Edmund Burke Huey: The Formative Years of a Scholar and Field

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INTRODUCTION

On Thursday, January 1, 1914, a group of people met at a hilltop cemetery overlooking the small town of Connell in eastern Washington State. They had come to pay their last respects to Professor Edmund Burke Huey.

If his life had not been cut short by illness, he might have been expected to live until 1950 or 1960. Instead, the first snow of the winter of 1914 covered his headstone, which bears the inscription, "Not my will, but Thine, be done" (Luke 22:42, KJV).

The greatest achievement of my great-great uncle's life was this book passed down to my grandmother, *The Psychology and Pedagogy of Reading* (Huey, 1908). Written a century ago, it explains how lifeless images on paper are transformed into thought, in all of its various forms as they pass through the reader's eyes and into his mind.

To this day, Huey and his pioneering work are still held in great regard by many in the fields of education and reading, and there is probably no one more appreciative than is Dr. Douglas K. Hartman. Today, it is my pleasure and honor to present to you Professor Hartman...as Professor Edmund Burke Huey.

(by David H. Davis, Great-Great Nephew of Edmund Burke Huey)

BACKGROUND

Ladies and gentlemen, colleagues and friends, I am Edmund Burke Huey. My colleagues called me by my last name: *Huey*. You should, too.

I come from the beginning era of reading research, which started in the 1890s and ran through the first decade of the 1900s. It was a time very different—and yet like—your own (Monaghan, Hartman, & Monaghan, 2003). There was a sense then that great things could be done ... and must be done. Grandiosity was an academic virtue to be embraced, and humility was a virtue not to be practiced. There was also great faith in science and the scientific method (Albrecht & Hull, 1977). As my doctoral advisor told me when I first met with him in 1897, "I am expecting 'great things' from you." It was a time when great scientific things were to be done by individuals and institutions just because they were to be done ... and needed to be done (Rasenberger, 2007).

I passed away many years ago, in December of 1913, but I've been resurrected in spirit for this research address to celebrate with you the first hundred years of reading research on this continent. As NRC's 57th conference draws to a close, I've been asked by your incoming President, Norm Stahl, to make a very different kind of research address this year ... that is, to make sure that the

last words spoken at this year's NRC meeting are from the first reading researcher on the North American continent...from me, Huey.

As many of you know, I published a book in 1908 titled *The Psychology and Pedagogy of Reading* (Huey, 1908). A century after its publication it is considered by many to be a classic in the field of reading. In fact, a survey conducted by one of your NRC colleagues some years ago found that my book is one of the top 10 "classic" pieces in reading research (Froese, 1981, 1982). It is also considered to be *the* book that marks the beginning point of reading research in North America. In the pages of this book I pulled together all the extent research at the time—which was 1908—from both sides of the Atlantic and synthesized it. It required reading works in English, German, French, Italian, Latin, and Greek. This was no small fete, as you can imagine.

To put things in perspective, my book on *The Psychology and Pedagogy of Reading* was really the first *Handbook of Reading Research*. It wasn't until 75 years later—in 1984—that David Pearson, Michael Kamil, Peter Mosenthal, and Becky Barr (Pearson, Barr, Kamil, & Mosenthal, 1984) edited the second *Handbook of Reading Research*, with the third and fourth volumes to follow. To your credit as a field, you've followed my lead. About every decade you've taken stock of your research efforts by producing another handbook.

All of this is to say, because you do research on reading and literacy, you have lived your professional life under the influence of my 1908 book, knowingly or unknowingly. If by chance you don't know my name or my work, you *will* by the end of this keynote. But I'm here to talk about much more than my name and work—I will speak about my formative years, my graduate studies, and my years as a professor. Not many scholars know about my life and there may be some historical lessons in it for your generation. As you know, one way to understand a book well is to know the conditions that shaped its author. By knowing me, I think you will come to know my book better, which many of you have read—and if you haven't, you'd better download the PDF copy and take it on your holiday travels. It is a book to know well—several times over—whether at the beginning of your career or at another point.

I do want to make one special point: This is a *living-history* research address, rather than an *academic autopsy* of a research body. NRC has always been a place where the play of forms and genre have been given *some* license; my genre-bending keynote follows in that playful scholarly tradition (Moore, Monaghan, & Hartman, 1997). As you know, embodying the analysis and presentation of historical data can be a challenge greater than a straight-on research address. So be on the lookout for the rigors peculiar to the historical scholarly form (Hartman, 2003; Stahl & Hartman, 2004).

To be sure, the data used for this address are extensive—they come from nearly 50 archives, societies, and individuals in North America, as well as the European continent. My great-great nephew, Dave Davis, and his wife Lorrie, of Auburn, Washington, who are visitors at this 57th Annual NRC meeting, were an invaluable source.

The analysis of data used for this address is exacting—it is squarely in line with the most rigorous historiography practices of today's scholarship (Monaghan & Hartman, 2000, 2001, 2002). And what about the presentation of findings? Well, it is a blend of clear reporting and theatrical license. There's really no better way to communicate about my life, our common work, and the future before us than by embodying it (Hartman, 2007a; Hartman & Sears, 2004, 2005, 2006). Otherwise you'll fall sleep. If by chance you do fall asleep this morning, make sure you hear

this: A biography of my life is being written by two of your NRC colleagues, Linda Kucan and Doug Hartman (Hartman, 2005, 2007a, 2007b). Their biography will tell the full story.

As I begin this morning's keynote, I humbly offer this purpose for your listening: Listen not only to the story of my life as a scholar, but listen for your own story. Use my personal history as a way to think about yours … and the history of NRC … and the history of our field this last century. No one I know ever fell asleep listening to their own stories.

BIOGRAPHY

I begin by telling you a little about my life. Not all of it, but something about several formative periods so that you'll understand my book on *The Psychology and Pedagogy of Reading* a little better when I get to it in a few minutes—or when you read it on your winter vacation (Carroll, 1968; Edfeldt, 1990; Notes and news, 1914; Obituary, 1914; Reed & Meyer, 2007; Scientific notes and news, 1914).

Youth

I'll start with my formative years. I was born into the Huey family in 1870, just a few years after the Civil War ended. One aspect of my youth is worth noting. The small village I grew up in was named…Huey. It is located about 60 miles due north of Pittsburgh, Pennsylvania. The community was named after my grandfather, who was a prominent farmer in the community. The village still exists today.

All the communities that surrounded the village of Huey—like Rimersburg, Sligo, and so on—borrowed their names from the towns of Ireland. Even the birth names of our village's newborns were borrowed from across the Atlantic. Take my name, for instance, *Edmund Burke Huey*; the first and middle names—*Edmund Burke*—were borrowed from the Irish politician—Edmund Burke—who served for many years in the British House of Commons.

Burke was astonishingly skilled at two things. He was an *innovative thinker* and he was a *skilled debater*. Few peers in the House of Commons could create, debate, and legislate like him. As his namesake, I was expected to do the same. And I did. I never minded living up to the aspirational expectations of the name Edmund Burke—it was a prophetic fit with my temperament and habits of thought (Crowe, 2003; Dickson, 1999; Gibbons, 2003; Lock, 1998, 2006; O'Brien, 1994).

So, my identity at an early age was forged in the new-world village of Huey, Pennsylvania, where the old-world shadows of Europe still reached across the Atlantic. To the extent that a person's name can shape a person's formative years, I was raised by a family and community where the signals pointed me to become like Edmund Burke. I took those signals to mean that I should think originally and argue well with others. Which I did … to my family's chagrin. If you've read my book *The Psychology and Pedagogy of Reading*, you've seen the extension of these qualities in the evidence and arguments. The new-world innovations and old-world shadows are in every chapter of the book. But I don't want to get ahead of myself by revealing too much just yet.

Teaching

Like many of the school-oriented young men of my day, I moved in the practical direction of teaching. I earned my teaching certificate at a Normal School in Valpraisio, Indiana. I then taught at

schools in Indiana and Pennsylvania. I continued on to college, graduating from Lafayette College in 1895. The two years immediately following college I taught Latin at the Harry Hillman Academy in Wilkes-Barre, Pennsylvania.

It was here, in 1896 that a turning point took place. As you can imagine, teaching adolescent boys to read a dead language like Latin was no small task for a teacher. Doing so challenged me to think in new ways ... struggling readers have a way of doing that to their teachers, as you know. In my search, I crossed paths with the books of two scholars who were part of the emerging new field of psychology.

One scholar was William James from Harvard, who had published his two-volume work on *The Principles of Psychology* (1890), six years earlier. I don't know if you've read these companion books, but they were a monument ... 1,200 pages in all. I couldn't put them down, heavy as they were. James' synthesis of the scholarship was comprehensive beyond compare. It took him 12 years to write the volumes, and it showed in every chapter. His basic point was this: The philosophical and psychological ideas that reached across the Atlantic were irrelevant for a new and modern psychology. James argued for a more functional way to conceive of the human mind. At the moment, I needed a more functional way to teach the minds of striving adolescents in my classes to read Latin. So my reading of James provided that functional way (Cooper, 2002; Feinstein, 1984; Richardson, 2006).

I didn't realize it at the time, but the influence of James on my teaching and research would show through in more ways than one. For instance, years later, when I was Professor of Psychology and Pedagogy at the University of Pittsburgh, students assembling the yearbook drew a cartoon figure of me fishing in a stream ... with a book in my coat pocket (WUP, 1907).

If you look closely at the cartoon, you can see that there are copies of James' (1890) books in my coat pocket. I think this cartoon is a powerful visual metaphor for James' central idea: that to find the meaning of an idea, you have to look at its consequences (Menand, 2002). Fishing is like that: to find where the trout lie in a stream, you have to evaluate the consequences of different possibilities—Here in the shade to hide from their prey, or over there, at the end of a run, where the food funnels down into a channel. The consequences of your cast tells you everything about trout. My students at Pittsburgh had learned James well, and had the insight to see how his central idea had become my mode of thinking and acting in the world—even when it came to my fishing.

The other scholar I crossed paths with in my reading was Wilhelm Wundt, from the University of Leipzig. I read his multi-volume *Principles of Physiological Psychology* (1873, 1874) in the original German. In those pages, I first learned how to be methodologically innovative and how to build an empirical argument. Wundt could do both exceedingly well, almost to a fault (Bringmann, Balance, & Evans, 1975; Farr, 1983; Rieber & Robinson, 2001; SEP, 2006). You see, he wrote prodigiously about so many new findings and explanations that a number of his research contributions went unrecognized for nearly a century. For instance, Wundt's discovery that short-term memory was limited to seven or so "pieces" of information didn't reach the light of day until George Miller's (1956) article in the Psychological Review on the "Magic Number Seven, Plus or Minus Two" (http://webspace.ship.edu/cgboer/wundtjames.html).

Anyways, by borrowing experimental methods from the study of physiology, Wundt devised an experimental psychology that carefully observed simple events, using introspection. I innovated

on Wundt's innovation some years later in my dissertation—by analyzing what I originally thought were the simple events of reading—eye movements. As you know, eye movements are not simple.

All of this is to say James' and Wundt's books were a quick and profound turning point. After reading them, I decided to quit teaching and pursue a Ph.D. I wrote for information about graduate study in psychology. And I continued to reread James and Wundt ... experimenting with their ideas and methods—on myself and in my Latin classroom.

The point in telling you about my decision for graduate school is this: We all have a book or article in our past—which we probably read while a teacher or graduate student—that turned our head right around. That piece had an idea, a question, a method, an argument, a perspective, or a style that made possible an otherwise unimaginable direction ... which was to leave what we were doing ... so we could think about our need or question in a new way—in a scholarly way. And ever since, that book or article has played itself out in various ways as we've defined the movement of our scholarly thought and action.

For me, it was the works of James and Wundt. For you, it was probably someone else. The effect on me was to think about graduate school—seriously—for the first time. Your book or article probably had a similar effect. For both of us, it was about a choice ... to pursue the academic path. In my case, you have the opportunity to see how James and Wundt's early influence shaped my later book on *The Psychology and Pedagogy of Reading* when you read it again, or for the first time.

Clark

Where did I end up going to graduate school? The answer to that question requires some explanation. In the late 1800s, the United States was perceived by most Europeans as an intellectual backwater. In most respects, that assessment was correct. There simply wasn't the intellectual capital to prepare home-grown doctoral students for faculty positions in the U.S. Certainly there were colleges in the states that prepared young men—and fewer women—for professional and leadership roles. The growing towns, churches, governments, hospitals, and schools across the continent needed their skills and knowledge. But when it came to getting a Ph.D. for a university position, most everyone went to Europe. There were exceptions, but more than 9,000 Americans studied in Germany alone during the 1800s (Menand, 2002). All of this is to say, I dreamt of following the path of others to Europe for my Ph.D. The shadow across the Atlantic loomed large in my life ... it had named the towns of my youth ... and named me, too. I hoped that following that path could offer me the education of a lifetime.

Wundt, at the University of Leipzig, was an obvious choice for study. I had read his work in the original. But there were other choices, too. As I wrote letters of interest and read of the possibilities, my dream of a European education was deferred. The cost of studying on the Continent was beyond my reach. In fact, the cost of studying in the U.S. looked beyond my reach, too. I had saved money from my meager teacher's salary. But I needed a fellowship or assistantship to cover the remaining costs. Like many doctoral students of today, I looked for the best program that could help support my educational expenses. James at Harvard was a real possibility in my mind. He had been a key player in forming the new field of psychology. But there were only two Universities that fit my criteria of excellence and financial support. So I wrote to both of them: James McKeen Cattell at Columbia University in New York City and G. Stanley Hall at Clark University in Wooster, MA.

Cattell had studied in Wundt's psychology lab at the University of Leipzig (BDNAEE, 1997; Cattell, 1914; Poffenberger, 1947; Sokal, 1981). Hall had been a student of James at Harvard (Hulse & Green, 1986; Pruette, 1926; Ross, 1972). So if I couldn't do my doctoral work with James and Wundt directly, I could study with their star doctoral students: Cattell and Hall.

Cattell really wanted me to study with him at Columbia. We were acquaintances from my undergraduate days at Lafayette College. He and I exchanged several letters in which I made clear my need for some financial support to help pay tuition at Columbia. When it came time for me to make a decision between Columbia or Clark, he was not able to offer what I needed financially.

So I headed for Clark University in central Massachusetts, with G. Stanley Hall as my advisor. He offered to cover the full cost of my studies with a fellowship, which I learned was the case for all doctoral students accepted to Clark. Such an offer made possible the way forward for me. I became a "Clark man," as we were called at the time.

To become a "Clark man" meant three things. One was that I would study at *the* place for psychology research in the late 1890s and early 1900s. Clark had become the place largely because of G. Stanley Hall, who was President and Professor of the institution (Koelsch, 1987; Kump, 1996; Ryan, 1885). Hall and the other productive faculty at Clark played a prominent role in the development of psychology as a discipline in the U.S. When I went to Clark in 1897, it was referred to as "the American Mecca for aspiring young psychologists." As one of my classmates explained, "I had thought of no other university for my doctorate" (Terman, 1930).

There were consequences to choosing Clark for doctoral study. The mantle of expectations laid on Hall's shoulders as a young scholar were in turn laid upon ours. He made this clear when we nervously met with him for our first advisor appointment: After assuring us that our "splendid training" and the "fine report" from references would serve us well as psychologists, he left no doubt that he was expecting "great things" of us. It was a powerful and frequently used pedagogical device of his: He affirmed our past and present accomplishments by setting the bar for our future performance astonishingly high. As students, we fully felt the sense of responsibility put on our shoulders by Hall's words "I'm expecting great things of you." Given those expectations, we learned to always live in a hurry. Hall's tempo was one of urgency. To him, being called to this newly emerging profession of psychology meant one thing: Focus your intellectual energies on the truly big problems and then reason about them rigorously from data. Why? Because the opportunities are nearly boundless, the problems are painfully great, and your time on this earth is short.

Becoming a Clark man meant a second thing: I was fully and generously supported by a fellowship for my entire doctoral experience. Remember how my graduate school decision was largely driven by financial need? Well, because I was fully and generously funded ... the full force of my effort was focused on my doctoral experience.

Finally, becoming a "Clark man" meant I would study in one of the most unusual Ph.D. programs in America at the time, and probably since. It's hard to describe in a few sentences what it was like to be a doctoral student at Clark in the late 1890s—because it is so different from most doctoral programs today. One of my classmates, Lewis Terman (1930), described the Clark experience like this:

> The Clark of my day was a university different in important respects from any other that has ever existed in America, if not in the world -- in spirit much akin

> to the German university yet differing from it because of the small student body. It enrolled in all its departments only about fifty full-time students ... [with] thirty of the fifty ... primarily for psychology, philosophy, and education. The informality and freedom from administrative red tape were unequalled. The student registered by merely giving his name and address to President Hall's secretary. He was not required to select formally a major or a minor subject. There was no appraisal of credentials for the purpose of deciding what courses he should take. Lernfreiheit [lairn-FRY-hite] was utterly unrestricted. (Terman, 1930)

Lernfreiheit. The German word that my Clark classmate used, Lernfreiheit, was one of the four defining elements of my doctoral experiences at Clark. The idea of Lernfreiheit was carried to the United States in the 19th century by scholars who had studied at German universities, which Hall had done. The word literally means "learning liberty." In practice, the idea meant that I as a student was free to pursue my own course of study, taking whatever courses I wanted to take whenever I wanted to take them. Put another way, there were no required courses, credit requirements, or grades for a Ph.D. at Clark. It was a graduate student's dream.

Here's how one of my classmates described the culture of Lernfreiheit at Clark:

> There were professors who proposed to lecture and there were students who proposed to study; what more was necessary? The student could go to three or four lectures a day, or to none. No professor, so far as I could see, kept a class list. Attendance records were, of course, unheard of. No marks or grades of any kind were awarded at the end of the year or semester. One could attend a course of lectures all year without being required or necessarily expected to do the least reading in connection with it. There were no formalities about candidacy for a degree. The student was allowed to take his doctor's examination whenever the professor in charge of his thesis thought he was ready for it. No examination except the four-hour doctor's oral was ever given.
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> A professor lectured only three or four times a week and on whatever subject he pleased. Lehrfreiheit [teaching freedom] was as unrestricted as Lernfreiheit [learning freedom]. There was no effort to make the courses of different professors dovetail. Chamberlain gave only two lectures a week; Burnham three, besides a seminar; Sanford two or three in addition to his laboratory courses; and Hall usually four, besides his weekly four-hour seminar.
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> I think the Clark situation as I have described it was of almost crucial importance in my development. I have never worked well under the restraint of rules and regulations, and it is hard to imagine a régime that would have been better adapted to my temperament than the one I found at Clark, if régime indeed it could be called. Because I was placed absolutely on my own responsibility, I was able to give my best with unalloyed enthusiasm. It is a method which affects not only the quality of a student's work, but also the nature of his later output. It is conducive to intense concentration and monographic production, rather than to well-rounded scholarship and the production of systematized treatises of the textbook variety. (Terman, 1930)

I thrived in this culture of Lehrnfreiheit, too. It was an uninhibited place. It was a form of graduate study unencumbered by older ways of thinking and doing.

Seminar. The second defining element of my doctoral experience at Clark was the Monday evening seminar held at G. Stanley Hall's house from 7:15 in the evening until midnight. It was not a course, but everyone was expected to attend. Two graduate students presented work they were doing in the psychological lab at these seminars. Thirty or so graduate students typically filled the room on Monday nights. No student missed the opportunity to present and debate with other Clark men at this Monday night gathering; I was no exception. One of my Clark classmates described the seminar in his biography:

> When Clark students of the old days [got] together, their conversation invariably revert[d] to Hall's seminar. All agree that it was unique in character and about the most important single educational influence that ever entered their lives. No description could possibly do it justice; its atmosphere cannot be conveyed in words. It met every Monday evening at 7:15 and was attended by all the students in psychology, philosophy, and education; in my day about thirty in number. Each evening two students reported on work which had occupied the major part of their time for several months. Usually we knew in advance who would hold forth, and an air of expectancy was general. If the reporting student was one whose ability and scholarship commanded respect, we were prepared to listen and learn. If he was an unknown quantity or was regarded with suspicion, we were prepared to listen and criticize. The longer or more important report came first. It was always under way before 7:30 and might last an hour or longer. Ordinarily, though not always, it was read from manuscript. It might be either a summary and review of the literature in some field or an account of the student's own investigation. When the report was finished Dr. Hall usually started the discussion off with a few deceivingly generous comments on the importance of the material that had been presented, then hesitantly expressed just a shade of doubt about some of the conclusions drawn, and finally called for "reactions." Sometimes when we were most critically disposed Dr. Hall's initial praise of the report momentarily spiked our guns. Soon, however, a student bolder than the others would dare to disagree on some fundamental proposition; others would then follow suit, and the fat was in the fire. When the discussion had raged from thirty minutes to an hour, and was beginning to slacken, Hall would sum things up with an erudition and fertility of imagination that always amazed us and made us feel that his offhand insight into the problem went immeasurably beyond that of the student who had devoted months of slavish drudgery to it. Then we were herded into the dining room, where light refreshments were served, and by 9:30 or so we were in our chairs listening to another report. Sometimes the second half of the evening was even more exciting than the first half, and we rarely got away before eleven or twelve o'clock. I always went home dazed and intoxicated, took a hot bath to quiet my nerves, then lay awake for hours rehearsing the drama and formulating the clever things I should have said and did not. As for Dr. Hall, he, as I later learned, always went upstairs to his den and finished his day by reading or writing until 1:00 A.M. or later. So inexhaustible was his energy!
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> If there is any pedagogical device better adapted to put a man on his mettle than a seminar thus conducted, I do not know what it is. To know that his contribution would be subjected to merciless criticism from every angle was enough to arouse even a naturally indolent person to Herculean effort. In preparation for the report, the student was likely to cut all his lectures for a week or so [if he was attending any] and to reduce his sleep to half the usual amount. If the report met with general disapproval, it was sometimes followed by a collapse of nerves that

> would send the poor victim to bed; in one case, by a breakdown that necessitated several months of vacation. (Terman, 1930)

I presented at one of these seminars. But there was no "collapse of nerves" for me ... rather, the mettle of my "Celtic tiger" showed through.

Elbow learning. Lest you think that all I did at Clark was cut lectures and tear into classmates' ideas at the Monday night seminars, there was a third defining element of my doctoral experience. Hall called it "elbow learning." It meant that I worked side-by-side, elbow-to-elbow with my professors and classmates in the research labs, which were really like workshops. "Elbow learning" dominated my graduate studies at Clark.

These lab settings were where the teaching and learning action took place for me at Clark. If you'll just imagine for a minute, this is where I carried out my dissertation study using cocaine to anesthetize the eyes of my professors so I could insert a porcelain eye cup with a short stylus on it to record their eye movements on a rotating cylinder. It was a very ingenious method for data collection, but one that would be troublesome in making it through your IRB process. Who would approve the use of cocaine in a reading research study of today? Especially for use with their professors?

"Elbow learning" was where graduate students and faculty worked together next to each other as colleagues, rubbing elbows and learning from each other in the nitty-gritty work of the research process. It was in these lab settings that we learned the research methods for scholarly work. There were no research methods courses or research credit requirements to meet. Instead, we talked through, hammered out, and puzzled around the rigors of ensuring that our method was sound and could withstand the probing of open inspection. The opportunity of "elbow learning" suited me and my classmates quite well. It assumed that faculty and doctoral students both brought important intellectual capital to the work at hand; in practice, "elbow learning" made the bringing of this capital possible.

Publishing. The fourth and final defining element of my doctoral experience was the all-out effort to "publish early and often." While the mantra to "publish early and often" never appeared in writing or spoken word, it was the organizing principle for all doctoral activity at Clark. We were to engage with problems, peers, professors, and the profession as intellectuals at all times, so that we could get published during the first year and every subsequent year of doctoral study. Quantity didn't matter so much as the experiences that constituted the pathway toward publishing our work. But the number of publications was certainly a byproduct of the "hot house" environment of Clark.

I hit the ground running when it came to publishing. During my first year at Clark I published a study in a top-tier research journal—which served to be the groundwork for my dissertation research (Huey, 1898). Before the journal pages even had time to cool down from the hot type plates, I was back at the lab workbenches rubbing elbows in preparation for a Monday night seminar that would sharpen my ideas for the dissertation study, which I completed a year later. The culture of doing a dissertation at Clark was peculiar by your modern standards. There was no formal dissertation document. Ph.D. students got out of Clark when—and only when—they published their dissertation work in a research journal. How would that milestone sit in your doctoral programs of today? We had no lengthy five-chapter documents at Clark that included full appendices and all the redundancies that many of the dissertation documents of today do. Clark

men received their diploma when they published their dissertation in a cogently written research journal or full-length book.

Given my type-A temperament, I did twice what was required for publishing my dissertation study. I wrote two journal articles from my study, which appeared one year apart. One in 1900 and the other in 1901 (Huey, 1900, 1901). All told, when I graduated from Clark I had three journal articles listed on my CV that were published in the top peer-reviewed psychology journal of the time. In its day, this was startling productivity for a newly minted Ph.D. My two closest classmates did the same. Lewis M. Terman produced three journal articles while studying at Clark (Terman, 1904, 1905, 1906). Henry H. Goddard, another Clark man, produced two journal articles and a book while studying at Clark (Goddard, 1899; Goddard & Schreuder, 1908; Johnstone & Goddard, 1905).

While the three of us are hardly a random sample, we were not unusual or exceptional in our publishing output for doctoral students. For a trio of Clark men, we were representative in every respect. Collectively, we published eight peer-reviewed journal articles and one book. Conference presentations, book chapters, and grants were not standard fare for doctoral students at the time. But our articles and books were impressive results for any three-person cohort in a U.S. doctoral program then—or even now.

Professorships

When I graduated from Clark there were jobs for professors everywhere in the U.S. It was the "wild west" era of university building (Lucas, 2006; Rudolph, 1990; Thelin, 2004; Veysey, 1970). Many, many faculty were needed to design programs and teach courses in nearly every state.

To find these jobs, I did the two things that every young faculty member of my day did: First, I wrote letters of recommendation for myself. You think I'm kidding, but I'm serious. I learned how to write these letters from my graduate school advisor, G. Stanely Hall. He was a master at self-promotion—and expected us to be the same. After all, who was better informed at helping me put my own best foot forward than me? Remember what I said earlier, "This was an academic era when grandiosity was a virtue to be embraced."

Second, I got on a train and traveled the country, stopping at as many places as possible. For example, one of my trips took this route: I boarded in Worcester, MA heading west through the Grand Canyon to Los Angeles. I spent a month there visiting friends and engaging with colleagues at California State Teachers College, which is now UCLA. I then went north to Stanford University, where I gave a talk and spent two weeks with colleagues there. I was offered a position at Stanford several months later, but turned it down because I wanted to live east of the Mississippi—where the critical mass of intellectual activity was located at the time. I suggested to the Dean at Stanford that he hire one of my Clark classmates, which they did.

I continued north to Eugene, OR and then Seattle, WA, where I made presentations and talked with colleagues about the possibility of working at the Universities of Oregon and Washington. After which I made the long trip back to Pennsylvania, stopping in Chicago to see what I could learn and do there. That's how a new Ph.D. pursued the ample supply of job opportunities in the early 1900s. It was a buyer's market, there simply weren't enough new Ph.D.s to cover all the openings.

With my sights on a prestigious faculty position, I unfortunately made more moves than a new Ph.D. should make. In the 12 years after my Ph.D. I held positions at eight universities and institutions:

- Minnesota State Normal School (Moorhead, MN)
- University of Berlin (Germany)
- State Normal School (Oxford, OH)
- Clark University (Worchester, MA)
- Western University of Pennsylvania (Pittsburgh, PA)
- University of Paris (France)
- Illinois State School & Colony (Lincoln, IL)
- Johns Hopkins University (Baltimore, MD)

The eight positions were scattered in the northeast quadrant of the U.S. and in western Europe. By moving so much, I sowed the wind and reaped the whirlwind. I did remain productive, but less than I could have if I had rooted myself, at least for more than a year at one institution. The upside of moving around, though, was that I remained unhindered by past relations and ideas. The arc of my ambition was to harvest a great idea or two that would be professionally and personally rewarding—but would benefit others more than me.

Pittsburgh. Given all the moving around I did, the one institution where I spent more than one year at was the Western University of Pennsylvania (WUP), which today is known as the University of Pittsburgh. Because I grew up in the village of Huey, just north of the steel city, it was like going home. The four years I spent in Pittsburgh were the best years of my academic life. I was hired to do three things at WUP: start a psychology department, start a school of education, and start a psychological lab like the ones at Clark and Leipzig. Never shy of challenge, I did all three. The university had great ambitions, and so did I.

I used a "tornado of talents" to build these programs at WUP. As if this wasn't enough, my teaching loads were exceedingly heavy. One term I taught 10 courses. I also chaired several important university-wide committees, including the library committee. And I advised every single student that entered the programs I was building. Was I intense? Yes, and it took a toll. I twice took leaves from academic work because of exhaustion, once was from my faculty position in Pittsburgh. Note though, as is often the case with accomplished academics, I never minded the many responsibilities. The work was too rewarding to do. As you might imagine, the most rewarding part of my four years in Pittsburgh was writing my first book, *The Psychology and Pedagogy of Reading*, all 469 pages. I was 38 when it appeared in print, but felt like a younger man.

CLOSING

In most every way, the publication of *The Psychology and Pedagogy of Reading* was my swan song to reading research. I subsequently published another book and several journal articles on the "feeble minded" (e.g, Huey, 1912, 1913), but nothing more on reading research or practice. Like my graduate school advisor G. Stanley Hall, I had learned to be an intent reader of academic and

societal signals. The winds of research in psychology were blowing in a new direction and I sailed with them. But not for long.

On December of 1913 I passed away at the age of 43. Life has a way of doing that sometimes ...ending when it seems like it is just started. In my brief 13-year career I had sowed the wind and unleashed a whirlwind with my book on *The Psychology and Pedagogy of Reading* (Huey, 1908), with the hope that others would get caught up in the whirl, too. I see from the 57 years that NRC members have been sharing their research at this annual meeting that my hope was not in vain.

To commemorate the centennial year of my book, three events are planned—modest as they are. The first is this research address, which is intended to provide background and context for my life and book. The third event is a symposium at the annual meeting of the International Reading Association, sponsored by the Reading Hall of Fame (of which I am a member). The program for the IRA symposium focuses in detail on my book, with papers presented by colleagues on:

1. An introduction to E. B. Huey's 1908 Work (Rob Tierney, University of British Columbia)
2. Huey's Perspectives Related to Whole Language Research and Pedagogy (Yetta Goodman, University of Arizona)
3. Huey's Work on Automaticity, Fluency, and Comprehension in Reading (Jay Samuels, University of Minnesota)
4. Huey's Thoughts on Oral Language in Relation to Reading: Toward a Multiple Life Cycles Education Policy (Tom Sticht, International Consultant in Adult Education)
5. Huey's Work and Its Impact on Scientific Research on Reading (Patrick Shannon, Pennsylvania State University)
6. Discussant (Alan Farstrup, Executive Director, International Reading Association)

Between these two public events is a personal one...which rests in your hands. Commit yourself to the reading of *The Psychology and Pedagogy of Reading* (Huey, 1908) during 2008. If you have not read it, you have every reason to do so. If you have read it, then read it again for the first time. It is, after all, the first handbook of reading research.

I end by doing three things at once. One is to express deep gratitude to Dave and Lorrie Davis of Auburn, Washington. They have been faithful stewards of their family history that makes a biography of me possible. From those who have committed their professional lives to the study and teaching of reading, we cannot thank Dave and Lorrie enough.

Another is to put a piece of reading research history in your hands. Across the front of the this large meeting room are several of the historical artifacts that Dave Davis, Linda Kucan, and Doug Hartman have gathered in their effort to chronicle my life. You are welcome to examine these item, which include: my Passport, the diploma for my Ph.D. degree at Clark University (Dave and Lorrie Davis are generously donating these first two items to the Special Collections at the University of Pittsburgh, where they will be carefully stored and made available for scholarly work), my application letter to the Western University of Pennsylvania (now the University of Pittsburgh) for their first faculty position in psychology and education, letters of recommendation written about me by colleagues around the U.S. for the position at Western University of Pennsylvania, a copy of my book that has been handed down several generations from my sister to the Dave and Lorrie

Davis family, and my letter to the President of Stanford University in 1911 inquiring about a faculty position in psychology at the university.

Finally, I must thank you. You have pressed onward in ways that I could never have imagined. As the premier reading research organization, the National Reading Conference has provided ways for these unimaginable issues to come to light. As I sat in my Pittsburgh study one evening in December of 1907, putting the final touches on the manuscript for *The Psychology and Pedagogy of Reading*, I had a sense that others might join the effort to study these issues. At the time, the only way to voice this sense of growing effort was to thank the colleagues who had helped in forming of the book. My acknowledgements read:

> To my fellows in research, whose investigations of reading and language are here joined with my own, this volume is presented by the author in the hope that it may render service, and with respectful appreciation of their part in its production.

I see today what I could not fully see that snowy December night in Pittsburgh: my hope has come true. *The Psychology and Pedagogy of Reading* has 'rendered service' to a growing profession of 'fellows' who study reading. Its most enduring service has been as a chronological anchor point to the endless flow of changes that have shaped the study and practice of reading. Today, as you navigate the newer worlds of online, out-of-school, mandated, and socially just reading while the older-world shadows of book culture are very much present, a volume like mine from the past just might be the one book for the many parts of the reading research community. Just as my head was turned around by the reading of James and Wundt's books, *The Psychology and Pedagogy of Reading* might be the book from the past that turns your head toward an idea, a question, a method, an argument, a perspective, or a style...and makes possible an otherwise unimaginable direction.

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