Increase Allen Lapham: Wisconsin's First Geologist

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Ever since my first landing in Wisconsin I have given more or less attention to its geology, improving every opportunity to examine the stone quarries, rock cliffs, &c; and have furnished several papers upon the subject. In 1855 and again in 1869, I published Geological Maps of the state.

–Increase A. Lapham, 1875

Wisconsin engineers claim Increase Allen Lapham (fig. 1) as their professional colleague. So do botanists, zoologists, and archaeologists. Surveyors insist that Lapham's central professional identity is of that calling. A distinguished meteorologist once expressed disappointment because a presentation about Lapham did not stress Lapham's contributions to meteorology, especially considering that Lapham had a substantial claim as a founder of the U.S. Weather Service.

Educators, geographers, limnologists, cartographers, and ecologists all can make a case that Lapham was among the earliest in Wisconsin to practice their disciplines. Lapham served all of these pursuits and therefore all of his collegial descendants can justifiably lay claim to him. His list of plants in the Milwaukee area was the first scientific paper from Wisconsin, his geography the first book published in Wisconsin; he was the first to survey and document the effigy mounds of Wisconsin, and his was the first locally produced geological map. Even his self-assessment was encompassing, according to his Milwaukee acquaintance, Samuel S. Sherman: "When asked by a gentleman well known in scientific circles, in what department of science he was laboring, he replied: 'I am studying Wisconsin' " (Sherman, 1876, p. 51).

But for the purposes of the North-Central Section of The Geological Society of America, it is justifiable to assert that Lapham was the first important Wisconsin geologist. In fact, geologist may be the identity that Lapham may have taken for himself. It certainly was his first scientific interest, for, as he wrote later in an autobiographical sketch, as a boy of 14 when he was helping his brother Darius as rodman on the Erie Canal being built at Lockport, New York, "I found my first fossils and began my collection" (Quaife, 1917, p. 3). Also, geology was his last professional employment. Lapham served as Wisconsin state geologist for 22 months, leaving that position only months before his death. First and last, therefore, he was a geologist.

Increase Allen Lapham was born March 7, 1811, in Palmyra, New York, the fifth of thirteen children of a Quaker couple, Seneca and Rachel Lapham (Lapham, 1875?). Seneca was an engineering contractor who worked on canals, as did some of his sons—first the Schuylkill Canal in Pennsylvania, then the Erie Canal at several places along its route, then the Welland and Miami, Ohio, Canals.

From childhood, Increase Lapham had an insatiable curiosity about the natural world, and he regretted throughout his life that he lacked money to study natural philosophy at Yale College (Thomas and Conner, 1973, p. 18 and 113). Even so, in 1828 and

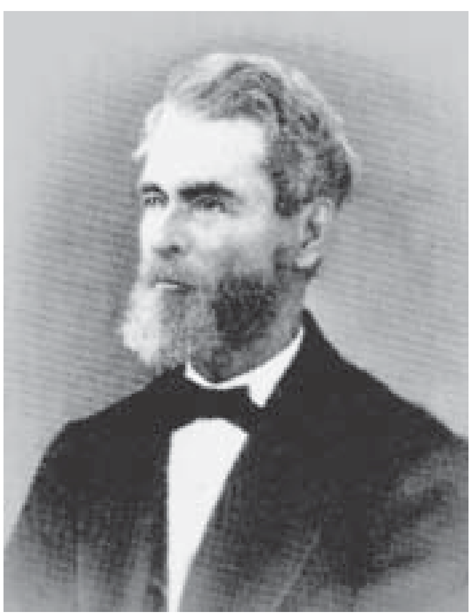

Figure 1. *Increase A. Lapham.*

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not yet 17 years old, he published his first scientific paper in Benjamin Silliman's *American Journal of Science*. He was living in Shippingsport, Kentucky, near Louisville, and working as a surveyor's rodman on a canal project when he wrote the paper entitled, "Louisville and Shippingsport Canal and Geology of the Vicinity" (Lapham, 1828, p. 65–69).

Later, he and his brother Darius cooperated on a paper published in 1832 in the same journal, called "Observations on the Primitive and Other Boulders of Ohio" (Lapham and Lapham, 1832, p. 300–303). The Lapham brothers had noticed smoothed boulders of greenstone embedded in clay in the banks of canals near Louisville. They surmised correctly that the boulders had been transported prehistorically from the north, probably from Canada, but they did not know how. They ascribed the cause to a great flood. (Within a decade a theory of continental glaciation would be advanced that would explain how greenstone boulders came to Ohio. Lapham would struggle with this theory for years, although he accepted it in full by the time of his death.) Lapham spent the years 1830 through mid-1836 in Ohio, first in Portsmouth, then in Columbus, where he was for three of those years Secretary of the Ohio Board of Canal Commissioners (Hawks, 1960, p. 17–36). While in Ohio, Lapham was to meet Byron Kilbourn, a young engineer working on the Ohio canals, who soon was to move to Wisconsin territory, where he founded Kilbourntown, one of the three villages that later consolidated as the city of Milwaukee. In 1836, at Kilbourn's invitation, Lapham moved to Milwaukee to work as surveyor and, shortly thereafter, chief engineer of Kilbourn's Milwaukee and Rock River Canal Co. (Hawks, 1960, p. 34).

Lapham arrived at the village that was to become Milwaukee on July 1, 1836, after a 10-day trip from Detroit on the steamboat *New York,* only three days before a Congressional act separated the Territory of Wisconsin from the Territory of Michigan, a move preparatory to conferring statehood upon Michigan. Also, his arrival came only four years after the Black Hawk War ended, an event that effectively opened Wisconsin to Yankee and European settlement. Lapham arrived at a town of 50 houses on mud streets on which Indians still were exchanging furs for trade goods. New houses were encroaching upon Menomonee and Potawotomi wigwams. There were 2,802 persons in Milwaukee County, which then extended all the way to Dodge County and included the present Waukesha County. The village on Lake Michigan was developing as a Lake Michigan port from which were shipped lead and copper from the southwestern Wisconsin mines and furs from the north.

Immediately, Lapham began noticing the natural history of the place, taking long walks on the Lake Michigan shore. In 1838, he published some results of his early observations, "A Catalogue of Plants Found in the Vicinity of Milwaukee," printed at the Advertiser, the first newspaper in Milwaukee. The late Milo M. Quaife of the State Historical Society of Wisconsin considered this paper to be the first scholarly publication in Wisconsin and perhaps the first west of the Great Lakes, at least north of St. Louis (Quaife, 1917, p. 7).

While Lapham's paper was about biology, Lapham certainly would have noticed the bedrock of Milwaukee and its fossils. He recognized that the bedrock here bore the same fossils that he had begun collecting as a boy of 14 in Lockport, and so he realized that the rock here was "Niagara limestone," as it was then called, an important insight. Most importantly, it meant that it would be a waste of time to search the area for fossil fuels, as the Carboniferous Period came much later than the Niagara formation. As he was to write: "It appears then from these facts that we may not hope to add coal to the other sources of mineral wealth with which a kind Providence has so abundantly supplied us" (Lapham, 1846, p. 62).

Lapham was an indefatigable collector of fossils, minerals, plants, and archaeological artifacts. In 1846, he sent a collection of Milwaukee area fossils to paleontologist James Hall of New York. With Hall's help, Lapham determined the correct stratigraphic position for eastern Wisconsin's bedrock, correlating it with New York's. In the 1850s, he unsuccessfully tried to enlist Hall as a collaborator in publishing a full study of American paleontology. After Lapham's death in 1875, the bulk of his fossil and mineral collection was sold to the University of Wisconsin, only to be destroyed when the university's Science Hall burned to the ground in 1884 (Mikulic, 1983. p. 8–9).

The canal for which development Lapham was invited to Milwaukee was abandoned a couple of years after his arrival when everyone realized railroads would do the job faster and cheaper. Only a single mile of canal was dug, a southwest-trending spur from the Milwaukee River just south of North Avenue. Its water powered a mill and provided water for tanneries, so the project was not entirely a commercial loss. However, with the canal project abandoned, Lapham had to find other ways to make his living. He plunged into a scientific study of his place, and in his subsequent travels throughout Wisconsin, he made copious notes about the state's geology. In 1840, he shared his sense of joy of discovery in a lecture to the Milwaukee Lyceum (of which he was a founder) about the moral virtue and esthetic charms of studying nature:

> *There is not life so long as to be in any danger of exhausting them. There is no condition of life debarred from these pleasures; all may study nature—the poor as well as the rich—old—young—male and female—the ignorant, the learned—all may enjoy the pure and simple pleasures they afford (Lapham, 1840).*

He wrote a book about Wisconsin, the first book published in Wisconsin, in 1844, entitled *A Geographic and Topographical Description of Wisconsin, with Brief Sketches of its History, Geology, Mineralogy, Natural History, Population, Soil, Productions, Government, Antiquities, &c., &c*. He intended it to be sold in the East to attract new Milwaukee settlers. It reveals the breadth of Lapham's interests and the extent to which he had satisfied them in less than a decade. In this book and in a second edition published in 1846, Lapham divided Wisconsin into four zones based on geological considerations: the north, underlain with "primitive" rocks such as granite, a sandstone-based western district, the lead and zinc mineral district of the southwest, and the "limestone district" of eastern Wisconsin.

He had already prowled the Kettle Moraine ridge that extends from near Manitowoc to near Janesville, passing west of Milwaukee, and he puzzled over this tortured landscape of kettles, mounds, and serpentine hills. He attributed the drift of which the ridges were composed to diluvial origin and the kettles to the "easily decomposed" limestone bedrock (Lapham, 1846, p. 13–14). He also speculated that Lake Michigan once must have had a southern outlet down the Illinois River to the Mississippi and that there must have been a barrier across the straits of Mackinac that would have prevented its drainage to the northeast (Lapham, 1846, pp. 133–134).

In these observations, as in the case of the Ohio greenstone erratics, he was struggling for answers. Indeed, there had been a barrier—one of ice—across northern Lake Michigan and the lake in fact had drained southward. Also, the Kettle Moraine landscape was formed by watery violence as two lobes of the great Wisconsin glacier thawed. But the theory of continental glaciation was just then being constructed, arising from the observations in Europe of young Louis Agassiz, who was to come to the United States in 1846, two years after Lapham's first edition was published. Lapham and Agassiz were to meet and to correspond, but years would pass before Lapham fully accepted the idea of a continental ice sheet. For instance, on February 11, 1848, in a lecture on the geology of Wisconsin to the students of Milwaukee High School at the Milwaukee Unitarian Church, Lapham mentioned Agassiz' theory. However, he added, "It is difficult to conceive of glaciers having sufficient extent to scatter fragments of rocks over the whole of the country from the Atlantic to the Pacific Oceans and move large blocks of granite from Lake Superior to the southern boundary of Wisconsin" (Lapham, 1848).

In 1849, Charles Whittlesey, head of a team of geologists working under the direction of David Dale Owen, chief U.S. geologist, surveyed the area of Wisconsin bordering on Lake Superior as well as areas of eastern Wisconsin. For the Iron Ridge area of Dodge County as well as the bedrock from Milwaukee to Madison, Whittlesey relied heavily on Lapham's findings, which were extensively quoted in Owen's (1852) *Geological Survey of Wisconsin, Iowa and Minnesota*. Whittlesey wrote that Lapham's observations were at the time, "so far as I know, the only authority on the rocks south of Lake Winnebago and east of the Rock River" (Owen, 1852, p. 448–451).

In 1855, Lapham finished a geologic map of Wisconsin, the first of two that he would complete during his life, superimposing his geological observations upon a geographical map published by the J.H. Colton Co., a publisher of atlases in New York. Intended for use in schools and to encourage investment in Wisconsin iron, lead, copper, and zinc mining industries, copies of the maps were distributed widely (Edmonds, 1985, p. 174–177).

In 1858, Lapham toured and later reported on the Penokee Iron Range of northern Wisconsin and the copper mining district of Upper Michigan. During this trip, he surely witnessed the extent of the great cutover of the northern forests, which may have given rise to insights that led to one of his most important contributions. In 1867, he led a three-man commission that issued a "Report on the disastrous effects of the destruction of forest trees now going on so rapidly in the state of Wisconsin." A full century before the Arab Oil Embargo of 1973, Lapham wrote:

> *On this question of fuel, we are to calculate by ages of the Earth, and not by the life of man. Fuel will be required so long as man shall inherit the Earth, for his comfort and for his existence. Without fuel, humanity would cease to exist. Viewed in this light, the deposits laid up during uncounted periods of time…in the shape of coal, petroleum and peat, and which man is now drawing out and using for fuel or wasting, must be exhausted (Lapham and others, 1867, p. 31).*

Here was twentieth-century thinking from a nineteenth-century mind. Two geological insights stand out, that Earth's age is vast and that the planet's store of fossil fuels is finite, thus exhaustible. Nonetheless, here they are in a pamphlet written and printed in Wisconsin not 20 years after Wisconsin became a state and during the most profligate human-caused environmental disaster wreaked upon Wisconsin in the nineteenth century, the cutting of the ancient north woods within half a century.

While still in Ohio, Lapham had played a role as a young man in lobbying the Ohio legislature to establish an Ohio Geological Survey, a measure that succeeded shortly before Lapham left for Wisconsin (Hawks, p. 32–33). As early as 1850, Lapham was promoting a geological survey for Wisconsin. The legislature responded in 1853 by appropriating $2,500 for the project and naming Edward A. Daniels as first state geologist, to be succeeded in 1854 by James Percival, who died only months later. Lapham, although busy with other projects, took time out in the summer of 1856 to complete Percival's report, commenting in doing so that much of Percival's work was based on his own.

The legislature made another try at a joint geological and agricultural survey in 1857, appropriating $6,000 a year for six years and naming Daniels, paleontologist James Hall, and Ezra S. Carr, professor of chemistry and natural history at the University of Wisconsin, as its three commissioners. During the next five years, which were marked by feuding among the principals, Lapham sought vainly to be employed by the survey. The Civil War intervened and Daniels joined the army. In February 1962, the legislature suspended the survey (Hawks, p. 226–235).

In 1873, partly as a result of the efforts of the Wisconsin Academy of Sciences, Arts and Letters, of which Lapham had been a founder three years earlier, the Wisconsin Legislature approved a measure calling for a full geological survey of Wisconsin. On April 10 of that year, Governor Cadwallader C. Washburn named Lapham state geologist (Washburn, 1873). At last, Lapham was in position to conduct a study he long had dreamed of and he could do it on a regular salary and with sufficient staff and support.

He hired two young geologists, Thomas Chrowder Chamberlin and R.D. Irving, and Moses M. Strong, Jr., a mining engineer, as his three chief assistants and set to work. Irving undertook to describe the iron ore deposits, the other strata, and the glacial remains in Douglas and Ashland Counties. Chamberlin was to study an area bounded on the east by the Niagara dolomite, on the south by Illinois, and by the crystalline rocks on the north. Strong was to survey the lead and zinc mining region of southwestern Wisconsin. Each spent the summer in the field, returning in the fall to compile his data and write reports.

By now, Lapham accepted the concept of continental glaciation. His 1873 "Report of Progress and Results," published posthumously in the four-volume *Geology of Wisconsin,* contained a section called "Relation of the Wisconsin Geological Survey to Agriculture." In it, Lapham referred to "the drift phenomena, gleaned from an extended and careful study of the loose materials covering and concealing the more solid rocks, left here by the glaciers of the ice period" (Chamberlin and others, 1877, p. 41). Agassiz's theory at last had persuaded Lapham, probably through the influence of Chamberlin, who fleshed out the theory and provided a sophisticated interpretation of glacial action as the chief sculptor of the Wisconsin landscape.

Lapham remained in charge of the Survey through a second season and reported again on progress in January 1875. A month later, disaster struck. Governor Washburn, a Republican, had been defeated for reelection by William R. Taylor, a Democrat, who paid off a political debt by appointing O.W. Wight, a man without qualifications in geology, as state geologist. Lapham legally could be removed, it turned out, because the state senate had neglected to confirm his appointment in 1873.

The scientific community was outraged, seemingly more so even than Lapham himself. He returned to his children's farm on the south shore of Oconomowoc Lake and resumed his quiet studies of natural history. "My time is divided between Milwaukee and Oconomowoc. I find it very pleasant to be 'on the farm,' among the lakes and drives…," he wrote to his brother (Lapham, 1875). On September 14, 1875, he finished a paper called "Oconomowoc and Other Small Lakes of Wisconsin, Considered with Reference to Their Capacity for Fish-Production," later published in the annual *Transactions of the Wisconsin Academy of Sciences, Arts and Letters* (Lapham, p. 30–36).

That afternoon, he walked down to the shore of Oconomowoc Lake and set off in a rowboat to do some fishing. At 6:30 p.m., Lapham, dead of a stroke, was found lying in the bottom of the boat just offshore. Coming so soon after his dismissal, his death rekindled public dismay over the way politicians had treated Wisconsin's first genuine scientist.

But his old friend and lifelong correspondent, the eminent botanist Asa Gray of Harvard College, cast Lapham's life appropriately. Calling Lapham "a modest retiring, industrious, excellent man," Gray concluded, "I have the idea that he had a happy, as well as a useful and honored life. What more could be asked?" (Sherman, 1876).

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The Reefs That Made Milwaukee Famous

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Figure 1. *Silurian exposures at the Schoonmaker Reef, Wauwatosa, Wisconsin (circa 1899). (Photograph by W.C. Alden, courtesy of U.S. Geological Survey.)*

ABSTRACT

The Silurian reefs of the Milwaukee region (fig. 1) were the first recognized fossil reefs in North America and among the first Paleozoic reefs described in the world. Serving as a textbook example of ancient reefs, they have inspired the interest of some of the most eminent North American geologists and paleontologists. However, their discovery and subsequent scientific prominence were primarily due to the efforts of local naturalists, who attracted the initial interest of scientists and supplied them with the extensive fossil collections needed to make their studies possible. Also critical was the economic importance of the reefs; without the quarrying of these structures, scientific study would have been impractical and their origin would have gone unrecognized. Study of these reefs began in the late 1830s when Increase A. Lapham discovered an abundance and diversity of fossils in a number of reef-controlled rock hills in the Milwaukee, Wisconsin, area.

Attempting to correlate these strata with James Hall's recently described New York "groups," Lapham sent a large fossil collection to Hall in 1846. Hall's subsequent research in the area led to his recognition of the reefal nature of these hills in 1862. In addition, Hall began the systematic description of the reef biota, which was recognized as one of the most diverse Silurian biotas in North America. In the latter half of the nineteenth century, collecting by gentlemen naturalists, such as F.H. Day (Wauwatosa), P.R. Hoy (Racine), T.A. Greene, and E.E. Teller (Milwaukee), was critical in providing collections for Hall and other prominent paleontologists. In a classic work on paleoecology and sedimentology published in 1877, T.C. Chamberlin expanded on Hall's initial observations contrasting the sedimentological character and biotic content of the Milwaukee reefs with surrounding non-reef strata. Research by A.W. Grabau, W.C. Alden, R.R. Shrock, and others highlighted the Milwaukee reefs throughout much of the early part of the twentieth century. As quarrying in this area declined, however, research focus on Silurian reefs shifted to the Chicago area with the work of J H. Bretz and H.A. Lowenstam.

For additional information about the Silurian reefs, see Mikulic and Klussendorf (1998).

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CHAMBERLIN, SALISBURY, AND COLLIE: A TALE OF THREE BELOIT COLLEGE GEOLOGISTS

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ABSTRACT

After graduating from Beloit College in 1866, Thomas Chrowder Chamberlin served as principal of Delavan High School, took graduate work at Michigan, taught at Whitewater State Normal School, and then returned to Beloit as a professor. Here, he had Rollin Daniel Salisbury and George Lucius Collie as students. It has been written that Salisbury was one of Chamberlin's students at Whitewater, but that is incorrect. Chamberlin had known Collie as a boy, however, because Chamberlin was a member of the Congregational church in Delavan where Collie's father was the pastor.

Salisbury and Collie were classmates and fraternity brothers at Beloit College, both graduating in the class of 1881. Both were excellent students and no doubt there was strong competition between them for grades and academic recognition. Salisbury, however, was the better of the two, won several academic awards, and became Chamberlin's favorite pupil. Shortly before they graduated, Chamberlin accepted a position with the U.S. Geological Survey (USGS). He appointed Salisbury as an assistant, and "Sals" moved into the Chamberlin home.

In 1882 Chamberlin submitted his resignation at Beloit College to devote full time to his USGS activities and moved to Washington, D.C. Two years later Salisbury became the chair of geology at Beloit. In 1887 Chamberlin assumed the presidency of the University of Wisconsin. Four years later he invited Salisbury to become a member of the geology department at Madison, and Salisbury again joined the Chamberlin household. His place at Beloit was filled by his former classmate, George Collie. Collie held the position of professor of geology for more than 30 years, but stepped aside in 1923 to assume a new chair in anthropology. Chamberlin, meanwhile, had resigned as president of the University of Wisconsin to found the geology department and the Journal of Geology at the new University of Chicago in 1892. Salisbury went along to Chicago, and the two remained close associates until Salisbury's death in 1922. Collie's retirement in 1931 ended 58 years of professorial service to Beloit College by the Chamberlin–Salisbury–Collie trio. All three served as president of the Beloit College Alumni Association, all three served on the Beloit College Board of Trustees, and all three were awarded honorary LL.D. degrees by their alma mater.

INTRODUCTION

Most Wisconsin geologists and historians are familiar with the name and professional career of Thomas Chrowder Chamberlin (fig. 1), unquestionably one of America's all-time great scientists. Many are also familiar with the name Rollin Daniel Salisbury (fig. 2), largely through his professional association with Chamberlin and the classic Chamberlin–Salisbury textbooks of the early 1900s (Chamberlin and Salisbury, 1904, 1906, 1909). However, few persons today are aware of the long and close personal friendship of these two men, and fewer still are familiar with a lesser-known Wisconsin geologist by the name of George Lucius Collie (fig. 3). For more than forty years Chamberlin, Salisbury, and Collie had a close professional and personal association.

That association began at Beloit College in 1878 and ended with Salisbury's death in 1922. Because much of that association was centered around the college, one might well refer to these three scientists as

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Figure 1. *Thomas Chrowder Chamberlin, 1843–1928. Photograph taken in 1892. (Photograph from Beloit College Archives.)*

Figure 2. *Rollin Daniel Salisbury, 1857–1922. Photograph taken about 1910. (Photograph from Beloit College archives.)*

Figure 3. *George Lucius Collie, 1858–1954. Photograph taken about 1895. (Photograph from Beloit College archives.)*

the Beloit College geology trio. A second trio—an institutional triad of Beloit College, the University of Wisconsin, and the University of Chicago—played a significant role in the lives of two of these people. As described below, these men and schools have also played an important role in the lives of several other geologists.

This paper summarizes the lives, careers, and professional contributions of Chamberlin, Salisbury, and Collie (especially Chamberlin) and focuses attention on the interwoven personal associations of these three men. It consolidates the content of my recent talks about these outstanding Beloit geologists of the past (Schneider, 1989, 1994, 1996a, 1996b, 1997, 1998).

SCIENTIFIC CONTRIBUTIONS OF T.C. CHAMBERLIN

Chamberlin's contributions to science were indeed enormous and highly significant. His biographers have described him as a master of research, a giant of geology, and the leading American geologist of his generation. One of his biographers—the great geologist Bailey Willis—listed Chamberlin as one of the world's greatest thinkers, placing him alongside Aristotle, Copernicus, Galileo, Newton, LaPlace, and Darwin. "Few among living investigators," Willis (1929, p. 23) wrote, "have demonstrated equal capacity for inquiry." Chamberlin's bibliography consists of about 250 titles. This number is somewhat misleading, however. Ten papers dealing with his studies of glacial motion in Greenland, for example, are listed as a single entry. His first paper was published in 1872 and his last in 1928, less than a month before his death.

So diverse and so significant were Chamberlin's contributions that it is difficult, indeed impossible, to state which was the most important. Some have argued that the "planetesimal hypothesis," which Chamberlin formulated with the astronomer F.R. Moulton, published in 1904, was the most significant. It essentially replaced the LaPlace "nebular theory," which was then the generally accepted theory of the Earth's origin.

Others would say that Chamberlin's paper in the *Journal of Geology* on multiple working hypotheses was his most important contribution. It has been described (Mackin, 1963) as one of the three outstanding papers on geologic method. Still others would argue for the three-volume textbook *Geology,* which Chamberlin co-authored with Salisbury. In this comprehensive work, the authors presented many original ideas. It was described as "the most thorough geology text ever written in English." It restated the planetesimal hypothesis, proposed new causes for vulcanism, subsidence and uplift, defined new geologic periods, and used cyclic changes in the earth as a basis for major time divisions.

Many others would surely argue that Chamberlin's most important contributions were in glacial geology. His major contributions in this field included evidence for multiple glaciations, the classification and nomenclature of glacial deposits, the origin of loess (a windblown silt deposit), studies of glacial motion, and global climatic changes and causes of continental glaciation.

Much has been written about Chamberlin (often misspelled Chamberlain; for example, Chamberlain Avenue in Madison, Wisconsin), perhaps more than about any other American geologist, with the possible exceptions of G.K. Gilbert and John Wesley Powell. The literature contains many accounts (far too numerous to cite here) of Chamberlin's life, his professional career, his scientific contributions, and even his philosophy and religion. Some of these accounts are more than 100 pages long, including Rollin Chamberlin's biographical memoir of his father, published by the National Academy of Sciences (R.T. Chamberlin, 1934), and a more recent Ph.D. dissertation by Susan Schultz (1976). Many today regard Schultz's biography as the definitive study of Chamberlin's life.

One of the most comprehensive treatments is a two-part account of Chamberlin's life written by George L. Collie (1932), his former student at Beloit College. Collie's accounts (1928, 1932) have been my principal sources of information about Chamberlin's life and are here acknowledged. I also acknowledge as primary sources Professor Hiram Densmore's (1931) biographical account of Salisbury's life and R.T. Chamberlin's (1931) memorial of Salisbury.

INTEREST OF THE AUTHOR IN THE BELOIT COLLEGE GEOLOGY TRIO

My interest in the Chamberlin–Salisbury–Collie association began in 1945, when I was an undergraduate student at Beloit. My major professor and academic advisor was Monta E. Wing (Schneider, 1978), who came to Beloit in 1923 with a fresh Ph.D. from the University of Chicago. The opportunity to join the Beloit faculty came when Professor George L. Collie left the geology department to organize and chair the anthropology department. (Wing is probably best remembered as one of the founders of AGT, now known as NAGT—the National Association of Geoscience Teachers).

Monty Wing came to the University of Chicago as a graduate student from Kansas, where he had been a field assistant to the late Raymond C. Moore. Chamberlin had retired from his position as professor and chair of the geology department just three years earlier; this was about a year before Salisbury's death. As a graduate student at Chicago, Wing had contact with Professors Chamberlin and Salisbury as well as with Professor Rollin Chamberlin. It was largely through the good offices of the Chamberlins, to be sure, that Wing obtained his teaching position at Beloit. And it was mainly through the stories of Professor Wing that I first became interested in the Chamberlin legend.

Wing often spoke of Chamberlin's influence upon his own career and of the Chamberlin–Salisbury and the Beloit–Chicago associations. It was, in fact, Monta Wing, more than anyone, who convinced Rollin Chamberlin to donate the Chamberlin homestead along Raccoon Creek west of Beloit to Beloit College before he died. I remember Monty's several trips to Chicago to visit the Chamberlins to accomplish this goal. I remember well one Arbor Day when many of us went to Chamberlin Springs with Professor Wing to clean up the grounds and unplug the main spring so that it could flow freely once again. At that time (1947) the president of Beloit College was Carey Croneis. Professor Croneis had left the geology department at Chicago, where one of his colleagues had been Rollin Chamberlin, to become president of Beloit in 1944. It was mainly through the influence of President Croneis that I enrolled at The Pennsylvania State University for my master's work, the relevance of which is explained below.

My interest in the Chamberlin–Salisbury association was reinforced by making the acquaintance of Dr. Collie and hearing him give an informative talk to the student body on the early history of the College (Collie, 1948). At that time (1947), Dr. Collie was 90 years old. My interest was further reinforced when, in 1949, I discovered, much to my surprise, Collie's paper on the Ordovician of central Pennsylvania (Collie, 1903). His Bellefonte field area was a mere 10 miles away and directly along strike from the section of Trenton Limestone that I was working on for my master's thesis (Schneider, 1951). It was this coincidence that prompted me to meet with Dr. Collie regarding his association with Chamberlin and Salisbury. Much of what he had recorded in his extensive biography of Chamberlin (Collie, 1932), he related to me in person.

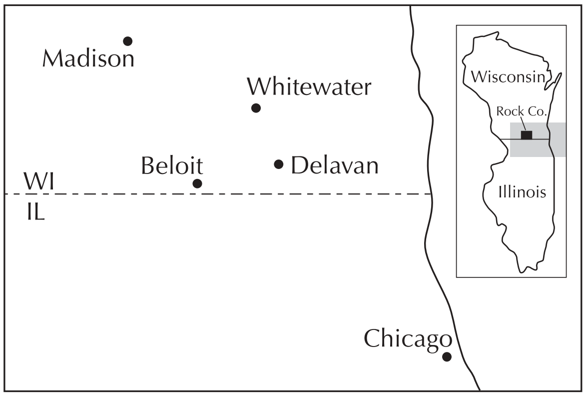

Figure 4. *Map of southeastern Wisconsin and northeastern Illinois showing significant locations mentioned in the text.*

CHAMBERLIN'S EARLY LIFE

Thomas Chrowder Chamberlin ("Tom" or "T.C."; fig.1) was born in Coles County, Illinois, near the present-day city of Mattoon, on September 25, 1843. Perhaps it is significant that his birthplace was at the crest of the Shelbyville Moraine (the outermost ridge of the last glacial age, which Chamberlin later named the Wisconsin), because for the first 30 years of his career Chamberlin's chosen field was glacial geology.

Thomas' father, John Chamberlin, was a Methodist minister and a farmer—a minister by preference and a farmer by necessity, according to his grandson Rollin (R.T. Chamberlin, 1934). In 1846 John Chamberlin moved his family to southern Wisconsin, where he purchased property from the government in Rock County near the city of Beloit and built a log house. For 40 of the next 46 years, T.C.'s life would be concentrated in southern Wisconsin (fig. 4).

As a youngster, Chamberlin was fascinated by nature. His interest was no doubt fostered by helping his four brothers quarry limestone building blocks for a new house and shoveling sand for mortar. He was particularly interested and puzzled over the "snakes and snails" (Ordovician fossils) he observed in these rocks.

Young Tom attended the district school, then the Beloit Academy, and in the fall of 1862 he enrolled at Beloit College. The backbone of the curriculum at the academy and the college was severely classical. The college curriculum was modeled after that of Yale. (For many years Beloit was known as the "Yale of the West"—an appellation seldom, if ever, heard today.) It was entirely prescribed, consisting mostly of Latin, Greek, rhetoric, and mathematics. It did, however, include some history, philosophy, and science. Although a good student, Chamberlin was not at all interested in the classics, but math and science relieved to some degree his boredom of Latin and Greek (Collie, 1932). However, it was Tom's good fortune to enroll in a course taught by Henry B. Nason, professor of chemistry and natural sciences. Nason had come to Beloit in 1858 with a fresh Ph.D. degree from the University of Goettigen in Germany and was the first Ph.D. on the Beloit faculty. Later, Nason was to be one of the founding fathers of The Geological Society of America. Nason's enthusiasm for geology and the natural sciences greatly influenced Chamberlin (Croneis, 1968).

CHAMBERLIN'S EARLY CAREER AS A PROFESSOR AND SURVEY GEOLOGIST

After graduating from Beloit in 1866, Chamberlin obtained a master's degree from Beloit, then served as principal of Delavan High School for two years. In 1869 he enrolled as a graduate student at the University of Michigan, where he studied under the renowned Alexander Winchell. After a year at Ann Arbor, Chamberlin returned to Wisconsin as professor of natural sciences in the state normal school at Whitewater (now the University of Wisconsin–Whitewater).

According to Collie (1932), Chamberlin's attention was first directed to glacial studies during his residence in Delavan. Here he pioneered the concept of field trips, frequently leading his high school students on hikes through the nearby fields and woods to make observations on rocks, plants, and animals at a time when learning was largely restricted to the classroom. No doubt his interest in glacial geology was reinforced during his three years at Whitewater, located at the southern end of the Green Bay glacial lobe and in the very shadow of the Kettle Moraine.

In the fall of 1873, Chamberlin returned to Beloit College as professor of natural history. In 1880 the Natural History Department was divided and Chamberlin was made professor of geology. Also in 1873, he was appointed assistant geologist with the Complete Geological, Mineralogical and Agricultural Survey of Wisconsin, under the direction of Increase Lapham (see paper by Hayes, this volume). After Lapham's death in 1875 and O.W. Wight's one year as chief, Chamberlin was appointed chief geologist (state geologist) in 1876, a position he held until 1882, although the survey work was largely completed by 1879.

During Chamberlin's days with the Survey, the organization was expanded from a staff of four to more than a dozen. The work of the survey culminated with the publication of the *Geology of Wisconsin,* a grand four-volume set that treated every aspect of Wisconsin geology and is still of considerable value today. Chamberlin himself contributed much, including now-classic treatments relating to the origin of lead and zinc ores of southwestern Wisconsin, description of the Paleozoic stratigraphy of eastern Wisconsin and of biotherms or reef structures in the Silurian rocks of southeast Wisconsin, and of course the topography, hydrology, soils, and glacial deposits of eastern Wisconsin, including the Kettle Moraine. His skill in directing the completion of the survey brought him national recognition and undoubtedly contributed to his being invited to accept the presidency of the University of Wisconsin.

SALISBURY'S EARLY LIFE

Rollin D. Salisbury ("Saul" or "Sals"; fig. 2) was raised on the family farm near the tiny rural community of Spring Prairie in southeast Wisconsin, which remains today much as it was during Salisbury's boyhood. At age 16 he entered Whitewater State Normal School, completed the four-year course in less than two and a half years, and graduated in 1877 as salutatorian of his class. His father, Daniel Salisbury, had been a teacher in upstate New York and two of Rollin's sisters had attended Whitewater, so it is not surprising that he should choose to enter the teaching profession at an early age (Densmore, 1931).

Following his graduation from the normal school, Saul taught for a year at the village school in Port Washington, Wisconsin. Virtually nothing is recorded regarding his experience there, except that it was then that he decided to attend college. He entered Beloit College as a sophomore in September, 1878.

THE CHAMBERLIN–SALISBURY–COLLIE ASSOCIATION AT BELOIT

Salisbury and Collie were undergraduate classmates and fraternity brothers at Beloit College. The two took classes together from Professor Chamberlin and graduated together in the class of 1881 (fig. 5). In their excellent book *Giants of Geology,* Fenton and Fenton (1952) wrote that Salisbury was one of Chamberlin's students at Whitewater, a statement that is clearly incorrect. Professor Chamberlin had already moved from Whitewater to Beloit before young Salisbury entered the normal school. The Chamberlin–Salisbury association did not begin until Salisbury was a student at Beloit (R.T. Chamberlin, 1931, p. 126) and apparently not until 1880 or 1881—the exact date is not clear.

The first meeting between Chamberlin and Salisbury was suggested by one Roger Leavitt, a mutual friend of Collie and Salisbury in the class of 1882. Salisbury was a melancholy, pessimistic, and temperamental individual with a complex personality. Although he was apparently tolerant of other's religious beliefs, Salisbury himself was not a believer in any re-

Figure 5. *Beloit College senior class of 1881. Salisbury is top row on the right; Collie is bottom row on the left. (Photograph from Beloit College archives.)*

ligious doctrine. In fact, he described himself as an infidel. Concerned about his friend's indifference, Leavitt suggested that Salisbury have a talk with Professor Chamberlin.

In contrast to Salisbury, Chamberlin was a cheerful, optimistic, and confident man. He was also a deeply religious person, although he did not accept many of the orthodox beliefs and creeds. He taught a Sunday school class at the Second Congregational Church and occasionally gave public lectures on various subjects related to the Bible, including a series of seven lectures on the beginning of the Earth as recorded in the first chapter of Genesis and another series on the philosophy underlying the Book of Job. After some time, Salisbury agreed to see Chamberlin. The meeting significantly altered Salisbury's attitude toward life and religion and marked the beginning of the lifelong association of the two men (Collie, 1932).

Chamberlin had known Collie (fig. 3) as a boy, however, because the Chamberlins were members of the Congregational church in Delavan where Collie's father, the Rev. Joseph Collie—also a Beloit graduate—was the pastor. When Chamberlin arrived in Delavan, George Collie was a young lad in the fourth grade, and he later recorded his boyhood memories of the "stalwart principal and his petite, charming wife, as they first appeared in the little pioneer village" (Collie, 1932, p. 412).

Figure 6. *Home of Professor T.C. Chamberlin adjacent to the Rock River in Beloit, Wisconsin, where Salisbury and Collie met for recitations as seniors. Photograph taken in the early 1880s. (Photograph from Beloit College Department of Geology.)*

At Beloit, Salisbury and Collie were good students, and no doubt there was strong competition between them for grades and academic recognition. Salisbury, however, was clearly the better of the two and won several academic awards. He won the Roger scholarship given to the student making the highest record in his class for the junior year and the Stanley scholarship for a similar attainment as a senior. He was also selected valedictorian of the senior class and was asked by the college faculty to deliver the valedictory address at the commencement ceremony (Densmore, 1931). It is not surprising, therefore, that he was Chamberlin's favorite pupil.

In addition to the usual geology course work, Salisbury and Collie took advanced studies together under Chamberlin in their senior year. Reading and field work supplemented the textbook assignments in these courses, and recitations and discussions of assigned readings were held in the study of Chamberlin's home (fig. 6). Chamberlin's favoritism of Salisbury is well illustrated by an event, recorded by both Collie and Densmore, which occurred early in 1881. After one of their weekly recitations at the Chamberlin home, Collie and Salisbury left the house together. "No sooner had we reached the street," wrote Collie (1932, p. 439), "when Salisbury began capering about, showing an exuberance of joy, unusual with him, which included slapping the writer on the back sharply. When asked the meaning of this outburst, he said, 'My future is assured. Professor Chamberlin has been offered the position of geologist on the Federal Survey and he has offered to appoint me as an assistant' " (Collie, 1932, p. 439; Densmore, 1931, p. 43–44). Collie apparently received no such offer. This event served to cement the friendship that had been growing between professor and student and marked the beginning of their long professional association, which lasted more than 40 years until Salisbury's death in 1922.

In 1881 Chamberlin was named chief of the new Glacial Division of the U.S. Geological Survey (USGS) by John Wesley Powell, who had been appointed director of the Survey the previous year. Following his graduation in 1881 and while working as Chamberlin's assistant on the USGS, Salisbury lived in the Chamberlin household. He was made to feel completely at home by Professor and Mrs. Chamberlin. Indeed, they treated him as a son, and when their own son (Rollin Thomas Chamberlin) arrived that same year, he was named after Salisbury.

In June of 1882 Chamberlin submitted his resignation at Beloit College to devote his full-time efforts to Survey research and moved to Washington, D.C. His glacial studies in Wisconsin soon merged into glacial studies of the entire northern United States, for he worked from the Atlantic coast to Montana. In these endeavors, he was ably assisted in different areas of the country by many associates, including Rollin Salisbury in Wisconsin and New Jersey. During his tenure with the Survey, Chamberlin authored many significant publications.

Figure 7. *Professor R.D. Salisbury with his geology class at Scott's quarry (Ordovician rocks), Beloit, Wisconsin. Salisbury is center right with beard, white hat, and white shoulder strap. Photograph taken in 1889. (Photograph from Beloit College Department of Geology.)*

SALISBURY'S CAREER AT BELOIT

When Chamberlin resigned his position at Beloit, Salisbury was appointed to assume T.C.'s teaching responsibilities, undoubtedly due to Chamberlin's influence and recommendation; the following year Salisbury became an assistant professor. Following the example set by his mentor, Saul frequently took his students into the field (fig. 7). In 1884 he was promoted to full professor and chair of the geology department at Beloit College.

Salisbury's reputation as an excellent teacher spread rapidly and was attested by several persons who had intimate knowledge of his ability as an instructor. In his long two-part biographical sketch of Salisbury's life, Densmore (1931) devoted several pages to Saul's teaching methods and his positive influence upon students. Densmore himself was a student of Salisbury in his senior year at Beloit (1885) and was thereafter a close friend. According to Densmore (1931, p. 122), another former Beloit student wrote that Salisbury "was a good geologist and did some most excellent original work in the field of glacial geology. His great forte, however, was that of a teacher, and among all of the men with whom I have worked he stood head and shoulders above the rest." Professor A.W. Burr, at one time principal of the Beloit Academy, stated that "Salisbury was a great teacher. There was business in his classroom from the moment he came through the door until he left the room . . . Professor Salisbury made not only good students, but marked teachers, the best product of a master in any calling" (Densmore, 1931, p. 123).

During his tenure with the USGS and as a member of the Beloit College faculty, Salisbury made several contributions to the geology of Wisconsin. These include studies of the Driftless Area (co-authored with Chamberlin), preglacial gravels of the Baraboo Hills, drift phenomena near Devils Lake and Baraboo, and the geography of Devils Lake and the Wisconsin Dells (both co-authored with W.W. Atwood).

CHAMBERLIN'S PRESIDENCY AT THE UNIVERSITY OF WISCONSIN

Early in 1885 Chamberlin was asked whether he would be a candidate for president of the University of Wisconsin. He resisted the pressure of the Regents to consider the position but finally agreed, took a year to complete his work with the USGS, and assumed the presidency at Madison in 1887 (fig. 8). He met some opposition to his appointment, for he was a scientist rather than a theologian. Some faculty members felt that university presidents should be clergymen in order to understand students' spiritual needs and deliver baccalaureate sermons (Fenton and Fenton, 1952, p. 306). Nevertheless, his presidency was highly successful. His ability as an organizer and administrator won him full support from the Regents, and during his term he changed the nature of the university and started it on its course to becoming the great university that it is today. He greatly strengthened the administration of the university, recruited outstanding faculty, doubled the size of the faculty, broadened the curriculum, established alternative systems of study, stressed the importance of science, and placed new emphasis on graduate programs and faculty research. He also established the first laboratory of psychology in the Midwest and launched an extension program to serve the entire state. (Bailey, 1981; Curti and Carstensen, 1949)

Chamberlin's active field studies largely ceased when he became president of the University of Wisconsin. However, he remained in charge of the Glacial Division of the USGS for 17 more years, until 1904.

About three years after Chamberlin became president of the university, he invited Salisbury to join the geology department at Madison. When the rumor spread that Salisbury might be leaving Beloit, a petition urging him to stay was circulated and signed by nearly every student at the college. Saul was so touched by this expression of student support that he declined the offer. However, when the offer was renewed the following year, he resigned his position at Beloit to join his former mentor at Madison and once again Rollin Salisbury—a lifelong bachelor—became a member of the Chamberlin household.

Chamberlin's service to Wisconsin as state geologist and as president of the University of Wisconsin are commemorated with a large Precambrian gneiss erratic and plaque atop a drumlin next to the observatory on the Madison campus (fig. 9). A classroom building on the campus also bears his name.

__Figure 9.__ Chamberlin Rock on the University of Wisconsin–Madison campus, commemorating Chamberlin's service to Wisconsin as state geologist and president of the university. (Photographs by the author.)

__Figure 8.__ The T.C. Chamberlin family in 1889, when Chamberlin was president of the University of Wisconsin and Rollin Chamberlin was eight years old. (Photograph from Beloit College archives.)

COLLIE'S CAREER AT BELOIT

Salisbury's place at Beloit was filled by his former classmate and lifelong friend, George Collie, who had earned a Ph.D. from Harvard University. Collie held the position of Professor of Geology for more than 30 years, but in 1923 he assumed a new chair in anthropology. In this position he gained national attention, organizing and participating in anthropological expeditions to many parts of the world. He was responsible for Beloit's first student field expedition in 1930, which was probably the first expedition for undergraduates in the country. For 25 years he served as dean of the college and twice served as Beloit's acting president. He was affectionately referred to by his friends and colleagues as Dean Collie.

Figure 10. *Original Chamberlin geology department faculty at the University of Chicago, 1892. From left to right: R.A.F. Penrose, Jr., J.P. Iddings, T.C. Chamberlin, C.R. Van Hise, and R.D. Salisbury. (Photograph used with permission from the Chair, Department of Geophysical Sciences, University of Chicago.)*

CHAMBERLIN AND SALISBURY AT THE UNIVERSITY OF CHICAGO

Meanwhile, Chamberlin had resigned as president of the University of Wisconsin in 1892, when the University of Chicago opened its doors. President William Harper invited Chamberlin to organize and head the geology department. Although he was reluctant to leave Wisconsin, Chamberlin was anxious to rid himself of administrative tasks and return to his great loves—teaching and research. Thus, he accepted President Harper's challenge, notwithstanding the monumental efforts of the University of Wisconsin faculty and students and the Madison newspapers to retain him. Salisbury went along as professor of geographic geology, and the two remained associates at Chicago until Salisbury's death. Upon the move to Chicago, Salisbury wished to remain a member of the Chamberlin household, but for reasons unknown to us this could not be arranged, and Salisbury had to rent his own quarters and look after himself.

As other writers have pointed out (particularly Willis, 1929), the new geology department at Chicago was fully staffed with men of national reputations from the very beginning, and within a year the department was recognized as one of the best in the country. In addition to Chamberlin and Salisbury, the original geology department at Chicago included R.A.F. Penrose, Jr., J.P. Iddings, and Charles R. Van Hise (fig. 10). Three of these men—Chamberlin, Penrose, and Van Hise—later served as presidents of The Geological Society of America. Van Hise, of course, was one of Wisconsin's early Precambrian geologists and served as chair of the Department of Geology and as president of the University of Wisconsin for several years (see paper by Dott, this volume).

In 1918, upon nearing his 75th birthday, Chamberlin resigned as professor and head of the geology department at Chicago and was appointed professor emeritus. His successor as head of the department was, of course, Rollin Salisbury.

If Salisbury did his best teaching during his tenure at Beloit, as was Densmore's opinion, his influence as a teacher certainly continued at Chicago. "If his students had been asked who was the greatest teacher they had had at the University, they would undoubtedly have said Professor Salisbury," someone stated (Densmore, 1931, p. 124). One of his colleagues at Chicago wrote that "all acclaim him as one of the greatest teachers of his time" (Densmore, 1931, p. 124).

Many of Salisbury's professional activities were either arranged by Chamberlin or were the result of collaboration. Saul's extensive field studies in glacial geology in the Upper Mississippi Valley, in New Jersey, and in Wyoming's Big Horn Mountains were made possible by his association with Chamberlin and the U.S. Geological Survey. It was Chamberlin who arranged for Salisbury to be a member of the 1895 Perry Relief Expedition to Greenland, where Chamberlin had been the previous year. In 1893, Chamberlin founded the *Journal of Geology,* with himself as editor-in-chief. Salisbury acted as managing editor as well as editor of geographic geology, and for 30 years the two collaborated in its publication.

As the years passed, however, the academic and scientific paths of these two great friends began to diverge. Although Chamberlin's research interests remained paramount, Salisbury's zeal for scientific discovery gradually diminished. Teaching and administrative work became an obsession. From 1897 to the time of his death in 1922, he served as dean of the Ogden Graduate School of Science. From 1903 to 1918, he organized and headed the Department of Geography and built what was probably the strongest and most progressive geography department in the nation (R.T. Chamberlin, 1931, p. 132). Nevertheless, the two men remained close friends.

TRIBUTES TO SALISBURY

In his memorial editorial in the *Journal of Geology* shortly after Salisbury's death in 1922, Professor Chamberlin stated that "Dr. Salisbury's greatest service to science lay in his singular success in stimulating and training young talent not only for the teaching of science but for research" (T.C. Chamberlin, 1922). With regard to Salisbury's various capacities as professor, head of the geography and geology departments, and Dean of the Ogden Graduate School of Science, Chamberlin (1922) wrote that "he (Salisbury) came into touch with thousands of young minds and gave them effective impulses toward sound scholarship and the higher life."

Professor Bailey Willis, in commenting on the Chamberlin–Salisbury association, wrote: "Salisbury ranked high as a teacher. It was for Chamberlin a great good fortune to have drawn to himself a spirit so loyal, a collaborator so competent, a fellow teacher so superior as Salisbury" (Willis, 1929, p. 27).

An example of Salisbury's legendary status is illustrated by an amusing incident that occurred about 25 years ago. One of my former colleagues at the University of Wisconsin–Parkside, in searching for colloquium speakers, phoned the geology department at Beloit and asked to speak to Rollin D. Salisbury. My colleague was familiar with Salisbury's name and reputation, but he obviously did not know that Saul had been dead for more than half a century.

The legend of Salisbury the teacher lives on and is perpetuated by the geology department at Beloit College, which is formally named the Rollin D. Salisbury Department of Geology. His portrait hangs in honor both at Beloit and at the University of Chicago. Appropriately, the building in which the geology department at Beloit is housed is named the Chamberlin Hall of Science.

TRIBUTES TO CHAMBERLIN

Thomas Chrowder Chamberlin died in 1928. He is buried in the family plot in Oakwood Cemetery in Beloit, just a few blocks from his beloved Beloit College. Chamberlin was one of The Geological Society of America's Original Fellows, he served as president of the Society in 1895, and in 1927 he was the recipient of the first Penrose Medal, the highest honor of the society. He also served as president of the Wisconsin Academy of Sciences, Arts and Letters (1885–87) and the American Association for the Advancement of Science. In 1941 a 13,000-foot mountain peak in the Sequoia National Forest in California was named in his honor.

In his years at Chicago, Chamberlin published approximately 145 papers. About sixty of these appeared in the *Journal of Geology*. Several of these papers dealt with his studies of glacial motion in Greenland with the Perry Auxiliary Expedition in 1894. One of his earliest papers on this subject was his Presidential Address of The Geological Society of America, which was published in the Bulletin of the Society.

When Carey Croneis, then chancellor of Rice University, was asked to speak at the dedication of Beloit College's Chamberlin Science Hall in 1968, it was suggested to him that his address should be titled "Thomas Chrowder Chamberlin, Beloit's Greatest Scientist." Said Croneis, "One cannot quarrel with such a title—except that it is too limiting. In many ways, T.C. Chamberlin was one of the most creative scientists the world has produced" (Croneis, 1968, p. 2).

EPILOGUE

When I last met with Dr. Collie in 1949, he was 92 years old but mentally alert. We talked at some length about Chamberlin and his (Collie's) undergraduate experience at Beloit. He expressed a good deal of bitterness about the Chamberlin–Salisbury relationship during his student days some 70 years earlier. Although I was unable to determine whether that bitterness was directed at Chamberlin or at Salisbury, it seemed to be directed more at Chamberlin and at the situation in which he (Collie) found himself, usually playing second fiddle to Salisbury. However, I can find absolutely no semblance of this bitterness in Collie's long biography of Chamberlin (Collie, 1932), which was written nearly 20 years before my last conversation with him. Indeed, his account of Chamberlin's career is one of great respect and admiration for his former teacher.

Collie's retirement in 1931 ended 58 years of professorial service to Beloit College by the Chamberlin–Salisbury–Collie trio. All three served as president of the Beloit College Alumni Association; all three served on the Beloit College Board of Trustees—Chamberlin for 23 years; all three were awarded honorary LL.D. degrees by their alma mater. It may be stated with assurance that all three remained loyal sons of Beloit College until their deaths.

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WISCONSIN AGRICULTURAL GEOLOGISTS: AHEAD OF THEIR TIME

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INTRODUCTION

Agricultural geology was the study of the origin, nature, composition, and distribution of soil from a geological viewpoint (Tandarich, 1998). It developed as an interest and eventually a subdiscipline of geology. Although it was named in the nineteenth century, it has origins reaching back much further. Tandarich and Sprecher (1994) and Tandarich (1998) discussed the origin and development of agricultural geology from its roots in classical mineralogy. Because this area of geology is little known, I shall trace the path of its development that led to its use in Wisconsin during the nineteenth century.

AGRICULTURAL GEOLOGY FROM EUROPE TO WISCONSIN

In northern Europe, the influence in the late eighteenth and early nineteenth centuries of Abraham Gottlob Werner of the Bergakademie Freiberg in Saxony is well documented by Ospovat (1971) and Laudan (1987). In particular, Werner's concept of geognosy is pertinent here. Geognosy *(Geognosie* in German) was defined by Werner and translated by Werner biographer Alexander Ospovat literally as "the abstract systematic knowledge of the solid earth" (Ospovat, 1971, p. 101). Werner's geognosic writings about agriculture, although unpublished, were the foundation of what came to be called *Agrikulturgeognosie* in northern Europe and Russia, *geologie agricole* in France, and agricultural geology in the United States in the nineteenth century.

Although he did not study directly with Werner, William Maclure credits Werner for the geognosic framework of knowledge that he used in his study of the geology and soils of the eastern United States (Maclure, 1809). Maclure, a native of Scotland, was directly influenced by a student of Werner's, Robert Jameson at the University of Edinburgh, and by a French friend, Comte de Volney, who had published a treatise about the soils of the United States (Volney, 1804).

Subsequent efforts of American geologists in the study of soils relied on the foundation laid by Maclure. Some of the people influenced by this developing academic tradition were Benjamin Silliman at Yale and his students, Amos Eaton and Edward Hitchcock. Although agricultural geology was occasionally taught elsewhere, Yale University in New Haven, Connecticut, developed the first systematic curricula in agricultural geology.

Eaton went on to teach geology at Rensselaer Polytechnic in New York. Many of Eaton's students became notable agricultural geologists—for example, George Hammel Cook, Edward Hitchcock, and Ebenezer Emmons. They were hired to work in the newly established state geological surveys that became active in the early to mid-nineteenth century. The geology of agriculture was considered part of the domain of study of the state surveys.

Eaton student Ebenezer Emmons co-founded the *American Quarterly Journal of Agriculture and Science* in 1844. In this journal he published articles about agricultural geology in 1845 (Emmons and Prime, 1845) and 1846 (Emmons, 1846). His 1845 article served to define this subdiscipline of geology and its practice in the United States.

Students of Eaton as well as those of other individuals and institutions staffed the state geological surveys and worked actively in agricultural geology during the nineteenth century. Many of these scientists were also associated with colleges and universities. Some of these investigators who were connected with Wisconsin were David Dale Owen, Increase Allen Lapham, and Thomas Chrowder Chamberlin.

Owen's agricultural geologic work extended to eight states, including Wisconsin, during the period from 1837 to 1860, although he is best known for his soil analyses in Indiana and Kentucky (Corgan, 1982; Zabilka, 1982). Nelson (1976) discussed his survey work in Wisconsin.

Lapham (1850, 1851) discussed the geologic aspects of the agricultural potential of Wisconsin.

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Figure 1. *Chamberlin (1882) map of the soils of Wisconsin.*

Agricultural considerations, particularly regarding an assessment of the composition and quality of soils of the state, were among the purview of the geological surveys of Wisconsin that preceded the Wisconsin Geological and Natural History Survey. This was not lost on Lapham's colleagues and successors, most notably Thomas Chrowder Chamberlin.

THOMAS CHROWDER CHAMBERLIN AND HIS LEGACY

The agricultural geologic work of Chamberlin is little known today, but is noteworthy in the history of geology, soil science, and particularly pedology. A native of Mattoon, Illinois, Chamberlin attended Beloit College, graduated in 1866, and returned to teach there from 1873 to 1882, while concurrently working for the Complete Geological, Mineralogical and Agricultural Survey, the immediate predecessor to the Wisconsin Geological and Natural History Survey. He studied for a year (1869–70) with Alexander Winchell at the University of Michigan.

Winchell's work on the agricultural geology of Michigan (Winchell, 1865) no doubt influenced Chamberlin, for while at the Wisconsin Survey Chamberlin published several treatises on the agricultural geology of Wisconsin starting in 1874 (Chamberlin, 1874a, 1874b, 1877, 1883). His view of agricultural geology is stated in the 1883 survey report:

> *Only a portion of the complicated questions that are involved in the highest utilization of the soil fall within the field of the geologist…. The geological aspects relate mainly to (1) the origin and nature of the soil, (2) to its waste [erosion] and reproduction by natural means, (3) drainage, and (4) natural fertilizers (Chamberlin, 1883, p. 678).*

Chamberlin produced two maps based on physical properties of soil, one of eastern Wisconsin in 1876 (Chamberlin, 1876) and another of the entire state of Wisconsin in 1882 (Chamberlin, 1882; fig. 1). In his map philosophy, Chamberlin anticipated the detailed soil maps of today:

> *There are few natural formations more difficult to map than soils. There is an almost infinite gradation of varieties between which there are no hard-and-fast lines, and it is nearly or quite impossible to represent these gradations on a map. Moreover these gradations run through more or less of their minor changes on almost every farm, and to attempt to represent these for the more than 50,000 square miles of land embraced within the limits of Wisconsin would be an undertaking of no small magnitude, and would require maps of very large scale and elaborate execution, and when executed, while extremely valuable for certain uses, the very confusion of details would be a source of inconvenience in the more general studies (Chamberlin, 1882, p. 657).*

These two maps are noteworthy and were recognized early in this century as being "modern" in approach by George N. Coffey, the first director of the national soil survey program in the U.S. Department of Agriculture (Coffey, 1912). In addition, the 1882 general soil map is the first large-scale state soil map of its type produced in the United States. The map units (fig. 2) of both maps were based on a soil physical property, that of texture (a term used more qualitatively than today's concept) (fig. 3). Chamberlin's ap-

Figure 2. *Southeastern portion of Chamberlin (1882) map showing soil delineations.*

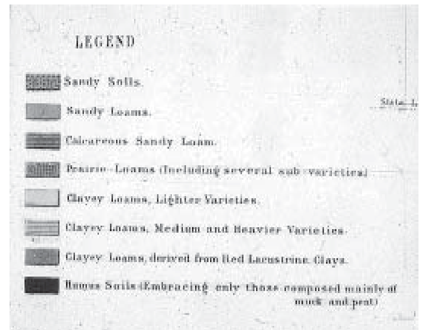

Figure 3. *Chamberlin (1882) map legend.*

proach was a departure from the practice of considering soils merely as a geologic unit.

Chamberlin was also interested in the problem of soil erosion (soil “wastage” as he called it) as early as 1876. He recognized that farmers in eastern Wisconsin were beginning to farm newly exposed subsoils and produced the 1876 map showing the textural groups of the subsoils. Later in his career, Chamberlin delivered an address on soil wastage in 1909 (Chamberlin, 1909) and published on the subject in the journal *Science* in 1911 (Chamberlin, 1911). The 1909 public address caught the attention of soil scientist Hugh Hammond Bennett, who afterwards decided to make soil conservation his life’s work, which ultimately led to the establishment of the U.S. Soil Conservation Service.

Chamberlin worked for the U.S. Geological Survey from 1882 to 1887 as head of the Glacial Geology Division and taught at the Columbian (now George Washington) University from 1885 to 1887. While employed at the U.S. Geological Survey, he guided W.J. McGee and Frank Leverett toward agricultural geologic work.

When Chamberlin was appointed president of the University of Wisconsin–Madison in 1887, he returned to Wisconsin and remained there until 1892. In 1889 he established the Department of Agricultural Physics that later, in 1905, was renamed the Department of Soil Science—the first such department established in the United States (Beatty, 1991). Chamberlin’s former assistant at the Wisconsin Survey, Franklin Hiram King was installed as professor and head of this new department (Beatty, 1991). King is best known for his pioneering work in soil physics and soil fertility and management (Tanner and Simonson, 1993).

In 1892, University of Chicago President William Rainey Harper persuaded Chamberlin to leave his post as president of the University of Wisconsin and assume the chair of the geology department at that newly organized university. According to Fisher (1963), the first faculty members of that department were Chamberlin, Joseph Paxson Iddings, Richard Alexander Fullerton Penrose Jr., Rollin D. Salisbury, and Charles Richard Van Hise.

Some University of Chicago geology students who became involved in Wisconsin agricultural geologic–Quaternary geologic–soil studies were William C. Alden, Allen David Hole (fig. 4), and Andrew Robeson Whitson. Alden, originally from Iowa, received his M.A. in 1898 and Ph.D. in 1903. One of his most notable works was his tome about the Quaternary geology of southeastern Wisconsin, which included a section on soils (Alden, 1918).

Hole, a native of Indiana, worked with Alden (1918) on his study of southeastern Wisconsin. He re-

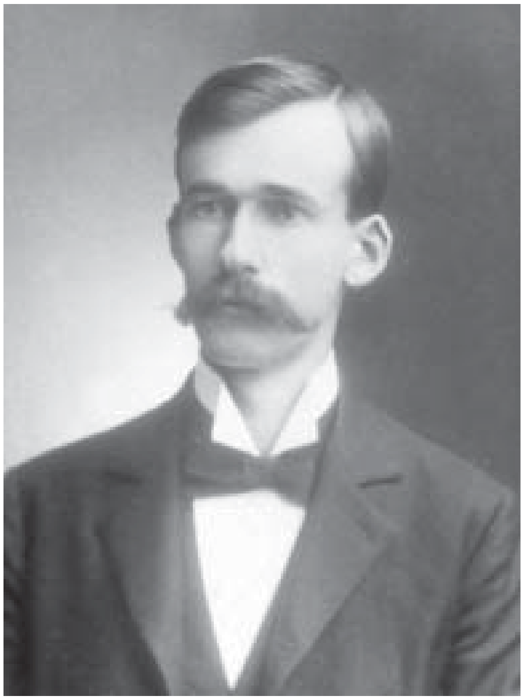

Figure 4. *Allen D. Hole, circa 1905. (Photograph courtesy of Francis D. Hole.)*

turned to teach at his alma mater, Earlham College, and trained several agricultural geologists, including his son Francis Doan Hole, now professor emeritus of the University of Wisconsin–Madison (Tandarich and others, 1988a, 1988b).

Whitson was a student of Chamberlin and Salisbury at the University of Chicago. He completed his B.S. under them at Chicago in 1894 and continued in graduate study there from 1894 to 1895 and 1899 to 1900, specializing in soils and crop production (Beatty, 1991; Hole, 1991). After returning to the University of Wisconsin–Madison, Whitson became the head, in 1901, of the Department of Agricultural Physics and then the head of the Department of Soil Science in 1905.

When the Wisconsin Geological and Natural History Survey was established in 1897, one of its missions was to study the soils of the state (Ostrom, 1988). Thus, the Survey published a general soil and agricultural report in 1903 (Weidman, 1903). Subsequent soil survey reports were published until the 1950s by the Survey in cooperation with the U.S. Department of Agriculture and the University of Wisconsin Soils Department (Beatty, 1991).

SUMMARY AND EPILOGUE

Agricultural geology is an important antecedent discipline of soil science. In Wisconsin, the agricultural geologic legacy of T.C. Chamberlin is particularly significant. Chamberlin's students directly and indirectly have been responsible for the development of several subdisciplines of soil science in the state and beyond, including soil physics, soil chemistry, soil fertility, and pedology. Chamberlin's innovations in conceptualizing a soil map and soil survey foreshadowed the national soil survey program established in 1899. The Wisconsin Geological and Natural History Survey and its predecessors published works on agricultural geology that, during the twentieth century, were known as soil surveys.

Internationally, a scientific society of agricultural geology or agrogeology had formed and held its first meeting in 1909 in Budapest (Szabolcs, 1997). That society of agrogeology changed its name to the International Society of Soil Science in 1924 and in 1999 became the International Union of the Soil Sciences. By the time of the First Congress of Soil Science held in Washington, D.C., in 1927, the practitioners of soil science no longer used the name agricultural geology.

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The Remarkable Legacy of the Wisconsin School of Precambrian Geology

Robert H. Dott, Jr.[1]

ABSTRACT

R.D. Irving, C.R. Van Hise, C.K. Leath, and W.J. Mead formed a remarkable intellectual genealogy, which must be unique in the history of geology. By the 1920s, their Wisconsin School of Precambrian Geology had become so prominent that it attracted students from around the globe. Roland D. Irving (1847–88) was Wisconsin's first true geologist. He laid the groundwork for all subsequent investigations of the Precambrian rocks of the Lake Superior region of the United States and pioneered the application of microscopic petrography there. His protégé, Charles R. Van Hise (1857–1918), earned the first M.S. and Ph.D. degrees awarded by the University of Wisconsin. Upon Irving's premature death in 1888, Van Hise suddenly became both head of the Department of Geology and Mineralogy and chief of the U.S. Geological Survey (USGS) Lake Superior Division, which had been established at the university in 1882. Nine monographs and several bulletins emanated from the Division.

Charles K. Leith (1875–1956) was in turn a protégé of Van Hise; Leith became both head of the geology department and chief of the USGS Lake Superior Division after his mentor was named president of the university in 1903. In 1908, Leith appointed his own protégé, Warren J. Mead (1883–1960), to the faculty, for he recognized that Mead's quantitative and experimental talents could help to fulfill the dedication of the Wisconsin School to a sound physical and chemical understanding of rock deformation and metamorphism. Together they made widely available through textbooks important new principles of structural and metamorphic geology developed by the Lake Superior Division. During both world wars, Leith was adviser to the federal government on mineral economics, and in 1921 he authored a textbook about economic geology. Beginning in 1905, he carried on an active consulting career in addition to his other duties. Mead followed suit, and soon pioneered the new field of engineering geology.

Among many outstanding students of the early twentieth century Wisconsin School, two were of particular significance. Florence Bascom earned the M.S. in 1887 under Van Hise, the Ph.D. at The Johns Hopkins University in 1893, was elected to Fellowship in The Geological Society of America in 1894, and initiated an important geology program at Bryn Mawr College in 1895. In 1926, Englishman Gilbert Wilson earned the M.S. under Mead, and then received the Ph.D. from Imperial College in London in 1931. In 1939 he joined the Imperial faculty, where he inspired a postwar revolution in detailed structural analysis in Britain. The impact of Wilson and his students soon spread over Europe, and even reached back to Wisconsin, the spawning ground for that revolution.

INTRODUCTION

Few geologists have the choice that I face every morning, whether to walk to the university along Chamberlin (misspelled Chamberlain) Avenue or Van Hise Avenue. Madison, Wisconsin, may be the only city in North America with two parallel streets named after geologists. This circumstance reflects the equally unusual fact that our university has had two geologists serve as president, namely these same two individuals. Moreover, each has a campus building named for him, and my children attended Van Hise Middle School before it was renamed.

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Why should geology loom so large in Wisconsin history? The public prominence of Thomas C. Chamberlin and Charles R. Van Hise is due to their distinguished administrative roles as presidents of the university. Indeed, most present faculty members and local citizens who are familiar with these names have no idea that both were distinguished geologists first. By 1903, when Van Hise took the university helm, a so-called Wisconsin School of Precambrian Geology had already developed a considerable reputation. Chamberlin had first gained prominence as the skillful director of the later phases of the Second Wisconsin Geological Survey of 1873–79. The four-volume reports of the *Geology of Wisconsin* were as good as the best publications from any survey in the world at that time. Largely because of his success with the survey, Chamberlin was tapped for the university presidency in 1887. But success breeds success, and in 1892 he was lured away to organize a department of geology in the new University of Chicago. Although his geological interests were different from those of Van Hise, being chiefly in glacial geology, Chamberlin remained in close touch with his Wisconsin colleagues for many years and continued to act as an important catalyst for the science here. He even recruited Van Hise to present a course on structural geology at Chicago in alternate years from 1892 to 1902; Van Hise appears in photos of the University of Chicago geology faculty from that period. When Van Hise became president of the University of Wisconsin, Chamberlin then recruited Charles K. Leith to present the same course from 1905 to 1917.

During Chamberlin's presidency (1887–92), the University of Wisconsin underwent revolutionary changes, which were to give it international stature (Curti and Carstenson, 1949). Chamberlin championed curricular reforms to bring more science, to introduce concentration in a major field, and to introduce the seminar method of teaching; a Ph.D. program was also instituted with C.R. Van Hise receiving the first such degree in 1892 (Bailey, 1981). Chamberlin doubled the size of the faculty and laid the groundwork for an extension program. He also argued that the scholarship of the university should benefit the entire state. All of these innovations were strengthened by his successor, Charles K. Adams, and were culminated during Van Hise's tenure (1903–18). In 1904 a formal graduate school was founded and in 1907, a medical school. Also in 1907 the Extension Division was expanded to fulfill the famous Progressivist Wisconsin Idea that "the boundaries of the campus should be the boundaries of the state."

With the rapid expansion of agriculture and industrialization had come pleas all across the nation for more practical educational opportunities. In 1862 and again in 1890, Congress passed the Land Grant Acts, which provided federal aid to states in the form of grants of federal land, which could be sold or otherwise exploited to fund colleges that would emphasize education in agriculture, the mechanic arts (engineering), home economics, and ROTC. Then in 1887, the Hatch Act was passed, which created Agricultural Experiment Stations to stimulate agricultural research. Wisconsin responded promptly to these initiatives. Both developments helped foster the important new idea that research should be a handmaiden of education, which had been formalized first at The Johns Hopkins University in 1879 with the creation of a formal Graduate School on a German model. Most American geologists were right in step with the new initiative, for they had already been doing research through state or federal surveys or with the mining industry. The timing was perfect for Van Hise to be president, for he had been practicing this dual approach for 25 years, that is, ever since receiving his bachelor's degree!

WHY AT MADISON?

Against the background of a national impetus for educational innovation, we may ask why did a great School of Precambrian Geology develop at Wisconsin at the beginning of the twentieth century? To answer this question, we must investigate a remarkable intellectual genealogy and link that with the industrial expansion of the nation as well as the sweeping changes in higher education outlined above.

The arrival of Roland D. Irving in 1870 marked the real beginning of a geology program at the University of Wisconsin (fig. 1). A grandnephew of Washington Irving, he had been professionally trained at the Columbia University School of Mines in New York. He quickly gained prominence within the university as a faculty leader and outside the university as a research investigator (Curti and Carstenson, 1949). A new geological survey of the state commenced three years after he arrived, and he was recruited to participate in this investigation. Irving first studied the iron- and copper-bearing regions of northern Wiscon-

Figure 1. *Roland D. Irving, the first professionally trained professor of geology and mineralogy at the University of Wisconsin (1870–88) and first chief of the U.S. Geological Survey Lake Superior Division (1882–88). This photograph dates from near the time of Irving's appointment to the federal survey in 1882.*

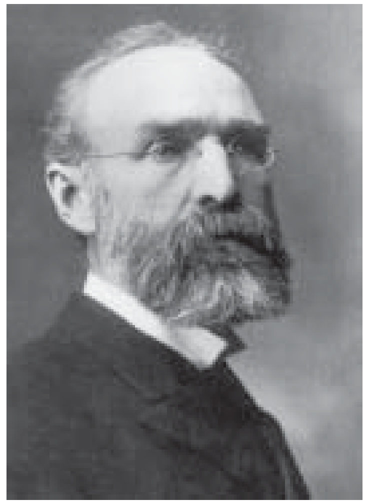

Figure 2. *Charles R. Van Hise, protégé of Roland D. Irving, professor of geology (1882–1903), second chief of the U.S. Geological Survey Lake Superior Division (1888–1903), and president of the University of Wisconsin (1903–18). This photograph shows Van Hise in 1905, early in his presidency.*

sin; one result was the first recognition of a Lake Superior syncline. Next he investigated a large portion of central Wisconsin, including the Baraboo District. In 1883, his full study of the copper-bearing rocks (Keweenawan) was published by the USGS as Monograph 5. He also invoked the new technique of "microscopic lithology" (petrography) in this research and introduced it into the teaching program. Irving's initial appointment was in Mining and Metallurgy, but in 1878 a separate Department of Mineralogy and Geology was created with him as its professor. In 1880, his star student, Charles R. Van Hise, was appointed as instructor (fig. 2). Like Irving, Van Hise also worked part-time for the state geological survey, principally doing petrographic studies of Irving's samples (Bailey, 1981).

The states' rights tradition had discouraged the national government from sponsoring geological surveys within the original states, but there was no such restraint within the territories created by the 1787 Northwest Ordinance. Two surveys of the upper Mississippi Valley region led by D.D. Owen (1840, 1852) and the survey of Northern Michigan and northeastern Wisconsin led by J.W. Foster and J.D. Whitney (1850–51) had set the precedent. Lead deposits had motivated the Owen surveys, while the discoveries in northern Michigan of native copper in 1831 and iron ore in 1844 had spurred the Foster–Whitney survey. In 1880, soon after the Wisconsin state survey was completed, the USGS recruited both Chamberlin and Irving as special agents for the Tenth National Census, which was to include for the first time the gathering of statistical data on mineral resources under the direction of the USGS (Rabbitt, 1980, p. 25; Nelson, 1999, written communication). Chamberlin had been one of 13 state geologists who supported the efforts of Director King to extend the federal survey's activities into the states in order to prepare a geological map of the entire nation, and in 1881 Chamberlin was appointed chief of a new Glacial Division, a position he held for five years. Meanwhile, Irving proposed to the new USGS Director, J.W. Powell, that an integrated survey of the Precambrian rocks of the Lake Superior region, which would include petrographic studies, was now needed (Geschwind, 1994, p. 42). In 1882 the USGS adopted his suggestion, and established a Lake Superior or Precambrian Division to investigate the iron-bearing rocks, with Irving in charge of a new office located at the University of Wisconsin in Madison. Irving continued also to head the Department of Mineralogy and Geology, a balancing act hardly imaginable today.

As part of the USGS expansion, college professors were hired on temporary appointments to assist both field and laboratory efforts. Besides Chamberlin and Irving of Wisconsin, George W. Williams of The Johns Hopkins University was appointed to work with Irving during the summers of 1885 and 1886. His training in the new techniques of microscopic petrography under H. Rosenbusch at Heidelberg (Ph.D., 1882) made him a valuable asset. Evidence of the complex organizational relationships among university, state, and federal personnel is exemplified by Raphael Pumpelly, who was variously professor of mining engineering at Harvard, temporarily state geologist of Michigan (1869–71) and an assistant doing petrography for the second Wisconsin survey in 1877. In 1879 he joined the new USGS, and it was he who transmitted Irving's manuscript for USGS Monograph 5 (Irving, 1883) on the copper-bearing rocks to the Director for publication. In 1881 Pumpelly left the Survey to lead a Northern Transcontinental (railroad) Survey, but he returned to take charge of the Archean Division from 1884 to 1890. This new responsibility kept him in contact with the Lake Superior Division in Madison.

The new federal initiatives did not go unchallenged. For example, the seven prominent editors of *The American Geologist* presented a protest in their new journal in 1888. They professed "serious misgivings as to the result of the influence of the national geological survey in extending its operation into the settled states....especially into the states in which official geological surveys are in progress" (Calvin and others, 1888, p. 2–3). Their expressed fear was that such concentration of effort might cause a loss of public support of geological investigations at the state level, but could they also have harbored some sour grapes?

Things were happening rapidly in the 1880s. Van Hise was granted one of the first two M.S. degrees given by the university in 1882, the basis for his degree being the research for the recent state survey. He was then promoted to assistant professor and was also appointed to a part-time post in the new Lake Superior Division. Irving put him in charge of field investigations of the Penokee–Gogebic Iron Range, which straddles the Wisconsin–Michigan border. The results of that work appeared in 1892 as USGS Monograph 19, co-authored by Irving and Van Hise. On the basis of this publication, Van Hise was granted the first Wisconsin Ph.D. degree ever awarded, also in 1892.

During the same period, Florence Bascom, daughter of university president John Bascom, studied geology under Irving and Van Hise. In 1887 she received the second M.S. degree ever in geology; it was granted for a petrographic study supervised by Van Hise of layered gabbros in the Penokee range around Mellen. After teaching in secondary schools for four years, Bascom applied to Johns Hopkins for further postgraduate study. Professor Williams, who had worked with Irving in Wisconsin while Bascom was studying for the M.S., acted as her adviser. Overcoming such obstacles to women as having to sit behind a screen during lectures, she received the Ph.D. in 1893 (Arnold, 1983). Bascom was the first woman geologist in the nation to earn that degree, and in 1894 became the second woman to be elected Fellow of The Geological Society of America. Because of her research promise, she was invited in 1895 to found a geology program at Bryn Mawr College in Pennsylvania, where she inspired many younger women to pursue careers in science. She continued to do outstanding research through a part-time appointment with the USGS.

In 1888 Irving died unexpectedly, and Van Hise suddenly inherited both the headship of the academic department and of the USGS Division. Chamberlin was now president of the university and Van Hise's classmate and friend, the Progressive politician Robert LaFollette, was governor. Geology thrived at Wisconsin. Directing a small army of geologists mapping all of the principal iron mining districts of Minnesota, Wisconsin, and Michigan was a formidable task, but one that Van Hise discharged with efficiency and imagination. An impressive series of detailed publications appeared over a 20-year period spanning the turn of the century. The most important of these are listed in table 1.

As a consequence of the massive effort of the Lake Superior Division, Van Hise became thoroughly familiar with the complex Precambrian geology of the entire region. For the federal survey, he also visited many other regions of the country where Precambrian rocks are exposed and consulted with the survey geologists working there. He always looked beyond the details of each district in search of a general synthesis, and in so doing, became a master exponent of multiple working hypotheses even before his colleague, Chamberlin, made that method of investigation famous. Van Hise became especially fascinated with the deformation and metamorphism that the old rocks displayed, and soon became a leading authority on the fundamentals of structural and metamorphic geology. This emphasis culminated in his most famous publications, "A Treatise on Metamorphism," a mere 1,285 pages long (Van Hise, 1904a), and a synthesis of all of the Lake Superior work, "Geology of the Lake Superior Region" (Van Hise and Leith, 1911). Through his own research and from his initial training in engineering and metallurgy, Van Hise came to appreciate more than most contemporaries that if geology was to advance from mere classification to the formulation of principles, its practitioners must become well grounded in basic mechanics and chemistry. He expressed this conviction emphatically in an address to an International Congress of Arts and Sciences at St. Louis in 1904, for which he was asked to address a daunting topic, "The Problems of Geology" (Van Hise, 1904b).

By the 1890s, Van Hise had recognized an urgent need for a much greater understanding of the behavior of minerals and rocks under conditions of pressure and temperature far beyond human experience. At the turn of the century, he was invited to join with T.C. Chamberlin and a few other visionaries to champion the establishment of a national laboratory to conduct experimental investigations on this geological frontier, and in 1906 was born the Geophysical Laboratory within the new Carnegie Institution of Washington (created in 1902). This laboratory would soon move the United States to the forefront of research on some of the most fundamental problems of earth science (Yochelson and Yoder, 1994). Coming very soon after he began his new career as president of his university, this was a fitting capstone to Van Hise's geological career and his state-of-the-art synthesis of structural and metamorphic geology.

THE VAN HISE–LEITH–MEAD DYNASTY

When Van Hise was called to the presidency in 1903, the publication history of the Lake Superior studies was at its midpoint (table 1), and there was still much to be done. Van Hise repeated his own inheritance from Irving 17 years prior by immediately promoting his most promising protégé, Charles K. Leith (fig. 3), to direct the Precambrian investigations, and, in 1905, also to chair the Department of Geology. Leith now supervised the completion of the publications about the iron ranges and also expanded the academic program in geology. It was remarkably fortuitous that Van Hise had such an able young colleague to whom he could pass his geological torch.

How did such a coincidence come about? Leith had entered the university in 1892 at the age of 17. Having previously taken some business training, he answered a help-wanted advertisement for a secretary

Table 1. *Principal publications from the U.S. Geological Survey Lake Superior Division during the Irving–Van Hise–Leith era (1883–1935).*

USGS publication	Subject of report (Author[s] and year of publication)
Monograph 5	Copper-bearing rocks of Lake Superior (Irving, 1883)
Bulletin 62	Greenstone schists (Williams, 1890)
Bulletin 86	Pre-Cambrian correlations (Van Hise, 1892)
Monograph 19	The Penokee–Gogebic Range (Irving and Van Hise, 1892)
Annual Report	Principles of Pre-Cambrian geology (Van Hise, 1896)
Monograph 28	Marquette district, Michigan (Van Hise and others, 1897)
Monograph 36	Crystal Falls district, Michigan (Clements and others, 1899)
Monograph 43	Mesabi Range, Minnesota (Leith, 1903)
Monograph 45	Vermillion district, Minnesota (Clements, 1903)
Monograph 46	Menominee district, Michigan (Bayley, 1904)
Monograph 47	Treatise on metamorphism (Van Hise, 1904a)
Bulletin 239	Rock cleavage (Leith, 1905)
Monograph 52	Synthesis of the Lake Superior region (Van Hise and Leith, 1911)
Professional Paper 184	Pre-Cambrian of the Lake Superior region (Leith and others, 1935; update of Monograph 52)

Figure 3. *Part of the University of Wisconsin Department of Geology personnel for 1919–20. From left to right: Back row: C.K. Leith and E.F. Bean; middle row: H. Weeks (student; older brother of Lewis G. Weeks), W.H. Twenhofel, and A.D. Conover (student); front row: W.J. Mead and A.N. Winchell.*

to Professor Van Hise, which job he hoped would finance his education. Leith soon became so fascinated by his employer's work that he chose to major in geology (McGrath, 1971). Van Hise recognized Leith's unusual ability, and immediately appointed him to the Lake Superior Division when he graduated. In 1901 Leith received the Ph.D. for a dissertation on rock cleavage, which was published as USGS Bulletin 239 (1905). There was a great and growing demand for expert consultants to the mining industry, especially for the man who had done the definitive study of the great Mesabi iron ores in Minnesota (USGS Monograph 43, 1903), which were then just beginning to be developed. So, in 1905, just two years after assuming direction of the division from his mentor, Leith changed his federal appointment to a per diem basis so that he could begin a long and lucrative consulting career.

During both world wars, Leith became much involved as an adviser to the federal government on strategic minerals, which led his career in an important new direction. Like most of his other work, even Leith's long involvement with the economic, strategic, and political aspects of minerals had a precedent in his mentor's career, for Van Hise had been an active participant in the earliest conservation movement championed by President Theodore Roosevelt. Van Hise's concern had been aroused early in his career by the impact upon him of the thoughtless decimation of the forests of the Lake Superior region and the resulting loss of soil by accelerated erosion. As a result, he became an outspoken leader in the movement at both state and federal levels. His vigorous challenge to corporate exploitation of forests and water embroiled him in political controversy, which brought questions of the propriety of such involvement by the president of the state university. In 1910 Van Hise published *The Conservation of Natural Resources of the United States,* the first general book on the subject. Leith's efforts in conservation took a somewhat different tack by emphasizing the importance of mineral resources in national and international affairs during both war and peace. His views were so influential that he was appointed as mineral adviser to the American Commission to negotiate peace, and in 1919 he accompanied Woodrow Wilson to the Versailles peace conference. In 1921 Leith published the textbook *Economic Aspects of Geology,* which not only treated all types of mineral resources and the geology of engineering construction, it also contained a novel section on the geopolitical implications of mineral resources, including a chapter titled "Geology and War." Ten years later he published a more popularly oriented book, *World Minerals and World Politics,* which stressed even more the importance of mineral resources in human affairs (Leith, 1931). He continued to write, speak, and advise on mineral resources during the remainder of his career, and he implemented a course at the university called Minerals as a Public Problem, which is still taught today.

In the years before World War I, industrialization was racing ahead, *laissez faire* capitalism was at its apogee, and the clamor from mining interests for his talents had made consulting irresistible to Leith. And why not, for he could have his cake and eat it, too. He could continue his university base and some research for the USGS as well as pursue private commercial ventures. Even Van Hise participated in some of those ventures in spite of his presidential duties. Capitalizing upon a friendship with Andrew Carnegie, Van Hise gained financial backing for mining exploration and speculation in Ontario from 1902 to 1907. Leith and, to a lesser extent, Van Hise, supervised a group numbering as many as 40, which was made up of geology graduate students and other assistants during the summer field season, mostly in Ontario. Included among the many workers were future state geologist W.O. Hotchkiss and future faculty members F.T. Thwaites and W.J. Mead. A later, even more ambitious Leith–Van Hise enterprise was an effort to develop a large, Mesabi-scale iron mine in Brazil (McGrath, 1971, chap. 6). Begun in 1911, this complex project finally collapsed during the 1940s when the Brazilian government adopted policies adverse to foreign investments. Although both men were circumspect about their commercial activities, especially their involvement with Carnegie, it is still amazing from a modern perspective how little fuss was made either by the press or the state government about both the university's president and a prominent department head being involved in such ventures (Vance, 1960).

Figure 4. *"Mead's Toy Shop": Some of the apparatus built by W.J. Mead to illustrate the fundamental mechanics of rock deformation (from Rettger and Emmons, 1921, p. 218).*

During mineral explorations in Ontario in the summer of 1902, some student fieldworkers conceived the idea of an annual yearbook or scrapbook named the *Outcrop* (Deming, 1926). Leith (ca. 1938) reported that it began as a kind of newspaper with a social column and a miscellaneous column concocted to relieve the boredom of rainy, tent-bound days. It soon developed, however, into an ambitious, liberally illustrated record of both the serious and diverting activities of the entire Department of Geology with special emphasis upon summer field work. The Geology Club's *Outcrop* was produced by elected student co-editors almost every year from 1902 through 1957. These elaborate volumes, which now reside in the University of Wisconsin Archives, are historical treasure troves of information about the department not to be found in official histories. Especially interesting are letters from alumni about their experiences exploring for minerals or petroleum in far-flung corners of the Earth. Beginning in 1924, a short extract was published in multiple copies under the title, *The Outcrop–Printed Version,* which contained lists of faculty and students, the addresses of alumni, and a few brief notes about departmental activities such as visiting speakers. Beginning in 1970, an Alumni Newsletter replaced the scrapbook format as a medium for recording annual events in the department; the name *Outcrop* was resurrected, however, as the title for an occasionally published alumni directory.

Continuing the Wisconsin intellectual genealogy, Leith appointed his own most promising student, Warren J. Mead, as instructor in 1908 and assistant professor two years later (fig. 3). Leith saw that Mead's special talent for experimental and quantitative approaches to geological problems would provide a fine complement to his own field approaches (Bailey, 1981). Mead built ingenious apparatus for investigating and teaching the deformation and metamorphism of rocks (fig. 4). Like Leith, he soon was in demand for consulting. Through the urging of Van Hise, Mead was asked in 1915 to investigate severe landslide problems, which were hampering the excavation of the Panama Canal. This experience led him to become a pioneer in engineering geology; he soon created what was probably the first course in this subject. In 1934, Wisconsin lost Mead to MIT.

In the first 15 years of his leadership, Leith quadrupled the size of the geology faculty, thus broaden-

Figure 5. *Group photo from the Lake Superior field trip of May 1926. W.J. Mead is second from the left at the back (with brimmed hat); Gilbert Wilson is at the right front. Others from left to right are: Back row: W.A. Seaman (professor at Michigan Institute of Technology), Mead, K. Fowler, E. Hahn. Middle row: C.H. Stockwell, W.F. Brown, J.M. Hansell; front row: W.P. "Texas" Rand, H.S. Bostock, and Wilson. Bostock and Stockwell had distinguished careers with the Geological Survey of Canada. Katharine Fowler received the Ph.D. from Columbia University, pursued a lifelong career in geology, and married Harvard structural geologist Marland P. Billings. In 1999 she received (posthumously) the first Wisconsin Department of Geology and Geophysics Distinguished Alumnus Award. Emily Hahn practiced engineering and geology for only four years before turning to journalism. In 1976 the university awarded her an honorary degree for her distinction as a writer and a champion of women's rights to pursue their own careers.*

ing the specialties represented (detailed in Bailey, 1981). Some of the more notable additions were A.N. Winchell in mineralogy and petrology (1908), F.T. Thwaites as museum curator (1911) and later lecturer in geomorphology and glacial geology (1928), and W.H. Twenhofel in sedimentation and paleontology (1916), to mention only three who remained on the faculty for many years. But it was the Leith–Mead partnership that originally put the Wisconsin department on the map. Leith's textbooks, *Structural Geology* (1913, 1923) and the Leith and Mead *Metamorphic Geology* (1915) publicized the pioneering concepts of Van Hise and disseminated widely the wealth of insights gained from the Precambrian studies in the Lake Superior region during the preceding three decades. In recognition of the stature of Wisconsin's pioneer geologists, the National Academy of Sciences inducted Van Hise to Fellowship in 1902, Chamberlin in 1903, Leith in 1920, and Mead in 1939. By 1910 the department's reputation had grown so much that students were applying from all over the United States and Canada. In 1927 Francis J. Pettijohn considered only Wisconsin, Berkeley, Chicago, and Yale for his pursuit of the Ph.D. (Berkeley won; Pettijohn, 1984, p. 99). Throughout the 1920s, graduate students and postdoctoral scholars were coming to Madison from Europe, China, and Japan—the boundaries of the campus had expanded over the entire globe!

GILBERT WILSON AND MODERN STRUCTURAL GEOLOGY

The most impressive measure of the global reputation of the Wisconsin School of Geology is provided by the career of a young Englishman named Gilbert Wilson, who came to study at Madison in 1925–26. He grew up in the English Lake District, but had crossed the Atlantic in 1920 to study mining engineering and geology at McGill University in Montreal, where he graduated in 1925. While there, he heard of the Wisconsin reputation, and decided to come here for postgraduate study. Wilson immersed himself in the Precambrian geology of Leith, Mead, and Winchell, studying structural and metamorphic geology, petrography, and ore deposits, and participated in field trips to the Lake Superior region (fig. 5). In 1926 he was awarded the M.S. for a thesis titled "The Pre-Cambrian Trendlines," which was supervised by Mead. Curiously, his mentor, who had already supervised several M.S. and at least two Ph.D. degrees, was himself awarded the Ph.D. rather tardily in the same year. Those were indeed different times when a professor could oversee Ph.D. candidates before receiving the Ph.D. degree himself, and also could carry on an extensive consulting career while retaining a full academic position.

After Wisconsin, Wilson worked in mining geology in Russia, Yugoslavia, Canada, and Africa. In 1931 he took the Ph.D. at Imperial College in London and then was a lecturer at Reading University and University College, London, before joining the Imperial faculty in 1939. Wilson principally taught structural geology and field mapping at Imperial. He was a dedicated teacher and had an exceptional talent for illustrating complex, three-dimensional features with lucid drawings (fig. 6). Building upon the principles that he had learned from Leith and Mead at Wisconsin, he proceeded to refine and expand the analysis of deformed rocks. He single-handedly invented structural geology as a subdiscipline in British universities, where it had been largely ignored; stratigraphy and paleontology together with mineralogy and petrology had dominated for years (Ramsay and Cosgrove, 1987). Wilson himself wrote the following acknowledgment of his debt to Wisconsin (in Wilson and Cosgrove, 1982):

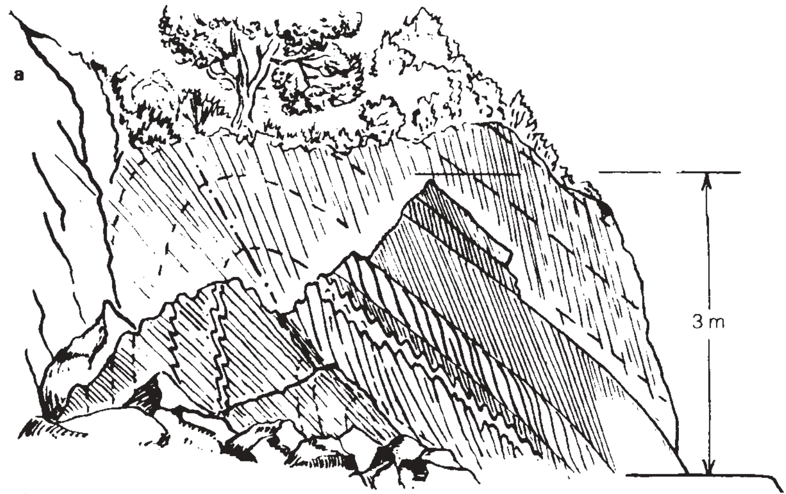

Figure 6. *Drawing by Gilbert Wilson of a medium-scale overturned anticline in Devon, southwestern England, showing the geometric relationship of slaty cleavage and small parasitic folds to the limbs and axial plane of the larger fold. It was such fundamentals of structural geology as this that Wilson had first learned at Wisconsin from Leith and Mead, and which he later refined and elaborated in Great Britain (first published in Wilson, 1961; reprinted in Wilson and Cosgrove, 1982).*

> *The foundations of much that I have written were laid in the lectures on structural geology by Professors C.K. Leith and W.J. Mead at the University of Wisconsin many years ago. The principles which they expound run through the whole of this work, in places disguised in modern jargon, elsewhere modified by more recent advances in knowledge, but it was they who instilled in me the importance of minor structures in the elucidation of the major structures in the field.*

What was it that so inspired young Gilbert Wilson at Wisconsin? It was that Van Hise, Leith, and Mead applied basic mechanics to explain the deformation of rocks in terms of maximum and minimum stress, directions of compression, shear, and extension, and the concept of the strain ellipsoid, that important device for the understanding of ductile deformation. The origin of slaty cleavage, schistosity, elongation of pebbles, and the like were emphasized, as was the value of such sedimentary features as cross bedding, graded bedding, and symmetrical ripple marks for determining original "way up" in overturned strata. Most specifically, however, as noted in his acknowledgment, Wilson was impressed with the ability to apply mechanical theory to the inference of obscure, large-scale structures from a systematic study of the geometric relationships of small-scale features like parasitic or drag folds and cleavage (fig. 6).

We must ask which of these structural concepts was original with the Wisconsin School. Given the centuries of quarrying of slate in Europe, it would be surprising if slaty cleavage had not attracted the early attention of geologists. Indeed, by the 1830s, cleavage was recognized by none other than Adam Sedgwick as distinct from bedding and that it was best developed in finer lithologies (Sedgwick, 1835). Several different origins were proposed, which included mechanical, chemical, electrical, and magnetic causes. During the 1840s, the distortion of fossils in slates was recognized and quantified, and this was taken by many as proof that cleavage must be of mechanical origin due to compression perpendicular to the cleavage planes (Sharpe, 1849). This inference seemed to be strengthened by the microscopic examination of slates by Sorby (1853) and compression experiments both by him and by Tyndall (1856). Many workers had noted that cleavage is remarkably consistent in its strike, tending to parallel the regional axis of elevation more closely than the local strike of bedding. Most workers thought the dip of cleavage planes varied so greatly in amount and direction that they failed to recognize any further pattern, but H.D. Rogers had observed in the Appalachian Mountains as early as 1849 that “the cleavage dip is parallel to the average dip of the anticlinal and synclinal axis planes” (Rogers, 1856, p. 447). He noted such deviations from his generalization as a fanlike arrangement at fold crests and a tendency for a sigmoidal shape across beds of contrasting lithology (that is, refraction). Rogers also suggested that “foliation is parallel or approximately so to the cleavage” and that these two phenomena were closely akin, having “originated at the same time and from one and the same cause” (p. 452). He rejected a mechanical origin, however, in favor of his own variation of Sedgwick’s early idea of molecular crystallizing forces ever resident in mineral matter, which have only to await the quickening influence of heat to awaken them (paraphrased from Rogers, p. 465 and 471). By the 1880s, however, the mechanical origin of cleavage under compression was fully accepted, and the strain ellipsoid was being invoked for its analysis (Harker, 1885). Debate now centered upon whether cleavage formed entirely after the folding of bedding or overlapped with that folding.

On the basis of pioneering microscopic studies, Sorby (1853) had argued that cleavage was produced during compression primarily by the rotation of platy minerals into a preferred orientation. In reviewing the subject in 1896, Van Hise (p. 633–668) concluded that the parallel development of new minerals was more important than flattening and rotation of earlier minerals. He also noted that elongate minerals tend to align in cleavage planes with their long axes parallel to the dip of those planes (that is, lineation). Leith, in his Ph.D. dissertation (1905) and textbook (1912), accorded with Van Hise and such earlier authors as Sorby (1853), Heim (1878), and Harker (1885) in distinguishing two types of cleavage, which Leith supported with a wealth of new microscopic data. He named them *flow cleavage,* having fine mineral grains oriented within the planes by rotation and recrystallization during rock flowage, and *fracture cleavage,* being a very closely spaced jointing or fissility lacking any parallel arrangement of mineral grains; relative degree of plasticity (or ductility) was considered the controlling genetic factor that differentiated them. Although both types had similar geometric relations to other structures and to each other, even grading into one another, flow cleavage was considered to be more pervasive (or penetrative). Like Rogers and others before, the Wisconsin workers envisioned a continuum of increasing dynamic metamorphism from slaty cleavage to schistosity and, in some cases, even to a gneissic texture.

The recognition that slaty cleavage and so-called drag or parasitic folds bear a systematic geometric relationship to larger structures was the principal contribution of the American geologists working in the Lake Superior region between 1880 and 1910. The complex Precambrian rocks of the Lake Superior region are so obscured by glacial deposits and vegetation that the early geologists were forced to learn how to use small-scale features visible in scattered outcrops in order to infer the large-scale structures, which were generally not visible, but which they knew must hold the key to an overall understanding of the region. Their applications in structural geology were taught routinely at Wisconsin by the turn of the century and were made available to a wide audience through Leith’s textbook in 1913.

It is difficult to trace to specific individuals the first recognition of each clue, but it is clear that these insights did emerge from the Lake Superior Division based at Madison. For example, hearsay gave former State Geologist William O. Hotchkiss credit for first recognizing the value of cross-bedding and graded bedding for determining the upward-facing direction in vertical or overturned strata. After much investigation of this elusive rumor, I finally found confirmation in his field notes and correspondence from the Florence District in June 1910 (Hotchkiss Notebook No. 1, June 21, 1910, p. 4, on file at the Wisconsin Geological and Natural History Survey and letter to C.R. Van Hise, June 26, 1910, in the C.K. Leith archive, Correspondence Box 37, at the Steenbock Library, University of Wisconsin–Madison). More details about the tangled history of these criteria are presented in another article (Dott, 2001).

Wilson himself refined and expanded the Wisconsin concepts to elaborate such things as lineations and the mechanics of thrust faulting. In so doing, he became the postwar master of small-scale structural analysis (see Wilson, 1961; Wilson and Cosgrove, 1982). Recapitulating his own intellectual ancestry, Wilson influenced a number of brilliant students, who themselves carried on the Wilsonian revolution in the detailed structural analysis that had its roots here in Wisconsin. One of Wilson's protégés was John G. Ramsay, whose publications are well known today in structural circles (for example, Ramsay and Huber, 1983). Ramsay pioneered especially the quantitative analysis of strain in ductilely deformed rocks as exemplified in the Scottish Highlands and the Helvetic nappes of the Swiss Alps. He succeeded Wilson at Imperial College, but later moved to the Geologisches Institut in Zurich. Another Wilson protégé was M.R.W. Johnson, who joined the faculty of the University of Edinburgh. One of Johnson's students was I.W.D. Dalziel, who joined the Wisconsin faculty from 1963 to 1966, thus closing the genealogical circle begun with Wilson's 1925–26 studies at Madison. To underscore the coincidence, Dalziel co-authored the *Geology of the Baraboo District* (Dalziel and Dott, 1970), which brought a modernized structural analysis back to one of the classic areas where it had been spawned nearly 100 years earlier and where Gilbert Wilson himself must have been instructed nearly half a century before.

CONCLUSIONS

The prominence of geology in the early history of Wisconsin was a consequence of the presence of important metallic ores and of a few exceptionally talented geologists just as American industrialization began to accelerate. But why did an unusually reputable School of Precambrian Geology develop here rather than, say, in Minnesota or Michigan? The explanation seems to be that the outstanding results of the Second Wisconsin Geological Survey of 1873–79 caught national attention and immediately increased the stature of Irving and Chamberlin. Moreover, both men had established connections with the USGS during the Tenth Census, and the Survey chose Irving as the most promising candidate to direct a thorough investigation of the iron-bearing Precambrian rocks of the Lake Superior region.

The establishment of the federal Lake Superior Division office at Madison in 1882 provided the opportunity for Irving and his protégé, Van Hise to launch an ambitious, well supported, and sustained research program. With an army of geologists at their command, they were able to launch a 30-year integrated investigation of all key areas of Precambrian rocks on the American side of Lake Superior. This was big science long before the Era of Big Science! Both men were of exceptional ability, and in Van Hise's protégé, Leith, comparable intellectual and organizational skills were recognized early, thus assuring the smooth passage of the torch from Van Hise when he assumed the presidency of the university in 1903. Both Van Hise and Leith left most of the details of the region to their geological foot soldiers and concentrated their own attentions upon the structural and metamorphic aspects of Precambrian rocks. They developed fundamental concepts that were to provide the foundations for the subsequent development of both subdisciplines throughout the twentieth century.

Because of the notoriety of the government publications on the iron districts, which emanated from the Precambrian Division at Madison, and more topical publications on structural and metamorphic geology, the Wisconsin department had achieved an enviable reputation already by 1910. The addition to the faculty of Leith's protégé, Mead, completed one of the most remarkable intellectual genealogies in the history of geology (fig. 7). Mead's special quantitative and experimental talents helped the reputation continue to grow and to attract students from abroad. Through Gilbert Wilson, Wisconsin structural concepts were carried across the Atlantic to be spread even farther like ripples on a pond as, in turn, Wilson's own intellectual children and grandchildren have propagated them all around the globe.

Intellectual dynasties, like political ones, tend to stagnate eventually. During the 1920s, Leith became more and more distracted with his commercial consulting and governmental advising activities; significantly, 1920 was the last year that he taught full time. Nonetheless, he continued to hold the reins of the department firmly, making practically all decisions without consultation with other faculty members. He also attempted to have his son, Andrew, appointed as his successor as department head, but this blatant nepotism was thwarted by a faculty coup in 1934 when C.K. was out of town (Bailey, 1981). He continued to hold his faculty position until mandatory retirement in 1945, however, even though he resided in Washington, D.C., most of the time. As a consequence, his specialties of structural and metamorphic geology languished while other specialties, such as sedimentary geology, forged ahead. It was not until the 1960s that Leith's specialties finally began to recover.

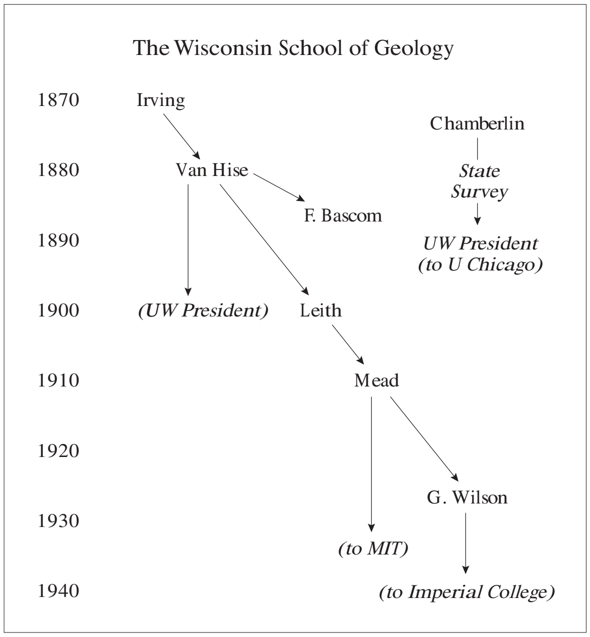

Figure 7. *A genealogical chart for the Wisconsin School of Precambrian Geology from 1870 to 1940. Besides the faculty succession from Irving to Mead, two unique graduate students, Florence Bascom and Gilbert Wilson, are shown. T.C. Chamberlin, although not directly linked to the Precambrian School, was nonetheless an important catalyst for the development of geology at the University of Wisconsin, even after he departed to the University of Chicago.*

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BECOMING A GEOLOGIST: FLORENCE BASCOM IN WISCONSIN, 1874–1887

Lois B. Arnold[1]

ABSTRACT

Florence Bascom (1862–1945) was a petrologist and field geologist at Bryn Mawr College who provided a basic description and interpretation of major areas of Pennsylvania and surrounding regions. This paper is the first of a two-part study that explores the question of how Bascom became a geologist. The initial phase of this process took place in Wisconsin, to which she moved at the age of twelve when her father became president of the University at Madison. Both of her parents supported women's education, rights, and suffrage.

Bascom graduated from the coeducational university in 1882 and, in a series of fits and starts in which scientific study alternated with social pleasures and non-scientific pursuits, she began to take a serious interest in geology. In 1883–84 she studied under two well known geologists there, Roland D. Irving and his student Charles R. Van Hise, obtaining a B.S. degree. Although she did not participate in field work, she conducted laboratory research on the gabbros of Lake Superior using the petrographic microscope and thin sections. She obtained a Master's degree from the University in 1887 on the basis of this investigation.

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Figure 1. *Florence Bascom in the field while professor of geology at Bryn Mawr College. (Photograph courtesy Bryn Mawr College Archives.)*

SEQUENCE STRATIGRAPHY AT THE TURN OF THE CENTURY: E.O. ULRICH'S OZARKIAN SYSTEM IN WISCONSIN

Charles W. Byers[1]

ABSTRACT

Edward Oscar Ulrich (1857–1944) was a major force in North American paleontology and stratigraphy, especially of the Paleozoic Era. Ulrich was immersed in correlation problems for many years; he came to believe that although fossils could tell the age of given strata, they were unsuitable for defining boundaries in the stratigraphic record. Instead, he advocated the use of erosional breaks—subaerial unconformities and transgressive surfaces—as natural and unambiguous boundaries for chronostratigraphic units, anticipating the heyday of sequence stratigraphy by many decades. His 1911 article Revision of the Paleozoic Systems *defined two extra systems, the Ozarkian and the Canadian, as lying between the Cambrian and the Ordovician. According to Ulrich, although Ozarkian strata are thin in Wisconsin, it is here that the lower boundary of the system is best developed as a basal conglomerate (Devils Lake Sandstone) exposed in the vicinity of the Baraboo Range. The ultimate failure of the Ozarkian to survive as a system was based on Ulrich's miscorrelation of unconformities. His inability to determine the exact age of erosion surfaces led him to accumulate too many boundaries (and hence too many chronostratigraphic units) in his composite stratigraphic column. Sequence stratigraphers face exactly the same problem today.*

INTRODUCTION

Edward Oscar Ulrich (shown in his later years in fig. 1) began his career as a paleontologist in 1864 at the age of seven, when he first saw fossils in the rocks of his native Cincinnati (Bassler, 1945). As an amateur and later as an employee of various state surveys and eventually the U.S. Geological Survey, he collected and classified huge numbers of Paleozoic marine invertebrates. He focused especially on groups that required painstaking attention to detail, such as the bryozoans, sponges, and ostracodes, but he also published extensively on mollusks and trilobites. His taxonomic expertise was put to use in biostratigraphy because the state surveys subdivided the geologic systems in North America primarily on the basis of fossils. Ulrich believed that fossil species should be established on the basis of very slight morphological variations—that is, he was a splitter—and that these species could be used to correlate stratigraphic units of very short time duration.

However, he recognized that there were problems with biostratigraphic correlation, especially the problem of defining the boundaries of stratigraphic units. Fossils might give the age of an interval, but their uneven distribution made them poor markers for the interval's base and top. So he turned to another method of subdivision, using erosional breaks in the stratigraphic record, what we now call sequence stratigraphy. In his own words:

> *In short, we require something that will supply the deficiencies of the purely paleontologic and lithologic methods, and thus assure greater definiteness in the delimitation of stratigraphic and time units. The means is at hand. It lies among those criteria of diastrophism . . . indicating alternate advance and retreat—displacement—of the strand line (Ulrich, 1916, p. 468).*

> *Concisely stated, the method followed is to divide the stratigraphic sequence at the first plane beneath the introduction of a new fauna or beneath a marked faunal change that exhibits evidence of diastrophic movements (Ulrich, 1911).*

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Figure 1. *E.O. Ulrich taking notes in the field (Wisconsin Geological and Natural History Survey photograph 459).*

These stratigraphic breaks—subaerial unconformities and transgressive surfaces—provided unambiguous natural boundaries and allowed Ulrich to revise the entire Paleozoic stratigraphic column. This revision (Ulrich, 1911) is 400 pages long, and yet it is incomplete; the detailed stratigraphic descriptions reach upward only to the Lower Ordovician!

OZARKIAN AND CANADIAN SYSTEMS

Ulrich's revision added two new stratigraphic systems between the Cambrian and the Ordovician. In the Appalachians and much of the midcontinent, the Cambrian–Ordovician boundary is obscure, lying within a thick sequence of carbonates that are not very fossiliferous. Ulrich believed that these poorly fossiliferous carbonates encompassed two geologic systems, lying above the fossiliferous Cambrian rocks and below similarly well defined Ordovician rocks. He named the new systems the Ozarkian and the Canadian (Weiss and Yochelson, 1995).

Table 1. *Ulrich's revised Lower Paleozoic.*

System	Formations	Type area
	Bellefonte	Pennsylvania
CANADIAN	Axeman	Pennsylvania
	Nittany	Pennsylvania
	Stonehenge	Pennsylvania
	Jefferson City	Missouri
	Roubidoux	Missouri
	Gasconade	Missouri
OZARKIAN	Proctor	Missouri
	Copper Ridge	Tennessee
	Potsdam	New York
	Potosi	Missouri
	Ketona	Alabama
	Briarfield	Alabama
	Nolichucky	Tennessee
	Maryville	Tennessee
CAMBRIAN	Rogersville	Tennessee
	Rutledge	Tennessee
	Russell	Virginia

The type formations for the new systems are shown in table 1. Notice that the Canadian units are all from Pennsylvania, but the Ozarkian formations are from Missouri, New York, and the southern Appalachians. We now know that the upper Ozarkian formations in Missouri are equivalent to the Canadian formations in Pennsylvania, and that both sets of units are of Early Ordovician age. They are lateral equivalents, but Ulrich stacked them vertically instead. Likewise, the lower Ozarkian units are actually Late Cambrian in age; Ulrich thought they were younger than that.

In summary, all the rocks assigned to the Ozarkian and Canadian are either Late Cambrian or Early Ordovician. Later geologists used "Canadian" as a series name, equivalent to Lower Ordovician, rather than as a full-fledged system as Ulrich proposed. But the Ozarkian, its component formations all reassigned, completely disappeared as a stratigraphic name at any level. In the words of Weiss and Yochelson (1995), it is a system "gone and nearly forgotten."

Table 2. *Comparison of current classification of Wisconsin Cambrian units with Ulrich's classification.*

Present column	**Ulrich (1924)**	
(Cambrian formations from Ostrom, 1967)	WESTERN WISCONSIN (Cambrian formations)	MADISON–BARABOO REGION (Cambrian and Ozarkian formations)
Jordan Formation	Jordan Sandstone Norwalk Sandstone	Madison Sandstone
St. Lawrence Formation	Lodi Shale St. Lawrence Limestone	 Mendota Dolomite Devils Lake Sandstone
		unconformity
Tunnel City Group	Franconia Formation	Mazomanie Sandstone
Wonewoc Formation	Dresbach Sandstone	Dresbach Sandstone
Eau Claire Formation	Eau Claire Shale	Eau Claire Shale
Mount Simon Formation	Mount Simon Sandstone	Mount Simon Sandstone

ULRICH AND WISCONSIN

What does all this have to do with Wisconsin? Ulrich spent time in the field here, and he recognized both of his new systems in the local stratigraphy. Table 2 compares the presently accepted stratigraphic column for the Cambrian formations in Wisconsin (Ostrom, 1967) with Ulrich's column, published in 1924 (also see Dunbar and Rodgers, 1957, p. 286). The formation terminology Ulrich used for western Wisconsin is similar to the present-day column. The Dresbach Sandstone has changed to "Wonewoc," and the Franconia and Mazomanie Formations (both glauconite-bearing sandstones) have been combined into the Tunnel City Group. Also, the Lodi Shale is recognized as a facies (member) of the St. Lawrence Formation, and the Norwalk as a member of the Jordan.

In the Madison–Baraboo area of south-central Wisconsin, Ulrich's column is distinctly different. He believed that the St. Lawrence, Lodi, Norwalk, and Jordan were missing because of post-Cambrian erosion. The unconformity was overlain by supposed Ozarkian strata: the Devils Lake Sandstone, Mendota Dolomite, and Madison Sandstone. As table 2 indicates, it is now understood that the Mendota Dolomite is simply a synonym for the St. Lawrence and the Madison Sandstone is really the Jordan. But Ulrich refused to accept them as Cambrian units; instead, he put them in the Ozarkian. Ulrich's paleogeographic scenario was as follows: In latest Cambrian time the St. Lawrence and Jordan were deposited across Wisconsin. Then in the Madison–Baraboo region only, the formations were eroded. The resulting erosion valley was filled in during Ozarkian time by a set of units that included two formations (Mendota and Madison) that mimicked amazingly the St. Lawrence and Jordan in lithology, thickness, and fossil content (Ulrich, 1916). Ulrich was driven to these stratigraphic gymnastics because of his interpretation of the formation that lay below the Mendota—the Devils Lake Sandstone, a conglomerate as much as 100 feet thick, exposed near Baraboo. That conglomerate is spectacular, with boulders of purple Precambrian Baraboo Quartzite enclosed in white sandstone. Ulrich reasoned that a basal conglomerate this thick must overlie a major unconformity, representing a huge break in deposition, one big enough to form the base of a whole geologic system, namely the Ozarkian: "the Devils Lake sandstone . . . affords a more impressive development and display of conglomerate than has been observed at this horizon anywhere else in America" (Ulrich, 1924, p. 104) and "the abundance and character of the conglomerates . . . prove to be the best objective evidence we have in establishing the verity of the break between the Cambrian and the Ozarkian" (Ulrich, 1924, p. 105).

So, even though Ulrich's Ozarkian System in Wisconsin was much thinner than in the Appalachians, its basal contact was remarkable, the best he had ever seen. Given that the Devils Lake was Ozarkian in age, the formations above it, the Mendota and the Madison, must also be Ozarkian.

Today we know that the "Devils Lake" conglomerates are facies developed in several Cambrian sandstone formations in the vicinity of the Baraboo ridges, which stood as islands in the Late Cambrian sea. During the Cambrian transgression, as the sea gradually inundated the weathered Precambrian surface, Baraboo Quartzite clasts were eroded from the islands and deposited nearby in the basal transgressive sands (Dalziel and Dott, 1970).

Where did Ulrich go wrong? The problem was, and is, that although unconformities provide unambiguous surfaces for boundaries, their ages are difficult to establish, so it is hard to correlate with them. One erosion surface looks much like another, so matching them in different locations is fraught with pitfalls.

Ulrich gave us a beautiful example of this in his 1916 discussion of field work near the town of Rock Springs (then called Ablemans), on the north limb of the Baraboo syncline. First he described an exposure of the Ozarkian contact, with its basal conglomerate, the Devils Lake Sandstone, full of boulders of Baraboo Quartzite. Here the unconformity at the base of the Devils Lake cuts into Dresbachian sandstone in the lower part of the Upper Cambrian. The middle and upper Upper Cambrian formations were absent, according to Ulrich: "the identifications . . . necessitated the assumptions that the Cambrian formations normally intervening . . . the Franconia sandstone, the Saint Lawrence formation, and the Jordan sandstone—were absent here either through nondeposition or pre-Ozarkian erosion" (Ulrich, 1916, p. 459).

Ulrich then discussed a second outcrop in a large quarry nearby. Here, there is an exposure of sandstone that Ulrich had previously thought to be Dresbachian (early Late Cambrian). After making this assessment, Ulrich had received a report during the winter that Franconian brachiopods had been collected from the exposure; that would indicate a middle Late Cambrian age for the rock. Ulrich hurried back to Wisconsin the next June to see for himself. Here is his description of the discovery:

> *The quarry in which the Franconia fossils had been found was the first to be visited. Its face exceeds 100 feet in height. The greater lower part of this quarry face, on close examination, again seemed to me surely Dresbach. Then it was learned that the fossiliferous bed is near the top of the quarry. . . . Just beneath the fossil bed a suspicious contact was observed, which, on being traced around the quarry, proved to be irregular and at one place very much so. Moreover, touching or lying on this uneven plane we found boulders of Baraboo quartzite, moderately rounded and up to five feet in diameter. Finally the sandstone for 10 to 15 feet above this contact was shown to be thinner-bedded and less silicified than is the more massive sandstone beneath it. Evidently the two belong to distinct formations. And thus we proved that an exposure of sandstone which until then had always been regarded as belonging to a single formation in reality contains adjoining parts of two unconformable formations (Ulrich, 1916, p. 459–460).*

Ulrich's discovery anticipated a major insight of modern sequence stratigraphy, that sequence boundaries can be cryptic, lying *within* apparently homogeneous lithologic units. For example, the top of a stratigraphic sequence (highstand systems tract) is likely to include shallow marine sandstone. The base of the overlying sequence (transgressive systems tract) may also consist of shallow water sandstone. The result is sand on sand, and the two units may be mapped lithostratigraphically as a single sandstone formation. The actual subaerial erosion surface separating the two sequences lies somewhere within the formation and may be hard to recognize. Ulrich had figured this out more than 80 years ago. Although he boasted in his 1916 article that it took him little more than an hour to find the unconformity, and his description makes it sound obvious, he points out in his conclusion that no one had ever noticed it before.

Unfortunately, Ulrich's miscorrelations, taxonomic oversplitting, and inability to recognize facies changes caused most of his stratigraphic work to fall into disrepute after the 1940s. Modern sequence stratigraphy was developed independently, beginning in the 1970s, and some of Ulrich's concepts have since been reinvented.

CONCLUSIONS

To review, near Rock Springs Ulrich saw two outcrops with erosion surfaces cut into the Dresbach Sandstone: At one locality the overlying sandstone contains abundant conglomerate and was interpreted to be Ozarkian in age. At the other locality, the overlying sandstone is less conglomeratic, but also contains Franconian fossils and was interpreted to be Cambrian.

In hindsight, it is clear that the erosion surface is the same one in both exposures. It is Cambrian, the contact separating the Wonewoc (Dresbach) from the overlying Tunnel City (Franconia). Because fossils were absent at one of the localities, Ulrich had no biostratigraphic evidence for the age of the erosion surface, and he relied on abundance of conglomerate instead. In this instance Ulrich was unable to match an unconformity correctly between two closely spaced outcrops. It is no mystery that he failed to get it right in correlating between Missouri and the Appalachians.

Sequence stratigraphers face exactly the same problem today. The standard Exxon sea-level curve shows scores of sea-level fluctuations, recurring on a scale of a million years or less. Given that the timing of sea-level change lies at or below the resolution of standard biostratigraphy and geochronometry, correlation by matching surfaces is open to the same errors made by Ulrich.

Andrew Miall pointed out the difficulty of correlation by unconformity-matching in his 1992 paper. Miall constructed four synthetic columns of unconformities using random numbers. He was able to match these randomly positioned surfaces to the Exxon column at a *minimum* of 77 percent of occurrences, using a resolution of plus-or-minus one million years. Miall's conclusion was that wherever one needs an unconformity for correlation, one will be available, but most likely it will be the wrong one. As the stratigraphic record is reinterpreted in the light of modern sequence stratigraphy, geologists need to be careful not to walk again down the path that Ulrich took.

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Frederick William Sardeson

Malcolm P. Weiss[1]

INTRODUCTION

Frederick W. Sardeson is an interesting figure in geology: bright, innovative, combative, productive, stubborn, arrogant, conceited, the victim of a changing university to which he was unable to adapt, and later a professional pariah mostly not of his own making. Considering the amount and value of the geologic work that he did, he was treated shabbily during his lifetime. The mainstream marked him then as cranky and unorthodox, and he has been largely ignored in succeeding decades—except by workers on the Ordovician rocks of the Upper Mississippi Valley.

Sardeson was born February 22, 1866, at Owego Mills, a grist mill on Whiteside Creek, near the village of Argyle in southwestern Wisconsin. He was the middle of five children of Joseph Sardeson and Petra Rossing. His father was of a family of millers that had immigrated from Lincolnshire, England, in the mid-nineteenth century. His mother was of a family of farmers in Lafayette County who also had immigrated in mid-century, but from Norway. Sardeson lived at the mill for only a few years because his family moved to Argyle, where the father and an uncle engaged in several businesses. Sardeson spent his youth in Argyle until the age of seventeen, when he enrolled in the Augsburg College and Theological Seminary (where an older brother was already a student) in Minneapolis, Minnesota—apparently because his mother wanted him to have a Lutheran education.

Sardeson did not like Augsburg and, when his family moved to Minneapolis in 1886, he transferred to the University of Minnesota. There he took mathematics instead of Greek, read Lyell's *Principles,* enjoyed N.H. Winchell's natural history museum, and collected fossils and agates. In the fall of 1887 he was enticed into geology by Christopher W. Hall, head of the Department of Geology and Mineralogy. Hall soon had Sardeson tending to fossil specimens, arranging exchanges with other institutions, and collecting in the field for C.D. Walcott of the U.S. Geological Survey (USGS) and E.O. Ulrich of the Minnesota Geological and Natural History Survey (the Winchell Survey). Sardeson (fig. 1) began to catch up on other sciences and geology and abandoned his earlier goal of becoming a lawyer. Even before graduating, he had begun his innovative work on the classification of the Middle Ordovician beds of Minnesota and Wisconsin. He graduated B. Lit. from the university on June 4, 1891, and was elected to Phi Beta Kappa.

Figure 1. *Frederick W. Sardeson as an undergraduate student of geology at the University of Minnesota, 1890, aged 24 years.*

At Hall's urging he began graduate work in the fall of 1891, and published articles as junior author to Hall. He prepared two short and two longer papers during 1891–92 for his master's thesis; they were published as Palaeontological Papers (Sardeson, 1892a, b, c, d) because the sort of thesis "book" that we know today was not required at that time. The two shorter papers are forgettable, but the others are of special interest. In 1892c Sardeson laid out his original classification of the Middle Ordovician beds of the Upper Mississippi Valley (of which more later); the concept and the work had been in preparation while he was still an undergraduate, as demonstrated by his joint publications with Hall in 1892. In 1892d he described

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the faunas associated with the several stratigraphic units that he erected in 1892c. That report included a number of descriptions of new species—many of them brachiopods—several names of which were "stolen" from him by the trickery of E.O. Ulrich for N.H. Winchell and for Charles Schuchert of the New York State Museum (Weiss, 1997).

Sardeson was awarded a master's degree on June 2, 1892, and was elected to fellowship in The Geological Society of America in December of that year. He stayed in the department with a scholarship of $350 (for 10 months) until 1894. The department at Minnesota had never awarded a Ph.D. degree (the first was not until 1897), and none of the teachers in the department—which included botany and zoology—had a doctoral degree. Sardeson's committee found unspecified deficiencies in him, but it was also reluctant to grant what its own members lacked. So he was turned down in the spring of 1894 for what we would today call "candidacy." Some members of the committee urged him to go to the University of Chicago for his Ph.D., but the president of the University of Minnesota advised him against it. In that event, he inquired of a German university, was encouraged, and went there that summer.

Early in his stay at the Albert–Ludwigs–Universität in Freiburg (fig. 2), Sardeson was tested in field work by some professors, and had to undergo a special examination to judge his qualifications for working toward the doctoral degree in only one school year (about 12 months, apparently). He passed both trials satisfactorily and graduated *multa cum laude*—the second highest of the four grades awarded at Freiburg—in 1895. At least two of his mentors were distinguished and widely known: Professors Weismann for zoology and Steinmann for paleontology and glacial and structural geology. Sardeson's dissertation topic—the relation of the tabulate corals to the Alcyonaria—was assigned by Steinmann. The work convinced Sardeson that the primitive bryozoans were corals; this view was not unusual at the time, but he stuck to it for the rest of his life, as we shall see.

Figure 2. *Sardeson as a graduate student at the Albert–Ludwigs–Universität, Freiburg, Germany, 1894–95.*

TEACHING AT THE UNIVERSITY OF MINNESOTA

Sardeson returned to Minneapolis in the fall of 1895, having been granted his doctoral degree provisionally; to confirm the degree, Freiburg required that the dissertation be published within a year of the defense—as it was, the following May (Sardeson, 1896). In November of 1896 he was appointed "scholar in paleontology" by the University of Minnesota, for $250 per year (10 months), which was less than he had been paid after his master's degree! He began immediately to work on the stratigraphy and paleontology of the Cambrian and Ordovician rocks of Minnesota and Wisconsin and some Pleistocene deposits as well. That work resulted in a flow of publications over the ensuing decade.

Sardeson continued as "scholar," with some increase in pay, until June of 1898, when he was appointed instructor in the department with a salary of $500. What teaching he may have done for Hall between November of 1895 and June of 1898 is not clear from the record. Because Hall always put Sardeson forward, it is likely that the latter did some teaching in addition to his own research and caring for the collections. He was promoted to assistant professor in July of 1905, the grade at which he remained until leaving the university. At that time a common academic table of organization had a professor at the top, and several lesser types to fill out the pyramid and do much of the work (the rank of associate professor was not known). The regents of the university had little interest in paleontology because they considered that the effort and money the Winchell Survey (a part of the university) had spent on fossils was largely wasted. Despite that bias, Sardeson received several raises in pay while in the department. His rank of assistant professor was honorable and satisfactory for one with a Ph.D. degree.

During Sardeson's tenure at Minnesota his salary was considered to be half pay. That may have been true of all in the department except Hall, who was dean of the School of Mines, Metallurgy and Mechanic Arts in addition to being head of Geology and Mineralogy. The Winchell Survey was winding down in the late 1890s and ceased on October 1, 1900, and the current Minnesota Geological Survey was not established until 1911. So for perhaps 15 years members of the teaching department were expected to offer consultation to the state, cities, villages, businesses, or individuals—for expenses and sometimes pay. Sardeson, for example, worked for the Chicago Great Western Railroad during the summers of 1896 and 1897. During his years in the department (fig. 3), Sardeson also did the sort of "departmental service" with which we are familiar today: public lectures on evolution or paleontology and field trips to sites of special geologic interest.

Figure 3. *Sardeson on the Minnesota faculty, 1898.*

Sardeson had very strong views on education and teaching; some he had learned from Hall and some he had adopted in contrast to Hall's methods. He believed that education and training are quite different: The first leads to logical thought, skill in comprehension, and the ability to think originally; the latter is for efficiency and habituation, and therefore constrains thinking along the line of the training rather than broadly. He believed that geology and paleontology were very valuable educationally to everyone, but of professional use to only a few. He considered them complementary to history and philosophy. In later years, when waves of students were prepared for careers in petroleum and economic geology, he believed that colleges and universities had forsaken education in favor of training.

Such views carried over into Sardeson's teaching. He taught a variety of courses in paleontology and stratigraphy, always emphasizing historical geology and organic evolution. Some classes were for geology or engineering majors, but apparently most were for graduate students (of geology, botany, and zoology) and for non-geology undergraduate majors who elected his classes. Not interested in "training," he began his courses with many details to show a principle and told them to forget the details; if they had trouble in the early weeks, he encouraged them to change registration. In addition, he recorded the array of science courses that his class members (undergraduates, apparently) had had, tailored oral exams to their degree of preparation, and judged written tests in that same light. Colleagues from those days told that Sardeson didn't care whether students learned the material or not; what they got was up to them. This certainly fits with his scorn for training and the belief that the university should not be in the training business. Oh, if he could but see the higher education of today!

Sardeson was himself very proud of his teaching, and said that the president once recognized the excellence of his teaching. Over many later years he needled the university administration about the deterioration of its geology program. He scorned especially the bachelors of geology who were "trained" to be oil geologists, and the General College that the University of Minnesota developed in the 1920s for students with weak preparation and no clear goals.

Sardeson once made a gift of fossils to the university—surely a valuable thing, for he was a particularly skilled collector of fossils. He turned down a call to the University of Washington about 1900. His loyalty to his own institution showed also in his departmental service, his frequent attendance at professional meetings, and his busy and productive research program. But his loyalty, productivity, and worthy principles and pedagogic practices did not protect him from new academic developments not to his liking. The University of Minnesota had set a major new direction under President George Edgar Vincent, who was new in 1911. Service to the state and more rigorous requirements for research and teaching reached across the university. For geology the changes included a new state survey as part of the department, and a strong effort in economic geology and petrology. These were features that the regents had looked forward to for some time; in lieu of more paleontology, they wanted the department to be more practical—to do things that would yield money, for the practitioners if not for the university. Sardeson was not interested in either, because these new features seemed to him to smell more of training than of education.

Despite his private hope in 1910–11 that he might succeed Hall as department head, and his confidence that a new president would improve the university, Sardeson very soon found the new trends in the department constraining and not to his taste. His teaching load was increased and he was expected to participate in the new thrust of the "new" department. This he was unwilling—perhaps unable—to do, and he was forced out two years later.

DISMISSAL FROM THE UNIVERSITY

The main instrument of Sardeson's downfall was William Harvey Emmons, hired from the USGS and the University of Chicago by President Vincent to be head of the teaching department and director of the state survey, at a huge salary (half as large as Vincent's). At the same time, the name of each entity was shortened, to Department of Geology and Minnesota Geological Survey (MGS). Emmons was an economic geologist with no interest in fossils, but eager to study and develop the mineral resources of the state. He was encouraged and aided in the Sardeson affair by Frank F. Grout, a petrologist and member of the department since 1907, who disliked Sardeson. Grout had hoped for a new state survey and was pleased to join Emmons in its development; he specifically requested that Emmons keep Sardeson out of it.

Hiring Emmons was made easy by Hall's illness in 1910 and his death in the spring of 1911, just before Vincent took office, so Sardeson no longer had a devoted friend as department head. President Northrup, also Sardeson's friend, of course had departed when Vincent came. Probably Emmons and Vincent held no antipathy toward Sardeson when they arrived, but Emmons represented and was interested in exactly the things for which Sardeson then cared nothing: economic geology, practical matters, and training. Evidently Sardeson was unaware—or cared not—that practical, professionally or vocationally oriented training was burgeoning nationwide, particularly in the public universities. He set his mind against this trend, and his opposition contributed to his downfall.

Starting in 1909 and 1910, Sardeson had joined Frank Leverett of the USGS in the study of the Pleistocene deposits of Minnesota. He was employed directly by the USGS for part of the work; for the cooperative USGS–MGS projects he was paid with USGS funds granted to the MGS, an element of the university. Emmons had assured Sardeson of his support for such projects; because of his position as director of the MGS and his contacts in Washington, Emmons surely could have followed through on that. Sardeson's projects ultimately yielded two USGS folios and a map and bulletins on the surficial deposits of the state. It seems possible that Emmons believed that Sardeson's outside income made it easier for him to fire Sardeson.

Emmons got his chance to act when the dean invited him in early April of 1913 to name any persons in the department who "should be discontinued." Emmons named Sardeson and one other, and the proposal started its way through channels. The dean hoped for action by the Board of Regents that spring, but the president didn't respond until mid-June. The slowness of the process was unfortunate, but it is clear from the record that the dean and his advisory committee joined Emmons in wishing Sardeson to be gone. Realizing that it was a serious matter to fire an established professor with a good record of teaching and research, President Vincent called a meeting with Sardeson, the dean, and Emmons on Saturday, July 5th. There is no record of what transpired at that meeting, but Vincent decided to call another for Sunday evening with the whole geology faculty and the dean.

That July 6th meeting must have been stressful for everybody. Six of the seven other members of the department were present, and Sardeson was flayed badly by them for his arrogant and conceited personality and habits: "extraordinary egotism," "bitter and sarcastic tongue," belittling of others, speaking contemptuously of colleagues to other colleagues, and carrying tales of department business and colleagues to friends in other departments. No one attacked his teaching record, his professional competence, or his research ability and productivity; the strong dislikes were all personal. In his record of the July 6th meeting, Vincent explained that Sardeson's defense was that his early history in the department had been "unfortunate" and that "the world was against him."

No details were given, and nothing in Sardeson's records of those early years seem now to justify such paranoia. Vincent offered Sardeson the opportunity to appear before the regents to explain himself, but Sardeson asked only that Vincent convey to them that he wished the opportunity to show that he could get along with his colleagues. He also asked that Vincent interview three of Sardeson's good friends and neighbors (all in the Physics Department). After Sardeson left the meeting, Vincent asked the geologists to reconsider whether Sardeson might mend his ways and be acceptable to them, but they remained opposed.

Vincent interviewed the three physicists on Monday. They had for some time deplored Sardeson's "habits of detraction and sarcasm," and had repeatedly warned him against it. One of the three suggested that the other seven geologists be fired and that Sardeson be kept—for his abilities, his long tenure, and his knowledge of the geology of Minnesota. That same man would not concur in the dismissal, but he offered no feasible solution. On Tuesday, now the 8th of July, Vincent consulted with a former regent who was a mining engineer. The latter recognized the seriousness of the loss of Sardeson's skills and experience, but concluded that releasing him was the only way to promote economic geology and to support Emmons in that quest.

For the Wednesday morning meeting of the regents (July 9th), Vincent prepared a brief memorandum in which he recommended that Sardeson be granted leave with full pay for the 1913–14 school year, and that he not be reappointed after June 30, 1914. The regents adopted that policy, and before the end of the day Vincent wrote to Sardeson (who was out in the field) to tell him of the fact. From Saturday to Wednesday, Sardeson's successful career came to a violent end—at age 47. Sardeson never fought back; his daughter believed that he was so hurt that he just buried it deep within him. Minnesota had no tenure policy then, or for decades more, and the American Association of University Professors was not organized until 1915, so there was no institutional help for Sardeson.

Many professors have been fired over the years without due process and with little opportunity to fight back. Religious or political heresy, being too liberal politically, teaching or believing in organic evolution, objecting to a war, or not supporting one with the requisite vigor, have all been popular reasons. A review of a number of published cases shows that such attacks have often been turned aside if the victim were supported by colleagues and sound argument. A few, wherein the victim had an objectionable personality and no support from colleagues, were discharged for other alleged causes. No case that has been described in the literature is like Sardeson's—dismissed merely and only because his colleagues didn't like him.

It seems that our man was destroyed and that no more need be said, but we have not taken account of his geologic work, done both before and after he left the university. Following his education and his own interests, Sardeson's contributions to geology fall into three great groups: paleontology, stratigraphy, and Pleistocene geology. Before dealing with his life outside the university, we'll summarize his contributions in those three fields.

While an academic, Sardeson published results of his investigations in standard journals. A factor that pertains to all of his later work is that Emmons would not give the cachet "Published with the permission of the Director of the Minnesota Geological Survey" to any paper that Sardeson wrote. Hard as it now is to believe, the standard geological journals offered such right of censorship to bureaucratic geologists in the several states. So between 1914 and 1922 Sardeson was unable to publish anything except the work that he did for the USGS or on cooperative USGS–MGS projects. The release from censorship in 1922 came through C.R. Keyes' *Pan-American Geologist*.

Keyes was himself a professional pariah—as Sardeson had now become—and operated a rather strange journal that featured innumerable pieces by Keyes himself, condensed articles from other journals, attacks on other geologists and institutions that offended Keyes, and many maiden abstracts and articles by young persons just getting started. The journal continued until 1942, when Keyes died. During those years it was the fashion of establishment geologists to ignore the journal for its unorthodoxy and its "cult of [Keyes'] personality." The *Pan-American Geologist* contained some good work, from many authors—but Sardeson, who had no other outlet, suffered from the scorn accorded the journal itself. Keyes provided no peer review, did not allow headings, sometimes rearranged figures (always of the simplest), and sent out no galley proofs. Against these disadvantages Sardeson, who often needed a strong editor, published 89 papers in 18 years. There is much good and some innovation in most of them; only a few are trivial.

PALEONTOLOGY

Sardeson made important contributions in paleontology in four major ways: superior collections, descriptions of species rather than specimens, anatomy, and ecology. He named a number of species (but no genera) early in his career, but in contrast to most workers trained in the nineteenth century he did not make a habit of it. A common thread that ran through all his work was an interest in finding evolutionary changes in the *fossils*—not the names—up the stratigraphic column. He was ahead of his time in this and in other ways. In addition, he was always unforgiving of workers who did not adhere to his standards of taxonomy.

Collecting and collections

Sardeson was a skillful and indefatigable collector, and saved and used what he collected. He always made a policy of collecting stratigraphically; the position of each collection in the rock column was recorded in detail. This not only permitted discovery of possible evolutionary changes, but it also provided precise zonation—the association of the faunas with the several layers of rock. Sardeson was not unique for his time in this regard, but he was unusual. E.O. Ulrich, for example, collected stratigraphically also, but the great Charles Schuchert did not—and neither did some of Schuchert's students. Collecting without regard to the various beds in a measured section yields bad paleontology and poor biostratigraphy, and Sardeson was never guilty of that.

Beginning with the work for Hall and during the Hall administration, Sardeson collected for the department. Hall did not like Winchell, and that may have been the reason why Hall requested, about 1896 or 1897, that Sardeson keep the specimens collected for the department in his home; this kept them out of Winchell's little museum and reserved them for the day when the university would build a museum of natural history. Although no such museum was built until the early 1940s, that policy turned out to be a fortunate one. During the year that Sardeson was on terminal leave, the fossil collections in the department (including those he had given to the regents in 1904) disappeared. However, those in his attic did not! Sardeson always believed that Emmons had distributed the departmental collection among Charles Schuchert at Yale, E.O. Ulrich at the USGS, and C.D. Walcott at the Smithsonian Institution. Accession records at those and other possible destinations, such as the University of Chicago, are so poor and disorganized that his suspicion cannot now be confirmed or refuted.

After leaving the university Sardeson continued to collect until the mid-1940s, when he left Minnesota. Thus, what he had collected early for Hall and later for himself was available to be bought by the university for $10,000 when Sardeson left the state in 1947. At that time the collection contained more than 3,400 "sets" of fossils, more than 800 of them European. A set contained from one to 2,500 specimens; many of the species available from the Ordovician rocks of the Upper Mississippi Valley were represented by hundreds or thousands of individuals. The scientific importance of his large collection was the variation within species that was displayed. Also, it permitted him to do paleontologic work over four decades after leaving the university. Further, during Sardeson's lean years in the 1920s he was able to sell a number of specimens and sets—mostly to Schuchert at Yale and to Ray S. Bassler at the U. S. National Museum.

Taxonomy

There are two ways to look at Sardeson and taxonomy: the principles that he urged and used in the naming of species and assignments to genera, and the new names that he gave to fossils. The first is important because of the modernity of his policies; the second is of less interest because—compared to his contemporaries—he did not create a lot of names.

A century ago naming species was almost a game. Honor was accorded to the worker with the most points—specific names. Aside from acts of pride, the defective science that led to so many new names was the fact that so many were given to *specimens* rather than to populations. It was always easier, you see, for workers to pick up one or two fossils and give a name than to collect persistently and massively as Sardeson did in order to discover the population represented. The dwarfed, the underfed, the obese, and the misshapen were also highly likely to be awarded new specific names by others. Sardeson named a number of specimens himself in the 1890s, but thought better of it and mended his ways about the turn of the century. Thereafter he always regretted that he had been so careless in his youth—careless perhaps, but in the mainstream of nineteenth-century practice. It is regrettable, in passing, that so much of that older, unscientific sort of work continued to be done by others well into the twentieth century.

Having changed his policy, Sardeson was ever afterward contemptuous of the many who still did paleontology the old way—the way that also had turned off legislators and university regents. In his papers and in many letters he argued for a populational approach to taxonomy, which considers the ecology and life habits of the fossils, and for avoiding the changing of names at formational boundaries or, worse, at state lines. A few specimens might stand as the types for a name and a population, but they should embody the range of variation in the population that was the species; the published description and illustrations should also represent the variation in the population. Pressing this point to Bassler, Sardeson once wrote in effect, okay, you are a species and I am another. His pleas for better ways to do taxonomy sound much like the views of species and speciation that were widely endorsed just after World War II, but Sardeson wrote about and practiced them years earlier. He put his ideas into practice abundantly in the 1920s and 1930s, mostly by restudying genera from the Ordovician beds of the Wisconsin–Minnesota region. He used his own collections in this work, most of which reduced numerous specific names to only a few. Taking a broad view, one would have to say that Sardeson was rather more of a lumper than a splitter.

Sardeson could be bitterly scornful of bad work of the older sort, particularly in his letters. He noted that Ulrich had beautifully described and illustrated the variation in some species for the Winchell Survey, but had also divided them into several newly named species. Sardeson knew that Ulrich had done so in some cases because Winchell paid only one or two dollars for a description and its lithograph, and wanted to publish and pay for only new species! Sardeson saw also that Ulrich gave new names to growth stages of the same trilobite in his Milwaukee Public Museum Bulletins (with C.E. Resser) of the early 1930s, long after other workers knew better. Troubled by space problems at the U.S. National Museum in February 1933, Bassler wondered to Sardeson what to do with "mutilated, macerated, weathered off, [and] duplicate fossils." Sardeson replied that "they should be saved, for if [August] Foerste does not outlive us they can be mounted in cement for his headstone; then he can get up every night between 12 and 1 and make new species out of them for all eternity!" He then suggested an epitaph for the headstone of that infamous namer of countless specimens.

Here lies Dr. August Foerste
Who never does his worst. He
Takes the fossil pieces;
He makes them into species
And all of them look very thirsty.

In rolle of Augustin[i]us
He shows his aweful genius,
As out of fractured species,
He makes up all the pieces
And calls them each a genus.
(Sardeson, 1933)

Sardeson himself named species of sponges, brachiopods, gastropods, bivalves, cephalopods, and crinoids. Many of the molluscs were from the St. Peter sandstone, from which few specimens have been discovered since. As these groups have yet to be revised in monographs, it is difficult to say whether Sardeson was "right" or not. Some species have survived; others have fallen into obscurity. Probably most would not be named today. Sardeson had better "luck" with the brachiopods, despite the fact that a number of his names were either suppressed by a phony publication (prepared by Ulrich for Winchell and Schuchert) prior to release of his own names or put into synonymy soon after that (Weiss, 1997). Sardeson did distinguished work on cephalopods and crinoids, but that had little to do with nomenclature. He worked also on corals and intensively on bryozoans, but never named species of either. Except for his bryozoan lineages and his work on the synonymy of several groups, Sardeson's taxonomic work was neither abundant nor remarkable.

One matter of taxonomy must be mentioned, although it reveals Sardeson's stubbornness and sensitivity to criticism (but he loved to hand out criticism). During his studies in Germany he followed the lead of his mentor (and others, including the great Karl von Zittel) in believing that the primitive bryozoans were truly corals. The contrary view was developing and gaining supporters just at that time, particularly in America. Sardeson never gave up his youthful conviction and fought a bitter rearguard action in favor of their coralline affinity, even after E.R. Cumings had proved otherwise convincingly in 1912.

Paleobiology

Sardeson's most useful and enduring contributions to the paleontology of the invertebrates are his work on anatomy, ecology, and lineages (evolution). Most of this work was done after he left the university, in his own attic laboratory, with microscope, provisions for thin sections, and so forth. Most of it was thus published in the *Pan-American Geologist* between the world wars. He worked on anatomical problems of horn corals, snails, crinoids, and bryozoans; the two latter are the more important. He worked many times on crinoids, suggested how a holdfast might be transformed into a float, assembled calices out of numerous loose plates, and offered the first published reconstruction of a Paleozoic crinoid. Sardeson suggested how bifoliate bryozoans may have developed from the basal expansion of arborescent forms; his work with bryozoan lineages has already been mentioned.

Sardeson's mature interest in ecology complemented his work on anatomy. He had a persistent interest in how creatures lived and in what environments. The factors that controlled the appearance of individuals—growth stages, pathology, repair of injury, dwarfism, gigantism, geologic distortion, and adaptations to the micro-environment—were fascinating to him, partly because these had to do with the variation in a population and, by extension, with taxonomy. Inquiries of this sort were carried out on corals, bryozoans, cephalopods, bivalves, brachiopods, and crinoids. Among the most interesting results were his finding certain brachiopods that lived attached to the sea floor and his recording of the succession of immigrants that followed a fall of volcanic ash.

In summary, Sardeson was an innovative student of fossils who anticipated many of the aspects of paleontology that are taken for granted today, but practiced by few in his day. He early on had a modern concept of species, and did a lot of good work, even if he was stubborn and sometimes flat wrong.

STRATIGRAPHY

Sardeson always did paleontology and stratigraphy hand-in-hand, and he believed that fossils were the most reliable guide to the historical succession. He never regarded lithostratigrahy as appropriate to science. He thought that biostratigraphy was scientific and that lithostratigraphy was a sort of technique. What he failed to understand was the high degree of subjectivity in paleontology and the relatively much greater objectivity in lithostratigraphy. Over the years he inveighed frequently about poor paleontological work: too many names and many of those given to specimens rather than to species. But he failed to realize the consequences of such poor work—that it resulted in defective zonation and biostratigraphy that had to be done over. With careless paleontology all about, lithostratigraphy made good sense; more than that, it permitted those not well grounded in paleontology to do good stratigraphy and historical geology. That said, and acknowledging that one of his major contributions—on the St. Peter sandstone—had little to do with fossils, one must recognize that he did a lot of stratigraphy of real distinction—including some sedimentary petrology.

Sardeson's most important contribution was the early rationalization of the stratigraphy of the Middle Ordovician beds of Minnesota, Wisconsin, and Iowa. Geologic work in that region began in the mid-1800s and gained intensity in the late 1800s. In those years some rock units were given names from distant regions—New York, Ohio, Tennessee—but without direct comparisons of rocks or fossils having been carried out. "Trenton," for instance, meant different things to different workers in different states and at different times. If a Trenton fauna was recognized accurately in both Minnesota and Wisconsin, but in different rocks, what should be recognized as "Trenton" in Iowa? Work and maps were provincial rather than regional, simply because more years were required to develop a thorough regional view.

Sardeson contributed to a regional view. He had a good idea, employed in preliminary form in his master's thesis (Sardeson, 1892c and d): to avoid using names from the east and to erect a column for the region that expressed the succession of rock types and faunal zones as well as the regional facies changes. Once such was achieved and the fossils accurately known, correlations could be made with any other region in the country or even internationally. His good idea had to wait until his return from Germany for its fullest expression, in a series of papers in the late 1890s. Somewhat later he published the first stratigraphic panel cross section of beds from the Glenwood to the Dubuque Formations across the region from Minneapolis via Iowa to about Beloit, Wisconsin (Sardeson, 1907). As with so many new ideas, his did not catch on promptly. One problem, doubtless, was the fact that Sardeson's named and numbered beds by which he expressed the stratigraphy and made connections from county to county across the region were defined by a mixture of rock and faunal characteristics. Surely those not well versed in the fauna preferred something else. He proposed a new formation, the Beloit Formation, and characterized it partly in that way. The term was little used by others and, when the name Platteville was published for substantially the same rocks (Bain, 1905), the latter name quickly became current. Despite Bain's rather muddled stratigraphic work, a bulletin from the USGS had more "sex appeal" and apparent authority. Sardeson complained about this lack of attention to priority (priority of terms was widely honored, but was not the policy of the USGS at that time), but to no avail, although the name Beloit is still used for the dolomitic section in south-central Wisconsin.

While still a student at Minnesota, Sardeson had also worked on the older part of the section—the Upper Cambrian and Lower Ordovician beds with Hall. After describing and revising the Middle Ordovician section, he tried to return to work on the Cambrian units and biostratigraphy—on his own in 1906, and with Samuel Weidman of the Wisconsin Geological and Natural History Survey in 1913. Both times he was frustrated because E.O. Ulrich of the USGS wanted no competitors working on those rocks and fossils. In the earlier instance, Ulrich was preparing his massive reorganization of the Paleozoic Systems (Ulrich, 1911). He took the view that stratigraphic work on the Cambrian of Minnesota and Wisconsin was his personal fiefdom and brooked no interlopers. USGS Director C.D. Walcott backed Ulrich up. You see, between 1900 and 1911 there was no Minnesota survey, so there was no bureaucratic institution or state geologist to whom courtesy and sharing might be owed by the federales. Ulrich's attitude was arrogant and selfish in the extreme and, more important, prohibited the interposition of views alternative to Ulrich's monolithic certitude.

In the second instance, Weidman hoped that he and Sardeson could coordinate what was known of the Cambrian rocks and faunal zones from Minnesota into Wisconsin, starting in 1913. This project was quashed as well—surely by the hand of Ulrich—because W.O. Hotchkiss, the Wisconsin state geologist and a pal of Ulrich's, simply told Weidman that the latter couldn't hire Sardeson nor work with him on such a project. The USGS, of course, never adopted Ulrich's new systems, so one could say that he had been defeated; but along the way he crowded a number of workers off "his" turf, in other states as well. Sardeson returned to the Cambrian and Lower Ordovician units and faunas several times in later years, but those works are largely faunistic and historical.

Sedimentary petrology

Sardeson commented often on conditions of deposition, particularly as they may have affected living creatures, but he worked intensively and innovatively on two aspects of the Middle Ordovician rocks: discontinuity surfaces and bentonite beds. He worked on both over a number of years, yet the only contemporary to pay attention to his work was Marshall Kay.

A number of peculiar surfaces or thin zones that are widespread in the Middle Ordovician carbonate rocks of the eastern United States and in Balto–Scandia show an interlocking (dovetailed) profile. The lower beds show evidence of solution and burrowing and are often coated with metal sulfides, and cavities in them contain material derived from the upper bed. The upper bed may contain chunks of the crust of the lower bed, as sand- or pebble-sized clasts. Sardeson recognized that submarine solution had been going on and named these features "corrosion zones." When he discovered that some had chunks of the lower rock floating in the upper bed he suggested "corrosion conglomerates." He quickly realized that such surfaces were time lines and demonstrated their utility in correlation over the Upper Mississippi Valley. Later work has improved our understanding of them and shown abrasion to be important locally on some surfaces. Thus they are preferably called discontinuity surfaces today.

The discovery of beds of altered volcanic ash in Ordovician rocks of Tennessee (Nelson, 1922) alerted Sardeson to the fact that similar beds lay in the Middle Ordovician beds of the upper Midwest—those waxy, soapy clays that he had described in his notes. He went back to the field and located several beds in the section. Early on he thought the fossil zones were better time markers, but soon realized that the bentonite beds and the corrosion zones were punctuation marks in time, and that each was a regionally synchronous surface in the rocks. Ultimately, he correlated the major bentonite beds—now called Deicke and Millbrig—from the Twin Cities to southwestern Wisconsin. He also realized that each ash fall killed most of the benthos, and recorded the succession of pioneering species onto the layer of volcanic mud.

GLACIAL DEPOSITS AND PLEISTOCENE HISTORY

Sardeson worked with glacial deposits for many decades, beginning with his mapping of loess in Germany. His work falls rather neatly into five categories: 1) occurrence and origin of loess, 2) mapping of Pleistocene deposits, 3) stages of the Pleistocene in the Midwest and the question of an Iowan glacial stage, 4) dating of Wisconsin ice sheets by recession of waterfalls, and 5) histories of river changes resulting from glaciation.

A hundred years ago, some American geologists believed that loess was formed by water or by water and wind in combination. This notion prevailed in the "Chicago school," led by T.C. Chamberlin and R.D. Salisbury, and was the orthodox view of Iowa geologists, especially Samuel Calvin, the Iowa state geologist. The Germans with whom Sardeson studied certainly did not believe that, and he came home convinced of the validity of the eolian origin of loess. He promptly began urging wind as the sole agent, at meetings and in short articles. In taking up this cause he once again gave offense to Chamberlin—not a helpful step for a young man trying to make his way, but scientifically honest. Sardeson made several telling points, points which others used as well, over time, to overcome the Chicago school. How might water-laid deposits blanket the extremes of topographic relief as wind could easily do? If loess were water-laid, why was there none in thousands of Minnesota lakes? He pointed out that those who believed loess to be eolian were never misled; they could identify local effects of subsequent slump or disturbance by water.

There was an unscientific reason why Chamberlin, Calvin, and others thought loess was partly water-laid. They believed in an Iowan glacial stage—a drift sheet lying between those we now know as the Illinoian and Wisconsin drifts. They argued that a great sheet of loess (the Iowan loess) was genetically related to a drift sheet of till and outwash; the loess sheet proved the Iowan drift, and the Iowan drift demonstrated the dual origin of loess. Chamberlin never adopted the newer view, but Frank Leverett, Chamberlin's successor as master of the Pleistocene of the Midwest, put an end to the notion of the dual origin of loess, prior to World War I. Being right on this issue did Sardeson no good, except that he and Leverett later had a productive partnership mapping the Pleistocene deposits of Minnesota.

From 1909 through 1915, Leverett and Sardeson mapped the Pleistocene deposits that constitute much of the surface of Minnesota. The classification of the materials and the maps were principal objectives, but the suitability of the soils in each region for agriculture was also a major goal. The work was done under a joint arrangement between the USGS and the MGS, and Sardeson was appointed USGS Geologist No. 67 for the work. He also was paid at the USGS rate—$7 per day—which was more than the state rate. Their work resulted in three map sheets of glacial and related deposits and three companion MGS bulletins; together they covered the state. The first pair, map and bulletin, concerned the northwestern quarter of the state, but Sardeson's name was omitted from authorship by MGS Director Emmons. Sardeson complained to G.O. Smith, Director of the USGS, and he was named junior author to Leverett on the remaining four parts of their big project.

Sardeson was involved in three mapping projects for the folios that the USGS published in those days; one was a bust, but the other two were fine pieces of work. The first was a joint effort with C.W. Hall in the first decade of the century. Hall had worked on the Pleistocene geology of the St. Croix Dalles (Taylors Falls) 15-minute quadrangle for some years, and Sardeson had mapped the Proterozoic and Paleozoic beds that peek out along the St. Croix Valley. Hall submitted a tentative draft to the USGS that brought down a lot of scorn. T.C. Chamberlin's son, Rollin, had advanced the knowledge of Pleistocene stratigraphy in the region beyond Hall's older views, and reviewers in Washington found the manuscript deficient in other respects. It was never published, but mostly because of Hall's outdated work on the Pleistocene deposits. In addition, as the quadrangle had two names, the USGS misunderstood the location and had promised Chamberlin that his son could have it to map. Once again, Sardeson and T.C. Chamberlin were in opposite corners.

Subsequently, Sardeson prepared a really distinguished folio (No. 201) of the Twin Cities area, on four 15-minute sheets. He did both the bedrock, with which he was already familiar, and the glacial deposits, which make up most of the area. Soon after that he mapped another four 15-minute quadrangles (Folio No. 210) in west-central Minnesota, across the southeastern edge of the deposits of glacial Lake Agassiz, an area with no bedrock outcrop at all. Both of these works were important contributions to the geology of Minnesota and to Pleistocene geology generally.

Sardeson always believed that there had been four major advances of glacial ice across the Midwest—what are now known as the Nebraskan, Kansan, Illinoian, and Wisconsin (which consisted of a complex of ice advances from several sources). As the history of the names is badly muddled—partly because Chamberlin moved some names from drift sheet to drift sheet—Sardeson liked to call them "older," "old," "young," and "younger." He tried, as did others, to establish lengths of the time intervals of glaciation and retreat, using depth of weathering and other geological tests; they all failed because they had no proper measure of age, which ^{14}Carbon provided much later.

Sardeson argued for years against the adoption of an Iowan glacial stage, saying that the "evidence"—the genetic relation of loess to a till sheet—was not only wrong, but unsupported. The "Iowan" was supposed to be "calcareous to the grass roots," a condition highly unlikely for a deposit that old, and Sardeson blamed the Iowans for mistaking a spoil heap of fresh till near Oelwein as the embodiment of the Iowan till. He was too harsh on them in that, but it has long since been established that there was no Iowan glacial stage, and that the alleged till to which the Iowan loess was supposed to be related is an erosion surface, now even named the Iowan Erosion Surface and abbreviated IES!

Sardeson improved on the concept of dating the melting of the Wisconsin ice by recession of waterfalls on the Mississippi River, first done by Winchell in the 1880s. He had much better stratigraphy and mapping to work with and he studied the walls of the Mississippi gorge for remnants of stages of the receding falls. He took the different hardnesses of the parts of the Platteville Limestone (the lip of the falls) and the dip of its beds into account. He was able to show that the falls had really begun farther down-river than Winchell had understood and, thereby, that the post-Wisconsin interval had been much longer than Winchell had concluded. It was very good field work and logic, and it was condensed for Folio 201 on the Twin Cities area, but the history is better known now because of radioactive dating methods.

In addition to the large corpus of work just described, Sardeson prepared several short studies of rivers and lakes in Minnesota. In each he described the preglacial situation that the geology suggested as well as the postglacial conditions to be found today. These were all published in the *Pan-American Geologist*. Those may have been reworked from some of the "thesis problems" that Hall had expected him to identify and make ready while he was still in the university. On balance, Sardeson's work on Pleistocene materials and concepts was not only extensive and intensive, but also very well done.

The man was always very able and resourceful. The three great phases of his work that we have just reviewed occupied his time and are represented in his publications for just about 50 years. From 1892 to 1913—21 years—he was with the university. But he continued work on these three broad fields after leaving the university until 1940—an additional 27 years. During that latter period he had no institutional support for his work, worked almost entirely alone, and was forced to publish most of his results in a maverick journal to which many workers paid no heed. He must have had remarkable drive to continue to work under those circumstances—wherein many might have said, "To Hell with it," and found some other ways to

Figure 4. *Sardeson on his 50th birthday, February 22, 1916, in the period between his posts at the university and the Securities Commission.*

fill their time. Having recognized his signal accomplishments in those three fields, we will now take a brief look at his professional life after leaving the university.

THE INDEPENDENT GEOLOGIST

The record is very slim for the years surrounding Sardeson's dismissal from the university. He definitely had notions, before he was fired, of leaving the university, but for what precise purpose is not clear. He had hoped to become state geologist when the new President Vincent came, but was thwarted in that by the hiring of Emmons for that job. He may have hoped that the mapping and other work for the USGS might keep him employed, but the record is not certain on this point. Once out of the university he was in desperate need of a regular job; he even asked Ulrich to try to get him onto the permanent roll of the USGS. Ulrich "tried," but I believe that it was a spurious try. After about 1915, when the mapping of glacial deposits wound down, Sardeson was hard up. After his daughter went to work in the mid-1920s, he was dependent on her for a living. He sold some fossils in the 1920s, but she was the engine of the family for the rest of his and her mother's lives. In this section we will consider briefly the professional work that he did away from the university after the glacial work was done.

He tried several times to begin new work for the USGS: mapping the Cretaceous deposits of Minnesota, mapping the St. Croix Dalles quadrangle, or correlating Cambrian beds and zones from Minnesota to Wisconsin, with Samuel Weidman of the Wisconsin Geological and Natural History Survey. Those gambits were all turned away "for lack of money," but the prohibition of his working with Weidman on the quadrangle and the interstate correlation of the Cambrian beds was surely engineered by Ulrich, who wanted no one working in the region who did not believe in his revisions of the Paleozoic systems! Sardeson (fig. 4) went into real estate work briefly, apparently because of his experience with soils and the knowledge that developers were interested in such features as he and Leverett had mapped. His real estate partner promptly died and left Sardeson adrift again.

A statewide association of developers hired him in 1916 to help lobby for a new bill in support of drainage of wetlands—to help farmers and to add to the state's arable acreage that might be sold to new farmers. He worked to educate the public as well as the legislature on that matter. Soon the attorney general enlisted his expertise in two interstate suits that were heard before special masters of the U.S. Supreme Court. In *North Dakota* v. *Minnesota,* Sardeson successfully defended Minnesota against the claim that it had caused flooding in North Dakota by the ditching of fields on the Minnesota side. He showed that the ditching was across the regional slope and, therefore, delayed drainage into Lake Traverse and the Bois de Sioux River. Culverts in North Dakota that were too small had been the real cause of North Dakota's flood problems. In *Minnesota* v. *Wisconsin,* wherein the former tried to budge the state boundary farther from its shore in Duluth harbor, and toward Wisconsin, Sardeson steered an even course. He explained the geologic aspects of the original specifications of the boundary of Wisconsin (the older state) and their geographic location at the time of the suit. Minnesota's suit was so frivolous, however, that an honest scientist could not have turned the outcome in its favor.

Even so, the attorney general appreciated his work enough to offer him a new post in the late fall of

1917. At that time of burgeoning exploration for and interest in petroleum and natural gas, the states bordering Minnesota all had laws requiring the licensure of companies seeking to sell stock in petroleum, mining, and related ventures in their states. Minnesota did not, and shysters were selling stock in all kinds of uncertain or crooked schemes to the unsuspecting and uneducated in Minnesota. In mid-1917 Minnesota enacted a similar requirement—the so-called "Blue Sky law"—that assured citizens that stock in licensed companies represented known assets and some reasonable expectation of success in the venture.

Sardeson was the second expert hired to carry out the Blue Sky examinations of properties and companies, and he started in December of 1917—for $10 per day and expenses while actually employed. From then until the early 1930s he visited properties and wells in the Midcontinent, Gulf Coast, and Rocky Mountain petroleum provinces many times. He made reports to a securities commission, which issued the approval or denial of the requests for a license. Up to the time he testified about Teapot Dome before the U.S. Senate in 1924, none of his recommendations had been overturned.

Sardeson's earnings from this work varied widely. Without the constant support of his daughter he couldn't have made it. Particularly was this true after 1930, when the East Texas field blew in and depressed oil prices countrywide. The Great Depression reduced his earnings as well. In 1934 the federal Securities and Exchange Commission was formed and took over the work of the state agency; the latter was disbanded and Sardeson was totally dependent upon his daughter thereafter. Throughout his tenure with the Securities Commission and up until 1940, Sardeson was constantly busy with geology: paleontology, stratigraphy, and some glacial geology. Of his nearly 100 papers in that interval, nearly all were published in the *Pan-American Geologist*. From then until his death he continued to expatiate on geologic subjects to correspondents, particularly Ray S. Bassler of the U.S. National Museum and W. Charles Bell of the universities of Minnesota and Texas. He died August 28, 1958 in Seattle.

SUMMARY

Sardeson was a brilliant man, full of curiosity and innovation, who accomplished a great deal of good geologic work. He was also paranoid, arrogant, disputatious, and set in his ways to such a degree that he could not adapt to the changing goals of higher education in the early years of the century. His body of completed work contains some really good ideas and conclusions—derived from his good qualities. The later work contains some dross and too much obscure writing because he had perforce to publish in a poorly run journal that was laughed at for its bad qualities at the same time that the good in it was ignored.

He was ahead of his time regarding the solely eolian origin of loess, a practical regional stratigraphy based on the rocks and fossils in that region, and a concept of species based on the variation in a population rather than on trivial differences in assorted individuals. Whatever one may think of Sardeson, one must recognize that he produced a great deal of constructive geology and many testable ideas while he was at the same time a professional and a social outcast. Who among us could do as well under such troubling and stifling circumstances?

This report is expanded from an oral report given at Madison, Wisconsin, May 1, 1997, but is much condensed from my biography of Sardeson (Weiss, 2000). The fuller treatment of Sardeson and his history in his biography also sets forth some aspects of the practice of geology and the operation of a state university a century ago. The abundant documentation of Sardeson's story is contained in that biography and omitted here for simplicity. A complete bibliography of his published work is also contained there.

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FREDRIK TURVILLE THWAITES

Lee Clayton[1] and John W. Attig[1]

INTRODUCTION

Geologists working in Wisconsin commonly check old field notes in Wisconsin Geological and Natural History Survey (WGNHS) files to see whether earlier geologists left information about their field area. They often find that F.T. Thwaites has been there before them and that his observations and interpretations usually agree with modern ones.

Neither of us ever met Thwaites, but we feel he is one of our closest acquaintances because we continually deal with his observations, interpretations, and idiosyncrasies. Thwaites is best known as a glacial geologist, but he also was an authority on the Paleozoic stratigraphy of Wisconsin and adjacent areas and on Wisconsin geology in general.

This brief biography is nearly the same as that presented at the symposium about Wisconsin geologists at The Geological Society of America meeting in Madison in 1997. This is a chronological review of his life, but it is only a preliminary evaluation of the contributions Thwaites made to geology. A fuller version needs to make use of the large amount of material related to Thwaites in the archives of the WGNHS, University of Wisconsin–Madison Department of Geology and Geophysics, the University of Wisconsin, and the State Historical Society of Wisconsin. Much of the following was taken from two short autobiographies (Thwaites, 1954 and 1961a) and a short biography by Bailey (1980).

THWAITES' EARLY LIFE

Our story begins in 1846 when Thwaites' maternal grandfather Henry Turville (one of several family members of that name) homesteaded one mile and three quarters south of the capitol (shown in a 1959 map of Madison; fig. 1) nine years after Madison was platted and two years before Wisconsin became a state. Through the years, the farm became crowded with outbuildings, homes for the extended family, a small boat works, and a commercial greenhouse (fig. 2). It is now the Turville Point Conservation Park. The remains of building foundations are hidden in the woods, but daffodils and other nursery plants still flourish.

Meanwhile, Fred's father Ruben Gold Thwaites (1853–1913), son of immigrant Yorkshire parents, moved from Massachusetts to Wisconsin in 1866. Here he worked as a farmhand and schoolteacher and

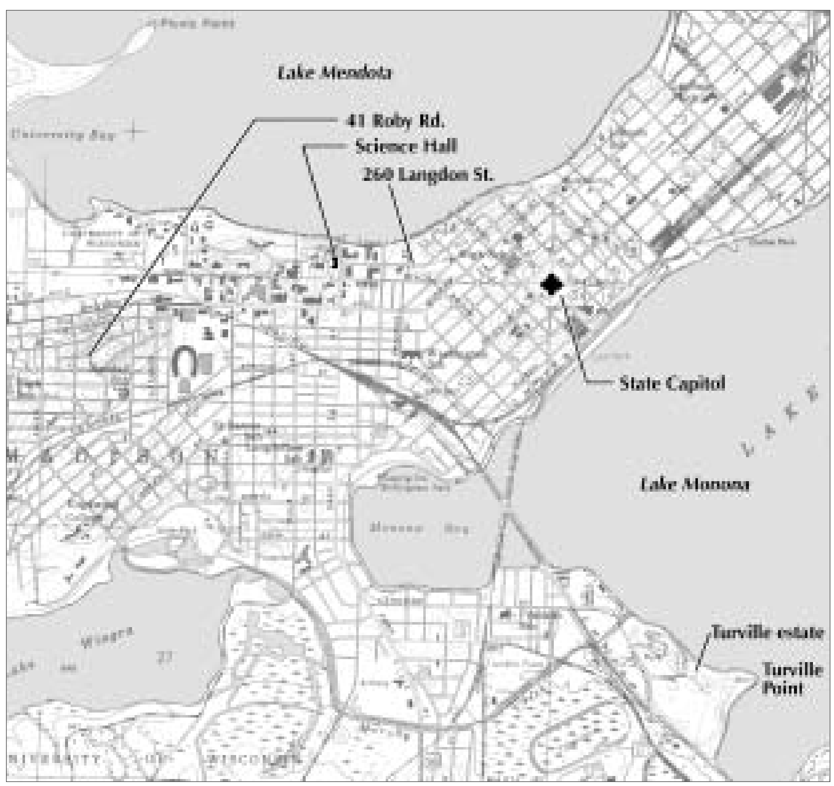

***Figure 1.** A 1959 map of Madison showing Thwaites' workplace and homes. The Turville estate is shown in the lower right corner of the map; a more detailed map of the farm is shown in figure 2.*

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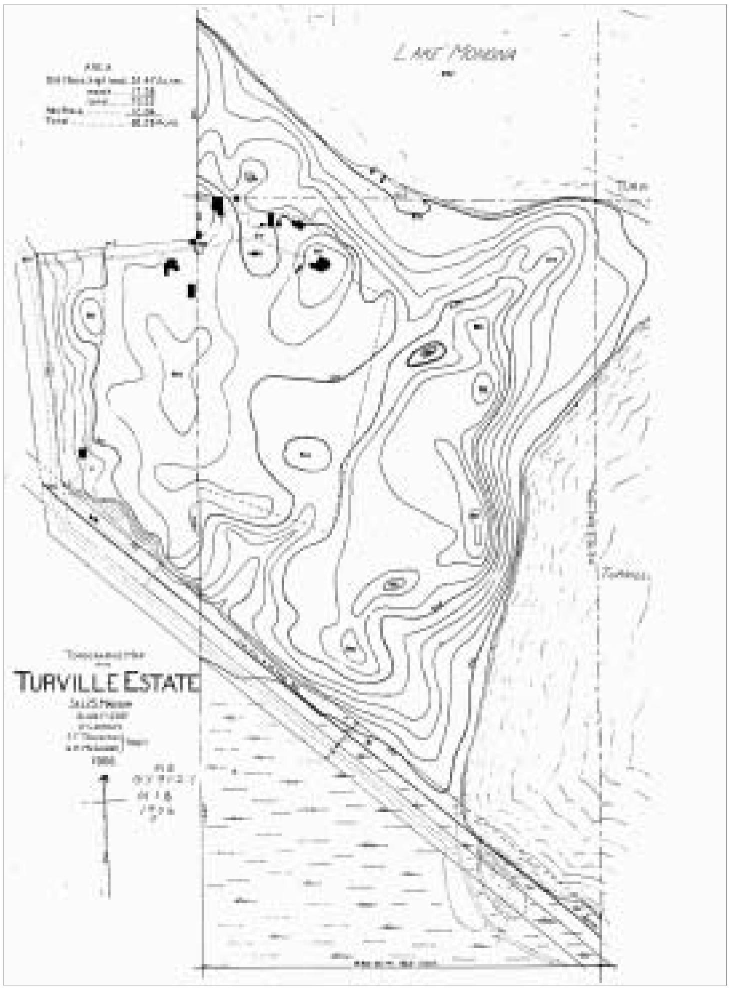

Figure 2. *The Turville property, as surveyed by F.T. Thwaites in 1906.*

was on the staff of the Oshkosh *Times;* he went back east to Yale and returned to Madison in 1876, becoming the managing editor of Madison's *Wisconsin State Journal*. A decade later he moved to the State Historical Society and soon became its superintendent and secretary. He was a celebrated historian, known for his writings on the settlement of the North American interior, especially during the French period in Wisconsin history, and he edited numerous American historical documents, including the Lewis and Clark journals and the 73-volume *Jesuit Relations*.

In 1882, Reuben married Henry's daughter Jessie Turville (1854–1938). The Turvilles were now considered a "prominent" Madison family. In 1883, Fredrik Turville Thwaites was born; a younger sister died of diphtheria as a baby. During his childhood the family lived in a mansion with a live-in maid at 505 Langdon Street (later renumbered 260), one-quarter mile east of campus; when he reached his twenties, the family moved to a house on the Turville farm (fig. 2). (We use the modern spelling in this paper, but the family name, along with Fred's middle name, was commonly spelled Turvill or Turvil until early in the twentieth century.)

On the whole, Thwaites seems to have had a comfortable Victorian upbringing. But he nearly died of diphtheria before he reached school age. Until the age of ten, he was home schooled by his mother, who had a botany degree from the University of Wisconsin. He continued to have health problems the rest of his life.

Highlights of Thwaites' early life include several trips that influenced the direction of his later career. When he was ten years old, he accompanied his father and mother on a 1,000-mile trip down the entire length of the Ohio River in a 15-foot skiff with sail. Little Freddy appears as "the boy" in Reuben's (1897) travelogue. This was just one of their many river trips. The family also traveled to Switzerland when he was 13 and Norway when he was 18, and they took a camping trip through Yellowstone Park and the Tetons when he was 19.

MAJOR INFLUENCES: KANSAS, LEITH, AND ALDEN

During his first two years at the University of Wisconsin, he took engineering courses, despite his domineering father's opinion that they were "trade school" courses. His first summer job was measuring depth to water in wells in western Kansas; he said this influenced his shift from engineering to geology, which was somewhat more respectable. In 1906, at age 22, he received his Bachelor's degree, with a senior thesis on the early Paleozoic and the Pleistocene geology of an area just southeast of Madison around Lake Waubesa and Lake Kegonsa.

That summer, he had a fateful encounter with C.K. Leith, who was head of the geology department at the University of Wisconsin during a large part of

Figure 3. F.T. Thwaites at camp on beach near Herbster, doing field work on the sandstone along the shore of Lake Superior. WGNHS photograph 2785, by F.T. Thwaites, circa 1910.

Thwaites' life. He spent the summer working for Leith, who was doing mineral exploration near Cobalt and Sudbury, Ontario. Thwaites and a helper were out in the bush doing dip-needle surveying when the helper developed appendicitis. Thwaites could not get the helper and all the equipment out by himself, so some equipment was left behind and never recovered. As a result, Leith decided Thwaites was irresponsible. From then on, they never got along very well.

The next summer, 1907, at age 23, Thwaites was a field assistant to William C. Alden, who was mapping the Pleistocene geology of southeast Wisconsin for the U.S. Geological Survey, including Thwaites' Master's thesis area. In 1908 (age 24) he received his Master's degree; his thesis was about the early Paleozoic and Pleistocene geology of an area just southwest of Madison, between Middleton, Verona, and Cross Plains, "it being the district traversed by the field excursions of the elementary classes in geology" (Thwaites, 1908). Thwaites was the first to work out the history of glacial Lake Middleton. His faculty advisor for his Bachelor's and Master's theses was N.M. Fenneman, but Alden seems to have been a greater influence; even the shorthand symbols used on his field maps the rest of his life were borrowed from Alden.

"UNFIT TO TEACH"

About this time, Thwaites began hunting for a teaching job in geology. But Leith had decided that he was unfit to teach, so the geology department would not hire him, and Leith would not give him a letter of recommendation. However, he did find a job with the Wisconsin Geological and Natural History Survey. During the summer of 1908 he was a field assistant to Samuel Weidman, who was mapping the Precambrian and Pleistocene geology of northwestern Wisconsin; he seemed to like Weidman but disagreed with many of his interpretations. In 1908 and 1909 he helped state geologist W.O. Hotchkiss with a new geologic road map of the state (Hotchkiss and Thwaites, 1912). In 1909 he began field mapping the Paleozoic geology of the Richland Center area, but had to quit because of illness.

From 1910 to 1912 he studied the Precambrian sandstone along the Wisconsin shore of Lake Superior (fig. 3). He used a small boat with a gasoline engine and was assisted by a Turville cousin. This work was published as WGNHS Bulletin 25 (Thwaites, 1912).

In 1911, at age 27, he was still lacking a letter of recommendation for a teaching job, so he took a half-time job as curator of the geology department's mu-

Figure 4. *Discussing stratigraphy of Cambrian sandstone, 5 miles north of Black River Falls. Left to right: F.T. Thwaites, H.R. Aldrich, E.O. Ulrich, E.F. Bean, R. Bayard, R.N. Hunt. WGNHS photograph 1517, by W.O. Hotchkiss, July 24, 1916.*

seum. The next year, it became a full-time job. He was a "glorified office boy," but he stuck with it for a frustrating 17 years.

In 1913 Thwaites did some glacial surveying in Glacier Bay for Lawrence Martin, who was internationally known for his Alaskan glacier studies with R.S. Tarr, but is best known in Wisconsin for his *Physical Geography of Wisconsin* (Martin, 1916). According to Thwaites, Alden had mistakenly mapped hummocky sand and gravel in southeast Wisconsin as "terminal moraine," and he credits Martin with showing him that it actually resulted from the deposition of outwash on stagnant glacial ice, as they had observed in Alaska.

In 1914, after a lapse of a couple of years, Thwaites went to work for the WGNHS again. He examined cuttings from water wells drilled throughout the state and produced a geologic log for each. He continued to do this for the next 43 years. During those years he compiled 2,000 logs.

As a result of knowledge gained during this process, he became the WGNHS expert on the subsurface stratigraphy of the state; he also served as the Survey groundwater geologist most of his life. He was WGNHS's only geologist other than the state geologist from 1935 to 1956. For most of that time he received only a token salary for this work. During this period, the WGNHS and the geology department were housed in Science Hall (fig. 1).

A decade associated with E.O. Ulrich began in 1914. Ulrich was a geologist with the U.S. Geological Survey. For a few weeks every summer for several years he studied Paleozoic stratigraphy of the Driftless Area, especially near the Baraboo Hills. Thwaites seems to have been his field assistant and chauffeur much of this time (fig. 4). According to state geologist Hotchkiss, Ulrich was "the best informed man on this continent on the stratigraphy of these formations," an opinion Thwaites did not share. Ulrich was trying to find evidence for an Ozarkian Period and a Canadian Period between the Cambrian and Ordovician Periods. Thwaites realized that the extra periods resulted from Ulrich's miscorrelation of formations, and he was not shy about telling him so. Their final falling out occurred in 1924 with a rancorous exchange of letters.

In 1916, W.H. Twenhofel, Lawrence Martin, and Thwaites began field work on the geology of the Tomah and Sparta 15-minute quadrangles in the middle of the Driftless Area (Twenhofel and Thwaites, 1919). That summer, the car replaced horse and bicycle for field transportation. This work was supposed to be published as a U.S. Geological Survey folio. The manuscript was finished in 1922, and years later it was placed in the WGNHS Open-File Series (Twenhofel and others, 1922), but was never published formally because of the stratigraphic disagreements with Ulrich.

TEACHING AT LAST

Through the influence of Lawrence Martin, Thwaites started teaching in the geology department in 1916 when Martin left for the World War. The position was part time and temporary. Thwaites was underweight and did not serve in the war, but he taught plane-table surveying to army trainees. Colonel Martin did not return to the university after the war, and he recommended that Thwaites take over his courses in glacial geology and geologic mapping (fig. 5). At age 32, Thwaites reached his goal of a permanent (but part-time) teaching position in the geology department, while remaining a half-time curator.

In 1922 he produced an early mimeographed version of his future book, which was to be called *Outline of Glacial Geology.* This was frequently updated until published 12 years later.

The years 1926 to 1928 were eventful. He was "fired by Leith," but the Survey had received a windfall to investigate road materials, including gravel. This apparently paid his salary for two or three years. During this time he began studying the Pleistocene geology of northeast Wisconsin (figs. 6, 7, and 8).

In 1928, he quit his half-time job as museum curator and was hired full time at the instructor level in the geology department; for most of the rest of his career he regularly taught geomorphology, physiography, glacial geology, and geologic mapping. These were popular courses, with large enrollments. His mapping class field trips to Devils Lake in April were especially memorable. It sometimes snowed, and the crew included one or two cooks and a chaperone for the girls. Plane-table mapping was emphasized. At its maximum, it had more than 90 students. His glacial class field trip was usually in May (fig. 5).

Figure 5. *Glacial geology class at Lake Michigan shore bluff just south of the present site of Two Creeks Buried Forest Unit of the Ice Age National Scientific Reserve. Amy Mueller, F.T. Thwaites' future wife, is upper right in the group of women; Thwaites is at the right end of first row. WGNHS photograph 3403, by F.T. Thwaites, 1925.*

That same year, when he was 44 and she was 33, Thwaites married Amy M. Mueller. She had been his student (fig. 5) and a WGNHS secretary. Thwaites said one of the changes marriage brought into his life was he was no longer allowed to carry unwrapped horseradish and swiss-cheese sandwiches to the university in his jacket pocket. In 1931, the first of their three sons was born.

In 1929, on the eve of the Great Depression, the state geologist lost his position on the Wisconsin Highway Commission; money was no longer available to do road material surveys, and the northeast Wisconsin Pleistocene work was discontin-

Figure 6. *F.T. Thwaites at test pit searching for red drift as part of road materials survey, 8 miles northeast of Shawano; assistant's hat, hand, and shovel are sticking out of pit. WGNHS photograph 4284, by F.T. Thwaites, July 25, 1928.*

ued. Thwaites' 1929 and 1930 field seasons were spent on a road-material survey in Illinois.

In 1934, his book, *Outline of Glacial Geology,* was published. It was updated several times and revised in 1945 and 1961. It was widely used as a textbook and was the only comprehensive American review of glacial and Pleistocene geology until R.F. Flint's textbook appeared in 1947.

Much of 1935 was spent preparing for the Wisconsin part of the ninth annual field trip of the Kansas Geological Society. This marathon ten-day excursion covered 1,500 miles in Iowa, Illinois, Wisconsin, Minnesota, and Michigan, and included a 471-page guide book. Thwaites led most of the Wisconsin segment.

Since his twenties, Thwaites had lived in one the houses on the Turville property, but in 1938 his mother died and the Turville estate sold the house, so the Thwaites family had to move. They bought a substantial house at 41 Roby Road in University Heights on the southwest edge of campus, where he lived the rest of his life. There his wife lived until her death in 1980.

In 1938, at age 56, he was promoted from lecturer to assistant professor. He never received higher rank, reportedly because he had refused to learn the French and German required for the Ph.D. degree (in rebellion against his multilingual father?), but probably also because of strained relationships with some other faculty members, including C.K. Leith.

He had been doing field work on the Pleistocene geology of eleven counties in northeastern Wisconsin since 1926. The results were published, with a 1:250,000-scale color geologic map, in 1943 in the *Bulletin of The Geological Society of America.* Aside from his book, this was his most elaborate publication. It remains the authoritative publication about much of that area, even though most of the mapping was done without aerial photographs or published topographic maps; the report also contains a 1:250,000-scale topographic map of the entire area, with a contour interval of 50 feet, that he constructed using an aneroid altimeter.

During the war years of 1943 to 1945 he taught physics to naval recruits, and from 1948 to 1952 he was engaged in unfunded field work on the Pleistocene geology of the Door Peninsula in eastern Wisconsin (Thwaites and Bertrand, 1957). In 1953 he led the Midwest Friends of the Pleistocene through northeastern Wisconsin on their fourth annual field trip. He retired from the department of geology in 1955 and from the WGNHS in 1957 at the age of 71. From late 1955 to early 1958, he was occupied compiling the Wisconsin part of a new glacial map of the United States east of the Rocky Mountains; in letters to H.B. Willman (Illinois State Geological Survey), who was one the chief compilers, Thwaites mentions having trouble with the fine details because his eyes bothered him. He died in 1961 at the age of 77.

A PERSONALITY ALL HIS OWN

He was described as "shy," "introverted," "reserved," "unassertive," "skeptical," "gentle," and "kindly." He apparently had an understated wry wit. Francis Hole, University of Wisconsin soil science professor (emeritus), remembers a field trip in northeastern Wisconsin, when Thwaites commented that he had heard about a hermit living down a side road, so he went down the road and found a whole family of hermits.

He never became a friend of C.K. Leith. On November 11, 1954, the year he retired, Thwaites gave a

Figure 7. *F.T. Thwaites (center), German Pleistocene geologist Paul Woldstedt (right), and student A.T. Eberhardt in northern Oconto County. WGNHS photograph 4449, by F.T. Thwaites, September 3, 1928.*

talk to the Geology Club. State Geologist George F. Hanson was there and took notes. The title was *History of Wisconsin Geology* (Thwaites, 1954), but it was actually the story of his life. This was the talk he was going to give at a Geology Club banquet, but did not because C.K. Leith attended. Thwaites said he didn't want to cause him to have a stroke.

In that talk he commented that he made a critical decision in his life in 1907 when he turned down a teaching assistantship in mineralogy at Brown University, perhaps the only offer he received. He decided to stay either because of illness or because his work that year with Alden was so enjoyable. What direction might his life have taken if he had left Madison then?

Figure 8. *F.T. Thwaites (left) with William Heritage, and Victor Hanson, taking advantage of a forestry railroad in northwestern Menominee County. WGNHS photograph 4535, by F.T. Thwaites, October 29, 1928.*

THE THWAITES LEGACY

Thwaites was widely liked and respected as a teacher. In his four decades of teaching he left a lasting impression on hundreds of students. He was the advisor on several dozen bachelor's theses, about a dozen master's theses (including one by Andrew Leith, son of C.K.), and a few doctoral theses, most dealing with Wisconsin geology.

His obituaries described him as knowing more about Wisconsin's geology than any other person. The list of publications that follows suggests the breadth of his interests; in general, he was interested in all aspects of Wisconsin geology, especially the glacial geology, geomorphology, and early Paleozoic stratigraphy. In addition to his publications and extensive field notes, he left behind a variety of manuscripts, including one for a textbook of geomorphology based on the principles of physics.

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GILBERT O. RAASCH, STUDENT OF WISCONSIN'S ANCIENT PAST

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ABSTRACT

Milwaukee-born geologist and paleontologist Gilbert O. Raasch conducted the most extensive study of Wisconsin Paleozoic rocks during the first half of the twentieth century. Largely self-educated, he assembled comprehensive paleontological collections from Cambrian, Silurian, and Devonian strata of the state, documenting his work with detailed field notes and maps. Beginning when he was in high school and continuing through his time as a college student and museum professional, Raasch wrote a number of innovative papers about the geology of Wisconsin. Significantly, his detailed biostratigraphic approach allowed him to develop evidence that resolved some important geological controversies and misinterpretations of these rocks. Although widely recognized as the expert on Wisconsin Paleozoic geology, unfortunately Raasch never was able to secure the research position in the region that would have allowed him to continue to follow his interests and further develop his ideas. Although he expanded his studies into surrounding states, he eventually had to abandon his true research interests in favor of employment in the oil industry of western Canada. Although Raasch was very successful in this new pursuit, our understanding of Midwestern Paleozoic geology and paleontology suffered a significant loss by his departure.

INTRODUCTION

Gilbert O. Raasch is widely acknowledged as Wisconsin's most prominent twentieth-century student of Paleozoic geology and paleontology. Through classic papers, meticulously documented collections, detailed field notes, and unpublished manuscripts, Raasch has provided scientists with a unique legacy invaluable to future research on the Lower Paleozoic strata not only in Wisconsin, but the rest of the Midwest as well. Although others have studied the geology and paleontology of these rocks, no one has left a similar wealth of irreplaceable specimens, observations, and ideas. In addition to his skills as a research scientist, Raasch was also a dedicated educator who made a lasting impact on the public's appreciation and understanding of area geology.

Raasch's accomplishments are even more impressive, considering that he made most of them over a short interval at the beginning of his long career. Unfortunately, his career can be viewed as a significant lost opportunity for Wisconsin. Despite his many contributions and the promise of an even more productive future, Raasch never was able to secure the type of employment in Wisconsin or the Midwest that his capabilities and accomplishments warranted. Sadly, his research in the region was cut short, and he spent most of the last sixty years of his life working elsewhere by necessity, not by choice. Although others have studied the same subjects and region more recently, no one has been able to fill the scientific void Raasch left behind in Wisconsin. As a result, critical documentation of the area's geology and paleontology was never done, and the opportunity to do so may have been lost.

More than simply documenting Raasch's career and scientific contributions, we have attempted to uncover the factors that prevented Raasch from enjoying the kind of employment that would have utilized his unique talents for the benefit of all. Although his entrance into the profession was atypical and his focus unusually intense, in the end it was the actions of others that steered Raasch's career away from his original goals and out of the Midwest. The history of Raasch's career can also serve as a lesson to young geologists.

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MILWAUKEE BEGINNINGS

The beginning of Raasch's scientific career marks an important change in the character of geological and especially paleontological studies in Wisconsin. Previously, wealthy amateur naturalists, such as F.H. Day, T.A. Green, E.E. Teller, and C. Monroe, supplied most of the fossil specimens used in research by professional scientists (Mikulic and Mikulic, 1977; Mikulic, 1983; Mikulic, 1991; Kluessendorf and Mikulic, 1997; Mikulic and Kluessendorf, 1998). In contrast, Raasch was the first to make extensive, systematically assembled, and well documented collections for his own stratigraphic and paleontologic studies. How does a small boy without mentors or a specialized background become an outstanding paleontologist and geologist? Part of the answer lies in his own curiosity and ambition, but the German cultural influence of early twentieth-century Milwaukee also played a major role.

Gilbert Oscar Raasch, the younger son of Henry C. and Matilda (Spetz) Raasch, was born in Milwaukee, Wisconsin, on May 27, 1903. He grew up in a German neighborhood on the northeast side of town in a family that embraced typical German values of the time, although his parents were proud that Gil spoke English at home while his cousins spoke only German. His father, a skilled tradesman and successful businessman, was the senior partner in the Milwaukee Tile & Mosaic Company. Henry Raasch was also active in local politics and labor organizations. He served several terms on the Board of School Directors for Milwaukee Public Schools, was a founding member of the *Milwaukee Leader*, an active member of the Socialist Party, and held the office of president in the International Tilers' Union (Usher, 1914). As a result, Gil had a very political upbringing and remembered attending "monster" political rallies at the Milwaukee Auditorium as a small boy. He was also introduced to a number of prominent Wisconsin politicians, such as Milwaukee mayor Daniel Hoan, Wisconsin governor Francis McGovern, and Congressman Victor Burger and his wife, who were also family friends.

Gil's first exposure to geology came at the age of four when he and his mother collected beach pebbles and fossils along the Lake Michigan shore at Whitefish Bay. Although his mother had an informal interest in nature, no one in the family made any effort to direct him into a naturalist profession. In fact, his father never was convinced of the utility of Gil's interests, which undoubtedly seemed strange in the hard-working practical culture in which he was raised.

During his adolescence, Gil began collecting Devonian fossils from the glacial drift along the banks of the Milwaukee River near his home, but he never gave much thought to the science of his finds. However, this all changed one day in his sixth grade geography class when he learned that geologists could determine the relative age of rocks by studying their fossil content. Fascinated

Figure 1. *Devonian exposures at the Milwaukee Cement Company Mill #2 quarry on west side of the Milwaukee River, Milwaukee, Wisconsin (circa 1890s). After this quarry was abandoned and partially flooded, Raasch would walk on the winter ice to collect fossils from the walls, including the tunnels such as the one at the right side of this photograph.*

by this idea, Gil was inspired to visit the recently abandoned Milwaukee Cement Company quarries, which he had seen in operation during a family picnic years before. Located along the Milwaukee River a couple of miles from his home, these exposures of the Devonian Milwaukee Formation proved to be highly fossiliferous and a great source of specimens for Gil's growing collection. Gil found that he could collect fossils even during the winter by walking on the ice of these water-filled quarries to reach otherwise inaccessible exposures. Occasionally, he would even venture onto weak ice in the partially flooded old mine entrances (fig. 1) —a dangerous practice that took the lives of many local youths.

Figure 2. *Silurian exposures at the Schoonmaker Reef in the G.D. Francey Coal, Stone & Supply Company quarry, Wauwatosa, Wisconsin (circa 1913). The west wall of the quarry seen in the background was the site of Gil's big adventure, around the time that this photograph was taken (from Mikulic and Kluessendorf, 1998).*

Interested in learning more about his fossil finds, Gil took advantage of local opportunities that were seldom available to young middle-class boys elsewhere and, as a result, a child's hobby became a lifelong career. Fortunately for Gil, early twentieth-century Milwaukee had notably progressive views on public education, which were inspired in large part by its German community. The city had an outstanding public museum and library that provided Gil with a unique opportunity to learn the details of local geology and paleontology and to identify the specimens he found. Instead of beginning with popularized books written for the general public, he borrowed the classic scientific monographs about Wisconsin written by Hall (1867), Chamberlin (1877), Whitfield (1882), and Cleland (1911). To supplement this published record, he made use of the museum's extensive exhibits of local fossils.

Through his library and museum work, Gil was able to learn of other important localities around Milwaukee, which he visited via the local trolley system. Most important of these sites was the famous Silurian Schoonmaker Reef at Wauwatosa (Mikulic and Kluessendorf, 1998), where his career was almost cut short. During one of his first visits to the site, Gil entered the empty quarry, thinking that the workman had quit work for the day. Much to his surprise, however, the quarry soon was rocked by a series of explosions, causing him to attempt a hasty retreat up the famous west wall (fig. 2) of the pit. Apparently the workers had left only temporarily after lighting explosive charges in large blocks that they were trying to break up. After the excitement subsided, Raasch and the workers returned to their respective tasks. The Schoonmaker reef became one of Gil's favorite localities, and, even at this young age, he was able to relate Hall's (1862) and Chamberlin's (1877) classic descriptions of the reef to the outcrops and fossils he encountered there. One of Gil's most important accomplishments at the Schoonmaker Reef was making a small collection of fossils that now represent the only specimens still available from the reef flank beds of this historically important locality.

All these early activities helped Gil develop the unique abilities that would figure so prominently in his later career. For example, he made extensive lists of the fossils he was finding at each locality and de-

Figure 3. *Shaft site and dump pile of Late Devonian rocks from which Raasch collected, the city of Milwaukee Linwood Avenue Intake Tunnel, near Lake Park, Milwaukee, Wisconsin (circa 1913; from Mesiroff, 1914).*

veloped the skills of fossil identification for which he was later renowned. He spent considerable time carefully preparing, identifying, and labeling his specimens, becoming an expert on the paleontology of local Silurian and Devonian rocks at a very young age.

These activities might have continued merely as an intense hobby; however, Gil's future changed in seventh grade when, by chance, he met Ira Edwards at the old Milwaukee Cement Company quarries. Edwards, who recently had been hired as the geologist at the Milwaukee Public Museum (MPM), was out on one of his first field trips to examine local outcrops. That same day, Gil also was out collecting, and he was amazed to see "a real geologist" wearing high-topped boots and equipped with a real geologist's hammer and chisel, at what had been "his" outcrops. Watching Edwards pound away on an exposure that he had already determined was a poor fossil prospect, Gil, although very shy, decided that he could not pass up the opportunity to talk to Edwards. Trying to think of a way to start up a conversation, he remembered that there was a fossil cephalopod in a block of rock near the railroad bridge too large for him and his friends to collect. Gathering his courage, Gil raced up to Edwards blurting out "I know where there is a *Gyroceras eryx* in a large block, and you can have it if you can get it out." Edwards, with a shocked look on his face, was momentarily speechless upon hearing the small scruffy boy correctly using formal scientific names for local fossils. This would become one of Edwards' favorite stories, to relate to all interested parties. Had it not been for this encounter, Gil thought he probably never would have become a professional scientist (Raasch, 1948a).

Shortly after this encounter, Gil made an important discovery, which resulted in his first scientific paper. In 1913, when he was in eighth grade, the city of Milwaukee began construction of a water intake tunnel out into Lake Michigan (Mesiroff, 1914). Rock excavated from this tunnel was dumped near the main construction shaft located on the Lake Michigan shore at Lake Park, about a mile from Gil's home (fig. 3). He recognized that it presented an excellent opportunity to collect fossils from the seldom exposed Devonian rocks overlying the Milwaukee Formation. He made an extensive collection of this material, balancing large slabs on a board during his trolley rides home, much to the bewilderment of fellow passengers. His specimens established the age of these rocks as Late Devonian—the youngest bedrock in the state. He later coauthored a paper on these rocks with Edwards (Edwards and Raasch, 1922). Gil's collection remains the only significant source of rock and fossil specimens from these youngest Paleozoic strata in Wisconsin.

Recognizing Gil's extensive knowledge of local paleontology and impressed by his collecting skills, Edwards hired him as a part-time assistant in the MPM Geology Department in 1919. Although only 17, Gil was the sole geologist at the museum when Edwards went on leave in 1920. During that time, Raasch "represented" the department at the Geological Society of America annual meeting in Chicago—certainly as one of the youngest participants. At the museum, Gil was employed primarily to identify and catalogue the fossil collection. He continued to explore the outcrops of southeastern Wisconsin, making extensive new collections while formulating his own ideas about local Paleozoic stratigraphy. The Devonian remained his primary interest, and he made a detailed section of a roadcut north of Thiensville, which he later designated as the type section of the Thiensville Formation. His museum work also included public education duties, including leading local field trips and giving lectures as well as other activities, such as participating in a museum expedition to Mount Rainier in 1921 (fig. 4). On the way to his first public talk, however, he was so scared that he hoped the trolley would get into an accident so he wouldn't have to show up. Luckily, fate did not intervene and he later became a very gifted lecturer.

During the 1920s, Raasch divided his efforts between museum work and his formal geologic education. Although these efforts continued to expand his expertise and accomplishments as a scientist, they also locked him into a situation that ultimately would force him out of the career he worked so hard to develop. After Gil graduated from Riverside High School in 1921, Edwards convinced him to pursue a college education in geology. Taking his advice, Gil enrolled at Milwaukee Normal School, which was located only a few blocks from his home. Although he soon left the school because it lacked geology courses, he did meet his future wife there.

In 1922, Gil enrolled in the geology program at the University of Wisconsin in Madison as a beginning student with a conspicuously nontraditional background. Not surprisingly, he discovered that his expertise and interests frequently distracted him from following the normal course work expected of a more typical student. Because of his extensive field and museum experience, Gil clearly knew more about Silurian and Devonian geology of the state than anyone on the faculty or anywhere else, for that matter. One of the high points of his freshman year at Madison was hearing his elderly "mentor" T.C. Chamberlin speak to one of his classes. Most of Gil's geologic efforts were still directed toward his museum job, however, and he continued to work there on weekends and during vacations. In 1923, Edwards arranged to have the MPM purchase his fossil collection for $100, as a way to help finance his student expenses. This purchase represented the MPM's first comprehensive and well documented collection of Milwaukee area Silurian and Devonian fossils, forming the nucleus of its extensive Wisconsin Paleozoic research material.

Figure 4. *Ira Edwards (left) and Raasch on the Milwaukee Public Museum expedition to Mount Rainier in 1921. (Photograph courtesy of Avis Worthington.)*

The 1920s marked the beginning of Raasch's lifelong research focus on Cambrian geology and paleontology of the Midwest, but the decade would end with Gil trapped in the middle of a major controversy involving some of the most prominent stratigraphers in the country. This new interest in the Cambrian arose from his participation in a road materials program of the Wisconsin Geological and Natural History Survey (WGNHS). In 1913, the WGNHS established a program to study the Paleozoic rocks of the state to find better sources of road materials (Hotchkiss, 1924). Because much of this work focused on understanding the stratigraphy of Cambrian and early Ordovician rocks, the WGNHS secured the assistance of Edward O. Ulrich of the U.S. Geological Survey. Ulrich recently had proposed a major revision in the classification of these rocks, erecting two new systems: the Ozarkian and the Canadian (Merk, 1985; Weiss and Yochelson, 1995; Byers, this volume). Therefore, the

Figure 5. *E.O. Ulrich (right), Fred Thwaites (left), and Willard Yeakel at lower Silurian outcrops along the shore of Green Bay, Idlewild Point, near Sturgeon Bay, Wisconsin. During one of the Wisconsin Geological and Natural History Survey summer road materials trips. (WGNHS photograph 741 by W.O. Hotchkiss, 1914.)*

WGNHS considered him to be "the best informed man on this continent with regard to the stratigraphy of these older formations" (Commissioners of WGNHS, 1916), and they needed him to work out the complex relationships of these and other Paleozoic rock units in Wisconsin. Over the next several years, Ulrich spent a few weeks each summer in the field with W.O. Hotchkiss, Ernst Bean, and other Survey staff studying outcrops of these rocks (fig. 5). In 1919, Hotchkiss invited Ira Edwards to accompany the field party on one of its summer tours; he returned to the MPM with a collection of Cambrian fossils. Raasch was surprised at the abundance and diversity of the fossils from the oldest Paleozoic rocks in the state, and he began to study them with his usual vigor.

When Gil began his university studies in Madison, two new opportunities arose that further directed him into Cambrian research: proximity to Cambrian outcrops and direct involvement with E.O. Ulrich and the WGNHS project. From his Madison locale, Raasch was able to spend many weekends traveling around central Wisconsin collecting Cambrian fossils for the MPM, where he still worked part time. He would explore the countryside by train, watching for promising localities. When he spotted one, he would disembark at the next stop and walk back to the site. On one of these trips in 1924, he discovered his famous Point Jude merostome (aglaspid) parting—the world's single richest known source of these unique fossil arthropods. During the same year, Gil became an official assistant to Ira Edwards in a new mapping effort related to the Ulrich/WGNHS road materials program. It had become clear that the project was too big for Ulrich to finish on his short summer visits, so the MPM and the U.S. National Museum (USNM) were formally included as participants (Ulrich and Resser, 1930). Raasch did an outstanding job collecting and identifying specimens from the detailed measured sections he made and, undoubtedly, Edwards was eager to have him assist in the work.

While working on this project, Edwards and Raasch became well acquainted with Ulrich, marking the real beginning of Raasch's formal Cambrian research. In early 1926, Ulrich provided funding for both men to spend a three-month "apprenticeship" working on the paleontology of the Upper Mississippi Valley Cambrian project at the USNM in Washington, D.C. This was a wonderful educational experience for Raasch because he was able to work with the USNM's large collections and library, learn techniques, be exposed to new ideas, meet many prominent paleontologists, and demonstrate his exceptional skills and knowledge to them. A special bonus of the trip was meeting Charles D. Walcott, Secretary of the Smithsonian Institution and the most prominent Cambrian worker in the world. Gil also used his time in Washington to expand his paleontological knowledge by examining the local Cenozoic outcrops and the full geologic range of the museum's collections. More important, he learned how to prepare, photograph, and write descriptions of his fossils for scientific publication. Of course, the main purpose of the trip was to begin work on the Wisconsin Cambrian faunas, which were divided among Ulrich (who, along with Charles Resser, covered the trilobites), Edwards (brachiopods), Rudolf Ruedemann (graptolites), and Raasch (Merostomata). These studies were to make use of the older USNM collections in addition to the extensive new and better localized material from the WGNHS and MPM collecting programs. Raasch spent most of his time in Washington preparing and photographing specimens for this work.

Throughout the 1920s, Raasch worked primarily on the Cambrian and Devonian, becoming well recognized for his abilities. His outstanding work at the MPM was rewarded in 1925 when he was made an assistant curator at the age of 22. That same year he married Polly Gutowski, whom he had met back at the Milwaukee Normal School. As he was "more interested in marriage" at the time, Gil completed his course work by correspondence while residing in Milwaukee, where he and his bride lived in a new house that his father built for them. His museum work provided him with the means to continue his Devonian research, including trips to Michigan and Ontario in 1927, which led to his second publication (Raasch, 1928).

In 1928, Raasch embarked on a comprehensive field program studying the Wisconsin Devonian, which led to some of his most important contributions to the stratigraphy of the state. Through his long-term study of Devonian outcrops, beginning when he was in high school, he was able to recognize the presence of older Devonian rock units below the well known Milwaukee Formation (Raasch, 1935a). Previous workers, such as Chamberlin (1877), had thought these poorly exposed rocks were Silurian, but Gil's meticulous paleontologic efforts demonstrated their true age.

Gil received his B.A. from the University of Wisconsin (UW) in 1929, producing a thesis on the Devonian of Michigan (W.H. Twenhofel, advisor). Later that year, he was presented with an outstanding opportunity to advance his career when the UW Geology Department hired him as its first full-time curator of the Geology Museum. On December 1, Raasch resigned from his comfortable ten-year MPM job, and moved to Madison. Things were looking up for Gil, and he seemed to have a bright future in the work he loved so much. Unfortunately, although the next decade would witness the publication of some of his most important research, it would end in the loss of his career in Wisconsin.

THE MADISON YEARS

Gil was an outstanding choice as curator of the UW Geology Museum. He was extremely well qualified with his extensive knowledge of Wisconsin geology and paleontology, his ten years' experience at the MPM, his enthusiasm, and his clear view of what would be needed to make the museum a success. The Geology Department's commitment to establishing a worthy museum was clear in its decision to hire the first full-time curator with an official faculty position. When Raasch arrived, he found the museum in a disastrous state, owing to years of neglect and a considerable loss of space (Burrell, 1975). It was not much more than a storage area with a few old exhibits that occasionally were used by students. The collections were modest and the most important specimens were some of R.P. Whitfield's type specimens figured in the old Geological Survey of Wisconsin volumes (Whitfield, 1882). The most significant exhibit material comprised a few of Ward's fossil vertebrate casts and the mounted mastodon skeleton from Richland, Wisconsin. Raasch made great plans to develop a real museum, having the same variety of programs he had participated in while at the MPM. Much to his disappointment, however, he soon discovered that, with the onset of the Great Depression, the department would not fund the museum at any level above his salary. Making the best of the situation, Gil set out to do what he could. His first priority was to curate the collections properly by instituting the Museum's first cataloging system and to expand the collections by acquiring new specimens during his research. Within a few years, he established what the department had never had previously: a functioning museum in which most specimens were cataloged and accessible, a new comprehensive collection of Wisconsin Paleozoic fossils, improved exhibits, and a program of public education that included radio interviews. Raasch had no departmental teaching responsibilities, although he was active in providing students with potential research topics and giving advice and assistance in their work. He was also able to continue his own research and pursue his graduate education, so his field work and collecting continued at an impressive level. The only change in his research was that the Cambrian became his main focus at the expense of the Milwaukee area Devonian.

Over the years, Raasch's role in Wisconsin Cambrian research had changed dramatically. He had begun as a field assistant to Edwards, but through his usual comprehensive work, he quickly became the expert on the subject. Unfortunately for Gil, his expertise placed him in the middle of a major controversy between E.O. Ulrich and Charles Schuchert, a situation from which he would not emerge unscathed. Ulrich and Schuchert had started out as amateur collectors, colleagues, and good friends in Cincinnati. Later, they became two of the most prominent stratigraphers in early twentieth-century North America. Along the way, they also became major adversaries, differing over scientific issues such as the validity of Ulrich's Ozarkian and Canadian Systems, but possibly having more personal disputes as well (Merk, 1985; Cloud, 1987; Weiss, 1992; Weiss and White, 1998; Weiss and Yochelson, 1995).

Sadly for Raasch, supporters on both sides of the Ulrich–Schuchert dispute were part of the Madison geological community long before he joined the department. Gil's future advisor, William Twenhofel (a former Schuchert student at Yale), and others at UW had been working on local Paleozoic geology for nearly as long as Ulrich. Twenhofel had a very dim view of many of Ulrich's ideas and was part of a local "Schuchert Camp" in competition with Ulrich's WGNHS Cambrian–Ordovician studies. For example, around 1918 L. Martin, W.H. Twenhofel, and F.T. Thwaites completed a manuscript on the geology and geography of the Sparta–Wilton topographic quadrangles, which was to have been published initially by the WGNHS and later by the USGS (Commissioners of the WGNHS, 1918, 1922). The manuscript was never formally published by either organization, possibly an early victim of the dispute. Edwards once told Raasch that Twenhofel had considered trying to get a law passed by the state legislature to bar outside parties (that is, Ulrich) from doing research in Wisconsin. Even if this was an exaggeration, it certainly suggests that there was considerable animosity between the two groups.

Around the same time, an even more serious controversy arose that involved not only the same local Paleozoic workers but many other Madison geologists, including C.K. Leith, chair of the UW Geology Department. In 1921, Schuchert was the nominee for President of the Geological Society of America (GSA). Ulrich, acting on his own personal dislike for Schuchert, became the principal player in, if not the originator of, a divisive attempt to subvert his candidacy (Weiss, 1992; Weiss and White, 1998). Ultimately, Ulrich's plan failed; however, many individuals were caught up in the controversy before it ended, with resulting long-term ill will. In Wisconsin, Leith became directly involved when he was unknowingly named as nominee for vice president on the Ulrich ticket (from which he promptly withdrew). In addition, Ulrich's friend and the head of the WGNHS, W.O. Hotchkiss, was also involved, functioning as Ulrich's Wisconsin coordinator to enlist other local GSA fellows to sign a petition for the special ticket, which he signed himself. Surprisingly, even Twenhofel was drawn into the fray on the Ulrich side at first, although he did not sign the petition (Weiss, 1992). He was likely misled by initial claims that the official ticket was being challenged over how nominees were chosen, rather than being aware of its true purpose to attack Schuchert. The fact that Leith, the alternate vice-presidential candidate, was chair of his department might also have had an impact on Twenhofel's early decision to support the petition. In the end, several others, in the department and the WGNHS, signed the controversial petition (Weiss, 1992).

Obviously, by the time Raasch started as department curator in 1929, significant potential for animosity between the Schuchert and Ulrich camps had developed in Madison. Initially, Raasch was considered a member of the "Ulrich Camp" because of his close association with his mentor Ira Edwards**,** his position as an understudy to Ulrich, and his active participation in the WGNHS mapping program. Gil was well treated and highly thought of by the Ulrich Camp, and his inclusion in their program and their support for his research had a significant impact in developing his career. Raasch became very well acquainted with Ulrich and remained fond of him throughout his life. He remembered that Ulrich was one of the first geologists he met who collected bed by bed, which was important for establishing precise biostratigraphic relationships that were needed to work out problems in the Cambrian. Of course, Raasch had not questioned Ulrich's ideas at first, but as he worked more independently on the Cambrian and took some of Twenhofel's classes, he began to realize that Ulrich had made some important misinterpretations about these rocks and fossils. Wanting to stay out of the controversy, Gil kept quiet about his new ideas for as long as he could, but eventually it became known that he held the key to resolving some of the scientific disputes between the two camps. Twenhofel had long thought that Ulrich was wrong, based on his understanding of facies, but he lacked the biostratigraphic proof needed to disprove Ulrich. Only Raasch had the skills, knowledge, and field program required to resolve the controversy, using a combination of comprehensive collecting and biostratigraphy.

In 1924, Ulrich had subdivided the Wisconsin Cambrian (and lower Ozarkian) into a thick sequence of 12 stratigraphic units. Denying the existence of facies, he believed that all the units were laterally persistent in character and separated by unconformities (Merk, 1985). In addition, Ulrich had relied on some marginally acceptable biostratigraphic information, including in some cases nothing more than small, old, poorly located collections made by others. Using Raasch's new data, Twenhofel, Raasch, and colleagues (Wannenmacher and others, 1934; Twenhofel and others, 1935) were able to demonstrate that a significantly thinner sequence of nine Cambrian rock units was a more realistic interpretation (Merk, 1985). They also showed that a number of Ulrich's successive units were really facies of one another.

Raasch began to publish his own papers on the Wisconsin Cambrian in 1935, the most important of which appeared in the Kansas Geological Society's Ninth Annual Field Conference guidebook (Raasch, 1935a, b). Most interesting of these contributions is his article on Paleozoic stratigraphy in the Baraboo area, which addressed the Ozarkian problem in Wisconsin (Raasch, 1935b). Ulrich (1924) had described the conglomerate at the base of his Devils Lake Sandstone and underlying rocks as "the best objective evidence we have in establishing the verity of the break between the Cambrian and the Ozarkian." On the basis of what he observed in the Baraboo area and in outcrops on Lake Mendota, Raasch had conclusive evidence that Cambrian fossils occurred in strata above those that Ulrich had identified as his younger Ozarkian strata. Moreover, his evidence appeared in the same outcrops that Ulrich considered the best examples of the unconformity separating the Cambrian from his Ozarkian. Raasch's work marked the beginning of the end of the Ozarkian System in Wisconsin, and Ulrich would have to look elsewhere for evidence to support his creation. Raasch still liked and had a high regard for Ulrich, so he wrote his paper in a complimentary manner to "soften the blow." Unfortunately, his plan didn't work as well as he had hoped and, indirectly, the paper was one of several factors that resulted in his leaving the curator position at Madison for a less-than-ideal alternative.

LEAVING MADISON

By 1935, Gil appeared to have been very successful at the University of Wisconsin, and it would have seemed that he had a very promising future to look forward to there. He was well on his way to completing his Ph.D. on the Cambrian Merostomata, he had accomplished a lot in the museum as curator, and he was recognized as an expert on the Cambrian of the Upper Mississippi River Valley by some of the most influential geologists in the country. He played a major role in the Wisconsin part of the eight-day, 1,542-mile-long Kansas Geological Society Field Conference to the upper Midwest in 1935 (fig. 6). At the time this was one of the largest field trips ever conducted in the area, attracting 115 participants, including many of the most prominent geologists in the region. It gave Raasch an opportunity to demonstrate his knowledge to this important audience, showing that he was on the cutting edge of the profession. However, even as he

Figure 6. *W.H. Twenhofel (left), Arthur Trowbridge (center, front row), and Raasch (right) on the 1935 Kansas Geological Society Field Conference. Photograph caption by F.T. Thwaites, photographer, reads: "Raasch arguing with Twenhofel and Trowbridge at Mendota." (Photograph courtesy of Avis Worthington.)*

enjoyed this attention, a number of factors that had developed over the years were beginning to make him feel very uncomfortable in his position as department curator.

Most obvious of these factors was his changing role in the Twenhofel–Ulrich controversy. He had tried to remain separate from the more personal aspects of the dispute, but in the end he found himself in what he described as "a no man's land" between the two camps. Some Ulrich supporters now viewed him as a traitor because he developed the evidence that allowed Twenhofel to get the upper hand in many of the arguments between the groups. Alternatively, the Twenhofel camp continued to view him with suspicion as a former opponent and now a "turncoat" whom they could never fully trust. In addition, some UW faculty viewed Gil as a little too ambitious for his position as curator; others thought he was too serious and focused on his work compared to the rest of the faculty. He did get along well, however, with the students and some of the faculty. One of his most notable friendships was with Robert R. Shrock (fig. 7). Raasch and Shrock, the department paleontologists, had frequent discussions about paleontological matters and had begun a joint research project on the Kentland Disturbance in Indiana.

But otherwise, Gil was beginning to feel friendless at Madison. He had worked hard not to antagonize the principal players in each camp. Surprisingly, Gil was most successful with Ulrich, even though he had dramatically undermined some of Ulrich's most important ideas on the geology of the area. This was most evident during a discussion on local Paleozoic geology during the Kansas Geological Society Field Conference dinner in Madison, when Ulrich stood up and announced to the entire crowd that "Raasch was the only one who knew anything about the Wisconsin Cambrian." Although this was a highly complimentary statement from a very prestigious individual, Raasch thought this comment would end up causing him more trouble with the Twenhofel camp. Instead of being able to enjoy the compliment, Gil leaned over and whispered to his wife, "after that I don't think I will still be in Madison a year from now," and he would be right.

Figure 7. *Robert R. Shrock (left) and Raasch in University of Wisconsin Geology Department staff photograph (circa early 1930s). (Photograph courtesy of Avis Worthington.)*

Was Raasch correct in his assessment of his position at Madison, or was he being somewhat paranoid? Undoubtedly, Raasch realized that some faculty would bristle at a person in his position getting that much recognition. For example, there was already some jealousy in the department because he was publishing as much or more than most of the teaching faculty. More important, however, was the nature of his relationship with Twenhofel, his advisor. Even though they had co-authored several papers and it was Gil's work and biostratigraphy that had provided the evidence to prove Ulrich wrong, he felt that Twenhofel didn't really like him, and their professional relationship continued to decline. Raasch respected Twenhofel and found him to be a great teacher, but a less-than-gifted field geologist. When in the field together, Gil recalled that he had to run ahead to the next outcrop whenever possible and quickly measure the section, otherwise Twenhofel would merely estimate the thickness of individual rock units and not bother to get exact information. Certainly, his being more knowledgeable than Twenhofel in certain areas and his initial association with the Ulrich Camp clouded their relationship. Joe Emielity, a student in the department in the late 1930s, remembers a rumor that "Twenhofel was taking credit for Raasch's work" (J. Emielity, 2000, verbal communication). However, in the end, the problems with his advisor were not the direct cause of Gil's departure from Madison. He probably would have stayed on as curator and completed his doctorate if this was all that he had to deal with. Quite by accident, he became aware of another, far more serious problem that affected his employment situation directly.

In 1934, Raasch applied for support from The Geological Society of America (GSA) to complete his merostome work and to publish it as a GSA Special Paper. Soon after applying, he was called into Twenhofel's office and told that his proposal had been returned because GSA would only fund proposals from a faculty member. Understandably, Gil was shocked to now discover that, although he was still curator, his faculty status had been revoked without his knowledge. At first, he thought that it had something to do with his poor relationship with Twenhofel, but he later discovered that he lost his position as part of a blatant exercise in nepotism. C.K. Leith, the very authoritarian chair who usually made all departmental decisions himself, had wanted his son Andy to succeed him in running the Geology Department someday (Bailey, 1981). As an early step towards realizing this goal, Leith appointed Andy as assistant professor in the department in 1934, surreptitiously using Raasch's faculty position because the university would not provide an additional new position for the department. Shortly afterwards, Leith was overthrown as departmental chair for other reasons, but the damage to Raasch had already been done.

After Twenhofel told him what had transpired, Raasch went to the new chair to try to rectify the situation. A year passed, and neither request was addressed. Raasch felt that his overall position in the department had continued to decline, and he decided that he had no choice but to leave. Having met some oil geologists on the Kansas Geological Society field trip who were very impressed with his capabilities, Raasch was offered two higher paying jobs in the oil industry, and he accepted one in 1936.

His departure from Madison was not the end of his trouble with the department, however. Gil later learned that some UW faculty members had secretly visited the MPM to search for specimens that they politely claimed he had taken without authorization from the UW collections. Some of this supposedly missing material was part of the Whitfield (1884) type specimens, which never were part of the UW collections to begin with and had long been housed at the University of California. Other "missing" specimens, such as his merostome collection, were collected either while he was employed by the MPM or collected at his own expense and, therefore, were never part of the university collection. Raasch was particularly incensed about this attack on his integrity, considering that he had single-handedly built up the UW geology museum's reference collections, had never been asked about any "missing" material before others were contacted, and no department policy existed against staff and students keeping their own collections of fossils, rocks, or minerals, which many did even if collected on university time.

IN THE OIL FIELDS AND WORLD WAR II

Raasch spent the next four years working as an oil geologist in Kansas and Oklahoma. Unfortunately, the fields he worked on were not good producers and by 1941 he was unemployed and on his way back to Wisconsin. His most important scientific accomplishment during this time was his 1938 discovery of a Permian insect bed in Oklahoma. Needing money to support his family, Raasch sold insect specimens to Ward's Natural Scientific Establishment in Rochester, New York. In turn, Ward's sold these specimens directly to an unnamed scientist who was planning to describe the material. After a while, Ward's informed Raasch that their customer was Frank M. Carpenter at Harvard University's Museum of Comparative Zoology, the most prominent fossil insect worker of the twentieth century, so that they could work together directly. In 1940, Raasch and Carpenter assembled a collection of more than 5,000 fossil insect specimens, with funding from the GSA (Raasch, 1946; Carpenter, 1947). Although this is one of the richest Permian insect localities known, the fauna remains largely undescribed.

Figure 9. *Raasch (left) on Wisconsin Geological Society field trip to Whitnall Park, Milwaukee Co., Wisconsin, June 8, 1941. (Photograph courtesy of Richard Worthington.)*

Figure 8. *Joe Emielity (left) and Raasch on field trip to western Wisconsin (circa 1940). (Photograph courtesy of Joe Emielity.)*

Upon returning to Milwaukee in early 1940, Raasch was penniless and needed to secure employment to support his family, which now included two daughters, Avis and Elaine. Of course, the most logical place to look for work was the MPM. His old friend Ira Edwards was now museum director and hired Gil as the supervisor of the museum's WPA program. Here he met Joe Emielity, a recent UW geology graduate who was also working in the program. Gil was given space to work on his research in the MPM Geology Department, where he resumed his Cambrian work and rekindled his interest in the local Silurian. Raasch and Emielity did field work together on the local Silurian and made several field trips to the Cambrian of western Wisconsin (J. Emielity, 2000, verbal communication). In addition, they met with Charlie Bell from the University of Minnesota and others for a Cambrian field conference (fig. 8). Raasch also continued his public education interests, participating in activities of the Wisconsin Geological Society, for which he had been a charter member in 1936 (fig. 9), and he was paid to conduct classes in geology and astronomy at the Milwaukee Social Center. Throughout his life, Gil had valued interaction with enthusiastic amateurs and schoolteachers, and he always felt that it was the duty of professionals to educate and encourage the public in science.

Aside from his WPA job, the MPM held the potential for better employment possibilities because the recent appointment of Edwards as museum director had opened up a curator position. Gil would have liked nothing more then to spend the rest of his career working there. Unfortunately for Raasch, the museum, and the state, he would not get the job. Competition for the position within the MPM had been intense, even before Gil returned to Milwaukee. Naturally, other museum employees interested in the position were not happy to see Raasch back in town, considering his qualifications and past association with the institution. At one point, he was invited to lunch with some of the staff, only to be threatened over his interest in the job, and he was told that, if he applied for the position, they would cause political trouble for him through their connections in City Hall. Although Edwards seemed very friendly towards Raasch, to the long-term detriment of the MPM, he was not hired for the position even though, unquestionably, he was best suited for the job. Why he was passed over for the position is unclear and surprised many at the museum (J. Emielity, 2000, verbal communication).

The only positive result from Rassch's WPA museum employment was that his friend Joe Emielity was later able to secure a position as an assistant scientist in the Geology Department. Gil had encouraged Emielity to work on local paleontology, which he did for the next 35 years. Although he was prohibited from doing local research and had no support, Joe documented numerous temporary rock exposures in the Milwaukee area, expanded the collections, and encouraged several generations of young collectors, many of whom became professional geologists, including the authors of this paper.

In the summer of 1942, the WPA program was canceled and Raasch was forced to find employment elsewhere. After working in a factory for a short time, he applied for and received an officer's commission in the Army Air Force, where he worked in intelligence. Assigned to SHAPE (Supreme Headquarters Allied Powers, Europe), he played a central role in developing the idea of using areal bombardment to destroy German rail lines instead of focusing on more traditional targets such as centralized rail yards. For his work in "Operation Strangle," Raasch was decorated with the Bronze Star. In January 1946, Raasch was discharged from military service with the rank of major, and he returned to UW on the GI Bill to complete his doctoral degree. By this time, he had already published his original dissertation topic on the Cambrian Merostomata (Raasch, 1939) and Twenhofel, his former advisor, had retired in 1945. Lewis Cline served as his new advisor, and his new dissertation topic on the Wellington Formation of Oklahoma addressed the geologic setting of the Permian insect beds that he had discovered in 1938. With his doctorate completed just five months later, in May 1946, Raasch left the University of Wisconsin for the last time.

THE ILLINOIS YEARS

After an interruption in his scientific career lasting almost ten years, Raasch was once again able to seek employment that would allow him to pursue some of his research interests in Midwestern Paleozoic geology and paleontology. The position he now secured probably was not what he had hoped for, but he did value public education and would also be able to continue some of his Midwestern research. In 1946 the Illinois State Geological Survey (ISGS) had decided to reestablish its educational program, which had been interrupted during the war. At the suggestion of Carl Bays, an ISGS geologist who had been a colleague in Madison, Raasch was hired to head the program, based on his vast experience in public education in Milwaukee and Madison. The intent of the ISGS education program at the time was more in the form of "public relations" intended to encourage the teaching of geology in state high schools.

Shortly after arriving in Urbana, Illinois, Raasch had rebuilt a program of public field trips and talks, radio interviews, and other activities (Raasch, 1948b), which were very successful. For the next seven years, he would lead almost fifty well attended trips to all parts of the state, covering all aspects of Illinois geology. Aimed towards high-school teachers, the field trips also included the general public, frequently drawing 50 to 100 participants each (fig. 10). They were so well received that the ISGS was still receiving complimentary letters about Raasch's leadership more than thirty years later.

Preparation for these trips took Raasch to all corners of the state, allowing him to expand his geological background and occasionally conduct some research. The trips themselves provided some important

Figure 10. *Raasch speaking on one of the Illinois State Geological Survey public field trips at an Ordovician exposure, probably in northwestern Illinois (circa 1950). (Photograph courtesy of Richard Worthington.)*

information. On a 1950 trip to the National Quarry at Joliet, one participant found a trilobite specimen (*Ekwanoscutellum laphami*) in Brandon Bridge strata, providing Gil with some critical information about Silurian stratigraphy of the region. (Unfortunately, he couldn't talk the boy out of the specimen.) Even though his Survey job was demanding, Raasch had several outstanding assistants, including Louis Unfer and Margaret Bargh, who played a key role in making the education program successful. Under Raasch's direction, the ISGS education program became the best of any state survey in the country, and it remains successful to this day.

While he was fulfilling all expectations with the education program, Gil had other talents to contribute to the ISGS, and, of course, he wanted to get back to his research interests. Unfortunately, he would again run into problems, not because he was neglecting his official duties, but because of turf wars among the Survey staff. Having been hired to run the education program, Raasch had to request permission to work on stratigraphy and paleontology, which were the domain of another division in the Survey. Because it had become well known that he was already an accomplished researcher and noticeably underemployed in his position at the Survey, he received permission to serve as the ISGS Cambrian expert. There was a distinct lack of enthusiasm for this arrangement from members of the Stratigraphy and Areal Geology Section (SAG Section), who were in charge of this kind of work. However, there was little Cambrian exposed in the state and no one else was really working on these rocks, so, given Raasch's reputation in the field, no serious objections could be made. Over the next few years, Gil played a major role in Cambrian studies, serving on the Cambrian Subcommittee of the Division of Geology and Geography at the National Research Council and publishing several papers (Raasch, 1950, 1951, 1952). At the ISGS he also worked with Herb Glass on an innovative approach to identifying the Cambrian–Ordovician boundary using clay mineralogy, which, unfortunately, never was published (H. Glass, ISGS, 1999, verbal communication).

Permission to work on the Cambrian would not be extended to other subjects, however, and it was Gil's interest in the Silurian that would lead to trouble later on. Since its beginning early in the twentieth century, the ISGS had a number of individuals working on Silurian rocks of the state. The most important project developed in the early 1930s, with J Harlen Bretz's mapping in the Chicago area and the insoluble residue studies of these rocks undertaken by Lew Workman. Although this effort lapsed, interest in the Silurian had been reestablished during the 1940s by Heinz Lowenstam's discovery that Silurian reefs controlled some of the Illinois oil reservoirs. Lowenstam (who was good friends with Raasch) left the Survey in mid-1948 and, by the time Raasch again started to work on his Wisconsin-based Silurian project in his spare time, little was being done in the Illinois Silurian.

Gil expanded his efforts and thought that his work was being done with official approval. In the ISGS interim board report dated February 24, 1953, Raasch was listed officially as “preparing a paper which will present a detailed Niagaran time scale with correlation” under the heading “Silurian and Devonian Stratigraphy” in the SAG Section, verifying that his efforts were common knowledge. However, when he later submitted the paper for review, a major uproar ensued. Claims were made that the paper was in direct competition with work being done in the SAG Section, that the section was unaware of Raasch’s work on the subject (even though it had been listed in the section head’s last report), and that much of the paper was inaccurate and could not be recommended for publication (memo from SAG Section head H.B. Willman to Morris M. Leighton, March 26, 1953). An undated memo from Willman to A.C. Beaven provides additional insight into claims against Raasch and reveals the territorial nature of research topics at the time. It reads, in part: “[Raasch’s paper] borrows much from our Silurian study, which is not mentioned… It skims the cream off of some of the obvious correlations we had planned to make…Such situations can only be avoided by having all the stratigraphic research of the Survey under central direction.”

Because most of his Silurian research had been conducted in Wisconsin before he arrived in Illinois, Raasch was incensed at the accusations that he was stealing the information and ideas of others at the ISGS, trying to preempt their work, and that he would be publishing substandard work. On May 8, 1953, Raasch presented his paper at the annual meeting of the Illinois Academy of Science and later sent out copies for comment. In an attempt to resolve the controversy, he met with Morris M. Leighton, Chief of the Survey, on June 2. Again, Raasch was accused of stealing the information that provided the basis for his ideas, told that his paper would preempt ISGS work, and subjected to other erroneous claims. In a June 3 memo to Leighton, Raasch again emphasized that his paper focused primarily on Wisconsin Silurian rocks and that the work was not done on Survey time. Leighton responded on June 9, telling Raasch that he could not publish his paper outside of the Survey and that he could no longer work on the Silurian until the SAG Section had finished its Silurian study. Specifically, Leighton stated, “The Silurian studies of the Survey are assigned to Dr. Willman. His prosecution of those studies must be protected on behalf of the Survey’s objectives. Any other staff member who has a contribution to make should make it to Dr. Willman for the Survey’s benefit.” In addition, Leighton directly accused Raasch of using information unethically, “Acceptance by a staff member of confidential information for his own use and which he may use in conflict with the Survey’s organized program is untenable.” Moreover, Leighton suggested that Raasch’s information was being withheld unjustly from the Wisconsin Survey (who had no interest in it), and observed that, if the work was published by a non-Wisconsin Survey employee working for the ISGS (even if done independently), interstate trouble could ensue.

Justifiably, Raasch became enraged at being accused of unethical research practices, forbidden to publish his paper even as a private individual, and directed to stop any further work on the Silurian even on his own time. As a result, on July 16, 1953, Raasch handed in his resignation to the Survey, having accepted a job offer from Lew Workman to work for Canadian Stratigraphic Service in Calgary, Alberta. Ironically, Workman, who had left the ISGS the year before, was one of the co-authors of the ISGS Silurian project from which Raasch was being accused of stealing. Although he revised his paper many times over the next twenty years, Raasch would never find time to conduct the field work needed to finish his Silurian work. Willman’s paper was not published until 1972, and he later expressed regret over the episode, stating, “I should have handled it differently” (H.B. Willman, 1980, verbal communication).

CANADA

In the late 1940s and early 1950s there was extensive exploration for hydrocarbons in western and Arctic Canada. Raasch was hired by Canadian Stratigraphic Service (CSS) specifically to use his extensive paleontological background to date and correlate Paleozoic rock units as part of this exploration. His skills of fossil identification and ability to establish biostratigraphic frameworks was well known, and he was given the task of working with collections from not only the Paleozoic but from younger rocks as well. He quickly became an expert on new parts of the stratigraphic column in which he had little previous experience. For example, when he arrived in Calgary, he had a copy of Stuart Weller’s monograph *The Mississippian Brachiopoda of the Mississippi Valley,* published by the ISGS in 1914. Apparently, no one else there had a copy and, as the Mississippian rocks were an important part of regional exploration, he became the expert on them. His extensive background in Devonian correlation was especially important as much of the oil discoveries in Alberta were in rocks of that age. In 1956, he was hired by Shell Oil as a consultant and worked there until 1967. Shell would not hire him as a regular employee, however, because he was over the age of fifty. One of his most important research projects with Shell was a comprehensive study of the biostratigraphic correlation of the North American Ordovician. Because this work was done for Shell, it remains unpublished.

After 1967, Raasch ran his own consulting firm, Raasch and Associates, with the valued assistance of Patricia Alexander. A detailed account of this part of his career is beyond the scope of this paper, but he was very successful and well respected for his skills in biostratigraphy. He fully enjoyed his Canadian geological work, especially when in the Rockies or the Arctic, because it fulfilled his boyhood dreams of being a great wilderness explorer (Avis Worthington, 2000, verbal communication). Gil wrote many papers on the Paleozoic of western Canada and the Canadian Arctic during that time, and he was still writing papers until the time of his death. Among other notable accomplishments during his years in Canada, he was co-organizer and the editor of a symposium on polar wandering and continental drift (long before they were popular subjects), the proceedings of which were published in the *Journal of the Alberta Society of Petroleum Geologists* in 1958. He was also one of the organizers of the very successful First International Symposium on Arctic Geology, held in Calgary in 1960, and was editor of the two proceedings volumes published in 1961. His lifelong contributions to Devonian research were recognized when the three-volume proceedings of the Second International Symposium on the Devonian System was dedicated to him in 1988 (McMillan and others, 1988). He contributed a paper to these volumes, summarizing his Devonian biostratigraphic work.

As much as he had accomplished in Canada, Raasch was able to achieve little concerning his old research interests in the Midwest. He did publish an innovative paper on Cambrian wind direction at Baraboo as part of his Polar Wandering and Continental Drift Symposium (Raasch, 1958), a subject he first mentioned in his 1935 article (Raasch, 1935a) on the Baraboo Paleozoic. In 1966, he published a paper on transgressive-regressive cycles in Croixan sediments (Raasch and Unfer, 1966), which was to be his last paper on Midwestern geology and paleontology. He "retired" as a consultant in 1988, when he was 85, but continued writing papers well into his 90s. Gil passed away at the age of 95 on January 20, 1999. He was preceded in death by his wife Polly and daughter Elaine.

RAASCH'S LEGACY AND UNFULFILLED PROMISE

What kind of legacy did Raasch leave after a career of almost 80 years? Gil was an exceptionally gifted and hardworking individual who had a very productive career. He is recognized as one of the most innovative and accomplished scientists ever to have worked on the Paleozoic geology and paleontology of the Midwest. He is also highly thought of among his former colleagues in western Canada. His scientific legacy will be invaluable to future research, especially in the Midwestern United States. This already became clear long before he died because during the past two decades, a new generation of scientists have found the collections, papers, and ideas of Gilbert O. Raasch critical for their thesis work and research.

As Midwesterners, however, we feel that his departure from this area was not only unnecessary and undesired, but also caused an irreplaceable loss to the geologic community here. The drive, ambition, and accomplishments of his early career promised much more for the future. His inability to get the kind of research job that he needed in the 1930s and 1940s, however, resulted not only in the loss of the research he never completed but also in the decline of the institutions and programs that he could have helped the most. Gil left many unfinished manuscripts from his early career that could not be completed because he could no longer undertake extensive Midwestern field work, and he had to work in jobs that offered little opportunity or time to publish his research. Likewise, many new research topics he had been interested in for a long time were never begun for the same reasons. In areas where rock exposures are always plentiful, Rassch's departure might not have had a long-term impact. In outcrop-poor southeastern Wisconsin, however, temporary exposures and quarries are the major source of geological and paleontological information. After Raasch left the region, little of this information was documented for nearly thirty years. Research interests dramatically declined after he left, and for decades little work was done by anyone.

The institutions with which Raasch was associated also had problems related to his departure. For example, the UW Geology Museum suffered almost thirty years of decline, causing significant damage to the collection. It was not until Klaus Westphal became its curator and Lewis Weeks provided the necessary funding that the Museum was able to become the institution that Raasch had worked hard to create. Likewise, the MPM Geology Department experienced a similar decline as exhibit renovation became its main focus and as a result, the collections were neglected.

If Raasch had left the area on his own to further his career, the loss would be more acceptable; however, for all intents and purposes, he was excluded for less than noble reasons. At the UW and the ISGS, he was virtually driven out by extremely poor treatment. At the MPM in the early 1940s, for unknown reasons he was denied a position for which he was best suited. Even after moving to Canada, Raasch always wanted to return to the Midwest, not only to complete some of his lifelong research interests, but also to find employment. At various times, he inquired about or applied for jobs in the area, including state geologist of Wisconsin, director of the MPM, and curator of the MPM Geology Department. Others were chosen to fill these positions and, as a result, Raasch never had the opportunity to finish his work here. Although he never expressed bitterness about the turn of events that his career had suffered, it was clear to us that Gil was very disappointed because he would have chosen to remain in or return to Wisconsin, if he had had the chance.

Does Raasch's career hold any lessons for scientists just starting their careers? More than anything, it demonstrates the impact that "office" politics can have within the scientific community. Even though Gil, as a student, had what would appear to have been some winning traits for a scientific career, they frequently caused him trouble, despite his hard work and demonstrated accomplishments. Starting out as a young expert with clearly defined research interests created problems, first in the conflict between his schoolwork and research and later in the lack of acceptance by his older scientific peers. Neither did his drive, well defined goals, and serious nature always win him friends among his colleagues, some of whom had more relaxed attitudes toward scientific research. Gil also discovered that knowing what he wanted to do and working toward that goal without the proper credentials was not always a successful career strategy.

Everyone who has an interest in Midwestern Paleozoic rocks and fossils is inspired by all that Raasch accomplished, but we will never know how much more he would have produced had he been able to continue his work in the region.

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