr. (1987), Ross (1972 esp. ch. 15), Shuttleworth (2010), Smuts (2006), and Zenderland (1998).

¹²⁷ Among those who reported case studies of children are Baldwin (1895a), Darwin (1872, 1877), Preyer (1888), Shinn (1900), and Sully (1895). On baby biographies in Imperial Germany see Brian (2011), and on the English context see Shuttleworth (2010). Milicent Shinn's

research was conducted in venues outside of the idealized space of the psychology laboratory.

Adopting a natural historical scientific orientation, these projects enlisted the assistance of psychological laypersons – especially female educationalists of various stripes – in extensive data collection endeavours. With masses of data in hand psychologists then struggled to find meaning in the mayhem.

In this chapter I trace the involvement of psychologists and educators in a multitude of questionnaire-based projects on children's mental life in the final years of the nineteenth century, before turning in the next chapter to the larger discussion over the appropriate methodology, and scientific soundness, of this kind of work. The literature on the history of psychological investigations of children includes little sustained discussion of the methods used in these inquiries or the contributions of non-psychologists to this work, elements that are at the core of my discussion.¹²⁸ Central to psychologically oriented child study research during the Progressive Era were Granville Stanley Hall and his associates at Clark University.¹²⁹ Although Hall began his questionnaire-based research in the 1880s, it was not until the mid-1890s that this work took the shape of a definite research program in which scores of Clark graduate students undertook their own questionnaire-based projects.¹³⁰ In employing questionnaires Hall put into practice his

American attempts to create a network of female observers of infant and toddler life is discussed in von Oertzen (2013).

¹²⁸ Among the works that address this history are Brooks-Gunn and Johnson (2006), Grant (1992), Green (1995), Herman (2003), Noon (2001, 2005b), Ross (1972), Shuttleworth (2010), Smuts (2006), White (1990), Wozniak (1995), and Zenderland (1998).

¹²⁹ Although now more than four decades old, Ross's (1972) biography of Hall remains the most complete account of his life and work. For somewhat more intimate accounts of his life and work see his autobiography (Hall, 1924), as well as a biography by his former student, Lorine Livingston Pruette (1926). On his scientific persona see Bordogna (2005).

¹³⁰ Although questionnaires continued to be produced at Clark until 1915, I focus here on the earliest years of this project, which were in many respects the most productive period of work. It was in this first decade or so that the project engaged most with the larger field of

conviction that “the modern psychologist is also to some extent a naturalist” (Hall, 1901, p. 138).

As naturalist, the psychologist sought to collect information on all of the aspects of mental life available to them and for Hall the questionnaire was *the* instrument of doing so. Those adopting this naturalistic approach engaged in psychological research in the field, notably in homes and schools, instead than enacting science within the sanctified space of the laboratory. In doing so, the form of knowledge amassed in these endeavours was of a naturalistic, rather than experimental, character.

The process of collecting masses of information on various features of child-life for psychological research was difficult. In order to do so efficiently, non-specialists were engaged in data collection efforts. Within the context of the nation’s popular child study movement, it was most often parents and educators who were enlisted to aid with these projects, as these individuals had unique access to child-life. This access constituted a kind of “residential” (Kohler, 2006) or “experiential” (Vetter, 2011) knowledge, one rooted in the common experiences of lay individuals in various contexts (Vetter, 2011, p. 132), in contrast to the “cosmopolitan” knowledge of psychologists which was decidedly “more global and theoretical” (Kohler, 2006, p. 157). The experiential knowledge of parents and educators was central to the success of psychologists’ efforts to research child-life via questionnaire and constituted an early form of citizen science (e.g., Lezaun & Soneryd, 2007; Marres, 2007; Marres & Lezaun, 2011). As in other data collection efforts during this period, women were some of the primary labourers in these child-study endeavours. Charles Booth’s *Life and Labour of the People of London*, one of the earliest social surveys, employed female researchers (see Bales, 1996), Jane Addams’s Hull House engaged Chicago-based women in social survey work (see Deegan, 1988), and Igo

psychological practice. For an overview of the work in child study across the United States until the mid-1890s, including an extensive accounting of the syllabi circulated, see Wiltse (1895).

(2007) has identified women as central to survey-based twentieth century data collection efforts.¹³¹ More particularly psychological in nature were the efforts by soon-to-be psychologist Milicent Shinn, beginning in the 1890s, to establish a network of mothers as scientific observers and recorders of infant and toddler-life under the auspices of the Association of Collegiate Alumnae (von Oertzen, 2013).

In this chapter I argue that female educators comprised a valuable network of collectors for questionnaire-based psychological research on children during the final decades of the nineteenth century, and that their male psychologist counterparts carefully confined this work to particular realms. This kind of engagement of lay individuals in psychological work was characteristic of much early questionnaire research, which was predicated for its success on a more distributed form of scientific practice than that typical of the Leipzig model of laboratory psychology (Danziger, 1985, 1990). For educators seeking some form of professional status, participation in this kind of work offered a much-needed avenue to authority over childhood and educational practices.¹³² Their work was characterized as valuable, but decidedly not scientific in and of itself. The very clear delineation of responsibility in questionnaire-based projects ensured that scientific authority remained in the hands of psychologists like Hall and those training with him at Clark. Once information was collected, it was this latter group that endeavoured to make sense of the mounds of reports before them. Doing so in any systematic or comprehensive way was a challenge, one never fully overcome, as will be discussed further in the next chapter.

¹³¹ For further discussion of many early social survey projects, and the involvement of women in such, see Bulmer, Bales and Sklar (1991).

¹³² On women's roles in education see Apple (1985) and Feinstein (2001).

G. Stanley Hall and Child Study

Hall's involvement in questionnaire research was not a fleeting endeavour, but a very nearly all-encompassing pursuit. Often remembered today as one of the first scientific psychologists, instrumental in the establishment of laboratory psychology in the United States, Hall in fact conducted little experimental research (Hall & Jastrow, 1886; Hall & Motora, 1887). Outside of his administrative and organizational work – founding the American Psychological Association, establishing and editing multiple periodicals, and serving as president of Clark University (Ross, 1972) – it was questionnaire research that occupied much of his time. Between 1894 and 1906 Hall and his extended network of students and associates distributed 147 distinct questionnaires, or “topical syllabi,” on a plethora of topics, including “the early sense of self,” “moral education,” and “training of teachers” to name but a few.¹³³ Most often oriented to aspects of child life, the number of syllabi issued at Clark rose to more than 200 by the time the research program came to an end in 1915 (Hall, 1924).¹³⁴ As early on in this endeavor as 1896, Hall asserted that in response to copies of questionnaires that had been distributed to various individuals, “at least a hundred thousand returns” (Hall, 1897b, p. 184) had been received, with sixty thousand returns received in 1895-1896 alone (Ross, 1972).¹³⁵ Even if an overstatement it is clear, nevertheless, that huge quantities of questionnaires were distributed, completed, and returned by a number of individuals during this period. Measured in terms of sheer data amassed, Hall's questionnaire-based project was a momentous success.

¹³³ Topical Syllabi, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹³⁴ Hall lists 194 questionnaires in his autobiography, but a number of other circulars that do not appear on this list are included among those in the Clark Archives bringing the total to just over 200 topical syllabi issued by Hall and his associates (Topical Syllabi, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹³⁵ The total number of responses to the questionnaires distributed between 1894 and 1915 is unknown as none of this material has survived.

Hall was born in western Massachusetts in 1844. After studying at Williams College and New York City's Union Theological Seminary, he spent several years in Germany studying philosophy before returning to the United States and working for a time as a teacher. Resuming his academic pursuits, he studied psychology with William James at Harvard University in the 1870s. These studies led him to obtain, in 1878, the one of the first doctorates in psychology conferred within the United States. A subsequent period in Germany studying physiology also included a period of work in Wilhelm Wundt's laboratory, making him the first of many American students to study with this key figure of the new scientific psychology. Returning to the United States in 1880, Hall faced an unfavorable hiring environment and only gained regular employment two years later, when he obtained a temporary academic position as a term lecturer in philosophy at Johns Hopkins University in Baltimore. In 1884 he became professor of psychology and pedagogy at the institution. Four years later he left Hopkins for the presidency of Clark University in Worcester, Massachusetts, where he spent the remainder of his career.¹³⁶

Upon his return to the United States from Germany in 1880, Hall became involved in the American child study movement.¹³⁷ Through a series of popular public lectures on pedagogy, organized and funded by Harvard, Hall engaged with educators and secured a leading role in the burgeoning field. Those with an interest in education, many of whom were Boston schoolteachers, gathered Saturday mornings in the winter of 1881 to hear Hall speak on various pedagogical topics ("The board of overseers," 1881). The popularity of these lectures led to the organization of similar lectures for two subsequent years (Ross, 1972; "The past year at Harvard," 1883). Following his move to Baltimore he continued to lecture publicly on pedagogy

¹³⁶ On Hall's life and work see Ross (1972).

¹³⁷ Ross (1972) attributes the formation of a British Child Study association to a visit to an American child study meeting by James Sully and three female teachers. On child study in Britain see Shuttleworth (2010).

on behalf of Johns Hopkins University (“Work at Johns Hopkins,” 1884). Trading on the scientific authority of his psychological training, Hall fashioned himself as an educational expert, capable of guiding the course of the nation’s child study movement. Massachusetts, having already witnessed the innovation of Francis Parker’s Quincy system of education in the 1870s, was a particularly fertile ground for such an undertaking (Ross, 1972). More generally, the final decades of the nineteenth century were a period of substantial educational reform across the United States, with the spread of compulsory schooling and the rise of professional training for educators.¹³⁸ These developments, and Hall’s involvement in child study more particularly, were part of the Progressive Era’s growing interest in scientifically grounded reform, as well as the increasing rationalization of motherhood (R. D. Apple, 2006; Grant, 1992).¹³⁹ Within rapidly expanding educational circles Hall quickly gained prominence.

The Contents of Children’s Minds

Hall’s questionnaire-based psychological research first began during his time as Harvard Lecturer on Pedagogy. In the fall of 1882, he undertook an investigation into the nature of the child modeled on previous research conducted in German schools by the Pedagogical Society of Berlin in previous decades.¹⁴⁰ Hall’s project investigated just what children knew upon their initial entrance to the city’s schools (Hall, 1883b; see Ross, 1972).¹⁴¹ Deciding that many of the

¹³⁸ On history of education in the United States see Cremin (1961). On the history of the American normal school see Ogren (2005).

¹³⁹ On this era in American history see Hofstadter (1955), Lears (1981, 2009), and Wiebe (1967).

¹⁴⁰ Near the time of Hall’s undertaking Galton (1880c) too commented on the potential benefits of undertaking questionnaire-based research within school settings: “An excellent way of obtaining average returns to psychological questions would be by the help of schoolmasters.... both boys and masters would enjoy the satisfactory feeling of having accomplished a substantial piece” (p. 252).

¹⁴¹ Hall later misattributes the study as beginning in the fall of 1880 (see Hall, 1893c, p. 13).

questions posed to children in the Berlin investigation were unsuitable for American children he spent “many preliminary half-days of questioning small groups of children and receiving suggestions from many sources” (Hall, 1883b, p. 250). These questions, Hall noted, “should lie within the range of what children are commonly supposed or at least desired...to know” (Hall, 1883b, p. 250). To assess this, on the basis of his preliminary investigations, Hall developed and printed a list of one hundred and thirty-four questions. In some instances, these questions were accompanied by physical objects, such as four colour questions in which children were instructed to pick out the colour named by the examiner from the materials comprising the Holmgren wool test for colour blindness (Hall, 1883b). More often than not, however, children were simply questioned verbally regarding their knowledge of the world.

As would often be the case with Hall’s later questionnaire-based psychological research, it was educators who were assigned the task of collecting data (Hall, 1883b, 1893c). To gain access to large numbers of students, Hall secured the co-operation of the superintendent of Boston schools and in order to question students at newly created kindergartens, he also secured the support of local kindergarten advocate Mrs. Quincy Shaw and superintendent Miss L. B. Pingree (Hall, 1883b). The questionning of students was primarily the work “four of the best trained and experienced kindergarten teachers” (Hall, 1883b, p. 251), all female: Sarah E. Wiltse, Miss L. H. Symonds, Miss E. M. Parker, and Miss C. Scandlin (see Hall, 1883b, p. 272).¹⁴² An additional sixty Boston teachers were involved in collecting data, though some collected

¹⁴² Wiltse later collected material for Hall’s topical syllabi endeavours at Clark (Ellis & Hall, 1896; Hall, 1898) and prior to his syllabi based inquiry into dolls she aided with a preliminary survey of children (see Ellis & Hall, 1896, p. 129). Hall also wrote the introduction to Wiltse’s monograph *The Place of the Story in Early Education, and Other Essays* (1892). In this volume she recounts some of her experiences collecting data for Hall’s inquiry into the contents of children’s minds. Wiltse (1895, 1896) also wrote two overviews of American child study endeavours for Hall’s *Pedagogical Seminary*.

responses from no more than three students. The questionnaire employed to collect data for the project was a printed document, but was not presented to the students in this form (Hall, 1883b).

Rather, teachers used the document as a guide to oral questioning of

three children at a time in the dressing-room of the school by themselves alone, so as not to interrupt the school-work. No constraint was used, and, as several hours were necessary to finish each set, changes and rests were often needful, while by frequent correspondence and by meetings with the writer to discuss details and compare results uniformity of method was sought. (Hall, 1883b, p. 251)

Even still, the information accumulated was far from perfect. Hall acknowledged that “many returns,” especially from those other than the four main questioners were “incomplete, careless, or show internal contradictions” (Hall, 1883b, p. 252).¹⁴³ From the outset of his questionnaire inquiries he was well aware of the difficulties associated with the method, especially the propensity for error in collected information (Hall, 1883b).

Topical Syllabi for Child Study

Despite an initial foray into questionnaire research in the 1880s, Hall’s work with the method only took full form more than a decade later. This was an era in which mass data collection was prevalent, most notably social survey work. These undertakings, like contemporaneous undercover investigations by individuals of various stripes (see Pettit, 2008; Pittenger, 1997, 2012), sought to document the experiences of different kinds of people in the United States, particularly the disenfranchised (see Bulmer et al., 1991; Katz & Sugrue, 1998). Unlike these more explicitly reform oriented projects, Hall self-consciously sought to position his questionnaire-based research as scientific undertakings. Doing so allowed him to claim

¹⁴³ Toward the end of this work Hall privately printed a thirteen page pamphlet “The Study of Children” to guide child study efforts (see Hall, 1924, p. 381; Ross, 1972).

dominion over child life and pedagogical practices not as a reformer, but as a scientist. While his insights might spur reform, they were to be rooted in science above all else.¹⁴⁴

In the early 1890s, now President of Clark University in Worcester, Hall faced a series of difficulties at the institution both with faculty members and the institution's benefactor (see Ross, 1972).¹⁴⁵ Hoping to gain popular support for the graduate education only university among the local community he turned again to educational issues. Higher education in pedagogy had been a focus of the institution from its founding (Clark University, 1889), but this work now became more inclusive of the broader educational community. In 1891 Hall established a new journal *The Pedagogical Seminary* and the following year instituted a summer school for educators, while also expanding and reprinting his earlier inquiry into the contents of children's minds (Hall, 1893c). And in the fall of 1894 he initiated a new program of questionnaire-based research, much of it devoted to the study of psychological aspects of child life and educational matters more broadly.

Prior to initiating his series of syllabi at Clark, Hall spearheaded a more ambitious undertaking, begun at the International Congress of Education. Held in Chicago in conjunction with the 1893 World's Columbian Exposition, the Congress included two separate meetings on the subject of psychology and education. One, on the subject of "Rational Psychology in Education," was organized by James McCosh and included papers by McCosh, Josiah Royce, and Edward Titchener, among others (Ross, 1972; "The education congresses," 1893). Hall organized an alternative meeting on "Experimental Psychology in Education," which dedicated

¹⁴⁴ Notably, prior to her time at Clark, first as a fellow and later on the research staff of the Children's Institute, psychologist Amy Tanner conducted an undercover ethnography of the waitressing profession (see Pettit, 2008).

¹⁴⁵ On the history of Clark University, including Hall's tenure as President, see Koelsch (1987). Disappointingly, this volume lacks references to source material.

all its time to discussion of child study, and fashioned itself as more progressive than what was increasingly seen as the out of date rational psychology dominate in McCosh's group (Hall, 1893a, 1893d; see Ross, 1972).¹⁴⁶ At the congress plans were made for a National Association for the Study of Children, which aimed

to bring together the scattered teachers, parents and others who are trying to make a scientific study of children, that they may mutually assist one another with suggestions, syllabi and outlines, and that materials bearing on particular subjects of investigation may find their way into the hands of those especially interested in working them up.¹⁴⁷

One of the main tasks of the association was to be the collection of both physical and mental measurements of large numbers of children.¹⁴⁸ As President of the short-lived Association Hall was ultimately unable to unite the diverse body of individuals interested in child study, regularly alienating those outside his immediate circle of supporters (see Ross, 1972).¹⁴⁹

¹⁴⁶ On the relation between psychology and the educational congress at the World's Columbian Exposition see Shore (2001).

¹⁴⁷ "National Association for the Study of Children: Preliminary Announcement," B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. No national association for this purpose was ever successfully formed outside of the child study department of the National Educational Association (see Ross, 1972).

¹⁴⁸ "Organization and Plan of Work of the National Association for the Study of Children"; "National Association for the Study of Children, Registration Card", B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. These include tests of physical growth as well as motor tests, tests of strength, rapidity, and exactness, among others.

¹⁴⁹ Among the initial members of the association were William L. Bryan, who served as Treasurer until his resignation (see "Organization and Plan of Work of the National Association for the Study of Children," B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA), as well as ten vice-presidents: "Miss Lillie Williams, Trenton Normal School, Trenton, NJ; Miss Millicent Shinn, Niles, Cal.; Earl Barnes, Prof. Education, Leland Stanford, Jr. University, Palo Alto, Cal.; Supt. Nathan G. Schaffer, Lancaster, Penn.; Prof. J. M. Bailey, of South Carolina; Prof. E. H. Russell, Mass. State Normal School, Worcester, Mass.; Prof. G. T. W. Patrick, University of Iowa, Iowa City, Iowa; Prof. M. V. O'Shea, Mankato, Normal School, Mankato, Minn.; Prof. E. R. Shaw, School of Pedagogy, Washington Square, NY; Mr. Louis Block, 723 Washington Boulevard, Chicago, Ill." ("National Association for the Study of

In the wake of the failure to institute a coordinated data collection effort through a national association, Hall began his Clark based program of questionnaire research. The decision to issue topical syllabi, rather than a broad request for any and all information on child life was a considered choice. Of his experience with the latter approach he noted, “I never got one single usable answer, although I circulated something like fifteen hundred copies” (Hall, 1897b, p. 182).¹⁵⁰ Far more productive to his mind were syllabi dedicated to “a narrow special topic” (Hall, 1897b, p. 182, see also 1897a). In reality the Clark syllabi, though limited to particular subjects, were far from narrow as they asked data collectors to provide information on nearly every conceivable facet of the topic at hand.

The aim of Hall’s topical syllabi project, especially in early years, was to describe the child writ large. Rather than depict individual children, he sought to produce a composite portrait of the normal child (see Hall, 1883b, p. 252, 1893c). Notably, not all children were equally worthy of contributing to this understanding of the idealized aggregate child. To distinguish among different types of children all questionnaires issued at Clark contained requests for information regarding certain characteristics. Those collecting information were instructed to provide the nationality, sex, and age for the child at the centre of each report.¹⁵¹ Occasionally

“Children: Preliminary Announcement,” B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹⁵⁰ What exactly rendered the responses worthless is unclear. Gault (1907) notes that “a comprehensive syllabus for child study” was issued by Hall in 1887, but abandoned because it “was found too cumbersome for practical use” (p. 378). Presumably, masses of unfocused information were even more difficult to manage than the targeted efforts of topical syllabi (see Chapter 4).

¹⁵¹ See, for example, “Anger,” Oct. 1894; “Crying and Laughing,” Dec. 12, 1894; “Dolls,” Nov. 1894; “Early Forms of Vocal Expression,” Jan. 1895; “Folk-Lore Among Children,” Jan. 1894 [*sic*]; “The Early Sense of Self,” Jan. 1895; B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Also, Ellis and Hall (1896, p. 130) and Hall (1899, p. 529). Hall, in his 1924 autobiography, identifies “race” rather than nationality as the characteristic required alongside age and sex for each observation (see Hall, 1924, pp. 389–90),

other characteristics were also asked after, such as family life, temperament,¹⁵² and “any other significant circumstance.”¹⁵³ The recurrent accumulation of information regarding various category memberships presumed the existence of difference along these lines, with the underlying assumption that certain groups were necessarily deficient. As a result one could speak in generalities about various “other” classes of children alongside the constructed sketch of normal childhood.

Syllabi Beyond Clark

At the time of Hall’s unsuccessful attempt to form a national association for child study, another similarly motivated society was organized. The Illinois Society for Child Study, headed by Colonel Francis Parker of the state’s Cook County Normal School, emerged in the wake of the educational congresses at the World’s Fair and strove “honestly and earnestly to investigate the nature and growth of the child, and the best conditions for his growth and education” (Parker, 1895c, p. 9). In its initial organization the society privileged the participation of psychologists, noting in its constitution that the executive committee was to include, first and foremost, “experts in rational, experimental and physiological psychology” (“Constitution of the Illinois Society for Child-Study,” 1895, p. 10). Among the early members of the executive committee were neurologist Henry Donaldson, formerly of Clark and now at the University of Chicago, psychiatrist Adolph Meyer, of the Illinois Eastern Hospital in Kankakee, Illinois, and William Bryan, a graduate of Clark University now at Indiana University in Bloomington (“Officers for

as the latter term often signified both categories in much of his earlier work.

¹⁵² “Anger,” Oct. 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Also, Hall (1899, p. 529).

¹⁵³ “Early Forms of Vocal Expression,” Jan. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

1895," 1895).¹⁵⁴ The inclusion of what in psychology were seen to be the increasingly outdated rational psychologists, a group Hall sought to exclude from his own efforts, secured a sound and wide-ranging base of support for the organization. Kindergarteners, Herbartians, as well as psychologists of all stripes participated in the society. Hall too joined the organization (see Ross, 1972).

The first two volumes of the society's *Transactions of the Illinois Society for Child-Study* regularly featured items from those engaged with the new psychology.¹⁵⁵ Notably, several psychologists contributed pieces to a series of twenty-one child study syllabi reproduced as part of the second issue of the publication, which was billed as a handbook for members. These syllabi were not all psychological questionnaires of child-life, evidence of the wide scope of the term syllabi within child study circles at the time. In addition to outlining in one syllabus a series of measurements that might be made on school children (Scripture, 1895b), psychologist Edward Scripture recounted in another the results of previous research into the mental and physical development of New Haven school children (1895a).¹⁵⁶ This work, undertaken by his student J. Allen Gilbert (1894), was itself an experimental investigation – including tests of discrimination, memory, and rapidity – rather than a descriptive data collection project guided by syllabus.¹⁵⁷ Gilbert (1895) also contributed a separate syllabus on how to calculate averages of

¹⁵⁴ Although initially a member of Hall's proposed National Association for Child Study, Bryan shortly resigned from his position as the organization's Treasurer (see "Organization and Plan of Work of the National Association for the Study of Children," B1-8-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹⁵⁵ These include Bryan (1894), Dewey (1897b), Hall (1897b), Jastrow (1897b), and Tracy (1897a, 1897b). Additionally, Tracy, Hall, Baldwin, Dewey, Scripture, Jastrow, Donaldson, and Bryan all contributed answers to a printed discussion on the question of applying the results of child study to education ("Results of child-study applied to education," 1895).

¹⁵⁶ For Scripture's views on education, see also Scripture (1892).

¹⁵⁷ Another syllabus outlined experiments on colour sensitiveness in children (Kinnaman, 1895).

the measurements made in some child study inquiries. Others filled their syllabi with discussion of child study, rather than providing instructions for collecting data in the field (e.g., T. L. Bolton, 1895a, 1895b; Eckoff, 1895).

Even still, data collection was a central aim of the issue's syllabi. In introducing the series, it was explicitly noted that the real labour of child study lay not in the hands of scientific men as

it is after all to a much more numerous class that we must look for the perfect mountain of facts that we need. We must set everybody at work on this business of collecting and recording. We must accept every contribution and look no gift horse in the mouth. Then have a number of psychologists upon whom to unload our data for classification and digestion. (E. H. Russell as quoted in "Syllabi of child-study," 1895, p. 18)

Only after the hard work of collecting facts was completed could psychologists produce scientific insight into child-life. This rendered the contributions of lay individuals a crucial component of the larger project of understanding children and pedagogical practices through the lens of science.

Most syllabi took a form similar to those issued at Clark, listing at length questions to guide data collection on a specific topic. This is perhaps unsurprising considering syllabi were contributed by a number of individuals with connections to Hall. Meyer (1895), formerly of the local Worcester Lunatic Hospital, requested information on mental abnormalities in children. Former Clark student F. B. Dresslar (1895), now of the State Normal School in Los Angeles, California, sought information on habit degeneration, while Bryan (W. L. Bryan & Griffith, 1895) requested details on imitation. Current Clark Fellow, Herman Lukens (1895) contributed

an inquiry on child language.¹⁵⁸ George Thomas White Patrick (1895a) of the University of Iowa, a former student of Hall's at Johns Hopkins (see Chapter 2), with great brevity asked that instructors rate children's intellectual proficiency and measure their height and weight so that the relation between physical development and mental proficiency might be ascertained.¹⁵⁹ Hall (1895c) also contributed his recent syllabus "Fears in Childhood and Youth," the eighth issued at Clark, while the following issue of the society's *Transactions* reproduced the previous seven syllabi (Hall, 1895d).¹⁶⁰

Also appearing within this collection of syllabi was a request for information from psychologist James Mark Baldwin (1895b). An inquiry into the social sense, particularly chumming and antipathy in children,¹⁶¹ the set of questions was also printed in the pages of *Science* that spring (Baldwin, 1895c). At the close of his questionnaire Baldwin reassured those collecting information for him that names and other personal details would remain "strictly confidential" (1895b, p. 20, 1895c, p. 237), while directing individuals to

¹⁵⁸ Although similar, in some respects, to a syllabus on "Early Forms of Vocal Expression" issued by Hall in 1895 ("Early Forms of Vocal Expression," Jan. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA), Lukens circular was its own entity. It is not listed by Hall in his autobiography (1924) as among those issued at Clark, nor is a copy among those in the Clark Archives. Lukens, with Hall, later issued a further syllabus on reading and writing ("The Beginnings of Reading and Writing," Oct. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹⁵⁹ Another syllabus inquired broadly into the mental and physical characteristics of both children and parents (Van Liew, 1895), while W. O. Krohn (1895) provide a scheme for collecting anthropometrical data.

¹⁶⁰ Topical Syllabi, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Later volumes included work from Hall's former student Frederick Tracy (1897a), including an account of a circular inquiry into left-handedness (1897b).

¹⁶¹ Baldwin's (1895b) syllabus, and no other, included a note regarding the use of the document: "In accordance with an agreement made with the author of this syllabus the results are to be sent him for interpretation as far as the workers are willing. He is at liberty to use these results wherever he desires, and this same schedule for securing data from other sources" (p. 19 fn.). The project was clearly positioned as his own and not that of the society.

cover as many of these enquiries as possible; yet observations of some of them only should still be sent in. *All observations should be carefully arranged under the headings of the schedule, i.e., by the numbers, letters, etc.*, in order to secure correct classification. (1895b, p. 20, 1895c, p. 237)

Those amassing information might aid the laborious process organizing data by adopting certain collecting practices. Following the scheme of classification outlined in the syllabus, rather than expounding at length on the topic without regard for the arrangement of the questions at hand, held the potential of streamlining Baldwin's later work of sorting and analyzing responses.¹⁶² This did not come to pass. Instead "the returns which it brought were filled with apologies for not having carried out the conditions!—and the results have little value when tested by the plainest rules of scientific control" (Baldwin, 1898, p. 219). These difficulties left Baldwin (1898) convinced "the syllabus method is bad through and through" (p. 219).

Data Collection

Before data could be ordered and interpreted it had to be collected. Although the public at large was invited to provide responses to the topical syllabi issued at Clark, it was largely women who engaged in data collection. In some instances parents, particularly mothers' clubs,¹⁶³ took up this work, but more often it was normal school instructors and their largely female students who engaged in the large scale collection of information on child-life.¹⁶⁴ As justification for this

¹⁶² Other syllabi appearing in the issue include Bailey (1895), Brown (1895), O'Shea (1895), and Parker (1895a, 1895b).

¹⁶³ On the involvement of mother's clubs in collection projects see von Oertzen (2013).

¹⁶⁴ Details regarding who was collecting data with syllabi can be seen in some syllabi themselves ("Anger," Oct. 1894; "Fears in Childhood and Youth," Feb. 1895; "Dolls," Nov. 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA); as well as in Ellis and Hall (1896) and Hall (1897a, 1898, 1899). See also discussion of data collection in Hall's correspondence with Herman Lukens (H. T. Lukens to G. Stanley Hall, Oct. 14, 1896; Jan. 12,

approach, Hall contended “anybody who happens to be on the ground can collect facts, just as anybody can pick up a fossil” (1896a, p. 344). All that was necessary was that syllabi “call for phenomena so marked that the non-expert parent or teacher can make reliable returns” (Hall, 1897a, p. 148). They like “the untrained observers” long employed in “the official and voluminous questionnaires worked by four European anthropological societies, by students of meteorological phenomena, migratory instincts of birds, fishes, etc.” (Hall, 1897a, p. 148, see also 1900) were ideally situated to collect information on child-life, as children were a regular presence in their lives.¹⁶⁵

Though anyone with appropriate access to children could complete the Clark questionnaires, women were seen as particularly skilled collectors of information on children. Hall noted that over the course of his program of research “most of the best data were furnished by women” (1924, p. 390). With the increasing presence of women in education, he pled “especially for the co-operation of women in child study” something he thought “calculated to bring out the full power of womanhood” (Hall, 1896a, p. 344). In Hall’s view, the inherently loving nature of women, provided they could balance their sentimentality with an appropriately scientific mindset, predisposed them to be good observers of children and thus producers of quality data on child-life. Like the more recent involvement of parents in autism research (see Silverman, 2012), women’s involvement in these endeavours was a form affective labour encouraged in large part because of their unique access to a population of interest. In order to get his syllabi into the hands of those interested in child study, and thus those most likely to engage

1897, B1-2-7, Box 9, Folder 6, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹⁶⁵ Elsewhere Hall (1895a) justified his data collection practices by noting, “nearly all anthropological matter is gathered in response to questionnaires addressed to missionaries and chance residents and travelers” (p. 36).

in data collection, Hall reproduced some of his syllabi in popular child study periodicals.¹⁶⁶ With Hall and the Clark program of research well known within educational circles, many individuals interested in collecting data also directly requested copies of the topical syllabi (see Ellis & Hall, 1896; Hall, 1899).¹⁶⁷ Sending out syllabi to all those who might provide responses was also attempted for a time, though the cost associated with doing so, especially considering many individuals did not respond, soon led to the discontinuation of this practice.¹⁶⁸ Over the course of the initiative responses were received from across the globe, including England, Germany, Australia, South Africa, India, China, Japan, and Canada (see Hall, 1924), but the bulk of material was collected by American educators. Information was predominately amassed in the North Eastern United States, especially in educationally fertile Massachusetts. That said, material was also regularly collected in Midwestern and Central states, especially as Hall's formal pupils took up positions at regional schools, as well as in California where child study had a foothold in the education department at Stanford University for much of the 1890s (see Ellis & Hall, 1896; Hall, 1897a, 1898, 1899; Ross, 1972).

On one level, the participation of mothers and educators in questionnaire research was seen as a contribution to the scientific understanding of childhood.¹⁶⁹ Yet, it was also recognized that much of the information amassed by these individuals was of little or no scientific value,

¹⁶⁶ The first eight syllabi produced at Clark were reproduced as part of the transactions of Illinois Society for Child-Study (Hall, 1895c, 1895d).

¹⁶⁷ Further aiding in this work was the increasing presence, from the mid-1890s on, of former Clark students in departments of psychology within normal schools, as well as joint psychology and pedagogy positions across the nation. Among the most prominent psychologists to hold, for a time, these kinds of positions were Lewis Terman and Henry Goddard (see Ross, 1972).

¹⁶⁸ “Fears in Childhood and Youth,” Feb. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Also, Hall (1897a, p. 149).

¹⁶⁹ Along similar lines, women were also important survey takers in the twentieth century work of Gallup (see Igo, 2007).

given their lack of scientific training, and in certain instances, their failure to adopt an appropriately scientific mindset (see Goddard, 1900; Hall, 1924; T. L. Smith, 1903). Exactly what about certain reports, beyond an overly sentimental tone, led them to be dismissed by Hall and others as of little scientific value is unclear, although in his earliest questionnaire work he found some reports worthless because they were “incomplete, careless, or show internal contradictions” (Hall, 1883b, p. 252).

Irrespective of the scientific import of their work, the involvement of mothers and educators in child study research was framed as a valuable. This work was promoted as a means of bettering educational and child rearing practices, as Hall contended that both systematic attention to children itself and subsequent scientific findings would benefit parents and educators. In inducing individuals to participate in child study Hall emphasized the practical value of this work, which “opened the eyes of parents and teachers to the nature and needs of childhood in an almost revolutionary way” (1924, pp. 391–92).¹⁷⁰ In the case of educators, Hall’s student Henry Goddard observed that child study questionnaires “could be used by the rural teacher with great benefit to herself and her school, if not to science” (1900, p. 128). Further, he asserted that

A result can be obtained which is of the utmost value, the instant you have induced that teacher to think of her individual pupils more than she has been accustomed to think. She will be a better teacher, brighter, more interested and more sympathetic...Gradually the

¹⁷⁰ Upon later reflection Hall (1924) noted, “Not a few of the records of intelligent mothers which have come to us, whether they followed one or more of the many schedules which...were so abundantly supplied to them or simply noted points they deemed in any way significant, may have real scientific value, although most of these are important only for the parties concerned” (p. 395-6).

most old fashioned teacher will modify her practice as new ideas which she has discovered come to her. (Goddard, 1900, p. 128)

Employing child study questionnaires might not always generate scientifically sound information, but the very act of recording child life could inform the investigator's behaviour in appreciable ways. The very act of observing children promised to better educators by inculcating in them a more modern approach to education.

The bulk of data collection employing questionnaires fell to normal school instructors and their students. Both educators and their pupils were predominantly female and tasked with much of the labour associated with this kind of research. Given the need of amassing significant numbers of responses to circulars, these projects inevitably involved large numbers of individuals. The delegation of responsibility in these undertakings proceeded down a hierarchy of scientific authority. At its head was Hall, followed in short order by his graduate student researchers, through to numerous normal school instructors, and finally to their students who were often assigned the task of collecting information with topical syllabi as part of their studies. Within the hierarchy interpretation of accumulated data was only to be undertaken "by those whose scientific training has been wide enough to judge the material and make use of whatever in it has true psychological significance and connection" (T. L. Smith, 1903, p. 407).

Beyond Hall's endeavours there was widespread interest within child study in collecting information, as well as attendant efforts to organize gathered material. Even when not directly involved, his influence was felt in these kinds of undertakings. Here I detail the data collection work engaged in at the normal school in Worcester, so as to situate the questionnaire-based research conducted at Clark within the wider field of child study. I then turn to the involvement

of two normal school teachers in data collection with Clark's topical syllabi.¹⁷¹ This discussion illustrates how engagement with Hall's program of research offered, a sometimes frustrated, route to greater professional prospects. To be sure, the potential benefit to teachers' professional status offered by work in child study was not limited to these two individuals. Rather, association with these kinds of endeavours provided educators at large with a – not always successful – means of professionalizing. Participation in projects like Hall's allowed educators to claim an authoritative scientific knowledge of children, something Hall recognized and explicitly emphasized in his attempts to draw educators into the fold (e.g., Hall, 1903). At the same time, in other contexts, Hall regularly dismissed educators' skills in order to elevate to his and other psychologists' expertise.

The Worcester Method

Shortly after Hall completed his initial questionnaire-based inquiry in Boston, Principal E. Harlow Russell instituted his own program of research at the Massachusetts State Normal School at Worcester. The project, Russell expressly acknowledged, was inspired by Hall's efforts "to induce parents and others having the care of children to record their sayings and doings with a view of increasing the data on which mental science rests" (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, pp. 21–2), as well as his observation that this might be undertaken as part of the training in psychology received by normal school students. Russell eagerly adopted Hall's suggestions and put the largely female students at Worcester State Normal School to work collecting information on child life with a series of annual circulars headed simply "Study of Children" (*Massachusetts State Normal School at*

¹⁷¹ Both were singled out as particularly valuable contributors to the Clark program of questionnaire research by its later appointed leader (T. L. Smith, 1903).

Worcester catalogue and circular, 1888, p. 22).¹⁷² Over the course of the project's operation Hall provided "helpful and stimulating criticisms and encouragements" (Russell, 1892, p. 343).

Collecting information was not a compulsory component of the school's curriculum, but Russell nonetheless found students eagerly took up the work. To begin with he explained the nature of the work to the students very carefully, giving special prominence to the necessity of having the records genuine beyond all possibility of question, of having them consist of a simple statement of what the child does or says without comment on the part of the writer, of making them without the knowledge of the child at the time or at any other time, and of noting the usual, rather than the unusual, conduct or remarks of the children observed. (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 22)

These observations of the "conduct of children in all circumstances, – at home, at school, in the street, at work, at play, in conversation with one another and with adults" (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 24) were to be recorded on a provided form as soon after being made as possible.

In order to streamline the often-messy process of data collection and categorization, Russell devised what Hall characterized as a "new and simpler method" (1893d, p. 430). The Worcester method consisted of a series of coloured blanks which were employed "for convenience in classification" (*Massachusetts State Normal School at Worcester catalogue and*

¹⁷² Data collection began at the Worcester State Normal School in 1884 or 1885 (see *Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 21; Russell, 1892). On the collection of data on children undertaken at the school see Bolton and Haskell (1898), Frear (1897), Haskell (1894, 1896), and Russell (1892, 1893). For contemporaneous popular discussion of this work see the *New York Times* ("The study of children," 1895).

circular, 1888, p. 22). Depending on the type of material being collected, records were made on forms of different colours:

white paper is used for such observations as students make themselves; red for well attested ones reported by others; yellow for reminiscences of their own childhood; green for mention of whatever they read on the subject; and chocolate for observations that extend continuously over a period of time. (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 22)

Several years later blue blanks were added to record observations of “exceptional or defective children” (*Massachusetts State Normal School at Worcester catalogue and circular*, 1891, p. 20). Outside of colour blanks were identical with space included for recording both the name and age of the observer and the child observed, as well as the nationality and sex of the child (see *Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 22).¹⁷³

The material collected with the Worcester method was deliberately broad. Worcester students set out to gather information on the breadth of children’s experiences, in contrast to the circulars that would soon be issued by Hall and his students at Clark. The latter were more limited in scope, in the sense of inquiring into a particular subject matter related to child life. This openness of subject matter in the Worcester undertaking was deliberate. Russell observed that “a determinate course and method, such as would suggest themselves to a scientific man as most likely to lead to the results desired, would often take our youthful and comparatively untrained students quite beyond their depth,” as they were “by no means competent to do exhaustive or “quantitative” work in experimental psychology” (Russell, 1892, p. 344). Lacking

¹⁷³ Also to be listed were the date, the address of the observer, and the “length of time between making the observation and recording it” (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 22).

appropriate training imprecision was the best recourse. It was hoped that the gathered mass of material would prove of some value to child study, but this aim was very decidedly secondary. The immediate benefit of the undertaking was in the education of normal school students, as through this work they both developed “the qualities most sought in a teacher” (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 23) and gained some preparation in the objective scientific mindset necessary for further studies in mental science. Thus, Hall commented on the practice’s “good effects on the powers of observation, tact, psychologic knowledge and interest of those who make them” (1888, p. xxiv), while acknowledging that the evidentiary value of the material was as yet undetermined.

The explicit absence of scientific analysis as object of the Worcester work meant that it was the process of collection that was of central import. As instructor Ellen Haskell explained, the records amassed are an incidental product remaining after the original and main purpose has been served. Whether they are the ashes of a furnace or a deposit containing precious metal, which will repay the labor of analysis and extraction, is for the promoters of science to determine. (E. M. Haskell, 1894, p. 30)

The handing off of scientific authority over the material to better trained individuals is especially apparent in the most comprehensive publication to arise from the undertaking, Haskell’s edited volume *Child Observations* (1896). The publication consisted simply of hundreds of pages of chronologically ordered excerpts of observations of imitation in children presented without analysis or commentary.¹⁷⁴ The 1208 accounts, each accompanied by the observed child’s name and age, represented a fraction of the total body of material accumulated since the project’s

¹⁷⁴ The volume was based on 500 records of imitative acts and included extensive excerpts from these records (E. M. Haskell, 1894, 1896).

inception.¹⁷⁵ The data, leftover from the valuable training process of having normal school students observe children, was simply made available to those with an interest in the matter, whether pedagogical, psychological, or otherwise. It was up to the scientifically-minded to make sense of the mass.

Haskell's volume exemplified the orientation of the project more generally. Prior to recording observations of children, normal school students were instructed to regard their undertaking as of the nature of an exploring expedition among the phenomena of child-life, and themselves as nothing more than faithful collectors of specimens which as the raw material of science may be worked up by experts later. (Russell, 1892, p. 345)

The understanding of students' role as that of collector, and decidedly not that of scientific synthesizer, was emphasized all the more with the 1893 addition of a quote from Charles Darwin to the top of the blanks used to collect information: "I worked on the true Baconian principles, and without any theory collected facts on a wholesale scale..." (*Massachusetts State Normal School at Worcester catalogue and circular*, 1893, p. 24).

Assigned the very specific role of data collector, students at the Worcester Normal School excelled in their effort in terms of the sheer quantity of material amassed. In the 1880s roughly two thousand records were accumulated each year (*Massachusetts State Normal School at Worcester catalogue and circular*, 1888, p. 24), with this rate increasing to more than three thousand per year in the 1890s (Russell, 1892). Alongside the general accumulation of observations, in the 1890s students at the school were also put to work collecting data for the Clark syllabi (e.g., Hall, 1899) and, in at least some instances, information was culled directly

¹⁷⁵ Russell estimated that no more than a twentieth of the collected material was represented in the volume (see Russell, 1896, p. vii).

from the larger body of previously collected observations held at the school (see Hall, 1897a, p. 149). By the time of Russell's retirement the body of material had swell to more than 60,000 questionnaires. With Russell's departure the documents were transferred to Hall (see Hall, 1924, p. 380) but he, like Russell, made no effort to produce any synthetic account of child-life from the material once it was in his possession. The very volume of collected material was undoubtedly overwhelming.

Female Educators and the Professional Possibilities of Child Study

Among the normal school educators attracted to the child study movement, and Hall's questionnaire method of collecting data, was Margaret Keiver Smith (1846-1934).¹⁷⁶ Born in Amherst, Nova Scotia, Smith earned a diploma from the State Normal School in Oswego, New York in 1883. She then spent some time studying philosophy, including two years at Cornell University as well as a period abroad in Germany, where she studied the work of philosopher and educationalist Johann Friedrich Herbart (1776-1841) in Jena. She returned to Oswego as an instructor in 1887 and remained at the school until the mid-1890s. Like many other women of the period who worked within the nation's burgeoning normal school system, Smith was interested in incorporating the scientific study of children into her teaching. Beginning in the mid-1880s she also endeavoured to obtain professional training in the new scientific psychology.¹⁷⁷

¹⁷⁶ For basic biographical information on Smith see Cattell (1932) and Ogilvie and Harvey (2000); the latter erroneously lists her full name as Kiever Smith. For more on Smith see Creese (1998) and Singer (2003); note, however, that the birth and death dates provided in the former are incorrect. Smith's published works include a translation of Herbart (1891), as well as Graff and Smith (1886) and Smith (1893a, 1893b, 1893c, 1893d, 1894a, 1894b, 1895, 1900, 1901, 1902, 1903, 1907a, 1907b, 1908a, 1908b, 1908c).

¹⁷⁷ See M. K. Smith to G. Stanley Hall, Mar. 10, 1890, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

Smith's contact with Hall began in the 1880s and proved both frustrating and not nearly as professionally advantageous as first hoped. Her initial letters expressed her interest in the new psychology in relation to education and her intention of pursuing advanced study in the field.¹⁷⁸ Five years later, in 1890, she was no further along in her pursuit. Learning of the new graduate level university Hall was heading she asked "whether it is only for men...[or] would it be possible for a woman to take the course of studies there upon the same conditions as the men take theirs?"¹⁷⁹ As she informed Hall,

I begin to feel like making a speciality [*sic*] of philosophy and of applying it to psychology and pedagogy. If it were possible to take a course at Clark University where I might perhaps work for a degree, I think I should be tempted to do it....I have a pretty clearly defined system of pedagogy in my mind ready for elaboration, but while teaching I do not know that I shall ever be able to work it out. My aim would be to fit myself for work in history of education, pedagogy, psychology, and history of philosophy.¹⁸⁰

Having been occupied with teaching responsibilities at Oswego for the past several years, she was eager to again pursue additional training. Although Hall was at least nominally supportive of Smith's interest in further education, he dashed her hopes of studying at Clark. Noting that women were not eligible for admission to the university and that special permission would have to be made, he informed her that such permission would not be forthcoming anytime in the near

¹⁷⁸ See M. K. Smith to G. Stanley Hall, Mar. 10, 1890, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁷⁹ M. K. Smith to G. Stanely Hall, Mar. 10, 1890, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁸⁰ M. K. Smith to G. Stanley Hall, Mar. 10, 1890, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

future.¹⁸¹ The institution's policy against co-education was in part an attempt to appease the University's benefactor, Jonas Clark, who opposed the general admittance of women to the new institution and especially the use of his funds for this purpose. Hall also opposed the coeducation of women, based on his recapitulationist beliefs regarding the nature of women and the consequences of education on reproduction. That said, from the university's establishment there was discussion of creating a separate department for women, housed within its own building, if outside funds for this purpose could be secured. Six years after Keiver's inquiry, the institution began to allow exceptional women, as judged by Hall, to take special courses at Clark (see Diehl, 1986).¹⁸²

Unable to pursue a degree at Clark, Smith nonetheless continued her correspondence with Hall. In the early 1890s, Hall had yet to resume his questionnaire-based research of children, and Smith's inquiries focused more on how she might assist with the data collection recently initiated at the school by anthropologist Franz Boas. At the end of the 1890 school year, Boas made physical measurements of some local Worcester area school children (see Boas, 1897).¹⁸³ A more ambitious series of measurements was proposed in January 1891 and approved by the

¹⁸¹ G. Stanley Hall to M. K. Smith, Mar. 11, 1890, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. No woman earned a degree from Clark until 1907 (Diehl, 1986). On the important role of female members of Clark University during Hall's tenure see Diehl (1991).

¹⁸² Notably, in the 1890s some female psychologists undertook informal study in Clark's psychological laboratory, under the direction of Edmund Sanford (Calkins, 1893b; Miles, 1895). It was only in 1909 that women were formally permitted to obtain graduate degrees from the university. Even still, the education of women at the institution was officially limited to the Educational Department and women were excluded from pursuing degrees within other departments at the university. Despite this, Hall made exceptions on an individual basis. This change in policy was made for strategic political reasons, including the university's desire for a tax exemption, as well as in an effort to secure favourable public opinion. Detailed discussion of the co-education policies at Clark, and Hall's views on such, can be found in Diehl (1986).

¹⁸³ On Boas's work during his time at Clark University see Cole (1999). For Boas's views on the growth of children see Boas (1892a, 1892b, 1912) and for his views on anthropometry see Boas (1893, 1894).

school board that spring, with measurements beginning in earnest in April of that year.¹⁸⁴ The project was controversial, attracting vocal opposition from local politicians as well as Worcester's sensationalist newspaper, which depicted Boas as a stranger to the community who was undertaking a project with no scientific value (see D. Cole, 1999).¹⁸⁵ Even still, Boas and four assistants, including perhaps his most prolific data collector Gerald West (1891, 1892, 1893, 1896), continued with the project.¹⁸⁶ Hall, wishing to avoid controversy, especially in the face of the disapproval voiced by the university's benefactor Jonas Clark, discouraged the endeavour in its early days. With the continuing support of the local school board, he eventually changed his

¹⁸⁴ The project also received the support of Principal E. H. Russell of the local State Normal School (D. Cole, 1999). Copies of various versions of the forms used to record information as part of Boas's anthropometric undertakings can be found in the Clark University Archives (B1-2-4, Box 6, Folder 10, G. Stanley Hall Papers, Clark University Archives, Worcester, MA). In addition to requesting a variety of physical measurements (e.g., height of ear, length of head, etc.), information was also requested regarding: age, sex, place of observation, name, nationality, place of birth, nationality of father, nationality of mother, occupation, color of eye, color of hair, as well as color of skin (very dark, dark brown-red, dark yellow-brown, red, yellow, yellowish white, white) for covered parts of body, uncovered parts of body, and palms of hands.

¹⁸⁵ The university's failure to connect with the local community in its earliest years is well documented by Ross (1972).

¹⁸⁶ This kind of work was not confined to Worcester. As part of the *Third Annual Report of the President* it was reported: "In November, 1890, Dr. G. M. West, a graduate of Columbia College, was appointed fellow in Anthropology, and devoted himself to the consideration of its physical side, taking a large part in the anthropometric investigation begun in Worcester schools, and afterward extended to other cities. During the summer of 1891 Dr. West was engaged in anthropological measurements of the Indian tribes of Quebec and the Maritime Provinces of Canada" ("Third Annual Report of the President," 1893, p. 122, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA). At the same time it was also noted that Alexander Chamberlain, a student in anthropology and assistant with Boas's work: "in April-May, superintended the extensive measurements of school children carried on in the public schools of the city of Toronto, Canada, with the approval of the authorities and cooperation of the teachers, under the auspices of the sub-Department of Physical Anthropology of Chicago" ("Third Annual Report of the President," 1893, p. 122-23, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

position, but Boas soon left the university.¹⁸⁷ Prior to his departure, Boas was placed in charge of the Physical Anthropology department of the World's Columbian Exposition, to be held in Chicago in 1893.¹⁸⁸ For the exhibit, beginning in 1891, he collected extensive physical measurements of individuals, particularly various "Indian" groups across North America with "the ultimate object of the inquiry ... to show the distribution of various types over the American continent."¹⁸⁹ The project employed nearly seventy field workers and ultimately produced more than 17,000 data sheets (see Boas, 1899; D. Cole, 1999).¹⁹⁰

It was to Boas's project that Smith initially offered her assistance, erroneously assuming it to be Hall's undertaking.¹⁹¹ Noting that she had already planned to undertake a set of physical measurements of children, she requested details regarding just what measurements were being made of Worcester school children.¹⁹² At the same time she forwarded on to Hall her "first

¹⁸⁷ Although Boas left at the same time as the mass exodus of Clark faculty to the newly created University of Chicago, he was not part of the group hired on there (see D. Cole, 1999).

¹⁸⁸ See "Second Annual Report of the President," 1891, p. 50, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁸⁹ "Second Annual Report of the President," 1891, p. 50, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁰ See also "Second Annual Report of the President," 1891, p. 45-50, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹¹ Smith later further followed in Boas's footsteps, undertaking tests on Indian students at the Indian School in Carlisle (see M.K. Smith to G. Stanley Hall, Nov. 13, 1902, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

¹⁹² Smith initially intended to measure "the height of the child, breadth of shoulder, bust measure, waist measure, length of limbs, a measurement of the arm at the shoulder, a measurement of the head which I might call the hat measurement as well as a measurement of the breadth of the forehead just about the eyebrows. Also, height of forehead" (M. K. Smith to G. Stanley Hall, Oct. 6, 1891, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA). For an overview of physical measurement projects in the United States during this period, including those of children, see (Park, 2006). A number of works detail anthropometric studies more generally, many of which included psycho-physical tests of some kind (Bagley, 1901; Boas, 1895; Burk, 1898; Carman, 1899; Galton & Hitchcock, 1889; J. B. Gilbert, 1977; Hartwell, 1893; Hitchcock & Seelye, 1888; MacDonald, 1899; W. T. Porter, 1893, 1894; R., 1899; Ripley, 1896).

schedule of the work upon the child,” which her students employed as a guide to their weekly “observations of special children.”¹⁹³ The information collected with the schedule was used to direct the course of class discussion of pedagogical matters related to children and was not necessarily intended to serve a larger scientific purpose per se. Provided by Hall with schedules for recording the physical measurements of children, she expressed her willingness to collect data herself and to put her students to work doing the same. The latter endeavour, however, she noted “must be merely tentative” at least for the first several weeks “as many of my students are not yet sufficiently practiced to make observations upon which one could rely.”¹⁹⁴ Though she was sufficiently trained to collect scientifically valuable information, her students were not yet competent enough to do the same.

Smith was enthusiastic regarding the educational value of observational work as guided by printed schedules, such as those provided to her by Hall.¹⁹⁵ Although occupied to some degree with this work, her ambition to receive further training in psychology did not abate. At the end of 1893 she told Hall of her continuing desire to obtain an education in the field, informing him that she was “ready for more advanced work than can be done in the Normal Schools.”¹⁹⁶ Beyond this she observed “with a degree I believe that I should have the assurance to apply for university work.”¹⁹⁷ Three years later, seeking more challenging work and broader

¹⁹³ M. K. Smith to G. Stanley Hall, Oct. 6, 1891, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁴ M. K. Smith to G. Stanley Hall, Oct. 9, 1891, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁵ See M. K. Smith to G. Stanley Hall, Nov. 23, 1891; M. K. Smith to G. Stanley Hall, Mar. 1892, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁶ M. K. Smith to G. Stanley Hall, Dec. 26, 1893, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁷ M. K. Smith to G. Stanley Hall, Dec. 26, 1893, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

professional opportunities, she left Oswego for Europe. She audited courses at the University of Göttingen for a time, but unable, as a woman, to audit classes in pedagogy or participate in seminars in subjects more broadly, she soon moved on to the University of Zurich (see Creese, 1998; Singer, 2003). There she completed a doctorate in psychology, in 1900, with a well-received study of rhythm (M. K. Smith, 1900). That fall she returned to the United States, conducting further experiments on rhythm in the Clark laboratory through the winter under Sanford's direction (see M. K. Smith, 1907a).

Despite her earlier hopes, she found her professional prospects little improved with a doctorate in hand. She again applied for positions at normal schools, and was shortly appointed Professor and Director of Psychology and Geography at the State Normal School in New Paltz, New York, but she insisted "it is not my intention to do Normal School work longer than I can help. I desire a position where I can work in Experimental psychology."¹⁹⁸ Even relegated to a normal school she continued to pursue experimental work, undertaking tests of reaction time and skin sensitivity of children for a period.¹⁹⁹

Aspirations aside, Smith was never able to secure an academic position devoted to experimental psychology. Instead, she returned to her role as data collector throughout the first decade of the twentieth century, amassing information on numerous subjects.²⁰⁰ Of an inquiry

¹⁹⁸ M. K. Smith to G. Stanley Hall, Sept. 14, 1900, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

¹⁹⁹ M. K. Smith to G. Stanley Hall, Jan. 29, 1903, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Smith likened these tests to those undertaken by Chicago Public Schools' Department of Child Study and Pedagogic Investigation (see Chicago Public Schools, 1898, Chicago Public Schools, 1899, Chicago Public Schools, 1900, Chicago Public Schools, 1902).

²⁰⁰ Smith also collected data for Jewell (1905; see Chapter 7). Correspondence related to Smith's data collection efforts for Hall include (M. K. Smith to G. Stanley Hall, Dec. 11, 1901; M. K. Smith to G. Stanley Hall, Nov. 13, 1902; M. K. Smith to G. Stanley Hall, Jan. 12, 1903; M. K. Smith to G. Stanley Hall, Jan. 29, 1903; M. K. Smith to G. Stanley Hall, Feb. 17, 1907,

into the education of women she rather diplomatically informed Hall, who was firmly convinced of the dangers of education for women (see Diehl, 1986): “whether you are right in your ideas about women, I am not as sure, but shall tell you just what I think when I answer your questions.”²⁰¹ As for other female psychologists during this period (see Pettit, 2008), remaining on friendly terms with Hall, who was her best connection within scientific psychology, was crucial to her continuing efforts to obtain employment directly in the field. She was very soon unhappy with her position at the New Paltz normal school, noting “I am doing the work of two teachers here for less than one teacher’s pay and the school is not prospering.”²⁰² With little prospect of employment in psychology she applied for positions at other normal schools.²⁰³ Hearing of a new women’s college, Sweet Briar College, being established near Lynchburg, Virginia she took the opportunity to write to Hall of her interest in chairing the institution’s department of psychology, regarding which she informed him “I know that I can do it, if I can get the opportunity.”²⁰⁴ Leveraging her usefulness as a regular collector in his questionnaire endeavours, she ended her request for a reference with the offer: “If I can make any other return,

B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

²⁰¹ M. K. Smith to G. Stanley Hall, Dec. 11, 1901, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See “Education of Women,” Dec. 9, 1901, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. For more on this project see, A. L. Brown to G. Stanley Hall, May 22, 1899, May 23, 1899; B1-6-2, Box 35, Folder 22, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²⁰² M. K. Smith to G. Stanley Hall, Dec. 26, 1903, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²⁰³ See M. K. Smith to G. Stanley Hall, Dec. 26, 1903, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²⁰⁴ M. K. Smith to G. Stanley Hall, Aug. 4, 1906, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

I shall be happy to do so.”²⁰⁵ Hall provided Smith with a largely positive, though brief, letter of recommendation, in which he opted to emphasize her teaching ability, rather than her psychological training and research.²⁰⁶ The position was not to be. Instead of continued work in experimental psychology, Smith turned to theoretical discussions of psychological and pedagogical aspects of language,²⁰⁷ and later to the study and treatment of a “backward” pupil,²⁰⁸ before leaving normal school work in 1909 to undertake the private education of a family for nearly a decade.²⁰⁹

A recurrent data collector herself, Smith was also quite likely the one to introduce Hall to his most prolific accumulator of questionnaire responses. In the spring of 1892, in advance of

²⁰⁵ M. K. Smith to G. Stanley Hall, Aug. 4, 1906, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²⁰⁶ Hall wrote: “I am requested to write you my impression of Dr. Margaret K. Smith’s fitness for a chair in your institution. I consider her one of the ablest women and one of the best teachers I have ever known, and although she is no longer young, seems to be in the zenith of her strength and enthusiasm. She took for yours [*sic* – four years] off some time ago to take a Ph. D. in Europe, has studied since one year here, has never failed, I think in anything she has undertaken. Of her incisiveness of mind her article on English, which is the best thing that has appeared for years in my judgment, is a sample. She is a thoroughly pleasant person to work with and commands the respect and enthusiasm of her class” (G. Stanley Hall to President J. Carhardt, Dec. 28, 1906, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

²⁰⁷ See M. K. Smith to G. Stanley Hall, Oct. 12, 1903; M. K. Smith to G. Stanley Hall, Oct. 19, 1903; M. K. Smith to G. Stanley Hall, Oct. 30, 1903; B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See also Smith (1903).

²⁰⁸ See M. K. Smith to G. Stanley Hall, Dec. 10, 1907, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Although Smith offered her account of the training of a backward boy for Hall’s *Pedagogical Seminary*, it instead appeared in three-parts in Lightner Witmer’s recently established *Psychological Clinic* (M. K. Smith, 1908a, 1908b, 1908c).

²⁰⁹ Smith’s final correspondence with Hall was a request that he provide a reference letter on her behalf to the National Research Council. The reference was part of her application to work with disabled and otherwise incapacitated soldiers returning from service who were under the care of the Division of Special Hospitals and Physical Reconstruction of the Surgeon General’s Office (M. K. Smith to G. Stanley Hall, Mar. 3, 1918, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

Clark University's first summer school on psychology and pedagogy, Smith provided Hall with a list of possible participants. Included here was the recently appointed "Teacher of Psychology" of the State Normal School in Trenton, New Jersey: Lillie (Leslie) A. Williams.²¹⁰ Along with Smith and some sixty-six others, Williams attended the summer school (see Clark University, 1893).²¹¹ This was also the year she founded a psychology laboratory at her institution (Garvey, 1929) and promoted the value of both teaching and practicing the new psychology in normal schools within the pages of Hall's *Pedagogical Seminary* (Williams, 1892). She also soon established herself as one of the "most valued contributors" (Hall, 1924, p. 389) to Hall's program of questionnaire-based research. Although she published only a single study of her own in the field, on children's interest in words (Williams, 1902), Williams contributed material to numerous topical syllabi based projects initiated at Clark, in some instances supplying as many as a third of the returns (e.g., Lindley & Partridge, 1897).

To Williams's mind the topical syllabi issued by Hall and his students at Clark allowed for the accumulation of "rich masses of facts" (Williams, 1896, p. 423).²¹² This body of material held the promise of producing "a science of the genesis of the mind" (Williams, 1896, p. 423) which might enrich educational practice.²¹³ Even prior to her collection efforts for Hall, Williams (1896) had engaged her students in child study through observational work along the lines of that

²¹⁰ M. K. Smith to G. Stanley Hall, April 6, 1892B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Williams was an instructor at the school from 1871 until the 1915-1916 academic year and served as Instructor of Psychology from 1891 on (Bauerle, 2003; Breland, 2010; Carnicom, Faye, & Baker, 2011; Chaffin & Gruenfeld, 1997).

²¹¹ On the summer school see Ross (1972).

²¹² See also "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²¹³ See also "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

instituted by Russell at the Worcester Normal School.²¹⁴ Put into contact with Hall, she soon shifted her students' efforts to the more specific collection of data for the many topical syllabi produced at Clark. These syllabi began to appear in the fall of 1894 and Williams, and by extension her students, were soon at work accumulating information on various subjects including anger (see Hall, 1899, p. 539), fears (see Hall, 1897a, p. 150), and the early sense of self (see Hall, 1898, p. 395 fn.).

As a dedicated collector of material on child life, Williams set out to ensure that information amassed using syllabi were of sufficient quality to be of scientific value. To this end, she composed a document to guide the work of data collectors: "How to Collect Data for Studies in Genetic Psychology" (1896). In addition to appearing within the pages of Hall's *The Pedagogical Seminary*, Williams's guide was printed and circulated alongside the Clark topical syllabi.²¹⁵ The document outlined what Hall considered "the most effective of all methods for collecting valuable returns to questionnaires" (Hall, 1897a, p. 150) one "calculated to eliminate very many at least of the possible defects and errors" (Hall, 1897a, p. 239, see also 1924, pp. 389–90). Use of the guide offered a means by which to, at least, strive toward the standardization of data collection efforts.²¹⁶ Within the document Williams at once detailed her method of using topical syllabi to collect information and the instrument's larger role in the normal school classroom. As she described, she first dictated the syllabus, question by question, to her students and asked them to recall their own childhood experiences and share them with one another. Following this discussion, students wrote out reminiscences of their own childhood

²¹⁴ See also "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²¹⁵ "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²¹⁶ On efforts at standardization in early American psychology see Coon (1993) and Kroker (2003).

experiences and then set out to observe children and record their observations with the syllabus, in each instance recording the age, sex, and nationality for the children observed (Williams, 1896; see also Hall, 1924, pp. 389–90).²¹⁷ Following this scheme, however, did not guarantee success when it came to collecting information on specific kinds of experiences, as she noted “the impossibility, in many cases, of getting at children at the right time and under circumstances otherwise favorable, proved a serious obstacle to success” (Williams, 1896, p. 422).²¹⁸ Simply because one set out to observe a particular kind of experience in children did not mean, of course, that it would be there to be found. In these instances another option was to respond to syllabi questions with information gleaned from hearsay, as well as incidents read about in books (see Hall, 1924, p. 380).²¹⁹ Only on some syllabi were respondents directly instructed to indicate “whether each record is from memory observation, literature or report” and little was made of these kinds of distinctions in published reports.²²⁰

Numerous normal school educators employed Hall’s syllabi, together with Williams’ guide to collecting information, and at one point Hall had at his disposal as many as fifty regular data collectors (Hall, 1924). As he later recalled, the “syllabi were often made the theme of instruction and we were not infrequently called on to furnish copies by the score for class purposes” (Hall, 1924, p. 389). Like Smith, Williams was appropriately and, given their status differential, necessarily deferential toward Hall. Discussing the difficulties of teaching the new psychology in normal schools, she noted

²¹⁷ See also “How to Collect Data for Studies in Genetic Psychology,” B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²¹⁸ See also “How to Collect Data for Studies in Genetic Psychology,” B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²¹⁹ See also “The Early Sense of Self,” Jan. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. On Williams’s work as data collector see Smith (1903).

²²⁰ “The Early Sense of Self,” Jan. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

how to secure apperception is ever the chief problem. We know that we must lead them to introspect, but we do not always know how to give beginners the sort of explicit direction which they need. Here Dr. Hall's carefully prepared syllabi come to our aid. (Williams, 1896, p. 422)²²¹

For normal schools seeking to incorporate the new psychology into their teaching, often in the absence of a designated and equipped laboratory space, Hall's syllabi offered a straightforward means of inculcating a psychological disposition in students.

In addition to praising the quality of Hall's research instruments, and their benefit to the teaching of scientific psychology, Williams was sure to position the work of data collectors like herself as crucial to the efforts of scientific researchers. Pupils were to be reminded that "every natural science must begin with the accumulation of facts" and

that the material can be obtained only through the coöperation of very many different observers, each contributing his mite to swell the whole, and that in this work each one of them may bear apart. I touch upon the honor of being co-laborers even in ever so small a way with the eminent scientist who is directing the research. (Williams, 1896, p. 419)²²²

Amassing of information for Hall and other researchers was a worthy endeavour, but one necessarily subordinate to the more important scientific work engaged in by trained others. Like Smith who considered her students to be insufficiently trained to produce much of value, at least in their initial forays into this kind of research,²²³ Williams was explicitly concerned with the status differential between trained and untrained, as well as how to best go about training

²²¹ "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

²²² "How to Collect Data for Studies in Genetic Psychology," B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See also Hall (1924, pp. 389–90).

²²³ M. K. Smith to G. Stanley Hall, Oct. 9, 1891, B1-6-11, [no box or folder number], folder id: B1 D44-35, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

students. Those like Hall who possessed expertise as a function of their scientific training were positioned as superior to normal school students who just were in the midst of receiving some modicum of training in the field. As intermediaries to this collection process, Smith and Williams each sought to claim some level of authority through their association with Hall, the ultimate expert in the field.

In her guide Williams carefully cultivated the data collector's role as invaluable in an effort to attract educators and their students to this work, and positioned the activity as a boon to professionalization. At the same time, she also made clear that collecting was ultimately a negligible contribution to the larger scientific undertaking. This latter qualification ensured that scientific authority remained in the hands of Hall and his researchers and was not dispersed among the laity. In terms of her own role, Williams at once positioned herself as mere data collector, and as something more. The founding of a psychology laboratory at her institution allotted her a certain amount of scientific standing, but more important was her role vis a vis the practices of collecting.²²⁴ By authoring *the* text on how data collection for Hall's questionnaire projects was to be undertaken, she skillfully fashioned herself as the authority on this matter, in effect commanding control over a new and esteemed form of expertise. She, as much as any scientific researcher, was the arbitrator of what constituted good data.

Conclusion

For Hall and his supporters questionnaires provided a relatively straightforward means of amassing information on child-life. Even still, collecting data in this way was difficult. To successfully do so, untrained collectors – largely female normal school students – were engaged in accumulating material in the field. This was in direct contrast to the intensive training

²²⁴ Hall (1894) noted of laboratories of experimental psychology “a few even of the normal schools have lately found out to their great benefit” (p. 713).

emphasized in laboratory psychology, most famously that emphasized in Wundt's Leipzig laboratory (Danziger, 1985, 1990). The participation of laypersons was vital to questionnaire research, but was framed as decidedly unscientific, in and of itself. Within the status hierarchy on which questionnaire projects rested, it was Hall and his students who retained the apex of scientific authority. Some, like Smith and Williams, attempted to negotiate greater status for themselves, with varying degrees of success.

The natural historical orientation central to these projects may have required the diffusion of responsibility to numerous individuals, but it did not guarantee equal standing for all forms of work. Much like the difficulties encountered by Jane Addams and her female compatriots at Hull House in relation to sociology at the University of Chicago (see Deegan, 1988), the labour of female educators in psychological investigations has largely gone unrecognized. Whether strictly scientific by Hall's estimates or not, the work of predominately female educators and their largely female students was a crucial component of the naturalistic form of science practiced in questionnaire endeavours. Employing women to collect information on children in the field, particularly homes and schools, offered a way a means of practicing scientific psychology beyond the walls of the laboratory. Doing so, necessarily, produced a very different form of knowledge. The difficulties that attended data of this kind are discussed in the next chapter.

Chapter 4

Child Study, Objectivity, and Scientific Styles

The extension of psychological work to untrained members of the public, while convenient, provoked debate among those within the discipline with differing views on the value of these kinds of undertakings. Some, like Hall, were more than happy to engage the laity in the process of data collection and, at times, interpretation as well. This was tied in very specific ways with Hall's own professional aspirations, both as a recognized expert in the nation's popular child study movement and as the President of a newly created university who was trying to establish the institution as a site of scientific excellence while also, out of necessity, courting the support of the local community (see Ross, 1972). In many respects, involving large swaths of the public in scientific research projects was simply expedient. This is not to say, however, that Hall's vision of science was necessarily bankrupt. Rather, a different vision of the scientific enterprise was adopted in the face of the challenges of compiling great swathes of information.

The appropriate relationship between psychology and pedagogy was a contentious point for the new scientific psychologists. Some, like Hall, saw psychological knowledge as the key to reforming educational practices upon firm scientific ground and educators as valuable contributors of information to psychological research projects. Others were less sanguine on one or both points. Objection was raised to the study of children, not so much on the basis of the inaccessibility of their minds, but rather on the grounds that doing so was antithetical to the proper role of parents and educators, the individuals best positioned to conduct this work. On the other hand, for some, investigations of children were best undertaken not with questionnaires that enlisted the aid of untrained individuals, but with experimentation done by psychologists. The cooperation of educators in the latter endeavours was appreciated, but not constitutive of

much in and of itself. Apart from studies that fell directly within the purview of child study, some psychologists engaged in research with children not so much because children were the object of interest, but simply because they served as a convenient population in which to study a particular issue. Though he was, by far, the biggest proponent of the questionnaire method in American psychology, Hall was certainly not alone in adopting the method. He and others employed questionnaires as a means of collecting large masses of data, while recognizing the inherent challenges of scientific practice as distributed among untrained individuals. For these psychologists, however, the benefits of enlisting a multitude of individuals in research outweighed any methodological deficiencies.

As I argue in this chapter, those adopting a naturalistic orientation – which privileged collection, classification, and analysis of large quantities of information (see Crombie, 1994; Hacking, 1982, 2002; Pickstone, 2001) – did so in addition to, not exclusive of, a commitment to experimentation as central to psychological practice (see Young & Green, 2013). A commitment to multiple styles of scientific thinking is especially apparent in the context of projects involving both experimentation and questionnaire-based data gathering. A necessary corollary of the naturalistic orientation associated with the latter form of practice was the emergence of a moral economy of data wherein affect-laden norms and values sanctified particular data driven practices within the discipline. Within this moral economy the quantity of accounts collected was a key component of legitimizing a project's scientific standing. While laboratory science relied on the use of standardized instrumentation wherever and whatever the endeavour (Benschop & Draaisma, 2000; Coon, 1993; Kroker, 2003), questionnaires and the practices of their employment were far less systematic. Here I draw on the work of Daston and Galison (2010), as well as Porter (1995), to argue that Hall and his associates, cognizant of the precarious scientific

status of this work, attempted to validate their endeavours by appealing to the discipline's existing standards of objectivity. Attempts to standardize data collection along mechanically objective lines, in terms of the adoption of both literally and metaphorically mechanical modes of practice, as well as appeals to trained judgment, in the organization and analysis of material, aimed to position questionnaire projects as part of, rather than a departure from, the field's accepted canon of practice. These efforts were never fully successful and, in the absence of any readily available means of demonstrating to others in the scientific community the quality of collected materials, the amount of material amassed served as a convenient stand-in for the value of these enterprises. This work occurred within the framework of the language of numbers that was increasingly dominating society, serving as an immediate, if superficial, marker of authoritative, scientific information (T. M. Porter, 1986, 1995). Efforts to transform collections of descriptive data into numerical form, of one kind or another, were attempts to capitalize on this development. Never fully successful in rendering this material into meaningful quantitative form, questionnaire research nonetheless sought to trade on the growing authority of numbers in its attempts to establish itself as a scientifically valid enterprise.

Beyond Clark Syllabi

To be sure, child study inquiries were by no means confined to questionnaires. Investigators, psychologist and otherwise, employed a variety of methods in their efforts to understand child life (e.g., Chicago Public Schools, 1898). Outside of circular inquiries, some psychologists saw value in undertaking experimental studies of children. At Clark, Boas's efforts to collect information on the physical characteristics of Worcester school children also spawned more psychologically-oriented projects including experimental work by Thaddeus Bolton (1892)

on memory and an inquiry by William Bryan (1892) on motor ability.²²⁵ These kinds of undertakings were also backed by Baldwin (1895a) who, although reluctant to endorse the work done by unskilled parents and educators, noted “children are experimented with so much and so unwisely, in any case, that it is possible that a little intentional experiment, guided by real insight and psychological information, would do them good” (p. 39). Elsewhere others also undertook investigations of children. At Wellesley College a number of investigations were undertaken with children, only some of which relied on questionnaires, including studies of the emotional lives of children, the stories and drawings of children, among others (Calkins, 1895b; Calkins, Buttrick, & Young, 1900; Learoyd, 1895). Similarly, at Columbia several projects employed children as subjects (Dexter, 1897, 1899; Franz & Houston, 1896). For his part, Edward Scripture (1892) of Yale University recommended

every pedagogical seminary should have a laboratory in which, in the first place, the methods of experimental psychology are to be taught and then these methods are to be applied to educational problems. Every teacher should have been a psychologist and a physiologist before he becomes a pedagogue, and these two sciences cannot be acquired anywhere but in the laboratory. (p. 113)

In his view pedagogy was to be subordinate to psychology, which could make valuable studies of children, such as the experimental work engaged in by his student J. Allen Gilbert (1892; 1894).²²⁶ For Scripture, experimentation with children was a valuable undertaking but questionnaire-based work was far less revered, leading him to offer “his criticism and ridicule of

²²⁵ See also “Second Annual Report of the President,” 1891, p. 45; “Third Annual Report of the President,” 1893, p. 117, B1-5-3, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Hall (1900) also endorsed experimental work with children.

²²⁶ For more on the research on children undertaken at Yale see (E. W. Scripture to G. Stanley Hall, April 14, 1893, B1-6-11, [no box or folder number], folder id: B1-D44-9, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

the “unscientific” child study, which is being carried on by President G. Stanley Hall, Principal Russell,” and others (“The New England superintendents,” 1896, p. 348).

Mixing Methods

While Hall and his associates distributed questionnaires to gather information on child-life, and some opted to study children via experimentation, other psychologists occasionally combined both practices within the context of a single study. Though rarely done at Clark (Sears, 1901), doing so allowed some investigators to both collect varied material on the subject at hand and enact more regimented laboratory practices. The challenge, however, was how to combine information gathered through these disparate efforts into some form of a coherent whole. As in questionnaire projects more generally, this was a difficulty never fully surmounted by investigators. What these kinds of investigations make clear, is that rather than the naturalistic orientation at the heart of questionnaire projects being antithetical to an experimental orientation, these epistemological and methodological boundaries were porous, if not non-existent. Even among the questionnaire investigators who did not undertake projects with multiple methods, their naturalistic orientation was not a de facto rejection of experimentation. Theirs was, instead, a broader conception of scientific psychology than those who dogmatically proclaimed the laboratory as the site, and experimentation the method, of the new discipline.

William James and the Investigation of Dizziness

In the early 1880s, around the time of Hall’s inquiry into the contents of children’s minds, William James initiated a study of dizziness. This proved to be some of his only experimental work, in keeping with his later observation: “I am by nature no experimentalist – an

exclusively critical, logical, and literary mind.”²²⁷ James’s study was motivated, in part, by his own experience with seasickness.²²⁸ Previous work with friend and Harvard physiology professor Henry Pickering Bowditch as well as general interest in the work of Hegel, and his non-dualistic conception of mind, among American intellectuals at this time led James to dizziness as a topic that was both physiological and psychological in nature (see Richardson, 2007).²²⁹ On the basis of the apparent lack of seasickness demonstrated by deaf-mutes he posited that the semi-circular canals, or the irritation thereof, was the physiological basis of the affliction (James, 1983b, 1983c). As he noted, the semi-circular canals were not involved in hearing, but were rather theorized to be the seat of “the sense, namely, of translation through space, which in its more extreme degrees becomes the feeling of dizziness or vertigo” (James, 1881a, p. 173, 1881b, p. 412, see also 1882). At least some deaf-mutes, he speculated, lacked functional semi-circular canals and were therefore insusceptible to seasickness. With this notion in mind, he set out to experimentally test his theory.²³⁰

Although experimentation was a key component of the inquiry, questionnaires were also employed (James, 1881a, 1881b, 1882). In effect James set out to gather information on the experience of dizziness, and the lack thereof, from as many sources as possible, however that might be achieved. This included accumulating information not only from deaf-mutes, but from

²²⁷ W. James to H. Münsterberg, 15 May 1892, Box 10, Folder 1834B, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

²²⁸ For James’s views on seasickness see James (1983b, 1983c). His interest in the role of the semicircular canals first made its way into print in a review of experimental work on the semicircular canals (James, 1880). In a footnote in the review he described his own experiments with frogs, testing compensatory head movements following rotation, though he was unable to determine if such movements were a function of the semi-circular canals.

²²⁹ Along similar lines, the doctoral dissertation Hall (1878) completed under James, based on work conducted in Bowditch’s laboratory, was an investigation of “The Muscular Perception of Space.”

²³⁰ For a popular account of this research see Milar (2012).

“normal” subjects as well (James, 1882). Key to the project’s success was the cooperation of officials at a number of institutions for deaf children, including the Horace Mann School in Boston, the Hartford Asylum in Connecticut, the National College for Deaf-mutes in Washington, the Clarke Institution in Northampton, and the Indiana Institution (James, 1881a, 1881b, 1882). James (1882) speculated “among their inmates must certainly be a considerable number in whom either the labyrinths or the auditory nerves in their totality have been destroyed by the same causes that produced the deafness” (p. 240). Not all deaf individuals were expected to be impervious to dizziness, but it was speculated that at least some proportion of them would be, with a further contingent affected to a lesser degree (James, 1882). That the subjects of his project were children was largely incidental, a result of the populations of deaf-mutes easily accessible within institutions charged with their care.

To test his hypothesis James attempted to induce dizziness in his subjects. Those at Massachusetts based institutions – the Horace Mann School, the Clarke Institution, and a number of individuals from Harvard College who were tested for comparison purposes (James, 1882) – were subjected to what James characterized as “by far the purest and most powerful means of inducing vertigo” (1881a, p. 173, 1881b, p. 412).²³¹ This method involved whirling an individual about in a rotary swing. To do so, the apparatus was rotated so that the swing’s ropes were tightly twisted and

the child is then seated on the board, with closed eyes, and head in any position desired, and the torsion of the ropes is left to work its effects freely. These consist in a rapid revolution of the whole apparatus, including its inmate. The moment the speed of rotation

²³¹ James’s brother Robertson (Bob) assisted with the examination of children from the Clarke Institute in Northampton (see James, 1882). Later investigations would also employ James’s family members (see Chapter 5). On the involvement of family members in nineteenth century science see Lindsay (1998).

slackens, the examiner stops the rotation, and sets the child, who has been instructed previously, to open his eyes and walk as straight as possible towards a distant point on the floor. (James, 1882, p. 243)

Those unable to easily walk to the point on the floor were recorded as dizzy, while those who made the trip with ease were noted to be without the experience.²³²

The inducement of dizziness via the rotary swing was the preferred, but not the only method employed. All those tested outside Massachusetts were tested by another means. Rather than undergoing to the “passive rotation” of the rotary swing (James, 1882, p. 242), these children were examined with a method that required no special apparatus. This involved active spinning about on the feet with the head successively upright, bent forward, and inclined on one shoulder....the eyes must be closed to eliminate optical vertigo pure and simple, but opened when the spinning is over, so that the patient may have every advantage for walking straight. (James, 1882, p. 242)

While simpler to conduct, this method came with the attendant difficulty of also inducing a state “muscular vertigo” as the result of whirling and the “involuntary continuance of muscular action” (James, 1882, p. 242). The passivity of the subject in the swing method avoided this potential confound to the dizziness that was of real interest.

Across these investigations 519 deaf-mute children were tested, 186 of whom did not experience dizziness. A further 134 were only slightly dizzy, 199 were normally so, and a few individuals were excessively sensitive. James (1882) was also careful to note that congenital and

²³² In reporting the findings from his project James (1882) reprinted a letter from American neurologist George Beard, wherein Beard states: “First, by means of the swing which you have used in your experiments. I find that persons when put into trance sleep and placed in a swing which is twisted up tightly, so that it untwists rapidly, and for a considerable time, feel no dizziness or nausea, but when brought out of the trance, at once walk away without the least difficulty” (p. 253; see Chapter 5).

semi-mutes were not particular to any one category, but could be found experiencing all levels of dizziness.²³³ In comparison, of the nearly 200 students and instructors James tested at Harvard only one individual did not experience vertigo and very few individuals reported themselves to be only “slightly” or “somewhat” dizzy (James, 1882, p. 242).

In addition to rotation, whether by swing or manual spinning, dizziness was also induced in some by the application of a galvanic current. As with the rotary swing these experiments were limited to children in Massachusetts institutions. The current was first calibrated to be “strong enough to make four normal adults, on whom it was tried, bend body and head strongly over” (James, 1882, p. 244). Applied to forty-three students in Northampton, only five out of twenty-three who were not dizzy following the rotation experiment showed this reaction. Of twenty who were dizzy following rotation fourteen demonstrated some degree of response. Examinations at the Boston school were less successful. Fifteen not dizzy boys received the current and only one showed a response. A further three slight cases and one dizzy case were tested, with only one of the former group reacting. More problematic were attempts to examine female students at the school. As James (1882) noted, at the prospect of receiving the galvanic current “the girls became so nervous that the few results I obtained with them were valueless” (p. 244). The current’s incitement of strong emotional reactions was hardly limited to female students as “subjective feelings … are so numerous and often so intense that a deaf-mute child experiencing them for the first time can hardly be expected to give a very lucid account of them” (James, 1882, p. 244). The child subjects in James’s experiments were far from passive

²³³ Some cases were reported multiple times and James (1882) made an attempt to remove the duplicates. James also alerted the reader “Dr. Clarence J. Blake examined the condition of the ears of the Northampton children, but not being able to deduce any conclusions relevant to my own inquiry from his observations, I leave them unrecorded here” (James, 1882, p. 254).

recipients of his manipulations.

Outside of James's various experimental attempts to stimulate dizziness he also collected information on the subject via questionnaire (James, 1881a, 1881b, 1882). Unlike his experiments, which deliberately included a large sample of non-deaf-mute individuals for comparison purposes, the printed circular was purposely distributed to mostly deaf-mute individuals (James, 1882). Among those who responded to James's questionnaire were a number of those who also participated in his experimental efforts, though by no means did all of these individuals complete his circular as the total number of responses remained far below that of his experiments (James, 1882). The full set of questions posed is unknown, but included among the items were: "Do you ever experience dizziness under any other circumstances?" (James, 1882, p. 243), "Are you dizzy on high places?", and "Have you a good bump of locality?" (James, 1882, p. 252).²³⁴ The latter, James noted, was "a rather stupidly expressed phrase, but one which I supposed would be popularly intelligible" (James, 1882, p. 252). The popular, rather than scientific, comprehensibility of questions was a critical consideration, especially in circumstances where they were to be completed independently by individuals among the general public.

Exactly how the results of his circular inquiry fit with the findings of James's larger experimental undertaking was largely unclear. Perhaps the most important result of responses to his questionnaire was James's (1882) realization of "the fact, notorious at deaf and dumb institutions but apparently not much known to the outer world, that large numbers of deaf-mutes stagger and walk zigzag, especially after dark, and are unable to stand steady with their eyes

²³⁴ James also inquired into the experiences of deaf-mutes when diving, speculating that without gravity to guide them non-dizzy deaf-mutes would experience profound disorientation under water due to their nonfunctional semi-circular canals. This line of questioning produced little by way of definitive results (see James, 1882, pp. 246–9).

closed” (p. 250). Deducing dizziness was not as simple a matter as requesting subjects walk in a straight line following rotation, a fact that soon became apparent in his investigation. At the same time James observed that the absence of swaying was not itself a definite indication of the absence of dizziness. Some of the individuals who were steady on their feet after rotation “did have some sort of a vertiginous feeling, which they expressed by moving the hand wavyly [sic] across the forehead, by saying they were “dizzy” or felt like “falling”” (James, 1882, p. 244). In light of the difficulty of identifying both dizziness and the lack thereof, James (1882) regarded “the experiments... as almost inconclusive” (p. 244). In his usual self-effacing manor, he published his findings with prefatory remarks highlighting his inability to complete an investigation into the subject and his hope that in publishing the results thus far obtained “some one with better opportunities may carry on the work. The regular medical attendants of deaf-mute institutions seem particularly well fitted for such a task” (James, 1882, p. 239).

Josiah Royce and Inquiries into Imitation

Like James more than a decade earlier Josiah Royce, his Harvard colleague, undertook a project that combined both experimentation and the circulation of a questionnaire. In the spring of 1894 Royce, an idealist philosopher with an interest in the new psychology, published a call for assistance with his latest research project within the pages of *Century Illustrated Magazine*. After reproducing his *Schedule of Questions on Imitative Functions*, a list of questions on various aspects of imitation he hoped readers would provide him information on, Royce (1894b) wrote:

Such are some of the matters of natural history concerning which I just now ask for assistance from kindly disposed persons. Of the precise value of a collection of such reports it is impossible to give any fuller account without going into technical details

beyond my present limits. Suffice it to say that all serious efforts to answer any of the foregoing questions will be valuable. (p. 144)

Like Hall and a number of other early psychologists, Royce explicitly conceived of his questionnaire-based inquiry as a form of natural historical research.²³⁵ His detailed, descriptive set of five questions asked for accounts of the place of imitation in child life, imitative games, naughtiness, and emotions, as well as children who failed to imitate.²³⁶ The significance of this kind of collection of material was not necessarily self-evident, but readers were reassured “the specific purposes of some of my questions will not at once be obvious to every reader. It is enough to say, in general, that all my questions bear upon some topic connected with the natural history of imitation” (Royce, 1894b, p. 143).²³⁷

As in child study projects, Royce’s ideal data collectors were parents and educators. It was “observant parents” as well as “teachers, other observers of children, and observant persons generally” (Royce, 1894b, p. 142) who were to supply the desired information on imitation. Even those not in the position to observe child-life could contribute to the project by offering their reminiscences of their own childhood experiences (Royce, 1894b, p. 142). As he noted,

what is most needed is the coöperation of many independent observers; and owing to the

²³⁵ Around the turn of the twentieth century Royce drafted another circular “for the sake of collecting material for a discussion of the relation of traditional Christmas festivities to the health, happiness, and general welfare” of children. He intended the inquiry to be circulated within a Mothers’ Club and beyond, but it is unclear if the circular was ever distributed and any responses collected (“The Influence of Christmas Festivities upon the Welfare of Children. A Circular of Inquiry,” [ca.1901], Box 92, Papers of Josiah Royce, HUG 1755, Harvard University Archives, Cambridge, MA).

²³⁶ On Royce’s views on imitation and the development of the self see Royce (1892, 1894a, 1894b, 1895a, 1895b, 1895c, 1895d, 1895e, 1897, 1898b).

²³⁷ Those sending in responses were reassured that their information would be kept confidential, no personal details or names revealed and reports “shall keep confidential statements in a safe place, where they will surely be destroyed without further examination in case of my death” (Royce, 1894b, p. 145). None of this material remains today.

nature of the facts concerning which I shall here ask, such observers will be able to contribute many useful data for comparison, even where the observers themselves are no experts in psychology....could I get many psychological data of certain kinds from various independent observers, widely sundered in place, and widely differing in their opportunities, I should be aided in guiding certain of my intended investigations into the nature, the development, and the factors of these imitative functions of mankind. (Royce, 1894b, p. 142)

Parents and educators, though untrained in psychology, were in the best position to observe and record the imitative actions of children. In doing so, educators in particular “become a psychologist, not in the systematic, but in the scrutinizing sense; not as scientific generalizer, but as observing naturalist, as collector of mental facts” (Royce, 1891b, p. 127). This inclination toward observation would not only benefit his project, but also prove useful in the teacher’s relations with individual children.²³⁸ The participation of untrained individuals in scientific endeavours was by no means unique to psychology during this period, serving as a crucial to success in research projects across a number of domains (Kohler, 2006; Vetter, 2011).

Non-specialists were central to gathering material on childhood imitation via circular, but involvement in experimental work was more circumscribed. In the fall of 1894, following the circulation of his questionnaire, Royce (1895a) began a laboratory-based study of imitation. Under the direction of Münsterberg, head of Harvard’s psychology laboratory, he engaged four adult women and an equal number of men “all of a fair although decidedly varied amount of

²³⁸ Royce gave a series of twelve public lectures, “Topics in Psychology of Interest to Teachers” in February through May of 1893 (see Royce, 1893a, 1893b, 1893c, 1893d). A further series of lecture was delivered in 1897-98 (“Lectures to teachers: Boston, 1897-1898,” Box 69-70, Papers of Josiah Royce, HUG 1755, Harvard University Archives, Cambridge, MA). On Royce’s views on education see also Royce (1883, 1891a, 1891b; see Rudy, 1952).

introspective preparation” (Royce, 1895a, p. 233) in his project. These at least somewhat skilled introspectors were asked “to listen to a rhythmic series of taps (made with an electric hammer), and then to reproduce this series by means of an electric key” (Royce, 1895a, p. 218). The original series of taps, as well as the attempted imitation was graphically recorded on the drum of a kymograph. Subjects were also instructed to record their subjective experiences of imitation immediately after the experiment (Royce, 1895a). The study was followed by another that sought to experimentally bring about inventiveness, held to be the counterpart of imitation (Royce, 1898b). Once again taking place within the space of the laboratory, the project had subjects engage in a series of drawing exercises in which they were to avoid imitation and strive for originality as much as possible.

Royce’s approach to his experimental work was much the same as that of his circular. In engaging subjects within the laboratory he aimed “to get a pretty careful series of records of the facts, and to wait for experience to indicate the best further procedure” (Royce, 1895a, p. 232). Collecting data, whether by circular or otherwise, was an exploratory process. Through trial and error Royce (1895a) refined his experimentation, altering the series of taps presented to subjects in his imitation study after concluding that some series were “too complex to promise any definite results” (p. 231). His persistency in this work was noted by James who observed “Royce comes almost daily, and covers sheets of paper with “imitations” and introspective notes by the imitators, showing an ultra-deutschen Ausdauer [ultra-German stamina], but so far no formulable results.”²³⁹ The final product of this dedication, like that of his questionnaire, is unclear. His records of imitation revealed “the most widely varying subjective processes, which do not seem to be constant, even for one subject” (Royce, 1895a, p. 234) rather than any definite pattern of

²³⁹ W. James to H. Münsterberg, 23 April 1896, Box 9, Folder 1834A, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

imitative experience. These records like the results of his inquiry into invention offered only “a first glimpse into the labyrinth of the influence of social suggestion” on both imitation and invention (Royce, 1898b, p. 128). The collection of facts, inside and outside the laboratory, offered only the barest hint of a larger understanding of the social processes Royce was hoping to document. The synthesis of information gleaned solely by questionnaire was challenging – as we shall see – and the addition of findings from experimental undertakings, as Royce and James discovered, did not improve the situation. Definitive conclusions, whatever methods adopted, were hard to come by.

Questioning the Questionnaire Method

By the end of the 1890s psychological work in child study and the proper relation between psychology and pedagogy were increasingly contentious issues. Particularly polemical were a series of articles published in 1898 by Hugo Münsterberg (1898a, 1898b, 1898c, 1898d), the director of Harvard’s psychological laboratory, and one of the chief advocates of experimentation as the discipline’s methodology. Within the pages of the *Educational Review* and *The Atlantic Monthly* Münsterberg made clear his antipathy for both Hall’s and Scripture’s brand of pedagogically-oriented psychology and the attempt to refashion pedagogy in the style of scientific psychology.²⁴⁰ Others in the field were soon drawn into the discussion.

Münsterberg was not always disparaging of Hall’s efforts. Prior to his published criticisms at the end of the century he and Hall crossed paths in person at an April 1895 meeting

²⁴⁰ Münsterberg’s (1898b) first *The Atlantic Monthly* article in this series began with harsh criticism of Scripture’s recent volume *The New Psychology* (1897), though he refused to name the author outright. In response, Scripture’s former student Charles Bliss (1898), now of New York University’s pedagogy department, published a defence of the value of psychological work in the training of teachers (see Benjamin, 2006). On Münsterberg’s denigration of applied psychology more generally, as well as his later involvement in applied work see Benjamin (2006). Following his change of mind regarding applied psychology Münsterberg published a volume on *Psychology and the Teacher* (1909).

of the Massachusetts Schoolmasters' Club. Though others spoke at the meeting as well, the two psychologists largely confined their remarks to each other. Münsterberg (1895), still something of a newcomer to the American psychological scene, noted at the time

the teacher who is a careful observer may furnish to the psychologist valuable material by the continual observation of children. The extremely interesting and important collections of such material, which my friend, President Stanley Hall has brought together, give the best and most encouraging illustration of such cooperative work. (p. 19-20)

At this point, Münsterberg was at least nominally supportive of Hall's efforts to collect enmasse accounts of child life. Doing so, he went on to comment, was something uniquely within the purview of educators:

the teacher has so many chances to see events in the child's mind which would escape the official psychologist if he could not rely on the teacher's help, just as the biologist collects the reports of hunters, who have chances for observation which the biologist in his laboratory never can have. (Münsterberg, 1895, pp. 19–20)

Offering an explicit articulation of the naturalistic orientation of these kinds of data collection projects he emphasized the important role of teachers as field workers. They, unlike the psychologist, were uniquely capable of enacting the kind of "unobserved observation" (Pettit, 2013b, p. 203) of the children in their care privileged by psychologists.²⁴¹ Privately, Hall wrote him

I certainly supposed from your remarks ... that you had little faith in Child Study. I think all who heard you got that impression, & its enemies received great aid & comfort from

²⁴¹ The persistent "appeal of the ideal of unobserved observation" (p. 203) among psychologists is discussed throughout Pettit (2013b). See also Morawski (1988a).

your words. I am still more surprised that you write to me that you believe teachers can do good work in this direction.²⁴²

Surprising or not, apparently in the “natural” setting of the classroom, Münsterberg considered educators uniquely positioned to witness and record features of child-life for the benefit of psychologists.

The value of teachers’ contributions to psychological knowledge notwithstanding, the latter was of little assistance to educational practices. So far as Münsterberg was concerned, modern psychological knowledge was of no help to teaching and, even more problematically, employing educators as data collectors was itself a hindrance to the teacher’s performance of their proper duties. Asserting that he does not, and would not, study his own children he implored teachers to “love their pupils instead of observing them” (Münsterberg, 1895, p. 25). In response, Hall (1895a) inquired: “does he regard child study as vivisection of the soul?” (p. 36). Accusing Münsterberg of considering child study “solely from this standpoint of laboratory and scientific apparatus” Hall (1895a) noted “I am sure his prejudices would be overcome if he would examine some of the thousands of returns, by non-psychologists, which I am now engaged in reporting on” (p. 36).

Three years later Münsterberg continued to argue that disinterested scientific stance characteristic of the new psychology was directly at odds with the needs of the teacher. In his view,

love and tact and patience and sympathy and interest are more important for the teacher than any psychological observations he can make on children, and that these observations

²⁴² G. Stanley Hall to H. Münsterberg, 4 May 1895, Folder 1766, Item 3, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

are natural enemies of his instinctive emotional attitudes because they dissolve the personality into elements. (Münsterberg, 1898b, p. 165, see also 1898d)

The incompatibility of the scientific attitude and the teacher's proper role was something others similarly noted. In his volume *Talks to Teachers on Psychology, and to Students on some of Life's Ideals*, based on a series of public lectures in the early 1890s, James (1899b) noted "I cannot too strongly agree with my colleague, Professor Münsterberg, when he says that the teacher's attitude toward the child, being concrete and ethical, is positively opposed to the psychological observer's, which is abstract and analytic" (p. 13).²⁴³ The United States Commissioner of Education, philosopher and educator William Torrey Harris informed Münsterberg

the true interests of education have been very much strengthened by your timely articles. For those true interests of education demand that the large body of teachers shall not be turned aside from a belief in moral responsibility and the freedom of the soul, the permanent personality of the child, by any analytic theories or hypotheses, useful and necessary as these latter may be in taking a scientific inventory.²⁴⁴

Similarly, Royce (1891b) contended "teaching is an art. Therefore there is indeed no science of education. But what there is, is the world of science furnishing material for the educator to

²⁴³ James was less than enthusiastic regarding his course of public lectures to teachers, given at Harvard's behest: "They are forcing me to give ten lectures here on "Topics of Psychology of interest to Teachers." It is lamentable work!" (W. James to W. T. Harris, Nov. 14, 1891 in James, 1999, p. 220). See also W. James to P. H. Hanus, April 6, 1891 in James (1999, pp. 148–9), as well as W. James to J. Royce, March 18, 1893 in James (1999, pp. 400–2). The lectures were delivered in Boston in 1892 and James continued to deliver the lectures for teachers across the country throughout the 1890s (see James, 1899b). See also W. James to H. Münsterberg, 2 September 1896, Folder 1834B, Item 15, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

²⁴⁴ W. T. Harris to H. Münsterberg, 22 April 1898, Folder 2273b, Item 1, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

study" (p. 132). Teaching, as a profession, demanded a very different skill set than science. Scientific psychology might provide useful information on children and pedagogy, but this information was to be the product of work by those other than educators.

Münsterberg took his argument against educators engaging in psychological research as a means of improving their pedagogy one step further. Inasmuch as teachers were unable to contribute anything of value to the body of psychological knowledge, psychology was also unable to guide the teacher in their work. The impulse of educators to adopt the research practices of new psychology with the hope of improving their pedagogical efforts was simply ill conceived. So far as Münsterberg (1898b) was concerned

this rush toward experimental psychology is an absurdity. Our laboratory work cannot teach you anything which is of direct use to you in your work as teachers; and if you are not good teachers it may even do you harm, as it may confuse you and inhibit your normal teacher's instincts.... You may collect thousands of experimental results with the chronoscope and the kymograph, but you will not find anything in our laboratories which you could translate directly into a pedagogical prescription. (p. 166)

Experimental psychology, the proper mode of the new psychology, was a discipline distinct from pedagogy. Work in the former field had no direct bearing on the practices of latter. Rather than attempting to subordinate pedagogy to psychology, along the lines of Hall and Scripture, it would be best if the two went their separate "ways, with sympathy, but without blind adoration for each other" (Münsterberg, 1898b, p. 167).

The vitriolic style of Münsterberg's critique attracted much attention. While Cattell was unimpressed with Münsterberg's concurrent critique of psychological measurement,²⁴⁵ he agreed "that the usefulness of experimental psychology to the teacher has been exaggerated in certain quarters."²⁴⁶ Nonetheless, he was unwilling to dismiss entirely the benefit of psychological knowledge to education: "if psychology is of value to the teacher and experiment has enriched psychology, then it is scarcely fair to say that experiment and laboratory work – whether as a training or as a professional help – are valueless."²⁴⁷ If teachers were to receive training in any one science it should be the one in closest relation to their field and this, undoubtedly, was psychology (Cattell, 1898). James likewise

rejoiced in any expression from an authority like him which might tend to destroy in the teacher's eyes the prestige of all this industrious mystification to which they are exposed about the immense help to which is to come to them from psychological laboratories and measurements.²⁴⁸

As far as James was concerned, the exaggerated claims of a fundamental connection between work in experimental psychology and teaching deserved to be struck down: "teaching and psychology have got to keep in connexion.... to flourish elementary measurements before the teacher's eyes as his own ideal goal seems to me to be little short of criminal."²⁴⁹ John Dewey

²⁴⁵ See J. M. Cattell to H. Münsterberg, 26 May 1898, Folder 1614, Item 3, Hugo Münsterberg Papers, Boston Public Library, Boston, MA. See also W. James to J. M. Cattell May 4, 1898 in James (2000, p. 363). Further comment on this aspect of Münsterberg's article can be found in Thorndike (1898f) and Titchener (1898a).

²⁴⁶ J. M. Cattell to H. Münsterberg, 26 May 1898, Folder 1614, Item 3, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

²⁴⁷ J. M. Cattell to H. Münsterberg, 26 May 1898, Folder 1614, Item 3, Hugo Münsterberg Papers, Boston Public Library, Boston, MA.

²⁴⁸ W. James to J. M. Cattell, May 4, 1898 in James (2000, p. 363, see also W. James to J. M. Cattell, May, 1898 in James, 2000, p. 362).

²⁴⁹ W. James to J. M. Cattell, May 4, 1898 in James (2000, p. 363).

(1900) expressed his agreement “with much of what Dr. Münsterberg says about the uselessness and the danger for the teacher of miscellaneous scraps of child study, of unorganized information regarding the nervous system, and of crude and uninterpreted results of laboratory experiment” (p. 106), his concern rooted in his view that the teacher “must not assume the psychological attitude. If he does he reduces persons to objects, and thereby distorts, or rather destroys, the ethical relationship which is the vital nerve of instruction” (Dewey, 1900, pp. 109–10). Royce (1898a) pointed out that the efforts of psychologists and educators were fundamentally at cross-purposes since “the laboratory psychologist properly desires these experiments for theoretical purposes [and] the teacher desires them to be of immediate practical significance” (p. 89).

As part of his rejection of the benefits of psychological work for educators Münsterberg also offered a far more negative view of educators as data collectors than that articulated three years earlier. In one of the most vehement and colourful critiques of the questionnaire method offered during this period he contended:

the work must be done by trained specialists or not at all. That child study which has for its aim only the collection of curiosities about the child, as an end in itself, may be grateful to the nurse who writes down some of the baby’s naughty answers or to the teacher who sacrifices half an hour of her lesson to make experiments in the classroom to fill out the blanks that are mailed to her. The students of that scientific child psychology which stands in the service of the general mind study know how every step in the progress of our science was dependent upon the most laborious, patient work of our laboratories and the most subtle and refined methods, and that all this seductive but rude and untrained and untechnical gathering of cheap and vulgar material means a caricature and not an improvement of psychology. (Münsterberg, 1898a, p. 115)

The educator, untrained in psychology, lacked the proper attitude for engaging in research. The solution to this was not the inculcation of this attitude in educators, but rather an appreciation that teachers must adopt a style other than psychological in order for them to do their jobs well. Baldwin (1898) concurred: “it is an insult to the teaching profession to tell them that their humanity needs this sort of cultivation, and to hoodwink them into thinking that they are making contributions to science” (p. 219). Jastrow (1901) similarly endorsed this critique, but despite these kinds of assertions, neither he nor Münsterberg were singularly committed to the laboratory as the site of science. Both, for instance, engaged in trickery modeled on the craft of stage magicians, rather than the techniques of laboratory practice, in their efforts to debunk psychical phenomena (see Pettit, 2013b). Loyalty to the laboratory thus served more as a rhetorical strategy of disciplinary inclusion and exclusion, than an absolutely observed value.

Others also embraced the argument that educators were ill equipped to collect data and that this work detracted from their proper teaching duties. Titchener (1896a) cautioned the method “has grave dangers, and must be used with great caution” (p. 238), while Royce (1898a) observed

the psychologist in the laboratory cannot well control the scientific use of the syllabus by persons outside of the laboratory. Lacking himself the time for an adequate personal study of children in school-rooms, the laboratory psychologist is consequently too often left dependent on the observations of unskilled persons for far too many of his data. Meanwhile the syllabus, often inexpertly used and filled out, through the busy teacher’s aid, may have led to much waste of time in the school-room. (p. 89)

Despite, or perhaps because of, his earlier questionnaire inquiry that relied on the participations of educators Royce was not convinced that the efforts of teachers were worthwhile. Bemoaning

that “we are left at present to crude observation or to loose generalization by the practical teachers, on the one hand, and to laboratory investigations or to statistical collections of far too theoretical an interest, on the other” (Royce, 1898a, p. 93), he suggested the creation of school oriented “consulting psychologists” as an occupational category which might bridge the gap between the competing aims of psychological research and teaching.

James, Münsterberg and Royce’s Harvard colleague, was similarly disposed. Speaking to teachers, as part of his series of 1890s public lectures, he reassured those before him

I fear that some of the enthusiasts for child-study have thrown a certain burden on you in this way... There are teachers who take a spontaneous delight in filling syllabuses, inscribing observations, compiling statistics, and computing the per cent. Child-study will certainly enrich their lives. And, if its results, as treated statistically, would seem on the whole to have but trifling value, yet the anecdotes and observations of which it in part consists do certainly acquaint us more intimately with our pupils....But, for Heaven’s sake, let the rank and file of teachers be passive readers if they so prefer, and feel free not to contribute to the accumulation. (James, 1899b, pp. 12–3)

The directive that educators must collect material for child study in order to best serve their students was simply ridiculous to James’s mind. Accumulating information on pupils for psychologists like Hall was unnecessary and ultimately inconsequential to the actual work of teaching. Edward Thorndike (1898f) likewise encouraged educators to continue their psychological undertakings, if they so desired, noting “very poor psychology it may be, very inaccurate and inconsistent and misguided but they can do work as good for the purposes of mental science as much of the work of naturalists has been for biology” (p. 650). For teachers these kinds of undertaking were, at most, benign activities. Both James and Thorndike stopped

short, however, of completely dismissing the psychological value of amassed observations on child-life.

Efforts at Objectivity

Even prior to the furor of criticism unleashed with Münsterberg's articles, Hall explicitly sought to frame his topical syllabi as objective, scientific enterprises. For its psychological adherents, the adoption of a taxonomic or natural historical style of scientific thinking (Crombie, 1994; Hacking, 1982, 2002; Pickstone, 2001), where the psychologist took on the role of mental naturalist, legitimized the pursuit of psychological knowledge through the construction, distribution, and collection of questionnaires. In Hall's case, however, such a vision of the scientific enterprise was not totaling. Alongside his naturalistic questionnaire research, Hall privileged the laboratory as a space of scientific psychology – going so far at one point as to claim some part in founding psychology laboratories that he was in no way associated with, much to other psychologists' chagrin (Hall, 1895b; James, Ladd, Baldwin, & Cattell, 1895). This kind of hubris recognized that work within laboratory spaces legitimized psychology as a science. Questionnaire work, which necessarily took place in more diverse spaces, was very much rooted in a different style of scientific practice. To reconcile this with his continuing espousal of the importance of psychology as a laboratory science and his persona of a modern scientist (see Bordogna, 2005), Hall strove to align his questionnaire work with the standards of objectivity at the heart of experimental laboratory investigations.

Hall hoped invoking a blend of mechanical objectivity, during the process of data collection, and trained judgment, during the organization and analysis of material, would substantiate the scientific nature of his syllabi work.²⁵⁰ Reliance on untrained collectors, Hall

²⁵⁰ On different standards of objectivity see Daston and Galison (2010) and Porter (1995).

was well aware, undermined the scientific standing of this work. In addition to the attempts to expertly analyze material after its inexpert collection, specific measures were taken to ensure the material amassed was of quality form. One means of tying questionnaire-based work to the exalted experimental mode of scientific psychology, was through the inclusion of items that inquired into associated physical states.²⁵¹ Linking the physical and the mental, as Osborn earlier sought to do in his questionnaire efforts (see Chapter 1), provided a physiological grounding for at least some of the descriptive material gathered on mental life. Far more pervasive, however, was the provision of instructions on how to go about collecting objective material.

The lay collectors employing Hall's syllabi were given directions on how to go about amassing mechanically objective data. Outside of the system of collecting Williams outlined (see Chapter 3), syllabi themselves included specific instructions on how individuals were to be observed and information recorded. If properly constructed, questionnaires

suggest every main aspect, but no one answer rather than another, and must call for form enough so that the data can be fully treated statistically, yet must leave freedom enough to bring out details of all important cases which may be abridged and cited. (Hall, 1897a, p. 148)

This did not mean, of course, that all responses were of value. As Hall (1897b) admitted "we get returns practically worthless. There are many tests no one but an expert psychologist can make, but the criticism that all tests are worthless unless so made is absurd" (p. 184). So long as syllabi

²⁵¹ See, for example, Hall's inquiry into Anger which includes items such as "Describe every vaso-motor symptom, such as flushing, paling, about forehead, cheeks, nose, neck, or elsewhere..." and "Describe all changes of muscle tension..." ("Anger," Oct. 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA). See also Hall (1899, p. 529). This aspect of his investigation was later commented on favourably in a review provided by Arthur Allin (1899) of the University of Colorado, a former Fellow at Clark and Hall's collaborator in a syllabus inquiry of "Tickling, Fun, Wit, Humor, Laughing" (Feb. 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA; Hall & Allin, 1897).

were “suitably directed...almost any good teacher or parent” (Hall, 1893d, p. 430) could collect scientifically sound information. The instructions on syllabi, as well as Williams’s (1896) guide to collecting, were to fulfill this function.

In constructing his questionnaires, Hall included specific instructions meant to ensure that precise, objective data were collected. Included on his syllabus on “Anger” was the edict to those collecting material: “In description be photographically objective, exact, minute and copious in detail.”²⁵² The talk of photography in these directives was neither incidental, nor merely metaphorical. The syllabus on “Crying and Laughing” asked “if you are a photographer get snap shots at children crying and laughing, and if you have a phonograph record a few cries and laughs.”²⁵³ Those investigating early forms of vocal expression with his syllabi were instructed

Be careful to record each sound or word with any spelling, accent, or other phonic method or sign that will show the exact pronunciation. The Bell method of notation, or better the phonograph, are suggested when practicable, but careful observation is the main thing.²⁵⁴

In gathering data via questionnaire collectors were to be mechanically objective. This objectivity could be enacted metaphorically by being “exact, minute and copious in detail,” systematically through the employment of standardized notation schemes, or literally through the employment of mechanical devices like photographic or phonographic equipment in data gathering. The latter effort to collect recordings made “by the camera and phonograph,” while never the central aim of

²⁵² “Anger,” Oct. 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See also Hall (1899, p. 529).

²⁵³ “Crying and Laughing,” Dec. 12, 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Photographic depictions of emotional states were, of course, earlier a central component of Darwin’s volume, *The Expression of Emotions in Man and Animals* (1872).

²⁵⁴ “Early Forms of Vocal Expression,” Jan. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

questionnaire research, was characterized by one commentator as “most praiseworthy” (Thorndike, 1898e, p. 89).

The invocation of mechanical objectivity did not, of course, mean that other forms of objectivity were inconsequential. While attempting to engage his collectors in mechanically objective practices, Hall also appealed to other forms of scientific authority. Discussion of bodies of data positioned collected material as valuable on the basis of trust (see Daston & Galison, 2010; T. M. Porter, 1995). Reports

by college or university students of psychology, by friends of the writer, in whose competence and reliability he has the greatest confidence, and particularly those from Miss Williams are made by a method calculated to eliminate very many at least of the possible defects and errors. (Hall, 1897a, p. 239)

This trust in Williams, with her much praised method of accumulating information, as well as others was predicated on personal relationships. These individuals were positioned as producers of quality, objective material, as they could be trusted to follow the directives for collecting data provided on Hall’s syllabi. Invoking an intimate familiarity with at least some of these collectors, Hall asserted his own scientific authority as guarantee that the work of these individuals was trustworthy, and thus of scientific value.

The hope, at the end of all of this, was to produce something like a composite portrait of the phenomena under investigation. Doing so, even given the attempt to amass objective data, was by no means a straightforward affair. Writing in his autobiography, Hall (1924) observed vast bodies of data were accumulated and sent to us in response to some of the more popular questionnaires, and it was a very perplexing question how to make the net resultant of it all into anything like a composite photograph of the subject, point by point.

(p. 390)

This aim was by no means merely an after the fact observation, as the goal of producing “composite portraits” was present from the earliest days of Hall’s questionnaire efforts (Hall, 1883b, p. 252, see also 1893d).²⁵⁵ A statistical approach was the best hope of achieving this goal, but as Hall (1893d) noted

the digesting and presentation of these returns, when very voluminous, is sometimes a difficult matter. Just what rubrics should be chosen, how the table and curves should be presented, especially the value and treatment of variations from the average, involve often the most complex methods of the statistician. (p. 430)

Given the difficulties of analyzing masses of descriptive information accumulated with syllabi the statistical method was never particularly successful. Synthesis, of the form of composite portraiture, was elusive.

Of course, not all material was of sufficient quality to include in a synthesis. Responses to syllabi came both from those Hall deemed trustworthy and a multitude of others. One rhetorical strategy adopted to deal with this complication was Hall’s (1897a) regular recognition of this feature of questionnaire-based research: “the imperfections of both the methods and results of all this work are very obvious, and everything depends on keeping them all in sight throughout. This cannot be said too distinctly, emphatically, or too often” (p. 239). Those undertaking this kind of research had constantly to keep in mind

most returns are not made by experts, but by young people with little knowledge of psychology or of the dangers of loose and inaccurate statement, and who are peculiarly

²⁵⁵ Elsewhere, Hall (1900) explicitly aligned this aim with his understanding of the Baconian grounding of his work (see Chapter 1): “The object here is to make what Bacon would call a *silva silvarium*, or a large collection of actual facts, and later … to present something a little like a composite photograph of the subject” (p. 697).

prone to exaggeration in describing their feelings. Some returns are seen to be of no value, and are rejected at the start. Many ... are filmy and no doubt far less real than the language would indicate. Some, too, no doubt, are almost purely imagined. The data have all degrees of value from nothing up to very great. (Hall, 1897a, p. 239)

This did not invalidate the method in toto, but meant that the investigator had to be ever aware that “returns are of all degrees of merit, from extremely good to worthless, and it requires great and constant critical acumen to sift the chaff from the wheat” (Hall, 1899, p. 530). Only a trained expert like Hall (1899) was capable of doing so “accurately and thoroughly” (p. 530, see also Hall, 1900, p. 692), thereby ensuring the scientific legitimacy a project.

Dealing with Data

As responses to topical syllabi came in by the hundreds, sometimes thousands, efforts were made to organize these masses of material (see Hall, 1924; Wiltse, 1895). At various points in time Hall employed assistants exclusively for this work. This was in direct contrast to his earliest questionnaire endeavour, where his solution to the difficulty of managing a large body of information was significantly different. In his study of the contents of children’s minds, he simplified the process of analysis by opting to base his conclusions on the responses of two hundred “average” children selected from a larger body of responses more than twice this size (Hall, 1883b, 1893c). Even still he noted “the work is laborious, involving about fifty thousand items in all” (Hall, 1883b, p. 270). Later investigations, recognizing the sway of large quantities of data even given its associated difficulties, dealt with the whole of collected information. Of the process of making sense of the material collected via syllabi, Hall (1897b) observed when these returns come in, one at first feels helpless before them. We have now at least a hundred thousand returns, some of them comprising forty and fifty pages of manuscript.

Statistical methods have been used, and the very best trained statisticians have to be employed, and we have no money for these. We have kept four people at work off and on for nearly a year. (p. 184-5)

This labour also fell along gendered lines as women were those tasked with analyzing data, including for a time a Miss Watson and a Miss Rawson, whose assistance was deemed “expert” (Hall, 1897a, p. 151). By the early twentieth century funds were explicitly secured for the purpose of employing “a competent and well trained research assistant” who was to devote their time “to working up data” (Hall, 1903, p. 96). The position was filled by psychologist Theodate Smith (Hall & Smith, 1903a, 1903b, 1903c; T. L. Smith, 1903), who like Williams positioned herself as an expert on the questionnaire method (see T. L. Smith, 1903).²⁵⁶ In many respects the explicitly statistical work associated with large masses of material proved as labour intensive as the process of collecting data.²⁵⁷ The difference was that in the former undertaking it was a select number of trained specialists who were employed rather than the multitude of laypersons engaged in the latter work. Even still, these female specialists were largely relegated to the mundane task of sorting and organizing data, rather than drawing conclusions on the basis of this material. Like the World War Two era women engaged in the clerical labour of computer programming, this feminized technical work was devalued and largely unrecognized (see Light, 1999).

Some attempts to organize data were almost appallingly elaborate. Hall lamented that his syllabi on fears lacked complete information, despite attracting information on the fears of more

²⁵⁶ The position was funded through a gift of \$1,000 from a Mr. Arthur S. Estabrook of Boston and a grant of \$2,000 from the Carnegie Institution (see Hall, 1903, p. 96). On Smith’s role at Clark see Diehl (1991).

²⁵⁷ For the explicit framing of questionnaire projects as statistical undertakings see Hall (1883b, 1897b) and Hall and Ellis (1896).

than 1,700 individuals and encompassing more than 4,000 manuscript pages.²⁵⁸ Of these some reports consisted of no more than a list of feared objects, while others included substantial detail of fears and fright. More problematic still, numerous accounts left out information regarding age or sex, or sometimes both. To synthesize this bulk of material an elaborate system was devised and implemented by Miss Watson and Miss Rawson:

every individual was first represented in large tables by a line showing each of his or her fears, with age and sex, and with fullness of presentation marked on a scale of 10, with hieroglyphic signs for special features and a wide column for miscellanies. In these elaborate charts returns from each locality were kept by themselves, and running numbers referred to the original papers. (Hall, 1897a, p. 151)

These extensive charts formed the basis for Hall's published discussion on the topic, but were far too detailed and expansive to print themselves. A description of the effort to synthesize the material had to serve as sufficient guarantee of the exacting work behind the presented findings. This kind of manipulation of information further distanced the scientific community from the original material, but provided much needed organization to otherwise incoherent masses of material. While providing some kind of organizational framework to the material, the undertaking was exceedingly laborious and ultimately contributed little to synthesizing the data into a meaningful whole.

Outside of the immediate milieu of Clark University, Joseph Jastrow (1891c, 1897b) spoke approvingly of the statistical possibilities of psychological work in child study. Emphasized, in particular, was the value of conducting large-scale studies of children, which might encompass the inherent variability of childhood and reveal the course of normal

²⁵⁸ "Fears in Childhood and Youth," Feb. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

development. This could only be achieved looking at individuals not singularly, but collectively, and applying to this material the technical logic of statistics, in much the same manner as was done with information from the national census (see Jastrow, 1897b).²⁵⁹ And, he noted, given a large body of collected data one could also

sub-divide it. We study men and women separately, and then little men and little women.

It is surprising to note how soon and how persistently differences in the development of the two sexes appear....We may divide our facts in many other ways, comparing children from town and children from the country, children from better-educated homes and those coming from ignorant homes. We can ask almost as many forms of questions as we have interests in the study of children. (Jastrow, 1897b, p. 106)²⁶⁰

A singular picture of normal development could be ascertained from masses of data, but so too could information regarding regular and persistent differences between groups. To achieve this, however, “we need trained students – we may call them psychologists, educationalists, students of Child-Study – to properly interpret the facts which may be gathered in our school rooms” (Jastrow, 1897b, p. 101). Any educator might gather information on child-life, but only the statistically savvy expert could make use of the body of data. Hall (1900) too characterized the analysis of syllabi responses as “an expert problem, in which the opinions of experts only have value” (p. 693). In doing so, he immediately characterized as invalid all criticisms of this research from anyone but other questionnaire experts. With Clark as the centre of these kinds of undertakings, he in effect dismissed the views of all the method’s critics.

²⁵⁹ Regarding the census see Jastrow (1885b; see Chapter 7).

²⁶⁰ Jastrow is referencing Hall’s earliest questionnaire work in which knowledge and ignorance, and differences between town and country, were key components (see Hall, 1883b, 1893c).

A key feature of questionnaire research was discussion of various forms of difference. As Hall (1900) saw it the aim of this work was “a large collection of actual facts, and later to group them according to sex, age, etc., so as to present something a little like a composite photograph of the subject” (p. 697). The published the results of questionnaire investigations presented information in various ways, including providing basic statistics on the collected material. Take, for instance, Hall’s study of the contents of children’s minds, which presented differences along several lines. In a table of the percentage of ignorance among children regarding various basic objects and concepts, he opted to divided his data into five columns: boys, girls, Irish children, American children, and kindergarten children (Hall, 1883b, p. 254). Providing this information in the form of a table allowed for easy comparison between groups regarding their knowledge of each item. This kind of simplified presentation was made possible by the very form of the data collected; individuals could only fall into one of two categories: those who were and were not ignorant of a particular item. This division along the lines of yes/no, or knowledge/ignorance, streamlined the presentation and interpretation of information in ways that were often impossible in later topical syllabi studies, which inquired much more broadly into a subject. Nonetheless, in subsequent studies efforts were made to transform masses of descriptive information into numerical form.

Even when data was presented in simplified tabular form in these later endeavours, the nature of this information was such that tables were impossible to read with ease. In the case of A. Caswell Ellis and Hall’s (1896) inquiry into children’s relationships with dolls, data was transformed into numbers and presented within seventeen category rows (see p. 152-3).²⁶¹ In

²⁶¹ This is the material collected with a supplementary syllabus on dolls issued following an earlier syllabus on the subject (“Dolls,” Nov. 1894, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA; Ellis & Hall, 1896, pp. 129–30). The former was issued

every case information from boys and girls was presented separately. Divided in this way were, among other categories, kindergarten practice schools in Boston, children below age 6 from Worcester, those between age 6 and 12 from Worcester, and foreign children. The sole group to feature a row of information from one sex, but not the other was feeble-mindedness, for which only data from girls was included. Columns in the table were numbered to correspond to items on the syllabi, but possible responses to these were not always limited to simple yes or no answers.²⁶² Consequently, reading the table required referencing a detailed description regarding what the numbers in each column represented. Further complicating the matter was the inclusion of two rows of numbers for each category row, the meaning of which varied from question to question. For instance, values for question 3 were the average “age of beginning and stopping doll play, placing the former over the latter” (Ellis & Hall, 1896, p. 154), while for item 12 “the upper figure designates the preference for large and the lower small dolls” (Ellis & Hall, 1896, p. 155). In no case was information regarding what values in the body of the table referred to included within the table itself. Synthesis of masses information into tabular form was a means of reducing material into a form that could be easily shared, but with descriptive data that could not be easily and consistently reduced to simple binaries the legibility of such was by no means guaranteed. Even when material was transformed into tables and curves it was not immune from criticism. Syllabus data, Baldwin (1898) conceded “if worked out, might bring some order into the dreary mass of facts, reports and syllabus returns” but instead he noted, with great sarcasm,

“to get better statistical results” (Ellis & Hall, 1896, p. 131; see “Dolls” (Supplementary Questionnaire), June 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

²⁶² The syllabi comprised 29 questions, but not all these were represented in the table (Ellis & Hall, 1896, p. 131; see “Dolls” (Supplementary Questionnaire), June 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA).

they are arranged “in tables, and if a curve, a holy curve, can be drawn, that is rich, though it bar the way to thought. Let no man think beyond the confines of the curve” (p. 220).

In many cases discussion of the results also included abundant extracts from collected responses, with the sex and age of the individual in question clearly indicated for each (e.g., Ellis & Hall, 1896; Hall, 1883b, 1897a, 1899). Occasionally, nationality was also provided. In reporting his study of anger Hall (1899) indicated twice that excerpts were from Irish individuals (see p. 535, 573), while also indicating two individuals were English (see p. 531, 538). In two further instances responses were identified as from “colored” individuals, including “a colored deaf mute” (see Hall, 1899, pp. 535, 542). At the same time, only two extracts were explicitly identified as from Americans (see Hall, 1899, pp. 533, 567). Outside of these explicit identifications the implication was that included responses were from Americans.²⁶³ In the absence of sharing the complete collection of responses, these extracts serviced as evidence both of the former’s existence and as an indication of their quality. At the same time, instances explicitly identified as from particular racial groups served as markers of necessary psychological differences between groups.

Discussions of collected data, both that presented in tabular form and otherwise, further reinforced the existence of disparities between groups. In his initial study of children’s knowledge, Hall (1883b) highlighted gender differences, such as the proficiency of girls “in knowledge of parts of the body, home and family life, thunder, rainbows...” (p. 269) over their male counterparts. At the same time, children attending the Boston’s kindergartens were noted to be less ignorant than others (Hall, 1883b). Of the sole non-American nationality distinguished in

²⁶³ In his study of fears a number of responses were similarly identified as from English individuals, likely the material collected in England by Miss Hughes, principal of a training school in Cambridge (Hall, 1897a). Colored individuals were also identified in this study (see Hall, 1897a, pp. 163, 190) and in a study of dolls (see Ellis & Hall, 1896, p. 147).

the study, in fitting with widespread prejudice toward the Irish at the time especially in the investigation's Boston locale (Jacobson, 1999; Lee & Casey, 2006; Pick, 1993; Roediger, 1991), it was noted "the Irish children tested were behind others on nearly all topics" (Hall, 1883b, p. 269). Suspecting differences between rural and urban children, Hall also quizzed thirty-six country children for comparison purposes. The country reared children were characterized as possessing an "average intelligence" higher than city children (Hall, 1883b, p. 255), dovetailing nicely with Hall's contention that "city life is unnatural" (Hall, 1883b, p. 255). So unnatural was the urban experience

that those who grow up without knowing the country are defrauded of that without which childhood can never be complete or normal....A few days in the country at this age has raised the level of many a city child's intelligence more than a term or two of school training could do without it. (Hall, 1883b, p. 255)²⁶⁴

Differences like these were regularly identified in questionnaire-based research, which, unlike laboratory studies of a restricted number of individuals, set out to document the wide variability between not just individuals, but groups. Requests for information regarding specific forms of difference, and efforts to collect information from particular groups, presupposed disparities between various classes of individuals, ones questionnaires were ideally positioned to reveal. As Hall (1899) noted, this was "the great advantage of this method" (p. 530). As "the range of individual differences is vast and the fecundity of human nature in so diversifying" it was

²⁶⁴ Nature study, as a crucial component of educational practice, would later figure as part of pedagogical work at Clark (e.g., Hall, 1896b; Jewell, 1906a, 1906b). On nature study see Green (1995). In reprinting his study of the contents of children's minds a decade later, Hall included data collected from 678 children, including 47 colored children, in 1883 in Kansas City by Superintendent I. M. Greenwood. This material was presented alongside his earlier Boston findings, with a division made in the Kansas results between "White" and "Colored" students (see Hall, 1893c, pp. 18–20). The latter group was characterized as more ignorant on a number of points.

“absolutely necessary as the basis for any valid psychology” to “first of all to gather a vast array of facts and cases” (Hall, 1899, p. 530).

Conclusion

For a number of early American psychologists the naturalistic orientation that made questionnaire-based research possible was a complement to, rather than a replacement for, an experimental style of scientific thinking. Grounded in an emerging moral economy of data, questionnaire research positioned itself as scientific in large part as a function of its accumulation of masses of information. Münsterberg, adopting a very different and more restrictive understanding scientific practice in the discipline, highlighted the problem of relying on untrained observers to collect psychological information, an issue that attracted much discussion from others in the field. Hall was not blind to these difficulties, and in an effort to frame his questionnaire-based research as scientifically valid he invoked existing disciplinary standards of objectivity. Collection aimed to be mechanically objective, along the lines of the field’s idealized laboratory undertakings, while the analysis of material was restricted to trustworthy experts. Critiques of the method from those like James and Royce, who earlier employed questionnaires in their own research, reflect an ambivalence toward the method experienced by many of those who employed questionnaires, including Hall. Masses of easily obtained psychological data were appealing, but problematic.

Collecting data was challenging, but so too was managing the resultant material. As one critic noted, “if the questionnaire method is worth working at all, it is only as a bare preliminary, simply as pointing out salient points for research” (Stanley, 1898, p. 240), as truly synthetic findings were nearly impossible to achieve with great volumes of descriptive material. As in efforts to accumulate information the labour of organizing hordes of responses fell largely to

female contributors. Attempts to distill masses of individual narratives into some coherent whole often produced difficult to decipher numerical data. This transformation of accrued descriptive accounts into numerical values, if only in the form of raw frequencies and percentages, was a never fully successful attempt to capitalize on the increasing scientific authority of numbers. Inherently intricate descriptive data, the kind central to most questionnaire investigations, did not cede well to these aspirations. This, however, for Hall (1901) was not a state unique to work with syllabi

the literature of laboratory psychology abounds in studies based on simple enumeration, and that of child study is no better. In the one records or measurements, and in the other returns are made into tables or curves, which seem to be regarded as something holy and fetishistic. All this matter may be and often is extremely suggestive and valuable, but much of it consists of only the mud sills of possible superstructures The counters, photographers, and tabulators... seem to labor in the hope of a coming redeemer, who will gloriously fulfill and supplement their work, and by whom all its defects will be clothed upon. (p. 130)

As Hall's project progressed into the twentieth century, the larger psychological community increasingly ignored this kind of questionnaire-based research. Relegated ever more to pedagogy rather than psychology, questionnaires became something of an oddity particular to Clark investigators.

Chapter 5

Researching the Limits of Experience:

Data Collection and the Nature of Evidence

The rejection of non-expert human testimony is, and has ever been, the first step in the development of a science; it is only by rejecting or ignoring all testimony save that of experts that any science is possible. Human testimony, indeed, handicaps mankind....Not out of the mouths of two or three witnesses nor of two or three millions, unless they be experts, can any great fact in science be established. (Beard, 1879, p. 70)²⁶⁵

American neurologist George Beard's view on what constituted valid data when it came to investigations of spiritualism was a position the country's psychologists increasingly came to adopt over the final decades of the nineteenth and into the twentieth century. This, however, was not the position of many of the first scientific investigators into psychical matters. Many of these individuals were part of the first generation of American psychologists. Interested in mental phenomena, they set out to define the limits of such through their involvement in a formal organization, the American Society for Psychical Research (ASPR). The Society's investigations capitalized on the American public's interest in seemingly supernatural phenomena, enlisting their assistance in questionnaire-based endeavours rather than confining research to the "experts" Beard clearly privileged.²⁶⁶ As in child study investigations questionnaires were a prominent

²⁶⁵ On Beard and spiritualism see Brown (1983).

²⁶⁶ I focus here on some of the earliest of these investigations, those of the final decades of the nineteenth century, though questionnaire-based studies into psychical matters continued into the twentieth century (see Schiller, 1901; J. H. Hyslop to H. Münsterberg, 1 Jan. (n.y.), Folder 1825, Item 7, Hugo Münsterberg Papers, Boston Public Library, Boston, MA).

means of collecting information in the realm of psychical research, which like the child was a subject of popular captivation.

Undertaking psychical investigations, much like studies of child-life, was often predicated on enlisting into the scientific enterprise a diverse body of practitioners. This, as Francesca Bordogna (2008) has argued, was a core tenet of William James's vision of the scientific enterprise.²⁶⁷ For James, one of psychical research's most ardent advocates, science was to involve – at least in theory – not just educated, elite gentlemen, but rather a larger range of individuals from across the social spectrum. Although the laboratory was emblematic of the new psychology's status as a science, in practice, for James and others, science was to be a broader endeavour:

Startling as the fact might be to professionalizing scientists and educated gentlemen, knowledge could be produced outside of gentlemanly, scientific, academic, and even educated circles by groups of people who inhabited different social worlds. (Bordogna, 2008, p. 131)

Collecting facts on a massive scale, if only for mere practical reasons, necessitated an expanded social community of participants in the scientific process. Enlisting the public in the collection of masses of data, via the circulation of questionnaires, was one means of practicing a more distributed form of science.

This is not to say, however, that science in the hands of James or other psychical investigators was a fully democratic endeavour. Although enlisting a greater number of more diverse individuals into the scientific enterprise, these participants were still limited in what their participation might entail. The accumulation of data might involve a diverse body of individuals,

²⁶⁷ On James and psychology see Evans (1990) and Skrupskelis (1995).

but interpretation of this material remained in the hands of a select few, much as was done in child study (see Chapter 4). This practice, I argue in this chapter, was one means of countering distrust in data amassed by the untrained public. Even for those occupying the position of researcher, psychical or otherwise, the information generated via individuals questioned with circulars was almost always suspect. The data generating public was never fully trusted to provide truthful responses to questions posed via circular. This distrust was furthered by concerns about not simply outright deceit, but also that respondents may not properly understand the questions at hand and that, even if the questions were properly understood, these individuals lacked the necessary introspective skills to generate scientifically valuable information. Together these concerns ensured persistent criticism of the method across a variety of subject areas, though these attacks were particularly pervasive in the already tense space of debate over psychical research.

The American Society for Psychical Research

In the latter half of the nineteenth century, interest in the reality of so-called psychical phenomena was building in the United States. Popular public psychologies, including phrenology and mesmerism had attracted a significant following in the nation earlier in the century, while tales of the apparent supernatural powers of the Fox sisters of Upstate New York in the nation's newspapers fuelled the public's interest in extraordinary phenomena. The scientific community and the laypeople alike were well positioned for further work on the fringes of what would become scientific psychology (Coon, 1992a; Sokal, 2001).

In the face of such interest, in 1884, two years after the formation of a British Society for Psychical Research (SPR), an independent counterpart was established in the United States (“American Society for Psychical Research,” 1885, “Scientific intelligence,” 1885; James,

1892).²⁶⁸ The Society was in some respects short-lived, as by the end of the 1880s personal and financial difficulties led the ASPR to be subsumed as a branch of the SPR, before once again regaining its independence in the early twentieth century. Among the ASPR's founding members were not only James, but also a number of other early American psychologists and psychologically-inclined philosophers, including Hall, Jastrow, George Fullerton and James's Harvard colleague Royce ("American Society for Psychical Research," 1885, "Scientific intelligence," 1885; Coon, 1992a). The association of these individuals with the ASPR was a reflection of the Society's attempt to position itself as an organ of the new psychology. Speaking of scientific psychology's potential as an agent of social improvement, the organization's first report asserted,

there is so much to be done for the theoretical and practical needs of psychology, so much experimental research necessary for the formation of a science that may yet have vast influence upon the art of education, upon the treatment of the insane, and upon the policy of society toward criminals, that all experimental beginnings of such a science in any direction much be greeted with satisfaction. ("The first report of the American Society for Psychical Research," 1885, p. 156)

Claims of the group's scientific aims and its lofty expectations of psychology's social benefit, along lines similar to child study, were not sufficient to ensure the continued participation of many of the Society's first members. Outside of James, most of the Society's psychologically-inclined members severed ties with the organization shortly after its formation (Coon, 1992a).

Among the ASPR's first initiatives was the issue of a series of circulars, both to its members and the public at large. While some of these circulars were mere announcements, a

²⁶⁸ On the history of the British SPR see Oppenheim (1985).

number took the form of questionnaires (see *Proceedings of the American Society for Psychical Research*, 1885-1889), which requested that interested parties use the document as their guide to collecting data. Those interested in assisting with the Society's research endeavours were often instructed to report not simply their own experiences, but to gather further information from as many individuals as possible.²⁶⁹ Animating this research were serious concerns about how to conduct scientific investigations of some of the most nebulous phenomena. As articulated by the ASPR's first president, Johns Hopkins mathematician and astronomer – and sceptic – Simon Newcomb (see Moyer, 1992),

the first and greatest obstacle we meet with in such investigations is the absence of clear ideas of what it is we are to look for, and how we are to distinguish between real relations of cause and effect and mere chance coincidences. (Newcomb, 1884, p. 372)

Determining what constituted genuine psychical phenomena rather than “mere chance coincidence” would come to occupy much of the conversation between psychical researchers and their detractors.

The Committee on Thought-Transference

Like its British counterpart, some of the ASPR's earliest investigations were into matters of thought-transference. Several ASPR committees investigated thought-transference, or telepathy, in a number of ways.²⁷⁰ Taking the lead in these investigations was, unsurprisingly, the Society's Committee on Thought-Transference, whose work was inspired by that of French physiologist and psychical researcher Charles Richet (Committee on Thought-Transference, 1885; see

²⁶⁹ The British Society for Psychical Research similarly amassed huge quantities of material on various cases of psychical phenomena (Oppenheim, 1985).

²⁷⁰ The term “telepathy” was coined by British psychical researcher Frederic W. H. Myers in 1882 (see Hacking, 1988; Oppenheim, 1985).

Hacking, 1988).²⁷¹ Rather than collecting accounts of individuals' past experiences with the phenomena through a simple list of circulated questions, the committee requested that the public undertake their own experimentation ("Notes and News," 1885). Efforts to foster these kinds of engagements were an early form of citizen science, which sought to distribute the practice of science among members of the public (see Vetter, 2011). By distributing a series of circulars, they sought "to collect statistics as to experiments of uniform character, but made by a large number of observers" (Committee on Thought-Transference, 1885, p. 10). The first of these, *Circular No. 4*, included a strict set of instructions for making 3 types of experiments on thought-transference, using coloured cards, dice, and numbers, respectively. In the case of experiments on number telepathy, participants were instructed to

let one person, who may be designated as the agent, enter in the first column of one of the ruled squares the ten digits in any order taken at random. Let him then concentrate his attention on the first of these numbers; and let a second person, the percipient, who has been so placed that he could not see the figures, attempt to guess this number. The agent enters the figure guessed at the top of the second column, and then concentrates his attention on the second figure of the first column: this, in turn, is guessed and recorded.

(Committee on Thought-Transference, 1885, p. 15)

These kind of experiments were extended in *Circular No. 5*, which provided similar directions for making experiments with different sets of cards, as well as with free drawing (Thought-transference committee, 1885). The latter involved "experiments in which a drawing thought of by one person, is reproduced by another, who has no visible means of obtaining information as to what the drawing may be" ("Thought-transference in Boston," 1885, p. 9, see "Appendix D.

²⁷¹ Richet's later physiological work on anaphylaxis is discussed in Kroker (1999).

Thought-transference by means of pictures," 1885, p. 44). In all these experiments, a specific kind of experience was to be created *de novo* and recorded in the moment specifically for the Society's research purposes.

Like many of the ASPR's investigations emphasis was placed on obtaining information on both the occurrence and absence of phenomena. Instructions to participants specified, experiments giving either positive or negative results will be equally acceptable to the committee, because the object is to ascertain whether thought-transference occurs with many persons, and, if so, to what degree and in what proportion. To insure important results, it is desirable that from one to two thousand guesses be recorded for each agent and percipient. (Committee on Thought-Transference, 1885, p. 13)

The collection of both instances of apparent thought transference and the absence of such was central to the probability-focused science practiced by the Society, as was the collection of large quantities of data.²⁷² Asking for public participation this kind of labour intensive self-experimentation was an ambitious enterprise that ultimately resulted in limited returns (Committee on Thought-Transference, 1885), and thus a largely unsuccessful effort at citizen science.

The Committee on Experimental Psychology

The Committee on Experimental Psychology similarly issued circulars requesting information from the public on a variety of psychical fronts. Among the committee's work was the circulation of a brief questionnaire on superstitions, intended to ascertain the prevalence of these kinds of beliefs in the American public. This questionnaire, *Blank F* ("Blank F," 1887), consisted of just four questions:

²⁷² The results of the committee's initial investigation failed to find any persuasive evidence of thought-transference (J. M. Peirce & Pickering, 1885).

Should you be influenced by any feeling (whether implying a belief or not is immaterial) in regard to:

1. Sitting down thirteen at table?
2. Beginning a voyage on Friday?
3. Seeing the new moon over your left shoulder?
4. Choosing, on your own account, between two otherwise equally desirable houses, one of which was reputed to be haunted? ("Blank F," 1887, p. 269)

Those responding to the questionnaire were simply asked to respond "yes" or "no" to each item and to also include with their responses their name, address, occupation, age, and nationality.

The result of this inquiry was the finding that women were more prone to superstition than men: "about one man in ten and two women in ten, have a tendency to superstition, and that about four men in ten, and six women in ten, are inclined to pay some attention to a superstition actually encountered" (Minot, 1887, p. 223).

This relatively straightforward inquiry into the prevalence of superstitions was not the only such investigation undertaken by the committee. Toward the end of the decade, the committee distributed circulars in the form of post cards. Printed on these cards was the request, "please draw ten diagrams on this card, without receiving any suggestion from any other person, and add your name and address" (Minot, 1889, p. 302; see "Psychical research in America," 1887, p. 8). This brief directive spurred speculation in the press regarding the study's aim, the purpose of the Society's singular postal card request is not explained. We suppose the investigators desire to accumulate a mass of material in the way of data for generalization as to the commonest directions which human thought takes in graphic expression with pencil or pen. Or possibly there is a still deeper psychological intention. It may be that this

is a wholesale experiment in thought-transference. ("Psychical research in America," 1887, p. 8)

The committee received a total of 501 completed post cards in response to its inquiry. The cards were then sorted on the basis of those completed by men and those by women, and the diagrams analyzed according to the kinds of images drawn (e.g., circle, house, stars, trees, etc.), the kinds of lines used (e.g., straight vs. curved), and the kind of objects represented (i.e., natural vs. artificial; see Figure 2).

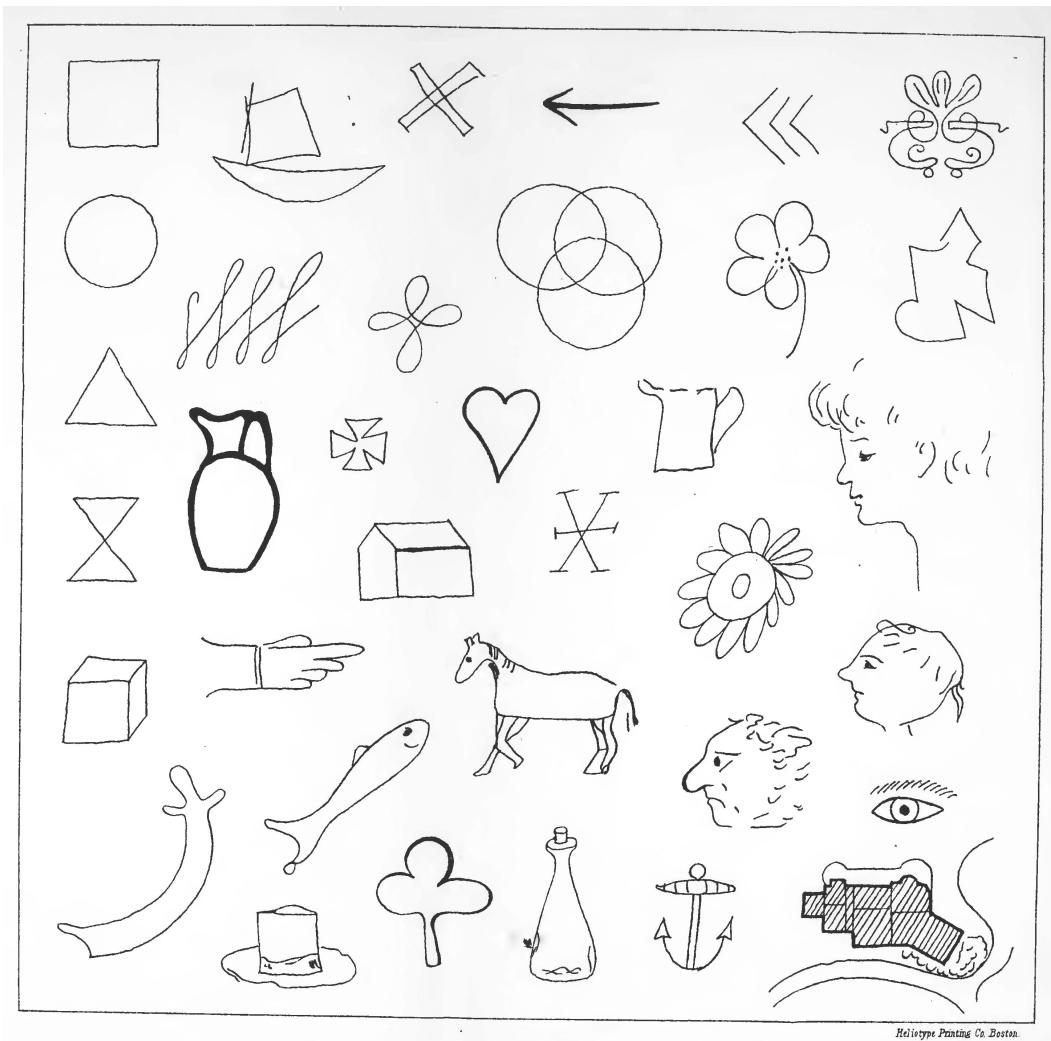


Figure 2. Reproduction of some of the most common types of drawings. Reprinted from Minot (1889, n.p.).

In the end the collected diagrams provided little indication of difference between individuals. Committee chairman, Harvard anatomist Charles Sedgwick Minot (1889), interpreted these results as evidence of “the slightness of our real individual distinction and separation” (p. 314). Rather than each respondent producing random, or at least individual, diagrams Minot (1889) observed,

it is evident that if two persons are requested to think of some one thing of a class, such as a letter of the alphabet, a playing-card, a baptismal name, there is by no means an equal chance of their selecting any one; on the contrary, there is not only the probability that they will think of a special one first, but there is a chance of their both thinking of the same one, for the relative frequency or preponderance of one idea or image out of a set has been shown to be similar for a number of people. In order to prove the reality of thought-transference, it must be demonstrated that the observed coincidence of thoughts can not be explained by the law of relative frequency. (p. 314)

The observation that individuals are likely drawing from a common pool of referents in studies like these had profound implications in Minot’s view.

If, in fact, individuals were more likely to respond to these kinds of requests in some ways rather than others, previous investigations into telepathy had been fatally flawed from the outset. The existence of these kinds of mental habits meant that these studies “having fundamentally misconceived the nature of the chances, of course fail to offer the necessary proof that the proportion of coincidences was greater than chance would account for” (Minot, 1889, p. 316). Given their failure to understand the true nature of chance, earlier studies of telepathy including as those undertaken by the Committee on Thought-Transference, as well as ones conducted by the Society’s British counterpart, were largely meaningless. For Minot (1889) “the general

conclusion is unavoidable that none of the experiments heretofore published afford conclusive evidence of thought-transference" (p. 317).

Minot's dismissal of previous evidence that seemed to support the existence of thought-transference was not the unanimous position of the Committee on Experimental Psychology. Although he understood the mental habits revealed in the committee's study of diagram drawing to be pervasive and determinate in psychical research, James disagreed. As a member of the committee James took it upon himself to respond publically to Minot's conclusions, which were offered in a report from the committee. Minot, James (1889) asserted "seems to me greatly to exaggerate the importance of this diagram-habit" (p. 317).

The Committee on Apparitions and Haunted Houses

Work undertaken by the ASPR's Committee on Apparitions and Haunted Houses fueled further debate on the veracity of psychical phenomena. Headed by James's Harvard colleague Josiah Royce, the committee distributed *Circular No. 6*, a questionnaire that inquired into individuals' experiences with the committee's titular interests (see Committee on Apparitions and Haunted Houses, 1886, pp. 130–1). The circulation of these circulars, in Royce's (1887) view, provided an opportunity to explore as yet uncharted territory,

but of the fantastic man, of the dreamer, of the man who lives a perfectly sane life in all but just some one or two realms of his mind, but who in those realms indulges in some sort of abnormal fancies, or is the helpless prey of some oppressive and diseased emotion or dream, of him we know in a scientific way far too little. Yet of such men the larger half of modern civilized humanity is probably made up. This wide and vast borderland region of human consciousness we need to study. (p. 229)

Declaring the committee free of any pre-existing beliefs with respect to psychical phenomena, he set out to gather a “mass of facts” (Royce, 1887, p. 228) on the basis of which hypotheses might then be formed.

The committee’s circular inquired into experiences with apparitions and haunted houses, whether personally experienced or accounts provided by others. Alongside this information, respondents were asked to provide information on certain personal characteristics, to whom and when did the experience in question occur? What was his (or her) age, nationality, and occupation; and what was his (or her) state of health or of mind at the time of the apparition? At what hour of the day did it appear, and at what place? (Committee on Apparitions and Haunted Houses, 1886, p. 130)

Other items on the circular specifically asked after apparent premonitory hallucinations.

Question 5, in part, read,

if the apparition seemed to give warning, or other knowledge, of any future or distant fact, did the narrator relate the incident to any one, or give notice of the warning conveyed, before he was able to verify the facts supposed to have been revealed?

(Committee on Apparitions and Haunted Houses, 1886, pp. 130–1)

It was inquiries into this subject matter that proved most fruitful for Royce and the committee.

Although much of the committee’s circular asked after experiences with ghosts and hauntings, the most engaging returns dealt only peripherally with these subjects. Information on haunted houses was particularly scarce. Rather “all our more interesting facts belong under quite another category...phantasms, visions, dreams, or presentiments” (Royce, 1887, p. 224).²⁷³

²⁷³ In the early 1890s Millicent Shinn forwarded on to Royce an account of a premonitory dream provided by an acquaintance (F. H. Longhead to M. Shinn, Feb. 13, 1890, Box 124 Folder 11, Papers of Josiah Royce, HUG 1755, Harvard University Archives, Cambridge, MA).

Given the nature of the material, Royce recommended changing the committee's name from the "Committee on Apparitions and Haunted Houses" to the "Committee on Apparitions and Presentiments" (Royce, 1887, p. 224).²⁷⁴ The change would also, Royce hoped, spare the committee the kind of public derision that the term "haunted houses" had attracted. Disdain was also demonstrated by scientific colleagues, including Newcomb (1884) who thought this kind of work

can hardly be regarded by lookers-on as anything better than very scientific children's ghost-stories. The extraordinary cases of events or accidents happening to one person being reproduced in the imaginations or visions of others at a distance, are nothing more than recitals of what we know, from the theory of probabilities, must be very frequent occurrences. (p. 373)

Despite criticism, through his work with the committee Royce became convinced, at least for a time, of the scientific value of questionnaire-based research later taking up this form of investigation in his work on imitation (see Chapter 4).

Over the course of his involvement with the committee Royce became particularly interested in presentiments, that is some feeling or impression of an event prior to, or at some distance from, its actual occurrence. Although initially open to the possibility that these stories were evidence of authentic phenomena, he came increasingly to argue that they instead documented the existence of what he termed pseudo-presentiments. As Royce (1889b) described these were "more or less instantaneous and irresistible hallucinations of memory, which make it seem to one that something which now excites or astonishes him has been prefigured in a recent dream, or in the form of some other warning" (p. 366; see also Münsterberg, 1899, p. 76). So far

²⁷⁴ In actuality the committee came to be renamed the Committee on Phantasms and Presentiments (Royce, 1889b).

as Royce (1888) was concerned these hallucinations of the sane, along with simple coincidence, were sufficient to explain much of the evidence in favour of telepathy:

that our so well-known hallucination of the ordinary double memory-consciousness appears almost exclusively among the sane, encourages me to suppose that this new form of double memory, once verified as an existing fact among the insane, may be found to be an incident of normal life sufficiently frequent to explain a large number of ‘telepathic’ incidents. (p. 247-8)

It was the successful documentation of the phenomena of pseudo-presentiments that Royce saw as the most promising result of the committee’s investigations. While refusing to rule out the existence of genuine telepathic phenomena completely he, like Minot, remained convinced that his studies revealed the contours of a phenomenon that provided an alternative, non-psychical, explanation for much of the previous evidence in favour of telepathy (Royce, 1889b).

Conclusions like these, however, were not the only ones reached by officers of the ASPR.

William James’s ASPR Investigations

A number of psychologists were early members of the ASPR, but most severed ties with the organization soon after its formation. The most notable exception was James.²⁷⁵ A central figure in the development of the new psychology in the United States, James was also a devoted believer in otherworldly phenomena, maintaining ties with both the British and American Societies for Psychical Research throughout his lifetime. In the final decades of the nineteenth and into the twentieth century James’s continuing advocacy of psychical matters as central to the new psychology attracted criticism from many of those within the former field. Tensions were especially high as psychologists sought to cultivate a distinct disciplinary identity, one in part

²⁷⁵ There is an extensive literature on James and psychical research (see Blum, 2006; Bordogna, 2008; James, 1986a; Knapp, 2003; Sech Junior, 2013; Sommer, 2012; Taylor, 1999).

crafted around opposition to the work of psychical investigators (Coon, 1992a). One source of contention was the style of research practiced in the psychical field. While criticisms of James's research methods, as well as his willingness to accept as valid the methods and conclusions of other researchers, may be understood as a not-so-veiled critique of the entire practice of psychical research, they may also be read as a split in allegiance to scientific method among psychologists.

Consciousness of Lost Limbs

James was himself both an early adopter, and a committed critic, of the questionnaire method (James, 1887b, 1890e). As such, in the 1880s James (1887b) constructed and circulated a questionnaire on the consciousness of lost limbs, in part motivated by his father's loss of a leg fairly early in life (G. E. Myers, 2001).²⁷⁶ This was not his first venture into questionnaire-based research (see Chapter 4), but it was his first research under the auspices of the ASPR. With the assistance of "some of the leading makers of artificial limbs" (James, 1887b, p. 249) he gained access to a population of amputees for his research project. Although he conducted interviews with some of these individuals, most of his information was gathered via the circulation of questionnaires.²⁷⁷ He began his questionnaire by informing respondents

I am engaged in scientifically studying the peculiarities of sensation experienced by amputated persons in their lost limbs. As the information I require can only be obtained by the statistical method of collecting and comparing a very large number of the facts in point, I trust you will not deem it too great a liberty if I beg you to communicate to me some details from your personal experience. (James, 1983a, p. 383)

²⁷⁶ What today might be termed phantom limbs.

²⁷⁷ The full questionnaire is reprinted in James (1983a).

In all he circulated eight hundred copies of his questionnaire on the consciousness of lost limbs and received responses from one hundred and eighty-five amputees.

It was James's hope that his inquiry would provide insight into why the psychological experiences of amputees varied. Even before beginning his research he recognized that not all amputees are conscious of their lost limbs, and that even among those who have these experiences, the characteristics of these experiences vary. Some experience their amputated limb in a fixed position, while in others the limb's position changes. For some, changes to the limb's position are the result of conscious effort, while others, despite their attempts, are utterly unable to alter the position of their removed limb. Ultimately, James (1887b) concluded from his research: "The results are disappointing, in that they fail to explain the causes of the enumerated differences. But they tell certain things and suggest reflections which I here set down for the use of future inquirers" (p. 249).

As Woodward (1983) has noted, James's work was a challenge to British associationism and German psychophysics. According to associationist theory, the amputee's continued experience of their lost limb was the result of previously established associations between bodily movement and incoming sensations. Yet, as James observed, not all amputees continued to experience their lost limbs. Invoking Darwinian evolutionary theory, James (1887b) speculated, phantasms of lost legs and arms are to the mental organism just what rudimentary organs are to the bodily organism. They have no longer any real relations with the environment, being mere vestiges of something which formerly had real relations. (p. 257)

Positioning phantom limbs as an evolutionarily neutral trait, James speculated that the tendency toward consciousness of lost limbs had simply failed to have been uniformly selected for or selected out by evolution. Consequently some individuals experienced their limbs after

amputation and others did not. In the process he positioned individual experiences of consciousness of lost limbs as normal and natural features of mental life.

An American Census of Hallucinations

Shortly after completing his inquiry into consciousness of lost limbs, James was charged with a much more ambitious project: a census of hallucinations. Unlike his study of lost limbs, the census was not an original endeavour. Rather, this undertaking was an extension of an earlier British effort to enumerate individual experiences with premonitory hallucinations. This work focused particularly on hallucinations classified as veridical; that is, ones that corresponded to events actually taking place some distance away, often an individual's death. Begun by psychical researcher Edmund Gurney (Gurney, Myers, & Podmore, 1886; see James, 1887a, 1895a; Sommer, 2012), the collection of cases sufficient to constitute a census was continued by the SPR following Gurney's premature death in 1888 (Oppenheim, 1985). As part of this effort – during the meeting of the International Congress of Experimental Psychology in Paris in the summer of 1889 – the census was extended beyond Britain to include several international endeavours, including an American census for which James was made the ASPR's designated agent.²⁷⁸

Although efforts at an official American census of hallucinations did not begin in earnest until the end of the 1880s, even prior to this the ASPR endeavoured to collect information on the subject. The investigations of Gurney and other SPR officers in this field, as documented in the

²⁷⁸ During the meeting the proposal to undertake a census met with some objection. James notes it was “a somewhat stormy sitting of the Committee on the census of hallucinations, M. Pierre Janet disbelieving in the utility of a popular inquiry, and being supported by others” (W. James to A. H. G. James, Aug 7, 1889 in James, 1998, p. 522). In addition to the America census, Italian, Swiss, German, and Brazilian surveys were agreed to, though at the next congress results were presented from only the latter two countries, along with findings from France and Russia (Blum, 2006).

volume *Phantasms of the Living* (Gurney et al., 1886), served as a model for this work.²⁷⁹ In October 1887 the ASPR's Committee on Experimental Psychology issued *Blank G* to collect accounts of cases where one person has had some remarkable experience, such as an exceptionally vivid and disturbing dream, or a strong waking impression amounting to a distinct hallucination, concerning another person at a distance, who was, at the time, passing through some crisis, such as death, or illness, or some other calamity. ("Blank G," 1887, p. 270)

Seeking to rule out "mere chance" ("Blank G," 1887, p. 270) as an explanation for portentous hallucinatory experiences, individuals were asked to collect responses to a series of eight questions from as many individuals as possible. Framed as simple yes or no questions, the circular simply required collectors to indicate an individual's response in one of the blank squares of the grid provided below each question. In this way the Committee hoped to ascertain

- (a) the number of persons in the community who have not had any such experiences at all; (b) the number of persons who have had such experiences coinciding with real events; (c) the number of persons who have had experiences which, though similar to the foregoing in other respects, did not coincide with real events. ("Blank G," 1887, p. 270)

Determining this required questioning a variety of individuals on the subject, not simply those likely to have experienced premonitory hallucinations. In this way, the Committee hoped to document not only the existence of these kind of psychical phenomena, but also the relative prevalence of such within the larger public. Given these aims, the necessity of obtaining a considerable number of responses was explicitly emphasized on the circular: "It is of the utmost importance to obtain answers from a very large number of persons, and it is hoped that many

²⁷⁹ The place of dreams in accounts gathered as part of *Phantasms of the Living* is explored in Groth and Lusty (2013).

thousands of replies will be received" ("Blank G," 1887, p. 271, see also "An investigation of dreams," 1887)

Although James was a member of the Committee on Experimental Psychology it is unclear to what degree he was involved with the Committee's aforementioned inquiry. Whatever his involvement in this work, James was clearly cognizant of larger efforts in this vein prior to undertaking the American census of hallucinations. Reviewing *Phantasms of the Living* in the pages of *Science* he commented on the authors' "untiring zeal in collecting facts, and patience in seeking to make them accurate" (James, 1887a, pp. 18–9). Little did he realize at this time, he would soon be charged with the same painstaking work.

As superintendent of the census for the ASPR, James was charged with gathering information on individual experiences of hallucinations, or the lack thereof, from as many individuals as possible. His efforts at large-scale data gathering involved soliciting assistance with the project in various popular and scientific publications, including, among others, the *New York Times* (James, 1890i), *Popular Science Monthly* (James, 1890a), and the *American Journal of Psychology* (James, 1890g, see also 1890b, 1890c, 1890d, 1890f, 1891; "Untitled," 1890, "Was it her ghost?," 1890). In these advertisements, he described the project's twofold purpose:

1st, to get a mass of facts about hallucinations which may serve as a basis for a scientific study of these phenomena; and 2d, to ascertain approximately the proportion of persons who have had such experiences. Until the average frequency of hallucinations in the community is known, it can never be decided whether the so-called "veridical" hallucinations (visions or other "warnings" of the death, etc., of people at a distance) which

are so frequently reported, are accidental coincidences or something more. (James, 1890d, p. 304, 1890g, p. 292)

Like many of the ASPR's previous inquiries, mass circulation of questions on the occurrence of hallucinations was to generate information on the prevalence of premonitory visions, and visions of what should be unknowable present states, amongst sane members of the general public (James, 1986b). Given the project's deliberate framing as a "census" of the phenomena, the necessity of amassing large – if not comprehensive – quantities of data was emphasized all the more.

To assess the frequency of hallucinations in the population, James's census asked individuals to response yes or no to a single question:

Have you ever, when completely awake, had a vivid impression of seeing or being touched by a living being or inanimate object, or of hearing a voice; which impression, so far as you could discover, was not due to any external physical cause? (James, 1890d, p. 304, 1890g, p. 292, 1986b, p. 58)

Much like the earlier ASPR inquiry on the subject, those interested in aiding with the census were sent a formally printed sheet, labeled "Schedule A," on which there was space to record the yes or no responses of twenty-five individuals.²⁸⁰ Demographic information for each individual, in the form of their name and address, sex, occupation, and age, was also requested (see James, 1986b, p. 58).

In seeking self-reported hallucinatory experiences, both familiarity with these experiences and the lack of such were important. Consequently, in the printed preface that

²⁸⁰ Given the project's ambitions of being a "census" the use of the term "Schedule" for each of its component parts was appropriate, as the same was regularly employed in governmental census undertakings (see Levitan, 2011).

accompanied “Schedule A,” those collecting information for the census were reminded, “the answer “No” and the answer “Yes” are equally important” (James, 1986b, pp. 56–7). The recording of both instances and absences of hallucinatory experiences were crucial to obtaining an accurate estimate of the prevalence of these experiences within the general public. Thus, it was not just the self-reported details of these experiences that were important, but also the relative frequency with which these types of experiences occurred. Members of the public, charged with undertaking much of the data collection for the project, were reminded of the necessity of collecting representative data.

Individuals who reported having experienced hallucinations, and thus answered “yes” to the question on “Schedule A,” were then asked to complete a longer questionnaire on the nature of their experiences. This form, labeled “Schedule B,” consisted of six questions on the circumstances related to the hallucinatory experience. For example,

1. Please state what you saw or heard or felt, and give the place, date and hour of the experience as nearly as you can.
2. How were you occupied at the time, and were you out of health or in grief or anxiety?
3. Was the impression that of some one whom you were in the habit of seeing, and do you know what he or she was doing at the time? (James, 1986b, p. 58)²⁸¹

As was generally the case with questionnaires during this period, all the questions on “Schedule B” were open-ended. Individuals were simply to report, in as much detail as possible, their personal experiences with hallucinations. Taken together, these collected accounts of

²⁸¹ A copy of the circular produced for the American census, including Schedules A and B, can be found in the Josiah Royce Papers (“International Congress of Psychology: Instructions to the Persons Undertaking to Collect Answers to the Questions on the Other Side,” Box 106, Folder 8, Papers of Josiah Royce, HUG 1755, Harvard University Archives, Cambridge, MA).

hallucinatory experiences were to provide a complete picture of the nature of hallucinations and the frequency of veridical, or premonitory, hallucinations in the general public.

Collecting Data and Interrogating Evidence

The entire questionnaire enterprise was predicated on the apparent value of masses of collected material. In the context of psychical research, the accumulation of this kind of information was intended to reveal the true nature of extraordinary phenomena. Were these occurrences all simply the result of some confluence of superstition and chance? Or, were there among these accounts authentic psychical happenings? Surveying the public concerning their experiences with the seemingly inexplicable held the promise of resolving these kinds of questions once and for all, but only once the validity of each case was interrogated. With this method “by taking the experiences of these persons as they come and applying the ordinary methods of science” psychical investigators could produce an “empirical natural history worthy of all encouragement and respect” (James, 1892, p. 741). Like other questionnaire-based inquiries, these projects were of an explicitly natural historical orientation, seeking to collect, categorize, and analysis the amassed body of data. When these kinds of studies were still in progress, for psychical sympathizers like James, scepticism was “a very dangerous and unscientific attitude. Where observations are in process of accumulation, and one doubts them, the best thing is to wait.... if the observations multiply, all such objections fall to the ground.”²⁸²

Other psychical researchers outlined the advantages of large quantities of material in more specific terms. Royce (1889b) argued that “the collecting of stories is not idle play” (p. 427), but rather that it produces “a collection of specimens of our material, — a little cabinet of

²⁸² W. James to G. Stanley Hall, Nov. 5, 1887 in James (1998, p. 283).

curiosities, if you will” (p. 352) suitable to the comparative method.²⁸³ In his view, the analysis of such a collection centred on classification and, unlike singular cases, was not dependent on the authorial intentions of individual correspondents. Instead, by focusing on the common characteristics across many accounts, researchers could analyze “the unconscious testimony, so to speak, of all the many persons” (Royce, 1889b, p. 351). Avoiding the particular limitations of the case study method, questionnaire research held the promise of “throw[ing] light on the dark things of mental life” (Royce, 1889b, p. 427).

Enlisting the Public

Across the various circular-based investigations undertaken by officers of the ASPR, public participation was vital.²⁸⁴ The success of each of these projects was predicated on the involvement of a large body of individuals in the data collection process. Consequently, researchers appealed “to the public for co-operation in the work of investigating that which is hidden, not in desert islands, or in glaciers, or in craters, or in crucibles, or in cuneiform inscriptions, but in human experience” (Phelps, 1885, pp. 254–5). Only through this participation could a significant enough mass of material be collected to warrant analysis and thus generate meaningful conclusions. This necessitated “flood[ing] the English-reading world with little circulars asking for authentic cases of mind-reading, or visions, as reported at first-hand by reporters willing to be personally investigated” (Phelps, 1885, p. 255). In the case of James’s census work this meant not only that the public would be required to provide first hand accounts of their experiences, but also that to accumulate this body of information “very many volunteer canvassers” were necessary “to secure success” (James, 1890b, p. 485).

²⁸³ On the role of curiosities and wonders in science see Daston and Park (1998).

²⁸⁴ For discussion of Francis Galton’s Anthropometric Laboratory at the International Health Exhibition in London 1884 as a “mechanically objective” form of public participation see Lundgren (2013).

Attracting not only the public's interest, but also their active involvement in these enterprises was a challenge. Early attempts to do so were not nearly as successful as the Society would have liked ("Request for cooperation," 1887). Several years later, however, James's active promotion of the census in the press was markedly successful, even if it fell far short of the census's goal of 50,000 replies (see James, 1890b). Requests for questionnaires were described as "pouring in at a great rate in consequence of my circular letter to the newspapers."²⁸⁵ As the more than seven thousand responses James eventually accumulated for the census of hallucinations testifies, reluctance on the part of the public to engage in these projects was surmountable. This involvement, in James's view, was crucial, for if the Society could become sufficiently well known to the public so as to attract reports of all phenomena related to the supernatural, its members could then thoroughly investigate these accounts and "we should end ere long by having a mass of facts concrete enough to found a decent theory upon" (James, 1892, p. 729).

In order to secure the participation of the public, the ASPR's projects were regularly featured in both scientific periodicals and the nation's newspapers. The presentation of the Society's work, however, varied considerably (e.g., "Notes and News," 1885, "Psychical research in America," 1887). In some accounts the organization's efforts were soundly dismissed,

The Society for Psychical Research can spend a century and millions of dollars in attempting to determine a principle relating to the dreams and visions and hallucinations, and will then be as involved in doubt as now. The "research" to be attempted is useless, a waste of time, and can profit no one. ("A useless inquiry," 1888, p. 5)

²⁸⁵ W. James to A. H. G. James, May 17 [May 16, 1890] in James (1999, p. 32).

On the other hand, some of the press on these projects was astoundingly positive, as in a report in New York City's *The Sun*:

They are exploring a field which men of science, as a rule, have been too reluctant to approach. The field is not the less interesting and important on that account, particularly to those who like their science with a dash of mystery in it. It is of great importance to settle definitely, one way or the other, the phenomena which gain so much by popular exaggeration and irresponsible narration; and to test the truth of those highly interesting superstitions which are the heirlooms of the centuries. ("Psychical research in America," 1887, p. 8)

Even in largely favourable accounts, the press was not above poking fun at aspects of the Society's work,

The American Society for Psychical Research has sent out a circular requesting that any person having some unusual experience, such as an exceptionally vivid and disturbing dream, or a strong waking impression, amounting to a distinct hallucination, concerning another person at a distance, make a statement of that fact promptly to the society.... The society might as well employ four-score secretaries and readers of letters at once. If a tenth of the people who have had some such experiences comply with the request the office of the society will be inadequate to hold the letters, spacious as it is. ("A useless inquiry," 1888, p. 5)

This comment was particularly on point given the challenge of corralling masses of data into a meaningful whole and the difficulties James in particular would face compiling the census of hallucinations.

The press's help and hindrance to the psychical cause was well recognized by the ASPR's members. Royce (1889a) commented on the press's positive role in the Society's efforts, "the newspaper press of the country ... at the cost of no small labor, has courteously helped us so much in our researches," before going on to remark on their equally detrimental habit of "reporting from time to time all sorts of marvels as having been vouched for by this or that officer or committee of the Society" (p. 517). This latter observation was tempered somewhat by his characterization of these reports as the press's "certain doubtless good-humored revenge" (Royce, 1889a, p. 517).

Although neither a uniformly positive nor negative force for the ASPR, the press was decidedly instrumental in ensuring the success of psychical research projects. In providing the organization with a venue to publicize their efforts, and interest readers in becoming involved in this work, the press was invaluable. The general request that the public provide the Society with information was a prominent feature of these accounts,

The society, by means of circulars, is collecting accounts of cases where persons have had some remarkable experience in the realm of phantasms, such as hallucinations, premonitions, dreams that have been the forerunners of actual occurrences, visits from ghosts, etc. All who have had such experiences are requested to inform the psychical inquirers of Boston. ("Psychical research," 1887, p. 4)

Other reports of the Society's work took care to underscore the scientific character of the research. Of the census of hallucinations Washington, DC's *Evening Star* emphasized, "There is nothing weird or fantastic or sensational in this investigation. It is purely scientific... The volunteer canvass undertaken in each vicinity by persons of an inquiring mind will aid materially

a high scientific object" ("Untitled," 1890, p. 4). Of the ASPR more generally the *Pittsburgh Dispatch* observed,

A deeper scientific interest is manifest. The members generally realize that patience and time are required in this department of research as well as in other branches of science, in order to secure satisfactory results. We must mass together a great number of cases before being able to draw trustworthy deductions from them, but these deductions are sure to follow, I am confident, sooner or later. ("The ghost hunters," 1890, p. 9)

In these reports the authority of science was invoked in an attempt to attract the necessary volume of volunteer canvassers for projects.

In some instances newspapers and other periodicals went so far as to reproduce the Society's circular questions verbatim, especially – though not solely – in the case of the census of hallucinations (James, 1890a, 1890b, 1890c, 1890d, 1890f, 1890g, 1890i, 1891; "Psychical research in America," 1887, "Was it her ghost?," 1890). The ability to reproduce the substance of questionnaires in this way is a unique facet of these psychological instruments, one that served psychical researchers well. This ease of reproduction made questionnaires an incredibly mobile technology, ones able to traverse boundaries, geographic and otherwise, in ways largely impossible with other instruments.

The mobility of questionnaires allowed anyone to use them for data collection. Given the reliance on untrained collectors, providing specific instructions to those taking on this role was crucial. Short of training in what investigators considered proper research protocol, directives to volunteers served as a bulwark against poor quality information, much as they did in Hall's child study efforts (see Chapter 3). Such instructions had to encompass both deferential and directive tones. Researchers at once sought to convince individuals to participate in the research

endeavour and to steer the shape of this involvement. In this vein, circular instructions included the entreaties such as “we therefore beg any reader of this circular [to collect data]” (“Blank G,” 1887, p. 270) and “I most earnestly bespeak the co-operation of any among your readers” (James, 1890b, p. 485). These appeals were followed by strict directives; for instance, readers were told to “repeat the following questions, verbatim, to as many trustworthy persons as possible, from whom he does not know which answer to expect, and who have not already been interrogated by some one else, and communicate the results” (“Blank G,” 1887, p. 270). The census of hallucinations was particularly detailed in this respect. The census circular informed volunteer canvassers

it is important that the question should be very widely asked and of all sorts of people – not only of those who are thought likely to have had such an experience or of those who are thought likely not to have had it. (James, 1986b, p. 56)

Further, canvassers were instructed not to question anyone who was known to have previously experienced insanity, as the circular was “not intended to include the hallucinations experienced in delirium” (James, 1986b, p. 57). Also to be excluded from the material were any second-hand accounts of the presence or absence of hallucinatory experiences, as well as answers from individuals who had already participated in similar inquiries.²⁸⁶ Volunteers were only to question for the census those 21 years of age and over, but were to ensure that these individuals’ responses covered the entire span of their lives. Edicts to collectors were not limited to the printed page. Ending his instructions, James (1986b) informed interested collectors “I shall be happy to give any further information that may seem to you necessary” (p. 57).

²⁸⁶ This may have been an attempt to exclude not only those who were previously questioned as part of the census, but also those who participated in the previous inquiry into the subject undertaken by the Committee on Experimental Psychology (see “Blank G,” 1887).

The Will of Contributors

While James and other investigators took care to direct the activities of collectors and respondents, nothing could ensure their participation in the first place. Prior to undertaking the American census Gurney warned James of some the difficulties inherent in this work:

the great difficulty the whole business has to contend against is not so much contempt as indifference. One's material being human beings, with wills of their own, one is continually [*sic*] baffled by the fact that they have no vision of the subject as a whole, or as a subject at all, & therefore cannot be brought to tender their item of help.²⁸⁷

Following the appearance of *Phantasms of the Living* (Gurney et al., 1886) James hoped this kind of resistance would diminish. Gurney was far less optimistic warning “I do not think it by any means certain, as you seem to expect, that evidence will now flow in more easily. I am not sure that the collection of facts will not be harder.”²⁸⁸ The prospect of collected accounts appearing in print was just as likely to deter individuals from participating in the project as encourage it. Acquiescence to researchers calls for contributors was no means guaranteed.

Convincing masses of individuals to contribute to studies of psychical phenomena was only the first challenge with such participation. Although researchers hoped both those collecting data and individual respondents would follow the instructions provided with circulars, this was by no means assured. For instances, in Minot’s postcard circular, most participants produced 10 diagrams as requested, but some disregarded this portion of the instructions to include more or fewer diagrams as they saw fit (Minot, 1889). James complained of his efforts to substantiate claims submitted as part of the census of hallucinations: “Our correspondents obstinately refused

²⁸⁷ E. Gurney to W. James, Jan. 16, 1887 in James (1998, p. 190).

²⁸⁸ E. Gurney to W. James, Jan. 16, 1887 in James (1998, p. 190).

to reply in a great many cases.”²⁸⁹ In these, as in other investigations, the participant-public was not a passive, compliant source to be mined, but rather asserted itself in ways often unsolicited – and undesired – by researchers.

Requests for Forms & Clerical Assistance

With the census of hallucinations James faced an influx of requests for “census-blanks” from individuals willing to take up the work of data collection.²⁹⁰ In this kind of a project, as James noted “the labor is enormous.”²⁹¹ Recognizing this fact in the midst of his census efforts, he sought to distribute the burden. This included not only the very necessary recruiting of members of the public into the collection process, but also delegating to some of his family members the tedious business of supplying this public with blank census forms. To deal with abundant requests for forms, he enlisted the assistance of both his wife Alice Howe Gibbens James and her sister Margaret Merrill Gibbens (later Margaret Gibbens Gregor).²⁹² Female familial involvements of this kind were relatively common in nineteenth century science. While women were largely excluded from professional scientific spheres, many were involved in scientific practice as it took place within households, though this work went largely unrecognized (see Lindsay, 1998).

²⁸⁹ W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 76).

²⁹⁰ See W. James to A. H. G. James, May 17 [May 16, 1890] in James (1999, p. 32).

²⁹¹ W. James to E. M. Sidgwick, May 15, 1892 in James (1999, pp. 269–70).

²⁹² See W. James to A. H. G. James, May 17 [May 16, 1890] in James (1999, p. 32); W. James to A. H. G. James, July 7, 1890 in James (1999, p. 51); W. James to A. H. G. James,, July 9, 1890 in James (1999, p. 55); W. James to A. H. G. James, July 21, 1890 in James (1999, p. 66); W. James to A.H. G. James, Aug. 3, 1890 in James (1999, p. 83). Margaret Gibbens, her sister Mary Sherwin (later Salter) and her mother, the widowed Elizabeth Putnam Webb Gibbens, all lived with the James for a time. Mrs. Gibbens was the one to introduce James to his “white crow” medium Leonora Piper and Margaret Gibbens also visited the medium (see James, 1986a, p. 400; see also Blum, 2006).

Over the course of the spring and summer of 1890, discussion of the labour associated with census work was a regular part of the correspondence between husband and wife. In these exchanges James regularly expressed his appreciation for the women's assistance, especially his sister-in-law who seems to have taken on much of the work: "found here no proofs, but a lot of hallucination letters, most of which I enclose for Margaret's noble treatment";²⁹³ and in another instance noting, "I will forward to her [Margaret] the applications after this, thanking her wondrously in advance. She too is a white souled being!"²⁹⁴ Their aid spared James much of the drudgery that attended such an ambitious project: "Margaret is addressing my census-blanks...It is a great relief to me and she doesn't seem to dislike it."²⁹⁵ Much as the direct data collection portion of census work necessitated an expansion of the scientific sphere so as to include members of the public, the administrative portion of the project blurred domestic and professional boundaries.

Despite the assistance James received with the census correspondence, he was still overwhelmed with the volume of requests. Simply keeping up with requests for forms, or as he termed them "hall.-blanks," was a challenge: "I hope you have been at the work of sending hall.-blanks to the applicants whose letters were in the big Epicure envelope in my valise. They're getting fearfully into arrears. I send a few more with this."²⁹⁶ To manage this influx, outside of the aid received from family members, James also relied on the ASPR's only paid employee, the

²⁹³ W. James to A. H. G. James, July 7, 1890 in James (1999, p. 51).

²⁹⁴ W. James to A. H. G. James, July 9, 1890 in James (1999, p. 55).

²⁹⁵ W. James to A. H. G. James, May 17 [May 16, 1890] in James (1999, p. 32).

²⁹⁶ W. James to A. H. G. James, July 21, 1890 in James (1999, p. 66).

society's Secretary Richard Hodgson.²⁹⁷ Hodgson's assistance was also crucial in other circular-based projects initiated by the society, as Royce (1889b) recognized,

Nearly the whole work of collecting our facts, and of corresponding with our friends and with our other informants, has fallen upon the shoulders of the one paid officer of the society, our able and devoted secretary, Mr. Hodgson, and our committee alone has employed nearly all of his time, as well as the time of his clerical aid. (p. 426)

Without the clerical services of Alice James, Gibbens, and Hodgson the extensive data gathering projects engaged in by the Society would have failed from the outset. Their work, though often unrecognized, especially in the case of the former two women, was instrumental to the success of projects predicated on securing a distributed network of contributors.

Census Correspondence

Of course, correspondence with data collectors was not limited to simple requests for blank census forms. Although published requests for assistance were perhaps the most productive means of ascertaining accounts of psychical phenomena, efforts to obtain information were also undertaken via more personal interactions. This includes both general word of mouth regarding research projects, but also personal communication between psychical investigators and potential data collectors, as well as individuals with relevant psychical experiences. James's census related correspondence, in particular, took both forms. Communication with respondents allowed him to thank them for their contributions, and also further direct their work. Writing to incipient psychologist Mary Whiton Calkins, James thanked her for the three Schedule Bs she had sent, while also inquiring after the absent Schedule A. He expressed the "hope that the A-one

²⁹⁷ See W. James to A. H. G. James, July 7, 1890 in James (1999, p. 51); W. James to A. H. G. James, May 22, 1890 in James (1999, p. 35).

will turn up in due time, filled.”²⁹⁸ Smith College Philosopher Harry Norman Gardiner, one of Calkins’s former instructors (Furumoto, 1979), also offered his assistance, inquiring as to a set of schedules for collecting information. James sent the schedules as requested, but also took the opportunity to provide Gardiner with further instruction on how best to go about collecting and recording information, including the directive: “the best rule is to send an account of all such doubtful experiences on Schedule B, but not to put them down on A as either yes or no.”²⁹⁹ Seeking as much assistance with the project as possible, he closed his letter to Gardiner with the request: “if you know of anyone else who can help me, I should be thankful for his address.”

Correspondence with census respondents also provided James with the opportunity to enquire in more detail into the nature of recorded experiences. When Katharine Peabody Loring, companion of his sister Alice James, provided him with a story of an hallucination, he sent her the *pro forma* Schedule B but also questioned her further on her experience, guiding her completion of the document: “you say in your own case: ‘I was conscious of the presence.’ Was this presence a distinctly exteriorized vision, or an ‘impression,’ or what? And was the voice an external voice? The distinction is rather important.”³⁰⁰ Distrust of individual, uncorroborated accounts of psychical events pervaded these kinds of investigations. Informed of an apparition experience in the years immediately preceding the census, James sought corroboration, or at least another opinion of the event in question, from the subject’s father, Shakespearean scholar Horace Howard Furness: “Is it too great a liberty for me to ask whether you have your self any distinct

²⁹⁸ W. James to M. W. Calkins, July 30, 1890 in James (1999, p. 75).

²⁹⁹ W. James to H. N. Gardiner, Mch 26, 1890 in James (1999, p. 15).

³⁰⁰ W. James to K. P. Loring, May 7, 1890 in James (1999, p. 23).

opinion about these facts.”³⁰¹ Individual accounts in and of themselves were not to be trusted, but if further confirmation of their occurrence could be secured these stories were deemed more plausible.

Census Labour: From James to Hodgson

Outside of engaging with individuals with whom he was personally acquainted, most submitted census-related material did not prompt a response. Instead, as he was inundated with census letters throughout the spring and summer of 1890, James simply allowed the census responses to accumulate. As this mass of material attests, the public, or at least some segment thereof, was more than willing to offer up their confessionals to the authoritative expert-stranger.³⁰² Although he made mention of efforts to tackle the growing expanse of material he received, “I have been trying to clear away my table of the enormous mass of unanswered letters (hallucinatory & other) which have accumulated on it, but have been able to make no headway whatever on acc! of the incessant interruptions,” little progress was made on the chore.³⁰³ James was simply unprepared for the realities of mass social investigation and the work attendant these kinds of inquiries.

This lack of sustained engagement with the material as it appeared proved a poor strategy. James later recognized, as he sought to produce a report on the American census for the

³⁰¹ W. James to H. H. Furness, Mch. 21, 1886 in James (1998, pp. 120–1). The Furness family was a great help in James’s psychical work, as he “mesmerized” another son on two occasions (see W. James to H. H. Furness, Mch. 21, 1886 in James (1998, pp. 120–1).

³⁰² The confessional aspect of the public’s engagement in these kinds of endeavours, unlike child study where questionnaire data were more often the result of observation (see Chapter 3), is noteworthy. This kind of confessional is not Foucauldian (Foucault, 1978) per se – whereby confession has been constructed as Western society’s route to liberatory individual truth – as *individual* truths and their therapeutic character were not the intent, nor so far as can be discerned the result, of these endeavours. Nonetheless, the willingness of individuals to engage in confession aligns with Foucault’s diagnosis of confession as one of the West’s defining features.

³⁰³ W. James to A. H. G. James, May 25, 1890 in James (1999, p. 39).

International Congress of Psychology to be held in London in the summer of 1892, that his lack of dedicated work on the project was a grave error. In advance of the meeting, as census submissions continued to pour in, James informed one of the coordinators of the international effort, British psychical researcher, Eleanor Sidgwick

My mistake was in not “keeping up” daily, or at least weekly, with the correspondence involved in all the schedules I received. In the midst of my other occupations, I was irregular, and let them go, keeping no systematic account of the correspondence, until now I fear the matter is irreparable.³⁰⁴

About to leave for a yearlong sabbatical in Europe, James abandoned his efforts to produce a report and instead handed the material over to Hodgson.

James did undertake the daunting challenge of organizing the bulk of the material, but he left the final analysis of such in Hodgson’s hands. Prior to transferring the material from his home to the Society’s office, and thus into Hodgson’s care, he ensured most of “the cases are analytically indexed, so that the work of classification will be easy.”³⁰⁵ Hodgson was informed, however, that along with these organized accounts there were “two boxes of unana[lyzed] cases—at least I think most of them are—and an index book of old American S.P.R. cases with analysis (mostly dreams), plus an envelope with some other cases.”³⁰⁶ There was also, among this material, “some unintelligible [sic] schedules A., which you will understand.”³⁰⁷ With these James included his “hallucination-book,” which served as an analytical index to the material. This was not solely the product of his work, but rather, as James explained, “the first couple of hundred cases of the analytic index were prepared by my student - - - Wood – the rest by myself,

³⁰⁴ W. James to E. M. Sidgwick, May 15, 1892 in James (1999, pp. 269–70).

³⁰⁵ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³⁰⁶ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³⁰⁷ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

mostly dictated to Mr. J. W Alger by me, as I read. He did a few alone.”³⁰⁸ Just as James could not conceivably have undertaken the work of collecting information for the census alone, the project of sorting this data was not feasible for only one individual. In addition to Hodgson, James found it necessary to enlist others in the effort.

Boxes of raw material and James’s analytical index were to guide Hodgson’s completion of the report on the American census. To further aid this effort, James sent along a “skeleton-paper” from Eleanor Sidgwick, which outlined her method of analyzing the British census results.³⁰⁹ Using this as a template, James informed Hodgson that if he could analyze the census material “so as to fill in the blanks in time to send her the paper by the July congress – ‘twould be well!” though he warned “Heaven help you anyhow: You’ll be troubled with duplicates and ambiguities enough.”³¹⁰ In the end, James was dissatisfied with the entire endeavour, concluding our census has been a terrible slouchy piece of work...I didn’t clearly foresee the exact line of argument at first; mistrusted the value of the whole thing and let the correspondence get into arrears, in spite of the weeks and weeks of time I gave to it. The result is a mass of uncorroborated stuff...I don’t advise any other country to try to take up the job. Irreproachable results would require too enormous an amount of labor.³¹¹

Perhaps unsurprisingly this was James’s final foray into questionnaire-based research.

Seeking Confirmation

As James recognized, the fact that circulars produced “a mass of uncorroborated stuff” was problematic. To address this, in the census and other similar projects, investigators

³⁰⁸ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³⁰⁹ See W. James to E. M. Sidgwick, May 15, 1892 in James (1999, pp. 269–70); W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³¹⁰ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³¹¹ W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 76).

deliberately set out to corroborate individual accounts of psychical phenomena. Doing so, however, was difficult. Not only did some respondents refuse to respond to further questioning, but accruing other forms of documentary evidence was an arduous undertaking.³¹² Simply organizing and classifying the many accounts the public provided overwhelmed James. Further inquiries were beyond him, especially considering the example set by Gurney, who informed James

it must be remembered also that the witnesses did not know—I doubt if more than a fraction of them even for a moment guessed—that registers & libraries &c &c were going to be or had been searched; & in correspondence I have used a great many little precautions & dodges with a view to detecting weak spots, & of avoiding leading questions.³¹³

In this vein, James (1895a) later praised the British census workers for “the energy and skill with which …[they] executed their burdensome task” recognizing that their census responders were “corresponded with or interviewed or in other ways subjected to as critical a scrutiny as circumstances allowed” (p. 69). This kind of investigative work – required to verify every account of a positive hallucinatory experience – was laborious to say the least and an already overwhelmed James proved ill prepared for the task.

While the census provided the biggest challenge with respect to verifying accounts, simply as a function of its sheer scope, other projects faced similar difficulties. Across the ASPR’s circular research endeavours efforts were made to secure confirmation, of one form or another, of psychical phenomena. In some instances, assurances of the plausibility of an account

³¹² See W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 76).

³¹³ E. Gurney to W. James, Jan. 20, 1887 in James (1998, p. 195).

were sought from those acquainted with an individual.³¹⁴ The Society's pre-census hallucination inquiry requested of those reporting hallucinatory experiences: "if the experience has been coincident with a real event, we specially request the percipient to send us an account of it" ("Blank G," 1887, p. 271). Ensuring the reality of the event in question then fell to the Committee. The attendant request "we desire to receive also (not for publication) the name and address of any person who answers yes" ("Blank G," 1887, p. 271), was a necessary part of the process of verification, providing investigators within enough information so that they might seek substantiation of an account from other sources.

Underlying these attempts at ascertaining confirmatory evidence was a basic distrust of individual stories. It was not so much outright fraud that concerned investigators, as simple errors of memory. As Royce (1887) recognized,

it is a very natural result of our public request for facts that the most of the cases which have been submitted to us by our correspondents are narratives dependent for their verification not so much upon documents as upon the memory of certain persons. (p. 224-5)

Because memory was such a fallible source of information, documents were often sought to substantiate accounts: "an authentic letter or diary in the hand is worth not only two but twenty remembered facts in the bush" (Royce, 1887, p. 225). Thus, an individual recalling a veridical hallucination would be well served to include with their proffered account some form of documentation from that same period of time confirming the hallucinatory experience.

³¹⁴ See, for example, W. James to H. H. Furness, Mch. 21, 1886 in James (1998, pp. 120-1).

The Sway of First Hand Testimony and the Distrust of Data

The distrust of disembodied accounts was pervasive. At the same time, first hand testimony held a powerful sway. James was informed of the appeal of personal accounts by Gurney, “I cannot describe to you the effect on my own mind which my hundreds of personal interviews have had.”³¹⁵ After he conducted his own census he admitted

I find however that narratives are a weariness, and I must confess that the reading of narratives for which I have no personal responsibility is almost intolerable to me. Those that come to me at first hand, incidentally to the Census, I get interested in. Others much less so; and I imagine my case is a very common case.³¹⁶

Individual narratives, told in person, were both far more interesting and convincing than masses of anonymous information. Better still were reports given by friends, or one’s own first hand experience with a phenomenon,

a single veridical hallucination experienced by one’s self or by some friend who tells one all the circumstances has more influence over the mind than the largest calculated numerical probability either for or against. I can testify to this from direct observation.

(James, 1895a, p. 74)

James and others found testimony persuasive, but these kind of first hand accounts were inaccessible to most. In terms of practicality, it was impossible for stories of psychical experiences to be communicated, in person, to everyone with an interest in such matters.

Distrust of the information gathered via circular extended beyond individual cases, to encompass the full expanse of collected data. The copious accounts collected on behalf of the ASPR were stored with Hodgson, and the press complained at points that the Society

³¹⁵ E. Gurney to W. James, Jan. 20, 1887 in James (1998, p. 194).

³¹⁶ W. James to F. W. H. Myers, Jan. 30, 1891 in James (1999, p. 139).

guards its gathered materials with great secrecy. Its rich fund of facts is not published until they have been passed upon and thoroughly examined by the various committees; even then the names of those who contribute their experiences are in no case furnished to the public. ("Our weird visitors," 1888, p. 16)

Though it was certainly true that most stories of psychical experiences were divorced of identifiers, the material itself was not necessarily as inaccessible as claimed. The Committee on Thought-Transference explicitly specified "the original papers sent in to us are preserved, and can be seen by any member or associate of the society, on application to the Secretary" (J. M. Peirce & Pickering, 1885, p. 17). For some at least, rendering the entirety of collected information accessible was a mark of its legitimacy.

In the absence of presenting the totality of accumulated accounts as evidence, readers of psychical research reports had to rely on the interpretations provided by investigators. Though inexpert volunteers collected information, expertise was a central component of the evaluation. For James (1887a), the determination of the veracity of an account "comes back essentially to the investigator's instinct, or nose, as one might call it, for good and bad evidence" (p. 19). While those reading the reports of psychical research might seek to make their own judgment of the material "it must be remembered, that, so far as expertness in judging of truth comes from training, no reader can possibly be as expert as the authors" (James, 1887a, p. 19). As in Hall's child study investigations, only the trained expert was capable of uncovering the truth within a mass of material.

Much of psychical research was predicated on the accumulation of great masses of material, but sheer volume was insufficient to produce valuable results. Knowing this, James (1892) argued that in this research

quality, and not mere quantity, is what has been mainly kept in mind. The most that could be done with every reported case has been done. The witnesses, where possible, have been cross-examined personally, the collateral facts have been looked up, and the narrative appears with its precise coefficient of evidential worth stamped on it, so that all may know just what its weight as proof may be. (p. 729)

It was simply infeasible to present all collected facts as evidence. Instead, psychical researchers sought to convince readers of their conclusions, by making transparent at least a portion of their process of assessing these facts. This, together with their expertise in the field and their self-styling as men of science, was hoped to be sufficient to persuade others to adopt their way of thinking.

Conclusion

Unlike Beard's characterization of the scientific process as one governed by the word of experts, psychical researchers embraced a somewhat more distributed model of science. Adopting questionnaires as a means of surveying the experiences of a wide spectrum of the public, these investigators took untrained testimony as the foundation of their scientific work. This is not to say, however, that the narratives provided by the public were uncritically accepted. As described in this chapter, expertise remained a critical component of the analysis of the masses of information accumulated via questionnaire. In at once democratizing the scientific process and further reifying the privileged position of the few, psychical research adopted a style of scientific thinking at odds with what many understood as objective science. The naturalistic orientation of their investigations, predicated on the collection of masses of data on individual experiences, necessitated placing portions of scientific practice into the untutored hands of the

public. This, together with their already suspect status as a function of their subject matter, made psychical work an easy target for criticism (see Chapter 6).

With respect to undertaking research on matters that fell within the sphere of psychical phenomena the value of personal observations as scientific evidence came into dispute. While accounts of personal experience, were satisfactory forms of evidence in the sphere of research on children, in the contested space of psychical research the personal was problematic. On this point William James informed sceptic G. Stanley Hall,

I should express the difference between our two positions in the matter, by calling mine a baldly empirical one, and yours, one due to a general theoretic creed.... I don't think it exactly fair to make the issue what you make it—one between science and superstition.³¹⁷

For James, the ASPR and the British Society's efforts to collect information on experiences outside of the norm was a more valid enterprise than Hall's *prima facie* dismissal of all such phenomena as mere wishful thinking or, more damningly, fraud.

Although psychical researchers readily adopted questionnaires as a means of undertaking a particular kind of science, they were themselves aware of the challenges associated with the method. The labour involved with these inquiries was enormous, both that of the participant-public and that necessary from researchers themselves. Publicizing the inquiry, sending out blank forms, collecting returns, corresponding with volunteers, and collating responses all required the devotion of much time and energy. Were these efforts worth the end result? Perhaps. In 1887, having completed his inquiry into the consciousness of lost limbs, James observed in a delicate inquiry like this, little is to be gained by distributing circulars. A single patient with the right sort of lesion and a scientific mind, carefully cross-examined, is

³¹⁷ W. James to G. Stanley Hall, Nov. 5, 1887 in James (1998, p. 284).

more likely to deepen our knowledge than a thousand circulars answered as the average patient answers them, even though the answers be never so thoroughly collated by the investigator. This is becoming apparent in many lines of psychological inquiry; and we shall probably, ere long, learn the limits within which the method of circulars is likely to be used with fruit. (James, 1887b, p. 249 fn.)

Despite these reservations, he went on to conduct an even larger questionnaire-based inquiry: the American census of hallucinations.

Chapter 6

Psychical Research, Probability, and Scientific Styles

Studies of telepathic phenomena were not limited to the kind of questionnaire-based investigations discussed in the previous chapter. Experimental work conducted in the laboratory also aimed to determine the parameters of mental life. Unlike questionnaire-based studies, experimental investigations were conducted with a limited pool of subjects,³¹⁸ yet still involved the collection of a mass of facts across numerous trials with those same subjects. A reliance on accumulating large quantities of data was necessary given the aim of detecting not self-evident regularities of mental functioning, but rather the occurrence of extraordinary ability. What to do with this mass of material was the looming question. Both kinds of studies, though rooted in fundamentally different methodologies, adopted similar styles of scientific thinking at the point of analysis and interpretation: the calculation of relevant probabilities.³¹⁹ These calculations, and their attendant interpretations, were fraught with difficulty and played a central role in discussions between believers and skeptics. These debates over the value and interpretation of particular forms of evidence offer insight into the methodological and scientific commitments of some of psychology's earliest practitioners.

Psychical investigations held as paramount the belief that statistics, as applied to a collected mass of facts, was what characterized their work as scientific. By this measure, the undertaking of a census, and the employment of questionnaires more generally, was of great benefit because it provided "... the means of collecting an enormous amount of material for study" (James, 1892, p. 733). Collected with relative ease from a wide segment of the public, this

³¹⁸ For an exception see Chapter 5.

³¹⁹ On psychical research and probabilistic thinking see Hacking (1988) and Porter (1986).

mass of material was not meaningful in and of itself, but required analysis and interpretation. This was similarly the case with experimental studies of psychical phenomena. Like Darwin “collected, collated, colligated his enormous array of facts” psychical researchers set out to “collect, collate, and colligate the disarray of their facts” (Phelps, 1885, p. 256).

While James was willing to involve members of the public in his scientific practice, other early psychologists were not. The particular American interpretation of Wundtian introspection, promoted most ardently by Edward Titchener provided one alternative. Remaining within the laboratory environment, Titchenerian introspectionism often abandoned the measurement of the speed of mental processes in favour of an introspective examination of the units of consciousness by those trained in the method. Training, in this case, was to ensure the attainment of the best results if only from a specialized few (C. D. Green, 2010). On the other hand, questionnaires forewent training and instead enlisted the untrained masses in the introspective process. Valuable results would be obtained in looking at the results of this kind of introspection as a whole, rather than relying on findings from a few intensely interrogated minds. Each system was predicated on a different understanding of where psychological facts could be best achieved, whether through the efforts of the expert or through the aggregation of the experiences of a portion of larger society. In each case mental phenomena otherwise outside the scope of study were rendered part of the purview of scientific psychology by an extension of the discipline’s methods.

In this chapter I focus on two specific debates over psychical phenomena: attempts to interpret the findings of the census of hallucinations described in the previous chapter and a debate over experimental work on telepathy that arose at the close of the nineteenth century. James was – unsurprisingly – a central figure in both discussions, joined in the latter instance by Titchener. The census of hallucinations was very clearly a questionnaire-based inquiry. The

studies of telepathy were not. Nonetheless, statistics were as crucial objects for debate in each enterprise as, irrespective of method, calculations of probability were central to competing interpretations. Unlike questionnaire endeavours that relied on the accumulations of masses of descriptive material, the debates discussed in this chapter often-involved precise numerical values. Though numbers were easier to synthesis than hordes of descriptive data, they remained open to interpretation and debate. As a counterpart to the census, the related debate over experimental inquiries into telepathy provides insight into the different methodological commitments of early American psychologists and their vision of what constituted psychological science and the proper route to truth. At the same time, I argue, the non-psychical understanding of mental life put forward in this debate itself rendered information gleaned from untrained members of the public – precisely the form of data collected in questionnaire projects – as inherently untrustworthy. This threatened to undermine the entire project of questionnaire research.

Interpreting the Census of Hallucinations

Amassing material for a census of hallucinations was, at least superficially, straightforward, but interpreting the results of this endeavour was more involved. James's public pleas for aid in conducting the census in the United States were successful, though his efforts resulted in only a fraction of the 50,000 responses it had been hoped would be produced as part of the larger international endeavour (James, 1890b). In the end the American census attracted 7,123 responses and even though this was far below the number of responses desired at the project's outset James was overwhelmed (see Chapter 5). Of these, 1,051 were reports of hallucinatory experiences (James, 1986b). After eliminating those whose experiences occurred when under the age of 10 and reports of vaguely similar experiences from a single individual,

James was left with “62 subjects with 71 cases of visual hallucination of some recognized living person.”³²⁰ In order to obtain a more accurate estimate of the number of visual hallucinations experienced by these individuals James then – as a correction for hallucinatory experiences that had been lost to memory, following the example of the treatment of figures for the British census – multiplied this number by 6 ½. This left him with the figure of 462, meant to represent the probable number of veridical hallucinations actually experienced by these individuals since the age of 10.³²¹ Further calculations using the death rate in the United States led James to conclude, “apparitions on the day of death are, according to our statistics, 487 more numerous than pure chance ought to make them.”³²² With these figures, James thought, one could argue “against the probability of chance coincidence” as an explanation for veridical hallucinations.³²³

Even prior to completing his analysis of the American census, James was convinced that veridical hallucinations occurred at levels above chance. He praised the British census for “an unusually careful handling of the raw material offered” which produced “a great accession of new facts” (James, 1895b, p. 69). Its more than 17,000 collected responses, James (1895b) determined, “affords a most formidable presumption that veridical hallucinations are due to something more than chance” (p. 75). The still to come final results of the American census, James (1895b) hoped, would only lend further credence to this conclusion.

In these census undertakings, investigators sought to define the probability, on the basis of chance alone, of an individual reporting a hallucinatory experience that corresponded an event

³²⁰ W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 74). Due to the smallness of this number, James opted not to correct the number for “suspicious” cases as was done with the results of the British census (see James, 1986b, p. 74).

³²¹ See W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 74).

³²² W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 74).

³²³ W. James to H. Sidgwick, July 11, 1896 in James (1986b, p. 74).

that in fact took place and to compare this to the frequency of these kinds of reports in the census data. As James (1892) described,

The result seems to be, roughly speaking, that about one adult in ten has had such an [hallucinatory] experience at least once in his life, and of the experiences themselves 14 per cent coincide with some real distinct event. In other words, one person out of every one hundred and forty in the community has had a veridical hallucination of some sort or other, vague or precise. The question is, Is this degree of frequency too great to be deemed fortuitous, and must we suppose an occult connection between the two events? (p. 733)

The presumption was that if such cases occurred with sufficient frequency psychical phenomena were substantiated. How accurate psychical researchers' calculations were was a matter of some debate.

Statistical Arguments

Disagreements over statistical interpretations of psychical findings were not, of course, limited to the census of hallucinations. The invocation of probabilities was a mainstay of psychical investigations. Like the census, other studies sought to prove that seemingly otherworldly phenomena were more than mere chance coincidence and, in fact, represented genuine wonders. Yet the sheer scope of the census, relative to other investigations, rendered the potential ramifications of its conclusions that much larger.

Immediately prior to the formal founding of the ASPR problems of data collection and interpretation were highlighted by the Society's soon-to-be first president. Writing in the pages of *Science*, Newcomb derided a recent call for accounts of experiences with thought-transference, apparitions, and related phenomena that had appeared in the newspaper the New-

York Nation: “it would be difficult for the society to put forth any thing better fitted than this advertisement to lower the estimation in which their work is held by common-sense people” (Newcomb, 1884, p. 372). In his estimation, a full percent of the population was susceptible to vivid dreams, visions, and illusions. On this basis he calculated a set of expected probabilities, which he used to argue that there occurred nearly everyday, purely as a result of coincidence, some correspondence between a death and “the dream or vision by a friend of the dying person” (Newcomb, 1884, p. 372). Just these kinds of calculations were central to psychical researchers’ projects, though it was often hoped that the “the application of the doctrine of chances” (Gurney, 1884, p. 509) would substantiate rather than explain away certain phenomena.

Studies undertaken in the years prior to the census were explicit about the evidentiary value of statistical analyses. The ASPR’s Committee on Thought-Transference, calculated probabilities associated with their collected evidence, but also highlighted the importance of allowing individuals to evaluate evidence for themselves. Given the latter commitment, they sought to provide as full an account of this evidence as possible in order “to enable persons who are unfamiliar with the principles of mathematical probability to form their own independent judgment of the soundness and the just application of our methods, and of the fairness of our conclusions” (J. M. Peirce & Pickering, 1885, p. 17).³²⁴ To what extent their readers were equipped to do so is an open question.

Problems of data collection further complicated attempts at statistical analysis. Toward the end of his work on the American census, James was disheartened by the quality of responses

³²⁴ Other psychical investigators similarly sought to leave conclusions to readers, presenting them with unadulterated evidence, rather than their own interpretation, through the reproduction of graphic recordings (see Bordogna, 2008, p. 113). James similarly explicitly strove to report only the facts, rather than his conclusions, in his investigation into the purported contacts made by medium Leonora Piper with Hodgson following the latter’s death (see Bordogna, 2008, p. 129).

and his own lacklustre efforts to organize the material. Particularly problematic was the first phase of the census, which sought to ascertain simply the proportion of individuals who had experienced veridical hallucinations during their lifetime, relative to those who had not: “My impression is that beyond the crude appearances, no accurate analytic statistics can be based on the [Schedule] A’s, from the imperfection of the record.”³²⁵ More still, much of the material was in disarray, due to his admitted failure to note – until the very end of the analysis – from which Schedule A individual accounts of hallucinatory experiences had originated. Of the more detailed Schedule Bs, which provided the specifics of hallucinatory experiences from those who answered yes on the first part of the inquiry, James was more optimistic. These, James contended, as he handed the material over to Hodgson for analysis, seemed “sufficient for statistics.”³²⁶ This pronouncement was rooted in James’s observation that he and Eleanor Sedgwick, the head of the British census in charge of statistical analyses, had arrived at “the same percentages.”³²⁷ The correspondence of numbers across the two endeavours was sufficient, in James’s view, to affirm the worth of the data collected as part of the American census.

The underlying rationale for the census, as well as other data collection projects into psychical phenomena, was known as the “cumulative evidence” or “faggot” argument. This argument held that it was more probable that a witness otherwise known to be truthful was being dishonest than it was that an improbable phenomena had taken place, but, and this is the crucial point, that with the an increase in the number of witnesses the probability of testimony being true would be surpass the improbability of a phenomena (see Bordogna, 2008, p. 105). To be sure, this chain of reasoning was not universally accepted, yet for his part James (1909) asserted,

³²⁵ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³²⁶ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

³²⁷ W. James to R. Hodgson, May 25, 1892 in James (1999, p. 271).

I am ... constantly baffled as to what to think of this or that particular story, for the sources of error in anyone observation are seldom fully knowable. But weak sticks make strong faggots; and when the stories fall into consistent sorts that point each in a definite direction, one gets a sense of being in presence of genuinely natural types of phenomena. As to there being such real natural types of phenomena ignored by orthodox science, I am not baffled at all, for I am fully convinced of it. (p. 587)

The faggot argument was convincing for James, but he was under no illusions about the persuasiveness of the argument for more sceptical individuals. Writing about his work on the American census he informed Eleanor Sidgwick that he thought the calculation of statistics regarding veridical hallucinations was unlikely to sway opinion on the matter.³²⁸ Even the larger international effort was deemed insufficient on this front as it was “still too small for knock down proof of occult cause” (James, 1895b, p. 74).

For those like James, who regarded telepathy as likely on the basis of other forms of evidence, including firsthand personal experience, census statistics simply reinforced this belief. On the other hand, James (1895b) noted

they will not convert the disbeliever, who will pooh-pooh the statistical method in toto when it takes 17,000 answers to get 30 good cases to cipher with, saying that the field is too vast and lean for profitable reaping, that figures got by applying so many hypothetical corrections to inaccurate crude data, savor too much of guess-work to inspire confidence, and that cooked returns are cooked returns, even though, like these, they be cooked for the safe side, the side adverse to the conclusion reached by their means. (p. 74)

³²⁸ W. James to E. M. Sidgwick, May 15, 1892 in James (1999, p. 269).

Doubt of the form James described was pervasive within scientific circles. Cattell (1896) vehemently expressed his view of the census: “when we have an enormous number of cases, and cannot find among them all a single one that is quite conclusive, the very number of cases may be interpreted as an index of the weakness of the evidence” (p. 582).

Hall, James’s former student, was similarly derisive of the evidence accumulated in psychical research. In reviewing the work of English psychical researchers, including the recently released *Phantasms of the Living* (Gurney et al., 1886), Hall (1887a) noted we must hasten on to note the fallacious conception of evidence in such a field. Much is said about “spreading responsibility,” the “cumulative” nature of the proof for telepathy, increasing the number of people who are knaves or idiots if it is not true, and the multiplication of instances is compared to increasing the size of a bundle of faggots, each one of which is easily broken, till together their evidential value is irresistible. (p. 143)

Psychical researchers were striving for quantitative proof in their investigations; that is, the accrual of a great *quantity* of cases. This, however, from Hall’s view was a fundamentally flawed approach. It was not quantity, but quality that was to take precedence through “more systematic and prolonged study of such cases as have been already found” (Hall, 1887a, p. 144).³²⁹ These kinds of concerns were just as relative to questionnaire-based child study investigations, but in that realm the stakes were not nearly so high. Though psychical researchers recognized the necessity of quality material (see Chapter 5) their emphasis on quantity often overshadowed other considerations.

The ability of psychical researchers to invoke probabilities that located the chance occurrence of an extraordinary event at astronomical levels was also singled out as problematic

³²⁹ The study of Mrs. Piper by Amy Tanner (1910), conducted under Hall’s tutelage, certainly fit this description.

(see Jastrow, 1900, pp. 97–8; Münsterberg, 1899, p. 76). As one observer commented, psychical researchers’ “use of the so-called doctrine of chances is so ludicrous as to be practically a burlesque of science” (Buckley, 1888, p. 454). Jastrow (1900), for instance, attributed faulty estimations of this sort not simply to a misunderstanding of probability theory, but also to the persuasiveness of personal experience for those making such calculations:

We find a misconception of the nature of the application of the doctrine of chance to mental coincidences, which brings about an apparent recognition but an intrinsic belittling of the rôle which chance really plays in the evidence advanced for telepathy; we find that this error is probably due to an unfortunate, intensely individual view of the problem, which insists upon an explanation of personal experiences, and disregards the essentially impersonal and statistical nature of the inquiry. (p. 94)

More still, investigators were cautioned to take greater precautions against error. Paralleling the overblown probabilities propounded as evidence in favour psychical events, Hall (1887a) advised that in this line of research the likelihood of error “would be shown so incalculably great could the calculus be intelligently applied to their estimation” (p. 144).

Some of the most pointed criticism of psychical researchers’ attempts to marshal probabilities for their ends came from more statistically literate individuals. Philosopher and logician Charles Sanders Peirce and his student Christine Ladd Franklin, both of whom undertook work that fell within the new psychology, were especially critical of the calculations of psychical researchers.³³⁰ At Johns Hopkins Peirce established a Metaphysical Club, many of the members of which were avowed skeptics of psychical phenomena, some undertaking work

³³⁰ On Peirce and psychology see Leary (2009) and Cadwallader (1974). On Ladd Franklin see Cadwallader and Cadwallader (1990) and Furumoto (1992).

explicitly to disprove these kinds of occurrences (e.g., Jastrow, 1886a).³³¹ Ladd Franklin was one member of the club (Behrens, 2005), and also a founding member of the ASPR (Coon, 1992b). Her criticisms of the work of others, however, seem to have been her only significant contribution to the psychical field.

In the mid-1880s, just after completing work for a doctorate at Johns Hopkins University, Ladd Franklin took French psychical research Charles Richet to task for what she characterized as his misuse of the theory of probabilities. Richet, a physiologist and psychical researcher, published results of a study into thought-transference within the pages of *Revue Philosophique* at the end of 1884 in which he purported to prove, through his invocation of the theory of probabilities, the phenomenon's existence. The application of probability theory to psychical phenomena was Richet's particular innovation, one soon taken up by other researchers (Hacking, 1988).

Although Richet undertook four different experiments on mental suggestion, including "guessing a photograph drawn at hazard from a set of six, finding a watch hidden under one of several orange-trees by means of the vibrations of a stick, and spelling out names by means of table-rappings" (Ladd Franklin, 1885, p. 133), for the purposes of her statistical critique Ladd Franklin focused on an experiment involving guessing cards drawn randomly from a deck. On the basis of his experiments Richet calculated the probability of mental suggestion to be 2/3, the ratio of the number of correct trials to the total number of trials. This, however, as Ladd Franklin (1885) points out, is not the probability that the findings were the result of chance: "a

³³¹ Among the Johns Hopkins Metaphysical Club members with connections to the new psychology were Cattell, Hall, Dewey, Jastrow, Ladd Franklin, Benjamin Ives Gilman, William Noyes, and Royce, a corresponding member of the group (Behrens, 2005). The club was founded in the wake of the more famous Cambridge Metaphysical Club of the 1870s, of which Pierce was also a member (Menand, 2001).

comparison of this sort affords no measure of the improbability of the observed facts being the result of chance" (p. 133). What is needed is not a calculation of the deviation from the average, but rather the probability that such a deviation should occur. Richet, Ladd Franklin (1885) notes, "does not seem to know that there is a mathematical formula by which this probability is determined" (p. 133). Applying her own mathematical knowledge to his numbers, she arrived at 1/70 as the probability of the deviation from the average found in his card guessing experiment, far from his original 2/3 value. Though taking no particular position with respect to the reality of psychical phenomena herself, Ladd Franklin (1885) thoroughly refuted Richet's conclusions: "his unfamiliarity with the theory of probabilities renders his numerical deductions, except the most obvious ones, misleading and useless" (p. 133).

The same year Ladd Franklin's commentary of Richet appeared, so too did a study by Peirce and Jastrow (1885) often regarded as the first experiment in scientific psychology in the United States. In their work Peirce and Jastrow offered a probabilistic criticism of the existence of the sensory discrimination threshold identified by German psychophysicist Gustav Fechner. In doing so, they linked their findings to psychical matters, arguing that purported instances of "telepathic" phenomena could be explained with reference to faint sensations outside of awareness (C. S. Peirce & Jastrow, 1885, p. 83; see also Cadwallader, 1974; Dehue, 1997; Hacking, 1988). A decade later Jastrow (1895) continued to assert, in response to claims of psychical phenomena,

the possibility of the transference of thought, apart from the recognized channels of sensation, has been too frequently discussed with the suppressed or unconscious assumption that our knowledge of the means by which we ordinarily and normally

convey to others some notion of what is passing in our own minds, is comprehensive and exhaustive. Nothing could be farther from the truth. (p. 571)

Studies like the one conducted with Pierce provided concrete evidence by which to support the idea that faint sensations, outside of conscious awareness, were viable causes of purported psychical experiences.

Peirce was highly sceptical of psychical phenomena, even given his friendship with the great believer James. Though James was much impressed with the SPR's *Phantasms of the Living* (Gurney et al., 1886), Peirce found much to fault in the volume, offering up an extensive critique of the study's methods and conclusions (see Hacking, 1988). He criticized the author's for their misapplication of statistical reasoning, particularly in the case of calculations related to "spontaneous telepathy" or veridical hallucinations:

they cipher out some very enormous odds in favor of the hypothesis of ghosts. I shall not cite these numbers, which captivate the ignorant, but which repel thinking men, who know that no human certitude reaches such figures as trillions, or even billions to one. (C. S. Peirce, 1887a, p. 150)

Outside of the extravagant probabilities presented, it was the character of the data on which these were based that most troubled Peirce. His outline of 18 specific points of contention with the study focused largely on the quality, or lack thereof, of the cases included in the analysis. In his opinion, given "the utter uncertainty of all the data, it would be very rash to draw any conclusion at all. But the evidence so far as it goes, seems to be rather unfavorable to the telepathic character of the phenomena" (C. S. Peirce, 1887a, p. 156).

Peirce's commentary did not go uncontested as Gurney (1887), the driving force behind the volume, provided his own point-by-point rebuttal. Unwilling to concede ground, Peirce

responded at length. Admitting a number of errors in his original commentary, notably in his identification of the numbers assigned to specific cases he considered suspect, he characterized Gurney's decision to focus on his errors rather than address the real issues at hand as a "line of argumentation [that] seems more appropriate to a school disputation than to a scientific inquiry" (C. S. Peirce, 1887b, p. 181). Once again attacking the logic of the undertaking and the form of the statistical analyses Peirce (1887b) concluded "until the telepathic theory of ghost-stories has been rendered far more antecedently probable than it now is, it is useless to try to establish it as a scientific truth by any accumulation of unscientific observations" (p. 215). Perhaps predictably, Gurney (1889) offered a further lengthy refutation only to have their ongoing debate cut short by his untimely death. In his absence Myers (1889), one of Gurney's co-authors on *Phantasms of the Living*, waded into the debate, arguing that Gurney

always intended to take a further census before long. It is to be hoped that his example, in thus substituting the laborious but fruitful methods of statistics for the vague generalities current on this subject before him, may be followed in England and elsewhere. (p. 301)

Though Peirce (1887b) was far from enthusiastic about this prospect, he did contend that if investigators were determined to continue with this kind of work, they would be better off undertaking "a new census ... upon a larger scale and with the sufficient means to carry it out in a thoroughly scientific manner" (p. 84). Its doubtful that Peirce considered the larger international census to have been carried "out in a thoroughly scientific manner" as it no more escaped criticism than did its problematic forerunner.

The James-Titchener Telepathy Debate

James involved himself with American census of hallucinations, but was also interested in other methods of inquiry in the field. In the final years of the nineteenth century, his

investment in the outcome of psychical research led to a dispute with Titchener over a set of experiments that seemed to refute previous findings in support of telepathic phenomena.³³² As revealed time and again over the course of this debate, the two men were by no means of one mind with respect to either the existence of psychical phenomena or, perhaps more importantly, the proper means of evaluating the reality of these kinds of experiences. The disagreement served as a site in which two of psychology's most distinguished contributors voiced very different opinions on what constituted proper investigatory practices and scientific reasoning. For Titchener, these investigations were to be laboratory-based (C. D. Green, 2010). James, on the other hand, was far less committed to the laboratory as *the* site of science. For him, scientific inquiry required the accumulation of as much information as possible on the subject under consideration. With this data at hand, relevant probabilities could be calculated to determine both a phenomenon's existence and prevalence. While one may argue that James's view of the scientific enterprise was coloured by his psychical commitments, he nonetheless articulated a very different view of how science was to be carried out.

The Sidgwicks' Thought-Transference Experiment

Strictly speaking the debate between James and Titchener over telepathy was confined to a series of seven articles published in *Science* from late-1898 through the spring of 1899 – as well as a series of personal correspondence on the matter during this time – but the issues under dispute dated much earlier.³³³ The discussion centred around experimental results on telepathy published by Danish researchers F. C. C. Hansen and Alfred Georg Ludwig Lehmann several

³³² On James and Titchener's relationship see Bjork (1983).

³³³ The debate between James and Titchener on telepathy comprises a number of articles, including Titchener (1898b, 1899a, 1899b, 1899c) and James (1898b, 1899a, 1899c). See also the reprint of the debate, with additional commentary, published by the ASPR ("Messrs. Hansen and Lehmann on the telepathic problem," 1899), as well as James (1986a, pp. 167–79).

years earlier (Hansen & Lehmann, 1895). This study was itself a response to still earlier work undertaken by the SPR on the topic.³³⁴ I focus here less on these original studies, both of which took place in Europe, than on how this work was presented in the pages of American scientific periodicals and taken up in discussion and debate by two of the most prominent new psychologists in the country. In the course of this dispute, James and Titchener each made explicit their differing scientific commitments. These scientific allegiances, in part, provide context for James's willingness to engage in, and entertain more generally, work with the questionnaire method, as well as the larger – often dismissive – disciplinary response to these undertakings.

In 1890 Henry and Eleanor Sidgwick, with the assistance of entertainer George Albert Smith,³³⁵ published findings from a series of experiments on thought-transference in the SPR's *Proceedings* (Sidgwick, Sidgwick, & Smith, 1890). Smith served as the telepathic agent in the experiment while 4 hypnotized subjects served in the role of percipient. After Smith randomly selected and looked at a 2-digit number (between 10 and 90) the percipient guessed what number had been drawn. Correctly guessed digits, irrespective of their order, were scored as successes. Over the course of 644 trials in which the agent and percipient were in the same room, 131 successes were recorded, while over 228 trials in which the agent and percipient were in separate rooms, only 9 successes were recorded. In the former setup 14 of the 131 recorded successes

³³⁴ Also see James's (1897) earlier review of Henry Sidgwick's response to the work of Lehmann and Hansen.

³³⁵ As an entertainer Smith employed mesmerism and telepathy in his performances, and attracted the attention of prominent British psychical researcher Edmund Gurney, who employed Smith as his private secretary. Gurney, it is rumoured, committed suicide in 1888 when Smith and another's deliberate deceit in a thought-transference experiment was revealed (Oppenheim, 1985).

were guesses in which the order of the digits were reversed; in the latter, this number was 1 of 9 (Sidgwick et al., 1890).

Anticipating some of the criticism to come, the Sidgwicks noted the possibility that hyperaesthetic whispering, that below the level of general awareness, had influenced their findings. Acknowledging that telepathy operated with far more success when agent and percipient were in the same room, the experimenters noted also “any whispering or faint pronouncing of the number would lead, one would think, to numbers whose names have common characteristic letters being mistaken for each other” (Sidgwick et al., 1890, p. 165). This kind of correspondence between errors and the selected number was, however, not found and instead the experimental results were taken as evidence in favour of telepathic phenomena (see also “Review of Ueber unwillkürliches Flüstern,” 1896, p. 98).

The Hansen & Lehmann Experiment

The Sidgwick experiments were presented as evidence in favour of telepathy, but their findings were soon challenged. Hansen and Lehmann repeated this experiment on telepathy with particular attention to the previously ruled out whispering explanation (see Hacking, 1988). As an unnamed reviewer for the *Psychological Review* described,

Repeating the experiment with two hemispherical mirrors, 90 cm. wide, opposite each other, the head of the agent being in the focus of one, and that of the percipient in the focus of the other, they found that the numbers could be *heard* by the percipient, and consequently named rightly; when the agent inwardly articulated them, even [though] the bystanders could hear nothing and the agent’s lips were tightly closed. (see also “Review of Ueber unwillkürliches Flüstern,” 1896, p. 98)

The finding that whispering was the source of results that could otherwise be interpreted as telepathy was further supported, in the reviewer's opinion, by Hansen and Lehmann's discovery that the mistakes made by the subjects in the Sidgwick's experiments mirrored those made by subjects in their study. These mistakes they argued, despite the Sidgwick's earlier dismissal of the idea, were the result of imperfect hearing and thus the confusion of phonetically similar numbers ("Review of Ueber unwillkürliches Flüstern," 1896). Presenting a highly favourable account of the Danish studies, the reviewer noted that Hansen and Lehmann's work constituted "a genuinely scientific contribution to the elucidation of so-called thought-transference phenomena, and contrasts most agreeably with the random abuse to which their recorders are accustomed" ("Review of Ueber unwillkürliches Flüstern," 1896, p. 99).

The Explanatory Power of Faint Sensations

The idea that some or all purported telepathic phenomena could be explained with reference to imperceptible sensations, as Hansen and Lehmann's study intimated, was not new. Just prior to assuming the presidency of the newly formed ASPR, Newcomb (1884) speculated that accounts of "feeling at a distance without the intervention of any physical agent" (p. 373) were simply experiences that had been mischaracterized:

We must remember that the physical connection through which one mind affects another may be of the most delicate kind; may, in fact, nearly evade all investigation. The slightest look, an unappreciable motion of the muscles of the mouth or eyes, made perceptible through the light which is reflected to the eye of the second person, constitute a physical connection. (Newcomb, 1884, p. 373)

An individual might perceive, below the level of general awareness, the subtlest of physical changes in another and this might be sufficient to account for occurrences so readily classified as telepathic.

Identifying whether individuals were capable of detecting these kinds of faint changes was a project for the new scientific psychology. Peirce and Jastrow's work on the sensory discrimination threshold, discussed earlier, and their finding that there was in fact no absolute minimum perceptible difference was a project targeted at just these issues:

The general fact [of no minimum perceptible difference] has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another's minds in large measure from sensations so faint that we are not fairly aware of having them, and can give no account of how we reach our conclusions about such matters. The insight of females as well as certain "telepathic" phenomena may be explained in this way. Such faint sensations ought to be fully studied by the psychologist and assiduously cultivated by every man. (C. S. Peirce & Jastrow, 1885, p. 83)

Though Peirce and Jastrow's experiment offered an alternative basis for ostensibly telepathic experiences, this work was not enough to discredit all future findings in this direction. Psychical researchers, rather than attributing evidence of telepathy to an agent's detection of subtle sensory information from the outset, sought to assess whether these kind of sensory processes were in fact operative.

The debate between James and Titchener over telepathy began in earnest with Titchener's publication of a brief research report in *Science* on the ability of individuals to detect when they

are being stared at.³³⁶ Referencing some laboratory experiments he and his students had conducted on the matter, he rejected the idea that the feeling of being stared at was the result of some ubiquitous telepathic ability. Rather, the phenomenon was described as no more than the result of an awareness that others are in a room and our tendency to direct attention to, what are otherwise normal and unremarkable, sensations like those at the back of one's neck. For Titchener (1898b) undertaking investigations with his students on what might be regarded as a trivial matter was justified as a project that allowed for the "breaking-down of a superstition which has deep and widespread roots in the popular consciousness" (p. 897). In doing so, this kind of project provided students of psychology with a useful introduction to the scientific mindset. His further remark that "no scientifically-minded psychologist believes in telepathy" (Titchener, 1898b, p. 897) was likely targeted at the field's staunchest supporter of spiritualistic phenomena: James. This, together with the final words of Titchener's article, which characterized Hansen and Lehmann's recent research on telepathy – or the lack thereof – as "brilliant work" (Titchener, 1898b, p. 897) moved James to respond.

Central to the resulting dispute was disagreement over the validity of Hansen and Lehmann's conclusions given their investigatory practices. In evaluating this work James and Titchener took markedly different approaches. Relying on a statistical analysis, much like in his earlier census work, James contended that Hansen and Lehmann had failed to provide sufficient proof that results of thought-transference studies were explicable on the basis of hyperaesthetic whispering. The less psychically sympathetic Titchener, held the experiment up as conclusive

³³⁶ The investigation of this subject had previously been suggested by Hiriam Stanley of Lake Forrest College, in Illinois, who specifically suggested there was need to investigate phenomena such as the ability of some individuals to detect when they are being stared at from behind (Stanley, 1891).

evidence against telepathy, very much in line with his own laboratory-based work on individuals' ability to detect being stared at.

James's Mathematical Refutation

Prior to Titchener's incendiary dismissal of telepathic phenomena and its believers, James published a critique of Hansen and Lehmann's experiment. The anonymous review of the study which appeared in the *Psychological Review* was followed shortly thereafter by a piece by James in which he reviewed the formal response issued by Henry Sidgwick to the work. In this review James, perhaps unsurprisingly, sided with Sidgwick's analysis of the research. He criticized Hansen and Lehmann for the paucity of their data, which consisted of only 500 observations, a number James deemed "obviously too small a number for safe conclusions" (James, 1897, p. 654 fn.). Convinced that more observations were required to reach definitive conclusions, he went so far as to collect data for "a series of upwards of 1,000 guesses at bi-digital numbers whispered with closed lips by the agent" (James, 1897, pp. 654–5 fn.). With his own set of data in hand James, following the method of Hansen and Lehmann, compared the four most frequent errors in both his results with those of the British researchers and found twenty commonalities between the two sets of data. He then compared the four most frequent errors in his series with the four *least* frequent errors in the Sidgwick experiment, finding that fifteen digits were the same across the studies. Given the similar levels of correspondence, James concluded there was no real difference between correctly and incorrectly guessed digits irrespective of the occurrence of whispering. In his view "both the agreements and the disagreements are thus probably accidental" (James, 1897, p. 655 fn.) and attributable to the limited data collected by the Danish

researchers.³³⁷ Like Sidgwick, James declared they had failed to prove the “hypothesis of whispering as the cause of the ‘thought-transference’ results” (James, 1897, p. 655 fn.).

In this undertaking, as in other endeavours, James privileged the accumulation of substantial quantities of data. He noted, “after analyzing my own 1000 cases that at least 5000, (not 500 as in L. & H.) would be required, if one wished any valuable conclusions – and possibly 10,000 or more.”³³⁸ Though recognizing the need for additional data James was uninterested in pursuing this work himself, which he attributed to both his “laziness” and the fact that this particular line of work would have no bearing on studies of thought-transference conducted with agent and percipient in separate rooms.³³⁹

James considered both his and Sidgwick’s criticisms of Hansen and Lehmann as constitutive of a mathematical refutation of their conclusions. He repeatedly informed correspondents of this fact, writing to Gardiner: “Sidgwicks criticism and mine are absolutely and as it were mathematically conclusive against Hansen’s—&L’s theory that the sidgwick [sic] phenomena were due to whispering.”³⁴⁰ He similarly informed Cattell of Hansen and Lehmann’s work: “the failure of their proof is as it were mathematical and absolute.”³⁴¹ This, of course, did not mean that their hypothesis regarding the true cause of some cases of thought-transference was invalidated, as James admitted the refutation was “not of the *hypothesis* of L. & H., but of

³³⁷ See W. James to H. N. Gardiner, Jan. 10, 1899 in James (2000, p. 484).

³³⁸ W. James to H. N. Gardiner, Jan. 10, 1899 in James (2000, p. 484).

³³⁹ W. James to H. N. Gardiner, Jan. 10, 1899 in James (2000, p. 484). Also a consideration was his desire to devote himself to the investigation of his “white crow,” medium Leonora Piper (see W. James to H. N. Gardiner, Jan. 10, 1899 in James, 2000, p. 484).

³⁴⁰ W. James to H. N. Gardiner, Jan. 8, 1899 in James (2000, pp. 480–1).

³⁴¹ W. James to J. M. Cattell, Jan. 10, 1898 [1899] in James (2000, p. 483).

their *proof* of the hypothesis.”³⁴² His earlier critique of this work constituted, in his view, an irrefutable argument against the evidence *contra* telepathy presented by Hansen and Lehmann.

Wishing further support for his conclusions, James wrote directly to Lehmann.

Referencing both his critique and that of Sidgwick he inquired,

Do you think, after these criticisms, that your 500 experiments are still demonstrative of your hypothesis that the Sidgwick results were due to whispering? Or do you agree with Sidgwick and myself, that both the series were too short, and that the coincidence of errors was probably an accident?³⁴³

Faced with this critique Lehmann admitted that his series was too short to serve as definitive evidence in favour of the whispering hypothesis as initially argued. This concession was readily seized upon by James who, in his continuing dispute with Titchener, reprinted a portion of Lehmann’s response: “Your own as well as Professor Sidgwick’s experiments and computations prove, beyond a doubt, that the play of chance had thrown into my hands a result distinctly too favorable to my theory” (as quoted in James, 1899a, p. 655).³⁴⁴

Irrespective of Lehmann’s judgment of the matter, Titchener remained unconvinced of James’s position. Describing his argument against the results of Hansen and Lehmann as “quasi-mathematical,” in the face of what he saw as Titchener’s continuing obstinacy, James (1899a) offered that he “was astounded at this hardness of hearing in my colleague” (p. 655). Though in James’s mind his, at least quasi-mathematical, refutation served as powerful evidence against the proof offered in favour of the whispering hypothesis Titchener strenuously disagreed. So far as he was concerned this line of argument did not amount to much: “I do not know that quasi-

³⁴² W. James to H. N. Gardiner, Jan. 10, 1899 in James (2000, p. 484). See also W. James to J. M. Cattell, Jan. 10, 1898 [1899] in James (2000, p. 483).

³⁴³ W. James to A. G. L. Lehmann, Jan. 10, 1899 in James (2000, p. 485).

³⁴⁴ See W. James to E. B. Titchener, May 21, 1899 James (2000, pp. 531–2).

mathematics has contributed much to psychology in any field of research" (Titchener, 1899b, p. 687).

Number-Habits

Probabilities were not the only thing invoked to challenge the evidence purporting to explain telepathic results to be a function of whispering. Both James and Titchener raised the possibility of number habits as a further complicating factor in the interpretation of the results (see Chapter 2).³⁴⁵ Indeed, the observation that individuals sometimes exhibited distinct preferences for certain numbers was not new. In the Sidgwick thought-transference study, after ruling out whispering as a determining factor in the results, a number of other characteristics of percipients' telepathic capacities were also remarked upon. It was noted that in detecting the number selected by the agent, the percipient's impression of the number tended to be of a visual nature (Sidgwick et al., 1890). More specifically, in the case of one percipient, identified only as "T.," a distinct number-habit was exhibited; that is, he tended to guess higher digits (7, 8, 9) far less often than other digits (see Sidgwick et al., 1890, p. 170).

Number habits were even more problematic in studies where agents selected their own numbers, rather than choosing at random from a deck of cards. The ASPR's Committee on Thought-Transference, through their *Circular No. 4*, provided individuals with instructions for making various experiments on telepathy including number telepathy (see Chapter 5). Directions for one experiment involved an individual assuming the role of agent and entering the digits 0 to 9 in any order in the first column of a supplied grid. They then focused on each digit in turn. The percipient, unable to see the digits listed by the agent, then guessed which digit was being attended to and their response was recorded (Committee on Thought-Transference, 1885). From

³⁴⁵ On Hansen and Lehmann's experiment and number habits see Hacking (1988).

the records amassed the committee was able to identify one percipient in particular who possessed “a very decided system of guessing, or an unconscious number-habit, which he was following all the while, and which was different from the habit of numbers of the agent” (Minot, 1886, p. 86; see “The First Report of the American Society for Psychical Research,” 1885). In following “certain systems of association in giving his numbers” (“The First Report of the American Society for Psychical Research,” 1885, p. 156) the percipient served as a decided confound. More problematic still were instances where both agent and percipient possessed number habits in the same direction as “some successful guesses may be made which are due not to thought-transference, but to similarity in the number-habits of the two experimenters” (“Messrs. Hansen and Lehmann on the telepathic problem,” 1899, p. 119).

Although to James’s mind his statistical analysis refuted the conclusions of Hansen and Lehmann, the similarity between the errors reported in the Sidgwicks’ experiments and those in the Danish study remained to be explained. To do so, James (1897) favoured Sidgwick’s speculation that such could be best accounted for by the tendency of subjects in both studies to favour certain numbers, observing that “in my own series, the tendency to run on favorite numbers in guessing was a well marked phenomenon, to eliminate the effects of which many thousands of guesses would be required” (James, 1897, p. 655 fn.). Hansen and Lehmann’s limited number of observations undermined their conclusion that whispering was the true cause of ostensibly telepathic phenomena, as the difficulties associated with established number habits in thought-transference research could only be surmounted through a much more extensive series of experiments into the matter. Informing Titchener of the results of his correspondence with Lehmann, James wrote

he absolutely succumbs to Sidgwick's and my contention as to the undemonstrative character of his own experiments, and that he adopts S.'S hypothesis that the coincidence between his error and Sidgwick's was probably in large part due to the accidental coincidence of similar number-habits in the experiments.³⁴⁶

Although James was in other instances dismissive of mental habits as alternatives to apparent instances of thought-transference (see Chapter 5), he was not above invoking these kinds of associations as evidence against the evidence against telepathy.

Like James, Titchener thought number habits were an important consideration. He was not, however, inclined to adopt James's number-habit explanation for the similarity in errors across the studies. For Titchener, even given Lehmann's change of mind, this correspondence remained evidence of the whispering hypothesis. Still, he held that in studies of thought-transference "the next stage of advance must be an exhaustive study of the 'number habits'" (Titchener, 1899b, p. 687, see also 1899c), as mental habits were a plausible explanation for instances of apparent thought-transference where whispering was not a sufficient or possible factor. Titchener was not alone in this kind of speculation. Both earlier American psychical investigators (Minot, 1886, 1889) and psychologists also saw mental habits as central to naturalized accounts of some extraordinary phenomena. In a critical account of work on thought-transference, Jastrow (1887) observed

the human mind is not calculated to act like a die-box or a raffling-wheel, and to have numbers chosen is a different thing from having them drawn. In fact, it is possible to suggest a certain kind of number-preference by the framing of the question. (p. 116)

³⁴⁶ W. James to E. B. Tichener, May 6, 1899 in James (2000, p. 524).

Similarly, Münsterberg (1899) characterized “the surprising coincidences illustrated by recent experiments” as no more than the product of “the concordance of associations and other similarities of mental dispositions” (p. 76).

Styles of Scientific Thinking

Different scientific styles fuelled the debate between James and Titchener and over its course they explicitly articulated divergent understandings of what constituted proper science. While James offered the opinion “that the experimental evidence for ‘thought-transference’ is lamentably poor in amount, and for the most part in quality, to serve as basis for admitting a phenomenon so subversive of our scientific beliefs,” he held as an exception to this state of affairs the experimental work of the Sidgwicks.³⁴⁷ He characterized their thought-transference study, which had precipitated the debate, as “an excellent model of research.”³⁴⁸ That their findings could not be explained with recourse to known natural laws was a state of affairs the scientifically-minded simply had to accept: “That seems to me the attitude of the truly ‘scientific psychologist’ — ‘ ,’ however anomalous, are worth more than all our theories, however many other facts the latter may explain.”³⁴⁹ For James, in word if not in deed, the role of the scientific psychologist was to accept the facts of mental life as they came, not to interrogate them in light of their own theoretical commitments. Facts, once established, held a privileged position. They were incontestable, while theories were open for revision and rejection as necessary (see also Royce, 1887, p. 228). Of course, in this instance, the facts seemed to affirm James’s own beliefs in psychical phenomena.

³⁴⁷ W. James to E. B. Titchener, May 21, 1899 in James (2000, p. 532).

³⁴⁸ W. James to E. B. Titchener, May 21, 1899 in James (2000, p. 532).

³⁴⁹ W. James to E. B. Titchener, May 21, 1899 in James (2000, p. 532). See also James (1896, p. 885).

Unlike James who regarded the facts procured in psychical research as indisputable, and his simple mathematical calculations as the best manner in which to evaluate the work of Lehmann and Hansen, Titchener privileged other aspects of scientific practice in his analysis. As a committed proponent of the laboratory as the place of scientific psychology, and experimentation as its mode, it was Hansen and Lehmann's methodology that was paramount. Their work was exceptional not simply because its findings refuted telepathy, but because it took place in a psychological, not psychical, laboratory. In conforming to this laboratory ideal, the study was characterized as "both ingeniously devised and rigidly controlled" (Titchener, 1899b, p. 687). As Titchener informed James toward the end of their dispute,

What I like in Lehmann, and what I believe he deserves great credit for, is that he has brought the question into the laboratory. We have left the medium, and the medium's apartments, and the hypnotisings, and the 'good subjects' who have been hypnotised so often before...and we have the bare laboratory walls, and the waking state of scientific scepticism, that we have in all other inquiries.³⁵⁰

In aligning with his adopted style of scientific reasoning, and providing evidence discrediting psychical phenomena, this study served as a prime example of good science for Titchener.³⁵¹ James offered his own experimental results on the matter, but these were decidedly less oriented toward data collection within the authoritative space of the laboratory, than to the collection of

³⁵⁰ E. B. Titchener to W. James, May 28, 1899 in James (2000, p. 536).

³⁵¹ Titchener continued to hold the Hansen and Lehmann study in high esteem well into the twentieth century. When asked by Münsterberg for advice concerning a possible monetary donation to Harvard for psychical studies, he responded that in terms of psychical research: "There are two things that are worth doing: (1) the explanation of mediums – like this new Beulah Miller, and (2) the laboratory experiment of the Lehmann-Hansen type.... if you could make prolonged and exact studies of telepathy, telekinesis, etc., under standard conditions, you would remove a great reproach that is now urged against laboratory men in general" (E. B. Titchener to H. Münsterberg, 2 February 1913, Folder 2191, Item 42, Hugo Münsterberg Papers, Boston Public Library, Boston, MA).

large quantities of material that might be used as a mathematical refutation. While James leaned more toward a taxonomical, or natural historical, style of scientific thinking in the realm of his own psychical research, Titchener was more dogmatically committed to experimentalism across the entirety of scientific endeavours (see Crombie, 1994; Hacking, 1982, 2002; Pickstone, 2001).

Like Titchener, other psychologists similarly upheld experimentation as a privileged methodology. In Jastrow's case, experimentation was valuable not simply because of its habitual location in the controlled space of the laboratory, but also because of what these kinds of studies afforded calculations of probability.³⁵² Thought-transference experiments, in particular, were advantageous since they presented the possibility of "accurately allowing for the effect of chance" (Jastrow, 1900, p. 97). The command of chance afforded by experimentation was presumably unattainable in undertakings like the census of hallucinations and questionnaire research more generally.

Method was paramount in Titchener's evaluation of psychical findings. At the same time, proper method could not be wielded by just anyone, as the qualifications of investigators were also determinative. In December 1898 Titchener gave a talk at Mount Holyoke College in which he refuted the existence of telepathy.³⁵³ Having attended the talk, Gardiner reported that Titchener described believers in telepathy as "either men who knew everything about one subject, but were without scientific training in departments outside their special field...or they were men who knew a great deal about a good many things, but had no thoroughly scientific training in anything" with James falling in the latter category.³⁵⁴ James's lack of sustained

³⁵² Despite the idealization of the laboratory Jastrow, like a number of other early psychologists, undertook work outside this space, particularly in his efforts to debunk psychical phenomena (see Pettit, 2013b).

³⁵³ See H. N. Gardiner to W. James, Jan. 9, 1899 in James (2000, p. 481).

³⁵⁴ H. N. Gardiner to W. James, Jan. 9, 1899 in James (2000, p. 482).

training in the new psychology both explained his credulity and rendered him unqualified to evaluate research. This was similarly the case with Sidgwick, whose commentary on the issue Titchener reportedly observed “wasn’t worth noticing.”³⁵⁵ As Gardiner reported “it would seem...that the only persons who really are competent to form a judgment in the matter are the laboratory psychologists, Lehmann, Hansen, Titchener & Co.”³⁵⁶ In privileging training as what qualified an individual to undertake research and to make definitive assertions regarding the value of findings, Titchener rendered himself – and not James – the arbitrator of truth in these matters.

Titchener’s definitive boundary between those qualified and not to undertake an evaluation of psychical research was reflected in his more general attitude toward the field. Like a number of other new psychologists at the end of the nineteenth century, Titchener was dismissive of the value of psychical research and the reality of spiritualistic phenomena. James’s support for both led Titchener to inform Cattell “his credulity and his appeals to emotion are surely the reverse of scientific.”³⁵⁷ This kind of condescension, also apparent in their more public printed interactions, antagonised James:

I suspect our beloved Tit. to be *lying* in this matter in his rejoinder to me [re. having read James’s and Sidgwick’s criticisms of Hansen and Lehmann], – such being the mode of warfare required probably by his idea of the ‘straight scientific path.’ Your ‘scientist,’ outside a few painfully acquired habits in a limited field, is a big a moral savage as any

³⁵⁵ H. N. Gardiner to W. James, Jan. 9, 1899 in James (2000, p. 481).

³⁵⁶ H. N. Gardiner to W. James, Jan. 9, 1899 in James (2000, p. 482).

³⁵⁷ E. B. Titchener to J. M. Cattell, Nov. (?), 1898, as quoted in Bjork (1983, p. 88).

one else, & no faith need be kept, he thinks, with mere outlaws from the tribe, like psychical researchers.³⁵⁸

Attempts by Titchener and others to depict scientists as constituting a morally superior class were in clear opposition to James's vision of knowledge production as a social activity best engaged in by many kinds of people. This kind of high regard for the work scientists in the realm of psychical investigations was pervasive in the final decades of the nineteenth century, but shifted to include more a more diverse cast of characters, including stage magicians, in the early twentieth century (see Pettit, 2013b). Titchener did not necessarily see James's criticism of the narrow-mindedness of scientists as negative characterizations. Granting "scientific people are one-sided," he responded that psychical researchers were similarly one-sided in their approach and that: "together, with many others, I rather resent the airs of martyrdom that psychical research puts on."³⁵⁹ The practices of psychical researchers and their form of argumentation fell outside his conceptualization of science. In doing so, they were readily dismissed and the forms of knowledge they produced invalidated in the eyes of Titchener and other likeminded scientists.

The scorn directed at psychical researchers irritated James to no end. In the face of his own beliefs regarding knowledge production, the dismissive tone of Titchener and others chaffed: "no part of the unclassed residuum has usually been treated with a more contemptuous scientific disregard than the mass of phenomena generally called *mystical*" (James, 1890h, p. 361). Cattell's vocal scepticism led James to respond, "your state of prejudice is so *absolute*, that quite naively and unconsciously you perpetrate acts of insolence quite as remarkable as your lapses of logic, as if I were some minor or child making a nuisance in the psychological

³⁵⁸ W. James to H. N. Gardiner, Jan. 8, 1899 in James (2000, p. 480).

³⁵⁹ E. B. Titchener to W. James, May 28, 1899 in James (2000, p. 480).

neighbourhood.”³⁶⁰ Categorizing the psychical field as one concerned with “empirical details,” he bemoaned that so many insisted on adopting “the attitude of “authoritative aloofness.””³⁶¹ Deviating from this aloofness was an *Atlantic Monthly* article by Münsterberg (1899) excoriating psychical endeavours, which appeared in the midst of James’s debate with Titchener. Of the piece James remarked, “How clever! but how essentially childish. The insolence of these fellows, sure of the applause of Scientism, whatever they may say, is amusing.”³⁶²

Scientism was clearly problematic, but the term “scientist” also held negative connotations for James. The word, to his mind, suggested a “priggish sectarian view of science, as something against religion, against sentiment, etc.”³⁶³ This was not James’s vision of the proper form of knowledge production. Recognizing the “mechanical rationalism” (James, 1896, p. 886) at the heart of modern science he rejected its dismissal of “the personal and romantic view of life” (James, 1896, p. 887), which included “religious thinking, ethical thinking, poetical thinking, teleological, emotional, sentimental thinking” (James, 1896, p. 886). Knowledge was, or at least could be, deeply personal and “perennially [sic] fed by facts of experience” (James, 1896, p. 887). Consequently, the kinds of collection enterprises engaged in by the ASPR were valuable because they related “to real experiences of persons. These experiences have three characters in common: They are capricious, discontinuous and not easily controlled; they require peculiar persons for their production; their significance seems to be wholly for personal life” (James, 1896, p. 887). There was pragmatic, personal value to knowledge produced outside the bounds of the developing strictures of “science.” One means of corralling this kind of information, while still engaging in the privileged domain of science, was through the adoption

³⁶⁰ W. James to J. M. Cattell, May 8, 1898 in James (2000, pp. 363–4).

³⁶¹ W. James to E. B. Titchener, May 31, 1899 in James (2000, p. 541).

³⁶² W. James to H. N. Gardiner, Jan. 10, 1899 in James (2000, p. 484).

³⁶³ W. James to J. M. Cattell, May 4, 1898 in James (2000, p. 364).

of a style of scientific reasoning other than the experimental. Naturalistically oriented science, such as the circulation of questionnaires, allowed for the collection of the diverse, often personal accounts of experience that James so valued.

Psychical endeavours, and by association their supporters, were disparaged by many but despite this derision James insisted on maintaining friendly relations with those on the opposing side. Ending his dispute with Titchener over the reality of telepathy, he wrote: “after this correspondence, dear Titchenor [sic], we shall meet somewhat as old friends.”³⁶⁴ Similarly, after excoriating Cattell for his dismissal of psychical research, James noted ‘I don’t care personally a rap for the treatment, or think the less well as human beings of the treaters – yourself included.’³⁶⁵ In the case of Münsterberg’s *Atlantic Monthly* article, James simply declined to discuss the matter with him insisting, “I have served my time with *a priori* arguments, and henceforward will only listen to those who bring definite talk about particular facts.”³⁶⁶ The refusal to engage directly in prolonged personal confrontation did not, of course, stop James from expressing his distain to others. Of Münsterberg’s criticisms he insisted he would “leave M’s rot lying in the gutter to decay with other similar garbage, awfully cleverly put as it is.”³⁶⁷ This strategy of maintaining personal relationships even in the face of sustained, and often bitter censure, was yet another aspect of James’s inclusivity. Though he regarded the rigidity of science as problematic, he did not wholly dismiss its results and rather contended “our debt to Science is literally boundless, and our gratitude for what is positive in her teachings must be

³⁶⁴ W. James to E. B. Titchener, May 31, 1899 in James (2000, pp. 541–2).

³⁶⁵ W. James to J. M. Cattell, May 8, 1898 in James (2000, p. 364).

³⁶⁶ W. James to E. B. Titchener, May 31, 1899 in James (2000, p. 541). See also W. James to H. N. Gardiner, June 21, 1899 in James (2000, p. 556).

³⁶⁷ W. James to H. N. Gardiner, June 21, 1899 in James (2000, p. 556).

correspondingly immense" (James, 1896, p. 886). The knowledge produced by science was valuable, but was not to supplant other forms of knowledge.

Conclusion

Whether involving questionnaires or experimentation research into psychical phenomena was a contested matter. Particularly problematic was the interpretation of data produced in these undertakings. Some opted to engage directly with the probabilistic thinking that was central to psychical investigations and framed their critiques within these lines. Others, Titchener included, refused to participate in disputes over the probabilitist particulars of research findings and instead upheld as axiomatic all evidence presented contra psychical phenomena. Though he continued to assert the validity of Hansen and Lehmann's initial conclusions, Titchener never undertook the kind of fact-based analysis James insisted upon. For him and other early American psychologists, including Cattell and Münsterberg, extraordinary experiences of the kind investigated by the ASPR were self-evidently within the bounds of natural law.

The conflicting interpretations offered by James and Titchener in their dispute over telepathy is reflective of their conflicting styles of scientific thinking more generally. James explicitly aimed to include a wider array of individuals in research projects than Titchener's style of science would allow. In his view knowledge was highly personal and necessarily socially inclusive (see Bordogna, 2008). While not opposed to the kind of experimentation Titchener privileged, James was unwilling to limit the scientific enterprise to just this form of practice or to restrict its practitioners to only those with specific training. This kind of public involvement in research was anathema to psychologists who saw the untrained masses as a source of fundamentally untrustworthy information.

Although Titchener opted to engage with psychical research through experimentation on issues at the periphery of the field and advocacy of others' experimental findings other psychologists, especially come the twentieth century, opted for a more direct approach. For these researchers, controlled experiments undertaken by experts, often in concert with those skilled in deception, were to be the final, and only, arbitrators of truth when it came to spiritualism (see Pettit, 2013b; Sommer, 2012). These kinds of efforts at debunking required rejecting earlier assumptions regarding the unsuitableness of psychologists for this work, including Münsterberg's (1899) contention that

daily work in scientific laboratories was a continuous training of an instinctive confidence in the honesty of their cooperators. I do not know another profession in which the suspicion of constant fraud becomes so systematically inhibited as it does in that of the scientist. (p. 78)

Research into the reality of psychical phenomena was circumscribed to efforts by psychical researchers, a group increasingly seen as distinct from psychologists.

In an effort to clearly delineate the boundary between the psychical and the psychological, and assert their professional authority, psychologists insisted that the investigation of psychical phenomena was best pursued "by the same methods and in the same spirit as are other psychological problems" (Jastrow, 1900, p. 55). Making themselves the authority on the genuineness of phenomena, they crafted "an interesting chapter to the natural history of error, showing how readily the emotions carry away the reason, and what a child the layman is before the professional expert in sense-deception" (Jastrow, 1889, p. 81). In doing so, the public's capacity to cultivate knowledge was eroded and, at the same time, phenomena previously understood to be supernatural were naturalized.

Chapter 7

Naturalizing Unconscious Mental Functioning

Normal human consciousness is only a narrow extract from a great sea of possible human consciousness, of whose limits we know nothing. (James, 1898a, p. 194)

In the final decades of the nineteenth century, questionnaires were popular instruments for the investigation of phenomena that straddled the boundaries of “normal” mental life. Taking place alongside studies of explicitly psychical phenomena were inquiries into seemingly more mundane matters. Psychological investigations of dreams and unconscious cerebration, though often presented divorced from psychical concerns, sought to map the full range of the mind’s abilities.³⁶⁸ In doing so, investigators sought to normalize mental functions, and patterns of mental association, that produced experiences often characterized as psychical. Rather than understand extraordinary occurrences as evidence of a genuine supernatural realm, one accessible only through otherworldly channels, psychologists charted the terrain of the mind so as to account for these kinds of experiences in terms of normal mental functioning, whether conscious or otherwise.³⁶⁹ The discipline’s interest in dreams was part of larger cultural interest in the subject during the nineteenth century, and research in this field allowed psychology to craft itself as the authority on the true nature of dream-life. Questionnaires proved a useful, if not sufficient, tool for collecting large bodies of data that encompassed the full range of human mental functioning. Alongside other methodological approaches to psychological science, many

³⁶⁸ For a review of psychological work on dreams in late-nineteenth and early twentieth century periodicals see Vande Kemp (1981); more generally, see Ellenberger (1970), Groth and Lusty (2013), Kroker (2007), and Pick and Roper (2004).

³⁶⁹ For contemporary discussion of the various meanings ascribed to the term subconscious see Münsterberg, Ribot, and Jastrow (1907) and Janet and Prince (1907).

of them also statistically oriented, this kind of research served as a site for differing articulations of what constituted acceptable evidence in the field.

Studies on dreaming, and unconscious mental life more generally, employed a number of methods in a variety of configurations, but were often oriented toward amassing large quantities of data. Although the term “statistics” was a moniker often associated with questionnaire investigations, it was not restricted to these kinds projects. Rather, statistics referred not so much to the collection of information from a great number of individuals, but to large data sets more generally, particularly ones amenable to analysis and classification along various lines. Among the most successful of these endeavours were those involving a restricted range of information, rather than the accumulation of great expanses of diverse descriptive material. These kinds of psychological projects were, I argue, part of an emerging moral economy of data within the new scientific psychology (see Daston, 1995). Large quantities of information served as means of refuting otherwise persuasive personal experiences with, and individual narrative accounts of, dreams and unconscious cerebration that interpreted these facets of mental life as psychical in nature. Research like this served as a form of boundary-work (see Gieryn, 1983), which demarcated psychological science from non-science. Natural, materially grounded mental phenomena, and not superstition-ridden psychical experience, were declared the discipline’s appropriate subject matter, and the latter’s reality was rejected.

In focusing on the plurality of methods at play in dream research, before turning to related efforts to document the occurrence of unconscious cerebration, this chapter locates questionnaire-based investigations within the larger methodological landscape of psychological science in its earliest years. The various research endeavors discussed in this chapter reveal the fuzzy boundary between what are often discussed as distinct forms of practice; those

characterized as introspective, questionnaire-based, or experimental studies. Among investigators, there was no firm agreement on what constituted work within a given methodology. Questionnaire-based projects were rarely isolated endeavours. This style of inquiry, rather than taking place completely divorced from other forms of practice, was but one of many methods applied to various subjects of interest within the new psychology. Within experimental inquiries undertaken by trained professionals small bodies of information were sufficient to form conclusions. In contrast, non-experimental projects, especially those involving the efforts of numerous untrained individuals, necessitated the accumulation of larger quantities of material to carry anything approaching the same weight. Depending on the mode of practice different standards of evidence applied.

Within the context of statistically oriented projects, information generated from only a few individuals served as counterpart to discussions of data amassed from much larger expanses of individuals. While the former served as a means of describing the minutiae of individual experiences in depth, the latter allowed for meaningful discussions of difference between individuals to take place, much as was done in Galton and Osborn's imagery investigations (see Chapter 1). These kinds of statistics were, in part, a means of revealing innate variation in mental life even during periods in which conscious awareness largely lapsed. As such they were part of efforts to produce a "natural history of the normal mind" (Jastrow, 1906, p. 222), one that encompassed both the conscious and unconscious realms.³⁷⁰ This kind of a natural history of the mind naturalized mental functioning, often by rooting it in directly material processes. Dream

³⁷⁰ More general discussion of the psychology of dreams, and the relation between dream-life and consciousness, were also undertaken during this period. This work was part of a broader discourse on dreams, one that involved neither statistical nor experimental studies (Gould, 1889a, 1889b, 1889c; Gulliver, 1880; Oswald, 1889; Redway, 1889; L. Robinson, 1893; Sully, 1889; "Untitled [Review of 'Dreams, sleep, and consciousness' by Geo. M. Gould]," 1889).

research, in particular, offered a site in which to study sensory and perceptual processes and to distinguish between mental life as directly influenced by physical stimuli and the functioning of the, at least theoretically, isolated mind. Employing diverse methodologies, these endeavours offered a counterpoint to more restrictive laboratory-based psychophysical projects on the relation between the physical world and sensation and perception and allowed for direct discussion of the experiential element of unconscious life.

Psychology's interest in dreams was in large part a function of its cultural circumstance. For more than a century prior the appearance of Freud's *Die Traumdeutung (The Interpretation of Dreams)* (1900), dreams were a popular cultural preoccupation, interest in which by no means abated into the twentieth century (Carroy, 2006; Groth & Lusty, 2013).³⁷¹ As Helen Groth and Natalya Lusty (2013) note in their recent cultural history of dreams and modernity, "new modes of writing about dreams emerge from the early decades of the nineteenth century that promised a more systematic approach to the epistemological and ontological conundrums that the multiplicity of dream phenomena generated" (p. 5). During this period dreams became the subject of popular and scientific inquiry. Among the public, dreams were a means to self-understanding as part of the increasing psychologization of the self that took place in the nineteenth and into the twentieth century (see Groth & Lusty, 2013).³⁷² American psychology's interest in dream-life capitalized on existent cultural practices related to the dream, particularly dream interpretation, the recording of dreams in dreams books, and the publication of these volumes. French psychology's similar co-option of cultural practices, with respect to automatic writing, has recently been documented by Alexandra Bacopoulos-Viau (2013). Much like child

³⁷¹ Carroy (2006) positions Freud as very much embedded in the nineteenth century cultural practices associated with dreams, and as a reader of the period's dream literature.

³⁷² The development of modern selfhood, in the French context, is also discussed in Goldstein (2005).

study and spiritualism, dreams – prophetic and otherwise – were of popular interest among the American populace. As a function of this popularity, dreams served as a venue in which psychology could attempt to enlist the assistance of members of the public in their data collection efforts, while asserting its authority over the nature of dream-life at a broad social level.

The Hidden Self

That conscious existence was only a portion of mental life was increasingly recognized in the nineteenth century (see Ellenberger, 1970). Within American psychology one of the most prominent proponents of the *subliminal* self was James for whom consciousness was not singular, but plural. This plurality made itself known in dreams, emotions, and various psychical phenomena (see Taylor, 1995, 1996). James's conviction that there was a subliminal dimension to the mind (James, 1890h) was reinforced through his work on the American census of hallucinations,

I have my self, during the past two years as American agent for the census, collected some five hundred cases of “hallucination” in healthy people. The result is to make me feel that we all have potentially a “subliminal” self, which may make at any time irruption into our ordinary lives. (James, 1892, p. 739)³⁷³

More generally, James's views on the subliminal were influenced by the work of English psychical researcher Frederic Myers, one of the co-authors of *Phantasms of the Living* (Gurney et al., 1886). According to this accounting, consciousness was but a small part of the total psyche. At the subliminal level there existed a whole host of other functions, most notably ones

³⁷³ Some criticisms of *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886), the volume that inspired the international census effort, explicitly tied hallucinatory experiences with dream-consciousness (e.g., Parish, 1897).

responsible for the supernatural phenomena subject to study by psychical researchers. More so, the subliminal in Myers's view, and as taken up by James, was a source of spontaneous variations and thus played a vital role in the evolution of consciousness. Dissociative states, such as those of hysterics, while possibly degenerative also offered a valuable opportunity for new aggregations of personality (see Sech Junior, 2013; Taves, 1999). In this scheme mental variation, at both the conscious and unconscious levels, was a crucial element to be explored.

Although James was drawn in by Myers's description of the subliminal as an evolutionarily advantageous realm responsible for psychical phenomena, other psychologists were less enthusiastic. Jastrow, though he noted Myers's work in his own account of the *subconscious*, rejected this view of the mind's depths. Unsurprisingly, this dismissal was largely based on Myers's endowment of the subliminal with extraordinary powers (Jastrow, 1906, 1930; see Taves, 1999). Psychical researchers like James and Myers saw psychical phenomena as genuine products of a subliminal mind, which possessed supernatural knowledge inaccessible to normal consciousness. For instance, James (1890h) reported that he knew a non-hysterical woman who, in her trances, knows facts which altogether transcend her *possible* normal consciousness, facts about the lives of people who she never saw or heard before. I am well aware of all the liabilities to which this statement exposes me, and I make it deliberately, having practically no doubt whatever of its truth. (p. 373)

As James intimated, new scientific psychologists increasingly rejected the reality of psychical phenomena. Jastrow contended that these kinds of experiences were better accounted for by operations of the subconscious mind and, critically, that mental functioning at this level was a normal, natural state and not the result of a subconscious conduit to the supernatural.

Documenting Dream-life

In his “systematic exposition of subconscious functioning,” presented in *The Subconscious* (Jastrow, 1906, p. viii), Jastrow repeatedly turned to the subject of dreams. This interest in dreams can be traced at least as far back to his questionnaire-based work on “The Dreams of the Blind” undertaken in the mid-1880s.³⁷⁴ First presented to the Biological section of the American Association for the Advancement of Science in 1886 (“Dreams of the blind,” 1886, “Notes in science and industry,” 1887, “The dreams of the blind,” 1886), the research appeared in print two years later.³⁷⁵ Given the pervasive interest in dreams on the part of the American public, the topic of dreams in the blind in general and Jastrow’s particular study of the subject attracted discussion in popular periodicals (Buckley, 1888; “Curious facts about dreams,” 1888, “Dreams of the blind,” 1886, “Notes in science and industry,” 1887, “Review 4,” 1888, “The dreams of the blind,” 1886). Jastrow’s work, which was published in *The New Princeton Review*, was one of the only endeavors by the new psychology to reference Osborn’s mental imagery investigations (see Chapter 1).³⁷⁶ For Jastrow, Osborn’s inquiry into imagery was especially relevant. It was not simply dream-life writ large that Jastrow (1888d, 1900) sought to interrogate

³⁷⁴ Jastrow’s interest in the blind persisted through the 1890s (Jastrow, 1894b) and into the twentieth century (Jastrow, 1900, 1906). He later initiated another questionnaire-based study, on logical reasoning, by reprinting within the pages of *Science* a logical statement along with a series of questions (Jastrow, 1897a). Readers were asked regarding the logical statement:

Is this reasoning correct or is it not? If regarded as correct, my request is to have the reasons for its correctness given as explicitly as possible. If it is regarded as incorrect, I wish in the same way a very explicit statement of the nature of the error. Answers are requested from all who are interested in the matter. I am particularly desirous of receiving replies from those whose interest in thought is a philosophical one, as well as from those who are more specially devoted to scientific pursuits. (Jastrow, 1897a, p. 26)

What came of this inquiry is unclear.

³⁷⁵ Interest in the dreams of the blind was not confined to inquiries undertaken by psychologists (e.g., “Blind men’s dreams,” 1886).

³⁷⁶ Reviews of Jastrow’s “The Dreams of the Blind” include Ribot (1888) and an anonymous review in the *American Journal of Psychology* (“Untitled [Review of ‘The dreams of the blind’ by Joseph Jastrow],” 1888). A circular based inquiry into sleep and dreams was also undertaken in Russia near this time (see “Statistics of Sleep and Dreams,” 1889).

but rather the visual character of dreams. Beyond his knowledge of the questionnaire-based inquiries of Osborn, and the work of Galton they were based upon (see Jastrow, 1888d), Jastrow was certainly aware of, and influenced by, Galton's larger body of work (Jastrow, 1885a, 1898; see Pettit, 2013b). This, together with his earlier work with Peirce (see Chapter 6), oriented him toward the probabilistic nature of the universe and the import of individual variation. His decision to attend to the visual element in dreaming was in line with the view expressed by Galton (1883) that the "faculty of visualising... supplies the material out of which dreams and the well-known hallucinations of sick people are built" (p. 84-5). Untangling dream-life from its association with meaningful hallucinatory experiences and the superstition of premonitory dreams was one of the recurrent features of larger body of dream research undertaken outside of directly psychical realms.

In Jastrow's case, the blind were an ideal population in which to study the visual character of dreams.³⁷⁷ Seeking to ascertain the developmental period of visual stimulation necessary for an individual to continue to possess the power of visualization, even after the loss of vision, he turned to individuals in institutions for the blind in Philadelphia and Baltimore.³⁷⁸

As he saw it,

if we accept as the test of the independent existence of the sight-centre its automatic excitation in dreams, the question can be answered by determining the age of the onset of

³⁷⁷ The blind were also a group investigated by a number of others, notably in the study of deaf-mute Laura Bridgman, which continued even after her death with anatomical studies of her brain (H. H. Donaldson, 1890; Hall, 1879; Sanford, 1886a, 1886b; "The brain of Laura Bridgman," 1891).

³⁷⁸ These were convenient locales for Jastrow. He spent most of his childhood in Philadelphia, after immigrating to the United States from Poland as a young child, and earned his undergraduate degree from the University of Pennsylvania in 1882. After obtaining his doctorate from Johns Hopkins in 1886, he was at least nominally a fellow at Johns Hopkins from 1886 to the spring of 1888. During this period he conducted some research, including this work on dreams, and wrote for pieces for *Science*, as well as popular magazines (see Jastrow, 1930).

blindness which divides those who still retain in their dreams the images derived from the world of sight, from those who do not. (Jastrow, 1888d, p. 21)

This project was one of many late-nineteenth and early twentieth century studies that sought information on dreams, including a number that focused particularly on the role of sensory and perceptual factors in dream-life. The importance of investigating the visual element of dreams, over other sensory dreams, was predicated, in part, on Jastrow's (1888a) conviction that "man is a visual animal; as a race we are eye-minded" (p. 598).³⁷⁹

To determine the degree of visualization in dreams of the blind required securing not only the cooperation of the blind themselves, but also that of "the authorities and teachers" (Jastrow, 1888d, p. 21) at the institutions within which they resided. Success on this front gave Jastrow access to a group of 183 blind individuals of both sexes. These individuals were personally examined, their sight tested, and the age and date at which their sight was lost recorded. Most importantly, each individual was asked "quite a long series of questions" (Jastrow, 1888d, p. 21) on their dreaming habits, beginning with the simply query "Do you dream?" (Jastrow, 1888d, p. 27). The exact procedure for administering these questions is unclear, but given the visual impairment of his subjects Jastrow undoubtedly posed the questions in his lengthy series to subjects orally, rather than providing them with a written list of questions to answer, as was, at least sometimes, the case in other questionnaire investigations.

Taking place contemporaneously with Jastrow's initial inquiry into dreams was a long-term project at dream collection by another Johns Hopkins graduate, Julius Nelson. After receiving his doctorate in biology from Johns Hopkins University in 1888 Nelson was appointed Professor of Biology of Rutgers College, later Rutgers University (Hanes, 2008). The same year

³⁷⁹ An allusion to Aristotle's *Metaphysics*.

as he completed his graduate studies, Nelson (1888) published a piece in Hall's newly founded *American Journal of Psychology* on his efforts to record his dreams and relate them to other facets of his life.³⁸⁰ Since beginning his project in November 1884 he had "accumulated the records of over one thousand dreams per year" (Nelson, 1888, p. 374). The intent of this endeavour, as he described it, was "the study of dreams as a convenient portal to the general subject of hallucinations, and with the hope of adding to our knowlege [sic] of this most fascinating field of psychology" (Nelson, 1888, p. 374). Despite this expressed aim Nelson had little interest in the content of dreams. So far as he was concerned,

the fact that a person dreams much or little is of more significance than *what* one dreams.

A curve representing the variations from day to day in the amount of dreaming has scientific interest, while the hobgoblins that we saw are of interest to children. (Nelson, 1888, p. 375)

Rather than attending to the content of dreams, Nelson used his extensive dream records to chart the relation between amount of dreaming and sexual life. Focusing particularly on nocturnal emissions, which he referred to as *gonekbole*, he found a direct correspondence between these and quantity of dreaming. In opting to focus on sexual rhythms and dreaming, Nelson explicitly pursued a line of inquiry that connected psychological and physiological functions, one he hoped might "stimulate others to study their own dreams" (Nelson, 1888, p. 374).

Efforts to understand dream-life through the collection of masses of information on one's own dreams were by no means confined to attempts to link dreams and sexual habits.³⁸¹ While

³⁸⁰ Nelson's decision to publish in the *American Journal of Psychology* was likely a result of, at the very least, an awareness of Hall as a consequence of their overlapping tenure at Johns Hopkins (see Hanes, 2008).

³⁸¹ Some later experimental studies in psychology inquired more into sleep states than dream-life per se (e.g., Sidis, 1908a, 1908b, 1908c).

studying the new laboratory psychology with Edmund Sanford at Clark University in the 1890s, Mary Whiton Calkins undertook a project on dream-life in which both she and Sanford recorded their dreams over the course of a number of weeks. Calkins (1893b) explicitly identified her dream project as an experimental endeavour, one that, given the collection of large quantities of dreaming data, was also described as a project on the “Statistics of Dreams.” The latter characterization was perhaps more apt as it was the collection of a bulk of information on dream-life that was the central feature of this work. In all Sanford recorded 170 dreams over 46 nights and Calkins 205 dreams over 55 nights, an average of roughly four dreams per night.

Collecting dreams proved a challenge. Calkins and Sanford recorded their dreams by making notes “during the night...supplemented by careful study and recollection on the following day” (Calkins, 1893b, p. 311).³⁸² This was hardly a foolproof process, as Calkins herself noted. Memory was especially fallible with respect to dreams, which were often quickly forgotten the next day. If not recorded in detail during the night, dreams were easily lost as was illustrated when

the account of one dream, apparently of peculiar significance, was written out in the dark by the experimenter, who then sank off to sleep with the peaceful consciousness of a scientific duty well done. In the morning the discovery was made that an un-sharpened pencil had been used, and the experimenter was left with a blank sheet of paper and no remotest memory of the dream. (Calkins, 1893b, p. 312)

³⁸² Several years later seven of Calkins’s students at Wellesley conducted “A Study of the Dream-Consciousness,” following the same method, in which they recorded their dreams over the course of 5 to 6 weeks (Weed & Hallam, 1896).

Reliance on memory meant what was produced from these efforts was not a record of the entirety of dream-life, but rather of recollected dream-life. The accounts produced by Calkins and Sanford were merely a partial record of the activity of this level of the mind.

The aim of collecting accounts of dreams was, in part, to document what associations existed between the content of dreams and life events. Doing so was part of Calkins's interest in association more generally (see Calkins, 1896a, 1896b, 1898; Münsterberg et al., 1894).³⁸³ That dreams could regularly be traced back to the events of waking life was seen as evidence of "the essential congruity and continuity of the dream life with the waking life" (Calkins, 1893b, p. 323). Nonetheless, Calkins (1893b) was able to identify three distinguishing features of dream-life: "the comparative feebleness of the attention and the will, the want of discrimination, and the relative lack of perception" (p. 334).

Calkins's analysis of dreams, like Jastrow's endeavour, also paid particular attention to the role of sense-perceptions in dreaming.³⁸⁴ This involved documenting to what extent dreams included, for instance, visual, auditory, tactile, and gustatory sensations. The analysis led Calkins (1893b) to contend

dreams may be most conveniently classified according as they are connected through sense excitation with the immediate present, or through the fact of association with the waking life of the past, according, that is, as they contain presentations, or merely representations. Or, in physiological terms, dreams are occasioned or accompanied by peripheral or by merely cerebral excitation. (p. 319)

³⁸³ Others were similarly interested in associations in dream-life, including Robert Woodworth (1897) who conducted a study of the rapidity of associations in dreams.

³⁸⁴ On visual and other sensory elements in dream-life see also Ladd (1892) and Stanley (1899).

This is to say, sensory experiences during dreaming were categorized as the result of one of two circumstances; either the direct excitation of a sensory organ, just as in waking life, or the mere mental representation of previous sensory experiences. These were termed *presentative* and *representative* dreams, respectively. In pointing to differences in the physiological processes underlying dreams Calkins, much like Nelson, deliberately linked psychological experience with physiological states, though in a much different manner. While Nelson focused on sexual functioning Calkins, who was clearly much more firmly rooted in the new psychology, tied her work to sensation and perception, the key subjects of experimental projects in the developing science.

The role of sense-perception in dreaming was also investigated by others at this time. Titchener (1895b) expressed an interest in the subject in the mid-1890s, publishing a brief report of five cases of taste dreams. Of these, only two were documented in detail, including one specifically collected following printed notice, within the pages of the *Philosophical Review*, of J. Clark Murray's "Do We Ever Dream of Tasting?," a paper presented at the 1893 meeting of the American Psychological Association (see "Notes," 1894). Murray, a philosopher at McGill University, asserted in his presentation that "it remains doubtful whether, when there is no real 'taste in the mouth' at the time, the dreamer ever revives a past taste in mere idea" contending instead that "when one dreams of tasting, the taste is not a dream-*image*, but a dream-*sensation*" ("III. Second annual meeting," 1894, p. 20); that is, taste dreams were always the result of "peripheral excitement in the organ" ("III. Second annual meeting," 1894, p. 20) which produced taste sensations just like any other and thus presentative dreams. For Titchener (1895b) this was an overly rash conclusion: "it does not seem justifiable to single out the taste center as not centrally excitable, because it is so very easily excitable peripherally" (p. 507).

Following in the footsteps of Calkins's work on dreams, Titchener set out to test the idea that true taste dreams, ones purely the result of cerebral activation, were possible. To do so he not only gathered information from several individuals on their experiences of taste dreams, but also performed a short series of experiments on his own dream-life (Titchener, 1895b). This small set of collected material served to refute Murray's contention that representative taste dreams never took place. Over the course of three evenings in late January 1895 he sought to bring about his own taste dreams through auto-suggestion prior to sleep, finally succeeding on the third night. For these experiments "every precaution was taken to avoid the occurrence of a presentation dream; the mouth thoroughly washed out, etc." (Titchener, 1895b, p. 507). Having ruled out immediate sensory information as the root of his taste dream Titchener was free to contend that at least some taste dreams were the result of solely cerebral excitation. Beyond this, like Calkins, he was able to trace "the associative connections between the dream and events of the waking life ... with rather exceptional completeness" (Titchener, 1895b, p. 508).

Experimentation of this kind on dream-life was also taken up with larger groups of individuals. At the State Normal School at Westfield Massachusetts Professor of Psychology Will Monroe, inspired by the recently reported work of Norwegian philosopher John Mourly Vold (see Warren, 1897), initiated a study of visual stimuli on dreams. Over the course of ten successive nights 14 female students in his senior psychology course recorded their dreams after looking at coloured stimuli in a variety of shapes (e.g., a red square, violet heart, light blue cat, etc.) for between two and ten minutes immediately prior to sleep (Monroe, 1898). An analysis of the 221 dreams collected showed little correspondence between stimuli and dreams, with only 18 dreams featuring the colour and 11 the form of a stimulus. A subsequent study of taste dreams was also undertaken with twenty female students at the school (Monroe, 1899). For ten

successive nights, just prior to bed, students were required to wash their mouths out and place a crushed clove upon their tongue. In comparing dreams across the two endeavours Monroe focused particularly on differences in dreams reported of each sensory modality, noting the somewhat larger proportion of gustatory and olfactory dreams in the taste study.³⁸⁵ Other efforts at experimentation on dream consciousness were also undertaken, including work by Barnard College mathematician Grace Andrews, a former Wellesley student. Andrews, in 1900, published a study of dreams undertaken at her alma mater. Six Wellesley students, including Andrews, recorded their dreams for four nights. Prior to sleep the participants looked at squares of coloured glass and paper for five minutes and then at two lithographs for a further ten minutes. The experiment failed to produce results as it was cut short after only four nights because of Andrews's (1900) dissatisfaction with the quality of the data collected and her distrust of the value of the experimental set up as whole.

Clark Questionnaires

Far simpler than attempting to experimentally induce dream content, was the survey of the subject via questionnaire. At Clark University, as was the case with so many other topics (see Chapter 3), a number of dream related questionnaires were produced in the 1890s and into the early twentieth century. These include: "Fancy, Imagination, Reverie" by E. H. Lindley, December 1895;³⁸⁶ "Reveries and Allied Phenomena" by G. E. Partridge, April 1897,³⁸⁷

³⁸⁵ Monroe (1899), like most investigators, found dream content was often best attributed to associations with life experiences.

³⁸⁶ "Fancy, Imagination, Reverie" Dec. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See also Lindley and Partridge (1897) and Hall (1924). Lindley and Partridge's (1897) report on mental automatisms, based on some of the responses to the syllabus on "Fancy, Imagination, Reverie," was briefly reviewed by Thorndike (1898d) as part of a series of reviews of the results of Clark questionnaires (Thorndike, 1898a, 1898b, 1898c, 1898e).

“Dreams” by James Ralph Jewell, October 1903;³⁸⁸ “Special Defects” by Augusta Wiggam, November 1908;³⁸⁹ “Study in Dreams” by Raymond Bellamy, November 1912;³⁹⁰ and “Dreams” by Vance Randolph, November 1914.³⁹¹ Much broader in scope than other investigations of dreams, these inquiries also focused less on the relation between the physical stimuli and sensation and perception and more on various forms of difference between groups, as was so often the case in questionnaire research where these kinds of comparisons were possible. Like much of the questionnaire work at Clark, these studies largely focused on pedagogical or developmental issues, with children, or at least childhood, the frequent locus of the inquiry.³⁹²

Even though these questionnaire-based projects largely eschewed experimentation there was an attempt by some to use the two methods in tandem. Jewell’s study into dreams, which began in 1903, was such a broad inquiry into the subject that one individual who took up data collection for the project felt free to pursue a related, but not explicitly specified, line of inquiry. In much the same vein as earlier dream experimentation this teacher “before giving her pupils this questionnaire … asked them to look intently at a bright object, red preferably, for some minutes before retiring, to see if it would influence their dreams” (Jewell, 1905, p. 2). In line with the previous findings of Monroe and Andrews, the visual stimuli had little influence on

³⁸⁷ “Reveries and Allied Phenomena,” April 1897, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. See also Partridge (1897) and Hall (1924).

³⁸⁸ “Dreams,” Oct. 1903, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Although Jewell (1905) published on the results of this questionnaire investigation, and claimed the project as his own in print, his name is not listed on the circular (see Hall, 1924). For a review of Jewell (1905) see Manchester (1906). Jewell (1906a) later completed his doctorate at Clark on nature study (see also Jewell, 1906b, 1907).

³⁸⁹ See Wiggam (1909) and Hall (1924).

³⁹⁰ See Bellamy (1915a, 1915b), Hall (1924), and Allen (1970).

³⁹¹ See Randolph (1915) and Hall (1924). See also Randolph (1925) and Halpert (1981).

³⁹² Later investigations, notably ones undertaken following Sigmund Freud’s visit to Clark in 1909 (Evans & Koelsch, 1985), often approached the discussion of dreams from a psychoanalytic perspective (e.g., Bellamy, 1915a, 1915b; Randolph, 1915).

dream-life. Lindley's 1895 questionnaire on "Fancy, Imagination, Reverie," which was by no means solely focused on dreams, also blurred the boundaries between the circular method and experimentation requesting:

Will you (a) kindly sit in a *perfectly* dark room, or else bandage, shut or cover the eyes in some way to exclude *all* light, but not to press the eye, and sit for five minutes in quiet, attending to the field of vision, and then write down consciously and in order the visual impressions you had, describing them fully; (b) stop the ears very firmly and constantly also for five minutes, and write what you seem to hear?³⁹³

These experiments, like those undertaken by others, focused on isolating the influence of physical stimuli on sensation and perception.

Although experimental investigations of dreams often made note of the large quantity of dreams recorded by a small number of individuals, questionnaire-based studies emphasized that these accounts of dreams were provided by a great many individuals. Of his discussion of 2,000 dreams collected from roughly 800 people, through both the circulation of a questionnaire on dreams and the dream diaries kept by himself and some friends, Jewell (1905) contended "no study of dreams from such a mass of data has ever been made before" (p. 2). Other Clark studies were similarly reliant on large quantities of data (e.g., Lindley & Partridge, 1897; Partridge, 1897; Wiggam, 1909), though Jewell collected by far the most extensive set of dream data. Unlike experimental investigations of dreaming, which tended to rely on large quantities of dream records from only a few individuals, questionnaires had the unique advantage of surveying a multitude of individuals and thus recording the diversity not only of dreams, but also of dreamers. Here, unlike in more experimentally oriented endeavours, it was the variation made

³⁹³ "Fancy, Imagination, Reverie," Dec. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

visible in the mass of material that was of central import, rather than the isolation of pure dream-life.

Trust and Training

Across inquiries into dream-life trust in, and the training of, participants were of central concern. Investigations that relied on the report of only one or two individuals, as well as those that gathered information from large numbers of individuals were often the ones least likely to comment on the difficulties of data collection. In the case of Calkins's project she and her data-collecting counterpart, Sanford, were positioned as trained practitioners ideally suited to the task of reporting the details of their dream-life. As such they fit well within the Wundtian model of the trained psychologist as psychological subject (Danziger, 1985, 1990), even if their undertakings occurred outside of laboratory spaces. On the other hand, the questionnaire-based inquiries produced at Clark relied in large part not on trained investigators, but rather on masses of untrained individuals who were asked to report on their dream-life. Their work thus adhered not to the Wundtian program, but rather to a Galtonian style of research, which emphasized populations over individuals (Danziger, 1985, 1990). In both types of endeavours the lack of commentary on the qualifications of those reporting their dreams is a reflection of their self-evident status as trained and untrained, respectively. It need not be said that Calkins and Sanford were psychological experts.³⁹⁴ And in much the same way it need not be stated – a decade or more into Hall's program of questionnaire-based research – that those supplying the material in these endeavors were psychological lay persons. These individuals remained largely anonymous, but some of the more productive intermediary data collectors were singled out for recognition.

³⁹⁴ Calkins was at this point still in the early stages of her training in the new psychology, though may still be understood as possessing a degree of expertise lacking in lay respondents (see Chapter 2).

For instance, as was standard practice for those publishing questionnaire-based investigations, especially those doing so out of Clark, Jewell explicitly thanked all those who aided with data collection.³⁹⁵

The combination of the involvement of only a handful of individuals and a limited body of collected material, however, often merited explicit commentary on the qualifications of respondents. In the case of Titchener's (1895b) report on taste dreams, he explicitly characterized the five individuals providing him with accounts of representative taste dreams as "trustworthy" (p. 508). Titchener's assertion aligns him with the well-documented ideal of cultivating trustworthy selves as a component of scientific practice during this period (Daston & Galison, 2010; T. M. Porter, 1995). That those providing him with information could be trusted was particularly important given his small body of collected data and the untrained status of those individuals providing information. The undertaking's aim of countering claims that all taste dreams were presentative, that is simply the result of sense-perception, further necessitated such assurances. Emphasis on the trustworthiness of the individuals from whom he collected instances of representative taste dreams was further reinforced in the sole description of another individual's dream-life actually provided in his report. It is left unclear who the other individuals reporting experiences of representative taste dreams are, with the exception of one Reverend A. Beele of Alfred, Maine (see Titchener, 1895b, p. 508). Although presumably not trained in the new psychology, it is difficult to think of any other class of individual who would fit Titchener's category of "trustworthy" better. If the account of a man of god could not be trusted, just whose word might one trust? At the same time, accompanying the description of Beele's taste dream

³⁹⁵ Those who lent their assistance to the investigation included a number of individuals with longstanding ties to the institution, among them Lillie Williams, Margaret Smith, Norman Triplett, Frederick Tracy, and G. E. Partridge, the latter of whom had earlier conducted his own work on dreams at Clark (see Jewell, 1905, p. 2 fn.; Lindley & Partridge, 1897; Partridge, 1897).

was a description of Titchener's experiments on his own dreams. Beele's status as a man of god ensured the trustworthiness of his report, but it was Titchener's status as a trained psychologist that served as a mark of his credibility. This scientific standing and its attendant trustworthy status, allowed Titchener to certify that his other amassed taste dreams, even though left undescribed, were "trustworthy" accounts. These kinds of assurances were essential to the statement of definitive conclusions on the topic.

Other attempts to amass accounts of dream-life from a handful of individuals faced similar challenges regarding the trustworthiness of collected data. Andrews's effort to gather information on dream-life at Wellesley relied on reports from just six students at the College, including Andrews herself. The project was cut short after only four nights due to the poor quality of the data, a result of what Andrews (1900) identified as a lack of training on the part of the other participants, as she was the only one, as she put it, "trained in dream introspection" (p. 131). The study's participants, herself excluded, failed to live up to the ideal of the trained psychological subject at the heart of the discipline's idealized Wundtian paradigm (Danziger, 1985, 1990). Involving untrained students in the project of recording dream-life was an easy means of undertaking research, but it was also problematic from a scientific standpoint. The problems of untrained data collection, however, only became apparent once the study was underway: "the inexperienced observers obviously required training in the observation and record of their normal dreams before the attempt to vary experimentally the conditions of their dreaming" (Andrews, 1900, p. 131). One solution to this difficulty would be to embed the participants in the Wundtian model through further training. More generally, she found the entire experimental set up for the investigation of dreams problematic. What was really needed, Andrews (1900) contended, was "the excitation of the dreamer through auditory, olfactory or

dermal stimuli, applied by the experimenter at different periods during the night" (p. 132).³⁹⁶

These aspirations were in line with the laboratory projects of psychophysics, whereby physical stimuli were directly tied to their sensory and perceptual counterparts. Applying these conditions in the context of dream research was extremely difficult, however, and not something Andrews pursued.

Even if the model experimental situation could be achieved in dream investigations, Andrews remain skeptical of the value of this kind of research. In her estimation, there is the disadvantage attendant upon all investigation in which a person experiments upon himself; there is the further objection that stimulation and supposed result are separated by a considerable interval of time; and, finally, there is the lack of any observer of the attendant conditions. (Andrews, 1900, p. 132)

Whether these difficulties could ever be overcome in the context of dream investigations was doubtful. Before one could even think of experimentally manipulating the conditions of dreaming in the way Andrews sought to do, those charged with recording their dreams needed more basic training in the method of dream introspection. In the absence of this kind of training, collected data was not simply deficient, but useless. Training might solve some difficulties associated with the project, but the nature of dreams was such that it was doubtful that research into the subject could ever meet Andrews' understanding of the technical standards of the field.

Physical States and Dream-Life

Given the challenges associated with investigations of dreams as a function of their very nature – and thus the improbability of ever achieving the rigorous standardization associated with experimental laboratory practice – a different research ideal was needed. One means of

³⁹⁶ For a personal account of the influence of external stimulation on dream-life see Baldwin (1888).

avoiding some of the difficulties identified by Andrews was to look at existing populations as a form of natural experiment. Jastrow's effort to link visual dreams of the blind with specific brain functioning was just this kind of an endeavour. Outside of this special population, he was cognizant of the pervasiveness of presentative dreams contending: "the feel of our body and the fluctuations of its condition surge up more distinctly as we dismiss the outer world; and their presentative dominance furnishes an ingredient in the stuff that dreams are made of" (Jastrow, 1906, p. 473). Examples of presentative dreams were pervasive, but irrefutably documenting the existence of representative dreams was much more difficult. Studying the dreams of the blind was a means of recording truly representative dreams, while also emphasizing "many points of interest in the nature and development of the cortical centres of the human brain" (Jastrow, 1888d, p. 33). Charting the continuing existence of visual dreams also offered compelling evidence of the developmental trajectory of the brain. Those who continued to experience visual dreams after their loss of sight were assumed to have the cortical centres functionally necessary for vision even though their perceptual apparatus was impaired. Moreover, continuing experiences of visual dreams following the onset of blindness was evidence of the occurrence of truly representative dreams. Though external visual stimulation could no longer have an influencing effect on dreams for the blind, this was not the case for dreamers more generally. It was only in the context of studies with this population that this factor could be controlled for and these kinds of assertions made with definiteness. Here, ostensibly, was perception fully divorced from physical stimulation.

Easier to accumulate than the precise determinants of physical stimulation on dream-life that Jastrow pursued were accounts of connections between the particulars of dream-life and specific physical states at the time of dreaming. Among questionnaire-based projects these kinds

of associations were sought by Jewell, whose circular on dreams specifically asked after the physical state of dreamers. For instance, respondents were asked “Can you discriminate between psychological and physical causes?” and “Give temperament and physical condition” (Jewell, 1905, p. 1).³⁹⁷ In discussing his findings several commonly associated dreams and physical states were singled out. These included the tendency for children to experience nightmares when overtired (see Jewell, 1905, p. 8), as well as the conclusion that dreams of smothering could be attributed to the “inability to get sufficient air to breathe” (Jewell, 1905, p. 9) at the time of dreaming. These findings aligned well with Calkins’s scheme of presentative (those that result from physical stimuli) and representative dreams (those that result from cerebral excitation due to memory association), which Jewell (1905) like other researchers adopted. Collecting accounts of correspondences between dream-life and physical states via questionnaire avoided the inherent difficulties of experimentation on dream-life.³⁹⁸

Altered States of Consciousness

Both discussion of dreaming also provided an avenue for discussion of altered states of consciousness. That dream-life could be influenced by suggestion was an open topic of discussion, both in questionnaire investigations and in more experimental work. In the case of daydreams, this conversation moved beyond efforts to document suggestion’s effect on the

³⁹⁷ “Dreams,” Oct. 1903, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

³⁹⁸ On the psychic front he similarly identified two causes of dreams: “(1) memory and association, and (2) suggestion” (Jewell, 1905, p. 9). The questionnaires produced by Lindley, Partridge, and Jewell all also took up, in addition to dreaming proper, the subject of daydreams. So far as Partridge (1897) was concerned daydreaming is “a habit common to normal minds” (p. 460). It was, in fact, an evolutionarily advantageous feature of mental life that provided a means of “adapting to new conditions of life.... In its dreams the mind tries all things, enters all walks of life, forms new associations, and, as in the race, these ideals represent the hopes and longings upward of the soul” (Partridge, 1897, p. 460). Like dreaming more generally, he linked daydreaming with specific physical states, particularly muscular relaxation.

content or quality of dreams (visual, auditory, etc.) to discussion of hypnotic states. Lindley's circular on "Fancy, Imagination, Reverie" asked

Describe any cases, whether in yourself or others, of reverie or day-dreaming, when the mind was vacant, far away, wool gathering, castle building, etc. Just what was the mind doing; how did you or the person observed, look, act, feel; what condition favors this state and how came the awakening? Do you think it is like hypnagogic phenomena, or those images experienced just as we are falling asleep? Do you deem them hypnotic?³⁹⁹

Jewell likewise saw parallels between dreaming and hypnotic states, but deemed the influence of suggestion as severely limited. In dreaming as in hypnosis "only the beginning of any certain mental content can be brought about by suggestion, – once induced, association takes its natural course, and repeated suggestions are necessary to direct it along desired lines" (Jewell, 1905, p. 2). This, in part, explained the difficulties of experimental efforts to induce dream content with exposure to various sensory stimuli. It was association rather than suggestion that ruled the dreaming mind.

Inquiries also positioned dreaming as a state comparable, if not identical, to altered states of consciousness induced by various chemical compounds. As a follow-up to Lindley's earlier effort, Partridge's questionnaire "Reveries and Allied Phenomena" asked individuals to report on the "effect of any kind of drug or narcotic upon reverie or any spontaneous actions of sense or mind. Describe any experiences you may have had with ether, gas, belladonna, laudanum, paregoric, mescal, opium, etc.; effects of food and drinks."⁴⁰⁰ Along similar lines, nearly two

³⁹⁹ "Fancy, Imagination, Reverie," Dec. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁴⁰⁰ "Reveries and Allied Phenomena," April 1897, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. Like research into dream states, interest in consciousness

decades after his investigation of dreams of the blind, Jastrow proposed a new inquiry. Rather than the blind, this investigation targeted to those under the influence of mild forms of anaesthesia. It was hoped that in collecting information of this kind insight would be gained into a “range of subconscious mental states, including simple states of distraction, absentmindedness, reverie, trance, hypnosis, dreams, the actions of drugs, alterations of personality, lapses of memory, states of confusion, and the reactions to anaesthetics” (Jastrow, 1905, p. 327). Since psychologists did not have ready access to those under anaesthesia he appealed “to those who are professionally engaged in their production, to step aside from their main interests to supply in a spirit of coöperation the data so valuable to students of a different and yet not unrelated science” (Jastrow, 1905, p. 327). With the cooperation of surgeons and anaesthetizers he hoped especially to explore congruencies between anaesthetic and hypnotic states, as demonstrated through suggestibility and automatic activities, as well as similarities between such states and dream-life. To ascertain this information he requested that “a patient be questioned as to what occupied his mind up to the moment of losing consciousness and again during the regaining of full consciousness” so that “a valuable collection of data regarding the waning and waxing states of consciousness” (Jastrow, 1905, p. 326) could be obtained. The relationship between anaesthetic phenomena and the “normal, waking, mental traits of the subject” was to be determined through “skillful questioning controlled, where possible, by ingenious tests”; these it was thought, “will be the most effective instrument of inquiry” (Jastrow, 1905, p. 325). This questionning was to be guided by Jastrow’s expressed interests in the subject, but no formal set of questions was provided. Though he later wrote of the experiences of those under anaesthesia it is unclear whether this discussion was informed by this effort at data collection and, indeed, if his request

while under the influence of various chemical compounds was also pursued through auto-experimentation (e.g., Cattell, 1980; James, 1898a; see Schmied, Steinberg, & Sykes, 2006).

for information met with any degree of success (see Jastrow, 1906). Across these kinds of inquiries, it seems, just what took place during various altered states of conscious proved easier asked than answered.

Differences in Dreamers

In the case of statistically motivated experimental work, like that of Calkins (1893b), variety in dreams could be documented but differences between classes of dreamers were impossible to enumerate because of the limited pool of participants. On the other hand, those who opted to investigate dream-life via questionnaire deliberately set out to survey a large, and hopefully diverse, body of individuals. These statistically oriented investigations, unlike their experimental counterparts, provided a means of ascertaining various forms of difference between dreamers. The overall aim of dream research, in its various guises, was made explicit by Jastrow (1906): “the natural history of dreaming requires equally that its distinctive traits, the differentia of its species as well as its community with the genus, shall be discerningly noted” (p. 211). The totality of the nature of dream-life could only be ascertained through broad inquiries of the kind most often undertaken with questionnaires. In rendering dreaming an object of scientific inquiry dreams were naturalized so that psychical and folk interpretations, often rooted in intimate personal experience, were invalidated as evidence of the nature of dreams. Psychological investigations of dreams, often reliant on the collection of masses of material, were instead positioned as authoritative. Through these inquiries dreams were rooted in material processes, rather than a supernatural realm.

In the case of Jastrow’s inquiry into dreams of the blind, a very particular form of difference was positioned as central. However, the study was decidedly not a comparative investigation of differences between the dreams of those with and without sight. What was at

issue was the nature of dreams in this already very particular group as a function of differences in age of onset of blindness. Although he noted “... it would be valuable to see corroborated by a similar study upon the dreams of normal individuals” (Jastrow, 1888d, p. 27) this was not something that Jastrow himself pursued. Rather, in questioning the blind about their dreams he was able to identify the “critical period” after which the loss of sight did not necessarily mean the loss of visual dreams, and thus loss of the power of visualization more generally. As Jastrow (1888d) put it this was the “point in the growth of the higher sense-centres, at which a divorce from sense-impression is no longer followed by a loss of their psychical meaning” (p. 34). Through his questioning of the blind in conjunction with records listing the date at which sight was lost, Jastrow identified the critical period as that between five and seven years of age. Within the 185 individuals he investigated, those who lost sight prior to the age of five did not experience visual dreams, while those who lost their vision after the age of seven all continued to experience visual dreams. This identification of developmental differences served as the comparative element of his investigation (Jastrow, 1888d).⁴⁰¹

⁴⁰¹ Jastrow’s interests in visualization and difference were not limited to his study of the blind. Appearing in print the same year as his study of dreams of the blind was a *Popular Science Monthly* article, “Eye-mindedness and Ear-mindedness” (Jastrow, 1888a). Here Jastrow expressed his view that individuals were either eye-minded (“*visionaire*”) or ear-minded (“*auditaire*”). Several means of testing which category individuals fell into were proposed, including tests of the limit of the capacity and the degree of error of each sense, as well concurrent tests of the senses to determine which was central to attention and later recollection. Knowledge of whether individuals were eye- or ear-minded, Jastrow (1888a) contended, would be valuable not only as a contribution to psychological knowledge but also in terms of its practical value to education: “the importance of recognizing and studying these distinctions, [is] alike for their educational utilization and as a contribution to a scientific psychology” (p. 608). Those in his study of the blind necessarily comprised a restricted range of these types.

Baldwin later proposed a similar scheme of educationally-oriented classifications (see Baldwin, 1895d, 1896c, 1896d). This was part of a much larger debate over the type theory of reaction, which proposed that the results of reaction time experiments were necessarily influenced by the type of individual (i.e., sensory or motor oriented) participating in the study (Baldwin, 1895d, 1896f; Lange, 1888; Titchener, 1895a, 1895c, 1896b). The theory was central

Attention to differences between individuals was a recurrent feature of inquiries into dream-life using questionnaires given their Galtonian orientation (Danziger, 1985, 1990). The particular form of statistical inquiry questionnaires allowed for made the collection of information regarding various forms of difference a central feature. Among the categories of difference regularly inquired into were “age, sex, [and] nationality,”⁴⁰² with others added as researchers saw fit, including “temperament,”⁴⁰³ and “state of health.”⁴⁰⁴ Jastrow’s (1905) inquiry into anaesthesia, while not involving a formal list of questions to be answered by respondents, did specify that those providing him with material also append information regarding not only “age, sex, occupation, condition in life, physical state, temperament,” but also the “purpose for which the anaesthetic was administered, length of period under its influence, degree of nervous shock accompanying the same, and so on” (p. 326-7). Information like this allowed researchers to divide individual respondents according to particular lines of interest and to make determinations regarding the essential nature of various groups.

Most prevalent, by far, were distinctions made along the lines of age, sex, and nationality.⁴⁰⁵ Regularities in the content of dreams with respect to age could be identified, such

in the development of American functionalism (see C. D. Green, 2009). Jastrow himself – along with Baldwin, Cattell, Lightner Witmer, and Sanford – was later part of the American Psychological Association’s Committee on Physical and Mental Tests (“Physical and mental tests,” 1898).

⁴⁰² e.g., “Reverie and Allied Phenomena,” April 1897, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁴⁰³ “Fancy, Imagination, Reverie,” Dec. 1895; “Dreams,” Oct. 1903, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁴⁰⁴ “Fancy, Imagination, Reverie,” Dec. 1895, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁴⁰⁵ This is not to say that the absence of any pronounced difference between some investigated categories in the collected material went unnoticed. For instance, Jewell (1905) concluded from his inquiry, “so far as can be inferred from the returns to this questionnaire neither the season, day of the week, nor the month have any marked effect on dreams” (p. 4). Nonetheless, assertions regarding differences took centre stage.

as the propensity for children to dream about animals and places and for adolescents to dream about people and events. At the same time emotionally tinged dreams were categorized as occurring most often in early childhood, puberty, and between the age 18 and 21 (Jewell, 1905). More generally, this work located the “age of dreams about the time of puberty and dawning adolescence” (Jewell, 1905, p. 34), with day dreams similarly characterized as prominent during this period (Partridge, 1897). These were, of course, not absolute statements about all members of a group, but rather general observations of a larger trend in the differences between dreams at various stages of development. Making such distinctions, both for the researchers undertaking their work at Clark and for Jastrow, was a means of suggesting, if only in the vaguest of terms, pedagogical uses for this kind of knowledge. Thus, while Jastrow (1888a), at the same time as his dream research, advocated for the classification of children according to their status as eye- or ear-minded, others emphasized the important role of the imagination in educational practices (Partridge, 1897).⁴⁰⁶

Adolescence was the time of dreams for Clark researchers, but the findings of Jastrow’s investigation into dream-life aligned more with earlier inquiries into mental imagery. Like Osborn and Galton he found both women and children to be the most prolific dreamers. His work with the blind led him to conclude

it is the vividness of the emotional background elaborated by the imagination that furnishes the predominant characteristic and tendency to dreams; for it is in the development of just these qualities that women excel men; the same view is favored by the relation of the prevalence of dreams to age....Childhood, the period of the lively

⁴⁰⁶ “Reverie and Allied Phenomena,” April 1897, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

imagination and highly tinged emotional life, brings the richest harvest of dreams.

(Jastrow, 1888d, p. 28)

The imagination, as revealed in dream-life, was the territory of women and children. Others emphasized the crucial role of race in dreaming. Jewell's (1905) work allowed him to assert "two-thirds of those who do not dream are clearly of Scandinavian or German parentage" and more generally that "what little data there is suggests strongly that the more stolid, phlegmatic races tend to less dreaming (remembered dreaming) than do the more active, nervous ones" (p. 6). As in earlier inquiries imagining, this time in the context of dream-life, was positioned – at least by some – as particularly pronounced in women, children, and "lower" races (see Chapter 1).⁴⁰⁷

Disputing Telepathic Dreams

Questionnaire-based studies of dreams were oriented toward determining differences between various groups, but dream research more generally was also concerned with disputing connections between dream-life and premonitory hallucinations. These two aims were not, however, mutually exclusive. In situating her dream research Calkins directly addressed the issue of prophetic dreams and dismissed accounts of such, including those provided in *Phantasms of the Living* (Gurney et al., 1886), as without any good evidence in their favour (see Calkins, 1893b). So far as she was concerned, "the number and the significance of these have inevitably been exaggerated. Such dreams are seldom accurately recorded, and the after experience (the so-called fulfillment) supplies details which one then believes that one has dreamed" (Calkins,

⁴⁰⁷ As in Hall's (1883a, 1893b) earliest questionnaire-based inquiry into the contents of children's minds, the Clark investigations also singled out differences between rural and urban (or city and country) children for discussion (see Chapter 4). In particular, burglar dreams were reported as a distinctive feature of the dreams of those in cities and towns, but as a type of dream completely foreign to those who resided in the country (Jewell, 1905).

1893b, p. 341).⁴⁰⁸ Cases presented as prophetic dreams were, for Calkins, no more than after-the-fact illusions of memory. Jewell's attempt to understand the nature of dreams included an explicit inquiry as to "Whether any dream comes true."⁴⁰⁹ In discussing the results of this investigation, undertaken in the years after work on dreams by those like Calkins, he contended "dreams have lost much of their significance to us now-a-days because we have learned that they are caused by some special stimulus or memory-association, that they are not prophetic – and so we have ceased to regard them with superstition" (Jewell, 1905, p. 29). This discussion of prophetic dreams also provided an opportunity to distinguish between the dream-life of modern, "civilized" individuals and that of "savages." So far as Jewell (1905) was concerned, civilized individuals "work harder, sleep more soundly and consequently dream less" and thus pay less attention to their dreams (p. 29). Together with a growing recognition of rational alternative explanations, there came about a general renunciation of the idea of prophetic dreams. The "savage," in contrast, "can well afford to pay more attention to his dreams, and, by the very act of paying attention, he gets to remember more and more of his dreams, and so magnifies them more and more" (Jewell, 1905, p. 29). With a larger body of dream-life to draw upon "savages" were seemingly inescapably oriented toward the irrational interpretation of dreams as premonitory.

The illusionary nature of dream-life was potentially problematic beyond the mischaracterization of dreams as prophetic. Jastrow (1906) made clear the necessity of distinguishing the products of the imagination from reality:

⁴⁰⁸ More general commentary on dreaming in relation to hypnotism, spiritualism, and premonitory hallucinations was also offered during this period (Buckley, 1888; C., 1890; Norton, 1888).

⁴⁰⁹ "Dreams," Oct. 1903, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

Unless I hold apart the world of fancy from the world of reality, I jeopardize my practical sanity. I must ever distinguish between my inventions and my experiences, my memories and my fancies, my hopes and my observations, my intentions and my deeds, and most decidedly between my dreams and the waking reality. Illusion, hallucination, error, fallacy, are common enough; and a considerable range of deception is the common lot.

(p. 489; see also Jastrow, 1889)

The mind's propensity to be deceived, by dream-life or otherwise, was of central concern, especially for Jastrow who took up deception as one of his abiding psychological interests (see Pettit, 2013b).

Statistics of Unconscious Life

Interest in the nature of dream-life connected with psychical matters beyond the issue of prophetic dreams. Most particularly, discussion of dreams often intersected with the notion of unconscious cerebration. Proposed in the mid-nineteenth century by British physiologist William Benjamin Carpenter, unconscious cerebration consists of “automatic activity of the highest level of the brain, without volition and without subjective awareness of the operative connections” (Danziger, 1982, p. 129).⁴¹⁰ As Danziger (1982) has described, within this category of mental activity fell not only ideomotor action, whereby specific ideas necessarily produced movement, but also

the association of ideas (especially when there were unconscious mediating associations), the phenomenon of involuntary attention, unconscious problem solving (as when the solution appeared suddenly after the problem had been put aside), the artistic productions

⁴¹⁰ On George Beard's development of the notion of “involuntary life” in response to Carpenter's work see Brown (1983, pp. 573–4).

of genius (where poems or sonatas might appear to be composing themselves), and also dreams and hallucinations. (p. 129-130)

These were not mutually exclusive as, for instance, dreams might themselves involve the association of ideas and unconscious problem solving.⁴¹¹

Beyond the circulation of the notion of unconscious cerebration within the developing scientific realm it also gained a certain amount of popular traction. Thus, in the mid-1870s Mark Twain could comment, of the writing of *The Adventures of Tom Sawyer*,

when the tank runs dry you've only to leave it alone and it will fill up again in time, while you are asleep – also while you are at work at other things and are quite unaware that this unconscious and profitable cerebration is going on. (as quoted in F. G. Robinson, 1995, p. 379; see also Gillman, 1989)

Twain also wrote of his belief in and experience with telepathy, or what he termed “mental telegraphy” (see Twain, 1891, 1895). Jastrow (1905) responded directly to these claims, asserting “it is necessary to emphasize the great limitations as yet surrounding our knowledge of the normal modes of sensation and the corresponding hesitancy with which we should regard any series of facts, no matter how apparently inexplicable” (p. 572). As in other instances, Jastrow invoked imperceptible sensory information as a plausible alternative account of presumed psychical experiences. Only by documenting the mind’s full range of ability, and explaining its functioning in natural rather than supernatural terms, could accounts like Twain’s be countered with scientific force.

⁴¹¹ Carpenter’s text, *Mental Physiology*, was assigned by Jastrow in his initial years teaching at the University of Wisconsin (see Jastrow, 1930; Jastrow et al., 1890). Jastrow (1906) at times intimates the occurrence of something like unconscious cerebration, but avoids the use of the term with its implied physiological mechanism, as he developed his own understanding of subconscious mental life.

The notion of unconscious cerebration also resonated with those seeking alternate explanations for purportedly psychical experiences. Rather than, like Twain, positioning unconscious cerebration as a psychical phenomena psychological investigators adopted the process as a feature of the normal mind.⁴¹² Royce invoked “unconscious cerebration” as an alternate, non-psychical explanation for the reportedly telepathic phenomena he collected in his psychical research via questionnaire. Cases of “double memory-consciousness” (see Royce, 1888), that is *déjà vu*, were explained in this way, both by Royce and by Osborn several years earlier in his work on illusions of memory (see Osborn, 1884a, p. 484). Royce (1889b) also characterized instances “where people by the aid of dreams have recalled forgotten facts, have found lost articles, or have solved problems that they had vainly attempted in their waking life” (p. 356) as instances of unconscious cerebration rather than psychical phenomena. To explain the “exceedingly small residue” (Jewell, 1905, p. 34) of seemingly prophetic dreams for which there was no readily available rational explanation, Jewell (1905) argued “there may be subconsciously injected into one’s dreams an element of truth which he does not recognize as subjective, hence they may take on a supernatural cast” (p. 34). As an example of this kind of process, he offered the case of an individual who dreams of acquaintances betraying his trust and the later occurrence of a betrayal then leading the dream to be interpreted as premonitory. In reality, Jewell contends, the dream was not prophetic but instead an avenue by which subconscious distrust made itself known.

The apparent telepathic quality of dreams was addressed in similar terms in Jewell’s dream research. As he explained,

⁴¹² On unconscious cerebration see also Burnham (1889), Donaldson (1893), and Robinson (1893).

when one has misplaced anything, he may not be able to tell where it is, but the missing association is made during sleep, which explains quite a number of dreams usually called premonitory. It is but a short step from this to dreams of finding something one has lost rather than mislaid. Somewhere down in the realm of sub-conscious activity is a recognition of the losing of the article, which is not sufficiently vivid to reach the higher levels of conscious memory. (Jewell, 1905, p. 12)⁴¹³

Imperceptible sensations, discussed as alternative explanations for various psychical findings (see Chapter 6), were similarly invoked as crucial features of unconscious cerebration. This kind of sensory information – together with unconscious mental processes that often occurred during dreaming – held the potential of producing revelatory information that appeared, at first glance, to be telepathically derived.⁴¹⁴

Collecting Accounts of the Unconscious

In March 1889 the findings of a questionnaire-based inquiry into unconscious cerebration appeared within the pages of *The Popular Science Monthly* (Speir, Jr., 1889).⁴¹⁵ The piece, “The Antechamber of Consciousness” was the work of Francis Speir Jr. (1856-1925), a peer of Osborn’s during their time as students at the College of New Jersey (see Chapter 1). The two, along with William Berryman Scott, were lifelong friends and conducted a groundbreaking 1877 paleontological expedition on behalf of the college (see Gregory, 1937; Osborn, Scott, & Speir

⁴¹³ For a criticism of the characterization of dream-life as unconscious cerebration see Pierce (1906). Jewell’s work on dreams also explicitly references Child’s (1892) work on unconscious cerebration, discussed below.

⁴¹⁴ More generally, it was suggested that the study of reverie would lead to similar insight as it “would seem to be the natural approach to the whole subject of association of ideas and new light might be expected from such a treatment, upon obscure facts of association, which the present doctrine seems inadequate to explain” (Partridge, 1897, p. 445).

⁴¹⁵ Discussion of this research also occurred elsewhere (“[Editorial notes],” 1888, “Mental science,” 1888).

Jr., 1878; Simpson, 1948). Speir continued his scientific work in the 1880s,⁴¹⁶ but although both Osborn and Scott made scientific endeavors their life's work, Speir eventually went on to a career as a lawyer (see "Student killed in motor," 1915). Before doing so he, like Osborn, undertook a research project within the new psychology.

In 1884 Speir began collecting information on the unconscious cerebration, broadly conceived. A report in *Science* that year described the project as an attempt "to cover the field of conscious mental activity in its relations with a possible unconscious cerebral activity" ("Notes and news," 1884, p. 426).⁴¹⁷ Speir's (1889) chosen method of amassing information was, as he characterized it, "the well-known one of the distribution of printed questions to be answered from personal experience" (p. 657). This material, it was reported, would then be used "for the purposes of a classification and co-ordination of the phenomena" ("Notes and news," 1884, p. 426). Discussion of Speir's project provided the *Science* commentator with the opportunity to address the issue of questionnaire-based research more generally. So far as he was concerned, "this work of collecting facts by means of psychological circulars is yet in its infancy, and its very life is threatened by any injudicious use of it" ("Notes and news," 1884, p. 426). Holding the work of Galton in this vein as a model of what successful questionnaire research was to look like, the reviewer contended

the necessary safeguards are, to ask only perfectly definite questions, to ask questions in whose answer our subject has no disturbing personal interest, and to be careful not to ask

⁴¹⁶ Speir replaced Scott as head of another Princeton scientific expedition in 1886 (see B., 1886).

⁴¹⁷ This was markedly similar to Speir's (1889) later description of the project: "It was with a desire to throw the light of further-collected facts upon the relation of a conscious activity to a possible unconscious cerebral activity that I undertook the task of collecting the necessary data" (p. 657). The report in *Science* ("Notes and news," 1884) on Speir's project did not list an author, but it was likely written by Jastrow in advance of his own questionnaire-based research.

questions that popular tradition has already answered by some poetical or otherwise interesting myth. Best of all are the questions whose answer our subject will never before have thought of at all, so that he will have no theory of his own. Unless we take some such care as this, our latest effort at the collection of psychological facts will degenerate into the most tedious of disastrous wanderings. ("Notes and news," 1884, p. 426)

In this context, the criticism offered of Speir's circular was as much a means of safeguarding the method more generally for use in future projects, as it was about addressing the inadequacies in that project.

Speir's questionnaire, beyond any material procured by the attention generated through its discussion within the pages of *Science*, obtained responses through its distribution to a number of university presidents and professors. Those who assisted Speir with his project included not only Osborn and McCosh at Princeton, but also James and Hall, the latter then at Johns Hopkins. Additional assistance was provided by Professor of Ecclesiastical History George P. Fisher of Yale College, President Ezekiel Robinson of Brown University, educationalist Dr. Nicholas Murray Butler of Columbia, and Professor of Philosophy Henry A. P. Torrey of the University of Vermont (see Speir, Jr., 1889, p. 658).⁴¹⁸ Much as in Osborn's earlier imagery investigation, the study relied on the assistance of these men for the collection of data within the university system. Perhaps distrusting the ability of the public to provide non-psychically tainted information on the phenomena, Speir opted not to advertise his project more broadly as was common in explicitly psychical investigations (see Chapter 5). More than 600 copies of the questionnaire were distributed and 102 responses ultimately obtained. Like most questionnaires of the period the name and age of respondents, as well as their address, was to be

⁴¹⁸ Torrey was John Dewey's undergraduate philosophy professor and was instrumental in his admission to Johns Hopkins for graduate study (see Dalton, 2002).

included with each submission. The request for information regarding sex was absent, though the directive that names be included was sufficient to ensure that the title of “Mr.” and “Mrs.” distinguished responses from the sexes (see Speir, Jr., 1889). Information on nationality or race was seemingly entirely absent, likely a consequence of the homogenous population surveyed at educational institutions.

Although Speir (1889) solicited information on a set of 11 questions, his published findings only addressed the results of 5 of these items.⁴¹⁹ Notably missing among the items discussed were ones singled out for specific criticism earlier in the decade within the discussion of the project in *Science*. As part of this critique the questions Speir listed on his circular were characterized, as a whole, as “of very unequal value and definiteness” (“Notes and news,” 1884, p. 426). Particularly problematic was the item: “What is the greatest number of distinct ideas you can consciously have before your mind at one time?” which the author described as “hopelessly vague” (“Notes and news,” 1884, p. 426). Other items were of limited value for other reasons. The query “Have you ever dreamed a dream precisely like one your parents or ancestors have dreamed?,” it was said, would produce “mere idle gossip” (“Notes and news,” 1884, p. 426). More damagingly, answers to this question

if negative, interest nobody: if they are affirmative, they might interest a collector of folklore; for, in telling his dream-experiences, who is very accurate at the best? In remembering and repeating them over and over, who is free from the manifold errors of memory? But in comparing one’s own dreams with the traditions of the dreams of one’s grandmother, who will be able to give answers that can be called scientific? The more

⁴¹⁹ These were items 2, 3, 6, 9, and 10 (Speir, Jr., 1889).

confident the reply, the less useful, in such a case, the supposed fact. ("Notes and news," 1884, p. 426)

To this reviewer's mind, keeping science and not superstition at the centre of the inquiry was of paramount importance. Only by doing so could unconscious cerebration be brought within the realm of natural mental functioning and the boundary between scientific psychology and unscientific psychical work be erected. It is unclear to what extent Speir concurred with the criticism, but he nonetheless opted to focus his analysis on responses to other questions on his circular. These other questions were of sufficient interest for the same commentator to note, if only lukewarmly, that he was looking forward to Speir's findings as "most of his circular is promising enough" ("Notes and news," 1884, p. 426).

The most substantive conclusion of Speir's inquiry into unconscious cerebration was the observation that most individuals had some familiarity with the kinds of experiences that fell within the category. Questioned about whether they had ever worked "for a lost idea, fact or fancy, while consciously devoting one's whole attention to something else" (Speir, Jr., 1889, p. 659), 91% of individuals responded in the affirmative. The results of other questions in Speir's report were discussed in much the same manner. Discussion of various facets of unconscious cerebration – such as the unconscious measurement of time – were addressed within the context of a representative question on the circular. For each of the five questions Speir took up, the results were presented as a series of excerpts from various respondents as well as a report of the various divisions among respondents, in the form of the percentage of individuals who did and did not experience different forms of the phenomenon. Much as in other questionnaire-based inquiries, simple dichotomous data, in this case yes and no responses, were easiest to synthesize. As a whole, the focus of the inquiry was normal, rather than pathological, mental functioning as

what was of interest to Speir (1889) was the “broad belt of border-land between consciousness and unconsciousness, whose limits are uncertain, yet where the manifestations of intellectual activity are recognized, which prove the kinship of the life of those two great regions” (p. 666). Surveying individuals about their experiences with various forms of unconscious cerebration was a means of scientifically documenting this relationship between conscious and unconscious mental life.

Beyond this project Speir did not pursue work on unconscious cerebration, but several years later incipient zoologist Charles Manning Child (1892) took up the project again. Most recently of Wesleyan University, where he earned a masters degree in zoology in 1892, Child became interested in the new psychology through his undergraduate work on the nervous system. Near the time of his project on unconscious cerebration, he traveled to the University of Leipzig where he briefly studied in Wundt’s laboratory. The experience proved disappointing and he quickly moved on to the study of zoology at the University, earning his doctorate in 1894. Upon his return to the United States he obtained a position at the University of Chicago, where he remained for the rest of his career (Hyman, 1957). In advance of this, however, the notion of unconscious cerebration drew his interest.

Child’s investigation of unconscious cerebration was not simply a replication of Speir’s work. Instead, he explicitly set out to extend the earlier investigation by adding to the material already collected. So as to ensure “the continuity of the inquiry it seemed best to re-issue the same set of questions, and these Mr. Speir very kindly furnished” (Child, 1892, p. 249). In addition to furnishing Child with his circular, Speir also provided Child with the full set of answers previously collected with the instrument. Despite assertions regarding the necessity of distributing “the same set of questions,” the questionnaire Child issued was not identical to that

previously circulated by Speir. The order of questions was changed and some questions were abandoned entirely as they were deemed, based on the results of the previous inquiry, to be “of no essential value to the subject” (Child, 1892, p. 250). The questions omitted from Child’s inquiry were three that Speir himself opted not to discuss in his earlier published findings.⁴²⁰

To Speir’s original set of 102 answers Child added nearly 100 further responses, bringing the overall total to 200. These were gathered with the assistance of Andrew Campbell Armstrong, Professor of Philosophy at Wesleyan who had earlier conducted his own questionnaire work extending Osborn’s mental imagery investigation (see Chapter 1). In much the same manner as Speir, Child discussed the results of each question in turn, providing both examples of answers and the percentages associated with various kinds of responses, but unlike Speir Child also divided the responses according to sex (having obtained responses from 49 women) and age. Since most of the responses received were from those under 30, and of these most from those under 25, he opted to divide the data into three groups: “Under 25 years,” “Between 25 and 30 years,” and “Above 30 years” (Child, 1892, p. 250). To display the differences between his sex and age groups, Child also included a number of tables depicting frequency of responses in each. Overall, he concluded that women were less familiar with unconscious cerebration than men and that there was a decrease in conscious awareness of unconscious activity with age. Of the latter finding he speculated: “It is possible that the decrease with increasing age is due to the decreasing plasticity of the brain molecules or brain cells under normal conditions” (Child, 1892, p. 259). In doing so he, much like so many other investigators of the period, attempted to link descriptive accounts of mental life with associated physical states.

⁴²⁰ Items 1, 5, and 11 (see Child, 1892).

Across their investigations, both Speir (1890) and Child (1892) framed their undertakings as “statistical” projects. At the same time, Child (1892) saw his work as comprising “an experimental verification of the psychological principles which others have maintained” (p. 258). In what respect the investigation constituted an experimental project was not specified, but this characterization fits with the larger trend at the time to see questionnaire investigations as experiments. Although Speir’s project encountered direct criticism on the methodological front (“Notes and news,” 1884), it was not he but Child who addressed these concerns. In Child’s (1892) view, “the general sources of error in statistics of this nature are well known” and included cases in which “some who answer evidently do not understand the questions and some are in doubt as to their answers” (p. 258). Even removing the items identified as deficient, as was done when he reissued Speir’s circular, was no guarantee that individuals would respond to questions as the researcher intended.

An even more substantial issue in these kinds of statistical inquiries, so far as Child (1892) was concerned, was “that of selecting a body of persons who will give average results” (p. 259). In the context of his investigation this was a concern as “the answers are from persons more or less accustomed to mental work and self-observation” (Child, 1892, p. 259) something that therefore was unlikely to produce “average” responses. This, however, Child contended was a difficulty largely overcome through the means by which individuals came to answer his circular. Information was collected from “largely college students and, moreover, not special individuals among the students, but entire classes, thus giving a body of persons selected perfectly at random, and likely to yield average results” (Child, 1892, p. 259). Having entire classes of students complete his circular might ensure a more representative body of data than simply selecting individual students, but it doubtful that this approach achieved his goal of

obtaining “average” results. Concern with producing an understanding of the average individual would later emerge as a overarching goal in twentieth century survey and polling work (Igo, 2007), while at the same time, student populations came to serve as convenient representatives of the normal human mind in much psychological research (Henrich, Heine, & Norenzayan, 2010). Just what “average” results, in the contest of Child’s inquiry, were to look like and how one was to ensure their attainment was left unspecified.

Conclusion

Research into unconscious mental functions was one means of contesting the characterization of certain phenomena as psychical. In documenting mental life outside the bounds of conscious awareness, a host of psychical phenomena that at first glance appeared inexplicable was now accounted for by an expanded conception of normal mental activity. The mind’s actions beyond the level of conscious awareness were in no substantive way different than its workings more generally. The subconscious James sought to associate with the supernatural realm was re-categorized by numerous psychologists as purely natural. Crucial to this repositioning was the new scientific psychology’s moral economy of data within which statistical investigations, in various forms, flourished. This bounty of material offered a means of countering personal experience of coincident events all too easily interpretable as psychical in nature. More experimentally oriented work followed the prototypical style of laboratory psychology, seeking to document the connection between the physical world and sensory experiences. Rather than detailing the particularities of experiences, these projects limited themselves to substantiating the very existence of certain features of mental life. Questionnaire-based projects, on the other hand, provided an avenue for discussing various differences between particular groups. Expansive bodies of data, whatever their methodological origins, offered

persuasive evidence of the mind's many, up to that point undocumented, unconscious processes.

Although psychical research of the kind described in previous chapters continued in the twentieth century, including that with questionnaires, it was increasingly circumscribed as outside the realm of psychology proper.⁴²¹ As psychologists came increasingly to repudiate the value of psychical research outside of debunking efforts (see Pettit, 2013b), and to deny the veracity of these phenomena more generally, questionnaire research spread into other realms. Psychologically oriented investigations of extraordinary mental states expanded to encompass more conventional religious experiences. In this vein a number of questionnaire-based inquiries into the varieties of religious experience were undertaken. Newer religious or quasi-religious practices that leaned more toward spiritualism, including mind cure and Christian Science, were investigated in this manner (Goddard, 1899; see Zenderland, 1998).⁴²² At the same time studies of largely Protestant religious experience, especially of conversion, were positioned as a normal part of the maturation process and thus as neither simple superstition nor pathology (Leuba, 1896; Starbuck, 1899).⁴²³ Here again was “the age for statistics” (Orr, 1903, p. 406) in yet another form.

⁴²¹ For instance, see J. H. Hyslop to H. Münsterberg, 1 January (n.y.), Folder 1825, Item 7, Hugo Münsterberg Papers, Boston Public Library, Boston, MA. See also Schiller (1901).

⁴²² “The Psychology of Health and Disease,” May 18, 1897, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA.

⁴²³ “Moral and Religious Experiences,” May 1895; “Religious Experience,” Feb. 1896, B1-7-1, G. Stanley Hall Papers, Clark University Archives, Worcester, MA. The various scientific investigations into religious experiences in the late-nineteenth and early twentieth centuries, much of which was associated with Hall’s program of questionnaire research at Clark University, have been explored by a number of scholars (Hay, 1999; Hood, 2000; Taves, 1999; C. G. White, 2008, 2009). This kind of work eventually coalesced into a distinct field of research on the psychology of religion.

Chapter 8

Numbering the Mind:

Thurstone, Likert, and the Re-Design of Questionnaires

In the mid-1890s, as Calkins was completing her research on mental associations, another Wellesley psychologist was employing the questionnaire method for a distinctly different purpose.⁴²⁴ Caroline Miles (1895), an Instructor in Psychology at the College, undertook work with the questionnaire less with the intent of revealing facts of individual psychology, the nominal subject of her inquiry, than with the aim of revealing the possibilities and limitations of method itself. Having traversed the space between the Wellesley, Clark, and Harvard psychology laboratories, much like Calkins, Miles came to articulate a position on the relationship between questionnaire-based investigations and other disciplinary practices, one that Calkins and others never clearly voiced.⁴²⁵ Arguing that questionnaire data could serve as a check to unverified introspection, as the psychologist who set about blindly examining his or her own mind might use accounts gathered via questionnaire as a means of corroborating their introspective findings, Miles (1895) also noted some of the method's distinct challenges:

⁴²⁴ Calkins is often remembered as *the* psychologist at Wellesley during the late-nineteenth and early twentieth centuries, but she was not the only psychologist employed at the institution during this period. In later years, Eleanor Gamble, a student of Edward Titchener, took over running the institution's psychology laboratory from the ever more philosophically-oriented Calkins (Creese, 1998). Prior this, in the mid-1890s, she was joined by Miles (later Miles Hill; 1866-1951). Educated at the University of Michigan, where she studied with John Dewey and earned a doctorate in philosophy in 1892, Miles taught first at Mount Holyoke College before moving to Wellesley (Attaway & Barritt, 2000; Creese, 1998).

⁴²⁵ While serving as Instructor in Psychology at Wellesley Miles, like Calkins, also undertook private instruction in the new field with Edmund Sanford at Clark (see Miles, 1895) and conducted work in the Harvard laboratory (Creese, 1998; Hill, 1898). Following her 1895 marriage, Miles moved to Chicago where her psychological work continued, with the assistance of Helen Thompson (Woolley), in the University of Chicago psychology laboratory (see Hill, 1898). In Chicago Miles also became actively involved in the social settlement movement, living for a time at Hull House (Attaway & Barritt, 2000; Creese, 1998).

to ask questions is easy, but to make the questionnaire an instrument of precision is very far from easy....to say nothing of the general difficulty of selecting truly cardinal points for questioning about, and the special rhetorical difficulty of framing questions that shall be perfectly clear as to the information required without at the same time prejudicing the answers to be received, there yet remains the difficulty of assigning their proper weight to the answers received. (p. 534)

Situated on the periphery of the discipline, and relatively disinterested in the fate of the questionnaire as method, Miles was uniquely positioned to comment on the potential benefits and intrinsic challenges of this mode of inquiry. Unlike other critics, the focus of her discussion was not the use of untrained individuals as data collectors, but rather the problems inherent in the construction of these instruments, as well as those that arose at the point of making sense of collected information. Her final conclusion that “in general, the questionnaire seems to the writer more valuable for the suggestions it gives the questioner than for its strictly scientific results” (Miles, 1895, p. 558), offered no solution to the method’s problems.⁴²⁶ These observations notwithstanding, it would be decades longer before the discipline arrived at a solution to the difficulties of constructing questionnaires and interpreting their findings.

⁴²⁶ Her investigation, as she chose to characterize it, was of “individual psychology” (Miles, 1895), an allusion to the work underway by Alfred Binet in France. Conducted under the direction of Edmund Sanford, head of Clark laboratory of psychology, the study was nonetheless undertaken at Wellesley where the questionnaire was given to 100 individuals (71 students, 29 faculty members) “taken at random” at from the College community in the winter of 1893-94 (see Miles, 1895, pp. 535–6). Miles first questioned these individuals verbally and recorded their responses. Based on these results new items were added to the initial series of questions and this new set of 15 questions, some of which consisted of multiple parts, was then printed and sent to the previous 100 individuals, 97 of whom responded. Among the items on the questionnaire were: “how do you recall a forgotten name?,” “How do you go to sleep when sleepless?,” and “Mention some story that has made you weep - the most pathetic you can think of” (Miles, 1895, p. 535). Discussion of the collected information often made use of various individual associations as explanatory, much as in Calkins’s contemporaneous work on mental associations (see Chapter 2).

In this chapter I discuss the evolution of the questionnaire over the first three decades of the twentieth century. Although Hall's program of questionnaire-based research continued unabated until 1915, it was increasingly relegated to the periphery of psychology proper. Many questionnaire-based investigations, especially those at Clark, fell within other fields – most notably education, religious studies, and psychical investigations⁴²⁷ – which were themselves evermore-distinct disciplinary entities. At the same time, mass data collection in psychology became increasingly a project of psychological testing. It was only in the 1920s, with an increased interest in tracking individual attitudes on social issues of various kinds, that psychologists returned to questionnaires with force. These psychological projects were not rooted in the accumulation of descriptive accounts of human experience, but rather sought to transform this method of data collection in significant ways by restricting the range of answers available to respondents to a set few. In doing so, the method adopted the scientific ideal of numerical values. This move from descriptive to numerical data, I argue, involved a shift from thick to thin description (T. M. Porter, 2012) and a distinct change in the public's relation to this work. Thin, numerical descriptions of psychological phenomena offered the promise of solving the issues of data analysis that had plagued earlier investigations. At the same time, concrete

⁴²⁷ Psychologists continued to be involved in psychical investigations in the twentieth century, but the tone of much of this involvement changed markedly. Rather than approaching psychical occurrences as potentially genuine phenomena, psychologists took on the role of debunkers out to expose not just the normal mental processes that might account for apparently psychical phenomena, but the outright fraud of mediums and their compatriots (Coon, 1992b; Pettit, 2008, 2013b; Sommer, 2012). Among the religious questionnaires circulated in the twentieth century was one by James Pratt, a former student of William James (W. James. [Answers to J. B. Pratt's questionnaire on religion]. Printed sheet with autograph annotations; [n.p., 1904]. William James Papers, 1803-1941, b MS Am 1092.9 (4474), Houghton Library, Harvard University, Cambridge, MA). James Hyslop also discusses a questionnaire investigation into psychical matters near the same time (J. H. Hyslop to Hugo Münsterberg, 1 January (n.y.), Folder 1825, Item 7, Hugo Münsterberg Papers, Boston Public Library, Boston, MA). Hall outlines the full tenure of his multi-decade program of questionnaire-based research in his autobiography (Hall, 1924).

numerical findings held the advantage of operating as immutable mobiles (Latour, 1986), a form of information able to circulate with ease not only within science, but also among government and corporate entities (T. M. Porter, 1995). The construction, application, and analysis involved in questionnaire research now had a set of strictures to follow, ones that both simplified interpretation and, at least superficially, certified such work as scientific. Though the reworking of questionnaires secured their place in the science's methodological repertoire, the space in which the method was revived was in some ways similar to the earliest projects in which the method was employed in the field. Once again, questionnaires were applied to issues of great social relevance. Decades into the twentieth century, however, the public was no longer invited to participate in this research process, but was instead the target of psychologists' pronouncements on the state of the social world.

Testing and Mass Data Collection

The re-working of questionnaires was influenced by other psychological practices that similarly sought the accumulation of masses of material, most notably intelligence testing. American psychology's interest in mental testing long pre-dated the advent of intelligence testing proper.⁴²⁸ Most famously, James McKeen Cattell instituted a program of mental testing, beginning in 1890 at the University of Pennsylvania and continuing later at Columbia College, in New York City. The initiative at the latter institution, where all incoming students to the College as well as those at the Columbia School of Mines were tested, consisted of a battery of motor tasks and physical measurements. Cattell's efforts were the most enduring and systematic, but others in the field were also interested in developing mental tests. In 1896 the American Psychological Association appointed a Committee on Physical and Mental Tests. In addition to

⁴²⁸ On this history see Carson (2007), Sokal (1987b), and Zenderland (1998).

Cattell, the committee comprised James Mark Baldwin, Joseph Jastrow, Edmund Sanford, and Lightner Witmer, all prominent members of the discipline in their own right. The committee failed to reach consensus regarding a battery of tests, instead listing alongside various tests those committee members in favour of the item's inclusion. The series of physical and mental tests was "regarded as especially appropriate for college students tested in a psychological laboratory" but "would also be suitable for the general public and, with some omissions and slight modifications, for school children" (Farrand, 1897a, p. 132, 1897b, p. 211).⁴²⁹ The tests were intended to be administered over less than an hour and were selected on the basis of "those which seemed likely to reveal individual differences and development, but also took into account ease and quickness in making tests and in interpreting and collating the results" (Farrand, 1897a, p. 132, 1897b, p. 211; see also "Physical and mental tests," 1898). Outside of Cattell's undertaking little sustained effort was made to test large bodies of individuals.⁴³⁰ This project ran into the twentieth century only to falter in the face of his student Clark Wissler's discovery that the tests failed to correlate with academic achievement to any appreciable degree (Sokal, 1982, 1987a). Notably, it was only with the development of statistical methods able to analyse collected masses of data that efforts to produce these kinds of tests came to an end.

⁴²⁹ This is a reflection, especially, of the position Cattell articulated the previous year: "The psychological laboratory can also be brought into mutually helpful relations with the community by extending the tests to any who wish to have them made. Children in the schools might be tested with special advantage. For this purpose tests are especially useful which can be made simultaneously on a large number of observers... In any case the making of the tests is good practice for advanced students preliminary to, or in addition to, special research. By bringing the laboratory into relations with the community we add to its influence and at the same time secure the material needed for research" (Cattell & Farrand, 1896, p. 647).

⁴³⁰ Early in the twentieth century a variety of tests were employed by Helen Thompson Woolley (1903) in her study of the sexes. For a review of the content of many of the mental tests used in the United States prior to 1910 see Whitley (1911).

Cattell's mental testing endeavours had little sustained influence on the discipline. It was instead in the realm of work with children that intelligence testing took hold in the United States. In the early twentieth century, psychologist Henry Goddard imported Alfred Binet's intelligence test for children from France in an effort to resolve the practical problem of evaluating those institutionalized at the Training School for Feeble-minded Boys and Girls in Vineland, New Jersey.⁴³¹ One of those who took up the newly translated Binet test was Lewis Terman, who produced the most popular American version of test, the Stanford-Binet revision (Terman, 1916). Both Goddard and Terman were former Hall students, though neither was directly involved in questionnaire-based child study research during their time at Clark.⁴³² Nonetheless, their subsequent employment capitalized on the Clark program's connections within educational circles, as each obtained work within normal schools for a time before moving on to other positions.⁴³³ What each took with him from their time at Clark was an openness to work with children, as well as an orientation toward the collection of masses of data. Rather than continuing to employ the Galtonian ideal championed by Hall as their vision of normalization, these investigators adopted a Queteletian notion aggregate as a central feature of their work (see Hacking, 1990; Hegarty, 2013).⁴³⁴ No longer was an idealized picture of childhood the goal of research on children. Rather an understanding of the distribution of children across a clear range

⁴³¹ On the development of intelligence testing in France see Carson (2007) and Nicholas, Andrieu, Croizet, Sanitioso, and Burman (2013).

⁴³² That said, Terman's doctoral dissertation sought to distinguish of "bright" and "stupid" children via the administration of mental tests (Terman, 1906).

⁴³³ Goddard was employed at the State Normal School in West Chester, Pennsylvania prior to his time at the Vineyard Training School (Zenderland, 1998). Terman worked for a time at the Los Angeles State Normal School, where he was joined by Arnold Gesell another former student of Hall's, before moving to the Department of Education at Stanford University (Carson, 2007; Chapman, 1988; Minton, 1988).

⁴³⁴ That said, there remains an element of the Galtonian ideal in later work, notably Terman's interest in the gifted (Hegarty, 2013).

of defined ability was used as a means of locating particular children along this dimension. In the context of intelligence testing, this information offered a means of sorting children on the basis of their mental abilities. In the hands of Hall's former students, mass investigations of children were now employed explicitly so as to locate individual children within a larger distribution of the population. This was a distinct departure from earlier questionnaire-based investigations where the aim was descriptive rather than diagnostic.

The scope of testing in the United States expanded considerably during World War One.⁴³⁵ Psychologists seeking to contribute to the war effort promoted various forms of testing as a means of sorting scores of new military recruits. In addition to a program of intelligence testing organized by Robert Yerkes (Carson, 2007), Walter Dill Scott developed aptitude tests for personnel selection (von Mayrhauser, 1989), and Robert Woodworth devised the Woodworth Personal Data Sheet to assess neurotic symptoms (Gibby & Zickar, 2008; Winston, 2006). The latter was completed too late for use during the war, but its lack of copyright facilitated its wide use with civilians (Gibby & Zickar, 2008). The statistically literate Woodworth, who taught mathematics for several years prior to pursuing work in psychology, was educated at Harvard under both James and Royce. He was also influenced by a year of study at Columbia University where he came into contact with both Cattell and the more statistically savvy Boas (Poffenberger, 1962; Woodworth, 1932), both of who had previously undertaken data collection projects.⁴³⁶

⁴³⁵ There is an extensive literature on mental testing during World War One, including Carson (2007), Chapman (1988), Fass (1980), Kevles (1968), Minton (1988), Samelson (1977), and von Mayrhauser (1991, 1992).

⁴³⁶ In 1904 Woodworth headed the Anthropological Department for the St. Louis World's Fair where 1,100 individuals received anthropometric and psychometric measurements and the results were compared to those of various "primitive" people on exhibit at the Fair (Winston,

The Woodworth Personal Data Sheet, identified by Winston (2006) as a “forerunner of modern personality tests” (p. 56), was constructed as a test of emotional stability at the behest of the American Psychological Association (Winston, 2006; Woodworth, 1932).⁴³⁷ By inquiring as to symptoms rarely seen in normal individuals the inventory was intended to be used as an alternative to time consuming psychiatric interviews in order to assess the emotional stability of army recruits. As Woodworth later recalled, he collected hundreds of neurotic “symptoms from reported case histories, I threw them into the form of a questionnaire which could be applied to a group of subjects at a time, the single questions to be answered Yes or No” (Woodworth, 1932, p. 374). The questionnaire was then tested on groups of normal subjects, and symptoms frequently reported by non-neurotic individuals were removed from the test. In limiting his questionnaire to questions that could only be answered Yes or No, Woodworth ensured that his collected information would be easily interpretable and set a precedent for future data collection schemes.

Like Woodworth’s Personal Data Sheet, the form taken by intelligence tests developed to meet the needs of the war effort proved influential to later developments in questionnaire construction. Though the military never fully embraced the use of intelligence testing’s findings as a basis for personnel decisions, more than 1.75 million individuals were tested with the instruments psychologists devised. Intelligence test results from nearly two million men produced massive quantities of information that could only be analyzed statistically. Efforts to construct a picture of American intelligence writ large from this collected material were far from

2006). Woodworth’s statistical work includes Woodworth (1912). On his life and work see Murphy (1963), Poffenberger (1962), and Winston (2006).

⁴³⁷ The history of personality tests is addressed in a number of works (Buchanan, 2002; Gibby & Zickar, 2008; Koppes, 1997; von Mayrhofer, 1989).

simple and not without controversy.⁴³⁸ That said, statistical approaches had developed significantly since the late nineteenth century making analyses possible, resulting in large tomes dedicated to explicating this amassed material (Brigham, 1923; Yerkes, 1921; Yoakum & Yerkes, 1920). Given the necessities of mass testing in the military context, psychologists devised multiple-choice tests suitable for administration in group settings; this included versions for both literate and illiterate individuals, the Army Alpha and Army Beta, respectively. For pragmatic reasons, possible responses to questions in these tests were restricted. Scoring hundreds of thousands of tests, and millions of individual questions, was far simpler in the case of multiple-choice items where there was a clearly defined correct answer, a scheme that rendered expertise an unnecessary part of the scoring process. Following the scoring of tests, the organization of data was also streamlined through the assignment of letter grades on a scale ranging from A to E, with grades nearer the beginning of the alphabet corresponding to higher intelligence (Carson, 2007). By the end of the war intelligence had been rendered “quantifiable on a unidimensional scale” (Carson, 2007, p. 197), setting the stage for the conceptualization of other attributes along similar terms.⁴³⁹

⁴³⁸ Particularly controversial was the contention that the average American soldier had a mental age below that of a thirteen year old (see Carson, 2007).

⁴³⁹ These developments were not, of course, limited to the United States. In addition to intelligence testing in France (Carson, 2007), work on testing was also undertaken in England where factor analysis proved a central component in this work, especially in the twentieth century. Among the most notable figures in this field are Charles Spearman, Cyril Burt, Raymond Cattell, and Hans Eysenck (see Buchanan, 2010; Levy, 1995; Lovie & Lovie, 1993; Tucker, 2009; Wooldridge, 2006).

Questionnaires Get an Attitude

One of the many psychologists involved in the World War One intelligence testing effort, though in a relatively minor role, was Louis Leon Thurstone (Carson, 2007).⁴⁴⁰ Like Woodworth and an emerging generation of social scientists in the early twentieth century, Thurstone was mathematically skilled. This was in direct contrast to an earlier generation of psychologists who were decidedly less capable in this arena, most notably Cattell who Sokal (1982) has characterized as “mathematically illiterate” (p. 337). Thurstone’s mathematical talents can be traced back to his undergraduate studies of electrical engineering at Cornell University. He then pursued graduate study in psychology at the University of Chicago with James Rowland Angell in the 1910s.⁴⁴¹ Following periods working with Walter Bingham at the Carnegie Institute of Technology, including work on intelligence testing for the war effort, and at the Institute for Government Research in Washington, DC, developing methods of improving civil service examinations, Thurstone returned to the University of Chicago in 1924. Three years later he was appointed to a research professorship in social science. The position was underwritten through the more than \$3.4 million in funding from the Laura Spelman Rockefeller Memorial the university received between 1923 and 1929, which was administered by the university’s Local Community Research Committee (later the Social Science Research Committee). Although funds were largely for work in political science, sociology, anthropology, and political economy, Thurstone secured support for his statistically oriented research, work which was broadly

⁴⁴⁰ Thurstone’s published prolifically on mental tests and scaling (Thurstone, 1919, 1921a, 1921b, 1925, 1926, 1927b, 1927c, 1927f, 1928c, 1928d, 1931b; Thurstone & Ackerson, 1929; Thurstone & Jenkins, 1929; Thurstone & Thurstone, 1930).

⁴⁴¹ Thurstone’s life and work is discussed in Bulmer (1984), Jones (1998), Guilford (1957), and Thurstone (1952).

influential on social scientists at the university (Bulmer, 1984).⁴⁴² It was from this position that his redesign of questionnaires took place.

In producing scaled questionnaires Thurstone took attitudes as his object of measurement. The precise meaning of the psychological term attitude, and research on such, was only just solidifying in the 1920s. Danziger (1997), in his discussion of the emergence of attitudes as a subject of psychological research, identifies early twentieth century psychologists, and brothers, Floyd and Gordon Allport as instrumental in formulating social attitudes as a component of mental life. Conceived of as learned individual attributes amenable to change, attitudes were not simply internal dispositions divorced from the world, but rather a “state of readiness” (Danziger, 1997, p. 145) that produced real effects in society. The positing of social attitudes allowed psychologists to investigate facets of the social world, more often the terrain of sociologists, via the study of individual attitudes *toward* something in the social world. Psychologists were able to comment on the state of society through their research on attitudes toward popular social issues of various kinds and, in doing so, the discipline’s larger visibility and social value was bolstered. Through the measurement of attitudes, psychologists could comment on the roots of social ills, which were positioned as at least partly the effect of underlying attitudes within individual members of the populace. The production of measurement techniques, including Thurstone’s scaled attitude measures, was crucial to this process, reifying the previously loose concept of

⁴⁴² Thurstone was also affiliated with the University of Chicago’s Institute for Juvenile Research, previously the Juvenile Psychopathic Institute (see K. W. Jones, 1999), at the time he developed his scaling method (Bulmer, 1984; Thurstone, 1952).

attitude and allowing the field to argue that state decision making should be informed by its findings (Danziger, 1997).⁴⁴³

Thurstone's work on scaled attitude measures grew out of his earlier development of mental tests and work on the scaling of intelligence measures.⁴⁴⁴ A prolific producer of research in the 1920s and early 1930s, especially following his appointment as research professor, he was well-versed in the scaling techniques at use in intelligence testing having developed some of these himself (e.g., Thurstone, 1925). In applying his scaling knowledge to the measurement of attitudes Thurstone was also inspired by difficulties he encountered teaching psychophysics. Finding differences in weights dull, he opted instead to give his students "a list of offenses presented in pairs with the instruction that they should be judged as to their relative seriousness" (Thurstone, 1952, p. 307). This classroom exercise eventually led to the formulation the law of comparative judgement (Thurstone, 1927a, 1927d), which outlined how a psychological scale could be constructed, independent of physical measurement, on the basis of a series of comparisons of stimuli. As he later noted, "the law of comparative judgment is entirely independent of the physical stimulus magnitudes. This circumstance enables one to use the law in the measurement of social and aesthetic values where physical stimulus measurement is entirely irrelevant" (Thurstone, 1952, p. 310). In this way Thurstone was able to "illustrate the connection between the study of social values and some methodological studies in psychophysics

⁴⁴³ Further discussion of the growth of work on attitudes in the 1920s can be found in Converse (1987), and another exploration of the term's history is offered by Fleming (1967). For discussion of the discourse on "scientific attitude" during this time see Jewett (2012).

⁴⁴⁴ Thurstone published extensively on attitude measurement (Thurstone, 1928a, 1928b, 1928e, 1929, 1930, 1931a, 1931c, 1931d; Thurstone & Chave, 1929). Outside of these works he also published a number of statistically oriented pieces during this time (Thurstone, 1922a, 1922b, 1926, 1927c, 1927e).

which, as such, seem to be rather remote from social science.”⁴⁴⁵ Like the recently solidified conception of intelligence, this scaling method consisted of unidimensional continuum along which variables could be straightforwardly measured in terms of more or less. In undertaking this work Thurstone’s particular interest was not the social per se, but rather the methodological challenges attendant the measurement of social values.

So far as Thurstone (1928b) was concerned “attitude” denoted “the sum total of a man’s inclinations and feelings, prejudice or bias, pre-conceived notions, ideas, fears, threats, and convictions about any specified topic” (p. 531). Given attitude’s existence as internal dispositions toward action, it was not attitudes themselves that were measured, but rather stated opinions which were used as proxies for such on the assumption that these were revelatory of underlying attitudes (Thurstone, 1928b). In taking up attitude research, Thurstone explicitly sought to improve upon earlier efforts at measurement, particularly those of Floyd Allport (e.g., Allport & Hartman, 1925). Allport’s assessment of attitudes had subjects select from a list of opinion statements a single statement most representative of their own views. Noting that Allport was “virtually dealing with rank order” (Thurstone, 1928b, p. 542), Thurstone offered his scaling method as a mere refinement of these earlier attempts. Rather than arriving at idiosyncratic rank orders of statements of opinion Thurstone (1928b) sought to produce a “rational unit of measurement” (p. 542) for attitudes, selecting as his initial topics attitudes toward pacifism-militarism, prohibition, and the church, all prominent social issues of the day.⁴⁴⁶

⁴⁴⁵ L. L. Thurstone to L. D. White, December 23, 1927, Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL.

⁴⁴⁶ See also “Experimental Study of Attitude Toward the Church,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL.

In Thurstone's (1928b) work attitudes were conceptualized in terms of more or less and scales were constructed so that

the shift in opinion represented by a unit distance on the base line seems to most people the same as the shift in opinion represented by a unit distance at any other part of the scale. Two individuals who are separated by any given distance on the scale *seem* to differ in their attitudes as much as any other two individuals with the same scale separation. (p. 542)

The decision to compress attitudes into a single dimension along a linear scale allowed for their precise measurement, but also attracted criticism from those who considered attitudes immeasurable and nonlinear (see Bulmer, 1984).

As Thurstone (1928b) outlined, the process of constructing an attitude scale involved a set series of steps. To begin with one identifies the attitude variable to be measured. From here a wide range of opinion statements related to the attitude are collected, both by way of written opinions on the issue obtained from individuals and through a search of literature “for suitable brief statements that may serve the purposes of the scale” (Thurstone, 1928b, p. 544).⁴⁴⁷ This list of statements is then narrowed to approximately one hundred statements that encompass the full range of attitudes toward the issue. These statements are then printed, individually, on small cards and several hundred judges are given the task of rank ordering the statements into eleven piles, ranging from those most strongly in agreement to those most strongly opposed to the issue

⁴⁴⁷ These statements, in addition to relating to the variable being measured, were to meet the following criteria: they must be brief, avoid double-barreled statements, be able to be endorsed or rejected based on the subject's attitude, and acceptance or rejection of a statement should indicate something about the subject's attitude toward a topic (Thurstone, 1928b). More recent work in psychology has critiqued the notion that attitudes are underlying predispositions of individuals, arguing instead that attitudes are created as a result of deliberative processes (e.g., Puchta & Potter, 2002).

at hand with a neutral pile in the middle. A attitude scale is then constructed on the basis of “the proportion of readers or judges who agree about the rank order of any two statements” (Thurstone, 1928b, p. 541). That is, for example, “the scale value assigned to the statement is so chosen that one half of the readers consider it more militaristic and one half of them consider it less militaristic than the scale value assigned” (Thurstone, 1928b, p. 546). After a scale value is calculated for each statement, ambiguous and irrelevant items are eliminated on the basis of various calculations, as outlined by Thurstone (1928b). Finally, a shorter series of between twenty and thirty items, which “constitute as nearly as possible an evenly graduated series of scale values” (Thurstone, 1928b, p. 552), are selected to comprise the final scale.⁴⁴⁸

Before being put to use the scale was then, ideally, subjected to an experimental test of its validity. Thurstone (1928b) contended that the scale could only be considered valid so long as it remained unaffected by the particular opinions held by the judges whose rankings informed its construction. One means of ensuring this was by constructing two scales from the same set of statements, one using the rankings provided by a group with attitudes that fell at the positive end of the spectrum and one with rankings from those with attitudes at the negative end. If the scale values produced by the groups aligned, the scale, and this method of scale construction more generally, was proved valid. This means of validating the method, he noted, “may turn out to be a severe test in practice, but the scaling method must stand such a test before it can be accepted as being more than a description of the people who construct the scale” (Thurstone, 1928b, p. 1).

⁴⁴⁸ For a brief review of Thurstone’s system of attitude scaling see Jones (1998, pp. 90–1).

547). Only through these tests could it be ensured that the scale was applicable beyond the initial group whose rankings informed its construction.⁴⁴⁹

Thurstone deemed his new method of scale construction “satisfactory” and termed it “the method of similar reactions.”⁴⁵⁰ Those completing the attitude scales were instructed to endorse any and all listed statements with which they were in agreement (1928b) and their individual score on the scale was computed as the average scale value of all the endorsed items.⁴⁵¹ These scores could then be plotted in a frequency distribution to reveal the attitudes of a given group toward a particular social issue.⁴⁵² At the same time, for comparison purposes, Thurstone’s

⁴⁴⁹ This was the approach to assessing validity of scales discussed by Thurstone (1928b) in his initial presentation of the method and referenced in other work at this time (Thurstone, 1928e). It is also discussed in Thurstone and Chave (1929), alongside other means of assessing validity. In neither instance are the results of this kind of analysis presented, though the latter publication notes that this work is underway. The results of this test are unknown.

⁴⁵⁰ L. L. Thurstone to T. V. Smith, October 2, 1928, Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL. See also Thurstone (1929, p. 240).

⁴⁵¹ See also “Experimental Study of Attitude Toward the Church,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL.

⁴⁵² Thurstone also experimented with other scheme for amassing information. In 1928, he produced a personality schedule that was given to University of Chicago freshmen. He noted “I hope that this test will enable us to identify the nervous students and I hope that we will be able to prevent several nervous breakdowns by obtaining psychiatric assistance for those students who need it” (L. L. Thurstone to T. V. Smith, October 2, 1928, Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL). The personality schedule instructed students to “draw a ring around” either “Yes,” “No,” or “?” for each question on the test, which consisted of dozens of items and spanned four pages (“Personality Schedule,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL). Among items from other sources, the content of Woodworth’s earlier inventory was used as part of the test (see Thurstone & Thurstone, 1930). Additionally, a questionnaire titled “An Experimental Study of Racial Attitudes” presented respondents with lists of paired races or nationalities and asked them to “underline the one nationality, or race, of each pair you would rather associate with” (“An Experimental Study of Racial Attitudes,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL). Although undated, the scale was produce around 1929.

questionnaires explicitly asked individuals to directly indicate their attitude toward the issue at hand; for instance, by marking an X, where they fell along a line ranging from “strongly favorable to the church” at one end to “strongly against the church” at the other with “neutral” as a mid-point.⁴⁵³

Though Thurstone’s systematized use of ranking to construct scaled questionnaires was novel, the employment of ranking as a means of organizing information was far from revolutionary. Earlier psychological questionnaires also employed ordered rankings, but to a different end. Galton’s (1883) mental imagery questionnaire, adopted by Osborn, employed descriptive rankings (i.e., very faint, faint, good, or vivid) in one of its items (see Chapter 1), while in Jastrow’s dream inquiry the occurrence of dreaming was recorded as either Yes, Seldom, Frequently, Every night, or No (see Jastrow, 1888d, p. 27). Perhaps most notably, Cattell’s *American Men of Science* project, which was precipitated by the newly founded Carnegie Institution’s desire to identify exceptional scientists to whom to funnel money (Godin, 2007), used rankings as a means of ordering scientists in various fields (Cattell, 1903a, 1903b, 1906a, 1906b, 1906c, 1910a, 1910b).⁴⁵⁴ The undertaking sought “to make a somewhat elaborate study of the natural history of American men of science” (Cattell, 1903b, p. 323), via the circulation of a memorandum to “ten leading representatives” (Cattell, 1903b, p. 311) from each

⁴⁵³ “Experimental Study of Attitude Toward the Church,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL.

⁴⁵⁴ Cattell’s project not only occupied much of the remainder of his life, but continues to this day (*American men & women of science*, 2013). For contemporaneous comment on Cattell’s undertaking see Bentley (1904). Cattell’s student Wells (1907) also undertook similar work. Earlier still a statistical study of belief, which asked individuals to order a set of questions on the basis of certainty of beliefs, was undertaken in the Psychological Laboratory at Columbia University, with suggestions from Cattell. On Cattell’s American Men of Science work see Godin (2007) and Sokal (1995). For an account of the rise of statistical work at Columbia, including Cattell’s efforts, see Camic and Xie (1994).

of twelve sciences who were to provide rankings on the basis of merit.⁴⁵⁵ From their ratings a picture of the ideal scientist was to emerge.

The process of constructing a scale by employing judges to sort statements was labourious, as Thurstone well recognized. In something of a reversal of the logic of the scaling method used thus far, he speculated that if one were to administer a set of statements to a large enough group, consisting of those in possession of a full range of attitudes, the endorsements provided by these individuals could themselves be used to construct the attitude scale. It was the frequency with which individuals endorsed pairs of items, in relation to those who endorsed each individual item, that could serve as the basis for a scale (see Thurstone & Chave, 1929, pp. 90–1).⁴⁵⁶ This simplified method of scale construction, although mathematically more complex, was soon rendered unnecessary by a still simpler method.

A “Simpler Method” of Scale Construction⁴⁵⁷

Near the time Thurstone published his new approach to attitude scaling, Rensis Likert was undertaking his own research on the subject.⁴⁵⁸ A graduate student at Columbia in the late

⁴⁵⁵ The study of great men was previously undertaken by Galton in his *English Men of Science* (1874) and more broadly by Pierce and a group of students at Johns Hopkins University in the mid-1880s. This work was characterized by Jastrow as a “natural history of great men” (Jastrow, 1886b, p. 10 fn., 1886c, p. 295 fn.) and intended to show how statistical analyses might be applied to impressionistic data (see C. S. Peirce, 1989, p. lxii). Though the work was never completed, but Jastrow later published several short pieces on the project, including discussions of the longevity (Jastrow, 1886b, 1886c) and precocity (Jastrow, 1888b, 1888c) of great men (see Jastrow, 1916). Peirce later took up this work again (C. S. Peirce, 1901a, 1901b).

⁴⁵⁶ See also “Report on Current Research Projects, Autumn Quarter, 1929,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL. The process of deriving a scale from a body of data in this way was later reimagined in Thurstone’s work on factor analysis (e.g., Thurstone, 1935, 1947).

⁴⁵⁷ See Likert (1932, p. 25).

⁴⁵⁸ On Rensis Likert see “The Career of Rensis Likert,” Institute for Social Research Newsletter, Winter 1971, Box 1, Folder 2, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI. See also Kish (1982) and Seasore and Katz (1982).

1920s, Likert was working with socially engaged psychologist, and psychical researcher, Gardner Murphy who had initiated a small project on social attitudes and enlisted Likert's assistance.⁴⁵⁹ Work on this project expanded to form the basis of Likert's 1932 dissertation, which described what came to be known as the Likert method of scaling (Likert, 1932; see S. E. Seashore & Katz, 1982).

Prior to his studies at Columbia Likert had never taken a psychology course. Like Thurstone, he initially pursued engineering as an undergraduate student. This shared background informed both men's view of psychology as a form of social engineering, which first

necessitated that relevant information was collected. At the University of Michigan Likert's study of chemical engineering, particularly metallurgy, led him to wonder

if you can analyze steel that way, perhaps the same kind of rigorous scientific approach could be applied to the problems of human behavior in industry or internationally, etc. that first errors of measurement would be large and graphical approximations rather than precise mathematical equations, but nonetheless you could use rigorous quantitative research to solve these problems.⁴⁶⁰

Likert attributed his growing interest in human behaviour and social issues to various factors, including his reading of historian James H. Robinson's *Mind in the Making*, recent domestic turmoil in the form of the Union Pacific Rail strike, which he witnessed firsthand, and his active involvement in a student Christian association dealing with the "costs and consequences of

Likert's other work, including survey research, has been discussed by a number of scholars (Converse, 1987; Crowther-Heyck, 2006; Jewett, 2013; Johnson & Nichols, 1998; Mahoney & Baker, 2007; Sewell, 1989).

⁴⁵⁹ Danziger (1997), in passing, erroneously identifies Likert as Thurstone's student. On Murphy see Pandora (1997).

⁴⁶⁰ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI

war.”⁴⁶¹ Given that “little has been accomplished by WWI” the group was “beginning to wonder if, perhaps, there weren’t better ways of dealing with international differences than warfare.”⁴⁶² For his part, “the more I thought about it the less sense to me it seemed to make.”⁴⁶³ This rising social consciousness led Likert to change course and earn a bachelors degree in economics and sociology.⁴⁶⁴

Following a year at Union Theological Seminary, Likert began graduate studies in the Department of Psychology at Columbia where his social engagement took on a decidedly social scientific bent (Kish, 1982). Murphy secured funds from the Columbia University Council for Research in the Social Sciences, part of a larger Rockefeller Foundation grant, to undertake a study of “attitudes of college students toward international and economic matters, race questions, political issues, [and] religious problems.”⁴⁶⁵ To do so, Likert and Murphy prepared a questionnaire. Like other efforts in this vein the instrument was constructed of items used by other researchers to measure attitude, as well as statements of opinion taken from recent magazines and newspapers, and “from books, addresses and pamphlets, and a number were made up by the experimenters” (Likert, 1932, p. 12). Initially taking an hour and forty-five minutes to complete, the questionnaire was later altered to include only half as many items, as well as “some additional items which were largely descriptive where you describe a situation and ask for

⁴⁶¹ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶² Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶³ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶⁴ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶⁵ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI. See also Likert (1932, p. 11). For Likert (1932) attitudes were “dispositions toward overt action” (p. 9).

reactions to it," and "a number of items having to do with [the] personal history of the individual."⁴⁶⁶ All items were to adhere to the criteria of "simplicity, clarity, and brevity" (Likert, 1932, p. 12).

The questionnaire items were all to be answered from a set series of responses. These were themselves various, involving questions of four types. Some items were to be answered "Yes," "No," or "?," such as the question "Do you favor the early entrance of the United States into the League of Nations?" (Likert, 1932, p. 15). Others items were multiple-choice questions each with their own set of possible answers. Two other kinds of questions were completed by selecting from a graduated series of "(a) *strongly approve*, (b) *approve*, (c) *undecided*, (d) *disapprove*, (e) *strongly disapprove*" (Likert, 1932, p. 14). The simplest of these items had individuals respond to a statement by selecting from this range of responses. For instance, "we should be willing to fight for our country whether it is in the right or in the wrong" (Likert, 1932, p. 17). In more complex items, individuals were presented with condensed newspaper stories on relevant social issues, the final portion of which described the outcome of a conflict: "In a community of 1,000 whites and 50 negroes, a drunken negro shoots and kills an officer who is trying to arrest him. THE WHITE POPULATION IMMEDIATELY DRIVE ALL THE NEGROES OUT OF TOWN" (Likert, 1932, p. 19).

Influenced by previous research documenting the contextual specificity of honesty and dishonesty, Murphy and Likert speculated that attitudes would be of a similarly specific nature.⁴⁶⁷ The aim of the overall project was

⁴⁶⁶ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶⁷ The work on deceit referred to is that of Hartshorne and May (1928); this research is discussed in Pettit (2013b). In publishing the results of his attitude study Likert (1932) offered a

to cut through the statistical confusion which has resulted from the whole specificity-generality argument, to bring out the actual points of disagreement which separate these rival groups of psychologists, to make clear the statistical assumptions involved in all such methods of reasoning, and above all to test empirically in an extensive way the actual coherence or clustering of attitudes on a variety of public issues. (Likert, 1932, p. 10)

It was thought, for instance, "that attitudes toward segregation, toward eating with the Negro and toward lynching would be independent, and that in general any one specific attitude toward the Negro would bear no clear relation to the attitudes on other issue" (Likert, 1932, p. 11). To assess this relationship the attitude questionnaire was administered to students at Columbia, as well as an additional eight schools including Yale, the University of Michigan, the City College of New York, and New York University. From these institutions more than 2000 individuals completed questionnaires (Likert, 1932).

Numerous responses to the questionnaire were accumulated, but this success soon proved problematic. Likert was well aware of the inherent challenges of efforts to quantify social phenomena. In previous attempts to measure attitudes, he noted, "the difficulty has lain in the statistical difficulties which are encountered when everyday aspects of social behavior, ordinarily handled as qualitative affairs, are treated from the mathematical point of view" (Likert, 1932, p. 5). This was not a dismissal of the latter approach, but rather a recognition of the complications so much a part of these endeavours. As Likert recalled, "we built the questionnaire and decided to collect all the data without really thinking through how we were going to analyze the data," but it soon became apparent that "it wasn't going to be feasible to do what we originally thought

critique of these earlier character inquiries and their conclusion that honesty and deceit were context dependent.

"we might do" with the collected material.⁴⁶⁸ Working on the assumption that there would be little relationship between the attitudes expressed in answers to different items, Likert sought to compare each individual's answers to every other answer on the questionnaire. This, however, proved impossible,

we had something over 400 items and if you did 400×399 by 2 you would have the number of tables that we would have. I checked with the tabulating services at Columbia University and it became clear that the magnitude of the job was such that we didn't have the money for it. If we had had the money for it, I would have had so many tables I would never have gotten out from under the [work].⁴⁶⁹

While attractive in principle, the abundant material collected for the project, even though now restricted to a set range of possible responses, proved difficult to manage just as in questionnaire investigations decades earlier. The development of equipment meant to aid in the tabulation of large amounts of information, proved both inadequate and out of reach given the costs associated with an inquiry of this scale.

Faced with what seemed to be an insurmountable amount of material to analyse, Likert attempted to simplify his task as much as possible. To this end, he opted to focus just on the data collected from students at the University of Michigan through the assistance of one of Murphy's former students. He recalls,

I studied their responses on large tabulation sheets that my wife and I plotted item by item by individuals across large sheets about a yard square. It became clear that there were consistent patterns. A person that had a hostile attitude toward Negroes on one item

⁴⁶⁸ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁶⁹ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

had a hostile attitude on other items. I became impressed with this pattern and I began to compare them. I would cut strips of a row or column out and slide it across and compare it with others and make that kind of analysis.⁴⁷⁰

Like Hall and his research associates in the late-nineteenth century Likert, out of necessity, employed paper systems as a means of managing and making sense out of his collected material (see Chapter 4). In doing so, Likert initially eschewed any attempt to synthesize the whole of his data in favour of detecting patterns among individual response sets. He also enlisted in this work his wife, and regular collaborator, Jane Gibson Likert (Kish, 1982; Likert & Likert, 1976) continuing the tradition of engaging women, in restricted capacities, in the labour associated with questionnaire research.

This initial piecemeal analysis facilitated the development of a new approach to scaling: the sigma method of scoring. As Likert (1932) made note of the relative stability of attitudes across contexts he also observed that attitudes measured with multiple choice and scaled approval items were normally distributed, as attitudes were in Thurstone's earlier research. In this system of analyzing the questionnaire results he was able to "compute the sigma scores for each item based on the distribution for that item, and then I could assign each person a sigma score depending on which item he checked."⁴⁷¹ The conversion of item responses into sigma values, that is, standard deviation units, allowed for the location of individuals across a spectrum of attitude positions without the use of judges to construct a scale. At the same time, a reliable attitude scale could be constructed with fewer items than was necessary in other endeavours.

⁴⁷⁰ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁷¹ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI. See also Likert (1932, p. 22).

This method of scoring was, therefore, a far easier approach of measuring attitude and analyzing the resulting mass of material (Likert, 1932).

At the suggestion of Albert Poffenberger, head of the Department of Psychology at Columbia, Likert set out to test the effectiveness of a still simpler method of scoring. Poffenberger pointed out that directly numerical rating scales were regularly used in industry for evaluating items, employees, and the like. Believing this form of measurement to be ineffective, Poffenberger informed Likert: “what you ought to do is show that this kind of a simple 1-2-3-4-5 scoring that is commonly used is not very good and show that they ought to use the sigma method of scoring.”⁴⁷² Likert readily took up the challenged. Each of the possible responses on the scale from ranging from strongly disapprove through strongly approve was assigned a numerical value from 1 to 5, with 5 always corresponding to positive end of the scale.⁴⁷³ To his surprise, “when I compared the 1-2-3-4-5 scoring I got the same level of reliability that I got with the sigma scores.”⁴⁷⁴ Moreover, comparisons of individuals’ scores on the questionnaire, as calculated with both methods, produced “correlations for total scores of .998, .997. So it was virtually identical as far as the distribution was concerned.”⁴⁷⁵ Not only was it “much simpler to build an attitude scale” with this new method, but scoring was easier than with the sigma

⁴⁷² Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁷³ In this initial work it appears that these values were not labeled directly on the questionnaire itself, but were rather used simply for scoring purposes (see Likert, 1932, pp. 25–6).

⁴⁷⁴ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁷⁵ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI. Likert (1932) also experimented with other numerical scale values, including a scale with heavier weighted extremes that ranged from 1 to 7, with no 2 or 6 values, and found little difference between the scales as they produced similar results.

method, as all that was necessary to produce an individual's final score was the summation of the numerical values associated with each response.⁴⁷⁶

Like Poffenberger, Likert had been sceptical of numerical rating scales as viable alternative means of measurement. As he noted in his dissertation, he "began this inquiry with a suspicious attitude toward the simple computations used in rating-scales, and adopted these simple procedures only in the light of evidence showing that the simpler methods gave the same results as the elaborate" (Likert, 1932, p. 27 fn.). The end results, however, constituted "a radical departure" (Likert, 1932, p. 6) from the scaling methods so recently developed by Thurstone. Simple numerical scales were superior to earlier measurement efforts, including Likert's own sigma method, "since they yield almost identical results with the sigma method and similarly do not involve any of the errors likely to be present in any technique in which experts, judges, or raters are used" (Likert, 1932, p. 26). The ease of this form of scaling was in direct contrast to Thurstone's "exceedingly laborious" (Likert, 1932, p. 6) procedure and this "Likert" method of scaling soon took hold in the discipline and beyond.

Participants and the Public

The move to the collection of more circumscribed material was accompanied by a shift in the role of the public in questionnaire-based endeavours. Unlike earlier investigations that sought to collect information via questionnaire, the work of collecting in attitude research in the 1920s and 30s was limited to those in professional spheres. While still seeking to amass large quantities of data, these endeavours did so not by engaging members of the public in the research process, but rather through networks of association largely internal to the university system. This work was to speak to broad social issues and, to some degree at least, the state of the public mind at

⁴⁷⁶ Oral History Interview Transcript, 1970, Box 1, Folder 1, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

large, hence its use of popular literature as source material. The public, however, was not actively involved in the process of arriving at these conclusions.

At the same time, the public at large was not among those targeted to provide questionnaire responses. It was instead readily accessible populations of university students who comprised the bulk of respondents in these investigations. Converse (1987), in her history of survey research in the United States, contends that the development and use of scaled attitude measures required several supporting factors, including: educated individuals capable of articulating “their intellectual, political, and moral positions; people, in sum, who were trained in having attitudes” (Converse, 1987, p. 58). University students were uniquely capable of meeting these requirements given their presence in intellectually stimulating educational environments where discussion of social issues was a regular occurrence. At the same time students possessed the time and tolerance necessary for participation in this kind of research. Students were a group whose subordinate position in relation to the researcher-academic meant they could be imposed upon to cooperate in research. Finally, students were inexpensive and accessible participants, which meant time and effort did not have to be spent looking elsewhere for respondents (Converse, 1987).⁴⁷⁷ In Thurstone’s case participants were predominantly University of Chicago instructors and students, including those at the undergraduate and graduate level, as well as divinity students. An additional group of participants was obtained from the local Chicago Forum Council, a goodwill group in which individuals gathered together to discuss social issues

⁴⁷⁷ Many similar points are identified by Rice (1930) in his criticism of the use of particular kinds of individuals as the judges in Thurstone’s scaling method. Likert highlights Rice’s criticisms in his own work and frames his new approach to scaling as avoiding these difficulties (see Likert, 1932, p. 24).

(Thurstone & Chave, 1929).⁴⁷⁸ Likert's participants were more diverse only in the sense of originating from multiple universities across geographic locales. The tendency to employ students as research participants, while an aspect of the earliest questionnaire-based investigations in the discipline, was a convenience that would soon come to dominate psychology as a whole. As in these earlier endeavours, the use of students as sources of generalizable knowledge is increasingly recognized as problematic today, though for largely different reasons (Henrich et al., 2010).

The use of students in Thurstone's work, while convenient, was ultimately problematic. In presenting his "simpler method" (Likert, 1932, p. 25) of scale construction and scoring, Likert positioned his work as superior to Thurstone's for reasons beyond its ease. To do so, he referenced criticism of the method by sociologist Stuart Rice who noted the construction of Thurstone type scales was dependent on the cooperation of individuals who were able to articulate their attitudes, a skill uniquely found in students and members of discussion groups. Given the power differential between students and investigators cooperation risked veering into (over)-compliance, a danger psychologists were cognizant of and concerned about during this period (see Pettit, 2013b ch. 6). Beyond this challenge, it was not these but "the more numerous work-a-day groupings of society ... about whose attitudes the social scientist is in the most need of information" (Rice [1930] as quoted in Likert, 1932, p. 24).⁴⁷⁹ This was impossible, however, if scales were constructed with the judgments of only certain kinds of members of society. The determination of equal appearing intervals was directly tied to those engaged as judges. There

⁴⁷⁸ Thurstone's monograph length collaboration with Chave is a rare instance in which he addresses who the participants in his work are, as his focus in publications is more often on scale construction and its intricacies (see Thurstone & Chave, 1929).

⁴⁷⁹ Sociologist and statistician Stuart Rice discussed methodological issues related to quantitative approaches to social measurement in Rice (1928, 1930).

was no reason to believe that the scale they produced would also serve as a scale of equal appearing intervals for other kinds of people. More problematically still, it was “difficult to imagine securing comparable judgments, or satisfactory measurements in the final application, from bricklayers, business men, Italian-Americans, nuns, stevedores, or seamstresses” (Rice [1930] as quoted in Likert, 1932, p. 24). This meant that, at least so far as some were concerned, the attitudes of a great many segments of society were necessarily off limits to investigators whose interest lay in these particularly psychological facets of individuals. This did not preclude, of course, more expedient commercial undertakings like opinion polling (see Igo, 2007; D. J. Robinson, 1999). Likert’s alternative approach to scaling avoided this difficulty by foregoing the use of judges altogether. The result was scales that could, conceivably, be applied to any and every type of member of society.

The use of students as the source of information did not, necessarily, limit conclusions to just this group. In Likert’s investigation the obtainment of responses from students at universities in both Northern and Southern states facilitated discussion of difference in attitudes along regional lines. The results of his scale indicated “there exists a clear-cut pro- or anti-Negro sentiment, an emotional and conative disposition....this, then, is a psychological fact transcending the mere statistical aspects of the scaling method involved” (Likert, 1932, p. 38). In concrete terms this difference in attitude manifested, for instance, in “Never” serving as a far more popular response to a question on the appropriateness of lynching from those in Northern schools, than from those in the South. Differences like this were evidence of group factors rooted in shared cultural patterns among those in various locales. Comparing results from one university to those of another in a similar region of the country offered “an empirical method of establishing the degree of uniformity of the cultural pattern in which these various groups of

students participate" (Likert, 1932, p. 38). The result was evidence of a difference in "general" attitude between those in the North versus the South attributable, in Likert's view, to a host of environmental factors – social, educational, economic, and otherwise. Likert framed his scaling method as a valuable "way of revealing general differences in the friendliness or antagonism shown by different social groups toward some nationality or class with which they have dealings" (Likert, 1932, p. 39). While couching his findings as revelatory of the attitudes of college students, he edged toward broader generalizations of the attitudes he found characteristic of those in the North and South, respectively. The aim of this kind of research was fundamentally broader than merely determining the attitudes of college students toward various social groups or issues.

Rather than enlisting members of the public in their work, early twentieth century psychologists were more likely to collaborate with those in other academic disciplines. More generally, questionnaire research felt the influence of, and itself made an impression on, other fields, particularly sociology. It is no coincidence that Columbia and Chicago were the sites at which questionnaire scaling was developed as sociologists from both universities dominated the field in the interwar years (M. C. Smith, 1994). Connections with sociology at Columbia most notably include the close friendship between Robert and Helen Lynd, the sociologists responsible for the 1920s Middletown survey study, and Murphy, Likert's doctoral supervisor (Pandora, 1997).⁴⁸⁰ In his scaling work, Thurstone collaborated with pastor Ernest John Chave, of the University of Chicago's Divinity School (Thurstone & Chave, 1929). Beyond this, Thurstone was part of a much larger social science community and his influence on those around him, especially sociologists and political scientists, was considerable (Bulmer, 1984). The

⁴⁸⁰ The work of the Lynds is ably addressed in Igo (2007).

clearest evidence of this impact is in the work of sociologist Samuel Stouffer who would go on to a career as a survey researcher and methodologist (Bulmer, 1984; Converse, 1987; Platt, 1996). Thurstone was not Stouffer's official advisor, but served as mentor to the young sociologist whose dissertation project was clearly influenced by Thurstone's work and departmental debates over the appropriate methodology for sociological research. Although taking prohibition as its nominal topic, the project was in reality a comparison of the efficacy of scaling versus case study methods, with the department's best coders enlisted to assist with the latter approach (Stouffer, 1930). Stouffer found high agreement between the two methods, but scaling's easier, more efficient process made this mode of evaluation the clear winner. This finding by no means resolved disputes over the appropriate methods of sociological research, but contributed to the larger trend toward greater quantification within the field in the interwar years.⁴⁸¹ Thurstone, more generally, was also a contributor to this shift through his statistical projects, advocacy of quantification, and lobbying for the purchase of equipment necessary for the analysis of large sums of material, all within the context of social science at the University of Chicago (Bulmer, 1981, 1984).⁴⁸²

The social role of the social sciences also changed considerably during the interwar years. Rather than confining social scientific work to empirical studies in academic spheres the social sciences increasingly took on what sociologist Paul Lazarsfeld termed "administrative research;" that is, new forms empirical research that aimed to solve the immediate practical problems of government and corporation interests (Converse, 1987; Dehue, 1997; House, Juster, Kahn, Schuman, & Singer, 2004; Igo, 2007). Research centres like Survey Research Centre (later the

⁴⁸¹ For a broader discussion of twentieth century developments in American sociology see Haney (2008).

⁴⁸² Discussion of the social sciences, especially sociology, during the interwar years can be found in Bannister (1987) and Smith (1994).

Institute for Social Research) at the University of Michigan, which Likert headed from the mid-1940s until his retirement in 1970,⁴⁸³ were part of the increasing commodification of social science research. Instead of simply putting into practice existing psychological findings these kinds of enterprises created their own psychological knowledge *de novo* in order to meet very specific ends. Newly redesigned questionnaires were one method put to use in these new kinds of research endeavours.

Both Thurstone and Likert revolutionized questionnaire research with their development of scaling techniques, but neither dogmatically adopted this mode of investigation. Thurstone and his students created numerous attitude scales before he opted to end his pursuit of these kinds of inquiries in the early 1930s. Deeming the mass creation of seemingly endless quantities of attitude questionnaires unproductive, “incomplete material for a dozen more attitude scales was thrown in the wastebasket and I discouraged any further work of that kind in my laboratory” (Thurstone, 1952, p. 312).⁴⁸⁴ Instead, Thurstone turned his attention to more quantitatively focused work on factor analysis, as was hinted at in his earlier speculation that scales might be derived mathematically through the identification of patterns of responses to attitude questions

⁴⁸³ “The Career of Rensis Likert,” Institute for Social Research Newsletter, Winter 1971, Box 1, Folder 2, Rensis Likert Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI.

⁴⁸⁴ Attitude scales produced by Thurstone in the late 1920s and early 1930s include those on Attitudes toward: Patriotism, the Treatment of Criminals, Censorship, Sunday Observance, War, Evolution (“Report for the Autumn Quarter, 1931,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL), the Church, the Negro, Birth Control, God, the United States Constitution, Communism, the Chinese, the Germans, Law, Capital Punishment, Patriotism, Public Office (“Report for the Winter Quarter, 1931,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL), Prohibition, Capital Punishment, the Chinese, and the Constitution of the United States (“Report for the Spring Quarter, 1931,” Box 14, Folder 9, University of Chicago Social Science Research Committee Records, Special Collections Research Center, University of Chicago Library, Chicago, IL).

within large, diverse groups (e.g., Thurstone, 1935, 1947). Likert similarly adopted a variety of methodological approaches to social research. In his later work for the United States Department of Agriculture (USDA) surveying farmers he came to advocate the use of structured open-ended interviewing, rather than questionnaires. He also played a pivotal role in the development of modern techniques of sample surveying (Converse, 1987; Platt, 1996).⁴⁸⁵ Methodological diversity was also apparent during his tenure heading the University of Michigan's Survey Research Centre. The discipline, in contrast, embraced Likert scales very nearly wholeheartedly.

Conclusion

What began in the final decades of the nineteenth century as a means of collecting large quantities of descriptive accounts of mental life had, by the 1930s, become a numerically oriented practice. This, I argue, constituted a profound shift from thick to thin description (T. M. Porter, 2012) in the discipline's mass data collection endeavours and was accompanied by a change in the public's relationship to this kind of research. The discipline's moral economy of data continued unabated, but took on a more decidedly numerical form. Tests, as tools for collecting information on various characteristics of a multitude of individuals, were pervasive by the time Thurstone and Likert turned their attention to questionnaires. Developments in this realm, notably the use of restricted sets of answers and the emergence of statistical techniques, were powerful influences on the reimaging of questionnaires. Although similarly oriented to issues of social relevance, these instruments were restricted to the sphere of experts including those engaged in more commercially oriented enterprises (Igo, 2007; D. J. Robinson, 1999), rather than involving a diverse body of practitioners in the collection process, as had been the case in many late nineteenth century projects. At the same time, easily accessible bodies of

⁴⁸⁵ Further discussion of the USDA's employment of academic experts in the 1930s can be found in Jewett (2013).

students were increasingly rendered the default subjects for this research. Despite these restrictions on participation, questionnaire research aimed to comment on the broad state of the public, particularly its position on prominent social concerns of the day. To do so, it was necessary to derive specific findings, something far easier to achieve with the reductive power of numbers, than the richly nuanced accounts of personal experience so much a part of earlier questionnaire investigations.

Conclusion

At the 1926 gathering of the Experimentalists, held just a year prior to founder Edward Titchener's death, the society unanimously "resolved, that this meeting deplores the increasing practice of collecting administrative or supposedly scientific data by way of questionnaires; and that the meeting deplores especially the practice under which graduate students undertake research by sending questionnaires to professional psychologists" (D., 1926, p. 468).⁴⁸⁶ This was followed by a note from E. G. Boring (1926) in the next issue of the *American Journal of Psychology* in which he urged psychologists to unburden themselves from their default practice of responding to "the promiscuous questionnaire" (p. 633). Characterizing regular requests for information via circulation of these instruments a "nuisance" (Boring, 1926, p. 633), he outlined a set of seven criteria for determining when and how to respond when faced with the request to complete a questionnaire. More than three decades after James's (1890e) observation that questionnaires "ranked among the common pests of life" (p. 194), the instruments remained as bothersome as ever. By the 1920s even psychologists had adopted the practice of using questionnaires as a means of collecting information for administrative, rather than research purposes.

Questionnaires are deceptively simple psychological tools. They are, at base, no more than a printed list of questions circulated to amass multiple responses. Despite, or perhaps because of, this simplicity the questionnaire has become one of psychology's most used instruments. Seemingly any study can benefit from the quick, concise, and easy data collection that a questionnaire allows, whether this is of basic demographic details or more pointed information

⁴⁸⁶ The term questionnaire was often employed by Titchener in reference to questionnaires (see Titchener, 1901, pp. 387–8). On the history of the Experimentalists, later the Society of Experimental Psychologists, see Boring (1938), and Goodwin (1985, 2005).

about some aspect of the participant's subjectivity.⁴⁸⁷ Although questionnaires did not originate within psychology, the discipline has had a profound influence on the form these tools have taken. Likert scales, developed in the 1930s, are now the dominant, if not default, style of questionnaire. Used in explicitly psychological research enterprises and in society more broadly, these instruments continue to enact a moral economy of data much like the one that governed psychology at its very inception as a discipline.

At the same time, psychology fetishizes experiments as much today as it did in its earliest years. Despite this proclivity, much of the research undertaken in the discipline employs other kinds of methods. Experiments can reveal causes and, given the right conditions, may produce highly generalizable findings. Questionnaires, surveys, and the like provide of means of generating very different kinds of knowledge. These instruments tell us not about causes, but instead allow us to describe populations and highly particular kinds of problems. Numerical Likert scales are one way of accessing this kind of information but, as qualitative researchers can attest, their popularity has by no means ridden the field of nuanced description. Thick description continues to exist alongside thin.

Likert scales, while dominant today, were not immediately heralded as the solution to the discipline's difficulties with mass data collection (e.g., Barclay & Weaver, 1962; Butcher, 1956; Edwards & Kenney, 1946; Edwards & Kilpatrick, 1948; Ferguson, 1941; Kriedt & Clark, 1949). Just how these kinds of questionnaires assumed their current ubiquity remains to be explored. Undoubtedly part of this success is rooted in practical considerations. By flattening experience to no more than a number on a scale, Likert scales generated information that was far easier to

⁴⁸⁷ Theoretical and critical discussion of the use of rating scales, and self-report data more generally, in psychology can be found in Haeffel and Howard (2010), as well as Rosenbaum and Valsiner (2011).

refine, combine, and plot. Mental life was rendered unidimensional and richly detailed accounts were replaced with the simple accounting of more or less on a linear scale. No longer was experience in all its diversity the hallmark of questionnaire research. This kind of depiction of mental life surely served to create particular kinds of selves and social groupings. The particular looping effects at work here (see Pettit, 2013a), are ripe for investigation as means of further historicizing the place of questionnaires in twentieth century psychology and society more broadly.

The shape of the public's engagement in psychological research has changed considerably over the discipline's history. Questionnaires, in particular, have adopted new mediums of existence and methods of dissemination. Rather than print requests for information in scientific and popular periodicals, as was done in the final decades of the nineteenth century, questionnaires are increasingly digital born, able to be accessed and completed on the Internet by individuals in many locales. This both simplifies and complicates efforts to amass significant quantities of data with questionnaires. At the same time, the move from paper to digital form once again altered the relationship of the public to questionnaires, while an attendant interest in big data has also influenced the broader collection practices of the discipline.

In the present moment Big Data is bigger than ever.⁴⁸⁸ As questionnaire-based endeavours in early American psychology attest, big data in this field is not new. Practices of mass data collection have been a part of psychology from its very inception and continue in the discipline today. Yet, the challenges faced in work with Big Data today are necessarily different from those encountered by psychologists in the final decades of the nineteenth century. Engaging directly

⁴⁸⁸ The literature on Big Data is itself massive. A small sampling of recent discussions of the subject, both within the history of science and more broadly, includes Boyd and Crawford (2012), Crawford, Gray, and Miltner (2014), Jurgenson (2014), Michel and colleagues (2011), Stevens (2013), and Weinberger (2011).

with big data, especially when experimental manipulations come into play, can be controversial as recent response to a psychological experiment involving nearly three-quarter of a million unaware Facebook users attests (Kramer, Guillory, & Hancock, 2014; Verma, 2014). Even in circumstances that do not involve these kinds of manipulations, Big Data comes with distinct challenges even with advances in techniques of data collection and analysis. New means of synthesizing great masses of material have, in some respects, taken us further than ever from meaningful findings. Just as in questionnaire researchers' earliest efforts to produce coherence from great masses of descriptive material, the meaning in the mass often remains elusive today. Important questions regarding the continuing collection practices of psychology, especially in the realm of Big Data, remain: What does collecting masses of material entail in our increasingly digital age? How does the public figure into this kind of collection? And how do individuals understand themselves in relation to work rooted in forms of Big Data that purport to include them?

This dissertation has spanned more than fifty years of disciplinary history to elucidate the changing character of the questionnaire in American psychology. In doing so, I have argued that a natural historical orientation has figured into psychological practice from the field's inception. This orientation facilitated a moral economy of data, wherein the objects and practices valued in the discipline are ones related to mass data collection. The privileging of data eventually fostered a move from the collection masses of descriptive material, of the kind central to early questionnaire endeavours, to the numerical forms of data produced by Likert scales. This was a shift from thick to thin description. In the process, questionnaire research's relationship with the public also changed. In the discipline's earliest questionnaire investigations members of the public were often integral to, though never full participants in, the research process. By the

1920s, the public was no longer invited to engage with questionnaire research in this way.

Instead, these instruments increasingly served as means by which to make pronouncements about the state of the public across various domains.

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